Impact Evaluation of a Conditional Cash Transfer Program

The Nicaraguan *Red de Protección Social*

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Contents

List of Tables iv

List of Figures vi

Foreword vii

Acknowledgments viii

Summary ix

1. Introduction 1

2. Design and Implementation of the *Red de Protección Social* 4

3. Design of the Evaluation, Methodology, and Validity 11

4. The Effects of Conditional Cash Transfers: The *Red de Protección Social* 26

5. Conclusions 56

Appendix A—Descriptive Chronology of RPS Program Activities in Phase I 59

Appendix B—Household Targeting in Geographically Targeted Areas 61

Appendix C—Contractual Indicators for One-Year RPS Evaluation in IADB Loan Contract 62

References 63

Tables

2.1 Nicaraguan RPS eligibility and benefits in Phase I 7

2.2 Nicaraguan RPS beneficiary co-responsibilities monitored in Phase I 9

3.1 Calculation of the double-difference estimate of average program effect 12

3.2 Survey non-response and subsequent attrition 20

3.3 Comparison of intervention and control *comarcas* at baseline 2000 22

3.4 Percentage of households benefiting from development programs and services 25

4.1 RPS average effect on annual total household expenditures 27

4.2 RPS average effect on per capita annual total household expenditures 29

4.3 RPS average effect on per capita annual food expenditures 30

4.4 RPS average effect on food shares (percentage) 31

4.5 RPS average effect on the composition of food expenditures 32

4.6 RPS average effect on hours worked last week: Women 34

4.7 RPS average effect on hours worked last week: Men 35

4.8 RPS average effect on enrollment, children age 7–13 in first to fourth grades 38

4.9 RPS average effect (2000–02) on school advancement, children age 7–13 in first to fourth grades, by starting grade in 2000 42

4.10 RPS average effect on school advancement, children age 7–13 in first to fourth grades (2000–02), by poverty group 43

4.11 RPS average effect on working, children age 7–13 in first to fourth grades 44

4.12 RPS average effect on percentage of children age newborn to 3 years taken to health control in past 6 months 45

4.13 RPS average effect on percentage of children age newborn to 3 years taken to health control and weighed in past 6 months 45

4.14 RPS average effect on percentage of children age newborn to 3 years taken to health control and weighed in past 6 months, by poverty group 46

4.15 RPS average effect on percentage of children age 12–23 months with updated vaccination 47

4.16 Malnutrition in Central American countries 50

4.17 RPS effect on percentage of children under age 5 who are stunted (HAZ < –2.00) 51

4.18 RPS effect on percentage of children under age 5 who are wasted (WHZ < –2.00) 52

4.19 RPS effect on percentage of children under age 5 who are underweight (WAZ < –2.00) 53

4.20 RPS effect on HAZ for children younger than age 5 53

4.21 RPS average effect on percentage of children age 6–59 months given iron supplement (ferrous sulfate) in past 4 months 54

4.22 RPS effect on percentage of children age 6–59 months with anemia 54

4.23 RPS effect on average hemoglobin for children age 6–59 months 54

Figures

3.1 Illustration of the double-difference estimate of average program effect 13

4.1 Density function of per capita annual total expenditures in 2002: Control versus intervention 30

4.2a Enrollment in 2000 for 7- to 13-year-olds who had not completed fourth grade, by age 36

4.2b Enrollment in 2000 for 7- to 13-year-olds who had not completed fourth grade, by poverty group and by sex 36

4.3a Current attendance in 2000 for 7- to 13-year-olds who had not completed fourth grade, by age 37

4.3b Current attendance in 2000 for 7- to 13-year-olds who had not completed fourth grade, by poverty group and by sex 37

4.4a RPS average effect (2000–02) on enrollment for 7- to 13-year-olds who had not completed fourth grade, by age 39

4.4b RPS average effect (2000–02) on enrollment for 7- to 13-year-olds who had not completed fourth grade, by poverty group and by sex 39

4.5a RPS average effect (2000–02) on current attendance for 7- to 13-year-olds who had not completed fourth grade, by age 41

4.5b RPS average effect (2000–02) on current attendance for 7- to 13-year-olds who had not completed fourth grade, by poverty group and by sex 41

Foreword

In the 1990s Mexico launched a new social program—PROGRESA (now known as Oportunidades). As a conditional cash transfer program, PROGRESA integrated investment in human capital with access to a social safety net. From 1998 to 2000, the International Food Policy Research Institute (IFPRI) assisted in evaluating the program. Much of the ensuing research is summarized in IFPRI Research Report 139 by Emmanuel Skoufias.

Since the inception of PROGRESA, several countries, particularly in Latin America, have implemented similar programs. One reason for the growing popularity of these programs is that, by encompassing 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 these pilot programs, the *Red de Protección Social* (RPS), modeled after PROGRESA, was begun in Nicaragua in 2000.

IFPRI conducted a quantitative impact evaluation of this program. Findings show that the program was effective in several domains, largely erasing differences in health-care and schooling outcomes across expenditure groups. Moreover, the program overcame obstacles found in the lower-income settings of Nicaragua, compared to Mexico, Colombia, or Brazil. One unique aspect of RPS was its approach to health-care supply. Government-contracted private providers supplied the services rather than the Ministry of Health. The results show that such an approach can be an effective delivery mechanism in areas where government provision might prove difficult.

In late 2002, based in part on the positive findings of the various evaluations, the government of Nicaragua and the Inter-American Development Bank agreed to a continuation and expansion of a modified program for three more years. IFPRI remained involved in the evaluation of that second phase, including a qualitative evaluation, and the results continued to show that the program was effective in a number of important areas. Nevertheless, at this writing the future of the program is uncertain. Rigorous evaluations are important components of the policymaking process, but they are not the only ones.

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Director General, IFPRI

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Summary

In recent years, investing in the human capital of the poor has been seen as crucial to alleviating long-term poverty. Concomitantly there is growing recognition of the need for social safety nets to protect poorer households from poverty and its consequences during the push for economic growth. Not only are the stimulation of economic growth and investment in social safety nets both important strategies for economic development, they are also potentially complementary, because effective social safety nets may contribute directly to economic growth via improved human capital.

Consistent with this view, several Latin American countries have introduced conditional cash transfer programs that integrate investment in human capital with access to a social safety net. The primary objective of these programs is to generate a sustained decrease in poverty in some of the most disadvantaged areas. And 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.

These programs target the poorest communities and households and condition the cash transfers on attendance at school and health clinics. This effectively transforms cash transfers into human capital subsidies for poor households. By encompassing various dimensions of human capital, including nutritional status, health, and education, these programs have been able to influence many of the key indicators highlighted in national poverty reduction strategies. One early such program was PROGRESA in Mexico, begun in 1997.

Nicaragua's RPS

Modeled after PROGRESA, the Nicaraguan *Red de Protección Social* (RPS) or "Social Safety Net" is designed to address both current and future poverty via cash transfers targeted to households living in poverty in rural Nicaragua. 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-term poverty. The transfers are conditional, and households are monitored to ensure that they undertake prescribed actions intended to improve their children's human capital. RPS's specific objectives include supplementing household income for up to three years to increase expenditures on food, reducing school dropout rates during the first four years of primary school, and increasing the health care and nutritional status of children under five years of age.

Piloted in 2000, RPS had a budget of $11 million, representing approximately 0.2 percent of Nicaragua's gross domestic product or 2 percent of annual recurring government spending on health and education. Based in part on a quantitative impact evaluation conducted by the International Food Policy Research Institute, the Inter-American Development Bank and the government of Nicaragua expanded the program in 2002 with a $22 million budget intended to continue the program for an additional three years.

Findings of the Impact Evaluation

This research report presents the main findings of a quantitative impact evaluation of RPS against its primary objectives. To the authors' knowledge, this RPS study is the first rigorous, experimental evaluation of a government program in Nicaragua, and as such the main contributions of the research are empirical. The evaluation design is based on a randomized, community-based intervention with measurements before and after the intervention in both treatment and control communities.

In its pilot phase, RPS had positive and significant effects on a broad range of indicators and outcomes. Where it did not, the lack of such effects was often due to improvements in the control group. Nearly all estimated effects were larger for the extremely poor, often reflecting their lower starting points. As a result, the program reduced the inequality of most outcomes across expenditure classes.

On average, RPS supplemented total annual per capita household expenditures by 18 percent, and most of this increase was spent on food. The program resulted in an average increase of 640 Nicaraguan córdobas in annual per capita food expenditures and an improvement in the diet of beneficiary households. Expenditures on education also increased significantly, though there was no discernible effect on other types of investment expenditures. The economic crisis experienced by these communities during the period studied enabled RPS to operate somewhat like a traditional social safety net, aiding households during a downturn.

RPS produced a massive average net increase in school enrollment of 13 percentage points and an even larger effect of 20 percentage points on current attendance for the target population. The number of children in grades 1–4 who advanced two grades between 2000 and 2002 increased by 7.3 percentage points, despite the fact that advancement past the fourth grade was not a formal requirement of the program. In tandem with the increased schooling, the percentage of working children aged 7–13 declined by 5.6 percentage points.

Furthermore, the impact evaluation revealed an average net increase of 16 percentage points in the participation of children under age 3 in VPCD, the health-care program. At the same time, the services provided by the program, as measured by process indicators (including whether the child was weighed and whether the child's health card was updated), improved even more. Participation by children ages 3–5 also increased substantially. As with the effects for expenditures and schooling, average program effects for VPCD measures are larger among poorer households.

While it was not possible to demonstrate statistically that RPS increased vaccination coverage for children ages 12–23 months in the intervention group, vaccination rates did climb 30 percentage points in the intervention and control areas at a time when they were, on average, decreasing in rural areas nationally.

Finally, the more varied household diet and increased use of preventive health-care services for children were accompanied by an improvement in the nutritional status of beneficiary children under age 5. The net effect was a decline of 5.5 percentage points in the number of stunted children. This decline was more than 1.7 times faster than the rate of annual improvement seen at the national level between 1998 and 2001. Despite improvements in the distribution of iron supplements to these same children, however, RPS was unable to improve hemoglobin levels or lower rates of anemia.

Conclusion

RPS has improved a number of the indicators included in the Nicaraguan national poverty reduction strategy, at a time when many of them were not on track to achieve the goals set out

in that plan. The evidence from the evaluation strongly suggests that, if the program were to be expanded elsewhere in the poor rural areas of Nicaragua (as it was in 2003), it would be effective. As such, RPS could prove to be an important component of Nicaragua's overall poverty reduction policy.

CHAPTER 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 households from poverty and its consequences during the push for economic growth (World Bank 1997). Although at first glance stimulating economic growth and investing in social safety nets appear to be 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 conditional cash transfer 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 encompassing 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 *Bolsa Alimentação,* a nutrition-oriented cash transfer program in Brazil. A third large program is the *Programa de Asignación Familiar* (PRAF) in Honduras. In this report, we examine a fourth such program, the Nicaraguan *Red de Protección Social* (RPS) or "Social Safety Net."

In 2003, the Inter-American Development Bank (IADB), a key actor in promoting and financing this type of program, indicated that "conditional cash transfer programs (CCTs) have replaced unrestricted cash payments and price subsidies in several Latin American countries" (IDEA 2003, p. 1). Nancy Birdsall, president of the Center for Global Development, states: "I think these programs are as close as you can come to a magic bullet in development. . . . They're creating an incentive for families to invest in their own children's futures. Every decade or so, we see something that can really make a difference, and this is one of those things."[1]

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-care 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

[1] "To help poor be pupils, not wage earners, Brazil pays parents," *New York Times,* January 3, 2004.

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 poor households in rural Nicaragua. The transfers are conditional, and households are monitored to ensure that, among other stipulations, their children are 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-term poverty. RPS's specific objectives include:

- Supplementing household income for up to 3 years to increase expenditures on food
- Reducing dropout rates during the first 4 years of primary school
- Increasing the health-care and nutritional status of children under age 5

RPS comprised two phases over 5 years, starting in 2000. The pilot phase (or Phase I) lasted 3 years and had a budget of US$11 million, representing approximately 0.2 percent of gross domestic product (GDP) or 2 percent of annual recurring government spending on health and education (World Bank 2001, annex 21). As a condition of the IADB loan financing the program, 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,[2] the Government of Nicaragua and IADB agreed to a continuation and expansion of the program, known as Phase II, for three more years with a budget of US$22 million.

This report presents the principal findings from the quantitative impact evaluation of Phase I of RPS for a wide 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 younger than 5 years of age.[3] Although they have a long history and are widely used in developed countries, rigorous, large-scale, randomized evaluations of social programs such as the one reported on have been rare in developing countries (Newman, Rawlings, and Gertler 1994; National Research Council 2001). Such studies have been increasing in popularity recently, however, after the widely cited case of PROGRESA (Rawlings and Rubio 2005; Skoufias 2005).

Although not new, the methodology employed in this report is generally considered "best practice" for evaluations. Moreover, to our knowledge, this research is the first

[2]The findings are summarized in a policy brief available in English and Spanish (Maluccio et al. 2005). Results reported there differ slightly from those presented in this report because of minor differences in the sample analyzed and in the methodology employed.

[3]In 2003, IFPRI was commissioned to carry out a qualitative evaluation. Adato and Roopnaraine (2004) present that evaluation which includes results on many (social) dimensions of the program not contemplated here, including program communication, empowerment, social effects of targeting, and relationships within households and communities. The focus in the present research report is on the earlier quantitative evaluation, although we draw on the qualitative work in some instances where the two overlap.

rigorous, experimental evaluation of a government program in Nicaragua. The main contributions of the research, then, are empirical. The results demonstrate that the program was effective in several domains. Indeed, it largely erased differences in health-care and schooling outcomes across expenditure groups. Nicaragua is a lower-income country when compared to Mexico, Columbia, or Brazil, where much of the research on conditional cash transfer programs has been done. As a result, there was skepticism about capacity to implement the program. Moreover, certain design features needed to be modified for the Nicaraguan setting. For example, because schooling outcomes are lower, the targeted population for school transfers was different from that used in PROGRESA. The results show that a conditional cash transfer program can be effective in a low-income setting. One unique aspect of RPS relative to most other programs of this type was its approach to health-care supply, in which services were provided by government-contracted non-governmental organizations (NGOs) rather than directly by the Ministry of Health. The results show that such an approach can be an effective delivery mechanism.

CHAPTER 2

Design and Implementation of the *Red de Protección Social*

To permit an assessment of how RPS altered behavior, it is first necessary to describe how the program operates and how it has evolved.[4]

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 (World Bank 2001). For Phase I of RPS, the Government of Nicaragua selected the departments of Madriz and Matagalpa from the northern part of the Central Region, on the basis of poverty level 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 were declining nationally (World Bank 2003). In 1998, approximately 80 percent of the rural population of Madriz and Matagalpa was poor, and half of these people 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 targeting purposively, RPS avoided devoting a disproportionate share of its resources in Phase I to increasing the supply of educational and health-care services.

In the next stage of geographic targeting, all six (out of 20) municipalities that had the small-scale participatory development program *Microplanificación Participativa* (Participatory Micro-planning [MP]) run by the national Emergency Social Investment Fund (FISE) were chosen.[5] 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, although this may not have been widespread, as MP did not completely cover the participant municipalities and it is unclear how

[4]Appendix A provides a descriptive chronology of activities undertaken during Phase I.

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

successful it was. Regardless of whether there was significant preexisting capacity, the six municipalities were well targeted in terms of poverty. Between 36 and 61 percent of the rural population in each of the chosen municipalities was extremely poor and 78–90 percent was extremely poor or poor (IFPRI 2002), compared with 21 and 45 percent, respectively, for Nicaragua as a whole (World Bank 2003). Although 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 was constructed, based on information from the 1995 National Population and Housing Census, and an index score was calculated for all 59 rural census *comarcas*[6] in the selected municipalities. The index was the weighted average of a set of *comarca*-level indicators (with respective weights in parentheses) known to be highly associated with poverty (Arcia 1999; Maluccio 2005):

1. Average family size (0.1)
2. Percentage without piped water in the home or yard (0.5)
3. Percentage without a latrine (0.1)
4. Percentage of persons over age 5 who are illiterate (0.3)

Higher index scores were associated with more impoverished areas. Since the index does not reliably distinguish between *comarcas* with similar scores, the 59 rural *comarcas* were grouped into four priority levels after the highest index score was renormalized 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. The 42 *comarcas* with priority scores 1 and 2 were eligible for the first stage of Phase I.[7]

Although the initial program design called only for geographic targeting (with *all* resident households eligible) in these 42 *comarcas,* about 6 percent of households deemed to have substantial resources by RPS were excluded ex ante from the program.[8] At the outset, then, just under 90 percent of households in intervention *comarcas* were beneficiaries. Consequently, the percentage of poor excluded from the program, or undercoverage, was less than 5 percent in these geographically targeted *comarcas.* Moreover, because the targeted areas were very poor, the percentage of beneficiaries who were non-poor (i.e., leakage) was also minimal, approximately 15 percent (IFPRI 2002). Despite these (quantitative) findings that the program was well targeted, there was substantial confusion at the local level about the beneficiary selection process (Adato and Roopnaraine 2004), raising the possibility of community tension as the result of excluding some families.

Program Design

RPS has two core components: (1) food security, health, and nutrition and (2) education.

Each eligible household[9] received a cash transfer known as the *bono alimentario* or

[6]Census *comarcas* are administrative areas within municipalities that typically include between one and five small communities averaging 100 households each. They are comprised of census segments and determined by the National Institute of Statistics and Censuses and in some cases do not coincide with locally defined areas also referred to as *comarcas.*

[7]IFPRI (2002) and Maluccio (2005) provide further details on the RPS targeting methodology.

[8]Appendix B describes how households were determined ineligible.

[9]RPS defined a household as a group of persons habitually living, eating, and cooking together. The same definition was used in all the survey work.

"food security transfer"[10] 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 with specially contracted providers. The workshops were held within the communities and typically included about 20 participants. They covered household sanitation and hygiene, nutrition, reproductive health, breastfeeding, and related topics. To ensure adequate supply in these poor, rural communities, RPS trained and paid private providers to deliver the specific health-care services required by the program. RPS adapted the individual counseling material from the Integrated Attention to the Child (AIN) program in Honduras (Van Roekel et al. 2000). These services, provided free of charge to beneficiary households, included growth and development monitoring, vaccination, and provision of antiparasite medicine, vitamins, and iron supplements. Children younger than age 2 were seen monthly and those ages 2–5, every other month. In practice, mothers brought their children to the local service location (typically a community center or house of one of the beneficiaries) to be seen by the health-care team working for the private provider. The prescribed procedure was the following. First, the professional nurse measured the child, inquired about the child's health and the caretaker's caring and feeding practices, and checked the vitamin A supplementation record. Then the doctor examined the child, prescribing appropriate antiparasite medicine or iron supplements according to the Ministry of Health protocol for making these prescriptions. If the child was growing well, the doctor congratulated the caretaker. Then the caretaker returned 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, whether the child had gained weight adequately the previous month, and whether the child had been ill.

Each eligible household also received 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 had not completed fourth grade of primary school. In addition, for each eligible child, the household received an annual cash transfer intended for school supplies (including uniforms and shoes) known as the *mochila escolar* or "school supplies transfer," which was contingent on enrollment. Unlike the school attendance transfer, which was a fixed amount per household regardless of the number of children in school, the school supplies transfer was for each child.

To provide incentives to the teachers, who had some additional reporting duties and were likely to have larger classes after the introduction of RPS, and to increase resources available to the schools, there was also a small cash transfer, known as the *bono a la oferta* or "teacher transfer."[11] This was given to each beneficiary child, who in turn delivered it to the teacher. The teacher kept one half, while the other half was earmarked for the school. The delivery of the funds to the teacher was monitored, but not their ultimate use.

Table 2.1 summarizes the eligibility requirements and demand and supply-side benefits of RPS. At the outset, nearly all house-

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

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

Table 2.1 Nicaraguan RPS eligibility and benefits in Phase I

	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) C$2,880 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	Health education workshops every 2 months Child growth and monitoring Monthly: Newborn to 2-year-olds Every 2 months: 2- to 5-year-olds Provision of antiparasite medicine, vitamins, and iron supplements Vaccinations (newborn to 5-year-olds)	*Bono a la oferta* (teacher transfer) C$80 per child per year given to teacher/school (US$6)

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

holds were eligible for the food security transfer, which was a fixed amount per household, regardless of household size. 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 distributing transfers. Table 2.1 shows the original U.S. dollar annual amounts and their Nicaraguan córdoba equivalents (using the September 2000 average exchange rate of C$12.85 to US$1). The food security transfer was US$224 a year and the school attendance transfer US$112.[12] On its own, the food security transfer represented about 13 percent of total annual household expenditures in beneficiary households before the program. A household with one child

[12]IADB calculated the transfer amounts taking into account the extreme poverty gap, that is, the difference between the extreme poverty line and the average level of expenditures of the extremely 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 US$0.58 and the extreme poverty gap, US$0.18. For comparison, the 1998 daily per capita poverty line is US$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 US$0.12, an amount designed to fill two-thirds of the average extreme poverty gap for extremely poor households.

benefiting from the education component would have received additional transfers of about 8 percent, yielding an average total potential transfer of 21 percent of total annual household expenditures. Over the 2 years, the actual average monetary transfer (excluding the teacher transfer) was approximately C$3,500 (US$272 or 17 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 (Caldés, Coady, and Maluccio 2004). 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 as a result of inflation over 2 years in Phase I. It is possible that any differences in the effectiveness of RPS over time resulted, in part, from a decline in the real value of the transfers, although 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 US$50 per beneficiary and the health-care services for children under age 5, approximately US$110, including the value of the vaccines, antiparasite medicine, 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(s) of the transfer if they failed to carry out any of the conditions listed in Table 2.2. The monitoring was done using the management information system (MIS) designed specifically for and by RPS. It comprised a continuously updated, relational database of beneficiaries, health-care providers, and schools. The MIS was also used to (1) select beneficiaries and prepare invitations to program incorporation assemblies, (2) calculate transfer amounts, (3) compile requests to the Ministry of Health for vaccines and other materials, and (4) monitor whether health-care service providers were meeting their responsibilities. Decision rules capturing the requirements in Table 2.2 were programmed directly into the MIS. Data forms for the various program participants were designed to feed into this system (including the household registry or census forms, school forms, and health-care provider forms that were all sent to the main office where they were entered into the computer).

Table 2.2 shows the four different "types" of beneficiary households in the program, who received different transfers and had to fulfill different requirements. Households with no children in the targeted age ranges were eligible only for the food security transfer but, at the same time, needed only to 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 had not completed the fourth grade) were also eligible for the food security transfer only, but had more requirements to fulfill, related to their young children. Households with children ages 7–13 who had not completed the fourth grade were eligible for both the food security and education transfers and were required to comply with the education-related conditions. To receive the education transfers, all target children had to comply; if one complied but another did not, the household did not receive the transfer. If, in addition, the household included children younger than age 5, the household was eligible for the same transfers but had more requirements to fulfill, in particular, those related to the health controls for young children. About 20 percent of the households had no targeted children, 25 percent only children under age 5, 20 percent only children ages 7–13, and the remaining 35 percent both children younger than 5 years and 7- to 13-year-olds.

RPS allowed this latter type of household to receive a partial transfer if it complied with the health-care requirements and

Table 2.2 Nicaraguan RPS beneficiary co-responsibilities monitored in 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 health education workshops every 2 months	✓	✓	✓	✓
Bring children to prescheduled health-care appointments Monthly (0–2 years) Every 2 months (2–5 years)		✓		✓
Adequate weight gain for children younger than 5 years[a]		✓		✓
Enrollment in grades 1–4 of all targeted children in the household			✓	✓
Regular attendance (85 percent, i.e., no more than 5 absences every 2 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.

not the education requirements or vice versa. During the first 2 years of transfers, approximately 10 percent of beneficiaries were penalized at least once and therefore did not receive, or received only one component of, their transfer. It was also possible for households to be expelled from the program. Reasons for expulsion included (1) repeated failure to comply with program requirements, (2) failure to collect the transfer in two consecutive 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.[13] Fewer than 1 percent of households were expelled during the first 2 years of transfers, although approximately 5 percent left the program voluntarily, for example, by dropping out or migrating out of the program area.

[13]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 never enforced. A third condition, punishment of children who did not have adequate weight gain, was dropped at the end of the Phase I 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. They also 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 that representative. The dominant reason for not selecting a woman was that either she was not living in the household or was not alive. 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 selected were women. These representatives attended the health education workshops and they were responsible for ensuring that the requirements for their households were fulfilled.

Although centrally administered, with its multisectoral approach across education, health, and nutrition, RPS required bureaucratic cooperation at the national, municipal, and community levels. Given funding and administrative oversight from FISE during Phase I, 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 schools 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 transfers, and any failures in fulfilling the conditions. Each *promotora* had, on average, 17 (SD 6) beneficiaries in her charge, although this average masked substantial variation ranging from 5 to 30 beneficiaries.

CHAPTER 3

Design of the Evaluation, Methodology, and Validity

To measure program effects, 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 would have happened if the program had not been available. The most powerful way to construct a valid counterfactual is to randomly select beneficiaries from a pool of equally eligible candidates.

Evaluation Design and Double-Difference Methodology

The evaluation for RPS was 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 of Phase I as described in the first section of Chapter 2) were randomly selected into the program. Thus, there are 21 *comarcas* in the intervention group and 21 distinct *comarcas* in the control group (IFPRI 2001a). Given the geography of the program area, however, control and intervention *comarcas* are in some cases adjacent to one another. Including a control from whom treatment was withheld was ethical because the effectiveness of the intervention was unknown and it was uncertain there was sufficient capacity to implement the intervention in all areas at once. In this case, random selection would seem to be about as fair as any other arbitrary decision rule for selecting the first set of beneficiaries.

The selection was done at a public event in which representatives from the *comarcas,* the Government of Nicaragua, IADB, IFPRI, and the media were present. The 42 *comarcas* were ordered by their marginality index scores and stratified into seven groups of six *comarcas* each. Within each stratum, randomization was achieved by blindly drawing one of six colored balls without replacement (starting with three blue for intervention and 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 1 year—that is, the control group was meant to be a control for only 1 year, after which it was expected there would be capacity to implement the intervention everywhere.[14] Because of delays in funding for RPS as a result of a governmental audit unrelated to the program, incorporation of households in the control

[14]As a result, another way to describe the experiment is that the program was randomly phased in.

Table 3.1 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 $(I_1 - C_1) - (I_0 - C_0)$

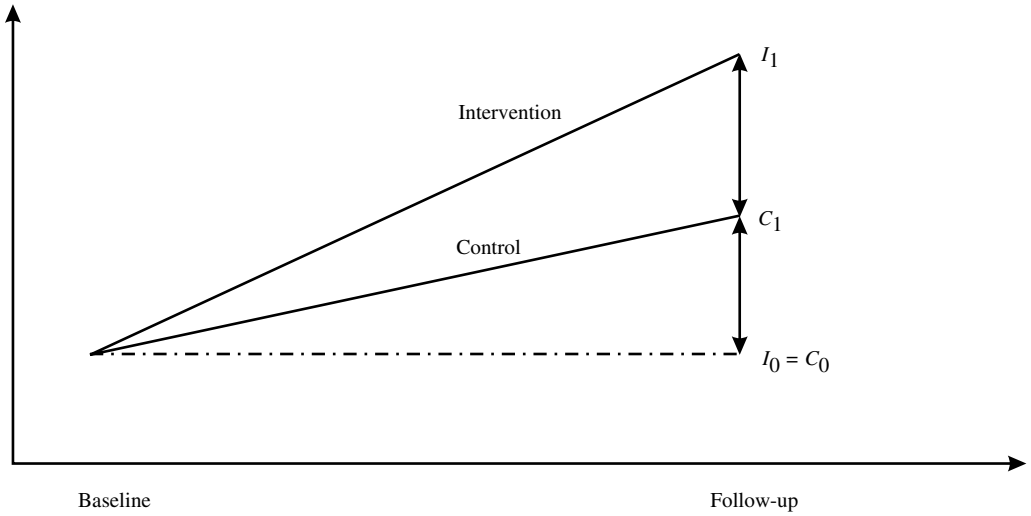
comarcas into the program was postponed until 2003, extending the possible length of the treatment–control evaluation by more than a year. In the end, control *comarcas* waited a little over 2 years before being fully incorporated into the program during its second phase. Even with this extension, however, the evaluation remains an evaluation of the *short-term* effects of the program (Thomas et al. 2003), although in some cases the effects on long-term indicators, such as child anthropometrics, are measured.

During the public selection, local leaders and government officials were informed generally about the program. This included that they were told that control *comarcas* would enter the program after 1 year (although as just described, 1 year turned into 2). This was done to ensure participation in the evaluation. While RPS did not publicize this plan to households in control *comarcas,* it would be naive to assume that households in control areas were entirely unaware of it. Over time they certainly would have learned about such a large program in neighboring villages and may even have been informed by local actors about their eventual possible participation. Some implications of this form of contamination for the experiment and interpretation of results are discussed later in this chapter.

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."[15] The resulting measures can be interpreted as the expected effect of implementing the program in a similar population elsewhere, subject to a number of caveats described below. The method is shown in Table 3.1 (which presents the basic format for all the results presented in this report). The columns distinguish between groups with and without the program (denoted by *I* for intervention and *C* for control), and the rows distinguish between before and after the program (denoted by subscripts 0 and 1). Anticipating one of the analyses presented below, consider the measurement of school enrollment rates for children. Before the program, one 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. Once the program has been implemented, however, one would expect differences between the groups as a result of the program. Furthermore, because of the random assignment, one would 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. 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 estimate obtained by subtracting the preexisting differences between the groups, $(I_0 - C_0)$,

[15]Ravallion (2001) provides a useful discussion on this and related evaluation tools. See also Baker (2000).

Figure 3.1 Illustration of the double-difference estimate of average program effect



from the difference after the program has been implemented, $(I_1 - C_1)$.

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 reflexive estimator of the program effect, the difference over time for the intervention group, $(I_1 - I_0)$. It is a potentially biased estimator because it includes 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 non-program-related change over time in the intervention group is the change over time in the control group, $(C_1 - C_0)$. Thus the average program effect is estimated 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, as in Figure 3.1. For an arbitrary indicator measured over time, it is assumed (for the graph) that as a result of the randomization, both the intervention and control groups start at the same level (on the vertical axis). No change in the indicator over time would lead to the outcome depicted by point $I_0 = C_0$ in 2002; if only the intervention group were being followed, one would then naively calculate the effect of the program as $I_1 - I_0$. However, as the control group makes clear, there was a trend over time that led to an improvement (in this example) of $C_1 - C_0$. Estimates ignoring this would overstate the effect of the program. Instead, the correct estimate of the program effect is $I_1 - C_1$; this is the double-difference estimate since $I_0 = C_0$. In the case where the trend line for the control group was declining, ignoring that effect would tend to understate the program effect.[16]

For this work, the double-difference technique is extended to account for three

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

measurements taken in 2000, 2001, and 2002. The basic estimating equation is shown in equation (1).

$$E_{ict} = \alpha_0 + \alpha_1 Y_1 + \alpha_2 Y_2 + \alpha_3 P_c + \delta_1 Y_1 P_c + \delta_2 Y_2 P_c + \mu_{ic} + \nu_{ict} \quad (1)$$

where

E_{ict} = outcome variable of interest for individual (or household) *i* in *comarca c* at time *t*
Y_1 = (1) if year 2001
Y_2 = (1) if year 2002
P_c = (1) if program intervention in *comarca c*
μ_{ic} = all (observed and unobserved) individual-, household-, or *comarca*-level time-invariant factors
ν_{ict} = unobserved idiosyncratic individual-, household-, or *comarca*-level and time-varying error

All the α and δ are unknown parameters.

The key 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). 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 individual-, household-, or *comarca*-level variables so that the δs can be consistently estimated. It is not necessary to include other variables in this regression for the consistency of the estimator for δ_1 and δ_2, although doing so increases the precision of the estimates.

Since we do not condition on the household-level decision to participate in RPS but instead only on whether the program was available in the household's *comarca,* this framework yields what is commonly referred to as an estimate of the intent-to-treat effect. The estimator is not subject to selection biases associated with the decision to participate in the program since it relies only on the randomized design. About 10 percent of the households in the intervention areas were either excluded by RPS or chose not to participate in the program. Survey sample households in this subgroup are not program beneficiaries so that basing estimates on the sample that includes them "dilutes" the estimated effects of the program. The intent-to-treat methodology we use is conservative, then, relative to measuring the effect of the treatment on the treated. To estimate the effect of the treatment on the treated, rather than estimating the double-difference, one would instead have to endogenize the participation decision, most likely using the random program placement as an instrumental variable. This approach amounts to rescaling the intent-to-treat estimates by the fraction of program participants. Since the participation rates are relatively high, it does not yield very different estimates, so we do not present them.

To assess differences in effects for the poor and non-poor for the analyses considered below, we also classify households into three household expenditure (or poverty) groups—extremely poor, poor (but not extremely poor), and non-poor—based on their preprogram 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). Double-difference estimates were calculated within each poverty group and when they differ across the groups are discussed in the text and presented in some of the tables.

In the double-difference analyses that follow, only households interviewed in all three survey rounds, that is, the balanced panel sample, are included. Standard errors are calculated allowing for heteroskedasticity and for clustering at the *comarca* level (StataCorp 2001). For simplicity, we do not control, for example, for the fact that the randomization was at the *comarca* level—so-called "community effects." When we do control for community effects or when we do not control for heteroskedasticity and *comarca*-level clustering, the precision of

nearly all the estimates increases, leading to stronger statistical significance. The estimations that follow 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 makes no substantive changes to the estimated effects.

Issues and Concerns Related to the Experimental Design

The value of 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).[17] A further advantage to a randomized design is that program impact is easy to calculate and, as a consequence, easier to understand and explain.

Of course, randomized evaluations are not a panacea. For example, they can be costly (both financially and politically) and often one must wait years for results, making them less useful for pressing policy decisions. Moreover, randomized design evaluations can provide only partial answers to some important questions. This is often referred to as their "black box" nature. The evaluation only allows us to assess most rigorously the effect of the program (or program components) that it was explicitly designed to assess.[18] Without further modeling and assumptions (not already explicit to the evaluation design), we are in the dark if we want to consider how even slightly changing the program alters the effect on the outcomes under consideration. For example, RPS provided a package of services in which all households were eligible for the food security transfer, regardless of whether they also received the educational transfers. Using only the randomized design on its own, it is difficult to tease apart the effects of the education component of the program from the nutrition component—all the observed effects, even those that pertain specifically to educational outcomes, are the result of the program as a whole. Similarly, it is difficult to assess the relative importance of the demand-side stimulus versus the supply-side interventions for the observed improvements in health care—all the observed effects reflect the combination of supply- and demand-side influences. Nor can we easily and reliably assess what the effect of the program would be if the size of any one of the transfers were to change, as they did when RPS was expanded in 2003.

While in this report we focus primarily on findings that derive directly from the experimental design, extensions to the analysis that begin to address the issues raised above or others are possible areas for future research. For example, the design feature that RPS, in contrast to most other programs of this type, offers household-level education transfers as opposed to child-level transfers, provides the opportunity to explore the effect of varying the transfer *per beneficiary* by comparing households with different numbers of school children. Indirectly, this would be an assessment of the effect of

[17]There is a recent increase in non-experimental approaches to evaluation, in particular using propensity score matching. The evidence is mixed, however, as to whether or not these provide a convincing alternative to experimental approaches (Heckman, Lalonde, and Smith 1999; Michalopolous, Bloom, and Hill 2004).

[18]Furthermore, because by design some members of society remain without the intervention, such evaluations can only measure partial equilibrium effects of the program, which may be different from the general equilibrium effects, for example, if the program were national (Heckman, Lochner, and Taber 1998).

varying the transfer size. Similarly, the different starting dates for the education and health components of the program (described below) could be exploited to investigate the relative importance of each of them.[19] Finally, using variation in availability or quality of schooling could provide an opportunity to separate demand- and supply-side effects on improvements in schooling (Coady and Parker 2004).

Even a well-implemented randomized design, however, is not without potential weaknesses. Heckman and Smith (1995) highlight that the apparent simplicity can be deceiving, particularly in poorly designed evaluations where contamination occurs as a result of (1) randomization bias, where the process of randomization itself leads to a different beneficiary pool than would otherwise have been treated; (2) anticipation effects, where control group behavior changes as the result of changes in expectations; or (3) substitution bias, where nonbeneficiaries obtain similar treatments from different sources. There is little reason to believe that the first consideration (randomization bias) is a concern in the RPS evaluation; for example, there was no evidence that households moved in part or in whole to obtain the program (Adato and Roopnaraine 2004). The fourth section of this chapter addresses the possibility of contamination of the control group suggested by the other two concerns.

Another limitation with randomized evaluations, or nearly any other evaluation for that matter, 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; the generalizability of the results is therefore less certain. 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. On the other hand, the observed outcomes may understate the likely outcomes if there was less need for RPS in these areas *because* of greater institutional capacity.

While it is not possible to claim that the 42 selected *comarcas* are representative of rural Nicaragua, there is evidence that they are similar in many key respects to a large number of other rural communities in the Central Region and in the country more broadly. First, using the marginality index scores based on the 1975 census (Arcia 1999), we estimate that three quarters of the approximately 150 rural *comarcas* in the departments of Madriz and Matagalpa have priority 1 or 2, as do three quarters of the approximately 1,000 rural *comarcas* in the country as a whole. If instead we consider levels of extreme poverty, there are more than 350 *comarcas* in the country with extreme poverty at or above 42 percent, the average level in the pilot areas. On these broad indicators used for geographical targeting, then, there are a large number of similar *comarcas,* indicating those chosen for the pilot were not atypical.

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 or longer-running 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., designing the MIS and preparing training materials for beneficiaries, *promotoras,* and health-care providers). Some of these activ-

[19]We acknowledge an anonymous referee for making this observation.

ities could have reduced the program's effectiveness during the pilot (Caldés and Maluccio 2005). 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—the Hawthorne effect (Krueger 1999). There are some patterns consistent with this phenomenon when we compare the effects after 1 year (2000–01) with those after 2 years (2000–02). Estimated effects on several outcome indicators were slightly lower in 2002 than in 2001, and not always entirely attributable to improvements in the control group. Unfortunately, we cannot directly test how much of this might be 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 in Nicaragua.

Data Collection

The data collected for the evaluation were an annual household panel data survey implemented in both intervention and control areas of RPS before the start of the program, in 2000, and after the program began operations, in 2001 and 2002.[20] A comprehensive household questionnaire based on the 1998 Nicaraguan Living Standards Measurement Survey (LSMS) was used (World Bank 2001). The LSMS questionnaire was expanded in some areas (e.g., child health and education) to ensure that all the 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.[21] An anthropometric module for children younger than age 5 was implemented in 2000[22] and 2002, although not in 2001. In this module, height (or length), weight, and hemoglobin (using portable Hemocue machines) were measured, following standard international procedures.

The household survey sample is a stratified (at the *comarca* level) random sample of all 42 *comarcas* described above and using the RPS population census collected in May 2000 as the sample frame. 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 of the 42 *comarcas,* 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 C. Assuming a random sample, a significance level of 5 percent, and a power of 80 percent, the indicator that required the largest sample size was enrollment for grades 1–4. To detect a minimum, statistically significant difference of 8 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 range. According to the 2000 RPS population census, 63 percent of households had at least one child between 6 and 12 years of age. 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,

[20]Results reported on here are based on the April 2005 release of the Nicaraguan RPS evaluation data, available from IFPRI.

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

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

the target sample was calculated as 1,764 households (42 households in each of the 42 *comarcas*).[23] 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.

Although 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 to select, contract, and train the NGO and private health-care providers. There were also delays in the delivery of transfers to households because of 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 transfers 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 the 2000 baseline, was that it would enable one to control better for possible seasonal variations in consumption and health. The change in survey date should have little effect on schooling, however, since the school year begins in late January and ends in November. With a control group, the possible bias introduced by seasonality is addressed by differencing across intervention and control groups. This difference in the timing of the survey, then, does not present a serious problem for the estimation of average program effects.

Moreover, 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 had only received 5 months of the health-care services, including the health education workshops. The unforeseen delay in operations illustrates the importance of having a credible control group—without the control it would have been risky to alter 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 non-response in the 2000 baseline survey and attrition and contamination in the follow-up surveys. Overall, 90 percent (1,581) of the stratified random sample was interviewed in the first round (see Table 3.2) with slightly lower completion in control *comarcas.* The principal reasons for failure to interview targeted sample households were that household members were temporarily absent (i.e., more than the several days the survey team would be in the area) or that the dwelling appeared to be uninhabited—both of which are likely to be associated with temporary or permanent migration. 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, the target sample was limited to these 1,581 first-round interviews and 1,453 (91.9 percent) were re-interviewed.[24] Among those classified as not being re-

[23]IFPRI (2001a) describes the sample size calculations in more detail and IFPRI (2001b, 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.

[24]Where possible, households who had moved within the six program municipalities were traced to their new locations.

interviewed are six households whose surveys were lost and 37 households living in control *comarcas* who in fact appear to have been program beneficiaries, despite being initially categorized as living in a control *comarca.* As described earlier, the *comarcas* used by RPS are census areas that often do not coincide with communities. These 37 households (spread across a dozen *comarcas*) possibly were included in the program as a result of reclassification of where they lived by RPS (they did not move), relative to the census boundary lines. Rather than retain them in the control group, thereby contaminating the results, they are dropped from the 2001 and 2002 samples. Not surprisingly, given that they represent only 5 percent of households in the control group, their inclusion or exclusion affects estimated results little. In 2002, just over 88 percent of target households (including the 37 contaminated) were re-interviewed, on a par with surveys of similar magnitude in other developing countries (Alderman et al. 2001; Thomas, Frankenberg, and Smith 2001).

Because the same target sample was used in 2002 as in 2001 (with the exception of the lost surveys and contaminated households) regardless of whether the household was interviewed in 2001, some households that were not interviewed in 2001 were successfully interviewed in 2002. Therefore, the sample for which there is a complete set of observations (in each of the three survey rounds) is 1,359, smaller than the 1,397 shown in the first row of the third column of Table 3.2. These 1,359 form the balanced panel sample used in the estimations throughout this report. After excluding the 37 contaminated households, the percentages interviewed in intervention and control groups were similar, indicating that at least the level of attrition was not significantly different between them.

Since the advantages of randomization are dissipated with attrition if it is non-random, we next examine the correlates of the observed attrition to assess the likely possible effects or biases on the ensuing analyses (Thomas et al. 2003). Because of the RPS census, which collected a variety of information relevant to the program for use in a proxy means prediction model and allows us to predict expenditures for each household, there is information on those households not interviewed in the baseline.

Probit regressions (not shown) on all original target sample households and predicting the probability of having been interviewed in at least the first round or, separately, having been interviewed in all three rounds, indicate that attrition is indeed non-random. Households that were not interviewed were more likely to have an older, more educated household head, larger family size, higher predicted expenditures, and more land. When the 37 contaminated households are included in the sample, households in the intervention group were more likely to be re-interviewed but when those households are excluded the indicator for the treatment group is no longer significant. A comparison of the means of these same characteristics for those re-interviewed compared with those not reinterviewed shows significant differences for a subset of them, but all of these differences are less than one quarter of a standard deviation of the variable in question, indicating they are not on average large differences.

Sample households are on average slightly better off than households lost to follow-up. Given the findings presented later in this report that estimated effects tend to be larger for poorer households, basing estimates on the balanced panel sample, as we do, is likely to underestimate the actual effects of the program.[25] Consistent with this,

[25]Similarly, households not captured in the first census and therefore not part of the sample frame nor in the RPS evaluation survey, were also slightly poorer on average (IFPRI 2001b), suggesting again the results presented in this report are conservative.

Table 3.2 Survey non-response and subsequent attrition

	Baseline 2000	Follow-up 2001	Follow-up 2002
Completed interview	1,581	1,453	1,397
	(89.6)	(91.9)	(88.4)
. . . *of which*			
Intervention	810	766	722
(percent intervention)	(91.8)	(94.6)	(89.1)
Control	771	687	675
(*percent control*)	(87.4)	(89.1)	(87.6)
Completed interview in	1,359	1,359	1,359
all three rounds	(77.0)	(86.0)	(86.0)
. . . *of which*			
Intervention	706	706	706
(percent intervention)	(80.0)	(87.2)	(87.2)
Control	653	653	653
(*percent control*)	(74.0)	(89.5)	(89.5)
Not interviewed			
Uninhabited dwelling	60	51	83
Temporary absence	100	28	46
Refusal	17	6	12
Urban (misclassified)	6	0	0
Contaminated	0	37	37
Lost questionnaire	0	6	6
Target sample	1,764	1,581	1,581

Source: Nicaraguan RPS evaluation data.
Notes: Percentage of target sample in parentheses.

when the sample is expanded to those who were not interviewed in all three rounds, most estimated effects increase in magnitude (and in precision, owing to larger sample sizes). A partial remedy to control for attrition bias is to estimate a household fixed-effects model, particularly if unobserved persistent heterogeneity is leading to attrition. When we estimate the models with these controls, the estimated effects again tend to increase slightly and are more precisely estimated. Taken together, we conclude that attrition is not a major concern for estimating program effects and emphasize that using only the balanced panel is likely to slightly underestimate effects, making it a conservative approach.

Validity of the Experiment and the Evaluation

Outcome of the Randomization

Although the selection of *comarcas* into intervention and control groups was undeniably random, it was at the same time only one of the millions of possible random draws. As a result, intervention and control groups may still differ in significant or, more importantly, substantive ways as the result of a "bad" draw. In this section, we provide evidence that the two groups are indeed similar, examining differences between the groups for a set of indicators (Behrman and Todd 1999). Even where between-group differences exist, however,

the evaluation can still measure impact because one can control for them in the analysis, using the double-difference method. Therefore, perfect "equality" between the two groups is not a necessary condition for the evaluation. Similarity does, however, put the analysis on a sounder footing, particularly if there is the possibility that there might be heterogeneous program effects associated with differences between the groups.

Table 3.3 first compares households in control and intervention areas on a set of demographic, education, wealth, and employment indicators. Of 15 indicators in the top panel, only two related to household demographics are significantly different at the 10 percent level of significance: (1) household size and (2) the number of children younger than 5 years old. These are unlikely to be problematic, however, because while they are significantly different, they do not appear to be substantively different. The difference in household size is less than 0.2 persons, 0.1 of which appear to be children younger than age 5.

In the bottom portion of the table, the proportion of households classified as extremely poor, poor, and non-poor are listed.[26] Because of the stratification into groups of six *comarcas* each according to the marginality index for randomization, the expectation is that the different poverty groups should be about equally represented in intervention and control areas. The extreme poverty rate in control areas is nearly 8 percentage points higher than in intervention areas, and significantly different. Differences between poverty rates are half that, but also significant at the 10 percent level. These differences are consistent with the difference in household size and may in part reflect that the per capita expenditure measures used here to calculate poverty do not adjust for demographic structure other than number of persons; that is, they are not per capita adult equivalents, for example (Deaton and Zaidi 2002). As noted earlier, however, the magnitude of these differences does not appear to be such that they should greatly affect interpretation of the results. Further, comparisons of the same set of 15 indicators across intervention and control areas *within* each poverty group (extremely poor, poor but not extremely poor, and non-poor) reveal only 5 significant differences in 45 comparisons.

The tables that show double-difference estimates of the effects of the program presented later in this report all show differences at baseline for the entire range of outcomes analyzed. In no instance were any of those measures significantly different at baseline. From this analysis, we conclude that the random assignment by *comarca* was effective in delivering largely similar groups on the whole, as well as similar groups of extremely poor, poor but not extremely poor, and non-poor.

Contamination of the Control Group: Expectation Effects

As described in the first section of this chapter, households in the control group were likely to have learned about the program and about the likelihood of their becoming beneficiaries in the future. Even if households knew of the plan to expand to control *comarcas* (in 2001), however, uncertainties around the funding and expansion of this

[26]These and other descriptions of poverty in the sample are calculated based on 2001 per capita annual expenditure poverty lines of C$2,691 (US$202) for extremely poor (calculated as the amount required to purchase a minimum requirement food basket) and C$5,157 (US$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. Consumer price inflation was about 4 percent per year during this period so that adjusting the poverty line for inflation makes no difference in the substantive findings reported here.

Table 3.3 Comparison of intervention and control *comarcas* at baseline 2000

Indicator	Intervention (n = 706)	Control (n = 653)	Difference (intervention – control)	Total (n = 1359)
Household size	5.93	6.12	**–0.19***	6.02
	(2.95)	(2.78)	[0.10]	(2.87)
Number of children <5 years old	1.09	1.19	**–0.10****	1.13
	(1.10)	(1.06)	[0.04]	(1.08)
Number of children <3 years old	0.71	0.77	–0.06	0.74
	(0.85)	(0.82)	[0.13]	(0.84)
Percentage of female-headed households	13.2	15.3	–2.1	14.2
			[0.26]	
Age of household head	44.6	43.9	0.7	44.3
	(16.1)	(15.3)	[0.57]	(15.7)
Years of education of household head	1.69	1.60	0.09	1.65
	(2.17)	(2.22)	[0.41]	(2.20)
Average years of education of adults	2.21	2.23	–0.02	2.22
	(1.87)	(1.85)	[0.58]	(1.86)
Percentage of children between 7 and 13 years of age who matriculated	68.1	68.5	–0.04	68.2
			[0.66]	
Time it takes to walk to school (minutes)	26.6	21.8	4.8	24.3
	(34.1)	(24.2)	[0.13]	(29.8)
Number of rooms in the home	1.50	1.53	–0.03	1.51
	(0.78)	(0.84)	[0.69]	(0.81)
Number of durable goods[a]	0.23	0.23	0.00	0.23
	(0.47)	(0.47)	[0.86]	(0.47)
Size of land owned (hectares)	1.41	1.49	–0.08	1.45
	(1.47)	(1.88)	[0.69]	(1.68)
Percentage working as agricultural producer	83.1	82.8	0.3	83.0
			[0.88]	
Percentage working in coffee	9.9	8.6	1.3	9.3
			[0.40]	
Percentage using credit	15.3	16.4	–1.1	15.8
			[0.58]	
Expenditure class in baseline				
Extremely poor	33.9	41.3	**–7.5*****	37.5
			[<0.01]	
Poor	75.6	79.5	**–3.9***	77.5
			[0.09]	22.5
Non-poor	24.4	20.5	**–3.9***	
			[0.09]	

Source: Nicaraguan RPS evaluation data.

Notes: Standard deviation in parentheses and p-values in brackets for test of equality of populations across groups using two-tailed proportion test for proportions and non-parametric Kruskal-Wallis test for all others (StataCorp 2001). Analysis based on 706 observations in the intervention group and 653 observations in the control group in each year. Bold indicates significance at *** the 1 percent level, ** the 5 percent level, and * the 10 percent level.

[a]Includes radio/tape recorder, stove, air conditioner, and fan.

government-run program would certainly have led them to assign a probability of less than one to that eventuality. The uncertainty is underscored by the fact that the original plan was in fact not fulfilled, and control households did not enter the program until more than a year after originally envisioned. Nevertheless, we cannot rule out the possibility that households had expectations about receiving the program, and that they changed their behavior based on those expectations.

Rational households aware of the program likely would have endeavored to learn about it, in an effort to better position themselves as potential beneficiaries. One indicator of program knowledge (available in 2002) is how well households answered six true-or-false questions about program requirements.[27] On average, about half of the households in the control group answered that they did not know. Of those who indicated they knew, approximately three quarters answered each question correctly. Although specific knowledge of the program was not the norm among non-beneficiaries, it appears that approximately one-third were aware of specific program features.

Thus, contamination of the control group due to expectations is a real possibility, although it may be limited to only a fraction of the households. The possible effects of such contamination are themselves ambiguous, however. Perhaps the largest influence of this type of contamination would be seen in school enrollment. If there was an expectation that households would participate in RPS (with its eligibility requirements around schooling) 1 or 2 years down the road, their incentives for current schooling decisions would change. There may be an inter-temporal substitution effect, in which households decrease school enrollment in the current period while it is unsubsidized, or there may be complementarities between current and future schooling (if it is costly to go back to school), raising current enrollment. There also may be incentives to hold children back so that they do not reach fifth grade before the program begins, although this is less clear in cases where there were younger siblings who would be eligible. Clearly, the sum of these possible effects on schooling decisions is ambiguous. It is even less clear whether and how other behaviors, for example, around care of children younger than age 5, might change in anticipation of the program. Unfortunately, we can bring little additional evidence to bear on these concerns.

We therefore emphasize the possibility that such contamination may confound some of the double-difference estimates. For example, in the case of school enrollments, double-difference estimates may tend to understate the effects of the program if control group expectations led to an increase in enrollment in control areas. Further, it may confound comparisons across subgroups to the extent such contamination was not uniform, for example, if it affected children of different ages differently. Therefore, when interpreting results such as those around educational decisions, we consider results that include as well as results that exclude older children who plausibly would have been affected differentially by the possible changes in behavior. Finally, in addition to double-difference estimates, the tables present single-difference estimates including reflexive differences within intervention *comarcas* over time, to permit an assessment of the extent to which such contamination might be affecting the double-difference estimates.

[27]Questions included statements such as: "To be a beneficiary of RPS, the household must take one-year old children to checkups every two months." This statement is false, as the requirement for children younger than 2 years old is monthly. Adato and Roopnaraine (2004) indicate that beneficiaries knew these requirements very well.

Contamination of the Control Group: Other Programs

A second possible source of contamination is attributable to other programs, referred to as substitution bias by Heckman and Smith (1995). This occurs when households in control *comarcas* participate in programs similar or complementary to RPS. The delay by 1 year before the control group was included may have increased the probability of such contamination, as it gave NGOs and others more time to adjust to the presence of RPS in intervention *comarcas,* possibly by pulling their programs out of intervention areas and increasing their efforts in control areas. While the RPS evaluation was sanctioned by the Government of Nicaragua and, therefore, plans were coordinated such that other large government programs (such as the *Programa de Atención Integral a La Niñez Nicaragüense* [PAININ]) avoided entering these areas over this period, other actors less tied to the government were not prohibited from doing so. To the extent other programs are not simply reacting to the evaluation itself, this design offers a more reasonable counterfactual compared to one in which no other program was allowed to enter the area.

Potential contamination by other development programs providing services to households was monitored in the annual household surveys. These data allow us to calculate the percentage of households in each *comarca* that were benefiting from an array of possible programs and services, and to determine whether or not RPS was the provider. Services provided by RPS fall into three of the dozen categories listed, shown in the top panel of Table 3.4: (1) provision of school supplies (via the education supplies transfer), (2) promotion and monitoring of child growth and development, and (3) food support to families. Table 3.4 shows that the percentages of households receiving these services from RPS increased dramatically between 2000 and 2001 and, with the exception of food support to families, continued to rise in 2002.[28] Receipt of these services by intervention-area households from non-RPS providers fell from 2000 to 2002, suggesting that other providers were crowded out by RPS. This decline was particularly sharp for the provision of school supplies but less so for the two other services, which started at much lower levels. This crowding out effect seems also to have extended to control areas, as the percentage of households receiving school supplies there declined from 35 percent in 2000 to 11 percent in 2001, rebounding in part to 20 percent in 2002. We are unable to determine whether these similarly sized parallel changes in the percentage benefiting from non-RPS provided school supplies in intervention and control areas were due to crowding out or simply a regional contraction of this program by the Ministry of Education.

In addition to the services provided by RPS, we also examine the percentages of beneficiaries for nine other kinds of programs that, while not actual components of RPS, may influence some of the same outcomes considered in the evaluation of RPS, such as household expenditures, primary school enrollment, or health-care and nutritional status of children. Except for school feeding, all of these other programs were operating at low levels in these marginal, rural areas, and were approximately equally distributed across intervention and control *comarcas.* Furthermore, the *comarca* level data show that, if anything, the density of these other programs is generally lower in control *comarcas.*

A notable trend in both intervention and control areas was the substantial increase in the percentage of households whose children benefited from school feeding. In 2000, 11–13 percent of households benefited from

[28]We are unable to explain why there was such a sharp drop in the "food supply" category in 2002 and suspect there was a problem of interpretation in training or in the field for that question in that survey round.

Table 3.4 Percentage of households benefiting from development programs and services

	2000		2001			2002		
	Control	Intervention	Control	Intervention		Control	Intervention	
	Non-RPS	Non-RPS	Non-RPS	Non-RPS	Including RPS	Non-RPS	Non-RPS	Including RPS
Provision of school supplies	34.6	27.6	10.7	6.4	45.3	20.2	10.8	57.4
Promotion and monitoring of child growth and development	3.1	6.1	2.9	2.4	42.4	4.3	1.4	53.8
Food support to families	4.4	3.1	5.4	3.5	90.1	3.5	2.8	11.8
School feeding	11.0	12.7	39.4	42.6	—	50.2	50.1	—
Adult literacy and education	4.0	2.5	3.4	1.8	—	2.1	1.1	—
Health and environmental training	4.3	6.9	4.1	9.9	—	3.4	2.3	—
Health outreach/preventive health training	4.0	5.1	3.4	8.8	—	2.5	2.4	—
Provision of money without obligation to repay	0.2	0.3	0.2	0.0	—	0.6	0.7	—
Provision of credit	4.0	6.2	6.7	2.7	—	6.6	2.3	—
Technical assistance and training	3.7	4.4	3.7	5.1	—	2.8	2.3	—
Provision of seeds or fertilizer for agricultural use	10.1	8.5	4.0	3.4	—	7.7	4.0	—
Community development training	2.8	3.0	1.2	1.6	—	0.6	1.1	—

Source: Nicaraguan RPS evaluation data.

Notes: Analysis based on 706 observations in the intervention group and 653 observations in the control group in each year.

school feeding, provided mainly by the Ministry of Education. By 2002, however, this had increased nearly fivefold, to 50 percent, in both intervention and control areas. The *comarca* level data show that school feeding programs were active in all control and all but two intervention *comarcas* by 2002, as the World Food Programme expanded its presence in these areas. Because the increase was identical across intervention and control *comarcas,* however, this trend can be controlled for in part using the double-difference methodology.[29] If RPS led to crowding out of school supplies services in control areas as well as intervention areas, it is possible the estimated double-difference effects are upward biased (relative to a situation in which RPS did not lead to a decline in control group services), although such bias is unlikely to be severe because the changes are similar in the two groups. In addition, school feeding, the only type of service other than those provided by RPS that changed substantially, increased equally in both intervention and control *comarcas.* We conclude that there is little evidence of substitution bias in the control group and therefore it is unlikely to be an important source of contamination biasing the estimation.

[29]We are unaware of any estimates for the effects of school feeding in Nicaragua (or Central America); such estimates would provide an opportunity to adjust the double-difference estimates, at least crudely.

CHAPTER 4

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

The IADB loan document contained a set of specific indicators and numerical goals for the first year of operations as a condition for approval of the second tranche of the loan financing RPS (see Appendix C), and these were used in the sample size calculations described in the first section of Chapter 3. Although the program achieved most of these goals and the loan extension was approved in 2003, we do not emphasize these specific indicators in our analysis because (1) they represent only a subset of the possible effects of the program, (2) they are somewhat arbitrary because there was little information on which to base numerical goals during the design stage, and (3) they largely measure process or inputs and did not capture the underlying objectives of the program such as improved human capital. While the trend by development institutions to embed project evaluations in projects from the start is positive, we would caution against overspecifying the goals or holding to them too rigidly, particularly when little evidence of similar programs exists.[30] Hence, although 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 younger than age 5.

Household Expenditures

Despite strong growth in the late 1990s and in 2000 when gross domestic product grew nearly 13 percent, Nicaragua had the lowest per capita GDP in Central America in 2000. Subsequent slow growth, however, meant that income per capita was essentially stagnant over the 2000–02 period (World Bank 2004). At the outset, 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. 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. Moreover, the majority of the remaining households, or "non-poor" 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.

[30]During Phase I, RPS and IADB renegotiated the indicators, indicating they were not rigidly applied in this case.

Table 4.1 RPS average effect on annual total household expenditures

Survey round	Intervention	Control	Difference
Follow-up 2002	21,327	18,268	**3,059***
	[706]	[653]	(1,633)
Follow-up 2001	22,194	17,810	**4,384*****
	[706]	[653]	(1,323)
Baseline 2000	20,725	20,483	242
	[706]	[653]	(1,288)
Difference 2001–2000	**1,469****	**–2,673*****	**4,142*****
	(706)	(955)	(1,174)
Difference 2002–2000	602	**–2,215****	**2,817****
	(798)	(956)	(1,230)

Source: Nicaraguan RPS evaluation data.

Notes: Standard errors correcting for heteroskedasticity and allowing for clustering at the *comarca* level are shown in parentheses (StataCorp 2001). Analysis based on 706 observations in the intervention group and 653 observations in the control group in each year (shown in brackets). Bold indicates significance at *** the 1 percent level, ** the 5 percent level, and * the 10 percent level.

Table 4.1 shows the average effect of RPS on nominal annual total household expenditures.[31] The control column shows that in 2000, before the program began, average annual total household expenditures in the control areas were C$20,483 (US$1,594). A year later, expenditures had declined by C$2,673 (US$208) to C$17,810 (US$1,386) although by 2002 they had recovered slightly, reaching C$18,268 (US$1,422). The intervention column shows preprogram annual total household expenditures of C$20,725 (US$1,613) in the beneficiary areas. After 1 year of operation, annual total household expenditures had risen by nearly C$1,500 (US$117) to C$22,194 (US$1,727), but then fell back to C$21,327 (US$1,660) in 2002, only C$602 (US$47 or about 3 percent) above the level reported in the 2000 baseline.

As shown in the right-hand "Difference" column, before the program began, annual total household expenditures in 2000 were very similar in the intervention and control areas (differing by only C$242 or US$19). One year later, however, that small initial difference had grown to C$4,384 (US$341), and the net average increase, or double-difference estimate of the effect of the program between 2000 and 2001, was C$4,142 (US$322). The estimated effect of the program declined to C$2,817 (US$219) in 2002,[32] however, with a decline in intervention areas of nearly C$1,000 (US$78) and the slight recovery in control areas.

For comparison, the average value of annual cash transfers for intervention *comarca* households in the evaluation survey over the period was C$3,500 or US$272 (as only five of the scheduled six transfers were

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

[32]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, absolute measures are presented to facilitate interpretation and comparison with the nominal transfer amounts.

made in each year). Thus the average estimated impact on expenditures was above average transfers in the first year, but below them in the second year. In both cases, however, it was of the same order of magnitude and in neither year can we reject the hypothesis that the estimated effect is significantly different from the average annual transfer amount of C$3,500. The C$700 (US$54) difference between estimated effects and transfer amounts may in part reflect imprecision in measuring annual household expenditures (IFPRI 2001b).

Nevertheless, we can conclude that beneficiary households are, on average, spending a large proportion of their transfers on current expenditures, although the fraction spent appears to have been smaller in the second year, perhaps in part because they were less critically needed as the area underwent a partial recovery compared to 2001. Comparing across the extremely poor, poor (but not extremely poor), and non-poor in the sample, we find that the largest estimated double-difference effect was for the extremely poor households (over C$3,800 or US$296 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 non-poor but held steady for the extremely poor.[33] 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. Although not designed as a traditional safety net program in the sense of reacting or adjusting to crises or shocks, the economic difficulties experienced in these communities allowed RPS to perform like a traditional safety net, enabling households to maintain expenditures during the downturn.

The substantial decline in expenditures in the control areas demonstrates the importance of having a baseline and 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 the insignificant C$602 or US$47 difference over time in the intervention group in the second to bottom row of the first column of Table 4.1).

The RPS effects on per capita annual total household expenditures are shown in Table 4.2. Average per capita expenditures in 2000 were just over US$300 for the sample, compared to a national average of nearly US$500 in 2000, again indicative of the relative deprivation of the program areas. The patterns seen for total expenditures, combined with little change in house-

[33]The drop in expenditures in the control group was not due to changes in household size or family composition, as we show in Table 4.2, which reports per capita expenditures. Another possibility is that there are biases in the reporting of expenditures. For example, in control areas it is possible that nonbeneficiaries who had learned about the program understated their expenditures in an effort to appear more in need of the program. However, at this stage, the program was being implemented using predominantly geographical targeting and being more or less poor would not have affected their 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 in 2002 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 significantly and substantially larger than the transfer.

Table 4.2 RPS average effect on per capita annual total household expenditures

Survey round	Intervention	Control	Difference
Follow-up 2002	4,346	3,378	**968****
			(385)
Follow-up 2001	4,462	3,194	**1,268*****
			(285)
Baseline 2000	4,021	3,738	283
			(290)
Difference 2001–2000	**441*****	**–544*****	**986*****
	(146)	(172)	(223)
Difference 2002–2000	**325****	**–360****	**686*****
	(153)	(168)	(224)

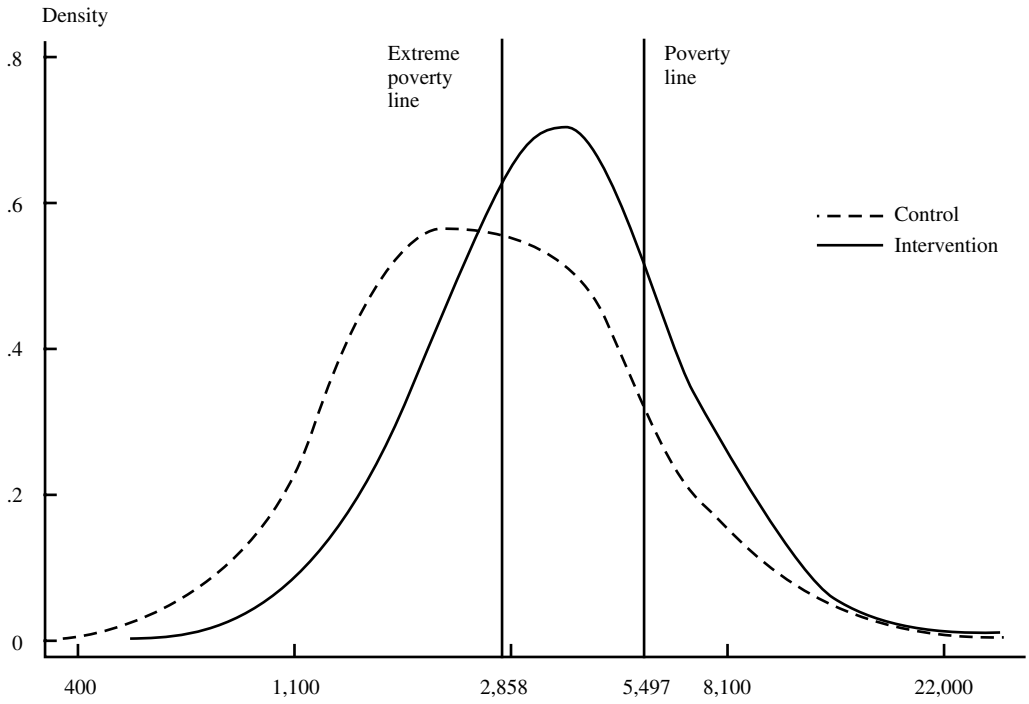
See notes for Table 4.1.

hold size, yield a significant increase in per capita annual total household expenditures within the intervention group, as well as significant declines for the control group. Taken together, these changes produce average program effects of C$986 (US$77) in 2001 and C$686 (US$53) in 2002. In 2002, the average effect of the RPS was nearly 18 percent of initial per capita annual household expenditures. This average, however, masks substantial differences among the extremely poor, poor, and non-poor in the sample. For the extremely poor, the estimated average effect is C$819 (US$64) for 2002 and represents 30 percent of initial per capita expenditures.

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 –22 percentage points and in 2002, –16 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 measures of current poverty. In Figure 4.1, 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. The program has shifted the density function to the right (and decreased its spread) and 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,* large proportions of households remain extremely poor (32 percent) or poor or extremely poor (77 percent).

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$871 (US$68) in 2001 and C$640 (US$50) in 2002 (Table 4.3), and these averages again mask larger effects for the extremely 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, Adato

Figure 4.1 Density function of per capita annual total expenditures in 2002: Control versus intervention



and Roopnaraine (2004) present evidence 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 how widespread this practice is). Whatever the mechanism, 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. Although we do not directly test this hypothesis, these results point to a likely increase in calories consumed (at the household level), as seen in PROGRESA (Hoddinott and Skoufias 2005).

Table 4.4 shows how the significant changes in per capita food expenditures

Table 4.3 RPS average effect on per capita annual food expenditures

Survey round	Intervention	Control	Difference
Follow-up 2002	3,029	2,207	**822*****
			(258)
Follow-up 2001	3,175	2,122	**1,053*****
			(217)
Baseline 2000	2,760	2,578	182
			(180)
Difference 2001–2000	**415*****	**–456*****	**871*****
	(140)	(132)	(190)
Difference 2002–2000	**269****	**–371****	**640*****
	(119)	(140)	(182)

See notes for Table 4.1.

Table 4.4 RPS average effect on food shares (percentage)

Survey round	Intervention	Control	Difference
Follow-up 2002	70.0	66.4	**3.6*****
	(0.9)		
Follow-up 2001	70.6	66.8	**3.8*****
	(1.2)		
Baseline 2000	70.0	70.8	–0.8
	(1.5)		
Difference 2001–2000	0.6	**–4.0*****	**4.7*****
	(1.2)	(1.3)	(1.7)
Difference 2002–2000	0.0	**–4.4*****	**4.5*****
	(1.0)	(0.9)	(1.3)

See notes for Table 4.1.

affected the share of food expenditures in the household budget. RPS produced a significant net increase of 4.5 percentage points in the food share in 2002 and this change was roughly the same across poverty groups. The percentage of the budget spent on food changed little for households in the intervention group after implementation, remaining at approximately 70 percent, a level consistent with the relatively high poverty rates for this group when compared to the 1998 national average of 60 percent.

Although this net average effect on the food share is consistent with the program's success in promoting increased food expenditures, it comes not from an increase in the food share for beneficiaries but from a decline in the food share of the control group. What is somewhat surprising is that the food share was declining for control households during an economic downturn marked by decreased expenditures. For a relatively poor population such as this, one would expect the food share to increase as total expenditures declined, since in this and other populations, 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 (before the economic downturn) on a randomly selected subset of baseline households, food shares were substantially lower than they were in baseline interviews (IFPRI 2001b). Seasonality seems to play an important role in the food shares we observe, although, as emphasized earlier, 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 non-beneficiary 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 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 12.1 (SD 5.5) different items excluding alcohol and tobacco. On average, households in the intervention areas report that

Table 4.5 RPS average effect on the composition of food expenditures

Food category	2000 Average (C$)	2000–2002 Average effect on expenditures (C$)	2000 Average (%)	2000–2002 Average effect on food share (%)
Grains, potatoes, bread	7,049	649	48.7	–2.9
Beans	1,485	–137	11.3	**–3.7*****
Meat	1,185	**761*****	7.3	**2.2****
Milk	560	139	3.4	0.5
Fats	1,302	**699*****	9.2	**2.4*****
Fruits and vegetables	784	**554*****	5.3	**2.5*****
Alcohol and tobacco	99	19	0.5	0.1
Sweets	1,011	**348*****	7.8	0.4
Other	824	–69	6.4	**–1.7****

See notes for Table 4.1.

they were buying nearly four (of the 60) additional food types in 2002, a result that was similar across the poverty groups (results not shown).

Not only did the number of food items purchased increase, but their nutritional value did as well. By organizing the types of food into different categories, two conclusions can be reached (see Table 4.5). First, in absolute terms, expenditure on nearly all food groups increased with the program (and nowhere did it significantly decrease).[34] Second, nutrient-dense foods (including meats and fruits and vegetables) emphasized in the health education sessions and 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 and oils as well, and were made possible by declines in two staples (grains and beans) that represented more than 50 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 nutritional improvements for children within these households is the subject of the fourth section in this chapter.

The majority of the additional expenditures induced by RPS were spent on food. A second key component of the program, however, is education. In 2002, the estimated average effect of RPS on educational expenditures was C$322 (US$25), slightly larger than the per student value of the educational supplies transfer (C$275 or US$21). These gains were concentrated in the extremely poor and poor households as well as in households with children ages 7–13 (where the estimated program effect was C$428 or US$33). Health-care expenditures actually decreased with RPS (–C$46 or –US$4), although this effect is significant only when the extremely poor are considered separately. This is consistent with the fact that the program provides many health-care 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.

The study also asked about other forms of expenditures related to investments at the household level, such as on household improvements, durable goods, and so forth;

[34]Information about alcohol and tobacco expenditures in these types of surveys is often unreliable; it is presented separately and we draw no conclusions from the reported information.

none of these showed significant changes. Finally, we examine expenditures on other specific non-food items including books, furniture, child clothing, remittances sent, lotteries, and parties, and find no significant program effects. Naturally, since total expenditures were flat while the percentage spent on food remained the same, it was unlikely that investments or expenditures like these would have changed very much. It is important to emphasize that the evidence indicates that households are indeed following the recommendations of the program; that is, they are spending most of their income from the program on current (food and education) expenditures.[35] This finding is somewhat weaker in the second year, however, where increased expenditures appear to be slightly smaller than the transfers. It is possible that any differences are reflected in increased savings (or increased leisure, discussed later), although we do not have the information to verify this. In either case, these patterns should be considered as RPS plans its exit strategy, and may have implications for the sustainability of the many effects described in this report.

Another obvious possible effect of the program on the household economy is on income generation, via labor participation and labor supply. In terms of "expenditures," one can think of this as an increase in the leisure consumed by household members. In short, the transfers may provide a disincentive for working. We explore this possibility examining first labor participation and second, hours of labor supplied.

The survey asked about labor force participation for primary and secondary activities in the previous week, and provides information about the number of hours worked (apart from domestic activities). All individuals older than 6 years of age were asked whether work was their primary activity in the previous week and, if not, why it was that 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. If the primary activity was not work, the individual was further prompted about other activities in the previous week. To assess labor participation and labor supply, we focus on adults in the household (child labor is examined in the second section of this chapter). Examining separately women and men age 15 and older, there were no significant changes in labor participation in the previous week (results not shown). More than 90 percent of men report having worked the week before, and there are no differences between periods or between intervention and control areas. For women, about 25 percent reported working in the previous week before the program. This percentage declined by about 10 percentage points in 2001 and 2002, in both intervention and control areas with the result that, as with the men, there were no program effects on the probability of their working.

Although the program appears not to have had an effect on labor participation, it is still possible that labor supply, that is, the number of hours worked, was affected, particularly for women who had a number of new responsibilities under the program. To explore this, we estimate the effect of the program on the total number of hours worked in the last week by female and male adults in the household, separately (Tables 4.6 and 4.7). There was a small decline (of about 3 hours) in reported hours worked in the last week by adult women living in intervention *comarcas,* consistent with increased responsibilities associated with the program. Nevertheless, the double-difference estimated effects, while negative, are small and insignificant. For men, however, there is evidence of a disincentive effect on hours

[35]Information on savings is not available so it is not possible to assess whether there was increased savings. Given the evidence on expenditures relative to the transfer size, however, any such increase was likely to be small.

Table 4.6 RPS average effect on hours worked last week: Women

Survey round	Intervention	Control	Difference
Follow-up 2002	9.0	13.5	**–4.5*****
			(2.0)
Follow-up 2001	8.1	12.8	**4.7****
			(1.9)
Baseline 2000	12.1	14.5	–2.4
			(1.8)
Difference 2001–2000	**–4.0*****	–1.7	–2.2
	(1.0)	(1.4)	(1.7)
Difference 2002–2000	**–3.1*****	–1.0	–2.1
	(0.9)	(1.5)	(1.7)

See notes for Table 4.1.

worked in the last week, of about 6 hours in both years. While not significantly depressing the labor supply of beneficiaries (which actually increased from 2000 to 2002) relative to before the program, it significantly reduced labor supply relative to their counterparts without the program.[36] The downturn in the economy appears to have led to increased labor supply by control group households. Crudely approximating the value of lost income from this decrease in hours worked at C$20 (US$1.50) per week (taking C$30 or US$2.30 as the daily wage) yields an amount approximately equal to the gap between the estimated increase in expenditures in 2002 and the average transfer. Thus, it seems that households took some of their benefits of the program in the form of increased leisure, although this was increased leisure relative to the control group not relative to before the program.

Last, we assessed the effect of the program on transfer or remittance behavior; cash transfers may crowd out remittances received by the household (Jensen 2003). The survey indicates that remittances are uncommon in this population, with fewer than 5 percent of respondents reporting having received remittances in the previous 12 months. For those who receive them, however, they can be substantial, averaging about C$4,000 (US$311) annually, approximately the same size as the average RPS transfer. Nevertheless, we find no evidence of crowding out as there are no significant program effects on the probability that remittances were received or average remittances received (results not shown), although this may be due, in part, to the characteristic that reported remittances were relatively uncommon in the sample.

Schooling and Child Labor

Education levels in Nicaragua are dismal. One third of adults older than 25 years of age have no formal education and another third have 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). Not surprisingly, these low enrollment rates are accompanied by a high incidence of child labor, particu-

[36]Adato and Roopnaraine (2004) found that adult males reported being able to work more often closer to home (e.g., on their own parcels), so it is possible that some of the decrease in labor hours supplied is due to decreased commuting time.

Table 4.7 RPS average effect on hours worked last week: Men

Survey round	Intervention	Control	Difference
Follow-up 2002	65.9	71.4	**–5.5***
			(3.1)
Follow-up 2001	63.1	69.2	**–6.1****
			(2.7)
Baseline 2000	62.6	62.5	0.1
			(4.0)
Difference 2001–2000	0.5	**6.7*****	**–6.2****
	(1.8)	(2.3)	(2.9)
Difference 2002–2000	**3.3***	**8.9*****	**–5.5***
	(1.6)	(2.7)	(3.1)

See notes for Table 4.1.

larly for boys. In 1998, 27 percent of boys ages 10–14 in rural areas were working an average of 30 hours a week (World Bank 2001). 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 72 percent.[37] 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 economic well-being. Figure 4.2a shows enrollment (or matriculation) rates by age for the target sample in 2000. For the targeted children, enrollment peaked at just above 80 percent for 9- and 10-year-olds but declined to 50 percent by age 13. Thus, even at its peak, there is 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. Another likely effect of the program would be to improve age-appropriate starts.

Figure 4.2b shows the enrollment rates for the same children by poverty group and by sex. These simple comparisons suggest that resources play a role in the decision to enroll children. Indeed, children living in households in the lowest per capita expenditure decile in the sample were more than one-third less likely to have enrolled than those living in the wealthiest decile (not shown). There was little difference between the enrollment rates for boys and girls. This accords with results from the qualitative study in which nearly all men and women interviewed said that they believed that education was just as important for boys as for girls (Adato and Roopnaraine 2004). 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.

[37]We use the survey data to assess enrollment and attendance, because administrative data are unavailable in control *comarcas.* In intervention *comarcas,* the data agree closely with separately collected administrative data from RPS, indicating that households do not appear to be overstating attendance since they know it is a program condition. Further, Adato and Roopnaraine (2004) found no evidence of falsification of school records sent to RPS, so the administrative data to which we compare the survey data appear to be accurate.

Figure 4.2a Enrollment in 2000 for 7- to 13-year-olds who had not completed fourth grade, by age

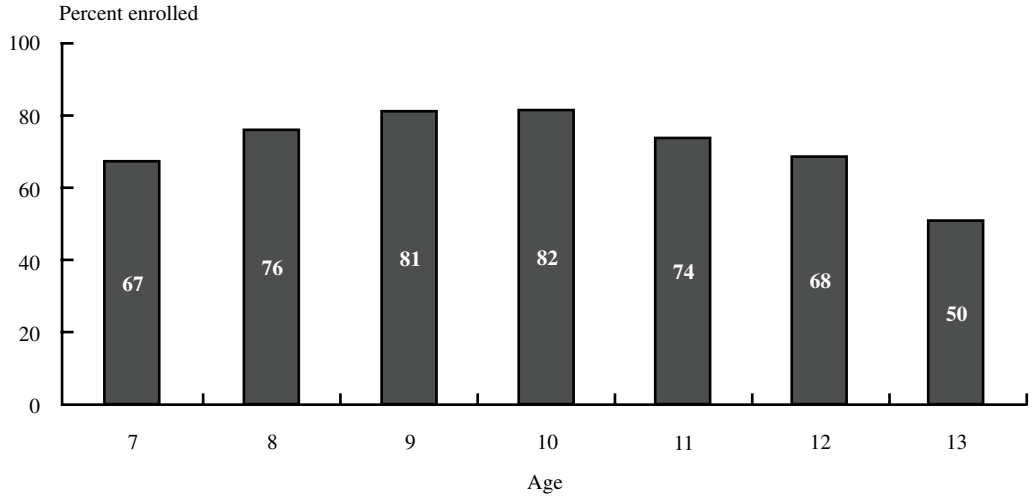
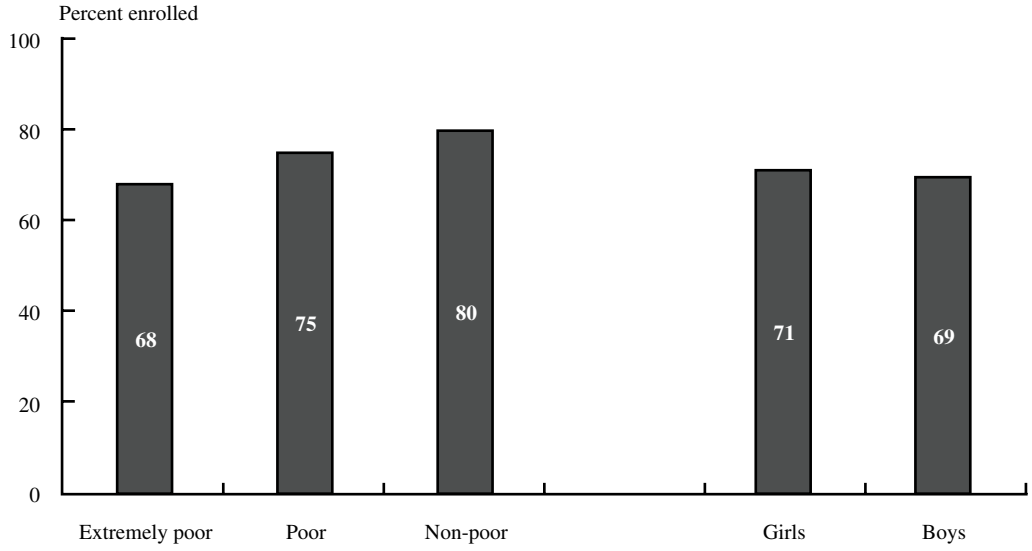


Figure 4.2b Enrollment in 2000 for 7- to 13-year-olds who had not completed fourth grade, by poverty group and by sex



Similar patterns emerge for current attendance in school in 2000, collected approximately 3 months before the end of the academic year and shown in Figure 4.3. A child is defined to be currently attending if he indicated he was still enrolled and had either missed 3 or fewer days in the past month or had missed more, but because of illness. As with enrollment at the start of the year, current attendance rises to ages 9–10 and declines thereafter. Dropout was common with the percentage of these children still in school toward the end of the academic year 9 percentage points lower than the percentage enrolled at the outset of the year. Attendance is also related to resources; again, it is evident that there was substantial room for improvement.

RPS induced a significant average net increase (2000–02) in school enrollment at the start of the school year of 18.5 percentage points in 2001 and 12.8 percentage

Figure 4.3a Current attendance in 2000 for 7- to 13-year-olds who had not completed fourth grade, by age

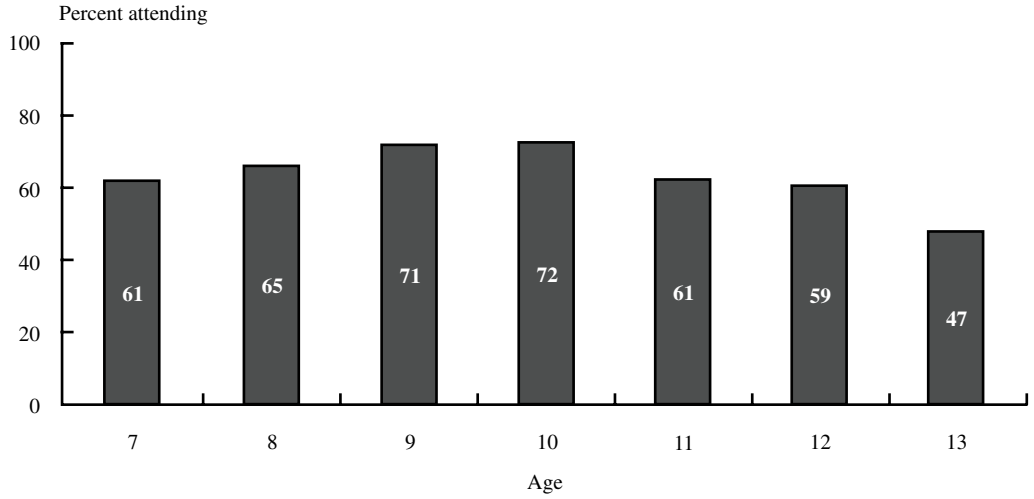
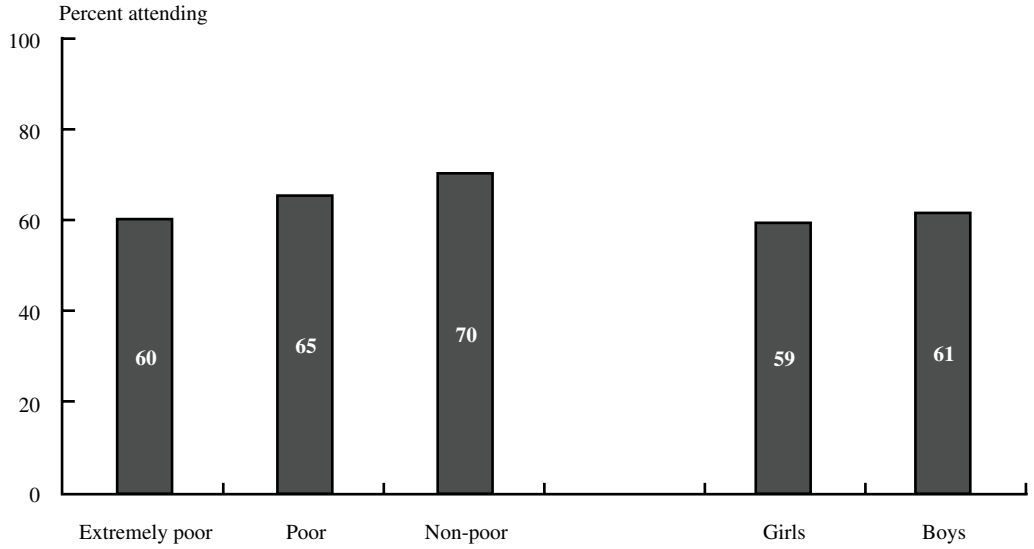


Figure 4.3b Current attendance in 2000 for 7- to 13-year-olds who had not completed fourth grade, by poverty group and by sex



points in 2002 (Table 4.8). Before the program, enrollment rates in intervention and control areas for this age group were very similar with approximately 72 percent of eligible children enrolling. With the program, enrollment rose to 92.7 percent in 2002. Enrollment in the control group also increased, however, by 7.6 percentage points. Even with that increase (which we discuss later), however, the transfers proved to be a huge additional stimulus. For comparison with a similar, although not identical program, double-difference estimates of changes in enrollment attributable to PROGRESA were less than 5 percentage points for primary school students (largely because enrollment in primary school in Mexico was already high) and approximately 12 percentage points for grades 6 through 8 (Schultz 2004).

To examine what underlies the average effect of 12.8 percentage points on enrollment, we consider the effect of the program

Table 4.8 RPS average effect on enrollment, children age 7–13 in first to fourth grades

Survey round	Intervention	Control	Difference
Follow-up 2002	92.7	79.2	**13.5*****
	[795]	[797]	(2.6)
Follow-up 2001	93.2	74.0	**19.2*****
	[821]	[773]	(3.1)
Baseline 2000	72.3	71.6	0.7
	[851]	[757]	(4.1)
Difference 2001–2000	**20.9*****	2.4	**18.5*****
	(3.1)	(2.1)	(3.7)
Difference 2002–2000	**20.4*****	**7.6*****	**12.8*****
	(3.5)	(2.6)	(4.3)

Source: Nicaraguan RPS evaluation data.

Notes: Standard errors correcting for heteroskedasticity and allowing for clustering at the *comarca* level are shown in parentheses (StataCorp 2001). Analysis based on all children 7–13 years old who had not completed grade 4 in 706 households in the intervention group and 653 households in the control group in each year (number of children shown in brackets). Bold indicates significance at *** the 1 percent level, ** the 5 percent level, and * the 10 percent level.

by age. The results are shown in Figure 4.4a, in which the bottom, dotted portion of each column shows the initial situation described earlier (Figure 4.2a), and the top, unshaded, portion is the double-difference estimated average program effect. In all cases, this effect was positive (although it was not statistically significant for ages 10–12). By 2002, enrollment rates in the intervention areas had risen to 90–100 percent for those ages 7–12, and no longer varied as much by age.[38]

Figure 4.4a 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, particularly 13-year-olds. 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 4.4b presents results for enrollment by poverty group and by sex. Clearly, the extremely poor and poor are benefiting most—the relationship between enrollment and per capita expenditures largely has been erased. The effects for boys and girls were the same.

Enrollment does not guarantee that a child will continue in school throughout the school year, nor does it mean that he or she attends school regularly. To continue receiving the education transfers, RPS required

[38]Although these bar charts are a convenient way of summarizing the program effects, it is not possible to interpret the sum of the two parts of each column as the enrollment rate with the program. This is evident, for example, in the subgroup of 9-year-olds, for whom the sum is an implausible 100 percent.

Figure 4.4a RPS average effect (2000–02) on enrollment for 7- to 13-year-olds who had not completed fourth grade, by age

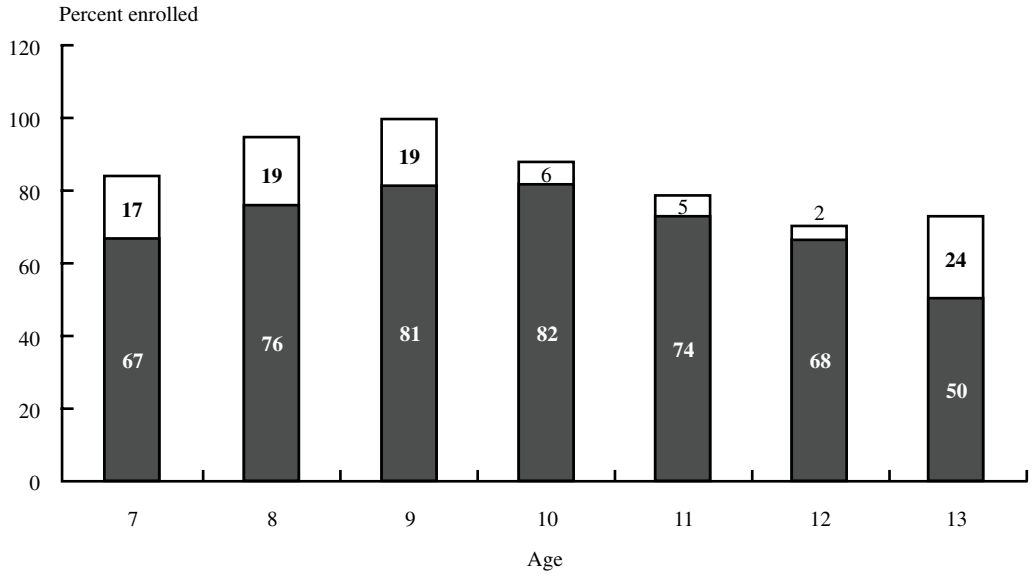
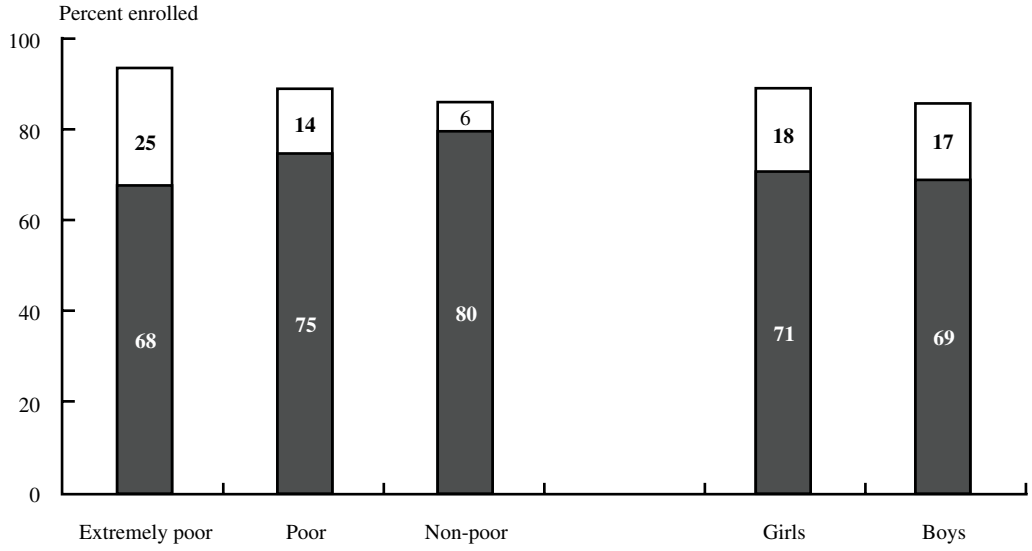


Figure 4.4b RPS average effect (2000–02) on enrollment for 7- to 13-year-olds who had not completed fourth grade, by poverty group and by sex



Notes: Double-difference estimated effects in top portion of bars. Bold indicates significance at the 10 percent level.

that no enrolled student have six or more unjustified absences in a 2-month period (Table 2.2). The effect of the program on current attendance was even larger than that on enrollment, with an average program effect of 20 percentage points for children ages 7–13. The effect was significant for all age groups except 12-year-olds. As with enrollment, the extremely poor and poor benefited the most (Figure 4.5b). Nonetheless, the non-poor also appear to have experienced gains in current attendance (although

given the small sample size these are less precisely estimated). Boys benefited slightly more than girls. There have been positive effects even for those children who were attending school prior to the program, as they are now attending more regularly.

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 Phase I, 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 percentage points in enrollment in grades 1 to 4 between 1999 and 2000, before the program. The increase was nearly 18 percentage points between 2000 and 2001, far higher than what occurred in the rest of the country during that period.

Although schools were generally available in RPS program areas as a result of the targeting described earlier, 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 also 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.[39] 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, although 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. For schooling outcomes, then, it is particularly important to underscore that we interpret the estimated effects as the combined effect of demand- and supply-side components of the program. Changes in control group enrollment call into question the validity of the experiment for the reasons outlined in Chapter 3. The observed increase of 7.6 percentage points was greater than the national rural average and appears to have been the net effect of several factors possibly contaminating the controls, including (1) increases in school feeding in the area (described in the fourth section of Chapter 3), (2) possible crowding out at the school level, (3) improvements in supply as a result of the program, and (4) changes in expectations in the control group. The expansion of school feeding may have spurred enrollment rates (although there is little reason to think the effects would not have been equal in intervention and control *comarcas*). In a small number of areas, individual schools served students in both intervention and control *comarcas.* This is problematic for the estimated effects because possible crowding in schools may have led individuals in the control group to be discouraged from attending (although given the small number this is unlikely to have been a large effect). Offsetting this possible, but likely small, effect, however, is that RPS induced an increase in the supply of teachers and grades offered in these municipalities (as described earlier).

In the fourth section of Chapter 3, we argued that the potential effects of expectations on control group schooling decisions

[39]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).

Figure 4.5a RPS average effect (2000–02) on current attendance for 7- to 13-year-olds who had not completed fourth grade, by age

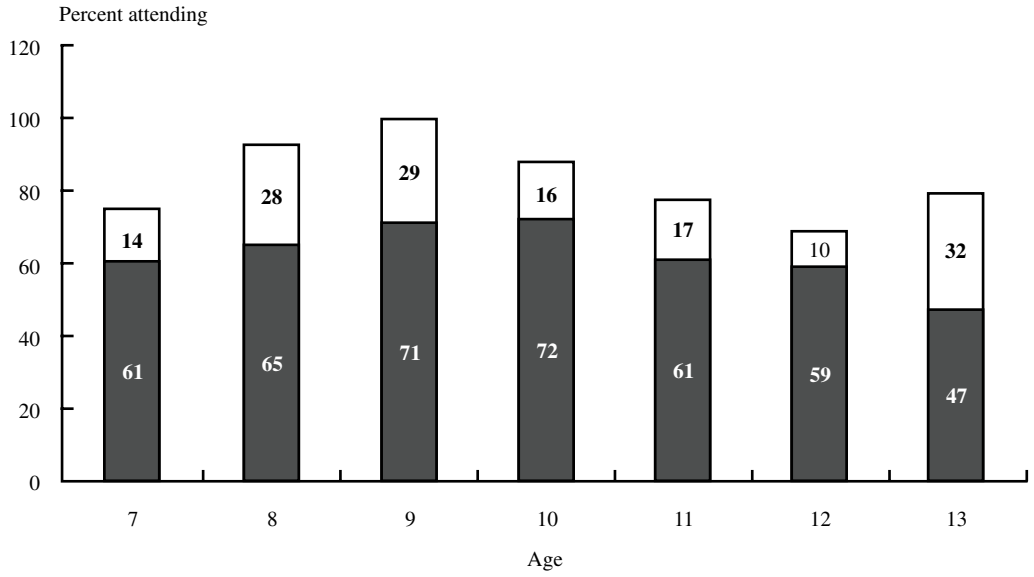
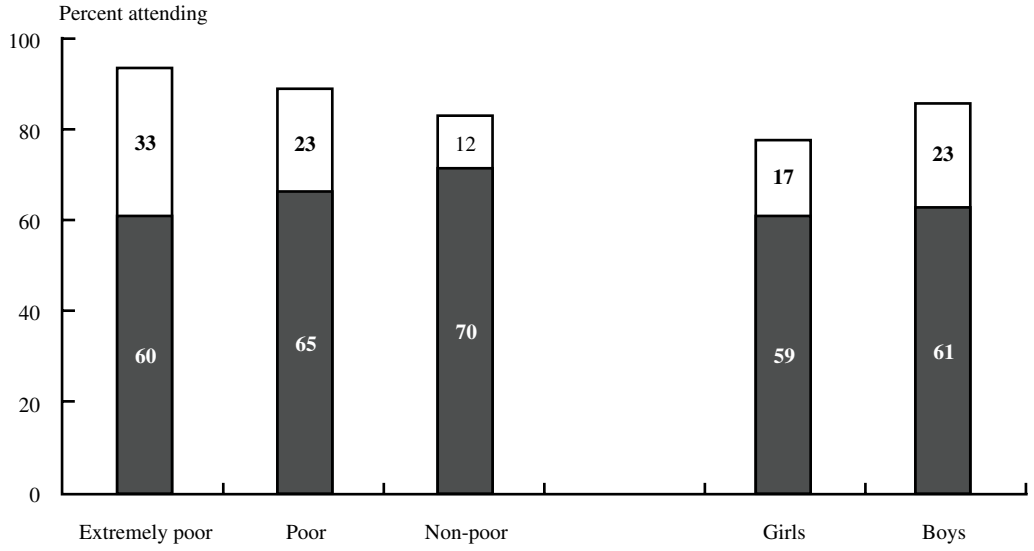


Figure 4.5b RPS average effect (2000–02) on current attendance for 7- to 13-year-olds who had not completed fourth grade, by poverty group and by sex



Notes: Double-difference estimated effects in top portion of bars. Bold indicates significance at the 10 percent level.

were ambiguous. Increasing enrollments, however, suggest that these effects may have been positive, on average. Enrollments increased in the control group for every age group except 13-year-olds, with the consequence that reflexive first-difference effects over time are greater than the double-difference estimator for every age group younger than age 13. As a result, the largest estimated double-difference effect was for the oldest eligible age children, 13-year-olds, because there were no increases in

Table 4.9 RPS average effect (2000–02) on school advancement, children age 7–13 in first to fourth grades, by starting grade in 2000

Grade	Grade 1	Grade 2	Grade 3	Grade 4
Intervention	92.7	89.7	84.7	86.8
	[330]	[156]	[131]	[114]
Control	84.8	84.3	76.2	77.5
	[310]	[178]	[105]	[89]
Difference	**7.9*****	5.5	8.5	**9.3****
	(3.0)	(3.8)	(5.5)	(5.3)

Source: Nicaraguan RPS evaluation data.

Notes: Standard errors correcting for heteroskedasticity and allowing for clustering at the *comarca* level are shown in parentheses (StataCorp 2001). Analysis based on all children 7–13 years old who had not completed grade 4 in 2000 and are in the sample in 2002. (Number of children shown in brackets.) Bold indicates significance at *** the 1 percent level, ** the 5 percent level, and * the 10 percent level.

enrollments in the control group for them. If parents were acting strategically in anticipation of the program, they would have been less likely, if anything, to change decisions regarding these older children who would be ineligible in the following year. We conclude that to the extent contamination exists it is leading to an underestimation of the effects of RPS on schooling.

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 continuing in the control areas. Overall, the effect is significant and shows an average improved progression rate of 7.3 percentage points. Table 4.9 shows the results by starting grade in 2000. Although less precisely estimated for these smaller subgroups, we still find significant effects for those starting in grades 1 and 4. An unanticipated benefit of the program was the large effect on those making the transition from third and fourth to fifth and sixth grades. This effect was unanticipated, because enrollment in the fifth grade or higher was not one of the conditions for continuing to receive the education transfer. Unfortunately, it is not possible at present to determine exactly why this is occurring. It may be simply an income effect of the program. It may also be due to potentially long-lasting changes in attitudes toward education. Finally, it may merely reflect confusion about the program requirements on the part of beneficiaries, although this seems unlikely given that Adato and Roopnaraine (2004) find that beneficiaries understand them well. Examining continuation rates for all four grades at once for the different poverty groups (Table 4.10) shows that, as with the other measures, there is a tendency for the largest effects of the program to be concentrated among the poorer households. Finally, as with the other results, the effects were similar for boys and girls.

Among those not enrolling, economic reasons were cited in nearly half the cases, and work was specifically cited in about 10 percent of the cases. For those who dropped out during the year, work was cited as the

Table 4.10 RPS average effect on school advancement, children age 7–13 in first to fourth grades (2000–02), by poverty group

Group	Extremely poor	Poor	Non-poor
Intervention	87.9	92.0	88.8
	[322]	[311]	[74]
Control	82.4	82.4	82.4
	[392]	[216]	[98]
Difference	**5.5***	**9.6*****	6.4
	(3.0)	(3.4)	(5.9)

See notes for Table 4.9.

main cause 20 percent of the time. The need to work plays a role in schooling decisions, although apparently not the dominant one. We now examine whether the implementation of RPS reduced child labor for the target school-age children.

Parents in beneficiary communities unanimously expressed opposition to child labor, although the realities of life often require it (Adato and Roopnaraine 2004). We consider a child to be working if work was a primary or secondary activity, with positive hours worked. Nearly all 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 likely reflects the likelihood that child labor increases household expenditures, our measure of well-being. Boys were substantially more likely to report working. By age 13, only one quarter of the girls reported working compared to nearly 60 percent of the boys. Conditional on working, boys also worked longer hours, averaging 25 hours per week compared to 16 hours for girls. Given the questionnaire's orientation toward economically productive activities aside from housework, the difference between boys and girls' reported work may reflect in part the underreporting of girls' domestic responsibilities.

For every age group, the percentage of children working was lower after the program. When we combine those children ages 7–13 who had not completed the fourth grade, the double-difference estimator shows a 4.6 percentage point decrease in the percentage of children working in 2001 and a 5.6 percentage point decrease in 2002 (Table 4.11). In both the intervention and control areas, the percentage of children working declined significantly in 2001. This may reflect the general economic downturn in the program area with possibly suppressed labor demand. Finally, while the effect on education outcomes was the same for boys and girls (as described earlier), the effect on reported work for boys was more than twice as large as for girls.[40] As in the case of

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

Table 4.11 RPS average effect on working, children age 7–13 in first to fourth grades

Survey round	Intervention	Control	Difference
Follow-up 2002	5.4	12.0	**–6.6*****
	[795]	[797]	(2.2)
Follow-up 2001	5.0	10.6	**–5.6*****
	[821]	[773]	(1.5)
Baseline 2000	12.7	13.7	–1.0
	[851]	[757]	(2.5)
Difference 2001–2000	**–3.1***	**–7.7*****	**–4.6***
	(1.7)	(2.2)	(2.7)
Difference 2002–2000	**–7.3*****	–1.7	**–5.6****
	(2.0)	(1.8)	(2.7)

See notes for Table 4.8.

PROGRESA (Skoufias and Parker 2001; de Janvry et al. 2004; Schultz 2004), RPS has shown that not only is schooling increased, but also work can be reduced through subsidized schooling.

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 an RPS health-care provider. All beneficiaries agreed that their children's health improved under the program and they were greatly appreciative of the preventive health-care services (Adato and Roopnaraine 2004). Before the program, just over 70 percent of children younger than age 3 had been taken for a well-child check-up in the previous 6 months. In 2001, RPS produced a significant average increase of 16.3 percentage points in the percent of children younger than age 3 whose parents had taken them for a well-child visit in the past 6 months, but an increase of only 8.4 percentages point in 2002 (Table 4.12).[41] This deterioration from 2001 to 2002 in the estimated effect was largely due to continued improvement in this indicator in the control group and reflects only a slight decline in intervention areas (from 95.8 to 92.7 percent). Indeed, during the period 2000–02, the percentage taken to a health-care visit in the control group increased by 10.5 percentage points.

The RPS effect on the percentage taken to a health-care provider and weighed during the visit in the last 6 months, another key aspect of the VPCD program, was even larger (Table 4.13). As noted earlier, there was an increase in the control group from 2000 to 2002, of 15.2 percentage points, but this change was swamped by a 28.3 percentage point change in the intervention group, leading to a double-difference estimated average effect of 13.1 percentage

[41]We focus on children ages newborn to 3 years to cover the most vulnerable period in a child's development, as discussed in the fourth section of this chapter.

Table 4.12 RPS average effect on percentage of children age newborn to 3 years taken to health control in past 6 months

Survey round	Intervention	Control	Difference
Follow-up 2002	92.7	84.1	**8.6****
	[276]	[350]	(3.5)
Follow-up 2001	95.8	79.4	**16.4*****
	[336]	[371]	(5.2)
Baseline 2000	73.7	73.6	0.1
	[369]	[371]	(6.0)
Difference 2001–2000	**22.1*****	5.8	**16.3****
	(5.0)	(5.3)	(7.2)
Difference 2002–2000	**19.0*****	**10.5*****	8.4
	(4.8)	(3.6)	(5.9)

Source: Nicaraguan RPS evaluation data.

Notes: Standard errors correcting for heteroskedasticity and allowing for clustering at the *comarca* level are shown in parentheses (StataCorp 2001). Analysis based on all children newborn to 3 years old in 706 households in the intervention group and 653 households in the control group in each year (number of children shown in brackets). Bold indicates significance at *** the 1 percent level, ** the 5 percent level, and * the 10 percent level.

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

As with the effects for expenditures and schooling, average program effects for VPCD measures are larger among poorer households. Table 4.14 presents results by poverty group for the two indicators just discussed. It shows that average effects were much larger among the extremely poor and, at least in 2002, were not significantly

Table 4.13 RPS average effect on percentage of children age newborn to 3 years taken to health control and weighed in past 6 months

Survey round	Intervention	Control	Difference
Follow-up 2002	89.0	75.6	**13.5*****
			(4.2)
Follow-up 2001	92.0	67.4	**24.6*****
			(5.7)
Baseline 2000	60.7	60.4	0.3
			(8.3)
Difference 2001–2000	**31.3*****	**7.0***	**24.2*****
	(6.1)	(3.8)	(7.1)
Difference 2002–2000	**28.3*****	**15.2*****	**13.1***
	(6.3)	(4.2)	(7.5)

See notes for Table 4.12.

Table 4.14 RPS average effect on percentage of children age newborn to 3 years taken to health control and weighed in past 6 months, by poverty group

Group	Extremely poor	Poor	Non-poor
Taken to health control			
DD 2001–2000	**17.5***	**20.6****	6.6
	(9.0)	(8.8)	(9.4)
DD 2002–2000	**15.2***	6.5	–9.1
	(8.3)	(6.4)	(8.6)
Weighed			
DD 2001–2000	**29.9*****	**23.5*****	13.0
	(9.6)	(8.9)	(12.1)
DD 2002–2000	**18.8****	7.3	8.3
	(9.5)	(9.1)	(13.1)

See notes for Table 4.12.

positive for the poor or non-poor. Most of the non-poor 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 there were no differences by sex (IFPRI 2003).

In addition to these measures, a variety of other process indicators related to the VPCD visits for children younger than age 3 were examined, including whether the child had a health card, the child's weight was graphed on the health card, the child's health card was up to date, and the child was given vitamins in the last 6 months. All of these showed patterns similar to the ones just described (IFPRI 2003).

Even though the indicators agreed upon by RPS and IADB (Appendix C) did not include effects on children ages 3–5, the program and program requirements do include children in this age range. Overall, the results for all the indicators described for this older age group show an even greater effect than for those younger than 3 years old (IFPRI 2003).

It is important to emphasize that for most of the indicators considered, the control group also showed improvements over the period, although on a smaller scale. A possible explanation for this increase is that other providers brought health-care services into the areas not covered by the program (the program providers did not offer or deliver services to non-beneficiaries in Phase I) in response to the evaluation. In the fourth section of Chapter 3, however, it was demonstrated that there was no influx of other health-care programs into control areas over the period. A second possibility, unrelated to newly entering programs but still 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 centers. With the program, demand for such public services dropped. 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 households in the control group changed their behavior based on the expectations as discussed in the fourth section of Chapter 3. For these reasons, double-difference estimates may be downward biased, as evidenced by the fact that the reflexive first-differences over

Table 4.15 RPS average effect on percentage of children age 12–23 months with updated vaccination

Survey round	Intervention	Control	Difference
Follow-up 2002	71.4	69.4	2.0
	[91]	[121]	(6.0)
Follow-up 2001	81.9	72.8	9.1
	[105]	[114]	(7.1)
Baseline 2000	38.9	41.5	–2.6
	[139]	[123]	(9.2)
Difference 2001–2000	**43.1*****	**31.3*****	11.7
	(7.1)	(6.8)	(9.8)
Difference 2002–2000	**32.6*****	**28.0*****	4.6
	(7.2)	(8.5)	(11.0)

Source: Nicaraguan RPS evaluation data.

Notes: Standard errors correcting for heteroskedasticity and allowing for clustering at the *comarca* level are shown in parentheses (StataCorp 2001). Analysis based on all children 12–23 months old in 706 households in the intervention group and 653 households in the control group in each year (number of children shown in brackets). Bold indicates significance at *** the 1 percent level, ** the 5 percent level, and * the 10 percent level.

time are consistently larger for the health input indicators measured in this section.[42]

Vaccination for Children Ages 12–23 Months

A variety of indicators 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.[43] The 1998 Demographic and Health Survey indicates that coverage in rural areas of Nicaragua according to this measure was 68 percent; in the RPS baseline 2000 it was 40 percent, reflecting the relative poverty of the program areas.

RPS produced an insignificant average net increase of 4.6 percentage points in up-to-date vaccination levels between 2000 and 2002 (see Table 4.15). In contrast to many of the other results, there was little difference across poverty groups. Both intervention and control groups improved tremendously between 2000 and 2001 but suffered setbacks

[42]Another, related possibility is that RPS had a demonstration effect. Control and intervention *comarcas* are in some cases adjacent to one another. 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 for the program.

A final possibility is that respondents are overstating their attendance under an expectation that this may influence whether or not they receive RPS in the future. We have little direct evidence to confront these trends with, except that in the following subsection we find that vaccinations (which are recorded after examining the child's health card) also go up, and falsification of those cards seems rather unlikely.

[43]According to the Nicaraguan Ministry of Health guidelines, a child ages 12–23 months should have (at least) the following vaccines: (1) one dose of bacillus Calmette Guérin (BCG), (2) three doses of polio, (3) three doses of either pentavalent or diphtheria, pertussis, and tetanus (DPT), and (4) one dose of measles, mumps, and rubella (MMR). 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 younger than age 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.

between 2001 and 2002. Control areas increased 28.0 percentage points over the period, offsetting nearly all of the 32.6 percentage point gain in the intervention areas in the calculation of the double-difference. 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.

An obvious proximate reason for the increased coverage in the control group is their increased attendance at health-care controls described in the previous section. In addition, discussions with RPS revealed that increased coordination with the Ministry of Health in the distribution of vaccines to RPS health providers in these municipalities may have 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).[44] Once again, it is likely that the double-difference estimates are downward biased, and that the first-differences over time may more accurately reflect the effect of the program, even with the variety of measurement problems described in footnote 44. Given the municipality level trends, 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 younger than age 5 in survey households, measuring their height, weight, and hemoglobin.[45] 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 younger than age 5, as they were in PROGRESA (Behrman and Hoddinott 2005). We first define the indicators we use to measure child nutritional status and their interpretation (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 quickly, young children have high nutritional requirements. Unfortunately, the diet commonly offered to young children in developing countries to complement breast

[44] When contrasted with RPS administrative data on vaccinations for beneficiary children, which show coverage of nearly 100 percent, the coverage reported in Table 4.15 and calculated for other vaccine measures are substantially lower. There are a number of reasons for the discrepancy: (1) the analyses in this report 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 the overall coverage.

[45] The anthropometric indicators were calculated using EpiInfo v6.4c, eliminating all the observations that EpiInfo indicated had an error in measurement.

milk is of low quality (i.e., monotonous and with low energy content and nutrient density) and, as a result, multiple nutrient deficiencies are common. At the same time, young children are very susceptible to infections, because their immature immune systems are unable to provide adequate protection. 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 infections, including withholding of foods and breast milk, are common. Thus infection and malnutrition often 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[46] 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 sex-specific reference population of the National Center of Health Statistics of the United States of America/World Health Organization, referred to as "NCHS/WHO" (WHO Expert Committee 1995).

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 below the median of the international reference population NCHS/WHO. In general, this condition is the result of a 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 height-for-age. Weight-for-age deficit is defined as weight-for-age *z*-score (WAZ) more than two standard deviations below the median of the international reference population NCHS/WHO.

[46] *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.

Table 4.16 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
Costa Rica 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
Guatemala 1995	7,768	49.7	26.6	3.3
Guatemala 1998–99	3,591	46.4	24.2	2.5
Honduras 1987	3,244	37.2	20.6	1.7
Honduras 1991	5,961	36.3	18.0	1.5
Honduras 1993–94 (12–59 months)	1,875	39.6	18.3	2.0
Honduras 1996	1,307	38.9	25.4	1.4
Honduras PRAF 2000 (subnational)	5,563	52.8	27.7	1.3
Nicaragua 1993–94	3,546	22.5	11.0	1.9
Nicaragua 1997–98	6,497	24.9	12.2	2.2
Nicaragua RPS 2000	**995**	**39.7**	**14.0**	**0.6**
El Salvador 1988	2,039	29.9	15.2	—
El Salvador 1993	3,515	23.1	11.2	1.3
El Salvador 1998	6,523	23.3	11.8	1.1

Source: (http://www.who.int/nutgrowthdb), IFPRI (2001c), and Nicaraguan RPS evaluation data.

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

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 of 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. It is, however, 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/dL for children 6–59 months of age (UNICEF/UNU/WHO/MI 1999).

In 1965–67, Costa Rica had 27 percent stunting (not shown). By 1982, the prevalence had fallen to 8 percent and stabilized (see Table 4.16, HAZ). The case 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 1998–9, 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.

Malnutrition levels in Nicaragua have 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

Table 4.17 RPS effect on percentage of children under age 5 who are stunted (HAZ < –2.00)

Survey round	Intervention	Control	Difference
Follow-up 2002	36.5	41.7	–5.2
	[469]	[518]	(4.7)
Baseline 2000	39.8	39.5	0.3
	[512]	[483]	(4.9)
Difference 2002–2000	**–3.4****	2.2	**–5.5***
	(1.3)	(2.8)	(3.0)

Source: Nicaraguan RPS evaluation data.

Notes: Standard errors correcting for heteroskedasticity and allowing for clustering at the *comarca* level are shown in parentheses (StataCorp 2001). Analysis based on children newborn to 3 years in 706 households in the intervention group and 653 households in the control group in each year (number of children shown in brackets). Bold indicates significance at *** the 1 percent level, ** the 5 percent level, and * the 10 percent level.

Salvador (23–30 percent), and high in comparison to Costa Rica. Table 4.16 shows that before the program 39.7 percent of children younger than age 5 living in the RPS program areas suffered from retarded growth because of malnutrition or illness. This is 1.6 times greater than the national prevalence for 1997–98 and nearly 20 times greater than the statistically expected prevalence for a healthy population. The areas where RPS operates exhibited substantially higher malnutrition than the national average. This is attributable 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 or serious illness (0.6 percent in RPS with a WHZ more than 2 standard deviations below the norm). Finally, malnutrition increases with the severity of poverty within the RPS baseline 2000 sample, as well. The poorest 20 percent show the highest levels of stunting. Less poor households have lower malnutrition and the best-off households in the program areas do not show, on average, marked growth deficits, particularly the wealthiest 20 percent.

Table 4.17 shows the calculations of the percentage of children younger than age 5 who were stunted. Before the program, intervention and control areas showed similar high rates of stunting. After 2 years of operation, stunting declined in the intervention areas by 3.4 percentage points, and increased by 2.2 percentage points in the control areas. The average net effect of RPS was to reduce stunting prevalence by 5.5 percentage points.

The current nutritional situation, as measured by wasting, is not a major concern in Nicaragua or in the program areas and therefore it is not surprising that there was no substantial change as a result of RPS. As shown in Table 4.18, 0.2 percent of children under age 5 were wasted in the intervention and control areas in 2002.

With no evidence of wasting, weight-for-age is best interpreted as an alternative indicator of chronic malnutrition. Table 4.19 shows the effect of RPS on WAZ and, as expected, it is a pattern similar to that seen in Table 4.17.

Before RPS, 13.7 percent of the children younger than age 5 in the intervention areas were underweight for their age. Two years later, the prevalence of underweight children

Table 4.18 RPS effect on percentage of children under age 5 who are wasted (WHZ < –2.00)

Survey round	Intervention	Control	Difference
Follow-up 2002	0.2	0.2	0.0
			(0.3)
Baseline 2000	0.8	0.4	0.4
			(0.5)
Difference 2002–2000	–0.6	–0.2	–0.3
	(0.5)	(0.4)	(0.6)

See notes for Table 4.17.

had declined by 3.9 percentage points; at the same time, the prevalence increased by about 2 percentage points in the control areas. The net effect, then, was that RPS significantly reduced the prevalence of underweight children by 6.2 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 HAZ in Table 4.20, finding a net average improvement attributable to RPS of 0.13, although it is not significant, likely a result of the small sample.[47] As with many of the other indicators, the gains in nutrition also appear to be concentrated among the poor. The estimated double-difference effect of the program on height-for-age *z*-scores among the extremely poor is 0.22 and significant at the 10 percent level (not shown). In 2 years of operation, RPS has significantly reduced stunting in children younger than age 5.[48] 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-care visits—we claim only that child nutrition improved as a result of the program; without further analysis, we are unable to specify the specific channels through which this was achieved.

The 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 2 years, reduced it by 5.5 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 6–59 months of age 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 4.21, we show that the

[47] When based on the unbalanced panel sample of households, the double-difference estimate is 0.15 and is significant at the 10 percent level.

[48] 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 not present results separated by sex.

Table 4.19 RPS effect on percentage of children under age 5 who are underweight (WAZ < –2.00)

Survey round	Intervention	Control	Difference
Follow-up 2002	9.8	16.6	**–6.8****
			(3.0)
Baseline 2000	13.7	14.3	–0.6
			(2.7)
Difference 2002–2000	**–3.9****	2.3	**–6.2****
	(1.6)	(2.0)	(2.5)

See notes for Table 4.17.

program had an enormous effect on the percentage of mothers receiving ferrous sulfate for their children in the past 4 months, as measured in the anthropometric survey. The double-difference estimated average effect was 38.0 percent in 2002, despite a huge increase in the control group as well.

This apparent success in distributing iron supplements notwithstanding, however, after 2 years of operation, RPS had not succeeded in improving the grave situation on anemia—the double-difference estimator is positive, small, and insignificant (Table 4.22). Not surprisingly, the results for hemoglobin mirror those for anemia—there was no average program effect (Table 4.23).

Although Table 4.21 showed that twice as many children in intervention areas had received iron supplements in the last 4 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 antiparasite medicine during 2001, so it is likely that complete supplements were not received at each visit. Unfortunately, this is something that the 4-month reference period for the survey question on supplementation delivery would not reflect. 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 younger than age 5 in the intervention areas had not received a supplement in the past 4 months.

Another possibility is that children are deficient in other micronutrients, potentially limiting the hematological response to iron supplementation. Allen et al. (2002) failed

Table 4.20 RPS effect on HAZ for children younger than age 5

Survey round	Intervention	Control	Difference
Follow-up 2002	–1.63	–1.80	0.17
	[1.13]	[1.20]	(0.13)
Baseline 2000	–1.73	–1.77	0.04
	[1.12]	[1.17]	(0.13)
Difference 2002–2000	**0.10****	–0.03	0.13
	(0.05)	(0.07)	(0.09)

See notes for Table 4.17.

Table 4.21 RPS average effect on percentage of children age 6–59 months given iron supplement (ferrous sulfate) in past 4 months

Survey round	Intervention	Control	Difference
Follow-up 2002	79.8	37.9	**41.9*****
	[431]	[470]	(4.6)
Baseline 2000	24.8	20.9	3.9
	[475]	[446]	(6.0)
Difference 2002–2000	**55.0*****	**17.0*****	**38.0*****
	(5.1)	(3.0)	(5.9)

Source: Nicaraguan RPS evaluation data.

Notes: Standard errors correcting for heteroskedasticity and allowing for clustering at the *comarca* level are shown in parentheses (StataCorp 2001). Analysis based on children newborn to 3 years in 706 households in the intervention group and 653 households in the control group in each year (number of children shown in brackets). Bold indicates significance at *** the 1 percent level, ** the 5 percent level, and * the 10 percent level.

Table 4.22 RPS effect on percentage of children age 6–59 months with anemia

Survey round	Intervention	Control	Difference
Follow-up 2002	33.3	30.9	2.4
			(5.8)
Baseline 2000	32.7	33.0	–0.3
			(3.9)
Difference 2002–2000	0.5	–2.1	2.7
	(0.5)	(0.5)	(6.7)

See notes for Table 4.21.

Table 4.23 RPS effect on average hemoglobin for children age 6–59 months

Survey round	Intervention	Control	Difference
Follow-up 2002	11.2	11.5	–0.3
	(1.3)	(1.4)	(0.2)
Baseline 2000	11.4	11.4	0.0
	(1.4)	(1.5)	(0.1)
Difference 2002–2000	–0.2	0.1	–0.2
	(0.2)	(0.1)	(0.2)

See notes for Table 4.21.

to find an improvement in hemoglobin in Mexican children ages 18–36 months supplemented with iron over 12 months in a controlled experiment. They conclude 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.

Probably the most important factor in RPS, however, is whether children were actually ingesting the supplements. This latter possibility was pursued in the qualitative study. It found that mothers knew the supplements were important for their children's health. Most even said that they administer them, but in reality this appears not to be the case (or to be only sporadic). They offered a variety of reasons for not doing so after probing, however, including: children did not like the taste, it caused vomiting or diarrhea, and it stained their teeth (Adato and Roopnaraine 2004).

CHAPTER 5

Conclusions

This report has presented the main findings of a quantitative evaluation of a randomized community-based intervention, RPS, against its primary objectives. Where possible, we err on the side of assessing what are the short-term effects of the program in conservative manners, for example, by presenting intent-to-treat estimates and in the treatment of possible contamination and attrition. The estimates presented represent the effect of the program as a whole, in particular combining supply- and demand-side components. They also represent only the short-term effects of the program (after 1 or 2 years), although some of the outcomes examined are themselves long-run indicators. In many instances, for example, when assessing the effects on expenditures during what turned out to be an economic downturn, the critical importance of having baseline data and a control group for the evaluation was evident —without one it would have been next to impossible to make reliable assessments of the program effects. Even with one, it was at times difficult to make certain assessments of effects for some indicators since the evaluation did not exist in a vacuum, and a variety of actors not under the control of the program continued to operate in the program areas.

Overall, we found that RPS had positive (i.e., favorable) and significant double-difference estimated average effects on a broad range of indicators and outcomes. Where it did not, it was often because of similar, although smaller, improvements in the control group, which were likely to have been influenced in part by the program itself. Nearly all estimated effects were larger for the extremely poor, often reflecting their lower starting points (e.g., percentage of children enrolling 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.

During Phase I, RPS 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 non-beneficiaries 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$640 (US$50) in per capita annual food expenditures and an improvement in the diet of beneficiary households. Expenditures on education also increased significantly, although there was no discernible effect on other types of investment expenditures. Labor market participation was apparently little changed with the program over time, although there was an indication of slightly fewer hours worked by men in the last week, relative to the control group. 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 large average net increase on enrollment of 12.8 percentage points and an even larger effect of 20 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 7.3 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 5.6 points.

For child health care, RPS induced an average net increase of 16.3 percentage points in the participation of children younger than age 3 in VPCD but the effect was only 8.4 percentage points (and insignificant) in 2002 since participation by control group households increased substantially. The health-care services provided by the program, as measured by process indicators including whether the child was weighed and whether his or her health card was updated, improved even more. Participation by children ages 3–5 also increased substantially. Although 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 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 this substantial improvement in large part 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 younger than age 5. The net effect was a 5.5 percentage point decline in the number of children who were stunted. This decline was 1.7 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 lower rates of anemia.

In related work, Caldés and Maluccio (2005) analyze program and non-program costs for RPS, including costs for design, planning, and execution of the program in the evaluation areas and everywhere else it was operating (see Appendix A). While they were unable to separate out program costs pertaining to the households evaluated in this report, they present a number of findings relevant to this study. In particular, they found that administrative costs were a little less than 20 percent in the pilot phase, after taking out fixed costs associated with design, planning, and evaluation. These were higher than the costs for PROGRESA or PRAF (Caldés, Coady, and Maluccio 2004), although the difference appears to be due to RPS's complicated design (with an involved supply-side component that the other two programs did not have), pilot nature, and its small size, particularly in comparison to PROGRESA. Furthermore, RPS arguably had the most impressive effects (Rawlings and Rubio 2005).

Nearly half of the costs were due to a number of special program features that are hallmarks of conditional cash transfer programs, in particular targeting, monitoring and conditioning, and coordinating the health-care supply. Removing these costs would reduce 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 (Caldés and Maluccio 2005).

The findings presented here played an important role in the decision in late 2002 by the Government of Nicaragua and IADB to continue and expand this effective program as RPS–Mi Familia, Phase II.[49] In the expansion, the program was altered in three important ways. First, the health-care

[49]The program was transferred within the Nicaraguan government from FISE to the Ministry of the Family (Mi Familia) in Phase II.

services were expanded to benefit additional household members, in particular with prenatal services for pregnant women, vaccinations for 6- to 9-year-olds, and preventive health care for adolescents and for all women age 20 and older. Second, initially designed as a 3-year program with both demand- and supply-side components lasting for 3 years, the supply-side components of the program (in particular the health-care services and the teacher transfer) were extended in Phase II to last for a total of 5 years, such that in the final 2 years participants would receive supply-side benefits only. Third, based on evidence of the effectiveness of the program and cost considerations, the transfer size was reduced by one-third.

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 the plan to 2015 (World Bank 2003). RPS demonstrates that it is possible in Nicaragua, as it has been in Mexico and other places, to combine short-term reduction in rural poverty with improvements in human capital of children. The evidence from the evaluation strongly suggests that if the program were expanded elsewhere in poor rural areas of Nicaragua (as it was in 2003), it would be effective, although due to the caveats discussed throughout this report, we stop short of saying it would necessarily be equally as effective. Nonetheless, it could prove to be an important component of Nicaragua's overall poverty reduction policy.

A crucial question that the current evaluation was unable to answer is whether the effects documented here will persist after the program exits, and whether there are longer-term effects that have not been captured in what is only a short-term, 2-year evaluation. In late 2003, RPS delivered the final demand-side transfers in the original intervention areas, although it is scheduled to continue offering health-care services and teacher transfers until the end of 2005. Continued survey work in these areas will provide some of the information necessary to examine the effects of that transition, and begin to understand better the sustainability of the large changes achieved by RPS.

APPENDIX A

Descriptive Chronology of RPS Program Activities in Phase I

The following is a descriptive timeline of the major activities undertaken during the pilot phase (Phase I) of RPS, constructed in conjunction with RPS staff. We start with the formal beginning of the program, disregarding the substantial planning efforts carried out before that time by the IADB team and the eventual director to prepare the loan proposal and any consultancies including the targeting strategy presented in Arcia (1999).

RPS was initiated in the final 2 months of 1999 with five full-time employees and the director. Even with substantial advance planning, at the outset there was still much to be done before RPS could become operational. Nearly all the effort and resources during those 2 months were devoted to writing the program's operational guidelines, preparing the contractual arrangements necessary to hire central and local office staff, and designing the terms of reference for the program evaluation. Finally, there was an effort to generate social and political support through advocacy and public advertising campaigns, useful for any new program but perhaps especially important for a program involving cash transfers, such as RPS.

In 2000, the first full year of operations, there were a number of activities associated with institutional strengthening, including selecting and training an experienced professional team in both the central and regional offices. By the end of the year, RPS had 37 persons on staff in the central office in Managua and 12 in the regional offices. This team designed informational materials for program beneficiaries, operational manuals for program counterparts (e.g., providers of health and education services), the accounting system, and the information system. The latter comprises a continuously updated, relational database of beneficiaries, health-care providers, and schools. The MIS was used to (1) select beneficiaries and prepare invitations to program incorporation assemblies, (2) calculate transfer amounts, (3) compile requests to the Ministry of Health for vaccines and other materials, and (4) monitor whether both beneficiaries and health-care service providers are meeting their respective responsibilities. Substantial time was dedicated to designing data forms for the various program participants that feed 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 were entered into the computer). In conjunction with IFPRI, RPS also estimated and calibrated an econometric model to be used for household targeting in the second stage of Phase I.

Another important activity started in 2000 was the census implemented in the program areas. After designing a two-page questionnaire (to be used in all the *comarcas*) that would serve both as a registry and as an input into the household targeting decisions, RPS contracted the National Institute of Statistics and Censuses to implement it for all households living in the 42 geographically targeted *comarcas* to identify potential beneficiaries and construct a registry

of them. This was first done in March and April 2000 with follow-up fieldwork in September and October. Citing coordination problems and concerns over data quality and coverage (part of the reason that a second round of fieldwork was necessary was that households had been missed), RPS itself carried out the census in 17 *comarcas* in which household targeting was to be implemented, in January and March 2001, and thereafter internalizing this part of the operation. The next main activity in 2000 was the preparation and implementation of the beneficiary-incorporation assemblies. Once beneficiaries had been identified and integrated into the database, they were invited in groups of 30 or fewer to assemblies typically held in the local school to be given the opportunity to participate and to be informed about the objectives of the program and their rights and duties as participants. About 6,000 households were incorporated into the program at the end of 2000; these are the beneficiaries living in the 21 geographically targeted *comarcas* sampled and studied in this report. An additional 4,000 beneficiaries were incorporated in mid-2001 (but are not included in the impact evaluation), using household targeting methods. After each incorporation, the volunteer representatives had to be selected and trained.

Another important set of activities in 2000 was related to program evaluation. The second tranche of the IADB loan and expansion of the project to Phase II were contingent upon various external evaluations, including an impact evaluation, a targeting evaluation, a beneficiary opinion survey, and an institutional evaluation. In conjunction with IFPRI, planning for the first two was completed in 2000 and the first baseline survey, similar in design to an LSMS survey, was carried out in the 42 intervention and control *comarcas* in August and September 2000, before the program started (IFPRI 2001a, 2001b). Two follow-up surveys were carried out in October 2001 and October 2002 (IFPRI 2003).

As designed, a normal year of demand-side operations in RPS includes six cash transfers (every 2 months) to households. In 2000, however, because the program was just beginning, only two transfers took place in the latter part of the year. In 2001, which can be considered a more operationally normal year, five of the scheduled six cash transfers were made. Finally, in 2002, owing to budget delays associated with the expansion loan, again five of the six transfers originally planned were made. Aside from the design of the monitoring system, there were no other activities associated with conditionality in 2000; these began in 2001 a few months after the transfers were first delivered and continued from then on.

The supply-side intervention of the program for the health component consists of the delivery of health-care services and workshops by private health-care providers. Payments for the services were supposed to occur each month, but because of the necessary preparation (such as designing the services and terms of references and contracting providers), implementation of this component was delayed and in 2001 only two transfers were made. Therefore, for supply of health-care services, 2001 cannot be considered a normal operational year. In 2002, 11 payments were made and only the payment for December is missing from the accounting data.

Finally, during both 2001 and 2002, a significant amount of time was devoted to planning the expansion phase, redesigning some program components as well as negotiating with government officials and financing institutions. Also in late 2002, the program was transferred from FISE to the Ministry of the Family (Mi Familia), in charge of social protection of vulnerable groups in Nicaragua.

APPENDIX B

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 6,000 households, substantially more than 5,000, the planned number of beneficiaries in the program during the first stage of Phase I. 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, a total of 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. None of these households, however, were drawn in the random sample for the baseline survey. 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

Thirty-seven households from this category were included in the random sample for the baseline survey.

Finally, 240 (4.2 percent) households did not attend the orientation assembly and/or chose not to participate. Thus in the first stage of Phase I, 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 tended 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), although probably not by very much since they represent only about 15 percent of households in the area.

APPENDIX C

Contractual Indicators for One-Year RPS Evaluation in IADB Loan Contract

Indicator	Goal
1. Percentage of children under age 3 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 who 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 (changed from original: An increase of more than 10 percentage points in the intervention group over the control group)
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 (changed from the original: 5 percentage points)
6. Percentage of 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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