The Impact of Conditional Cash Transfers in Nicaragua on Consumption, Productive Investments, and Labor Allocation

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Abstract

This paper examines the effects of a conditional cash transfer program in Nicaragua on a wide range of outcomes related to productive investments. This is done using a randomized community level evaluation in which households were interviewed both before and after the program began in both randomly selected treatment and control areas. While limited information was collected on productive activities, the strength of the evaluation design permits a rigorous assessment of many possible productive investment behaviors. Overall, there is evidence of small increases in investments in economically productive activities and negative effects on labor supply for beneficiary households. This is unsurprising, given the findings from many studies that indicate that the bulk of the transfers are directed to current consumption, consistent with most programs secondary objective of increasing food expenditures. In contrast to the gains made in human capital development of children (reported elsewhere), the potential for long term increases in consumption as a result of increased investment, while positive, may be limited.

Key Words: Impact evaluation, Conditional cash transfer, Productive assets.

JEL: O15, Q12.

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1. INTRODUCTION

In recent years, increasing emphasis has been placed on the importance of human capital in stimulating economic growth and social development. Consequently, investing in the human capital of the poor is widely seen as crucial to alleviating poverty, particularly in the long term. There is also growing recognition of the need for social safety nets to protect households from poverty and its consequences during the push for economic growth (de Janvry et al. 2004; World Bank 1997). Consistent with this, several Latin American countries have introduced conditional cash transfer programs that integrate investing in human capital with access to a social safety net (Handa and Davis 2006; Rawlings and Rubio 2005). One of the first, and largest, programs was the *Programa Nacional de Educación, Salud y Alimentación* (PROGRESA) in Mexico, begun in 1997 (Skoufias 2005). Another large program is *Bolsa Alimentação*, a nutrition-oriented cash transfer program in Brazil. A third such program, examined in this paper, is the Nicaraguan *Red de Protección Social* (RPS).

The broad objective of these programs is to generate a sustained decrease in poverty in some of the most disadvantaged regions in their respective countries. Their entry point for doing so is human capital, starting with the premise 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 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.

There has been substantial research on the effectiveness of such programs for a wide range of outcomes related to their primary emphasis on human capital development. Much of this research shows that these programs have indeed had a number of positive effects (Rawlings and Rubio 2005). Less, however, is known about other potential program effects that also may contribute to the overall objective of poverty reduction, such as increased productive investments made possible with program transfers (Gertler, Martinez, and Rubio-Codina 2006). In this paper, I assess the extent to which RPS has stimulated such productive investments, exploring some of the other direct and indirect mechanisms through which one might expect programs such as these to reduce poverty in the medium and long term.

More specifically, I present quantitative impacts of RPS for a wide range of outcomes related to productive investments. This is done using a randomized community level evaluation in which households were interviewed both before and after the program began in both randomly selected treatment and control areas. While limited information was collected on productive activities, the strength of the evaluation design permits a rigorous assessment of many possible productive investment behaviors. Overall, there is evidence of small increases in investments in economically productive activities and negative effects on labor supply for beneficiary households. This is unsurprising, given the findings from many studies that indicate that the bulk of the transfers are directed to current consumption, consistent with most programs secondary objective of increasing food expenditures. In contrast to the gains made in human capital development of children (reported elsewhere), the potential for long term increases in consumption as a result of increased investment, while positive, may be limited.

2. DESIGN AND IMPLEMENTATION OF THE *RED DE PROTECCIÓN SOCIAL*

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 things, their children are attending school and making visits to preventive health-care providers. When households fail to fulfill those obligations, they lose their eligibility. RPS's specific stated 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, and
- Increasing the health-care and nutritional status of children under age 5.

RPS comprised two phases over six years, starting in 2000. The pilot phase (or Phase I) lasted three years and had a budget of $11 million, representing approximately 0.2 percent of GDP or 2 percent of annual recurring government spending on health and education (World Bank 2001, annex 21). As a condition of the Inter-American Development Bank (IADB) loan financing the 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. In late 2002, based in part on the positive findings of the various evaluations, the Government of Nicaragua and IADB agreed to a continuation and expansion of the program, known as Phase II, for four more years with a budget of $22 million. In Phase II, original beneficiaries were phased out of the program as new beneficiaries were incorporated.

To permit an assessment of whether and how RPS altered productive investment behaviors, it is first necessary to describe how the program operates and how it has evolved.

2.1 Program targeting

For Phase I of RPS, the Government of Nicaragua selected rural areas of the departments of Madriz and Matagalpa from the northern part of the Central Region, on the basis of poverty as well as on their capacity to implement the program. This region was the only one that showed worsening poverty between

1998 and 2001, a period during which both urban and rural poverty rates were declining nationally (World Bank 2003). 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). In 1998, approximately 80 percent of the rural population of Madriz and Matagalpa were poor, and half of those were extremely poor (Maluccio and Flores 2005). In addition, these departments had easy physical access and communication, relatively strong institutional capacity and local coordination, and good coverage of health posts and schools. By purposively targeting, 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, six (out of 20) municipalities were chosen based on criteria similar to those used at the department level. The six were well targeted in terms of poverty. Between 36 and 61 percent of the rural population in each of the chosen municipalities were extremely poor and 78–90 percent were poor or extremely poor (Maluccio 2005), compared with 21 and 45 percent, respectively, for Nicaragua as a whole (World Bank 2003). While not the poorest municipalities in the country, or in the chosen departments for that matter, the proportion of impoverished people living in these areas was still well above the national average. Clearly there was substantial need in this population, and hence scope for alleviating poverty.

In the last stage of geographic targeting, a marginality index was constructed for all 59 rural census *comarcas*[1] (hereafter localities) in the selected municipalities. The index was the weighted average of a set of locality-level indicators (including family size, access to potable water, access to latrines, and illiteracy rates) in which higher index scores were associated with more impoverished areas. The 42 localities with the highest scores were eligible for the pilot phase's first stage and form the evaluation area examined in this paper. Although the initial program design called for geographic targeting alone in these 42 localities (that is, with all resident households eligible), about six percent of households, deemed to have substantial resources, were excluded ex ante from the program,[2] a feature of the targeting I return to when discussing the econometric strategy.

2.2 Program design

RPS has two core components:

Food security, health, and nutrition. Each eligible household received a bimonthly (every two months) cash transfer known as the "food security transfer," contingent upon attendance at bimonthly health educational workshops and on bringing their children under age 5 for scheduled preventive (or well

[1] 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*.

[2] Maluccio (2005) provides further details on the RPS targeting methodology.

child) healthcare appointments with specially contracted providers. The specific healthcare services required by the program were provided free of charge to beneficiary households and included growth and development monitoring, vaccination, and provision of anti-parasites, vitamins, and iron supplements. Children under age two were seen monthly and those between two and five, bimonthly. 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.

Education: Each eligible household received a bimonthly cash transfer known as the "school attendance transfer," contingent on enrollment and regular school attendance of children ages 7–13 who had not completed fourth grade of primary school. Additionally, for each eligible child, the household received an annual cash transfer intended for school supplies (including uniforms and shoes) known as the "school supplies transfer," which was contingent on enrollment. Unlike the school attendance transfer, which was a fixed amount per household regardless of the number of children in school, the school supplies transfer was 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 "teacher transfer." The delivery of the funds to the teacher was monitored (and was a program condition for continued eligibility), but not their ultimate use.

At the outset, nearly all households were eligible for the food security transfer, which was a fixed amount per household, regardless of household size. 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 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) were as follows: the food security transfer was $224 a year and the school attendance transfer $112. On its own, the food security transfer represented about 13 percent of total annual household expenditures in beneficiary households before the program. A household with one child benefiting from the education component would have received additional transfers of about 8 percent, yielding an average total potential transfer of 21 percent of total annual household expenditures. Over the two years, the actual average monetary transfer (excluding the teacher transfer) was approximately C$3,500 ($272 or 17 percent of total annual household expenditures).[3] 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

[3] The value of the supply-side services, as measured by how much RPS paid to the providers, was also substantial. On an annual basis, the education workshops cost approximately $50 per beneficiary and the health-care services for children under age 5, approximately $110, including the value of the vaccines, antiparasites, vitamins, and iron supplements, all of which were provided by the Ministry of Health.

(Caldés, Coady, Maluccio 2006). 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 due to inflation over two years in Phase I. In Phase II, which began in 2003 and incorporated new beneficiaries, demand-side transfers were reduced. The food security transfer was $168 for the first year of program participation and then declined to $145 and $126 in the second and third years. At the same time, the school attendance transfer also declined slightly to $90 a year. Partly offsetting these reductions were increases in the school supplies transfer, which rose from $21 to $25 per student and the teacher transfer, which rose from $5 to $8 per student. These figures represent potential transfers, such that households participating in the program may have received the full amounts or less, depending on their fulfillment of the conditions.

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 described above. During the first two years of delivering transfers, approximately ten percent of beneficiaries were penalized at least once and therefore did not receive the food, education, or combined transfers; a household can receive one of the transfers if, in the same period, compliance for one component is complete while compliance for the another component is incomplete. Only the designated household representative was allowed to collect the cash transfers, and where possible, RPS appointed the mother to this role. As a result, more than 95 percent of the household representatives were women. These representatives attended the health education workshops and were responsible for ensuring that the requirements for their households were fulfilled.

2.3 Principal findings from earlier quantitative assessments of RPS

Overall, RPS had positive and significant double-difference estimated average effects on a broad range of indicators and outcomes from 2000 to 2002, including expenditures, health-care inputs, nutritional status of children under age five, and school enrollment. Where it did not, it was often due to similar, though smaller, improvements in the control group. Nearly all estimated effects were larger for the extremely poor, reflecting their lower starting points (e.g., lower percentages of children enrolled in primary school before the program). Among poorer beneficiaries there was simply more potential for improvement on many of the indicators. As a result, the program reduced inequality across expenditure classes for a variety of outcomes (Maluccio and Flores 2005).

3. DESIGN OF THE EVALUATION, METHODOLOGY, AND VALIDITY

3.1 Evaluation design and double-difference methodology

The evaluation for Phase I of RPS was based on a randomized, community-based intervention. One-half of the 42 localities (targeted in the first stage of Phase I as described in Section 2.1) eligible for the pilot phase were randomly selected into the program; thus, there are 21 localities in the intervention group and 21 distinct localities in the control group. The random selection was carried out after ordering the localities by the marginality index described above into seven strata of six localities each, and selecting three localities as treatment and three as control from each stratum. Given the geography of the program area, control and intervention localities are in some cases adjacent to one another.

In mid 2003, original control areas were incorporated into the program (during Phase II). Initially, RPS was designed to provide transfers and related services for a period of up to three years. During implementation, however, it was decided to extend the supply-side health and education (the teacher transfer) components for an additional two years, but not the demand-side transfers. As a result, in early 2003 when the original control areas were beginning to receive the program, the demand-side transfers were terminated in the original intervention areas, though the original beneficiaries continued to receive the supply-side health and education components through the end of the evaluation period examined here. I return to a discussion of this cross-over in Section 3.2 where I qualify the interpretation of the single-difference estimator for the 2004 period and in Section 4.2 where I describe the main empirical results.

Household and individual level data were collected in both the intervention and control localities before and after RPS was implemented (see Section 3.3). This enables the use of the double-difference method to calculate "average program impact."[4] 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. For this work, the double-difference technique is extended to account for four measurements taken in 2000, 2001, 2002, and 2004. The basic estimating equation is shown in (1).

(1) $$E_{ict} = \alpha_0 + \alpha_1 Y_1 + \alpha_2 Y_2 + \alpha_4 Y_4 + \alpha_5 P_c + \delta_1 Y_1 P_c + \delta_2 Y_2 P_c + \delta_4 Y_4 P_c + X_{ic0}'\beta + \mu_{ic} + \nu_{ict}$$

Where

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

Y_1 = (1) if Year 2001,

Y_2 = (1) if Year 2002,

Y_4 = (1) if Year 2004,

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

X_{ic0} = a vector of time invariant household demographic characteristics measured in 2000

μ_{ic} = is a time invariant household random effect,

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

all the α's, β, and δ's are unknown parameters.

[4] Ravallion (2001 and 2007) provide useful discussions on this and related evaluation tools.

The key parameters of interest are δ_1, δ_2 and δ_4; δ_1 is the double-difference estimator of the average program effect for 2001 (relative to 2000), δ_2 for 2002 (relative to 2000), and δ_4 for 2004 (relative to 2000). For 2004, however, since the control group had entered the program by this time and the original intervention group had ceased to receive cash transfers about 10 months earlier, the estimated effect is the four year effect of having had the full program for three years and the supply side components for one year relative to having had no program for three years and the full program, though with slightly lower transfers (as described in Section 2.2), for the previous year. 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 locality-level variables so that the δs can be estimated consistently. It is not necessary to include other variables in this regression (apart from the year dummies) for the consistency of the estimator for δ_i, though doing so increases the precision of the estimates.

In some cases, where information on certain outcomes was not collected in the baseline but only in 2002 and 2004, I present instead the results from single difference equations (i.e., the difference between treatment and control areas after the program had begun), also a valid estimate of the program effect given the randomized design of the evaluation. The estimating equation simplifies to (2) where everything is defined analogously to equation (1) and δ_{s2} and δ_{s4} represent the single-difference estimates of the program effect corresponding to δ_2 and δ_4 above.

(2) $$E_{ict} = \alpha_0 + \alpha_4 Y_4 + \delta_{s2} Y_2 P_c + \delta_{s4} Y_4 P_c + X_{ic0}'\beta + \mu_{ic} + \nu_{ict}$$

Since I 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 locality, 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 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—therefore they are not presented.

In the analyses that follow, households interviewed in any of the pertinent survey rounds, i.e., the unbalanced panel sample, are included. All estimates include household random effects (results are similar if instead they are treated as fixed effects, and Hausman tests indicate no significant differences between them) as well as a set of household demographic conditions measured before the start of the program. Not controlling for household effects or the household level variables, generally leads to less precise estimates and therefore weaker statistical significance. The estimations that follow also ignore the stratified sample design which can be corrected for statistically by using locality-level sample weights; correcting for this aspect of the design makes no substantive changes to the estimated effects.

3.2 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).[5] A further advantage to a randomized design is that program impact is easy to calculate and, as a consequence, easier to understand and explain.

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 there is contamination due to (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). Section 3.4 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; therefore the generalizability of the results is 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,

[5] 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, including with these data (Handa and Maluccio 2007; Heckman, Lalonde, and Smith 1999).

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 localities 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 1995 census (Maluccio 2005), I estimate that three-quarters of the approximately 150 rural localities in the departments of Madriz and Matagalpa have priority 1 or 2, as do three-quarters of the approximately 1000 rural localities in the country as a whole. If instead one considers levels of extreme poverty, there are more than 350 localities in the country with extreme poverty at or above 42 percent, the average level in the pilot areas. On these broad indicators used for geographical targeting, then, there are a large number of similar localities, indicating those chosen for the pilot were not atypical.

3.3 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, 2002, and 2004. 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 relevant to this study (e.g., income from labor and other sources) to minimize respondent burden and ensure collection of high-quality data in a single interview.[6]

The household survey sample is a stratified (at the locality level) random sample of all 42 localities described above and using the RPS population census collected in May of 2000 as the sample frame. Forty-two households were randomly selected in each of the 42 localities, yielding an initial target sample of 1,764 households. 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. For programmatic reasons, the RPS 2001 follow-up survey was delayed until the beginning of October, to allow additional time for the interventions to take root. Of course, the advantage of the originally 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. With a control group, however, the possible bias introduced by seasonality is addressed by differencing across intervention and control groups. This small difference in the timing of

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

the survey, then, does not present a serious problem for the estimation of average program effects. The 2002 and 2004 surveys were also carried out in October.

As with any panel survey, first round nonresponse and latter round attrition in the survey are potential concerns for the analysis. I now document nonresponse in the 2000 baseline survey and attrition and a small amount of contamination in the follow-up surveys. Overall, 90 percent (1,581) of the stratified random sample of 1,764 was interviewed in the first round (see Appendix Table 1) with slightly lower completion in control localities. In a handful of localities, the coverage was 100 percent, but in six it was under 80 percent. For the follow-up surveys in 2001 and 2002, the target sample was limited to these 1581 first round interviews. In 2002, 91 percent of these were re-interviewed (including a small number who had migrated within the six municipalities and were tracked for re-interview). Again, however, coverage in six of the localities was substantially worse, with less than 80 percent re-interviewed. In 2004, 85 percent of the original 1581 were re-interviewed, but 90 percent of the households targeted that year (which included only those who had been interviewed at least once in either 2001 or 2002).

The principal reasons for failure to interview targeted sample households were that all household members were temporarily absent (i.e., more than the several days the survey team were in the area) or that the dwelling appeared to be uninhabited—both of which are likely to be associated with temporary or permanent migration. Among those classified as not being re-interviewed are six households whose surveys were lost and 37 households living in control localities who in fact turned out to have been program beneficiaries, despite being initially categorized as living in a control locality. The localities used by RPS are census areas that often do not coincide with communities. These 37 households (spread across a dozen localities) 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, 2002, and 2004 samples. Unsurprisingly, since they represent only 5 percent of households in the control group, their inclusion or exclusion affects estimated results little. Both the completion rate at baseline and subsequent attrition levels are on a par with similar surveys in other developing countries (Alderman et al. 2001; Thomas, Frankenberg, and Smith 2001).

Since the advantages of randomization are dissipated with attrition if it is nonrandom, I next examine the correlates of the observed attrition to assess the likely possible effects or biases on the ensuing analyses (Thomas et al. 2003). Due to the RPS census, which collected a variety of information relevant to the program for use in a proxy means prediction model allowing one to predict expenditures for each household, there is information on those households not interviewed in the baseline or follow-up surveys. 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 four rounds, indicate that attrition is indeed nonrandom. Households that were not interviewed were more likely to have an older, more educated household head, larger family size, higher predicted expenditures, or more land. A comparison of the means of these same characteristics for those re-interviewed compared with those who attrited 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. Moreover, the coefficient on an indicator of whether the household was in the control or treatment group is both small and insignificant.

The estimating strategy employed, while not formally incorporating attrition, is likely to be robust to attrition bias, particularly if unobserved persistent heterogeneity is leading to attrition. Such heterogeneity is directly controlled for in the household random effects regressions. Moreover, results using only the balanced panel (not shown) are similar to those estimated on the unbalanced panel, suggesting attrition related changes in sample composition are not driving the results. Therefore, I conclude that attrition is not a major concern for estimating program effects in these data.

3.4 Validity of the experiment and the evaluation

a. Outcome of the randomization

While the selection of localities into intervention and control groups was undeniably random, it was at the same time only one of the millions of possible random draws from a finite (42) number of groups. As a result, intervention and control groups may still differ in statistically significant or, more importantly, substantive ways as the result of a "bad" draw. In this subsection, I 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 initial differences between the groups.

Table 1 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 < 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 under age 5.

[TABLE 1 – ABOUT HERE]

In the bottom portion of the table, the proportion of households classified as extremely poor, poor, and nonpoor are listed.[7] Because of the stratification into groups of six localities 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; for example, they are not per capita adult equivalents (Deaton and Zaidi 2002). As above, 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 (extreme poor, poor but not extremely poor, and nonpoor) reveal only five significant differences in 45 comparisons. In all these comparisons, then, the percentage of indicators that are statistically significant across groups is as expected using a 10% significance level.

b. Potential contamination of the control group: Other programs

A possible source of contamination is due to other programs, referred to as substitution bias by Heckman and Smith (1995). This occurs when households in control localities participate in programs similar or complementary to RPS. The delay by one 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 localities, 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 with similar objectives (such as the *Programa de Atención Integral a La Niñez Nicaragüense*, or 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 realistic counterfactual compared to one in which no other program were allowed to enter the area (what one might call a "sterile" control group)—in Nicaragua, as in many developing countries, there are a multitude of overlaying programs in policies with related objectives. If other programs *do* react to the intervention, while the pure RPS program impact is more difficult to assess, it is also reasonable to

[7] These and other descriptions of poverty in the sample are calculated based on 2001 per capita annual expenditure poverty lines of $C2,691 ($202) for extreme poor (calculated as the amount required to purchase a minimum requirement food basket) and C$5,157 ($386) for poor, which adds nonfood requirements (World Bank 2003). Households are classified into poverty groups based on their initially measured (in 2000) per capita annual total household expenditures (including own-production) using these 2001 Nicaraguan poverty lines. 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.

characterize the changes in other programs as being a result of RPS and therefore the net impacts as those reflecting the effectiveness of the program in the field.

Potential contamination due to 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 locality that were benefiting from an array of possible programs and services, and to determine whether or not RPS was the provider. Overall, the extent of such development programs operating in these areas was limited, and there were no substantive changes in programs related to the outcomes assessed in this paper, though there is a little evidence that RPS crowded out some programs related to its primary objectives of education and health-care (Maluccio and Flores 2005).

4. THE EFFECTS OF CONDITIONAL CASH TRANSFERS: THE *RED DE PROTECCIÓN SOCIAL*

4.1 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 were extremely poor and 78–90 percent were extremely poor or poor, compared with national averages in 1998 of 21 and 45 percent, respectively. Within the 42 localities selected for the program evaluation, 42 percent of the population was extremely poor before the program—that is to say that 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 "nonpoor" in the sample, was in the bottom two-thirds of the national Nicaraguan per capita expenditure distribution and so was near-poor.

Maluccio and Flores (2005) report extensive results of the program on household expenditures over the 2000–2002 period. Here, I extend those results to 2004 (Table 2). Estimation is carried out on the unbalanced panel sample using household random effects and a set of demographic and other controls measured at baseline.[8]

[8] The controls not reported in the table include: a constant; the number of persons in different demographic categories (children 4 and under, children 5 to 10, boys 11 to 14, girls 11 to 14, boys 15 to 19, girls 15 to 19, men 20 to 34, women 20 to 34, men 35 to 60, women 35 to 60, and men above 60, with the left out category being women above 60), education of the head of household (and its square), whether the household head was female, age of the household head (and its square), predicted per capita expenditures (and its square and cube) described in Maluccio (2005).

For comparison, the average value of annual cash transfers for intervention locality households in the evaluation survey over the first two years was C$3,500 or $272, about C$700 on a per capita basis. Thus the average estimated impact on total or per capita expenditures was above average transfers in the first year, but somewhat below them the second year. In both cases, however, they were of the same order of magnitude and in neither year can one reject the hypothesis that the estimated effect is significantly different from the average annual transfer amount of C$3,500 (or C$700 on a per capita basis). The difference between estimated effects and transfer amounts may in part reflect imprecision in measuring annual household expenditures (Maluccio and Flores 2005). In the fourth year, the estimated impact flips, reflecting the cross-over design and raising the possibility that household expenditures did not undergo a permanent increase in the original intervention areas, a theme I return to below.[9]

[TABLE 2 – ABOUT HERE]

The estimated impacts in Table 2 suggest that beneficiary households are, on average, spending a large proportion of their transfers on current expenditures, though the fraction spent by the original intervention group appears to have been smaller in the 2002, the second program year (when average transfers were about the same as in the first year). The drop in expenditures in the control group over the first two years (as seen by the coefficients on the annual dummy variables) seems to have been due in part to an economic downturn in the areas where RPS was operating and in Nicaragua more generally. Within the control group, expenditures fell among the poor and nonpoor but held steady for the extremely poor. Two events affecting the area included a severe drought in 2001 and a sharp, persistent, drop in international coffee prices, which affected many of the agricultural laborers in that industry (Varangis et al. 2003). The rural Central Region of Nicaragua was the most affected by these events and was the only region showing an increase in poverty rates between 1998 and 2001 (World Bank 2003). The transfers provided by RPS apparently compensated for income losses during this downturn. While not designed as a traditional safety net program in the sense of reacting or adjusting to crises or shocks, the economic difficulties experienced in these communities allowed RPS to perform like a traditional safety net, enabling households to maintain expenditures during the downturn.

Showing a pattern similar to total and per capita annual total household expenditures, RPS produced significant net average increases in per capita annual food expenditures of nearly C$900 in 2001 and C$700 in 2002. Except for 2004, the average increases in the per capita annual food expenditures are approximately 85–90% of the average changes in per capita annual total household expenditures in each year. Consistent with the program's goals and direct advice, additional expenditures as a result of the transfers were spent predominantly on food. During the incorporation assemblies and some of the health

[9] Results are similar if I instead formulate the dependent variable as logarithmic expenditures; the absolute measures are reported for ease of interpretation and comparison to the transfer amounts.

education workshops, an informal "requirement" that the income supplements are primarily intended for food purchases is emphasized. In addition, Adato 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 in the first two years. This tendency to spend more on food is further strengthened by the relatively small (75%) fraction of the impact spent on food in 2004, which could be due to continued higher food expenditures in the original intervention group after their transfers ceased. Though I 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).

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$332 ($25), slightly larger than the per student value of the educational supplies transfer in Phase I (C$275 or $21).

4.2 Household consumer durables and productive assets

To this point, the evidence indicates that households are following the recommendations of the program, i.e., they are spending most of their income from the program on current (food and education) expenditures.[10] Expenditures increased substantially with the program, and possibly more than the amount of transfers given (at least in the first year). This is suggestive that investments were being made with the funds yielding positive returns. I next look for evidence that transfers are being invested (I do not observe savings), or productive activities within the household are changing. The questionnaire asked about other forms of expenditures related to investments at the household level, such as on durable goods and certain investments in agricultural or industrial machinery and equipment.

First, I explore ownership of consumer durables, many of which might make individuals more productive in the household, allowing more time for other productive activities or leisure, or even be used in productive activities themselves. The list asked about in the questionnaire includes radios, tape recorder, televisions, video, refrigerator, stove, iron, grinder (for maize), fan, toaster, mixer, microwave, air conditioner, sewing machine, typewriter, computer, car, boat, bicycle, or motorcycle. Ownership of all of these items was extremely rare in the sample before the program—less than 2 percent—with the exception of radio, television, grinder, tape recorders, and bicycles. Results examining the ownership of these items, in addition to increases over time for both treatment and control groups, shows mixed evidence that the program led to increased ownership of these single items, though there were three (out

[10] 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.

of seven) small but significant increases in 2002 (Appendix Table 2). The story is similar when I consider either the total (i.e., the sum of the) number of items reported or the total value of the items owned, as shown in Table 2. There is some slight evidence that the value of consumer durables increased in the 2004 as a result of the program, though by less than 200 córdobas, in the year after transfers stopped for the original intervention group. This is consistent with their having invested in those things during the program years.

A similar pattern of limited evidence of increased investment emerges from an analysis of an array of productive goods associated with agricultural activities. Questions about these assets were not asked in the first two rounds of the survey so the effect of the program must be estimated using single difference techniques. Due to the randomized design, the first difference between outcomes across intervention and control groups is a valid estimate of the program impact in 2002 and 2004, though again for the latter this represents the effect of the cross-over design. While it is not possible to compare directly the initial levels of these characteristics before the program, there is no obvious reason to think that the similarity between intervention and control groups described above does not extend to these measures as well. Table 3A presents the results for program effects on agricultural investment (and Appendix Table 3 the results for individual items).

The first two columns show estimates of the effect on the number of productive agricultural items owned (from a list including animal implements such as plows, water pump, sprayer, tools, carts, and other, all shown separately in Appendix Table 3) and the value of those items. There is slight evidence of a positive effect of the program on the number of items in 2002. Stronger evidence (significant at the 10% level) is seen in the third and fourth columns where I consider an alternative approach that examines expenditures on agricultural or industrial equipment taken directly from the expenditure module for expenditures made in the previous 12 months (as opposed to the total current asset measures and their valuation).

[TABLE 3 – ABOUT HERE]

A potentially easier avenue for expanding agricultural production than acquiring or cultivating new lands is to expand animal activities. There is little evidence, however, that the program affected investment in animal husbandry, as shown in Table 3B. In addition to the indicators considered in the table, estimates on the effect of the program on 1) whether a household had a certain type of animal; 2) the number of animals of each type; and 3) the value of animals of each type yield no systematic pattern of investment in any of the animal types.

The final asset considered is land ownership. It is possible to examine whether households were more likely to be cultivating land, to own any land, or, on the intensive margin, whether they owned more land (these questions were asked in 2004 only). Given that these latter measures represent longer-term investments, even in 2004 one would expect that the estimated program effect yields a good estimate of the effect of the program and is not subject to the immediate shift in behavior that was seen with the effect of the program on general household expenditures, for example. This exploration, however, yields no evidence of a program effect on these outcomes (results not shown).

4.3 Household productive activities and labor allocation

The final dimension along which I consider changes in productive activities is the running of small businesses or economic activities and labor supply and allocation of adults in the household. For these measures it is again possible to carry out double-difference estimation since information was collected in all years. Table 4 presents the home business activities and Table 5 labor supply and allocation. Approximately 10 percent of the population report working in some sort of economic activity related to producing and selling goods (apart from agriculture), reselling or retailing goods, or selling specialized services (such as repairmen). For 2001 and 2002, the estimated program effect on these home business activities was negative, though small. In 2004, there was no effect on them. Rather than spurring these sorts of initiatives, the program was a disincentive to work in these areas. It is plausible that this is because they are marginal activities with low returns.

[TABLE 4 – ABOUT HERE]

Turning to labor supply and allocation at the household level, there are a number of clear patterns. First, in 2001 and 2002 the program had a negative effect on hours of labor supplied at the household, adult, male, and female levels, with the disincentive highest for males who carry out the vast majority of the reported labor.[11,12] Nearly all of these hours were in agriculture, in other words the estimated effects on hours worked in agriculture at the household level were the same as those shown for total hours worked in Table 5. The reduction in hours for these beneficiaries, however, was about evenly offset by an increase in reported hours worked in those years, arguably a response to the economic crisis mentioned earlier. So while there was a disincentive to labor, it is not that there was a reduction in labor supply after the onset of the program, overall labor supply remained about the same. Transfers for those starting in the program in 2004 were lower than in 2002, so that any disincentive for labor supply was likely to be smaller. No significant effects were seen for 2004, however, as both original intervention and the former control groups had equally increased labor hours relative to 2000.

[TABLE 5 – ABOUT HERE]

[11] These estimates are household random effects models without controls, to allow interpretation of the constant term as the average number of hours worked. The estimated program effects are little changed if instead the household controls are included.

[12] Part of this effect may be operating through changes in household composition (Winters et al. 2006). The results are similar if measured on a per capita labor supply basis, however.

As a crude indicator of how labor is being allocated, I consider last the fraction of hours devoted to agricultural labor at the household level to explore whether households are changing their productive activities. As the results in the final column show, this does not appear to be the case. Thus neither increased labor supply (on the part of the beneficiary households) nor changing labor composition appears to explain the large increases in expenditures with the program.

5. CONCLUSIONS

This paper has presented a set of findings from the quantitative evaluation of a randomized community-based intervention, RPS, as they relate to expenditures, productive activities, and labor supply. Where possible, I 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. The estimates presented represent the effect of the program as a whole, in particular combining supply- and demand-side components. They also only represent the short-term effects of the program (after one, two, or four years), though some of the outcomes examined are themselves long-run indicators, such as investment in productive goods and land.

A crucial question that previous research has not addressed is whether the effects of RPS will persist after the program exits, and whether there are longer-term effects that have not been captured in what is necessarily only a short-term assessment. In late 2003, RPS delivered the final demand-side transfers in the original intervention areas, though it continued offering health-care services and teacher transfers until the end of 2005. At the same time (mid 2003), the original control group became beneficiaries of the program. Continued survey work in 2004 begins to 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.

Program impacts in the early years when transfers were being given clearly are reflected in increased expenditures and the lion's share of this is on food expenditures. Those increases disappear, however, when the transfers cease. With those findings, I turned to an assessment of the program on investment of various types and of labor allocation. There is some limited evidence that the program led to an increase in ownership of consumer durables or agricultural investment goods. Nor did it lead to dramatically increased entrepreneurial activities or labor supply in general. In fact, labor supply, while similar to pre-program levels, was negatively affected relative to the control group, where labor supply increased in the first two years of program operations.

These findings do not imply that the program has no long term effects—it almost certainly does in terms of investment in child health and education which should continue to lead to benefits for many years to come. In contrast to Mexico, however, where there seems to be a great deal of investment and returns from that (Gertler, Martinez, Rubio-Codina 2006), there is only weak, albeit positive, evidence that RPS is leading to improved investment activities in the rural areas in which it operates, possibly due to limited opportunities.

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Table 1–Comparison of intervention and control households at baseline 2000

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

a. Includes radio/tape recorder, stove, air conditioner, and fan.
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 2005). Analysis based on 706 observations in the intervention group and 653 observations in the control group in each year. *** indicates significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level.

Table 2–The effect of Nicaraguan RPS on expenditures and consumer durables, 2000–2004

	Household total expenditures		Per capita total expenditures		Per capita food expenditures		Education expenditures		Number of consumer durables		Value of consumer durables	
	Households		Households		Households				Households		Households	
Year 2001	-2794.6	***	-590.3	***	-492.2	***	172.7	***	0.1039	***	86.14	
	(6.23)		(5.55)		(6.32)		(4.34)		(2.49)		(1.13)	
Year 2002	-2261.8	***	-430.0	***	-419.2	***	214.0	***	0.1357	***	43.15	
	(5.01)		(4.02)		(5.35)		(5.34)		(3.23)		(0.55)	
Year 2004	7635.2	***	1805.4	***	1097.5	***	576.7	***	0.4305	***	198.88	**
	(16.7)		(16.7)		(13.8)		(14.2)		(10.1)		(2.55)	
Year 2001 × RPS area	4359.8	***	1032.5	***	889.1	***	156.0	***	-0.0036		-152.02	
	(7.85)		(7.82)		(9.34)		(3.10)		(0.07)		(1.64)	
Year 2002 × RPS area	2981.4	***	788.4	***	713.7	***	332.3	***	0.0596		-81.30	
	(5.27)		(5.87)		(7.37)		(6.49)		(1.10)		(0.88)	
Year 2004 × RPS area	-3732.7	***	-749.8	***	-550.9	***	-189.2	***	0.0339		178.09	*
	(6.49)		(5.49)		(5.59)		(3.64)		(0.62)		(1.89)	
Household random effects	Yes		Yes		Yes		Yes		Yes		Yes	
Household controls	Yes		Yes		Yes		Yes		Yes			
Number of observations	5777		5777		5777		5777		5777		5777	

Household random effects (double-difference) models estimated on unbalanced sample with the following controls: a constant; the number of persons in different demographic categories (children 4 and under, children 5 to 10, boys 11 to 14, girls 11 to 14, boys 15 to 19, girls 15 to 19, men 20 to 34, women 20 to 34, men 35 to 60, women 35 to 60, and men above 60, with the left out category being women above 60), education of the head of household (and its square), whether the household head was female, age of the household head (and its square), predicted per capita expenditures (and its square and cube) described in Maluccio (2005).

Table 3A–The effect of Nicaraguan RPS on agricultural and other investments, 2002 and 2004

	Number of productive agricultural goods Households	Value of productive agricultural goods Households	(1) if Expenditure agricultural or industrial equipment or machinery	Ln Exp. on agricultural or industrial equipment or machinery
Year 2002 (1st Diff)	0.1022 ***	-23.79	0.0332 ***	0.1370 ***
	(2.81)	(0.37)	(3.29)	(3.09)
Year 2004 (1st Diff)	0.0327	9.72	0. 0354 ***	0.1489 ***
	(0.88)	(0.15)	(3.44)	(3.29)
N	2743	2743	2743	2743

Household random effects (single-difference) models estimated on unbalanced sample with household controls (see Table 2).

Table 3B–The effect of Nicaraguan RPS on animal ownership, 2002 and 2004

	Number of types of animals owned	Number of draft animals	Value of animals owned
Year 2002 (1st Diff)	-0.0377	0.0459	-136.03
	(0.74)	(0.60)	(0.23)
Year 2004 (1st Diff)	0.0136	0.0728	-155.12
	(0.26)	(0.94)	(0.26)
N	2743	2743	2743

Household random effects (single-difference) models estimated on unbalanced sample with household controls (see Table 2).

Table 4–The effect of Nicaraguan RPS on home business activities, 2000–2004

	Number of people working in home business Households		(1) if home production for sale (apart from agriculture) Households		(1) if resell/retail purchased goods Households		(1) if sell services of some sort (not day labor) Households		(1) if home production, retail, or services (previous 3 columns) Households	
Year 2001	-0.0195	**	-0.0073		0.0494	***	-0.0035		0.0445	***
	(2.12)		(0.85)		(4.48)		(1.05)		(3.36)	
Year 2002	-0.0087		0.0146	*	-0.0238	**	0.0009		-0.0079	
	(0.94)		(1.69)		(2.15)		(0.26)		(0.60)	
Year 2004	0.0001		-0.0062		-0.0361	***	0.0087	***	-0.0263	**
	(0.00)		(0.71)		(3.21)		(2.55)		(1.95)	
Year 2001 × RPS area	-0.0080 (0.75)		-0.0028 (0.27)		-0.0844 (6.35)	***	0.0010 (0.27)		-0.0807 (5.02)	***
Year 2002 × RPS area	-0.0043 (0.40)		-0.0239 (2.26)	**	-0.0325 (2.40)	**	-0.0032 (0.80)		-0.0488 (2.98)	***
Year 2004 × RPS area	-0.0119 (1.07)		0.0071 (0.66)	***	0.0024 (0.17)		-0.0051 (1.26)		0.0026 (0.16)	
Household random effects	Yes		Yes		Yes		Yes		Yes	
Household controls	Yes		Yes		Yes		Yes		Yes	
Number of observations	5777		5777		5777		5777		5777	

Household random effects (double-difference) models estimated on unbalanced sample with household controls (see Table 2).

Table 5–The effect of Nicaraguan RPS on labor supply and labor allocation, 2000–2004

	Total hours worked last week Households		Total adult hours worked last week Households		Total adult male hours worked last week Households		Total adult female hours worked last week Households		Fraction of labor allocated to agriculture last week Households	
Year 2001	3.5086	*	4.5060	**	6.1505	***	-1.2700		0.0169	
	(1.82)		(2.46)		(4.33)		(1.18)		(1.28)	
Year 2002	8.7941	***	9.4897	***	9.6842	***	0.1243		0.0283	**
	(4.52)		(5.14)		(6.78)		(0.11)		(2.13)	
Year 2004	9.6016	***	10.7339	***	8.4721	***	2.6523	**	-0.1068	***
	(4.87)		(5.75)		(5.86)		(2.41)		(7.86)	
Year 2001 × RPS area	-13.1645	***	-10.9542	***	-7.2302	***	-4.2404	***	-0.0051	
	(5.25)		(4.60)		(3.87)		(3.15)		(0.30)	
Year 2002 × RPS area	-14.1438	***	-12.0000	***	-8.0734	***	-4.4775	***	0.0071	
	(5.55)		(4.96)		(4.25)		(3.27)		(0.04)	
Year 2004 × RPS area	3.9267		2.2804		0.9386		0.7382		-0.0156	
	(1.52)		(0.93)		(0.49)		(0.53)		(0.90)	
Constant	80.9532	***	76.2770	***	62.6275	***	13.6496	***	0.8257	***
	(56.40)		(55.89)		(53.11)		(20.02)		(95.11)	
Household random effects	Yes				Yes		Yes		Yes	
Number of observations	5777		5777		5777		5777		5777	

Household random effects (double-difference) models estimated on unbalanced sample without household controls (see Table 2).

Appendix Table 1—Nicaraguan RPS evaluation survey non-response and subsequent attrition

	Baseline 2000	Follow-up 2001	Follow-up 2002	Follow-up 2004
Interviewed	1581	1453	1397	1346
(% target sample)	(89.6)	(91.9)	(88.4)	(90.3)
… of which				
Intervention	810	766	722	693
(% target sample)	(92.5)	(94.6)	(87.5)	
Control	771	687	675	653
(% target sample)	(87.4)	(89.1)	(89.1)	
Interviewed in all 4 rounds	1259	1259	1259	1259
(% 2000 sample)	(79.6)	(79.6)	(79.6)	(79.6)
… of which				
Intervention	644	644	644	644
Control	615	615	615	615
Number households not Interviewed				
Uninhabited dwelling	60	51	83	98
Temporary absence	100	28	46	30
Refusal	17	6	12	17
Urban (misclassified)	6	0	0	0
Contaminated	0	37	37	37
Lost questionnaire	0	6	6	0
Target Sample	1764	1581	1581	1491

Source: Nicaraguan RPS evaluation data. Target sample in 2001 and 2002 comprised households interviewed in 2000. Target sample in 2004 comprised households interviewed at least once in 2001 and 2002.

Appendix Table 2–The effect of Nicaraguan RPS on individual consumer durables, 2000–2004

	Radio	Television	Sound system	Iron	Maize grinder	Bicycle	Sewing Machine
Year 2001	0.0526 ***	0.0101	0.0001	-0.0299 **	0.0645 ***	-0.0039	-0.0011
	(2.52)	(0.96)	(0.01)	(2.02)	(3.73)	(0.48)	(0.20)
Year 2002	0.0406 **	0.0265 **	-0.0506 ***	0.0378 **	0.0732 ***	0.0123	-0.0082
	(1.93)	(2.50)	(3.52)	(2.53)	(4.20)	(1.47)	(1.47)
Year 2004	-0.0549 ***	0.0458 ***	-0.0306 ***	0.2955 ***	0.1364 ***	0.0113	0.0049
	(2.58)	() 4.26	(2.09)	(19.54)	(7.73)	(1.34)	(0.88)
Year 2001 × RPS area	0.0461 *	-0.0106	-0.0635 ***	0.0237	0.0334	0.0070	-0.0017
	(1.81)	(0.78)	(3.64)	(1.31)	(1.54)	(0.70)	(0.26)
Year 2002 × RPS area	0.0621 **	-0.0253 *	-0.0170	-0.0169	0.0623 ***	0.0192 *	0.0035
	(2.40)	(1.83)	(0.96)	(0.92)	(2.82)	(1.89)	(0.51)
Year 2004 × RPS area	0.0520 **	0.0404 ***	0.0005	-0.0660 ***	-0.0175	0.0125	-0.0072
	(1.97)	(2.88)	(0.03)	(3.53)	(0.78)	(1.21)	(1.03)
Household random effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household controls	Yes	Yes	Yes	Yes	Yes		
Number of observations	5777	5777	5777	5777	5777	5777	5777

Household random effects (double-difference) models estimated on unbalanced sample with household controls (see Table 2).

Appendix Table 3–The effect of Nicaraguan RPS on individual agricultural equipment items, 2002 and 2004

	Small tools	Water pump	Fumigation pump	Cart	Plow or other animal equipment	Other agric equipment
Year 2002 (1st Diff)	0.0494 ***	0.0024	-0.0095	0.0144 **	0.0045	0.0397 ***
	(3.04)	(0.74)	(0.42)	(2.32)	(0.41)	(4.11)
[N=1397]						
Year 2004 (1st Diff)	0.0212	0 .0110 ***	-0.0441 *	0.0047	0.0390 ***	0.0006
	(1.28)	(2.53)	(1.91)	(0.75)	(3.50)	(0.06)
[N=1346]						
Household controls	Yes	Yes	Yes	Yes	Yes	
Number of observations	2743	2743	2743	2743	2743	2743

Household random effects (single-difference) models estimated on unbalanced sample with household controls (see Table 2).

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