The Impact of Conditional Cash Transfers on Consumption and Investment in Nicaragua

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ABSTRACT This article examines the impact of a Nicaraguan conditional cash transfer programme on measures of expenditures and productive investment. Despite clear evidence from a randomised evaluation that the programme increased current expenditures, there is little evidence that it increased agricultural or non-agricultural investment. An estimated marginal propensity to consume out of the transfers of nearly one, combined with no effect of cumulative past transfers on current consumption, corroborate the direct evidence on investment. In contrast to gains made in human capital investment, the potential for long term increases in consumption as a result of other forms of increased investment may be limited.

I. Introduction

In recent years, increasing emphasis has been placed on the importance of human capital in stimulating economic growth and social development. 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; de Janvry et al., 2006). Consistent with this, more than a dozen Latin American countries have introduced conditional cash transfer programmes that integrate investments in human capital with access to social safety nets (Rawlings and Rubio, 2005; Handa and Davis, 2006). One of the first, and largest, programmes was the Programa Nacional de Educación, Salud y Alimentación (PROGRESA, now called Oportunidades) in Mexico, begun in 1997. Another large programme is Bolsa Familia, in Brazil. A smaller programme, examined in this article, is the Nicaraguan Red de Protección Social (RPS).

The broad objective of these programmes is to generate a sustained decrease in poverty in some of the most disadvantaged regions in their respective countries. Typically, their entry point for doing so is human capital, under the premise that
a significant cause of the intergenerational transmission of poverty is the inability of poor households to invest in the human capital of their children. These programmes attack this problem by targeting transfers to poor households and conditioning them on actions intended to improve children’s human capital development. This effectively transforms cash transfers into human capital subsidies, thereby increasing investment in human capital.

Substantial research has demonstrated the effectiveness of such programmes in improving a range of outcomes related to current consumption and investments in human capital (for example, Hoddinott and Skoufias, 2004; Schultz, 2004; Behrman and Hoddinott, 2005; Rawlings and Rubio, 2005; Skoufias, 2005). Much less, however, is known about other, more indirect, programme effects that also may contribute to their overall objective of poverty reduction, such as increasing economically productive investments in agriculture and livestock, often the main income-generating activities in the rural areas these programmes target (Davis and Stampini, 2002; Vakis et al., 2005; Gertler et al., 2007; Todd et al., 2010). When households are liquidity constrained, assets may be underemployed or inefficiently allocated. By providing transfers, these programmes likely alleviate the liquidity constraints typically thought to inhibit investment in such activities in developing countries (Rosenzweig and Wolpin, 1993), and can thus increase living standards both via increased current expenditures as well as increased future expenditures resulting from the return on the investments (Gertler et al., 2007). For example, in the context of the Mexican programme PROCAMPO, which includes cash transfers, Sadoulet et al. (2001) demonstrate that the indirect effects of transfers loosening such liquidity constraints may include purchase of productive inputs (leading to short-run gains) as well as productive assets leading to longer-run gains. Given their size (nearly 20% of total household expenditures), the mere transfer of cash under RPS, even with conditionality, appears to have relaxed liquidity constraints (Nielsen and Olinto, 2007).

In this article, I explore whether RPS has stimulated such productive investment, examining some of the indirect mechanisms through which the programme might have contributed to a reduction in poverty. Since the programme effectively subsidised human capital investments, it is even possible that the investment portfolio of households would turn away from the sorts of agricultural investments considered here, for example, if child human capital and these other investments are substitutes. At least in part due to this possibility, variations in the design of programmes like RPS that emphasise more directly asset accumulation (via technical and vocational training or small business grants), are also being considered (Macours and Vakis, 2005).

I first examine the effects of RPS on consumption, measured by household expenditures, as well as on a range of outcomes related to productive investment. This is done using a randomised evaluation in which the same households were interviewed both before and after the programme began, in both intervention and control areas. While only limited information was collected on productive activities, the strength of the evaluation design permits a rigorous assessment of several possible agricultural and non-agricultural productive investment behaviours. Second, as an alternative and complementary approach to exploring whether such productive investments are being made, I estimate a consumption equation which
yields the marginal propensity to consume (MPC) out of transfers, as well as the effect of cumulative past transfers, which may have been invested (Gertler et al., 2007).

While there is ample evidence that the programme increased expenditures, there is little evidence that it increased investment on agricultural or other equipment. Moreover, the assessment of the MPC out of transfers shows that a high fraction of the transfers were spent, and cumulative past transfers had no effect on consumption. To some extent, the results are unsurprising, given the programme objectives of increasing food expenditures and improving child human capital. In contrast to the gains made by RPS in human capital development of children (reported elsewhere, for example, Maluccio and Flores, 2005; Gitter and Barham, 2008), the potential for long term increases in consumption resulting from other types of increased productive investment may be limited.

II. Design and Implementation of the Red de Protección Social

Modelled after PROGRESA, RPS was designed to address both current and future poverty through cash transfers targeted to poor households in rural Nicaragua. The transfers were conditional, and households monitored to ensure that, among other things, their children were attending school and making visits to healthcare providers, and that a designated household representative attended a series of health education workshops. When households failed to fulfill these obligations, they lost their eligibility. There were no formal or informal conditions related to productive investments other than (child) human capital.

RPS’s specific stated objectives included:

- supplementing household income for up to three years to increase expenditures on food,
- reducing dropout rates during the first four years of primary school, and
- increasing the healthcare and nutritional status of children under age five.

The programme started in 2000 and comprised two phases over six years. Phase I (the pilot phase) lasted three years with a budget of $11 million, representing approximately 0.2 per cent of GDP (World Bank, 2001). 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 the programme for four more years with a budget of $22 million. In this phase (Phase II), original beneficiaries were phased out of the programme and new beneficiaries were incorporated. After a change in government, the programme was discontinued in 2006.

Programme Targeting

For Phase I of RPS, the government first targeted rural areas in six municipalities of the Central Region, on the basis of poverty as well as on their capacity to implement the programme. The focus on rural areas reflected the distribution of poverty in Nicaragua – of the 48 per cent of Nicaraguans designated as poor in 1998, 75 per
cent resided in rural areas (World Bank, 2001). While not the poorest municipalities in the country, or in the Central Region for that matter, the proportion of impoverished people living in these areas was still well above the national average (World Bank, 2003). In addition, these areas had easy physical access and communication, relatively strong institutional capacity and local coordination, and good coverage of health posts and schools.

In the next 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, all taken from the 1995 national census) in which higher index scores were associated with more impoverished areas. The 42 localities with the highest scores were selected as eligible and form the evaluation area examined in this article. Although the initial programme design called only for geographic-level targeting in these 42 localities (that is, with all resident households eligible), about 6 per cent of households, deemed to have substantial resources, were excluded ex ante from the programme (Maluccio, 2009).

**Programme Design**

RPS had 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 the designated household representative attending bimonthly health educational workshops and bringing their children under age five for scheduled preventive healthcare appointments with specially contracted providers. Children under age two were seen monthly and those between two and five, bimonthly. The workshops were held within the communities and covered household sanitation and hygiene, nutrition, and other related topics.

**Education.** Each eligible household also received a bimonthly cash transfer known as the ‘school attendance transfer,’ contingent on enrolment and regular school attendance of children aged 7–13 who had not yet completed fourth grade of primary school. Additionally, for each eligible child, the household received an annual cash transfer at the start of the school year, intended for school supplies (including uniforms and shoes) known as the ‘school supplies transfer,’ which was contingent on enrolment. Unlike the school attendance transfer, which was a fixed amount per household regardless of the number of children in school, the school supplies transfer was per child.

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 aged 7–13 who had not yet completed the fourth grade of primary school were also eligible for the education component of the programme. The initial US dollar annual amounts and their Nicaraguan Córdoba (C$) equivalents (using the September 2000 average exchange rate of C$ 12.85 to US$ 1.00) were as follows: the food security transfer was $224 a year and the school attendance transfer $112.
On its own, the food security transfer represented about 13 per cent of total annual household expenditures in beneficiary households before the programme. A household with one child benefiting from the education component would have received additional transfers of about 8 per cent, yielding an average total potential transfer of 21 per cent of total annual household expenditures. Over the two years, the actual average monetary transfer was $272 (C$ 3500 or 17 per cent of total annual household expenditures). This is approximately the same percentage of total annual household expenditures as the average transfer in PROGRESA (Caldés et al., 2006). The nominal value of the transfers remained constant, with the consequence that the real value of the transfers declined by about 8 per cent due to inflation over two years in Phase I. In Phase II, which began in 2003 and incorporated new beneficiaries, the size of the demand-side transfers was reduced. The food security transfer started at $168 for the first year of programme participation and then declined to $145 and $126 in the second and third years. The school attendance transfer also declined, to $90 per year but the school supplies transfer rose to $25 per student. These figures represent potential transfers.

To enforce compliance with programme requirements, beneficiaries did not receive the food or education component(s) of the transfer when they failed to carry out any of the relevant conditions described above. Annually, approximately 10 per cent of beneficiaries were penalised at least once and therefore did not receive the food, education, or combined transfers. Only the designated household representative was allowed to collect the transfers and, where possible, RPS appointed the mother to this role. As a result, more than 95 per cent of the representatives were women. RPS also worked with local volunteer coordinators (beneficiary women chosen by the community) to implement the programme. The coordinators were charged with keeping beneficiary household representatives informed about upcoming healthcare appointments for their children, upcoming transfers, and any failures in fulfilling conditions.

Principal Findings from Earlier Quantitative Assessments of Phase I of RPS

Overall, RPS had large positive and significant double-difference estimated average effects on a broad range of indicators and outcomes from 2000 to 2002 (during Phase I), including expenditures, healthcare inputs, nutritional status of children under age five, and school enrolment. Where it did not have significant effects, it was often due to similar, though smaller, improvements in the control areas. Nearly all estimated effects were larger for the extremely poor, reflecting their lower starting points (for example, lower percentages of children enrolled in primary school before the programme). As a result, RPS reduced inequality across expenditure classes for these outcomes (Maluccio and Flores, 2005).

III. Design of the Evaluation and Econometric Methodology

Evaluation Design

The evaluation for Phase I of RPS was based on a randomised, community-based intervention. One-half of the 42 eligible localities were randomly selected into the
programme; thus, there are 21 localities in the ‘original’ intervention group (starting in late 2000) and 21 distinct localities in the ‘original’ control group. The selection was carried out after ordering the localities by the marginality index into seven strata of six localities each, and randomly selecting from each stratum three localities as intervention and three as control.

In mid-2003 (during Phase II), original control localities were incorporated into the programme. Initially, RPS was designed to provide transfers and related supply-side services for a period of three years. During implementation, however, it was decided to extend the supply-side health and education (which included a small transfer to the schools) components for an additional two years, but not the demand-side transfers. As a result, in 2003, as the original control localities were beginning to receive the programme, the demand-side transfers were terminated in the original intervention localities, though households in those areas continued to be eligible to receive the supply-side health and education components through the end of the period examined in this article. In the section on Economic Methodology for Estimating Programme Effects, I discuss the implications for the analysis of this ‘cross-over’ design.

When randomised evaluations are done well, recipients and nonrecipients have, on average, the same observed and, more importantly (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. Even a well-implemented randomised 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) randomisation bias where the process of randomisation itself leads to a different beneficiary pool than would otherwise have been treated; (2) anticipation effects where control group behaviour changes as the result of changes in expectations; or (3) substitution bias where non-beneficiaries obtain similar interventions or services from different sources.

There is little reason to suspect that randomisation bias is a concern in the RPS evaluation. For example, Adato and Roopnaraine (2004) found no evidence that households moved in part or in whole to obtain the programme. As for anticipatory changes in behaviour in the control group, since the programme was targeted to nearly all households in the localities, it is difficult to determine even theoretically what such effects might have been. To the extent that households in original control localities incorporated the probability of receiving the transfers in the future into their decision making, however, the most likely is that, if anything, they would have increased expenditures before the programme began, possibly making the results reported below conservative.

Contamination due to substitution bias can occur when households in control localities participate in programmes similar or complementary to RPS. A delay of an additional year (beyond that originally planned) before incorporating the original control localities may have increased the probability of such contamination, as it gave NGOs and others more time to adjust to the presence of RPS in original intervention localities, for example by pulling their programmes out of those localities or increasing efforts in control localities. While the RPS evaluation was sanctioned by the government and, therefore, plans were coordinated such that other
large government programmes with similar objectives avoided entering these localities over this period, other actors less tied to the government were not prohibited from doing so.

Potential contamination due to other development programmes providing services to households was monitored in annual household surveys. These data allow one to estimate the percentage of households in each locality that were benefiting from an array of possible programmes and services, and to determine whether or not RPS was the provider. Overall, the extent of such development programmes operating in these localities was limited, and there were no substantive changes in programmes directly related to the outcomes assessed in this article, though there is evidence that RPS crowded out to a small extent some programmes related to its primary objectives of education and healthcare (Maluccio and Flores, 2005).

Another limitation with randomised 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). In the case of RPS, the purposive selection of the programme area may have affected programme performance; therefore the external validity is less certain. As described earlier, the selection of municipalities was conditioned on poverty as well as institutional capacity to implement the programme, so that the observed outcomes might exaggerate the likely outcomes from programme expansion to other areas with, for example, weaker capacity or less poverty. On the other hand, the observed outcomes may understate the likely outcomes if there was less need for RPS in the targeted areas possibly even because of greater institutional capacity.

Finally, while it is not possible to claim that the 42 selected localities are statistically representative of rural Nicaragua, there is evidence that they are similar to a large number of other rural areas in the Central Region and elsewhere in the country on a number of dimensions. First, three-quarters of the approximately 150 rural localities in the Nicaraguan departments of Madriz and Matagalpa have marginality index scores in the same range as the programme areas, 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 per cent, the average level in the targeted areas (Maluccio, 2009). On these broad indicators used for geographical targeting, then, there are a large number of similar localities, suggesting those chosen were not atypical.

Data Collection

The data collected for the evaluation included a household panel data survey implemented in both intervention and control localities of RPS before the start of the programme, in 2000, and then afterward 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 evaluation questionnaire was expanded in some areas (for example, child health and education) to ensure that all the programme indicators were captured but cut in other areas (such as income
from all activities and agricultural inputs) to minimise respondent burden and ensure collection of high-quality data in a single interview.  

The household survey sample was a stratified (at the locality level) random sample of all 42 localities using as the sample frame a household census specially collected for RPS in these localities in May of 2000. Forty-two households were randomly selected in each of the 42 localities, yielding an initial target sample of 1764 households. The first wave of fieldwork was carried out in late August and early September 2000. For programmatic reasons, the first follow-up survey was delayed until the beginning of October 2001. The 2002 and 2004 surveys also were carried out in October.

As with any panel survey, first round non-response and latter round attrition in the survey are potential concerns for the analysis. Overall, 90 per cent (1581) of the stratified random sample of 1764 was interviewed in the first round with slightly lower completion in original control localities (87%) compared with original intervention localities (92%). For the follow-up surveys, the target sample was limited to the 1581 first round interviews. In 2002, 88 per cent of these were re-interviewed and in 2004, 85 per cent, with even more similar percentages between original intervention and control localities. The potential for attrition bias in the analysis is examined in the section on Sensitivity to Attrition.

Outcome of the Randomisation

While the selection of localities into intervention and control groups was clearly random, it remains only one of the many possible draws. As a result, original intervention and control localities may still differ in substantive ways due to an ‘unlucky’ random draw, even though this possibility was mitigated with the stratification of localities by marginality index. In this subsection, I provide evidence that the two groups are indeed quite similar for a set of indicators examined in the analyses below (Behrman and Todd, 1999). Perfect ‘equality’ between the two groups is not necessary for the analysis, since for most outcomes considered it is possible to estimate double-differences which control for pre-existing differences between the two groups. Similarity, however, does put the analysis on a sounder footing, particularly if there is the possibility of heterogeneous programme effects related to the initial differences between the groups, as well as for the programme effects estimated using single-difference techniques.

In Table 1, I first compare households in original intervention and control localities on a set of demographic, education, wealth, and other indicators. Of 15 indicators compared in the top panel, none are significantly different at a 10 per cent significance level. Moreover, the magnitudes of the (insignificant) differences in mean values in original intervention and control localities are small for all outcomes (always less than 15% of the overall average).

In the bottom portion of the table, a subset of the outcome variables and the proportion of households classified as extremely poor, poor but not extremely poor, and non-poor are compared. There is no evidence of significant or substantial differences in the expenditure or consumer durable measures between original intervention and control areas. Due to the stratification of localities according to the
### Table 1. Comparison of intervention and control households at baseline 2000

<table>
<thead>
<tr>
<th>Indicator</th>
<th>Intervention (n = 810)</th>
<th>Control (n = 771)</th>
<th>Difference (Intervention – control)</th>
<th>All (n = 1581)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Household size</td>
<td>6.06 (3.02)</td>
<td>6.25 (2.94)</td>
<td>-0.19 (0.23)</td>
<td>6.15 (2.99)</td>
</tr>
<tr>
<td>Number of children &lt; 5 years old</td>
<td>1.10 (1.11)</td>
<td>1.17 (1.04)</td>
<td>-0.07 (0.38)</td>
<td>1.14 (1.08)</td>
</tr>
<tr>
<td>Number of children 5–10 years old</td>
<td>1.13 (1.12)</td>
<td>1.17 (1.19)</td>
<td>-0.04 (0.45)</td>
<td>1.15 (1.16)</td>
</tr>
<tr>
<td>Female-headed household (%)</td>
<td>13.5 (1.2)</td>
<td>14.5 (1.1)</td>
<td>-1.1 (0.23)</td>
<td>14.0 (1.1)</td>
</tr>
<tr>
<td>Age of household head (%)</td>
<td>44.3 (16.1)</td>
<td>43.4 (15.3)</td>
<td>0.89 (0.43)</td>
<td>43.9 (15.7)</td>
</tr>
<tr>
<td>Years of education of household head</td>
<td>1.65 (2.18)</td>
<td>1.61 (2.21)</td>
<td>0.04 (0.78)</td>
<td>1.63 (2.19)</td>
</tr>
<tr>
<td>Average years of education of adults</td>
<td>2.12 (1.87)</td>
<td>2.19 (1.85)</td>
<td>-0.07 (0.73)</td>
<td>2.16 (1.86)</td>
</tr>
<tr>
<td>Number of rooms in the home</td>
<td>1.50 (0.79)</td>
<td>1.49 (0.82)</td>
<td>0.01 (0.88)</td>
<td>1.50 (0.81)</td>
</tr>
<tr>
<td>Toilet (%)</td>
<td>59.0 (5.0)</td>
<td>55.5 (5.1)</td>
<td>3.5 (0.57)</td>
<td>57.3 (0.57)</td>
</tr>
<tr>
<td>Block or brick walls (%)</td>
<td>14.7 (14.7)</td>
<td>13.7 (13.7)</td>
<td>1.0 (0.83)</td>
<td>14.2 (0.83)</td>
</tr>
<tr>
<td>Dirt floor (%)</td>
<td>82.1 (21.4)</td>
<td>82.7 (21.4)</td>
<td>-0.6 (0.88)</td>
<td>82.4 (0.88)</td>
</tr>
<tr>
<td>Electricity (%)</td>
<td>25.4 (25.4)</td>
<td>22.2 (22.2)</td>
<td>3.3 (0.61)</td>
<td>23.8 (0.61)</td>
</tr>
<tr>
<td>Size of land owned (hectares)</td>
<td>1.38 (1.45)</td>
<td>1.45 (1.82)</td>
<td>-0.07 (0.55)</td>
<td>1.41 (1.64)</td>
</tr>
<tr>
<td>Work as agricultural producer (%)</td>
<td>81.0 (81.0)</td>
<td>81.5 (81.5)</td>
<td>-0.5 (0.92)</td>
<td>81.2 (0.92)</td>
</tr>
<tr>
<td>Use credit (%)</td>
<td>14.9 (1.19)</td>
<td>15.4 (1.34)</td>
<td>-0.5 (0.96)</td>
<td>15.2 (1.26)</td>
</tr>
<tr>
<td>Total annual household expenditures (SC)</td>
<td>20387 (12085)</td>
<td>20188 (12878)</td>
<td>199 (0.87)</td>
<td>20290 (12475)</td>
</tr>
<tr>
<td>Per capita annual household expenditures (SC)</td>
<td>4001 (2872)</td>
<td>3755 (2907)</td>
<td>246 (0.36)</td>
<td>3881 (2891)</td>
</tr>
<tr>
<td>Number of consumer durables</td>
<td>1.65 (1.19)</td>
<td>1.64 (1.34)</td>
<td>0.01 (0.96)</td>
<td>1.65 (1.26)</td>
</tr>
<tr>
<td>Value of consumer durables (SC)</td>
<td>395.0 (1155)</td>
<td>362.0 (883)</td>
<td>33.0 (0.69)</td>
<td>378.9 (1031)</td>
</tr>
</tbody>
</table>

#### Expenditure class

<table>
<thead>
<tr>
<th></th>
<th>Extreme poor (%)</th>
<th>Poor (%)</th>
<th>Non-poor (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Extreme poor (%)</td>
<td>34.8 (34.8)</td>
<td>42.0 (42.0)</td>
<td>-7.2 (0.15)</td>
</tr>
<tr>
<td>Poor (%)</td>
<td>76.4 (76.4)</td>
<td>79.5 (79.5)</td>
<td>-3.1 (0.34)</td>
</tr>
<tr>
<td>Non-poor (%)</td>
<td>23.6 (23.6)</td>
<td>20.5 (20.5)</td>
<td>3.1 (0.34)</td>
</tr>
</tbody>
</table>

**Notes:** Standard deviations in parentheses and p-values in brackets for test of equality of populations between groups using standard errors calculated allowing for clustering at the locality level (StataCorp, 2007).
marginality index for randomisation, it is unsurprising that the different poverty
groups are about equally represented in original intervention and control localities.
Further, comparisons of the set of 15 indicators in the top panel between original
intervention and control localities within each poverty group (extreme poor, poor but
not extremely poor, and non-poor) reveal only five significant differences in 45
comparisons, about as would be expected by chance.

Econometric Methodology for Estimating Programme Effects

Household-level panel data were collected in both the intervention and control
localities before and after RPS was implemented. This enables the use of the double-
difference method to estimate the average programme effect (Duflo et al., 2008;
Ravallion, 2008). The basic estimating equation employing double-differences
and incorporating household-level fixed effects is:

\[ Y_{ict} = \alpha_0 + \alpha_1 D_{2001} + \alpha_2 D_{2002} + \alpha_3 D_{2004} + \delta_{d1} P_{c,2001} + \delta_{d2} P_{c,2002} + \delta_{d4} P_{c,2004} + \mu_{ic} + \varepsilon_{ict} \]  

where \( Y_{ict} \) = outcome variable of interest for household \( i \) in locality \( c \) at time \( t \), \( D_t = 1 \)
if year \( t \), for \( t = 2001, 2002, \) and 2004, \( P_{ct} = 1 \) if programme intervention in locality
\( c \) at time \( t \), this changes over time under the cross-over design, \( \mu_{ic} = \) all household-
level time-invariant factors for household \( i \) (the household-level fixed effect), \( \varepsilon_{ict} \) =
the unobserved idiosyncratic household- or locality-level, time-varying error,
and all the \( \alpha \)s and \( \delta \)s are unknown parameters.

The parameters of interest are \( \delta_{d1}, \delta_{d2}, \) and \( \delta_{d4}; \delta_{d1} \) is the double-difference
estimator of the average programme effect for 2001 (relative to 2000), \( \delta_{d2} \) for
2002 (relative to 2000), and \( \delta_{d4} \) for 2004 (relative to 2000). For 2004, however,
since the original control localities had entered the programme by this time and
the original intervention localities had ceased to receive cash transfers about 10
months earlier, \( \delta_{d4} \) is the estimated effect of having had no programme for three
years and the full programme (with slightly lower transfers as described in the
section on Programme Design) for the previous year, compared to having had
the full programme for three years and the supply-side components only for the
fourth year. Given the randomisation of \( P_{ct} \), it (and any interactions involving it)
is uncorrelated with all observed or unobserved household- or locality-level
variables so that the \( \delta \)s can be estimated consistently and be given a causal
interpretation.

For some outcomes examined below, for which information was not collected in
the baseline survey but only in 2002 and 2004, it is only possible to estimate single-
difference equations, that is, the difference between intervention and control
localities after the programme began. Given the randomised design and the evidence
that original intervention and control localities were similar before the programme
began, this is a valid estimate of the average programme effect. The estimating
equation simplifies to (2) where elements common to both equations are defined
analogously and \( \delta_{e2} \) and \( \delta_{e4} \) represent the single-difference estimates of the average
programme effect corresponding to \( \delta_{d2} \) and \( \delta_{d4} \) above. As it is no longer possible to
include household-level fixed effects in this specification, I control instead for a set of baseline demographic and education characteristics of the household (vector $X_{ic}$), measured in 2000 before the programme began. One reason for adding these controls is that they are likely to improve the precision of the estimates. A second reason is to help mitigate possible attrition biases.

$$Y_{ict} = \alpha_0 + \alpha_4 D_{2004} + \delta_2 P_{c;2002} + \delta_4 P_{c;2004} + X_{ic} \theta + \epsilon_{ict}$$ (2)

Neither of the above specifications condition on the household-level decision to participate in RPS but instead only on whether the programme was available in the household’s locality. As a result, they estimate what is typically referred to as the (single- or double-difference) ‘intent-to-treat’ average programme effect. Such estimates are not subject to selection biases associated with the decision to participate in the programme since they rely only on the randomised design. Approximately 10 per cent of the households in the intervention localities, however, were either excluded by RPS or chose not to participate in the programme. Therefore, specifications estimating instead the ‘treatment-on-the-treated’ average programme effect differ only slightly. In all the analyses presented, households interviewed in any of the relevant survey rounds, that is, the unbalanced panel samples, are included. Standard errors are calculated allowing for clustering at the locality level (Statacorp, 2007).

IV. The Effects of Conditional Cash Transfers on Consumption and Investment

Household Expenditures

Despite strong growth in the late 1990s and in 2000 when gross domestic product grew nearly 13 per cent, Nicaragua had the lowest per capita GDP in Central America. Subsequent slow growth meant that income per capita was essentially stagnant over 2000–2002 (World Bank, 2004). Moreover, the Central Region experienced worsening poverty between 1998 and 2001, a pattern attributed in part to the decline in coffee prices which affected many of the agricultural labourers as well as a drought in 2001 (Varangis et al., 2003; World Bank, 2003; Vakis et al., 2005). Within the 42 localities selected for the programme evaluation, about 40 per cent of the population was extremely poor before the programme – that is, their per capita annual household expenditures were less than the amount necessary to purchase a food basket providing minimum caloric requirements (World Bank, 2003) – and 80 per cent extremely poor or poor. Finally, the majority of the remaining households, or non-poor in the sample, was in the bottom two-thirds of the national Nicaraguan per capita annual household expenditure distribution and so was nearly poor.

In the left-hand panel of Table 2, I present the intent-to-treat double-difference estimated average programme effects of RPS on expenditures. The 2001 and 2002 annual dummy variables for total, per capita total, and per capita food expenditures show about a 10 per cent reduction for the original control localities relative to 2000, consistent with the economic downturn in the region. The coefficient on the 2004 annual dummy (which captures the difference in original intervention areas from
Table 2. The double-difference estimated programme effect on expenditures and consumer durables, 2000–2004

<table>
<thead>
<tr>
<th></th>
<th>Total expenditures (C$)</th>
<th>Per capita total expenditures (C$)</th>
<th>Per capita food expenditures (C$)</th>
<th>Education expenditures (C$)</th>
<th>Number of consumer durables&lt;sup&gt;a&lt;/sup&gt;</th>
<th>Value of consumer durables (C$)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Year 2001</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>-2585**</td>
<td>-507**</td>
<td>-423***</td>
<td>175***</td>
<td>0.104</td>
<td>98</td>
</tr>
<tr>
<td></td>
<td>(1065)</td>
<td>(195)</td>
<td>(148)</td>
<td>(50)</td>
<td>(0.105)</td>
<td>(83)</td>
</tr>
<tr>
<td><strong>Year 2002</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>-2138*</td>
<td>-367*</td>
<td>-368**</td>
<td>214***</td>
<td>0.136</td>
<td>45</td>
</tr>
<tr>
<td></td>
<td>(1085)</td>
<td>(193)</td>
<td>(159)</td>
<td>(49)</td>
<td>(0.101)</td>
<td>(38)</td>
</tr>
<tr>
<td><strong>Year 2004</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>3941***</td>
<td>1003***</td>
<td>500***</td>
<td>371***</td>
<td>0.461***</td>
<td>366**</td>
</tr>
<tr>
<td></td>
<td>(1009)</td>
<td>(180)</td>
<td>(124)</td>
<td>(68)</td>
<td>(0.085)</td>
<td>(161)</td>
</tr>
<tr>
<td><strong>Year 2001 × RPS beneficiary (DD)</strong></td>
<td>3941***</td>
<td>905***</td>
<td>789***</td>
<td>140*</td>
<td>-0.010</td>
<td>-183*</td>
</tr>
<tr>
<td></td>
<td>(1344)</td>
<td>(260)</td>
<td>(219)</td>
<td>(75)</td>
<td>(0.131)</td>
<td>(107)</td>
</tr>
<tr>
<td><strong>Year 2002 × RPS beneficiary (DD)</strong></td>
<td>2670*</td>
<td>676**</td>
<td>621***</td>
<td>321***</td>
<td>0.055</td>
<td>-103</td>
</tr>
<tr>
<td></td>
<td>(1425)</td>
<td>(264)</td>
<td>(211)</td>
<td>(73)</td>
<td>(0.120)</td>
<td>(77)</td>
</tr>
<tr>
<td><strong>Year 2004 × RPS beneficiary (DD)</strong></td>
<td>4142***</td>
<td>882***</td>
<td>653***</td>
<td>206**</td>
<td>-0.028</td>
<td>-164</td>
</tr>
<tr>
<td></td>
<td>(1425)</td>
<td>(311)</td>
<td>(229)</td>
<td>(103)</td>
<td>(0.162)</td>
<td>(196)</td>
</tr>
<tr>
<td><strong>Constant</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>20423***</td>
<td>3875***</td>
<td>2669***</td>
<td>333</td>
<td>1.656***</td>
<td>387***</td>
</tr>
<tr>
<td></td>
<td>(424)</td>
<td>(76)</td>
<td>(64)</td>
<td>(21)</td>
<td>(0.041)</td>
<td>(34)</td>
</tr>
<tr>
<td><strong>F-statistic</strong></td>
<td>64.2***</td>
<td>28.8***</td>
<td>20.0***</td>
<td>30.2***</td>
<td>7.1***</td>
<td>2.6**</td>
</tr>
</tbody>
</table>

Notes: Intent-to-treat household-level fixed-effects models estimated on unbalanced household sample of N = 5777. Standard errors allowing for clustering at the locality level shown in parentheses (Statacorp, 2007). ***Denotes significance at 1, ** at 5, and * at 10 per cent level.

a. Consumer durables include the following items: radio, sound system (tape recorder or stereo), television, video player, refrigerator, stove, iron, maize grinder, fan, toaster, mixer, microwave, air conditioner, sewing machine, typewriter, computer, bicycle, motorcycle, and car.
(2000 to 2004), however, suggests a gain of similar magnitude, consistent with an economy-wide recovery but also with possibly persistent increased expenditures for this group. In 2001 and 2002, when the original control group had not yet received the programme, the estimated programme effects were large and significant for all the expenditure categories considered.\textsuperscript{11} For comparison, the average value of annual cash transfers for beneficiary households over the period 2000–2004 was approximately C$ 3500 (C$ 750 on a per capita basis). Thus the estimated programme effect on total annual household expenditures was more than C$ 400 above average transfers in 2001, but below them (by approximately C$ 800) in 2002. In both years, however, effects were of the same order of magnitude as average transfers and in neither year can one reject the null hypothesis that the estimated programme effect is the same as the average annual transfer amount for that year.

One possible explanation for the relatively large effects is that with the economic downturn and associated decline in incomes, intervention households’ marginal utility of current consumption increased, leading them to spend a larger proportion of the transfers than they would have otherwise. Regardless, it is clear that the programme was effective in increasing total annual household expenditures, at least in the short term. In 2004, the estimated effect (comparing the original control localities that in 2004 had begun to receive the programme, compared to the original intervention localities that were no longer receiving transfers but continuing to receive the supply-side benefits) was even larger. The size of this effect suggests the possibility that household expenditures did not undergo a permanent increase in the original intervention localities as a result of the programme, but rather that the increase seen over time in those areas (compared with 2000) was the secular trend, a possibility I explore further in the section on The Marginal Propensity to Consume Out of Transfers. It also suggests that the large effects were not due to the economic downturn, since by 2004 the economy had recovered substantially.

The estimated programme effects shown in the left-hand panel of Table 2 also suggest that beneficiary households were spending a large proportion of their transfers on food. RPS led to significant increases in per capita annual food expenditures of about C$ 790 in 2001, C$ 620 in 2002, and C$ 650 in 2004. These increases were approximately 75–90 per cent of the average effects on per capita annual household expenditures. Consistent with RPS goals and the agreement signed by beneficiaries (in which the first ‘condition,’ though not officially monitored, was that they would use the transfer for improved nutrition and schooling of their children), additional expenditures as a result of the transfers were predominantly for food. Recommendations that the transfers be spent on food were emphasised during incorporation into the programme and in the health education workshops. In addition, Adato and Roopnaraine (2004) present evidence that some of the volunteer coordinators took this informal aspect of the programme quite seriously, asking to see receipts after transfers had been made, though it is not possible to gauge how widespread this practice was.

Another key component of the programme was education. Estimated average effects of RPS on educational expenditures were also significant; for example, in 2002 the effect was about C$ 320, slightly larger than the per student value of the educational supplies transfer in Phase I (C$ 275). The observed increases in expenditures on food and education are also consistent with evidence that additional
resources in the hands of women (recall that 95% of the designated household representatives, the only persons eligible to receive the transfers, were women) often lead to increased expenditures in these areas (Thomas, 1990; Schady and Rosero, 2007; Gitter and Barham, 2008).

**Household Consumer Durables and Economically Productive Assets**

To this point, the evidence suggests that households were closely following the programme recommendations. Expenditures increased substantially with the programme, as was the case in other contexts, such as PROGRESA (Hoddinott and Skoufias, 2004), and possibly more than the amount of transfers given (at least in the first year, 2001 for the original intervention group and 2004 for the original control group). Such increases, however, are consistent with different underlying behaviours. For example, it is possible that investments made with the transfers were yielding (short-term) positive returns that enabled increased expenditures. Alternatively, it could be that households were spending nearly all of their transfers on consumption (and even borrowing against future transfers) and investing or saving little. If the latter were the case, then one would expect to find the MPC out of transfers to be close to one. In this and the following subsection, I explore whether and how much households were (1) investing transfers (savings are not observed) or (2) changing the allocation of their time and resources in different productive activities. The questionnaire asked about various forms of investment, broadly defined, such as on consumer durable goods, agricultural or non-agricultural machinery and equipment, and livestock.12

First, although current expenditures on consumer durables were included in the expenditure analysis, here I directly explore the effect of the programme on ownership and value of consumer durables, many of which might make individuals in the household more productive (allowing more time for other productive activities or leisure) and a few of which might themselves be used in productive activities. Ownership of most of the items asked about before the programme was rare – 2 per cent or less – with the exception of radios, sound systems, televisions, irons, maize grinders, and bicycles (see Table 2 for the complete list). There is little evidence that the programme led to increased ownership of either the number or value of consumer durables (Table 2, right-hand panel) – ownership (and value) increased over time, but apparently not as a result of the programme.13 Indeed, there is some suggestion that the programme might have even led to a small decrease in the value of consumer durables owned in 2001, though this finding is not robust to a logarithmic transformation of the value.

When I examine instead an array of productive goods associated with agricultural activities, there is again little evidence of increased investment. Questions about these assets were not asked in the baseline or 2001 rounds of the survey; Table 3 presents the single-difference estimated average programme effects, controlling for baseline characteristics of the households. The first two columns show only limited evidence of a positive programme effect on the number or value of productive agricultural items owned, including ploughs, water pumps, sprayers, tools, and carts. There is some evidence of a statistically significant positive programme effect on the number of items in 2002, though it is small and not corroborated by the reported value of
Table 3. The single-difference estimated programme effect on agricultural and non-agricultural investments, and on animal ownership, 2002 and 2004

<table>
<thead>
<tr>
<th></th>
<th>Number of productive agricultural goods</th>
<th>Value of productive agricultural goods</th>
<th>(1) if any expenditure on agricultural or non-agricultural equipment</th>
<th>Expenditures on agricultural or non-agricultural equipment</th>
<th>Number of types of animals owned</th>
<th>Value of all animals owned</th>
<th>Tropical livestock units</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Year 2002 × RPS</strong></td>
<td>0.086*</td>
<td>3.4</td>
<td>0.030</td>
<td>3.1</td>
<td>−0.032</td>
<td>−110.1</td>
<td>−0.019</td>
</tr>
<tr>
<td><em>RPS beneficiary</em></td>
<td>(0.050)</td>
<td>(47.4)</td>
<td>(0.019)</td>
<td>(2.4)</td>
<td>(0.103)</td>
<td>(459.0)</td>
<td>(0.111)</td>
</tr>
<tr>
<td>(1st Diff)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Year 2004 × RPS</strong></td>
<td>−0.023</td>
<td>−18.2</td>
<td>−0.037*</td>
<td>−5.8</td>
<td>−0.008</td>
<td>208.4</td>
<td>−0.079</td>
</tr>
<tr>
<td><em>beneficiary (1st Diff)</em></td>
<td>(0.057)</td>
<td>(91.5)</td>
<td>(0.019)</td>
<td>(3.4)</td>
<td>(0.106)</td>
<td>(959.4)</td>
<td>(0.140)</td>
</tr>
<tr>
<td>F-statistic</td>
<td>16.8***</td>
<td>14.1***</td>
<td>1.6</td>
<td>1.6</td>
<td>6.9***</td>
<td>3.8***</td>
<td>3.5***</td>
</tr>
</tbody>
</table>

*Notes: Intent-to-treat models estimated on unbalanced household sample of N = 2743 from years 2002 and 2004. In addition to a constant, a year 2004 dummy variable, and annual interactions with the program placement dummy, household controls include baseline measures of: 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), an indicator of whether the household head was female, and age of the household head (and its square). Standard errors calculated allowing for clustering at the locality level shown in parentheses (StataCorp, 2007). ***Denotes significance at 1, **at 5, and *at 10 per cent level.

a. Productive agricultural goods include: animal implements including ploughs, water pumps, sprayers, tools, and carts.
b. Whether there were expenditures on such equipment as agricultural or other tools and machines in the previous 12 months.
c. The three (broad) types of animals considered included cattle, work animals (horses and mules), and poultry.
d. Tropical Livestock Units calculated using Central American-specific weights for cattle [0.7], horses [0.50], pigs [0.25], and poultry [0.01] (Zezza et al., 2007).
those items. A similar lack of effects is seen in the third and fourth columns where I consider an alternative approach that examines the estimated programme effect on expenditures on agricultural or industrial equipment made in the previous 12 months. If anything, those entering the programme in 2004 were less likely to have made these sorts of expenditures.

Another possibility is that households were expanding their livestock activities. There is no evidence, however, that the programme affected investment in animal husbandry, as shown in the right-side panel of Table 3. In addition to the indicators considered in the table, there were no significant programme effects on: (1) whether a household had a certain type of animal (for example, sheep, goats, etc.); (2) the number of animals of each type; or (3) the value of animals of each type.

### Non-agricultural Micro Enterprises

The final dimension along which I consider changes in productive activities is household engagement in non-agricultural micro enterprise activities (Table 4). Such self-employment activities are potentially important since some research suggests that rural self-employment is associated with reductions in poverty at the household level in Nicaragua (Davis and Stampini, 2002; Davis and Murgai, 2003). For such activities, it is again possible to carry out double-difference estimation since information was collected in all survey years. In 2000, just over 10 per cent of the sample had some sort of non-agricultural enterprise related to producing and selling

<table>
<thead>
<tr>
<th></th>
<th>Year 2001</th>
<th>Year 2002</th>
<th>Year 2004</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1) if non-agricultural home production for sale</td>
<td>-0.003 (0.011)</td>
<td>0.017 (0.015)</td>
<td>-0.001 (0.015)</td>
</tr>
<tr>
<td>(1) if resell/retail purchased goods</td>
<td>0.055 (0.050)</td>
<td>-0.019 (0.018)</td>
<td>-0.040 (0.027)</td>
</tr>
<tr>
<td>(1) if sell services of some sort (not day labour)</td>
<td>-0.002 (0.003)</td>
<td>0.001 (0.004)</td>
<td>0.004 (0.005)</td>
</tr>
<tr>
<td>Constant (DD)</td>
<td>0.044*** (0.006)</td>
<td>0.089*** (0.012)</td>
<td>0.005*** (0.002)</td>
</tr>
<tr>
<td>F-statistic</td>
<td>0.8</td>
<td>2.8**</td>
<td>1.8</td>
</tr>
</tbody>
</table>

**Notes:** See notes to Table 2.
non-agricultural goods, reselling or retailing goods, or selling specialised services (such as tailor services), and during the evaluation period, the overall percentage declined slightly. For 2001 and 2002, the estimated programme effects on these activities were generally negative and in some cases statistically significant. In 2004, there were no significant effects. While the evidence is mixed, if anything, rather than spurring such micro enterprise activities, RPS appears to have provided a disincentive to participate in them. It is possible that the emphasis on child schooling reduced the available labour pool for such activities (for example, by raising the shadow wage for children). It is also possible that in the rural areas where the programme operated, characterised by relatively poor transportation infrastructure, such activities only yield low marginal returns.

The Marginal Propensity to Consume out of Transfers

The previous subsections provided ample evidence that RPS led to increased expenditure, but only minimal evidence that there was increased economically productive investment of the types examined. An indirect approach to exploring the latter possibility further is to examine whether households spent their transfers predominantly on current expenditures, estimating the MPC out of transfers (hereafter, MPC). If the estimated MPC is close to one, for example, it would suggest that most spending was on current consumption and that there was little saving or investment, as these are not included in the expenditure measures. Because income and saving are not observed, I follow Gertler et al. (2007) and posit a consumption equation in which expenditures are a function of actual current transfers, past actual accumulated transfers, initial assets and demographics (to control for family labour), and productivity shocks (captured in the error term). Within this framework, initial assets and demographics are the determinants of permanent income, obviating the need to control for income directly. The estimating equation is:

\[
C_{ict} = \alpha_0 + \alpha_1 D_{2001} + \alpha_2 D_{2002} + \alpha_4 D_{2004} + \beta_1 T_{ict} + \beta_2 \sum_{s=2001}^{s=t-1} T_{ics} + X_{ic} \theta + \epsilon_{ict} \tag{3}
\]

where, in addition to the terms already defined above, \(C_{ict} = \) total annual household expenditures for household \(i\) in locality \(c\) at time \(t\), \(T_{ict} = \) actual amount of current (or same-year) transfers to household \(i\) in locality \(c\) in year \(t\), \(X_{ic} = \) vector of baseline (2000) characteristics listed in Table 3, and all the \(\alpha\)s, \(\beta\)s, and (vector) \(\theta\) represent unknown parameters.\textsuperscript{14} The summation term represents actual cumulative past RPS transfers to household \(i\) up to, but not including, year \(t\)’s actual transfers (so that \(s\) ranges from 2001 to 2004). Households in original intervention localities had zero actual cumulative past transfers in 2000 and 2001, \(T_{ic},2001\) in 2002, and \((T_{ic},2001 + T_{ic},2002 + T_{ic},2003)\) in 2004. Those in original control localities had zero actual cumulative past transfers in all years but 2004, when they were \(T_{ic},2003\).\textsuperscript{15} Thus \(\beta_1\) represents the MPC and \(\beta_2\) the increase in consumption from investment of previous transfers. It is possible for \(\beta_1\) to be one and yet for \(\beta_2\) to be positive, if expenditures made in earlier periods yield positive returns.

Since it depends both on the household-level programme participation decision, as well as fulfilment of the programme requirements, the actual (and thus cumulative)
amount of transfers received is endogenous and may be correlated with total expenditures for reasons other than the programme. Therefore, I present instrumental variables estimates using potential same-year and potential cumulative past transfers as instruments for actual same-year and actual cumulative past transfers. Potential transfers are calculated using the specific characteristics of each eligible household in the sample in each period. For example, a household with two school-age children eligible for the programme would be eligible for the food security, school attendance, and two school supplies transfers (Programme design section).

Results, both with and without actual cumulative past transfers, are presented in Table 5. Controlling for the endogeneity of actual transfer amounts had only a small effect, increasing the estimated MPC coefficients by 5–10 per cent compared with results which do not endogenise transfers (not shown). When actual transfers are included but not cumulative transfers, the estimated MPC is greater than one, 1.037. This coefficient estimate raises the possibility that, despite the limited evidence on investment in previous sections, transfers had a small multiplier effect on consumption expenditures, similar to (but smaller than) the income multiplier effects found in Mexico (Sadoulet et al., 2001). For at least two reasons, however, such a multiplier effect seems unlikely. First, the estimated coefficient for MPC is not statistically different from 1.0. Second, when the effect of transfers is allowed to vary by year of participation in the programme (which is not the same for all households given the cross-over design), a striking pattern emerges. In column two, the second row represents the MPC estimated from transfers made during the first full year that a household was a programme beneficiary. For Phase I households, this was 2001 and for Phase II households, 2004. The MPC during this year was higher still, 1.17, though again not statistically different from 1.0 (p = 0.19 from a two-sided t-test). In later years, however, the MPC drops by nearly 50 per cent to 0.610, well below and statistically different from one (p = 0.09). Results are nearly identical if the year 2004 observations, when Phase II households began receiving full transfers, are excluded. Gertler et al. (2007) report a similar pattern of declining MPC with programme tenure for PROGRESA, and present a conceptual model allowing them to interpret it as reflecting a decline in liquidity constraints. Upon entering the programme, households are more likely to be liquidity constrained such that their current MPC is initially high but then declines over time with continued transfers.

According to the second column of Table 5, first-year beneficiary households spend even more than the actual value of transfers received. It is possible that households make use of the additional resources provided by their beneficiary status, including transfers projected to last for three years, and spend accordingly, perhaps satisfying pent-up demand via borrowing. This is supported by the observation that the MPC decreases substantially in the second year. The results are also consistent with delays that occurred in the distribution of transfers. In the first year of Phase I, only five out of a scheduled six transfers were distributed before the 2001 household survey, due to delays outside the programme’s control (the final transfer was made later). If households planned to spend the entire transfer amount for that year (and could borrow, for example from shopkeepers, against future transfers), we would see a spending rate of 6/5 or 1.2 for every Córdoba transferred, nearly identical to the estimated coefficient for the first year.
Because there are more observations in the sample that were first-year programme beneficiaries, the MPC estimate in the first column (1.037) is not the simple average of the two MPC estimates in the second column. The simple average of the two coefficients would imply an overall MPC of approximately 0.90, similar to that estimated by Gertler et al. (2007). Since it was clear to households (as part of programme enrolment) that the transfers were to last for three years (and thus were not permanent), this suggests households closely adhered to programme recommendations regarding the use of transfers, as estimates for developing countries for the MPC out of transitory income are typically much lower, below 0.50 (Paxson, 1992).

Another possibility relevant to the first intervention group is that households spent more out of transfers during the economic downturn than they otherwise would have. This would have the effect seen in the results that spending out of the three years of transfers does not seem to have been smoothed over a longer period. At odds with this possibility, however, is that the results for MPC for the original control group, when they began to receive transfers in 2004 after the economic decline had ended, are similar.

The final column presents results including actual cumulative past transfers. If some of those transfers were invested in income-generating activities that had generated short-term returns leading to increased expenditures, then $\beta_2$ would be positive and significant. This does not seem to have been the case; actual cumulative past transfers had no discernable effect on current expenditures. While it is possible that investments undertaken might have required a longer period than covered here (up to three years in the case of the original intervention group) to yield returns, combined with the limited evidence on investments in the earlier sections, this result makes it seem likely that there were no such strong effects.

<table>
<thead>
<tr>
<th>Total expenditures</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Actual transfer</td>
<td>1.037***</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(1st year beneficiary)</td>
<td>(0.095)</td>
<td>1.173***</td>
<td>1.204***</td>
</tr>
<tr>
<td>Actual transfer</td>
<td></td>
<td>0.610***</td>
<td>0.591***</td>
</tr>
<tr>
<td>(later year beneficiary)</td>
<td>(0.134)</td>
<td>(0.160)</td>
<td>(0.209)</td>
</tr>
<tr>
<td>Past actual cumulative transfers</td>
<td>0.004</td>
<td>(0.142)</td>
<td></td>
</tr>
</tbody>
</table>

Notes: Instrumental variables models estimated on unbalanced household sample of $N = 5777$. Actual transfers and past actual cumulative transfers treated as endogenous and their potential transfer counterparts used as instruments. Regression also includes year dummies for 2001, 2002, and 2004, all the household controls listed in Table 3, and a constant (not shown). Standard errors calculated allowing for clustering at the locality level shown in parentheses (Baum et al., 2007). ***Denotes significance at 1 per cent level.
Previous evaluations of RPS show that for a number of indicators the programme was more effective for the extreme poor and poor, for example with larger estimated average programme effects on expenditures for the extreme poor and poor, compared with the non-poor (Maluccio and Flores, 2005). While this continued to be the case for the estimated effects on expenditures presented in Table 2, it was not true for the other outcomes examined – results (not shown) for productive investments and the MPC out of transfers did not differ by baseline poverty groups, in contrast to PROGRESA in Mexico (Gertler et al., 2007; Todd et al., 2010).

Sensitivity to Attrition

Although levels for the completion rate of the survey at baseline and subsequent attrition are on a par with similar surveys in other developing countries (Alderman et al., 2001; Thomas et al., 2001), what is of ultimate concern in this analysis is not the level of attrition, but whether, and to what extent, the attrition invalidates the inferences made using these data. The principal reasons for failure to interview targeted sample households were that household members were temporarily absent (that is, more than the several days the survey team was in the area) or that the dwelling appeared to be uninhabited – both of which are likely to be associated with temporary or permanent migration. It is possible, then, that the advantages of randomisation are dissipated with such non-random attrition. I address concerns about sample attrition bias in two ways, concluding that it is not a major concern for estimating RPS programme effects with these data.

First, in results not shown, I compare average characteristics of attritors and non-attritors and examine the correlates of the observed attrition (Thomas et al., 2001). A comparison of those re-interviewed with those who eventually attrited for all the variables shown in Table 1 yielded some evidence of statistically significant, though generally small, differences between the groups. Separate probit regressions on all original target sample households predicting the probability of having been interviewed in each of the follow-up survey rounds corroborate the findings based on the comparison of means; unsurprisingly, attrition appears to have been non-random. Households with older, more educated household heads, larger family size (in particular, adults), and higher pre-programme expenditures were more likely to have been interviewed. It is in part for this reason that I control for household fixed effects (or demographic and education characteristics when fixed effects are not feasible) in the analyses. This estimation strategy, while not formally incorporating attrition, is likely to be robust to attrition bias, particularly if (observed or unobserved) persistent heterogeneity is leading to attrition. Such heterogeneity is directly controlled for in the household-level fixed-effects regressions and somewhat controlled for in the other estimates which include the large set of baseline controls.17

For the analyses presented in this article, of particular concern is whether attrition is associated with the intervention in ways that are not controlled for but have direct effects on the outcomes examined. To explore this possibility, I next incorporate into the attrition probits described above an indicator of whether the household was in an intervention area (P_{ct}). For attrition in 2001, the estimated coefficient on P_{ct} is statistically significant, indicating a higher attrition rate (6 percentage points) in
control areas. In each of 2002 and 2004, however, the difference is less than 2 percentage points and insignificant. Finally, when I add to these probits interactions of $P_{ct}$ with each of the baseline control variables, in all three regressions the set of interactions of $P_{ct}$ with the baseline variables is jointly insignificant. These findings suggest that attrition in the sample was not systematically related to the intervention.18

V. Conclusions

I use a randomised community-based evaluation to explore the extent to which a conditional cash transfer programme in Nicaragua led to economically productive investments other than those promoted by the primary programme objective, improved child human capital. This was done by assessing programme effects on both consumption and investment, during a period marked by economic decline and then recovery. The estimates presented are the overall average programme effects, combining supply- and demand-side components of the programme. They represent the short-term effects of the programme (after one, two, or four years), although some of the outcomes examined are themselves long-run indicators, such as investment in productive goods.

An important question is the extent to which effects of RPS will persist after the programme exited (in late 2006). This includes an assessment of whether there might be longer-term effects due to a changing investment portfolio, for example including more or less agricultural investment. In late 2003, RPS delivered the final demand-side transfers to households in the original intervention localities, though it continued offering supply-side health and education services until the end of 2005. At the same time in 2003, the original control group became beneficiaries of the programme. Continued survey work in 2004 provided information to examine the effects of that transition and understand better the sustainability of the changes achieved by RPS.

In years when transfers were being given, the programme increased expenditures, and the lion’s share of the increase was on food and educational expenditures. These findings are consistent with the programme’s orientation toward increasing current expenditures (one of its three key objectives) and the required conditions, as well as with evidence from a variety of settings that resources in the hands of women are more likely to be directed to these types of expenditures. With those findings, I turned to an assessment of the programme on investment of various types. Overall, there was only limited evidence that the programme led to increases in the agricultural and non-agricultural types of investment considered. These results are corroborated by a separate analysis estimating a consumption equation, which demonstrated that even though the transfers were to last only for three years and thus were transitory, the average MPC out of transfers was approximately one. Moreover, cumulative past transfers had no effect on current expenditures.

The findings do not imply that the programme had no long-term effects—it very likely did via increased investment in child health and education, which should continue to lead to benefits for many years to come. In contrast to PROGRESA in Mexico, where there seems to have been substantial agricultural investment and
returns from it (Gertler et al., 2007; Todd et al., 2010), there is only weak evidence that RPS increased these other investments in the rural localities in which it operated.

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Notes

1. Census comarcas are administrative areas within municipalities that typically include several small communities.
2. The value of the supply-side services, as measured by how much RPS paid to the providers, was also substantial – approximately $50 for the education workshops and $110 for the healthcare services for children under age five, per beneficiary household per year.
3. LSMS surveys are typically implemented in two visits to the household (Grosh and Glewwe, 2000).
4. These and other descriptions of poverty in the sample are calculated based on 2001 per capita annual expenditure poverty lines of $202 (SC 2691) for extreme poor (calculated as the amount required to purchase a minimum requirement food basket) and $386 (CS 5157) for poor, which adds nonfood requirements (World Bank, 2003). Households are classified into poverty groups based on their per capita annual total household expenditures (including own production) measured before the programme in 2000.
5. The household-level fixed effects control for locality-level fixed effects as well. Household-level fixed effects are automatically controlled for in the case where (1) is estimated on a balanced panel data set, but not when using an unbalanced panel. The ‘main’ effect, Pct, does not appear in (1) as it is subsumed by the household-level fixed effects.
6. I considered two additional estimation approaches, household-level random effects and weighted regression, to verify the robustness of the results reported and discussed below. Because of the randomised design, a possible alternative is to estimate a household-level random effects model. In some instances, however, (for example, total expenditures), a Hausman test (Wooldridge, 2002) rejected the equality of coefficients across the household-level fixed- and random-effects models. For this reason, I present fixed-effects estimates throughout, even though according to the Hausman test, random effects were acceptable for some outcomes. The reported results disregard the stratified sample design which can be corrected for statistically by using locality-level sample weights (Deaton, 1997); correcting for this aspect of the design (instead of controlling for household-level fixed effects) made no substantive changes to the results.
7. Including 2004 does not change the double-difference results or interpretation for 2001 or 2002.
8. To estimate treatment-on-the-treated effects, Pct is replaced throughout with a dummy variable indicator of actual participation by that household, Pct, and household-level programme participation is endogenised by using Pct as instrumental variables for the household participation decision (Pct). Since household participation rates are high, the instrumental variables approach yields estimates similar to, though in nearly all cases slightly larger than, the intent-to-treat estimates, with identical patterns of significance.
9. Except where noted in the text, results are similar if I instead estimate using logarithmic transformations of expenditure or value variables.
10. I use nominal Córdoba figures to facilitate direct comparison with transfer sizes; all regressions incorporate the relevant year dummy variables, capturing inflationary and other trends common to
both intervention and control localities. Changing to real figures does not alter the substantive findings, though the secular time trends are less sharp.

11. I focus on total expenditures in the discussion since there is evidence that the programme influenced migration (Stecklov et al., 2007) which complicates the interpretation on the per capita measures, though it is less of a concern for the comparisons across per capita measures made in the text.

12. There were some limited questions about agricultural land use, but since they were not similar across survey rounds I do not examine them. There were no questions on the types of agricultural inputs analysed in Sadoulet et al. (2001).

13. Results examining the ownership of the set of more common individual items (estimated via seemingly unrelated regression, as suggested in Duflo et al., 2008) also show little evidence that the programme led to increased ownership of consumer durables.

14. Results are unchanged if I estimate using household fixed effects rather than the set of baseline controls.

15. Although there was no household survey in 2003, the transfers made to original control households that year are observed from an RPS administrative data source.

16. F-statistics on the excluded potential transfer and past cumulative transfer instruments are over 100 for all first-stage regressions (Bound et al., 1995).

17. Also, separate estimates based on a balanced panel data set, including only those 1259 households interviewed in all four years, were very similar to those based on the unbalanced sample, suggesting that selection related changes in sample composition (due to attrition) are not driving the results.

18. Bounds tests, along the lines suggested by Duflo et al. (2008) were also evaluated, but did not substantively change the reported finding.

References


StataCorp (2007) *Stata statistical software: Release 10.0* (College Station, Texas: Stata Corporation).


