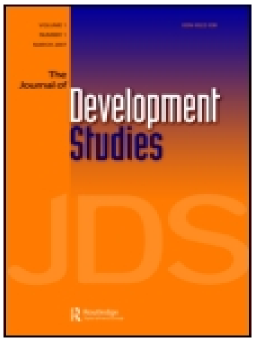

The Journal of Development Studies

ISSN: 0022-0388 (Print) 1743-9140 (Online) Journal homepage: http://www.tandfonline.com/loi/fjds20

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The Journal of Development Studies, 2013
Vol. 49, No. 10, 1397–1411, http://dx.doi.org/10.1080/00220388.2013.812200

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Early-Childhood Nutrition and Educational Conditional Cash Transfer Programmes

SETH R. GITTER*, JAMES MANLEY* & BRADFORD L. BARHAM**
*Department of Economics, Towson University, Towson, MD, USA, **Department of Agricultural and Applied Economics, University of Wisconsin, Madison, WI, USA

Final version received July 2013

ABSTRACT *Conditional cash transfer (CCT) programmes have been linked to improvements in education, but effects on nutritional status are unclear. We develop a theoretical household model demonstrating how CCTs' educational requirements may constrain households to shift resources from younger to older children to sustain school attendance. This could limit households' capacity to invest in young children's nutritional status, particularly given a negative income shock. In a Nicaraguan pilot CCT, recipients' consumption and nutritional status increased on average, but less in households with school-age children. Effects are stronger in communities dealt an exogenous income shock.*

1. Introduction

Conditional cash transfer (CCT) programmes provide payments to mothers based on their household's utilisation of health services and children's school attendance. CCTs have multiple goals that appear quite complementary, such as increasing school-age children's school attainment, decreasing poverty, enhancing women's autonomy and decision-making capacity, and improving early-childhood development. While these programmes have been shown to be successful in achieving some of these goals, their effect on younger children's (under four years old) nutritional status is less clear (Fiszbein & Schady, 2009; Manley, Gitter, & Slavchevska, 2013). In particular, CCTs have had limited success at improving height-for-age z (HAZ) scores of young children, a common measure of nutritional status (Behrman & Hoddinott, 2005; Fernald, Gertler & Neufeld 2008; Maluccio & Flores, 2005; Macours, Schady, & Vakis, 2012). In Nicaragua two different pilot programmes failed to generate statistically significant increases in HAZ (Maluccio & Flores, 2005; Macours et al., 2012), though the earlier programme decreased stunting and the later programme improved cognitive development.

One contending explanation for weak child nutrition outcomes is that poor families face difficult trade-offs in human capital investment for younger and older school-aged children. Sending school-aged children off to work is an unfortunate but sometimes necessary event in poor households, especially in response to negative income shocks. If CCTs provide income conditional on school attendance, then participating households may be constrained in their use of child labour and expenditure patterns in ways that trade off human capital investments in younger and older children.

Red de Protección Social (RPS), the Nicaraguan CCT studied in this article, linked payment size to the average cost of school attendance, including opportunity cost, for a typical household, but did not adjust payments for family size beyond a small payment for direct schooling costs. This design

Correspondence Address: Dr Seth R. Gitter, Department of Economics, Towson University, 8000 York Road, Towson MD 21252, USA. Email: srgitter@gmail.com

feature was implemented in part to discourage fertility choices linked to payment streams (Maluccio & Flores, 2005). Although the opportunity cost coverage was intended to supplement household income to the same degree as having the child work outside of the home, the payment cap may have limited RPS's ability to fully cover those costs, particularly in households with more school-age eligible children.

Opportunity costs of CCT participation extend beyond sending school-age children to work. In addition to working outside the home, older children's attendance at school may be at the cost of time spent caring for younger siblings. In Oportunidades, Mexico's national CCT, mothers increased their time spent caring for younger children when older children increased their school attendance (Dubois & Rubio-Codina, 2012). In Colombia sisters of CCT recipients worked more and attended less school (Barrera-Osorio, Bertrand, Linden, & Perez-Calle, 2008), and in Cambodia a CCT increased school enrolment for recipients but not their siblings (Ferreira, Filmer, & Schady, 2009). Thus, households face opportunity costs of income and time associated with a child's school attendance, and increased schooling investments induced by CCTs could hinder younger children's development. This trade-off is more likely among CCT recipients under duress because of chronic low incomes and/or serious negative income shocks.

Nicaragua's RPS provides a useful example because the panel data include a major shock to income that many recipients experienced. Specifically, the price of coffee dropped substantially in the early 2000s, with large repercussions for the coffee-producing areas that comprise about half of the sample. When wages dropped, households needed income more than ever, and they were forced to consider sending school-age children out to work and/ or keeping such children home from school to help care for younger siblings while adults in the household went off to work. If the estimated opportunity costs of child labour and time as evaluated by RPS organisers fully cover the income the child might have generated, as well as any additional income the household might need in order to make sure that the child was adequately prepared for school, we will see no adverse effects: the household will have a clear gain in utility from programme participation. However, if the transfers do not fully cover that cost, the potential for households to benefit is more limited. Poorer households choosing to participate may have to cut consumption by members other than the school-bound child. As the marginal utility of income rises, the trade-offs made by households become starker, and it is in this context that we find evidence to support our theory.

Maluccio (2005) takes advantage of the coffee price shock in his analysis of the programme, and we reconsider the data in light of our theory to explain some of his results, including some that he finds puzzling. Overall, he finds that RPS seems to work: children get more education; they work less; and household consumption expenditures increase. Further, RPS functioned as a safety net when recipient households in coffee-producing areas faced the income shock. However, Maluccio (2005) glosses over a result that seems counter-intuitive: he finds that programme effects on child height-for-age are lower in coffee-producing areas. Since marginal effects on nutritional status should be higher among the poorer households (as found by Manley et al. (2013) in their meta-analysis), this is a curious result. Maluccio struggles to explain it, ultimately ascribing it to the 'smaller sample size' (2005, p. 36). While smaller sample size might explain a larger standard error, such an issue is unlikely to change the sign: the coefficient on RPS effects on HAZ overall is 0.36 and significant, while the interaction term (the modifier on RPS effects in coffee regions) is –0.24. Thus, in recipient areas producing coffee, the overall effect of the programme is considerably reduced.

Next we offer a theory to explain this discrepancy and a broader view of how the number of older children in CCT programmes might affect the HAZ of younger siblings, and then we find empirical support for the theory via econometric analysis of the RPS data. In the theory section, we develop a household model which shows that school and early-childhood nutritional investment trade-offs are most likely to occur when households face the following conditions: (1) they have older children who would not have gone to school without the cash payment; (2) the cash payment is sufficient to get the children to attend; and (3) the transfers are smaller than the total cost of schooling, including direct and opportunity costs. In other words, if households gain utility from sending children to school, they may decrease their current consumption to do so. The conditional cash transfer reduces the cost, making schooling more accessible. Our empirical analysis finds that RPS participants experienced substantially less benefit when faced with the trade-off between older children's schooling and younger children's health, particularly during an exogenous income shock.

2. Theoretical Model

Conditional cash transfer programmes are designed to foster the human capital development of both school-aged children and younger children (ages six and under).[1] If consumption goods improving younger children's health are normal goods, then it follows that unconditional cash transfers should have a positive impact. However, with finite resources, substitution between investment in younger and older children can occur, especially if transfers are conditioned in a manner that requires investment in older children. Below we propose a simple household model to show how transfers conditional on schooling can potentially have diminished or even negative effects on younger children's development because of this required investment.

We build on previous work on stochastic shocks to production and school attendance (Gitter & Barham, 2009; Jacoby & Skoufias, 1997; Kruger, 2007) by considering their influence on younger siblings' health. The household utility function (Equation 1) has three parts represented by adults (a), the younger children (yc), and school-aged children (o). The utility function for adults, u, reflects only their consumption (c_a). The young child's human capital (h) is a function of the young child's consumption, c_{yc}, and care from elders (z). The third part is the school age children's human capital, H, which has two inputs: consumption, $c_{o;}$ and time spent on education, e. We assume that all three functions have diminishing marginal returns to any input.

The household faces four constraints. The first is that the older children can divide their total time, represented by the N (the number of older children), between three activities: education (e); childcare (z_o); and wage labour, L_o. Adults in the household divide their time between child rearing (z_a) and wage labour (L_a).

The total production of childcare (z) and income (wL) is the sum of the production of the adults and the older children. The older children's productivity in childcare is a fraction (α_z) of adults' productivity, where $0 \leq \alpha_z \leq 1$. This is based on findings in Dubois and Rubio-Codina (2012) that mothers are better caretakers than a child's siblings. Similarly, in the wage market older children earn a wage that is some fraction of adults' childcare productivity, α_w, where $0 \leq \alpha_w \leq 1$. The relationship between α_z and α_w could depend on local conditions and child characteristics, such as gender and age. It is worth noting that the core results do not change if we assume that adult and child labour are perfect substitutes in terms of childcare or wage labour.

As to the budget constraint, households spend all of their money, with no borrowing or saving allowed in this model.

$$
\begin{aligned}
U &= u(c_a) + h(c_{yc}, z) + \sum\nolimits_{i=1}^{N} H_i(c_o, e) \\
s.t.\, N &= e + z_o + L_o \\
1 &= z_a + L_a \\
z &= z_a + a_z z_o \\
L &= La + a_w L_o \\
wL &= c_a + c_{yc} + c_o
\end{aligned}
\tag{1}
$$

We next consider two potential forces that could affect human capital outcomes of the younger child, $h(c_{yc}, z)$: a cash transfer conditional on schooling; and a shock combined with a cash transfer conditional on school attendance.

2.1 The Impact of a Conditional Cash Transfer on Young Child Development

Suppose that the household receives the transfer conditional on the older child attending school full time, so that $e = 1$. The effects of the conditional transfer, *CT*, on the younger child (h) are the sum of the impacts on his or her consumption (c_{yc}) and changes in childcare, z.

$$
\begin{aligned}
&\frac{\partial h}{\partial CT} = \frac{\partial h}{\partial c_{yc}}\frac{\partial c_{yc}}{\partial CT} + \frac{\partial h}{\partial z}\frac{\partial z}{\partial CT} \\
&wL + CT = c_a + c_{yc} + c_0 \\
&CT > 0 \text{ } if \text{ } e = 1, h \geq h', \text{ and } t > 0 \\
&CT = 0 \text{ } if \text{ } e < 1, h < h', \text{ and}/or \text{ } t = 0 \\
&1 = z_a + L_a + t
\end{aligned}
\tag{2}
$$

We would expect that increasing household income potential by providing a conditional cash transfer would improve the younger child's human development through increased spending on the younger child's consumption and increasing quality of childcare. However, the size of the effect will depend on several eventualities, including whether the transfer fully compensates for the total loss of older children's income and costs associated with the older children's schooling. As older children move from work to school, they go from contributing income and childcare to consuming more household resources. Adults may increase childcare when older siblings cannot take care of the younger, but adults also might increase their labour time to compensate for the loss of the labour of older children now in school. In this case a conditional transfer could also reduce childcare and the welfare of younger children.

2.2 An Income Shock Combined with a Cash Transfer Conditional on Schooling

Previous research shows that income shocks can make CCTs' conditionality of schooling more likely to bind (de Janvry, Finan, Sadoulet, & Vakis, 2006; Gitter and Barham, 2009; Maluccio 2005) as households typically use child labour as a buffer against shocks. All three papers showed CCTs had greater impacts on schooling and child labour during a shock, and the last two examined RPS and the coffee shock. When a household faces a consumption deficit, the conditional transfer suddenly represents a dual opportunity cost. Households choosing to take the transfer lose the potential income older children might bring home, and instead face the added cost required to support a child participating in education. If the transfer is not enough to cover these costs, the household has committed to supporting one member to a certain degree without ensuring that consumption in the rest of the household will increase. With the transfer, household consumption may stay the same or even increase, but depending on intra-household distribution the younger child may or may not see an increase in consumption and/or may not continue to receive the same level of childcare, limiting improvements in his or her nutritional and development status. Without enough income to cushion the cost of sending a child to school, the underlying trade-off involved in sending children to school becomes apparent.

3. Data Summary and Descriptive Statistics

3.1 Programme and Data Description

Our data come from Nicaragua's Red de Protección Social (see Hoddinott & Bassett, [2008] for detailed descriptions and comparison to other CCT programmes in Mexico and Honduras, and Maluccio and Flores [2005] for a detailed description of the programme).[2] We use two survey rounds, one collected in 2000, before the programme had begun, and another in 2002, in the second year of payments. It is worth noting that RPS, like other CCTs, randomised treatment at the community level. Thus, we have data on 42 communities, with half each in control and treatment groups. Maluccio and

Flores (2005) show that treatment and control groups are not substantially different across most major indicators.

The average payment was $27 a month, about the same as for the Mexican CCT programme Oportunidades, though the share of household income accounted for by the transfer was considerably larger in Nicaragua. To avoid incentivising fertility, household payments and eligibility were determined ex-ante of programme implementation and additional children were not funded. For the most part, payment size was not linked to household size. Only a yearly $20 'school supplies transfer' was awarded per child, which was not enough to compensate households for the full opportunity cost of sending additional children to school; it merely offset school fees and transportation for each additional child. Thus, households with any school-age children received approximately the same transfer regardless of the number of children. However, to receive the education transfer all children had to attend school (Maluccio, 2005). Furthermore, the transfer was enforced as 10 per cent of households lost benefits due to failing to meet conditions (Maluccio & Flores, 2005).[3] To the extent that the additional transfer failed to cover the (twofold) opportunity costs, sending each child to school imposed an additional cost on the household.

RPS also included conditions aimed at improving younger children's physical development. Children under 24 months got monthly care and growth monitoring from clinics, while older children received bimonthly care and less attention. Adequate weight gain was also a condition of payment. Furthermore, RPS increased the provision of iron supplements, though, as in PROGRESA, these failed to decrease anaemia rates (Maluccio & Flores, 2005). Previous analyses of RPS show positive but limited effects. Maluccio and Flores (2005) find that RPS decreased stunting by 5.5 per cent, a statistically significant outcome. Another study by Maluccio (2005) found similar effects, but also noted that RPS effects on HAZ were diminished in coffee communities (p. 35). In a different study, Hoddinott and Wiesmann (2008) show that households in RPS increased calories from fruits and vegetables, as well as the number of unique foods consumed. Thus, households improved their nutritional intake, but this appears to have had only a limited impact on children's height, particularly in coffee-producing communities.

3.2 Describing the Shock

The shock of interest is the steep decline in coffee prices that occurred during the implementation of RPS. Figure 1 shows the change in coffee prices over the time period in question along with the years of data collection. The first round of data collection occurred as coffee prices were falling. The final

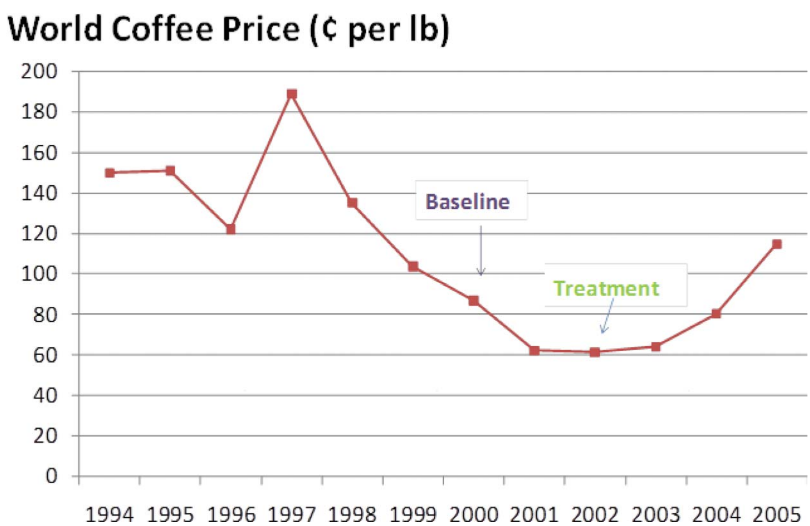

Figure 1. Data collection timeline.

round of data was collected during the low point of coffee prices when prices were only slightly above 60 cents per pound, less than one-third of their high point in 1997. A previous analysis of the sample populations in Nicaragua (Maluccio, 2005) shows that the shock was associated with a 16 per cent decline in per capita consumption between the baseline year 2000 and the second year of treatment 2002.

The shock affected about half of the communities. As Maluccio (2005) notes, coffee is only grown at certain altitudes, and therefore is only feasible in certain communities. Coffee communities were identified using surveys given to community leaders, which ask questions about the presence or importance of coffee crops. Of the 42 surveyed communities, one-half were coffee-growing communities, with 11 treatment and 10 control communities in both coffee and non-coffee communities. The community measure may not control for heterogeneous effects associated with a household's participation in the coffee sector; however, it is a more inclusive measure because it encompasses households that may have experienced indirect impacts from the coffee shock through falling local wages.

3.3 Sample Statistics

We have two outcome variables of interest. First is per capita consumption, the average amount of resources available to household members (see Maluccio and Flores [2005] for more details on this measure). Second, we use height-for-age z scores (HAZ) to measure younger children's development. Growth patterns of children under age five are similar for all ethnic groups (WHO, 2006) and growth charts allow the conversion of child heights into HAZ based on observed means and standard deviations for children of a given age and sex as compared to a reference population. Height-for-age is often described as an indicator of long-term nutritional status among children and associated with long-term physical development (Hoddinott & Kinsey, 2001, Strauss & Thomas, 1998; Victora et al., 2008, Walker et al., 2011; Waterlow et al., 1977). It is also an indicator of a child's underlying health status, and children showing lower levels of physical development for their age are often delayed in their mental development as well. Many studies have evaluated children's growth with reference to such a standardised population in order to estimate the health effects of natural disasters, economic crisis and various policy interventions, (see, for example, Balk et al., 2005; Ferreira & Schady, 2009; Hoddinott & Kinsey, 2001; Paxson & Schady, 2005).

RPS collected data on anthropometrics for all children in treatment and control groups under the age of five. Maluccio (2005) analyses a sample of all children aged 6 to 48 months at the time of measurement. We use a slightly different sample, focusing on children aged 24 to 60 months. We do this for two main reasons. First, in this data, children under the age of 24 months measured in 2002 would have been in utero after the start of the programme. Such children are beneficiaries of prenatal care provided by the programme and thereafter received extra attention as part of the programme. Our age restrictions still ensure that children evaluated after the programme began were no more than age three at the time of the intervention, a level seen as an upper limit of the time during which child growth is highly susceptible to damage (Victora et al., 2008).

Second, as noted by Grantham-McGregor et al. (2007), a pattern of growth faltering common in developing countries involves a pronounced drop-off in height-for-age continuing through the first 12–18 months as difficult conditions combine with high susceptibility of nutritional status to risk factors. In accordance with this global estimate, we note in this dataset a large drop in height-for-age through the first 18 months or so, as shown in Figure 2. Since variation in HAZ is largely attributable to a secular trend during this period, comparatively subtle effects such as those associated with a programme like this are likely to be more noticeable if they happen after that window.

To investigate the theory described in Section 2, we examine the determinants of two outcomes, per capita consumption and HAZ, by household type. For the descriptive statistics we break down households into those with at least two children aged 8–13 (44 per cent of the sample) versus those with one or no children of that age. In the baseline there are an identical number of treatment and control communities. Note that the results are similar whether we use the number of school aged children or a binary dummy for those households with two or more children.

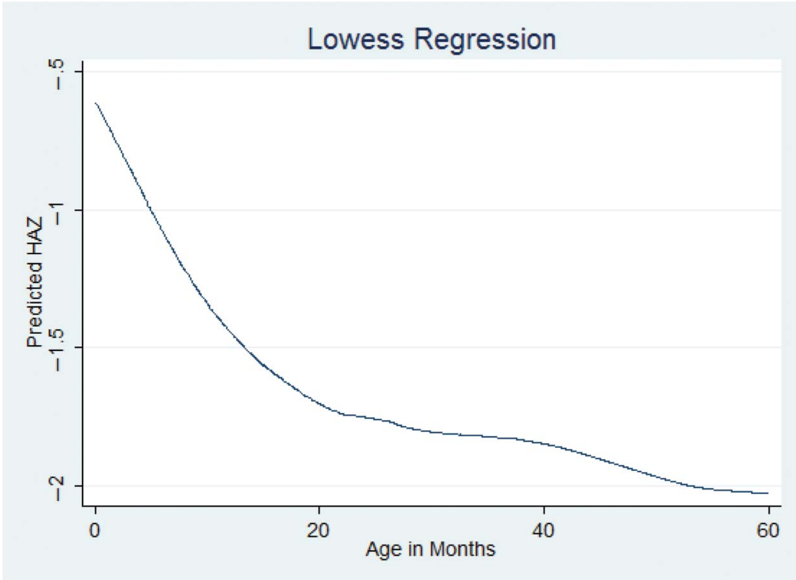

Figure 2. Height for age lowess regression.

Table 1 summarises the data on per capita consumption and younger children's z scores with a difference-in-difference comparison of baseline vs treatment year across households with zero or one school-age child (8–13 years old) as opposed to those with two or more. Households with more school-aged children see half the increase in total per capita consumption (501 vs 1,012); this may be due to the smaller per capita transfer for larger households. Finally, in terms of HAZ scores, small treatment households saw an increase of 0.3 over controls, while treatment households with more children dropped by 0.13 compared to similar controls. This result is partly attributable to the increase

Table 1. Dependent variable outcomes

	Per capita total consumption, household			
School-aged children	Zero or one		Two or more	
	Control	Treatment	Control	Treatment
Baseline	3746	3893	2918	3058
Treatment year (2002)	3242	4401	2881	3521
Difference-in-difference		1012[a]		501[a]
		[3.52]		[2.09]
	Height for age: children 24–60 months			
School-aged children	Zero or one		Two or more	
	Control	Treatment	Control	Treatment
Baseline	–1.74	–1.89	–2.02	–1.91
Treatment year (2002)	–1.87	–1.73	–1.88	–1.9
Difference-in-difference		0.29[b]		–0.13
		[1.92]		[0.62]

Notes: (C$) is September 2000, Nicaraguan córdobas. US$1 is equivalent to roughly C$12.85.
[a] indicates difference-in-difference significant at 5 per cent; [b] at 10 per cent; t-stats in brackets below.

Table 2. Descriptive statistics by group

School-aged children	Non-coffee communities				Coffee communities			
	Zero or one		Two or more		Zero or one		Two or more	
	Control	Treatment	Control	Treatment	Control	Treatment	Control	Treatment
Number of children 8–13 (baseline)	0.49	0.52	2.54	2.46	0.45	0.52	2.40	2.41
Total household size	5.88	5.70	8.82	8.24	5.65	5.53	8.31	8.29
Child age in months (children 24–60 months only)	41.57	41.31	41.43	41.79	40.46	39.57	41.79	41.66
Age of child's mother (years)	28.82	28.21	36.12	36.49	28.02	27.38	36.67	35.87
% of mothers who can read[a]	0.67	0.71	0.55	0.51	0.56	0.36	0.44	0.43

Note: [a]The share of mothers who can read is the only variable evincing a statistically significant difference across sub groups or coffee and non-coffee communities, and that is only at the 10 per cent level.

in the control group's HAZ scores for larger households. Even ignoring this change, smaller families saw HAZ scores increase 0.16 compared to 0.01 in larger households.

Before we can evaluate the effects of the shock, we must establish that the groups are similar at baseline. As shown in Table 2, 52 per cent of the sample is in coffee communities, which are evenly split among treatment and controls. Adults in coffee communities do appear to have lower education levels, but are otherwise similar to their counterparts. Accordingly, we control for education in the regression analysis. This difference could bias the results if maternal education influences the programme effects or the triple interaction of programme effects, community type and the number of school age children. However, including those interactions does not change the main results. Nor are those terms statistically significant predictors of height-for-age. Age and household size differences will be controlled for directly when we use regression analysis, but first we consider some simple averages.

In Table 3 we review the effects of the coffee price shock, verifying that it amplifies contrasts between households and particularly the difference between large and small families. First, with respect to consumption, control households with zero or one school-age child in coffee communities saw consumption fall roughly 25 per cent from the baseline period, while larger households declined less. Households in the control group with two or more children in coffee communities only lose about 10 per cent of per capita consumption (2,940 to 2,617). This supports the theory in Section 2, which anticipates offsetting consumption losses by increasing child labour.[4] The other per capita consumption effect of note is that in both types of communities (coffee and non-coffee) the treatment households were able to maintain or increase per capita consumption levels during the shock, while control households were not. In three of the four group comparisons, the difference-in-difference (DID) for per capita consumption was positive and statistically significant between treatment and control households.

There are several observations to draw from the lower half of Table 3 regarding our other outcome of interest, HAZ. First, HAZ scores are generally lower in coffee communities than non-coffee communities, which could reflect the inherent trade-offs associated with having a local economic activity that is relatively intensive in the use of older children's labour. Second, treatment households in coffee areas with zero or one school age children saw HAZ increase by 0.19 across the study period, while control households in the same categories saw HAZ decline by the same amount. A simple DID suggests that in coffee areas treatment is associated with an increase of 0.38 z scores in households with no or one older child. This is the type of salutary effect that programme designers might seek for early-childhood

Table 3. Outcomes by village coffee vs non-coffee status

	Per capita total consumption, household							
	Non-coffee communities				Coffee communities			
School-aged children	Zero or one		Two or more		Zero or one		Two or more	
	Control	Treatment	Control	Treatment	Control	Treatment	Control	Treatment
Baseline	3613	3891	2901	2980	3877	3895	2940	3127
Treatment year (2002)	3383	4539	3064	3612	3084	4235	2617	3430
Difference-in-difference		877[a]		469		1133[a]		626[b]
		[2.40]		[1.35]		[2.55]		[1.90]
	Height for age z scores							
	Non-coffee communities				Coffee communities			
	Zero or one		Two or more		Zero or one		Two or more	
School-aged children	Control	Treatment	Control	Treatment	Control	Treatment	Control	Treatment
Baseline	–1.54	–1.56	–1.75	–1.6	–1.91	–2.17	–2.25	–2.1
Treatment year (2002)	–1.64	–1.53	–1.71	–1.58	–2.1	–1.98	–2.06	–2.1
Difference-in-difference		0.13		–0.02		0.38[b]		–0.19
		[0.64]		[.05]		[1.79]		[0.71]

Notes: [a] indicates difference-in-difference significant at 5 per cent; [b] at 10 per cent; t-stats in brackets below.

development. However, an opposite effect is evident for households with two or more school age-children in those communities. There, in coffee communities, control households show some improvement in HAZ scores over time, while treatment households do not. This is consistent with the proposition that treatment households are sending their older children to school rather than having them take care of their younger siblings or work outside of the home to improve household consumption. The role of family size in shaping programme impacts on HAZ deserves deeper attention.

4. Econometric Specification and Difference-in-Difference Estimation

We use the 'gold standard' difference-in-difference measures of programme impacts (Hoddinott & Skoufias, 2004; Maluccio & Flores, 2005; Schultz, 2004) to see whether outcomes in treatment households depend on the number of school-age children. Our regressions allow us to control for possible confounding factors to explore more cleanly the implications of the theoretical model presented in Section 2 for per capita consumption and height-for-age z scores of younger children under five years. Maluccio (2005) tests the interaction between RPS and being in a coffee-producing area using a specification similar to Equation 3. This is called a triple difference approach, and we redeploy that approach here.

The two main coefficients of interest are the programme effect (α_4) and how an additional school-age child changes that effect. In our model we explore whether the number of school-aged children influences the effect of RPS by interacting those terms (α_7). Specifically, we interact the number of older children (aged 8–13) with all combinations of year and treatment dummies. In order to test whether the income shock exposes the trade-off associated with the CCT, we would ideally add a fourth interaction distinguishing children in households with older siblings from children without them. However, since the number of regressors doubles with each additional set of interactions, such a specification is too unwieldy, and we settle for running two triple difference regressions of the form specified below in Equation 3: one on households in coffee producing regions; and another in non-coffee producing areas.

$$y_{ict} = \alpha_0 + \alpha_1 OlderChildren + \alpha_2 CCT_c + \alpha_3 year + \alpha_4 CCT^{*}year + \alpha_5 \\ OlderChildren^{*}year + \alpha_6 OlderChildren^{*}CCT + \alpha_7 OlderChildren^{*}CCT^{*}year + \beta_i X_i + \mu_{ic} \quad (3)$$

Here, the unit of observation depends on the outcome in question. We use per capita consumption (PCC) for our first dependent variable, and in this case, each household is an observation. For height for age z scores (HAZ), each observation is a child between 2 and 5 years old. *OlderChildren* refers to the number of children in the household aged 8–13 years.[5] Note that one of the household characteristics included in the X_i vector is total household size, so the *OlderChildren* variable is capturing more than the effect of adding one person of any age to a household. This is particularly important with PCC. Using per capita consumption expenditures as a dependent variable helps to identify how households responded to both the shock and potential transfers.

5. Results

5.1 Per Capita Consumption

The results of triple difference regressions on per capita consumption are shown in Table 4. Households randomised into CCT communities were on average better off through the time period, and as one might expect such households were much better off after the programme actually began (that is, in 2002, as shown by the CCT*Year coefficients). Consumption expenditures rose substantially: total expenditures increased roughly 25 per cent, while food consumption climbed by about a third. The results on the triple interaction term (CCT*Year*Members8–13) are consistent with our second prediction that each additional school-aged child would reduce the transfer's impact on per

Table 4. Per capita consumption outcomes of CCT and household structure

	All areas	All areas	Non-coffee	Coffee
	Total cons	Food cons	Food cons	Food cons
Year	–475.0	–497.8	–300.3	–703.1
	(163.8)[c]	(112.0)[c]	(152.0)	(153.4)[c]
CCT	216.8	137.5	191.8	83.5
	(299.9)	(206.4)	(309.2)	(283.8)
CCT*year	910.5	883.7	825.8	922.3
	(259.8)[c]	(188.8)[c]	(279.9)[c]	(234.9)[c]
CCT*year*members8–13	–140.8	–144.5	–101.7	–171.1
	(98.4)	(83.4)[a]	(125.7)	(102.0)[a]
CCT*members8–13	–115.5	–78.9	–107.8	–51.3
	(104.0)	(80.4)	(116.3)	(112.7)
Year*members8–13	75.6	50.8	7.3	87.5
	(72.3)	(55.2)	(84.3)	(41.2)[b]
Members8–13	151.9	82.0	77.2	91.1
	(76.0)	(57.8)	(81.7)	(82.0)
Household size	–362.4	–222.4	–212.7	–234.2
	(22.1)[c]	(15.4)[c]	(22.0)[c]	(21.5)[c]
Constant	5716.7	3816.4	3724.1	3916.7
	(281.7)[c]	(192.2)[c]	(312.9)[c]	(226.3)[c]
Observations	2383	2383	1244	1139
R-squared	0.17	0.16	0.17	0.15

Notes: Robust standard errors in parentheses, errors clustered at community level.
[a] significant at 10 per cent level; [b] at 5 per cent level; [c] at 1 per cent level.

capita consumption. The final prediction of the theoretical model is that a negative income shock should amplify the triple interaction term (CCT*Year*Members8–13), so we would expect this term to be larger in magnitude in coffee communities. Indeed, the triple interaction term is larger in absolute terms and statistically significant at the 10 per cent level in the coffee growing region, implying that each additional school-age child cut into the effect of the programme in all CCT areas, but more significantly in those that also experienced the negative shock.

The effect of transfers on per capita consumption for households with more school-aged children is smaller, although this marginal effect is only statistically significant at the 10 per cent level for per capita consumption of food. The magnitude of the term suggests that each additional school-aged child reduces the effect of the CCT by roughly one-sixth.[6] Note that in non-CCT coffee areas, school-age children in 2002 helped buffer household consumption from the effects of the shock (as shown by the Year*Members8–13 coefficient, which is significant at the 5 per cent level). This is consistent with those children attending school when it is affordable and going to work (or providing childcare to enable adults to work) as necessary when it is not. Finally, note that household size was included, and it has the expected strong negative coefficient. This ensures that the estimated effects of school-age children describe their effects not simply as an additional mouth to feed, but reflect their particular status as children of this age.

5.2 Child Height-for-Age

This article's central finding about the role of family size and programme structure on HAZ scores is shown in Table 5. The dependent variable is the height-for-age of children under age five, and the first two columns present results for all households with and without control variables for maternal education and literacy. The second two columns separate results by coffee and non-coffee communities with the aforementioned control variables.

The regression estimates confirm the predictions of the theory elaborated above. The first two rows of the pooled results show weak declines in height for age from 2000 to 2002 and in CCT vs non-CCT

Table 5. Height for age associations with CCT, year, and older children in the home

Coffee/non-coffee community	All	All	Non-coffee	Coffee
Year	–0.16	–0.09	–0.04	–0.17
	(0.10)	(0.12)	(0.18)	(0.14)
CCT	–0.21	–0.09	–0.10	–0.17
	(0.15)	(0.16)	(0.24)	(0.20)
CCT*year	0.39	0.26	0.14	0.43
	(0.15)[b]	(0.14)	(0.18)	(0.21)[b]
CCT*year*members8–13	–0.21	–0.17	–0.07	–0.29
	(0.09)[b]	(0.10)	(0.15)	(0.13)[b]
CCT*members8–13	0.10	0.07	0.08	0.14
	(0.08)	(0.11)	(0.17)	(0.10)
Year*members8–13	0.13	0.09	0.05	0.15
	(0.06)[b]	(0.08)	(0.10)	(0.08)[a]
Members8–13	–0.02	–0.01	0.01	–0.05
	(0.05)	(0.09)	(0.14)	(0.07)
Age in months	–0.01	–0.02	–0.01	–0.02
	(0.00)[c]	(0.00)[c]	(0.00)[b]	(0.00)[c]
Male	0.04	0.02	0.08	0.03
	(0.06)	(0.07)	(0.12)	(0.06)
Per capita spending baseline	0.14	0.11	0.13	0.11
	(0.02)[c]	(0.02)[c]	(0.03)[c]	(0.03)[c]
Mother's age		0.01	0.00	0.01
		(0.01)	(0.01)	(0.01)
Mother can read = 1		0.43	0.35	0.37
		(0.09)[c]	(0.13)[b]	(0.13)[c]
Constant	–1.67	–2.01	–1.93	–1.95
	(0.16)[c]	(0.26)[c]	(0.36)[c]	(0.34)[c]
Observations	1420	1202	598	604
R-squared	0.08	0.1	0.1	0.11

Notes: Robust standard errors in parentheses, errors clustered at community level.
[a] significant at 10 per cent level; [b] significant at 5 per cent level; [c] significant at 1 per cent level.

communities, but those effects are not statistically significant. In the next row the pooled model shows that for households with no school-aged children, RPS increased HAZ by almost 0.4 z scores (CCT*YEAR). Given that the estimated effect of most CCTs on HAZ is typically less than 0.2, this 0.4 increase represents a substantial gain. However, as seen in the triple interaction term (CCT*Year*Members8–13), each additional school-age child in a CCT-recipient household reduces that gain by more than half, and that outcome is driven by the coffee community experience as reflected in the largest estimate shown in that column. The two rows below the triple interaction (CCT*Members8–13 and Year*Members8–13) show that older children in general help the nutritional status of younger children: all coefficients (in pooled and separate models) are positive and again they are occasionally significant, such as (in CCT and non-CCT areas combined) in coffee areas in 2002.

It is only the combination of CCT treatment, older children and an income shock that make for negative effects on HAZ scores of younger siblings. This result is consistent with older children not being available to help to compensate for the negative shock through labour efforts or childcare, and/or decreased consumption of the younger child. In the pooled regression, the statistical significance is muted when additional controls for maternal age and literacy are added, but the magnitudes and signs are the same. The significance holds up most robustly in the fuller specification for coffee communities, and this outcome is consistent with the arguments raised above regarding negative CCT impacts on HAZ scores during times of shock. Finally, household controls for baseline consumption and maternal literacy show the expected positive relationship with children's height-for-age and male children (MALE =1), but do not show statistically significant differences with female children.

Consider carefully the latter two columns of Table 5 to compare coffee producing communities with non-coffee communities. Height-for-age declined more in coffee communities in the second year of the study, reflecting the shock, though the difference is not statistically significant. CCT areas were marginally (and insignificantly) worse off the first year, but the second year that was strongly turned around as the payments began. The statistically significant improvement of about half a standard deviation in the height of younger children is larger than has been linked to most previous programmes. Again, those gains are strongly undercut by the presence of older (school-age) siblings in a household. Each sibling cut the programme effect by about 0.3 of a standard deviation, an amount that is statistically significant. This loss is biting, as we see that in coffee communities in general (including CCT and non-CCT areas) older siblings tended to be a help, on average adding about 0.15 standard deviations to the height-for-age of their younger siblings. This effect was significant at the 10 per cent level. An effect of comparable size is associated with having school-age siblings in a CCT area prior to the shock, though it is not statistically distinguishable from zero. Finally, we see negligible effects of having school-age siblings when averaged across all situations (that is, across the years and whether the community was randomised into the CCT or not). Age is strongly negatively correlated with height for age, and per capita spending at the baseline is also significant and with the expected positive sign. Maternal education is always highly significant and salutary for the child's nutritional status as well.

To sum up, all signs are as expected. At a baseline level, younger children's nutritional status is not much affected by having older children around. In coffee communities, they are overall helpful, perhaps as they help cushion the income shock by providing childcare or leaving school to work. However, in households committed to the CCT, they become strongly negative during the shock, as older children are unable to contribute to their siblings' HAZ and instead become an added constraint on the household's potential investment in early-childhood development. As a result, younger children apparently lose out as parents dedicate more scarce resources to school-age children.

6. Discussion

We find that participation in RPS, Nicaragua's CCT programme, helped households cope with negative income shocks on younger children's development. However, the effectiveness of the programme in improving consumption expenditures and the nutritional status of young children is limited in households with more school-age children. This unfortunate limitation is the product of three major facets of the situation. First, RPS transfers to households were a fixed amount, independent of the number of household members or the number of children. This was done to avoid promotion of fertility, but it may have had negative side effects by constraining household choices. Second, an income shock made the potential trade-offs more urgent. These two issues combined to render painful the conditionality constraint that households send older children to school, and may have forced those children's consumption and schooling outcomes to be pursued at the expense of the early-childhood development of their siblings.

Omitted variable bias is also a possibility in any regression analysis. Our three-way difference-in-difference specification (differencing across time, treatments and number of school-age children in the household) plus running separate regressions for coffee-producing and non-coffee-producing areas reduces the potential for competing explanations, but it is possible that some unobserved phenomenon correlates with each of the dimensions across which we are taking differences. We would expect such an omitted variable to also impact consumption; however, an examination of the consumption data does not support the presence of an omitted variable in RPS coffee growing communities.

7. Conclusion and Policy Implications

Maluccio (2005) describes how RPS apparently increased child enrolment in education, decreased child labour and improved the level of consumption expenditures in poor areas. However, our re-examination of the data shows that improvements in consumption for households with more school-aged children were lower. This drop in effectiveness may be due to the insensitivity of the transfer to the number of school-age children in the household. Programme designers should reconsider the relationship between payments and household size, particularly since there is little evidence that CCTs influence fertility (Stecklov, Winters, Todd, & Regalia, 2007).

We show how CCTs could fail to help young children in recipient households experiencing a consumption shock. We link this possibility to the conditionality of the transfers, which requires school-age children to attend school. When not attending school, older children generate labour earnings or participate in childcare, contributions which take on added significance when income is exogenously reduced. Programme impact could be improved if funds were increased to compensate for lost child labour, including both wage labour and help in child rearing. It is worth noting that CCTs have not been designed with the intention of adapting to shocks (Bourguignon, 2000; Grosh, Del Ninno, Tesliuc, & Ouerghi, 2008). Grosh et al. (2008) rightly point out that targeting after a crisis may be particularly difficult, given the need for real-time data in low-capacity countries and the fact that monitoring earnings in informal markets makes monitoring and targeting difficult.

This work suggests that there are trade-offs between investment in education and early childhood nutrition and households are most likely to face those trade-offs during a shock. These types of shocks are common in areas that are often the focus of CCTs, and they may interact with programme stipulations to have unintended negative effects on early-child development. One strategy that many countries used during the global economic downturn was to expand cash transfer programmes by increasing funding and starting new programmes (Fiszbein, Ringold, & Srinivasan, 2011). Likewise, higher payments to more households during shocks may help overcome the issues raised in this article.

Acknowledgements

The authors are grateful for financial support provided by the Inter-American Development Bank. The authors thank helpful comments of reviewers as well as participants and organisers of both IDB sessions for helpful input on ways to improve this article, and invite further comments. All errors are solely our responsibility.

Notes

1. The RPS programme targeted school children aged 7–13. Most impacts on human capital development are seen before the age of three (Bhutta et al., 2008; Walker et al., 2011), but for simplicity we chose to have two groups of children.
2. Data are available at http://www.ifpri.org/dataset/nicaragua.
3. Household level data on enforcement are not available, so we cannot control for household size or differences in communities in enforcement.
4. Maluccio (2005) and Gitter and Barham (2009), using the same data, find increases in child labour commensurate with this observation.
5. This age range was chosen because RPS provided a school transfer for those aged 7–13. However, we found that essentially none of the seven-year-olds worked, so we dropped them from the older cohort.
6. Only three households have more than five school-aged children, so the negative effect of having older children is almost never predicted to supersede the positive effects of receiving the transfer.

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