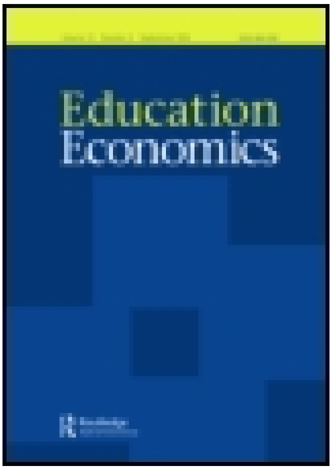

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The influence of conditional cash transfers on eligible children and their siblings

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The influence of conditional cash transfers on eligible children and their siblings

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Conditional cash transfers (CCTs) are used to reduce poverty while incentivizing investments in children. Targeting CCTs to certain groups of children can improve efficiency, but positive effects on eligible children may be offset by reductions in investments for ineligible siblings. Using data from Nicaragua, we estimate program effects on eligible children and older siblings who aged out of eligibility. We find that CCTs had the largest effects on eligible children, but older brothers also benefited through increased schooling and fewer hours worked. These results suggest that income effects of CCTs apply to both eligible and ineligible children.

Keywords: conditional cash transfers; child labor; access to education

1. Introduction

Conditional cash transfers (CCTs) incentivize parents to invest in children's human capital by linking cash payments to children's school attendance and access to health care. The study of CCTs has provided great insight into how families allocate resources when the marginal benefits of schooling are changed through policy. A thorough review of the literature evaluating CCTs found 'clear evidence of program success in increasing school enrollment rates, improving preventive health care, and raising household consumption' (Rawlings and Rubio 2005, 51).

It is unlikely that countries can afford to provide CCTs for all poor families, making the design of program eligibility rules a pathway to improve efficiency (Bourguignon, Ferreira, and Leite, 2003; De Janvry and Sadoulet 2006; Cigno 2011). The risk of imposing eligibility rules is that CCTs might induce strategic behavior within families in favor of eligible siblings. This study contributes to our understanding of CCTs by examining the effect of Nicaragua's CCT program on outcomes for siblings who were not eligible for the program due to age. We examine the direct effect of CCTs on eligible children, the indirect effects on ineligible children with eligible siblings, and the indirect effects of eligible and ineligible siblings on eligible children. We find positive effects on all eligible children and ineligible boys when an eligible sibling is present. This is driven by a larger income effect for boys than girls in a setting where boys are more likely to be out of school and engaging in work. This suggests that extra income induced parents to provide more human capital investments for all male children, while female children benefited only if they were eligible for CCTs. Policy-makers should carefully consider sibling spillover effects when

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designing CCTs, with potentially the greatest benefit when subsidies target children who are most likely to attend school.

2. Theoretical framework

Within families, parents make decisions about investments in each child through a joint estimation of the family's utility (Becker 1964). The labor of one child may facilitate the schooling of another, based on a family's estimations of the marginal costs and benefits of children with different costs and benefits of schooling. Glick (2008) and Alderman and Gertler (1997) identify gender differences in returns to schooling as an important theoretical factor in both investments in human capital and responses to policies such as school subsidies. In the context of family allocation, this means that the labor of children might be used to support investments in the education or health of other siblings. Research has observed differential investment across brothers and sisters in arrangements where sisters often work to provide resources for human capital investment in brothers (e.g. Parish and Willis 1993; Glick and Sahn 2000; Glick 2008; LaFortune and Lee 2014).

This raises interesting questions about the potential impact of CCTs on siblings (Glick 2008). Ferreira, Filmer, and Schady (2009) present a theoretical model with three potential effects of CCTs on families. The first is the substitution effect as the conditionality reduces the cost of schooling for eligible children only. The second is the income effect of the transfer, which is positive for all siblings. The third is a displacement effect where parents substitute the labor of ineligible children to enable eligible children to attend school. Displacement would be positive for eligible children, but negative for any ineligible siblings whose labor can serve as a substitute. Aggregating these effects, the total effect of CCTs is expected to be positive for eligible children but ambiguous for ineligible siblings depending on the relative size of income and displacement effects.

Confirming this theoretical model, many studies have demonstrated that CCTs induce eligible children to increase school attendance and reduce hours of labor (e.g. Schultz 2004; Maluccio and Flores 2005; Rawlings and Rubio 2005; Handa and Davis 2006; Filmer and Schady 2008; Dammert 2009; Ferreira, Filmer, and Schady 2009). However, several empirical tests of the effect of CCTs on other family members have not resolved the ambiguity regarding effects on ineligible children. Ferreira, Filmer, and Schady (2009) find no effect on schooling or labor of ineligible siblings. Barrera-Osorio et al. (2011) find negative effects of CCTs on the schooling of ineligible siblings and particularly ineligible sisters. However, Bustelo (2011) finds positive effects of Nicargua's CCT program on ineligible siblings.

The model presented by Ferreira, Filmer, and Schady (2009) provides reason to believe that the design of a particular CCT program and the context in which it is deployed may drastically alter its effects on the school and labor of ineligible children. As stated above, the net effect on schooling for ineligible children is dependent upon the relative size of the income and displacement effects. These in turn depend on the size of the transfer and the relative likelihood of eligible and ineligible children enrolling in school in the absence of the program.

To illustrate, consider (as do Ferreira, Filmer, and Schady 2009) families with two children, one eligible, and one ineligible. Families who send no children to school will send the eligible child to school if the size of the transfer is sufficient to send only one child to school, and they might send both children to school if the transfer is larger.

In this family, the eligible child is likely to experience a positive effect, and the ineligible child will experience no effect or a positive effect. Families already sending both children to school will continue to do so, and both the eligible and ineligible child will experience no effect from the program. Families already sending the eligible child to school will also send the ineligible child to school if the transfer is large enough. In this case, eligible children experience zero effect and ineligible children experience either a positive or zero effect. However, families previously sending only the ineligible child to school might displace the ineligible child if the transfer is small. This case represents a dangerous policy area of positive effects for eligible children but negative effects for ineligible children.

Mixed results from past studies may be indicative of differences in the targeting strategy for CCTs. In the Colombian experiment studied by Barrera-Osorio et al. (2011), targeted children had to have completed fifth grade, while untargeted children were in lower grades. If older children are more likely to dropout of school to work, this means that in Colombia eligible children were those who were more likely to be out-of-school. Thus, Colombian parents were faced with a decision of whether to discontinue enrollment for ineligible children to facilitate enrollment for eligible children. In the Cambodian experiment studied by Ferreira, Filmer, and Schady (2009), sixth graders at high risk of dropping out were targeted for scholarships for seventh, eighth, and ninth grades. In this case, the targeted age range is quite small, but similar to Colombia, the ineligible children are more likely to be in school a priori. In the Nicaraguan experiment, which is the focus of our study, ineligible siblings are older and therefore less likely to enroll and more likely to work a priori. Bustelo (2011) finds positive effects of Nicaragua's CCTs on older children's enrollment and negative effects on child labor, but does not explore if this is related to siblings. This study contributes to this prior research by estimating the effects of eligible and ineligible siblings on eligible and ineligible children in a setting where ineligible children are least likely to attend school.

3. Nicaragua's CCT experiment

It is likely that only a large cash transfer will induce measurable income effects that are not offset by displacement. We use the Nicaraguan CCT example for this study because it reflects the largest CCT as a percent of income (approximately 20%) (Fiszbein, Schady, and Ferreira 2009), the rate of child labor is quite high and the rate of schooling for ineligible children is lower than the rate for eligible children. This provides the best opportunity to observe both large income effects and large labor displacement effects. CCTs were implemented from 2000 to 2002, as a component of Nicaragua's *Red de Protección Social* (RPS) with funding from the InterAmerican Development Bank. The objectives of RPS were to supplement income, reduce school dropout, and improve the health and nutrition of children.

To achieve these objectives, a series of three transfers were provided to eligible households. The first of these was a food security transfer (the *bono alimentario*), which provided US$224 per household per year. This transfer was contingent on attendance at monthly health education workshops and bringing children to well-child checkups (monthly for children under 2 and every two months for children 2–5). The second transfer was a school attendance transfer (the *bono escolar*), which provided US$112 per household per year contingent on enrollment and regular school attendance for each child aged 7–13 years who had not yet completed fourth grade. This amount was fixed per household regardless of the number of eligible children.

Finally, the program provided a school supplies transfer of US$21 per eligible child per year, also contingent on school enrollment. In this case, eligible children include those of ages 7–13, and ineligible children of age 14 and up.[1] We exploit this eligibility rule to estimate the program effects on children by eligibility status.

The pilot program followed an experimental design to facilitate evaluation. Forty-two eligible *comarcas* (communities) were selected based on a high poverty rate and sufficient administrative and school infrastructure to successfully implement the program. *Comarcas* were randomly assigned to treatment and control groups during the pilot period from 2000 to 2002. During this time, the International Food Policy Research Institute conducted an annual survey of households with children in all 42 *comarcas* to gather baseline and follow-up data.

4. Empirical estimation

The empirical objective of this paper is to estimate the direct effects of RPS on eligible and ineligible children, as well as the indirect effects of sibling eligibility. We can observe the latter effects by comparing households with and without siblings of different ages. In addition to predicted treatment effects, we expect that school enrollment and child labor would change as children age regardless of the treatment. We estimate the effects of the program through difference-in-differences (DID) models that estimate the effects of the treatment on changes in outcome measures for the treated. The first difference is the change over time, and the second difference is the gap in outcomes between the treated and untreated.

To examine the effect of an individual's program eligibility on the effects of RPS, we first divide the observations based on a child's eligibility status during the implementation period and estimate the effects of treatment for each age group: (1) ages 6–10 during year 1, and eligible for RPS throughout implementation; (2) ages 11–13 during year 1, and aging out of RPS eligibility during the implementation; or (3) age 14 and up during year 1, and never eligible for RPS. For each age group and outcome, we estimate the following:

$$Y_{it} = \alpha_1(\text{RPS}) + \alpha_{2t}(\text{post}) + \alpha_{3t}(\text{post} \times \text{RPS}) + \varepsilon_{it}, \tag{1}$$

where child i is either treated or untreated based on the period t and her/his community's random assignment to RPS. The DID model estimates the effect of baseline differences in treatment and control groups (α_1), the effect of maturation and history (α_2), and the effect of RPS on treated communities (α_3).[2] We estimate Equation (1) separately for boys and girls, and include robust standard errors for clustering within communities. We also include specifications that control for observable child and family characteristics.

Our second empirical strategy examines the effects of sibling eligibility. To do this, we first divide the sample into children who were eligible for RPS by age and those who were not. We also identify the presence of eligible and ineligible siblings by age and include dummy variables to indicate the presence of each type of sibling. We then add interactions between the post-treatment variable and the presence of an eligible sibling and the presence of an ineligible sibling. These interaction effects measure the effects of RPS through sibling eligibility. Children without siblings provide the appropriate comparison within treatment and control groups as any indirect effects of

RPS should be absent in these families. While RPS may influence family composition, it is reasonable to assume that the composition of school-aged siblings is exogenous, since these children were born at least six years prior to the program. For each eligibility group and gender, we estimate:

$$\begin{aligned} Y_{it} = & \beta_1(\text{RPS}) + \beta_{2t}(\text{post}) + \beta_{3t}(\text{post} \times \text{RPS}) + \beta_{4it}(\text{eligible sibling}) \\ & + \beta_{5it}(\text{eligible sibling} \times \text{post} \times \text{RPS}) + \beta_{6it}(\text{ineligible sibling}) \\ & + \beta_{7it}(\text{ineligible sibling} \times \text{post} \times \text{RPS}) + \varepsilon_{it}, \end{aligned} \tag{2}$$

where β_5 measures the indirect effect of RPS through an eligible sibling who brings RPS subsidies to the family, and β_7 measures the indirect effect of RPS through an ineligible sibling who can provide substitute labor. We also test the effects of the siblings' gender by running separate specifications with controls for female siblings and male siblings. This tests whether families trade-off work effort differently for boys and girls under RPS eligibility rules.

5. Data

The data set for this study includes 2302 children ages 6–17 years old during the baseline year, who matured to ages 8 to 19 years during the two-year implementation. A small number of children were excluded if they were married, had children, were not residing in the household, or had missing data. We control for child and family characteristics related to human capital investments including gender, age, household size, and the mother's education level. Household size is reflected in the number of siblings, where a sibling is liberally defined as any child under age 19 in the household, not necessarily with a shared parent. We also include a dummy variable indicating if the household has a modern latrine to reflect the family's relative wealth within these low-income communities. Child outcome measures include whether the child attended school, whether the child worked in the previous week, and weekly hours worked. The dummy variable for school attendance was constructed from a survey question asking if the child 'currently attends school' (i.e. at the time of the survey). The variable for work participation is a dummy variable equal to one if a child was reported as providing any labor whether paid or unpaid, including domestic work and work on family enterprises. In our robustness checks, we test RPS effects on different types of labor. Work effort is the number of total hours worked (paid or unpaid) in the week prior to the survey, as reported by the parent. From the child's age at the pretest, we determined eligibility for RPS in each program year.

Table 1 displays mean values of baseline individual demographics and pre- and post-RPS outcome variables in the treatment and control groups. The average child in the sample is 10.4 years old, has 4 siblings, and has a mother with 1.6 years of schooling. Approximately three quarters of the sample was eligible for RPS at baseline. The treatment and control groups are statistically similar across all demographic variables. At all times, outcomes vary significantly by gender with boys reporting lower school enrollment, greater work participation, and more hours worked, a finding supported by other studies in Nicaragua and Central America (Guarcello et al. 2006).

Importantly for measuring causal program effects, some baseline outcomes for boys are statistically significantly different in the treatment and control groups. Boys living

Table 1. Summary statistics and outcomes for RPS treatment and control groups.

	All		Boys			Girls	
	Control	Treatment	Control	Treatment		Control	Treatment
Baseline demographics							
Female	0.480	0.480	0.000	0.000		1.000	1.000
	(0.500)	(0.500)	(0.000)	(0.000)		(0.000)	(0.000)
Age	10.489	10.416	10.620	10.557		10.348	10.263
	(3.061)	(3.007)	(3.090)	(2.983)		(3.026)	(3.028)
Siblings	4.266	4.095	4.283	4.024	*	4.249	4.171
	(2.087)	(2.286)	(2.145)	(2.264)		(2.024)	(2.310)
Latrine in household	0.607	0.638	0.620	0.658		0.593	0.617
	(0.489)	(0.481)	(0.486)	(0.475)		(0.492)	(0.487)
Mother's highest grade	1.570	1.587	1.491	1.572		1.656	1.604
	(1.707)	(1.711)	(1.631)	(1.668)		(1.783)	(1.757)
Period 1 outcomes (*pre-RPS*)							
Attends school	0.673	0.697	0.623	0.692	*	0.727	0.701
	(0.469)	(0.460)	(0.485)	(0.462)		(0.446)	(0.458)
Works	0.209	0.188	0.344	0.285	*	0.063	0.083
	(0.407)	(0.391)	(0.476)	(0.452)		(0.244)	(0.276)
Hours worked per week	6.177	5.092	10.656	7.844	*	1.316	2.108

	(14.066)	(12.716)		(17.316)	(15.094)		(6.479)	(8.549)	
Period 2 outcomes (*post-RPS*)									
Attends school	0.701	0.869	*	0.658	0.865	*	0.749	0.873	*
	(0.458)	(0.338)		(0.475)	(0.342)		(0.434)	(0.334)	
Works	0.204	0.139	*	0.351	0.241	*	0.045	0.028	
	(0.403)	(0.346)		(0.478)	(0.428)		(0.207)	(0.166)	
Hours worked per week	7.114	4.367	*	12.279	7.679	*	1.507	0.774	
	(15.423)	(12.317)		(18.506)	(15.434)		(8.013)	(5.768)	
Period 3 outcomes (*post-RPS*)									
Attends school	0.690	0.818	*	0.644	0.808	*	0.740	0.829	*
	(0.463)	(0.386)		(0.479)	(0.394)		(0.439)	(0.377)	
Works	0.275	0.194	*	0.450	0.332	*	0.084	0.044	*
	(0.446)	(0.396)		(0.498)	(0.471)		(0.277)	(0.206)	
Hours worked per week	9.980	6.212	*	16.236	10.710	*	3.188	1.332	*
	(17.991)	(14.284)		(20.092)	(17.271)		(12.190)	(7.502)	
Number of children	1122	1180		584	614		538	566	

Note: Sample includes children of ages 6–16 during period 1 with full data for demographic and outcomes variables.
*Difference between treatment and control is significantly different from zero ($p < 0.05$) based on two-sample t-test (for demographics and hours worked) or two-sample test of proportions (for attends school and works).

in communities randomly assigned to the treatment have higher school attendance and lower work rates. Girls are significantly more likely to work in the treatment group. For this reason, evaluation of RPS requires DID regression analysis with control variables, as described above, instead of simple comparison of group means. Because of this artifact of imperfect random assignment and the remaining possibility of selection on unobservables, we must exercise caution in interpreting results as causal.[3]

Treatment and controls groups saw increases in school enrollment and decreases in work in period 2. The gaps between treatment and control groups are larger after implementation for all outcomes except unpaid work, with larger gaps for boys than girls. Based on these summary statistics, the RPS appears to have been an effective strategy to increase school enrollment and decrease child labor in treatment communities, particularly for boys.

In any experiment, it is important to address the problem of sample attrition. The RPS experiment included household attrition of 8.1% between baseline and first follow-up and 11.6% between baseline and second follow-up. In total, 14% of the baseline sample is missing from the first or second follow-up surveys. The level of attrition was not found to be significantly different between control and treatment groups. However, the initial evaluation found that missing households 'were more likely to have an older, more educated household head, larger family size, higher predicted expenditures, and more land' (Maluccio and Flores 2005, 19).

Attrition in the analytic subsample is reported in Appendix. The child attrition rate is 29% in control group, and 26% in the treatment group. Attrition affects baseline equivalence differently for each group. In the control group, children who are omitted due to attrition were more likely to be female and less likely to attend school, but similar across other demographics and labor outcomes. In the treatment group, children who are omitted due to attrition are older, have more siblings, are less likely to have a latrine in the household, and have lower mother's education. The omitted children in the treatment group are also significantly different across all three outcome variables. Thus, differential attrition may bias the results here, as the treatment group sample excludes relatively worse off children at a higher rate than the control group.

6. Regression results

6.1. *Effects of eligibility*

We begin by estimating the direct effect of RPS eligibility using an analytic sample that includes all children up to age 16 at baseline. This sample contains three distinct groups: (1) children aged 6–11 at baseline who were eligible for RPS during both post-test; (2) children aged 12–13 at baseline, who were originally eligible for RPS and aged out of eligibility; and (3) children aged 14 to 16 at baseline, who were never eligible for RPS. We estimate effects of RPS separately for each age group for boys and girls. Results for boys are displayed in Table 2 for school attendance, Table 3 for work, and Table 4 for work hours. Results for girls are displayed in Tables 5–7. Preprogram differences in the outcome variables by treatment condition are measured by the coefficients on *RPS*, and maturation effects are measured by the coefficients on *post*. The coefficient on the interaction of *RPS* $\times$ *post* measures the effect of the treatment on the treated.

Table 2. DID estimates of the probability of attending school by age group, boys only.

	(1)	(2)	(3)	(4)	(5)	(6)
	Ages 6–11	Ages 12–13	Ages 14–16	Ages 6–11	Ages 12–13	Ages 14–16
	(Always eligible)	(Ages out of eligibility)	(Always ineligible)	(Always eligible)	(Ages out of eligibility)	(Always ineligible)
RPS	0.060	0.072	0.040	0.038	0.078	0.050
	(0.058)	(0.081)	(0.079)	(0.050)	(0.079)	(0.074)
Post	0.083*	0.000	−0.094*	0.068*	0.230***	0.126*
	(0.034)	(0.050)	(0.038)	(0.031)	(0.058)	(0.051)
RPS × post	0.118*	0.122	0.081	0.125*	0.128	0.076
	(0.051)	(0.073)	(0.062)	(0.051)	(0.072)	(0.061)
Age				0.254***	−0.766	−0.551
				(0.046)	(0.457)	(0.281)
Age squared				−0.013***	0.023	0.013
				(0.002)	(0.017)	(0.008)
Latrine in household				0.059*	0.049	0.075
				(0.025)	(0.055)	(0.048)
Mother highest grade				0.058***	0.017	0.018
				(0.010)	(0.017)	(0.010)
Siblings				−0.008	0.013	0.000
				(0.005)	(0.010)	(0.011)
No. of observations	2172	663	759	2172	663	759
No. of children	724	221	253	724	221	253

Notes: Sample includes all boys with complete data within each age group. Results are displayed as marginal effects from linear probability model. School attendance is reported by parents. Standard errors reported in parentheses are robust for clustering within communities.
*$p < 0.01$.
**$p < 0.05$.
***$p < 0.001$.

Results are presented both with and without control variables. The issues of a lack of baseline of equivalence and differential attrition suggest that regressions with control variables are most needed, but the results are similar across the two specifications. The regressions by age estimate a positive maturation effect on schooling for all three age groups. RPS is associated with a significant 12.5% increase in the probability of attending school for boys who are always eligible. Boys who are sometimes eligible have a similar positive effect, but the coefficient is not significant. The estimated effect on boys who are never eligible is smaller and not significant.

The effect of RPS on work participation is negative but not significant for the always eligible group, who also work less due to age than the other groups. Boys who are sometimes eligible for RPS have a significant 19.8% reduction in the probability of work associated with RPS implementation. Boys who are never eligible have a positive but insignificant effect of RPS on work participation.

The effect of RPS on boys' hours worked is negative for all three groups, but the coefficients are significant only for those boys who are always or sometimes eligible for RPS. RPS is associated with an estimated reduction in work of 2.8 hours for the youngest group and 8.7 hours for the sometimes eligible group. Overall, RPS appears to have the largest effect on schooling for boys who are always eligible and on work for boys who age out of eligibility during the program. We find no significant effects of RPS on ineligible boys in this specification.

Table 3. DID estimates of the probability of working by age group, boys only.

	(1)	(2)	(3)	(4)	(5)	(6)
	Ages 6–11	Ages 12–13	Ages 14–16	Ages 6–11	Ages 12–13	Ages 14–16
	(Always eligible)	(Ages out of eligibility)	(Always ineligible)	(Always eligible)	(Ages out of eligibility)	(Always ineligible)
RPS	−0.043	0.036	−0.109	−0.048	0.031	−0.111
	(0.030)	(0.090)	(0.068)	(0.028)	(0.090)	(0.071)
Post	0.033	0.127*	0.060	−0.048	−0.089	−0.072
	(0.024)	(0.057)	(0.036)	(0.027)	(0.074)	(0.049)
RPS × post	−0.048	−0.184*	0.048	−0.050	−0.198*	0.052
	(0.034)	(0.077)	(0.059)	(0.034)	(0.077)	(0.059)
Age				−0.067	0.135	0.379
				(0.038)	(0.443)	(0.276)
Age squared				0.006**	0.000	−0.009
				(0.002)	(0.016)	(0.008)
Latrine in household				−0.018	−0.004	−0.013
				(0.020)	(0.066)	(0.045)
Mother highest grade				0.002	−0.009	−0.004
				(0.009)	(0.017)	(0.010)
Siblings				0.006	−0.008	0.005
				(0.005)	(0.012)	(0.010)
No. of observations	2172	663	759	2172	663	759
No. of children	724	221	253	724	221	253

Notes: Sample includes all boys with complete data within each age group. Each child is observed during one pre-RPS period and two post-RPS periods. Results are displayed as marginal effects from linear probability model. Work includes paid and unpaid work, including wage labor, domestic work, and farm labor. Standard errors reported in parentheses are robust for clustering within communities.
$^{*}p < 0.01$.
$^{**}p < 0.05$.
$^{***}p < 0.001$.

The results for girls disaggregated by age also suggest that RPS had effects on eligible children only. RPS is associated with a significant 15.8% increase in the probability of enrollment for girls who were always eligible, and a 17.8% increase in the probability of enrollment for those who were sometimes eligible. For girls who were never eligible, the model estimates a negative but insignificant coefficient of RPS implementation. There are no significant effects of RPS on the probability of girls' work. For the youngest group only, RPS is associated with a significant reduction in hours worked by approximately half an hour per week.

6.2. *Effects of sibling eligibility*

The group of ineligible children contains some children from families that benefited from RPS through a sibling. We next disaggregate by eligibility for RPS at time *t* and add dummy variables to interact the presence of an eligible or ineligible sibling (*RPS* × *post* × *eligible sibling* and *RPS* × *post* × *ineligible sibling*). Because children may be influenced differently by same and opposite gender siblings, we also run specifications by the sibling's gender.

Summary statistics for eligible and ineligible children by RPS treatment assignments are displayed in Table 8. Importantly, a child may be represented in both

Table 4. DID estimates of weekly hours worked by age group, boys only.

	(1)	(2)	(3)	(4)	(5)	(6)
	Ages 6–11	Ages 12–13	Ages 14–16	Ages 6–11	Ages 12–13	Ages 14–16
	(Always eligible)	(Ages out of eligibility)	(Always ineligible)	(Always eligible)	(Ages out of eligibility)	(Always ineligible)
RPS × post	−2.552***	−7.985***	−2.238	−2.771***	−8.734***	−2.243
	(0.714)	(2.160)	(1.686)	(0.675)	(2.013)	(1.644)
Age				−5.234**	−6.524	37.451
				(1.620)	(34.179)	(21.830)
Age squared				0.309***	0.427	−1.027
				(0.084)	(1.211)	(0.648)
Latrine in household				−0.258	0.036	−0.557
				(0.622)	(1.845)	(2.157)
Mother highest grade				0.321	0.740	−0.789
				(0.195)	(0.584)	(0.450)
Siblings				0.247	−0.135	0.006
				(0.163)	(0.457)	(0.441)
Hours worked, period 1	−0.641***	−0.619***	−0.692***	−0.698***	−0.615***	−0.714***
	(0.067)	(0.051)	(0.041)	(0.068)	(0.047)	(0.040)
No. of observations	1448	442	506	1448	442	506
No. of children	724	221	253	724	221	253

Notes: Sample includes all boys with complete data within each age group. Each child is observed during two post-RPS periods. Results are displayed as marginal effects from OLS model. Work includes paid and unpaid work, including wage labor, domestic work, and farm labor. Standard errors reported in parentheses are robust for clustering within communities.
*$p < 0.01$.
**$p < 0.05$.
***$p < 0.001$.

subsamples if he/she ages out of RPS eligibility over time. We have baseline equivalence for all demographics and outcome variables for the subsample of ineligible children, and 80% of ineligible children have an eligible sibling. In the sample of eligible children, we have baseline equivalence across all variables, except the control group that is more likely to have an ineligible sibling, reflecting the overall larger family size in the control groups in the RPS data set. All post-RPS outcomes are significantly better in the treatment group than the control group for both eligible and ineligible children. These summary statistics suggest that ineligible children benefited from RPS through both greater opportunities to attend school and reduced labor participation and effort.

The regression results for the subset of ineligible children are displayed in Table 9 for boys and Table 10 for girls. There are no direct positive effects of RPS implementation on ineligible boys, but there are positive effects for boys with an eligible sibling in the house. For school attendance, the coefficients for the interaction of an eligible sibling, sister, or brother are all positive. Only the coefficient for *RPS × post × eligible sister* is significant. This coefficient estimates that an ineligible boy is 12.3% more likely to attend school if he resides with an eligible sister – an indirect effect nearly equal to the direct effect of RPS on overage boys, reported in Table 2. Thus, much

Table 5. DID estimates of the probability of attending school by age group, girls only.

	(1)	(2)	(3)	(4)	(5)	(6)
	Ages 6–11	Ages 12–13	Ages 14–16	Ages 6–11	Ages 12–13	Ages 14–16
	(Always eligible)	(Ages out of eligibility)	(Always ineligible)	(Always eligible)	(Ages out of eligibility)	(Always ineligible)
RPS	−0.040	0.024	−0.034	−0.038	0.010	−0.033
	(0.062)	(0.058)	(0.059)	(0.051)	(0.057)	(0.058)
Post	0.089***	−0.096	−0.118*	0.059**	0.042	−0.008
	(0.020)	(0.053)	(0.046)	(0.018)	(0.076)	(0.083)
RPS × post	0.160**	0.181*	−0.031	0.158**	0.178*	−0.028
	(0.056)	(0.074)	(0.063)	(0.055)	(0.076)	(0.065)
Age				0.333***	−0.158	−0.203
				(0.044)	(0.443)	(0.406)
Age squared				−0.017***	0.003	0.004
				(0.002)	(0.016)	(0.012)
Latrine in household				0.060*	0.088	0.028
				(0.024)	(0.050)	(0.071)
Mother highest grade				0.053***	−0.007	0.028*
				(0.008)	(0.016)	(0.014)
Siblings				−0.011*	−0.013	−0.004
				(0.005)	(0.010)	(0.008)
No. of observations	2121	582	609	2121	582	609
No. of children	707	194	203	707	194	203

Notes: Sample includes all girls with complete data within each age group. Each child is observed during one pre-RPS and two post-RPS periods. Results are displayed as marginal effects from linear probability model. School attendance is reported by parents. Standard errors reported in parentheses are robust for clustering within communities.
*$p < 0.01$.
**$p < 0.05$.
***$p < 0.001$.

of the positive effect of RPS on older boys' probability of attending schools appears to come from subsidies derived from sending a younger sister to school. Contrary to a prediction of large displacement effects, having an eligible sibling (of either sex) is associated with a statistically significant *reduction* in both the probability of working (−16.7%), and hours worked (−6 hours per week). The effects of a younger, eligible brother on work and hours worked are also significant. As expected, there is no significant effect of an ineligible sibling on an ineligible child.

The regressions for ineligible girls show no significant effect of *RPS* × *post* × *eligible sibling*. However, the results are notable in contrast to the results for boys. A sister's eligibility for RPS positively influences school enrollment for older brothers but not older sisters.

We next investigate the effect of displacement on RPS-eligible children. If displacement occurs, we should see larger program effects for children with an ineligible sibling to provide substitute labor. Results of estimates for eligible boys are displayed in Table 11 and eligible girls in Table 12. We find no evidence of positive displacement effects from older siblings for eligible boys or girls. Instead, income effects dominate. The presence of an eligible sister interacted with RPS implementation is associated with

Table 6. DID estimates of the probability of working by age group, girls only.

	(1)	(2)	(3)	(4)	(5)	(6)
	Ages 6–11	Ages 12–13	Ages 14–16	Ages 6–11	Ages 12–13	Ages 14–16
	(Always eligible)	(Ages out of eligibility)	(Always ineligible)	(Always eligible)	(Ages out of eligibility)	(Always ineligible)
RPS	0.015	0.011	0.051	0.015	0.026	0.047
	(0.023)	(0.066)	(0.056)	(0.022)	(0.063)	(0.057)
Post	0.006	−0.059	0.039	−0.007	−0.110*	−0.029
	(0.009)	(0.037)	(0.050)	(0.010)	(0.048)	(0.057)
RPS × post	−0.026	−0.046	−0.123	−0.027	−0.046	−0.124
	(0.024)	(0.069)	(0.072)	(0.024)	(0.068)	(0.073)
Age				−0.003	−0.079	0.210
				(0.018)	(0.275)	(0.326)
Age squared				0.001	0.004	−0.005
				(0.001)	(0.010)	(0.010)
Latrine in household				−0.010	−0.077*	0.002
				(0.008)	(0.031)	(0.037)
Mother highest grade				−0.002	0.002	−0.000
				(0.004)	(0.008)	(0.008)
Siblings				0.004	0.017*	0.005
				(0.003)	(0.007)	(0.006)
No. of observations	2121	582	609	2121	582	609
No. of students	707	194	203	707	194	203

Notes: Sample includes all girls with complete data within each age group. Each child is observed during one pre-RPS and two post-RPS periods. Results are displayed as marginal effects from linear probability models. Work includes paid and unpaid work, including wage labor, domestic work, and farm labor. Standard errors reported in parentheses are robust for clustering within communities.
*$p < 0.01$.
**$p < 0.05$.
***$p < 0.001$.

a significant reduction in the probability that an eligible girl will work and a significant reduction girls' work hours.

7. Effects on different types of work

The aforementioned results aggregate child labor in a single category. It is possible that gender differences in RPS effects are related to gender differences in the type of labor performed. For example, in gendered labor markets, it may be easier to substitute wage labor between brothers than between a brother and sister. We also tested sibling effects of three specific types of labor: paid, unpaid, and farm labor.[4] We find that the negative effects of an eligible sibling on an ineligible boy are focused on unpaid labor and farm labor, rather than wage labor. For ineligible boys, unpaid labor is significantly reduced by an eligible male sibling, and farm labor is reduced by siblings of either sex. Similarly, for ineligible girls, an eligible brother significantly reduces the probability of farm labor, while there are no effects for paid or unpaid labor.

For RPS-eligible boys, we find no differential effects of RPS on types on work. For RPS-eligible sisters, having another eligible sister reduces the likelihood of both paid and farm work, and having an eligible brother reduces the likelihood of unpaid

Table 7. DID estimates of weekly hours worked by age group, girls only.

	(1)	(2)	(3)	(4)	(5)	(6)
	Ages 6–11	Ages 12–13	Ages 14–16	Ages 6–11	Ages 12–13	Ages 14–16
	(Always eligible)	(Ages out of eligibility)	(Always ineligible)	(Always eligible)	(Ages out of eligibility)	(Always ineligible)
RPS × post	−0.503*	−1.952	−3.832	−0.486*	−1.855	−4.052
	(0.215)	(1.014)	(1.973)	(0.220)	(0.948)	(2.030)
Age				−0.601	−20.121	40.040
				(1.006)	(24.677)	(25.545)
Age squared				0.037	0.771	−1.160
				(0.052)	(0.883)	(0.776)
Latrine in household				−0.260	0.970	0.037
				(0.221)	(1.303)	(1.916)
Mother highest grade				0.036	0.419	0.118
				(0.096)	(0.288)	(0.399)
Siblings				0.099	0.151	0.471
				(0.062)	(0.234)	(0.329)
Hours worked, period 1	−0.968***	−0.698***	−0.903***	−0.984***	−0.685***	−0.908***
	(0.037)	(0.143)	(0.070)	(0.037)	(0.152)	(0.072)
No. of observations	1414	388	406	1414	388	406
No. of children	707	194	203	707	194	203

Notes: Sample includes all girls with complete data within each age group. Each child is observed during two post-RPS periods. Results are displayed as marginal effects from OLS model. Work includes paid and unpaid work, including wage labor, domestic work, and farm labor. Standard errors reported in parentheses are robust for clustering within communities.
*$p < 0.01$.
**$p < 0.05$.
***$p < 0.001$.

work. Again, we find no effects of labor displacement through ineligible siblings influencing the outcomes of eligible children and only positive income effects of additional eligible siblings.

8. Discussion

As CCTs gain popularity as an efficient and equitable approach to poverty reduction, it is important to understand the complex responses of families to characteristics of transfer schemes and the potential effects on children. The Nicaraguan context provides an excellent setting to study the impact of CCTs on child labor, as the communities in this experiment exhibited high poverty, high rates of initial child labor, and significant gender gaps, and the program provided cash transfers that were relatively large as an addition to family wealth. While prior studies find no effects of CCTs on siblings' labor, we find significant effects of the Nicaraguan CCT on the schooling and work of both eligible children and ineligible siblings. As predicted by Ferreira, Filmer, and Schady (2009), we find the largest effect for eligible children who benefit directly from income and substitution effects, with smaller effects for ineligible boys through eligible sisters. We also find that for eligible sisters, an eligible brother can reduce

Table 8. Summary statistics for eligible and ineligible samples.

	Eligible at time t			Ineligible at time t		
	Control	Treatment		Control	Treatment	
Female	0.480	0.480		0.478	0.477	
	(0.500)	(0.500)		(0.500)	(0.500)	
Age	9.127	9.193		14.273	14.293	
	(2.220)	(2.214)		(1.497)	(1.542)	
Number of siblings	4.256	4.139		4.296	3.954	
	(2.065)	(2.253)		(2.151)	(2.387)	
Latrine in household	0.581	0.619		0.680	0.700	
	(0.494)	(0.486)		(0.467)	(0.459)	
Mother's highest grade	1.048	1.097		3.020	3.141	
	(1.279)	(1.290)		(1.899)	(1.939)	
Has an eligible sibling	0.834	0.855		0.798	0.827	
	(0.372)	(0.352)		(0.402)	(0.379)	
Has an ineligible sibling	0.490	0.425	*	0.481	0.449	
	(0.500)	(0.495)		(0.501)	(0.498)	
Period 1 outcomes (*pre-RPS*)						
Attends school	0.712	0.734		0.566	0.580	
	(0.453)	(0.442)		(0.497)	(0.495)	
Works	0.135	0.135		0.418	0.357	
	(0.341)	(0.342)		(0.494)	(0.480)	
Hours worked	3.321	3.045		14.111	11.583	
	(10.168)	(9.536)		(19.380)	(18.207)	
Period 2 outcomes (*post-RPS*)						
Attends school	0.781	0.976	*	0.568	0.689	*
	(0.414)	(0.154)		(0.496)	(0.464)	
Works	0.094	0.047	*	0.389	0.293	*
	(0.292)	(0.212)		(0.488)	(0.456)	
Hours worked	2.814	0.912	*	14.329	10.177	*
	(9.689)	(5.001)		(19.923)	(17.646)	
Period 3 outcomes (*post-RPS*)						
Attends school	0.836	0.966	*	0.517	0.658	*
	(0.371)	(0.182)		(0.500)	(0.475)	
Works	0.140	0.062	*	0.435	0.337	*
	(0.347)	(0.241)		(0.496)	(0.473)	
Hours worked	3.557	1.259	*	17.604	11.566	*
	(10.466)	(5.657)		(21.708)	(18.306)	
Number of children	851	922		537	590	

Notes: Sample includes students who are eligible or ineligible at time t for RPS based on age. A child who ages out of RPS eligible between period 1 and period 3 will appear in both samples at different times. *identifies significant differences between treatment and control groups ($p < 0.05$).

the probability of farm labor. We find no significant effects of older siblings on eligible siblings. This suggests that income effects are large for boys and smaller for girls, and displacement is either small or zero.

The context in Nicaragua is likely to be particularly sensitive to both income and substitution effects, as both poverty and child labor are high in the targeted communities. Child labor was especially high for boys, and we find that boys are more sensitive to the RPS than girls. This evidence supports Glick's (2008) hypothesis that demand for schooling and supply of child labor are more income elastic for groups that receive lower investments in human capital. Our results also support the prediction that a gender neutral CCT policy can reduce gender gaps, as the group with initially

Table 9. DID estimates of the effect of RPS on ineligible boys with sibling interactions.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Attends School			Works			Weekly Hours Worked		
	Any sibling	Sisters only	Brothers only	Any sibling	Sisters only	Brothers only	Any sibling	Sisters only	Brothers only
RPS	0.050	0.047	0.052	−0.121	−0.122	−0.121			
	(0.062)	(0.062)	(0.061)	(0.065)	(0.065)	(0.065)			
Post	0.133***	0.118**	0.141***	−0.088*	−0.089*	−0.085*			
	(0.036)	(0.036)	(0.031)	(0.041)	(0.039)	(0.039)			
RPS × post	0.000	0.042	0.087	0.141	0.007	0.090	1.782	−3.251	−2.262
	(0.081)	(0.068)	(0.061)	(0.080)	(0.071)	(0.072)	(2.880)	(2.313)	(1.999)
Eligible sibling	0.013	−0.020	0.049	0.030	−0.015	0.034	−0.725	−1.566	0.605
	(0.048)	(0.033)	(0.038)	(0.049)	(0.035)	(0.038)	(1.972)	(1.632)	(1.682)
RPS × post × eligible sibling	0.111	0.123**	0.021	−0.167**	−0.007	−0.134**	−5.981*	−2.034	−4.791*
	(0.060)	(0.045)	(0.052)	(0.055)	(0.052)	(0.049)	(2.734)	(2.405)	(2.349)
Ineligible sibling	0.004	0.065	−0.006	0.006	−0.012	−0.012	3.926	1.195	1.006
	(0.046)	(0.040)	(0.037)	(0.043)	(0.033)	(0.040)	(2.125)	(1.816)	(1.801)
RPS × post × ineligible sibling	0.014	−0.015	−0.011	0.002	0.041	−0.004	−2.923	−0.943	0.130
	(0.068)	(0.057)	(0.050)	(0.057)	(0.051)	(0.061)	(2.460)	(2.283)	(2.389)
No. of observations	1347	1347	1347	1347	1347	1347	1044	1044	1044
No. of children	599	599	599	599	599	599	599	599	599
r^2	0.302	0.304	0.300	0.228	0.222	0.227	0.381	0.372	0.373

Notes: Sample includes one pre-RPS and two post-RPS periods. Analytic sample includes boys who were not eligible for RPS based on age at time t. Sibling eligibility is also determined by age at time t and represented by a dummy variable equal to one if there is an age-eligible sibling in the household. Siblings are defined as all children in the household and need not have the same parents. Results are displayed as marginal effects from a linear probability model. School attendance is reported by parents. Work includes paid and unpaid work, including wage labor, domestic work, and farm labor. Standard errors reported in parentheses are robust for clustering within communities.

$^{*}p < 0.01$.

$^{**}p < 0.05$.

$^{***}p < 0.001$.

Table 10. DID estimates of the effect of RPS on ineligible girls with sibling interactions.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Attends school			Works			Weekly hours worked		
Siblings	Any	Sister	Brother	Any	Sister	Brother	Any	Sister	Brother
RPS	−0.014	−0.017	−0.019	0.014	0.012	0.016			
	(0.043)	(0.043)	(0.041)	(0.049)	(0.048)	(0.049)			
Post	0.043	0.031	0.045	−0.018	−0.019	−0.020			
	(0.035)	(0.036)	(0.032)	(0.040)	(0.038)	(0.039)			
RPS × post	0.029	0.067	0.100	−0.088	−0.114	−0.037	−4.829*	−5.115**	−2.283
	(0.071)	(0.056)	(0.057)	(0.066)	(0.064)	(0.063)	(2.141)	(1.685)	(1.611)
Eligible sibling	−0.097	−0.050	−0.046	0.015	−0.005	0.025	−0.584	−1.486	0.628
	(0.058)	(0.052)	(0.043)	(0.028)	(0.028)	(0.026)	(1.550)	(1.341)	(1.633)
RPS × post × eligible sibling	0.071	0.089	−0.004	0.019	0.059	−0.043	2.024	3.194	−1.041
	(0.073)	(0.059)	(0.054)	(0.034)	(0.041)	(0.037)	(1.668)	(1.736)	(1.824)
Ineligible sibling	−0.004	0.086	−0.027	−0.034	−0.056*	−0.017	−0.988	−2.047	−0.640
	(0.052)	(0.047)	(0.055)	(0.024)	(0.027)	(0.020)	(1.441)	(1.893)	(1.301)
RPS × post × ineligible sibling	−0.018	−0.093	−0.051	0.018	0.073*	−0.005	0.967	2.580	0.425
	(0.064)	(0.058)	(0.070)	(0.033)	(0.034)	(0.032)	(1.725)	(2.122)	(1.603)
No. of observations	1172	1172	1172	1172	1172	1172	895	895	895
No. of children	528	528	528	528	528	528	528	528	528
r^2	0.220	0.222	0.220	0.037	0.042	0.036	0.375	0.378	0.374

Notes: Sample includes one pre-RPS and two post-RPS periods. Analytic sample includes girls who were not eligible for RPS based on age at time *t*. Sibling eligibility is also determined by age at time *t* and represented by a dummy variable equal to one if there is an age-eligible sibling in the household. Siblings are defined as all children in the household and need not have the same parents. Results are displayed as marginal effects from a linear probability model. School attendance is reported by parents. Work includes paid and unpaid work, including wage labor, domestic work, and farm labor. Standard errors reported in parentheses are robust for clustering within communities.
*$p < 0.01$.
**$p < 0.05$.
***$p < 0.001$.

Table 11. DID estimates of the effect of RPS on eligible boys with sibling interactions.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Attends school			Works			Weekly hours worked		
Siblings	Any	Sister	Brother	Any	Sister	Brother	Any	Sister	Brother
RPS	0.052	0.054	0.051	−0.027	−0.026	−0.027			
	(0.050)	(0.049)	(0.049)	(0.037)	(0.037)	(0.036)			
Post	0.093**	0.088*	0.092**	−0.058*	−0.053	−0.057*			
	(0.033)	(0.034)	(0.032)	(0.028)	(0.028)	(0.027)			
RPS × post	0.131*	0.143**	0.113*	−0.111*	−0.086*	−0.085	−3.328**	−2.799**	−2.475*
	(0.062)	(0.052)	(0.054)	(0.050)	(0.042)	(0.044)	(1.190)	(0.837)	(1.112)
Eligible sibling	0.018	0.025	−0.006	−0.017	0.016	−0.003	−0.534	0.934	0.496
	(0.031)	(0.027)	(0.030)	(0.027)	(0.028)	(0.022)	(0.925)	(0.736)	(1.043)
RPS × post × eligible sibling	−0.012	−0.010	0.013	0.059	0.030	0.014	0.934	−1.015	−0.378
	(0.036)	(0.034)	(0.034)	(0.036)	(0.037)	(0.032)	(1.082)	(1.080)	(1.193)
Ineligible sibling	−0.010	0.037	−0.029	−0.015	−0.030	−0.014	0.915	−0.509	1.046
	(0.030)	(0.030)	(0.029)	(0.020)	(0.025)	(0.024)	(0.968)	(0.949)	(1.070)
RPS × post × ineligible sibling	0.015	−0.017	0.032	−0.013	−0.009	0.007	−0.737	0.597	−0.982
	(0.032)	(0.033)	(0.033)	(0.038)	(0.033)	(0.041)	(1.153)	(1.150)	(1.288)
No. of observations	2247	2247	2247	2247	2247	2247	1352	1352	1352
No. of children	913	913	913	913	913	913	913	913	913
r^2	0.137	0.139	0.138	0.148	0.150	0.147	0.343	0.343	0.343

Notes: Sample includes one pre-RPS and two post-RPS periods. Analytic sample includes boys who were eligible for RPS based on age at time *t*. Sibling eligibility is also determined by age at time *t* and represented by a dummy variable equal to one if there is was an age-eligible sibling in the household. Siblings are defined as all children in the household and need not have the same parents. Results are displayed as marginal effects from a linear probability model. School attendance is reported by parents. Work includes paid and unpaid work, including wage labor, domestic work, and farm labor. Standard errors reported in parentheses are robust for clustering within communities.
$^{*}p < 0.01$.
$^{**}p < 0.05$.
$^{***}p < 0.001$.

Table 12. DID estimates of the effect of RPS on eligible girls with sibling interactions.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Attends school			Works			Weekly hours worked		
Siblings	Any	Sister	Brother	Any	Sister	Brother	Any	Sister	Brother
RPS	−0.020	−0.024	−0.023	0.021	0.022	0.020			
	(0.048)	(0.047)	(0.049)	(0.026)	(0.025)	(0.026)			
Post	0.059*	0.054*	0.062*	−0.027*	−0.028*	−0.027*			
	(0.022)	(0.022)	(0.023)	(0.013)	(0.013)	(0.013)			
RPS × post	0.160**	0.178**	0.145*	−0.029	−0.013	−0.046	−0.160	−0.100	−0.672
	(0.057)	(0.051)	(0.058)	(0.027)	(0.023)	(0.029)	(0.220)	(0.203)	(0.334)
Eligible sibling	0.003	0.017	−0.023	0.019	0.029*	0.008	0.726*	0.634*	0.391
	(0.032)	(0.030)	(0.037)	(0.011)	(0.013)	(0.011)	(0.276)	(0.269)	(0.328)
RPS × post × eligible sibling	0.024	0.023	0.016	−0.016	−0.036*	0.012	−0.682	−0.731*	−0.072
	(0.035)	(0.031)	(0.039)	(0.015)	(0.017)	(0.015)	(0.345)	(0.348)	(0.368)
Ineligible sibling	0.050	0.092**	−0.002	−0.010	0.008	−0.023	−0.302	0.078	−0.547
	(0.025)	(0.027)	(0.028)	(0.010)	(0.018)	(0.011)	(0.262)	(0.276)	(0.424)
RPS × post × ineligible sibling	−0.043	−0.093**	0.006	0.014	−0.014	0.020	0.233	−0.317	0.391
	(0.031)	(0.033)	(0.034)	(0.014)	(0.020)	(0.015)	(0.342)	(0.357)	(0.471)
No. of observations	2140	2140	2140	2140	2140	2140	1313	1313	1313
No. of children	860	860	860	860	860	860	860	860	860
r^2	0.158	0.164	0.155	0.043	0.046	0.045	0.380	0.380	0.381

Notes: Sample includes one pre-RPS and two post-RPS periods. Analytic sample includes girls who were eligible for RPS based on age at time *t*. Sibling eligibility is also determined by age at time *t* and represented by a dummy variable equal to one if there is was an age-eligible sibling in the household. Siblings are defined as all children in the household and need not have the same parents. Results are displayed as marginal effects from a linear probability model. School attendance is reported by parents. Work includes paid and unpaid work, including wage labor, domestic work, and farm labor. Standard errors reported in parentheses are robust for clustering within communities.
*$p < 0.01$.
**$p < 0.05$.
***$p < 0.001$.

lower investments will see a greater response due to income and substitution effects (Glick 2008). There is strong evidence that Nicaragua's CCT program reduced gender gaps in schooling and child labor for both eligible children and their ineligible siblings. However, all results of this and other RPS studies must be taken in context of problems of imperfect baseline equivalence and differential attrition.

These findings add to our understanding of the allocation of human capital investments and labor within families. We find positive income effects for schooling for ineligible children on top of positive substitution effects for children whose attendance is tied to the transfer. We find no evidence that older siblings are required to substitute labor for younger siblings when the younger siblings' schooling is tied to an income transfer. Instead, we find that having a younger sibling who is eligible for a transfer is positive for an ineligible sibling in both increased probability of attending school and reduced participation, particularly in unpaid and farm labor.

For policy-makers, these results contribute to a growing literature that emphasizes the importance of policy design in the success CCTs. Prior studies based on the intrahousehold allocation framework suggest that CCT success is related to how transfers are allocated, with greater benefits for children when transfers go to mothers than fathers (Adato et al. 2000; Attanasio and Lechene 2002; Gitter and Barham 2008). This study and others (Barrera-Osoria et al. 2011; Bustelo 2011; Ferreira, Filmer, and Schady 2009) suggest that it is also important how transfers are targeted with potentially greater total benefits when eligible children are those who are most likely to attend school.

Disclosure statement

No potential conflict of interest was reported by the authors.

Notes

1. Bustelo (2011) finds that compliance on the part of targeted households was high. Some ineligible students were registered in the administrative system but did not actually receive the transfer.
2. We are not able to verify that all eligible children in the data set received RPS in treatment districts or whether ineligible children in treatment districts also received RPS. Thus, the model measures the effects of 'intent-to-treat' defined by residence in a treated community. Although we are unable to confirm receipt of subsidies at the household level, we confirmed through regression analysis that, on average, households in treated and untreated districts have similar household expenditures prior to RPS, and in post-RPS periods, households in treated districts have significantly higher expenditures than households in control districts. This finding holds when controlling for other household characteristics.
3. A joint test of equality of all baseline demographics variables in Table 1 was conducted using seemingly unrelated regression on the probability of RPS treatment. We fail to reject the null hypothesis that the demographics are jointly equivalent in the treatment and control groups.
4. Available upon request.

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Appendix. Attrition in the analytic sample

	Control group			Treatment group			Attrition group		
	Analytic sample	Attrition		Analytic sample	Attrition		Control group	Treatment group	
Demographics									
Female	0.480	0.552	*	0.480	0.510		0.552	0.510	
	(0.500)	(0.498)		(0.500)	(0.501)		(0.027)	(0.028)	
Age	10.489	10.809		10.416	10.977	*	10.809	10.977	
	(3.061)	(3.405)		(3.007)	(3.359)		(0.091)	(0.191)	
Number of siblings	4.599	4.848		4.375	4.812	*	4.848	4.811	
	(2.279)	(2.859)		(2.407)	(2.819)		(0.157)	(0.160)	
Latrine in household	0.607	0.618		0.638	0.497	*	0.618	0.497	*
	(0.489)	(0.487)		(0.481)	(0.501)		(0.487)	(0.501)	
Mother's highest grade	1.484	1.503		1.603	1.158	*	1.503	1.158	*
	(1.840)	(1.981)		(1.806)	(1.632)		(1.981)	(1.632)	
Period 1 outcomes (*pre-RPS*)									
Attends school	0.673	0.603	*	0.697	0.412	*	0.603	0.412	
	(0.469)	(0.490)		(0.460)	(0.493)		(0.490)	(0.493)	
Works	0.167	0.164		0.149	0.198	*	0.164	0.198	
	(0.373)	(0.371)		(0.356)	(0.399)		(0.371)	(0.399)	
Hours worked per week	5.553	5.776		4.591	6.880	*	5.776	6.880	
	(13.819)	(14.887)		(12.521)	(15.795)		(14.887)		
N	1122	330		1180	308		330	308	
Rate of Attrition	0.294			0.261					

*Analytic sample and attrition sample are statistically different at $p < 0.05$ based on two-tailed t-test (for demographics and hours worked) or two-sample test of proportions (for dichotomous outcome variables).