Do Education and Health Conditions Matter in a Large Cash Transfer? Evidence from a Honduran Experiment

FIORELLA BENEDETTI
Harvard University

PABLO IBARRARÁN
Inter-American Development Bank

PATRICK J. MCEWAN
Wellesley College

I. Introduction

Conditional cash transfers (CCTs) provide cash to poor households, thus reducing short-run poverty, while encouraging human capital investment by requiring participants to use education and health services (Fiszbein and Schady 2009; Adato and Hoddinott 2010). The voluminous and often experimental evaluation literature is now the subject of many systematic reviews and meta-analyses. This literature shows that CCTs increase school enrollment and attendance (Petrosino et al. 2012; Saavedra and Garcia 2013; Baird, Ferreira, et al. 2014), reduce child labor on the intensive and extensive margins (Kabeer, Piza, and Taylor 2012; de Hoop and Rosati 2014), and increase the use of preventive health services among mothers (Glassman et al. 2013) and children (Lagarde, Haines, and Palmer 2007; Gaarder, Glassman, and Todd 2010; Owusu-Addo and Cross 2014).

Consistent with these results, an early Honduran evaluation of the Programa de Asignación Familiar (PRAF) found that per child transfers of no more than $50 per year had substantial effects on increasing primary school enrollment (by 8 percentage points, or 12% of the control-group enrollment rate), reducing child labor participation (by 3 percentage points, or 30%),

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and increasing various measures of health service use (Morris et al. 2004; Galiani and McEwan 2013). Children between age 6 and 12 were obligated to enroll in school and attend regularly, while children under 3 and pregnant or nursing mothers were required to regularly attend health centers. Some conditions were weakly enforced, especially school and health center attendance (Glewwe and Olinto 2004; Morris et al. 2004). But the costs of enforcing even minimal education and health conditions were nonnegligible: from 1999 to 2001, they constituted 20% of program expenditures (excluding the actual transfers).[1]

An important question, in Honduras and other resource-constrained contexts, is whether the imposition and monitoring of conditions increases outcomes beyond those of an unconditional cash transfer. Cash alone may increase demand for schooling or health services via an income effect, but a CCT decreases opportunity costs and could further occasion a substitution effect (Baird, Ferreira, et al. 2014). Whether this occurs is an empirical question, analyzed in a growing body of papers.[2]

It has been most compellingly studied in randomized experiments with unconditional and conditional treatment arms.[3] However, none of this evidence is from Latin America, where researchers have relied on nonexperimental—but perhaps exogenous—variation in the enforcement of conditions.[4] In Ecuador and Mexico, the effects on school enrollment were lower when some households believed the cash transfers were unconditional, due to quirks in program implementation (Schady and Araujo 2008; de Brauw and Hoddinott 2011). In the Ecuador experiment, however, the effects on child labor were the same, regardless of households' beliefs about education conditions (Edmonds and Schady 2012). In Colombia, health conditions were only en-

[1] Caldés, Coady, and Maluccio (2006) conduct a full cost analysis of PRAF-II. The reported percentage relies on their data but omits costs of the external evaluation and the delivery of a separate treatment (grants to schools and health centers).

[2] Baird, Ferreira, et al. (2014) review this literature on education, including the studies cited below, and find that effects on education enrollment are positively related to a qualitative index of the conditions' strength.

[3] Adding school-related conditions increased the impact of transfers on dropout rates in Malawi (Baird, McIntosh, and Özler 2011). In Burkina Faso, conditions increased school enrollment but only among subgroups of girls, younger children, and lower-ability children (Akresh, de Walque, and Kazianga 2013). The same experiment found that health center visits increased substantially among young children in the presence of conditions (Akresh, de Walque, and Kazianga 2015). In Zimbabwe, the positive effects on school attendance were similar across conditional and unconditional treatment arms (Robertson et al. 2013). A "labeled" cash transfer in Morocco—promoted as an education support program—produced large gains in attendance that were mostly unaffected by added conditions (Benhassine et al. 2015).

[4] A Colombian experiment found that secondary attendance increased with an attendance-conditioned transfer but increased even further in the presence of a graduation condition (Barrera-Osorio et al. 2011).

forced among children born before mothers registered for the CCT (Attanasio, Oppedisano, and Vera-Hernández 2015). Health center visits were substantially lower among children born after the registration date.

This article assesses whether the enforcement of education and health conditions mediates the size of effects on school enrollment, child labor participation, and health service use. It does so in the context of a new Honduran CCT (Bono 10,000) that was evaluated with a large cluster-randomized trial between 2012 and 2013. Households received transfers of either L 10,000 (approximately $500) or L 5,000, depending on conditions described below. A typical household received per capita transfers equal to 18% of median per capita consumption, substantially more than PRAF-II (Glewwe and Olinto 2004; Galiani and McEwan 2013).

Despite the larger transfers, the full-sample impacts were smaller than PRAF-II. Bono 10,000 increased enrollments by about 4 percentage points (6% of the control group enrollment rate) and reduced child labor by 1.2 percentage points (5%), although the second estimate was not statistically significant. For health service use, we find mixed results: young children in the treatment group were more likely to be regularly weighed and attend checkups, but there is little evidence that the treatment affected mothers' prenatal or postnatal use of health services. (We report a complete set of full-sample estimates, since the experiment has not been previously analyzed.)

What can explain the smaller effects in Bono 10,000? A plausible explanation is that conditions were enforced for fewer children.[5] In PRAF-II, education and health conditions were homogeneously enforced for all children. In Bono 10,000, households received the larger transfer—labeled an "education transfer"—if at least one 6–18-year-old enrolled in grades 1–9 (regardless of the number of such children in the household). Thus, education conditions were not enforced for all eligible children in multichild households. Households received the smaller transfer—labeled a "health transfer"—if children under 6 and pregnant or nursing mothers registered in a health center, but only in the absence of children eligible for the education transfer. The transfer was doubled in the presence of just one education-eligible child, but young children and mothers were no longer required to register (nor was it labeled a health transfer). We leverage both quirks in the early implementation of Bono 10,000 to identify the effect of conditions on enrollment,

[5] Another explanation is that effects were larger because the PRAF-II sample was poorer, given both the time period in which it was conducted (2000–2002, on the heels of Hurricane Mitch) and the slightly poorer municipalities sampled for the PRAF-II experiment. We subsequently compare samples using the only common data set, the 2001 census, and find that both have very high rates of extreme poverty in 2001: 89% and 84% in PRAF II and Bono 10,000, respectively.

child labor participation, and health service use. (Since 2014, following the experiment, education and health conditions have been uniformly applied to all children.)

We found that short-run effects on the enrollment and labor participation of eligible children were largest when there was only one child in the household eligible for the education transfer (i.e., a 6–18-year-old who had completed no more than eighth grade). Enrollment increased by 7.9 percentage points, while labor participation decreased by 5.8 percentage points. The effects were closer to zero and were not statistically distinguishable from zero in households with more eligible children. It is tempting to interpret this heterogeneity as the result of conditions, since the conditions were not binding for all children in larger households. But it could also be due to treatment interactions with variables that are correlated with the number of eligible children. First, the presence of more eligible children in the household decreases the per capita transfer (given a fixed household transfer), increases the mean age of eligible children, and decreases the chance that an eligible child is firstborn. Second, households with more eligible children are poorer, a stylized fact throughout Latin America.

We address the first concern by including dummy variables for discrete categories of household size, age, and child birth order, while interacting continuous household size, age, and birth order variables with treatment dummies. In these estimates, the pattern of heterogeneity by the number of eligible children persists among households at the estimation sample means of household size, child age, and birth order.

We argue that the second concern—a positive relationship between the number of children and poverty—stacks the deck against finding larger effects in households with a single eligible child. In Honduras and other countries, CCTs have larger effects on relatively poorer households (Fiszbein and Schady 2009; Galiani and McEwan 2013). Indeed, restricting the sample to poor households (while still making the aforementioned controls) reinforces the pattern of heterogeneity. The effects on enrollment and labor participation are 11.5 and −11.7 percentage points, respectively, when there is one eligible child in the household who was enrolled at baseline. We conclude that imposed conditions played an important role in increasing the magnitude of enrollment and labor participation effects.

In households with young children and pregnant or nursing mothers, the health condition and label were applied if no child was eligible for the education transfer. This, of course, is perfectly collinear with a $250 transfer instead of $500. Despite this, the only statistically significant effects on health service use occur in households subject to the condition. In poor households with zero children eligible for the education transfer, the treatment increased the probability (by 11.4 percentage points) that a young child's last health center visit was a checkup. The point estimate was smaller and statistically insignificant in households with one eligible child (as before, we control for household size, child age, birth order, and interactions with treatment indicators). We found a similar pattern of results for two indicators of maternal health service use: tetanus immunization before or during a pregnancy and the likelihood of receiving a postnatal checkup. We attribute this pattern of results to the enforcement of a requirement to register in health centers (and perhaps a labeling effect, although we cannot disentangle the two).

The article makes two main contributions to the literature on the role of conditions. First, it provides credible evidence that conditions matter in a Progresa-style, Latin American CCT for education, child labor, and health outcomes. Prior evidence from Mexican and Ecuadorean experiments relies on unintended variation (within experimental samples) in the understanding of conditions, but it focuses on education outcomes (Schady and Araujo 2008; de Brauw and Hoddinott 2011). The Ecuador experiment did not show any effects of households' beliefs about conditions on child labor (Edmonds and Schady 2012). The only evidence on health conditions in Latin America relies on a quirk in eligibility of some young children for conditions but must leverage quasi-experimental variation in CCT distribution (Attanasio et al. 2015).

Worldwide, the most credible evidence on the role of conditions comes from African experiments with unconditional and conditional treatment arms (Baird et al. 2011; Akresh et al. 2013, 2015; Robertson et al. 2013; Benhassine et al. 2015). The meta-analysis of Baird, Ferreira, et al. (2014) shows that the great majority of variance in the effects of cash transfers cannot be explained by observed design elements—such as conditions, transfer size, and baseline enrollment—suggesting a large but imperfectly understood role for variables such as the regional and country context. In lieu of a better understanding of the external validity of the African evidence, it is prudent to leverage the many Latin American experiments.

Second, and also related to external validity, this article provides a rare example in which a pioneering social experiment in a developing country is repeated. In both PRAF-II and Bono 10,000, a cluster-randomized experiment was embedded within the large-scale distribution of cash transfers (by the same government agency) to high-poverty communities. However, the size and structure of the cash transfers increased considerably, posing an interesting question as to why the full-sample effects of Bono 10,000 were relatively smaller. This article's estimates suggest a plausible explanation: that enforcement of conditions attached to cash transfers is relevant in the Honduran context.

II. The Bono 10,000 Experiment

A. Background

The Honduran Programa de Asignación Familiar (PRAF), or family allowance program, has distributed cash transfers to poor households since the early 1990s (Moore 2008). The initial phase (PRAF-I) distributed cash to families with young children and pregnant or nursing mothers, conditioning its receipt on school enrollment and health center visits. However, the conditions were not enforced, poverty targeting was weak, and the program was not rigorously evaluated (Moore 2008).

A successor program, PRAF-II, is more familiar to researchers. It identified the 70 poorest municipalities in Honduras (out of 298) and distributed cash transfers to households in a random subset of 40, from 2000 to 2002 (Glewwe and Olinto 2004; Morris et al. 2004; Galiani and McEwan 2013). Children age 6–12 were eligible for education transfers of about $50 per year per child if they (1) had yet to complete the fourth grade and (2) enrolled in and regularly attended primary school. Children under 3 and pregnant and nursing mothers were eligible for health transfers of $40 per year per person if they regularly visited health centers.

The PRAF-II transfers were modest: about 7% of pretransfer consumption versus 27% in Mexico's well-known Progresa experiment (Fiszbein and Schady 2009). Moreover, PRAF weakly enforced the conditionalities. Enrollment but not attendance was enforced in the education transfer (Glewwe and Olinto 2004), and health center attendance was not actively enforced, beyond the implied threat of the conditionality (Morris et al. 2004). Despite this, eligibility for education transfers increased enrollment by 8 percentage points (or 12%, against the control group enrollment rate) and reduced child labor by 3 percentage points, or 30% (Galiani and McEwan 2013). Health transfer eligibility increased the proportion of mothers with five or more prenatal appointments by 19 percentage points (a 38% gain over the control group), increased young children's health center attendance by 20 percentage points (46%), and also increased infant growth monitoring by at least 16 percentage points (more than 100%; Morris et al. 2004).

B. The Bono 10,000 Treatment

In 2010, the newly elected administration modified and renamed the CCT. Under Bono 10,000, PRAF offered payments of either L 5,000 or L 10,000 per household per year (about $250 or $500, respectively), payable in three installments. To qualify, households signed a letter of commitment in the presence of a PRAF representative.[6] The representative determined whether households were eligible for the larger transfer (at least one 6–18-year-old who had not completed ninth grade) or the smaller transfer (at least one younger child or a pregnant or nursing mother but no older children). After the determination of eligibility, households received a first unconditional transfer equal to 1/12 of either L 10,000 (labeled an education transfer) or L 5,000 (labeled a health transfer). Households received the second and third education transfers if at least one child—among all 6–18-year-olds who had not completed ninth grade—was actually enrolled in school. PRAF did not consistently enforce the attendance condition. Households received the second and third health transfers if the household was registered in a health center, although PRAF did not regularly enforce the requirement of regular attendance.

The treatment differed from PRAF-II in two ways. First, the size of per capita transfers was larger in Bono 10,000. In the baseline households of this experiment—all of which qualified as poor given a government-applied proxy means test—the median consumption is L 10,789 per capita per year (about $1.48 per day).[7] Given eligibility conditions, a typical household would have received L 1,946 per capita, or 18% of median consumption. This figure is closer to other Latin American CCT programs such as Progresa/Oportunidades (Fiszbein and Schady 2009).

Second, PRAF-II transfers were made on a per child basis, and conditions were homogeneously enforced among children. In contrast, Bono 10,000 introduced variation across eligible households in the chance that children were subject to education and health conditions. Households with any number of children eligible for the education transfer received transfers if they enrolled only one. This suggests that school-age children in larger families had a smaller chance of being subject to a binding enrollment condition. Relatedly, households with young children or pregnant or nursing mothers were subject to the health conditions in the absence of children eligible for the education transfer. In the presence of such children, the larger transfer was not labeled "health," nor was health center registration enforced.

[6] The one-page document stated that school-age children should be enrolled in school (with at least 80% attendance) and that very young children and pregnant or nursing mothers should attend health center appointments. Note that this "labeling" component of the treatment was provided to all treated households before they were assigned to receive a labeled education or health transfer. In the full-sample estimates, the possible effect of signing this letter cannot be empirically disentangled from other treatment components.

[7] The head-count ratio is 77%, implying that the proxy means test allowed substantial leakage of nonpoor households into the sample.

C. Village Sample and Randomization

The government began implementing Bono 10,000 during the experiment's design phase, in 2010 and the first half of 2011. It initially targeted the poorest of Honduras' 3,727 villages, or *aldeas* (nested within 298 municipalities and 18 departments). Given this constraint, the experimental sample was drawn from 816 slightly less poor villages not already treated.[8] On September 9, 2011, the treatment and control groups—each consisting of 150 villages—were randomly drawn from this group of 816 (see fig. 1).[9]

The shaded area in figure 2 denotes 70 municipalities included in the earlier PRAF-II experiment because they had the highest stunting rates, a poverty proxy (Galiani and McEwan 2013). They are dominated by historically poor and indigenous departments of western Honduras, especially Intibucá and Lempira. The 300 villages in the Bono 10,000 experiment are outlined. Forty-nine, or 16%, are located in PRAF-II municipalities.

We used the 2001 census—the only person-level data set with complete coverage of both sets of territories—to compare 0–18-year-old children residing in PRAF-II municipalities and Bono 10,000 villages (with the caveat that the Bono 10,000 experiment occurred a decade later). In PRAF-II municipalities, the average child's mother had 2.3 years of schooling, compared with 2.7 in the 816 villages initially eligible for the Bono 10,000 experiment (see table A1, available online only). The census does not measure household income or consumption, so we used a 1999 household survey to estimate a logit regression of an extreme poverty indicator—based on household income per capita—on the individual and household variables shared across the survey and census (for details, see Galiani and McEwan 2013). We averaged across the predicted probabilities of extreme poverty in the census sample (Tarozzi and Deaton 2009). This estimate of the extreme poverty rate was 89% in PRAF-II municipalities and 84% in Bono 10,000 villages. In short, the Bono 10,000 experiment included poor, rural villages distributed throughout the country but not some of the very poorest villages in western Honduras.

D. Household Sample and Data Collection

The household sample was obtained in three steps. First, the government of Honduras conducted a household census in the 300 villages, gathering

[8] The early treatment status of all villages cannot be fully reconstructed, but it appears to have relied heavily on village-level extreme poverty estimates based on a poverty-mapping exercise.

[9] The randomization occurred during a ceremony attended by participating organizations, in which 816 numbered balls were placed in a receptacle. Balls were drawn, and alternately assigned to treatment or control groups, until the desired sample size was reached. PRAF agreed to not treat any villages in the control group, and the treatment status of villages was not publicly announced.

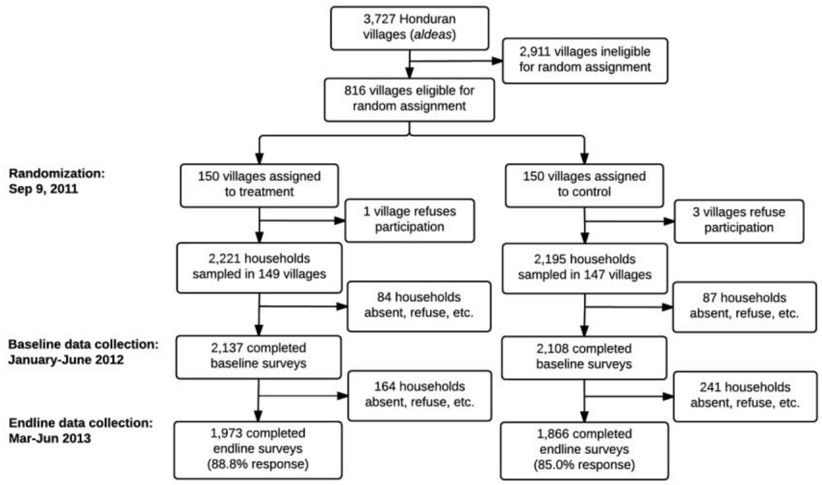

Figure 1. Profile of the Bono 10,000 experiment. Follow up response rates are calculated using the number of sampled households. If one further imputes 15 households for each nonresponding village, then response rates are 88.2% and 83.3% for treatment and control groups, respectively.

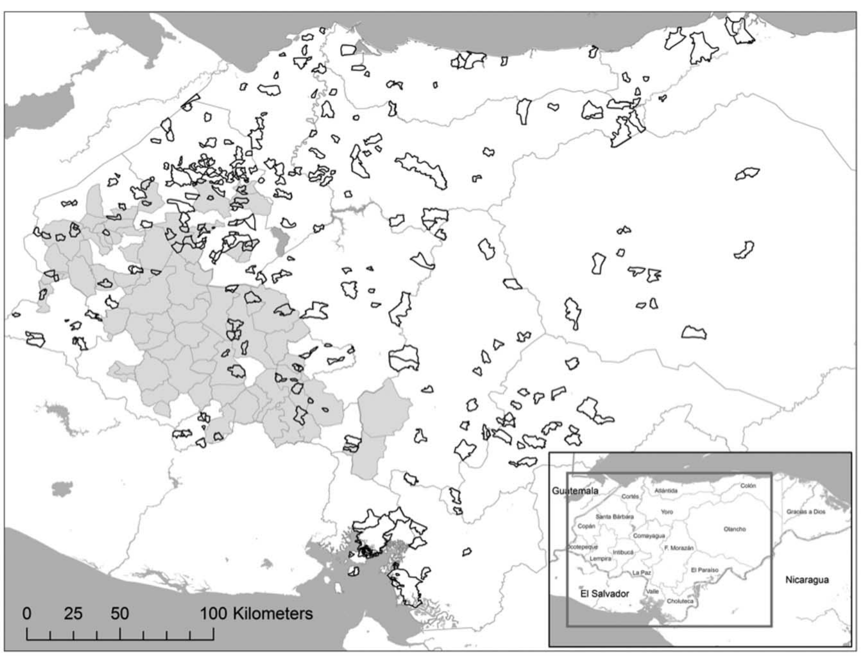

Figure 2. Sampled territories in the PRAF-II and Bono 10,000 experiments. Shaded areas indicate sampled municipalities in the PRAF-II experiment (Galiani and McEwan 2013). Outlined areas indicate sampled villages (*aldeas*) in the Bono 10,000 experiment. Seven villages in the eastern department of Gracias a Dios are not visible.

a modest number of variables related to household structure, dwelling quality, and asset ownership. Second, the government applied a proxy means test and constructed a list of nominally poor households.[10] Third, NORC at the University of Chicago randomly sampled poor households from each village's list.[11]

One village in the treatment group and three in the control group refused to participate in the survey. Thus, the final sample included 4,416 households in 296 villages. NORC enumerators applied baseline household surveys between January and June 2012. The lengthy baseline survey period partly overlapped with the beginning of the treatment, an issue that we discuss further below. The survey included sections on dwellings, the composition and characteristics of household members, education, infant and child health (including anthropometrics), maternal health, and income and expenditures. An endline survey, using the same questionnaire, was applied over a shorter period between March and June 2013.

E. Baseline Balance and Endline Attrition

Table 1 confirms that socioeconomic variables measured at baseline are balanced across treatment and control groups, in the sample of children under 18 years old.[12] The variables include measures of parental schooling and literacy, household structure, dwelling quality, and access to utilities. The mean baseline differences are small in magnitude and not statistically different from zero, using standard errors adjusted for clustering within villages.

The endline survey response rates were high, with 88.8% of the treatment group households and 85% of the control group responding (see fig. 1). These rates are slightly lower—88.2% and 83.3%, respectively—if one imputes 15 nonresponding households for each of the four nonresponding villages. To assess whether nonrandom attrition introduced observed differences across treatment and control groups, table 1 reports treatment-control differences in baseline variables for the restricted sample of households that responded to the endline survey. The differences are still small and not statistically significant. Despite this, differential attrition raises the possibility of selection on

[10] We do not have sufficient information to replicate the proxy means test and do not have access to the score generated or the cutoff point used to select households. The model used five variables, including availability of electricity and sewer services, and a household asset index.

[11] In villages with more than 15 households, 15 were randomly chosen. In four villages with fewer than 15, all were chosen.

[12] Note that these differences are based on a sample that already omits four nonresponding villages and a small amount of nonresponding households in the baseline survey of 296 villages (see fig. 1). Using the 300-village sample from the 2001 census (see table A1), the conclusions are similar. Two statistically significant differences in father's schooling are small in magnitude.

TABLE 1
MEAN INDIVIDUAL AND HOUSEHOLD VARIABLES AMONG CHILDREN AGE 0–17

	Children in Households Responding at Baseline Survey		Same Children in Households Responding at Follow Up Survey	
	Control Group Mean (SD)	Treatment Control Difference (SE)	Control Group Mean (SD)	Treatment Control Difference (SE)
Female (1/0)	.488	−.012	.487	−.011
	(.500)	(.010)	(.500)	(.011)
Age at baseline (years)	8.722	−.026	8.719	.007
	(5.095)	(.102)	(5.085)	(.108)
Mothers' schooling (years)	3.585	−.003	3.544	−.011
	(2.824)	(.125)	(2.770)	(.127)
Mother is literate (1/0)	.730	−.008	.728	−.010
	(.444)	(.020)	(.445)	(.021)
Fathers' schooling (years)	3.439	−.012	3.440	−.014
	(2.798)	(.128)	(2.781)	(.132)
Father is literate (1/0)	.729	−.005	.731	−.009
	(.444)	(.021)	(.444)	(.021)
Household size	6.459	−.110	6.465	−.074
	(2.525)	(.130)	(2.501)	(.131)
Number of children age 0–5	1.088	−.045	1.091	−.045
	(1.026)	(.046)	(1.031)	(.047)
Number of children age 6–18	2.738	−.053	2.733	−.023
	(1.622)	(.078)	(1.614)	(.076)
Adults in household who are Lenca (proportion)	.053	.000	.050	.001
	(.216)	(.019)	(.209)	(.018)
Number of rooms in dwelling	3.310	−.053	3.276	−.008
	(1.407)	(.068)	(1.395)	(.071)
Dwelling has bathroom or latrine (1/0)	.753	.015	.752	.020
	(.432)	(.027)	(.432)	(.027)
Dirt floor in dwelling (1/0)	.354	.009	.354	.010
	(.478)	(.028)	(.478)	(.029)
Piped water in dwelling (1/0)	.179	−.017	.181	−.017
	(.383)	(.019)	(.385)	(.019)
Electricity in dwelling (1/0)	.661	−.026	.672	−.035
	(.473)	(.039)	(.469)	(.040)
Landline or cell phone access (1/0)	.847	.018	.849	.016
	(.360)	(.020)	(.358)	(.020)
Dwelling only accessible by footpath (1/0)	.299	.023	.293	.023
	(.458)	(.031)	(.455)	(.030)

Note. Maximum number of child observations in the treatment (control) group is 5,764 (5,723) on the baseline survey and 5,379 (5,114) on the follow-up survey. Standard errors of mean differences are adjusted for clustering within villages.

unobservables. Thus, our estimates will report bounds based on a trimming procedure (Lee 2009).

III. Empirical Approach

A. Main Estimates

Given randomized assignment, we report full-sample estimates of the unadjusted mean difference between treatment and control households:

$$O_{ij} = \beta_0 + \beta_1 T_j + \varepsilon_{ij}, \tag{1}$$

where O_{ij} is the endline outcome of unit i—whether household, child, or mother—in village j, and T_j is a treatment dummy. Robust standard errors are adjusted for clustering within villages. Whatever the outcome variable, we limit the sample to endline respondents who were present in the household at baseline (thus excluding individuals who were subsequently born or moved into the household).

We present two specifications as robustness checks. The first specification controls for baseline socioeconomic variables from table 1, dummy variables indicating any missing values of these variables,[13] and dummy variables indicating Honduran departments and the week in which the endline survey was conducted. The second specification further controls for the dependent variable measured at baseline.

We later show that some members of the treatment group received the first payment before the baseline survey. This raises the concern that some outcome measures—self-reported at the baseline—might have been influenced by the payments. This is especially true for outcomes that reference the week or month before the survey.[14] Given this, our preferred estimates do not control for (potentially endogenous) baseline outcomes. The point is somewhat moot, however, because unconditional estimates are rarely sensitive to additional controls.

B. School Enrollment Conditions

Recall that households received L 10,000 per year if (1) they included any number of children age 6–18 who had not completed ninth grade (i.e., chil-

[13] Puma et al. (2009) report simulations suggesting that dummying-out adjustments for missing data performs well in settings in which independent variables with missing values are balanced across treatment and control groups, such as a randomized experiment.

[14] For example, this is more likely with labor force participation (reported in the week before the survey) or household expenditures (reported in the last month). It is less likely for enrollment, which is retrospectively reported as a child's enrollment at the end of the 2011 calendar and school year. The latter variables are still subject to self-reporting error, even in the absence of a causal effect.

dren eligible for the education transfer) and (2) at least one eligible child enrolled in school. It suggests that an eligible child's probability of being subject to conditions, and perhaps her outcome, depends on the number of such children in the household.[15] In the subsample of eligible children residing in households with one to four eligible children, we estimate

$$O_{ij} = \beta_0 + \sum_{k=1}^{4} \beta_k \times T_j \times 1\{C_{ij} = k\} + \sum_{k=2}^{4} \gamma_k \times 1\{C_{ij} = k\} + \varepsilon_{ij}, \qquad (2)$$

where C_{ij} is the number of eligible children in the household at baseline and $1\{\cdot\}$ is an indicator function.[16] The β_k represent treatment effects in households with one, two, three, or four eligible children.

Suppose that the $\hat{\beta}_k$—the effects on enrollment—monotonically decline as k increases. It is tempting to conclude that children with more siblings are less likely to enroll because of nonbinding conditions, but there are three alternate explanations. First, the per capita transfer declines as the number of children increases (given a fixed household transfer). Enrollment effects in Mexico were increasing in the size of the per-child transfer (de Janvry and Sadoulet 2006), although a Cambodian study found diminishing returns to transfers (Filmer and Schady 2011).

Second, the mean age of eligible children increases from 10.6 to 11.5 as the number of eligible children in the household increases from one to four (see table A2, available online only). The earlier PRAF-II experiment found that a per-child transfer had larger effects on younger children (Galiani and McEwan 2013), although the sample only included primary-age children.

Third, mean birth order increases from 1.2 to 2.7 as the number of eligible children increases from 1 to 4 (see table A2). Earlier-born children in Latin America (i.e., a lower order of birth) typically lag behind the human capital development of later-born siblings from infancy to adolescence (see De Haan, Plug, and Rosero [2014] and the citations therein).[17] Suppose these devel-

[15] This raises the question exactly how households choose which child (among at least two eligible ones) to enroll in school. It is plausible that parents favor the child with the highest perceived return to the schooling investment, which is likely correlated with child ability. We cannot test this directly, given the lack of such measures in the baseline survey. In this spirit, an experiment in Burkina Faso (Akresh et al. 2013) found that conditional transfers (vs. unconditional ones) were similarly likely to increase enroll of "favored" children (including high-ability ones). However, conditional transfers were relatively more effective in increasing the enrollment of less favored children, including lower-ability ones.

[16] An earlier version of this article (Benedetti, Ibarrarán, and McEwan 2015) defined C_{ij} as the number of children between 6 and 18 in the household. It also did not include the birth order controls discussed below. This does substantively affect conclusions.

[17] We corroborated the general pattern of these findings in our data. In table A3, available online only, we regress child outcomes on dummy variables indicating child age, gender, and birth order, as

opmental lags are associated, all else equal, with lower baseline enrollments. The accumulated literature suggests that CCTs should have a larger effect on this group (Fiszbein and Schady 2009).

In each case, it is plausible that relatively larger treatment effects in one-eligible-child households are explained by variables other than conditions. To address the first concern, we leverage the fact that per capita transfers remain constant—even as the number of eligible children increases—as long as the total household size is constant. We extend equation (2) by allowing each of the four treatment effects to interact with household size:

$$O_{ij} = \beta_0 + \sum_{k=1}^{4}\beta_k \times T_j \times 1\{C_{ij} = k\} + \sum_{k=2}^{4}\gamma_k \times 1\{C_{ij} = k\} + \sum_{k=1}^{4}\theta_k \times T_j \times 1\{C_{ij} = k\} \times (H_{ij} - \overline{H}) + \sum_{n=2}^{\max(H_{ij})}\delta_n \times 1\{H_{ij} = n\} + \varepsilon_{ij}, \quad (3)$$

where H_{ij} is the total household size for an eligible child, $\overline{H}$ is the estimation sample mean, and the δ_n are separate intercepts for each household size. Each θ_k represents the amount by which the corresponding β_k changes as household size increases by one. We do not interpret these estimates in the text. Instead, H_{ij} is centered at the sample mean, such that the $\hat{\beta}_k$ can be interpreted as the heterogeneous effects (by number of children) for children in households of average size. We adopt the same approach for other variables, including dummy variables for discrete categories of age and birth order, as well as interactions between continuous age and birth order and the four treatment dummies.[18]

Finally, we note that the number of eligible children is associated with increasing poverty (from 71% to 90%), a pattern that is also observed for common poverty correlates such as dwelling quality and access to utilities (see table A2). The accumulated evidence on CCTs, particularly in Honduras, suggests that larger enrollment effects will be observed in poorer households (Fiszbein and Schady 2009; Galiani and McEwan 2013). This would tend to stack the deck against finding larger enrollment effects in one-eligible-child households. Thus, we also report estimates in subsamples of poor and nonpoor

well as household fixed effects (following De Haan et al. 2014). The outcomes include school enrollment, child labor participation, anthropometric outcomes, and child anemia. Given the clustered sample design, most estimates are quite imprecise. However, fourth-born children are more likely to be enrolled in school.

[18] That is, we also control for $\sum_{k=1}^{4}\lambda_k \times T_j \times 1\{C_{ij} = k\} \times (A_{ij} - \overline{A}) + \sum_{g=2}^{\max(A_{ij})}\pi_g \times 1\{A_{ij} = g\}$, where A_{ij} is the child's age and $\overline{A}$ is the estimation sample mean. Similar controls are included for birth order.

households, using the consumption-based poverty estimate from the baseline survey.

C. Health Conditions

A household received a labeled health transfer of L 5,000 and was subject to the health center registration condition if (1) it included at least one child under age 6 or a pregnant or nursing mother and (2) it did not include a child eligible for the education transfer. Therefore, we estimate (in the subsample of eligible young children or mothers who reside in homes with no more than one child eligible for the education transfer)

$$O_{ij} = \beta_0 + \beta_1 \times T_j \times 1\{C_{ij} = 0\} + \beta_2 \times T_j \times 1\{C_{ij} = 1\} + \gamma \times 1\{C_{ij} = 1\} + \varepsilon_{ij}, \tag{4}$$

where O_{ij} is a measure of health service use and C_{ij} is the number of children in the household eligible for the education transfer. If the label or health condition matter, we anticipate that $\hat{\beta}_1$ will be larger than $\hat{\beta}_2$. The same caveats still apply, and so we include controls, as previously described, for household size, age, and birth order (excluding birth order controls in the sample of mothers). We also report estimates in subsamples of poor children and mothers. However, note that the household transfer decreases by 50% when the health label and conditions are applied. This stacks the deck against finding that health conditions matter (presuming that demand for health services increases with income).

IV. Results

A. Transfers to the Treatment and Control Groups

To assess implementation, we linked the household sample to PRAF's administrative payments database. In figure 3, the *y*-axis measures the cumulative payment per household, and the *x*-axis measures days. The control group—corresponding to the solid line—was effectively excluded from the transfers. The dotted line suggests that the treatment group received the largest payments in the days just after the baseline survey and just before the endline surveys. By the endline, treated households had received L 6,834 more than control households, on average, or about $342 (see table 2).

Table 2 provides further nuance. In fact, 58% of treated households (vs. 6% of control households) received any payment before their baseline survey was applied. Our conversations with PRAF personnel—confirmed by the data—indicated that these were the unconditional payments (received when the letter of commitment was signed). The payments were either L 417 or L 833,

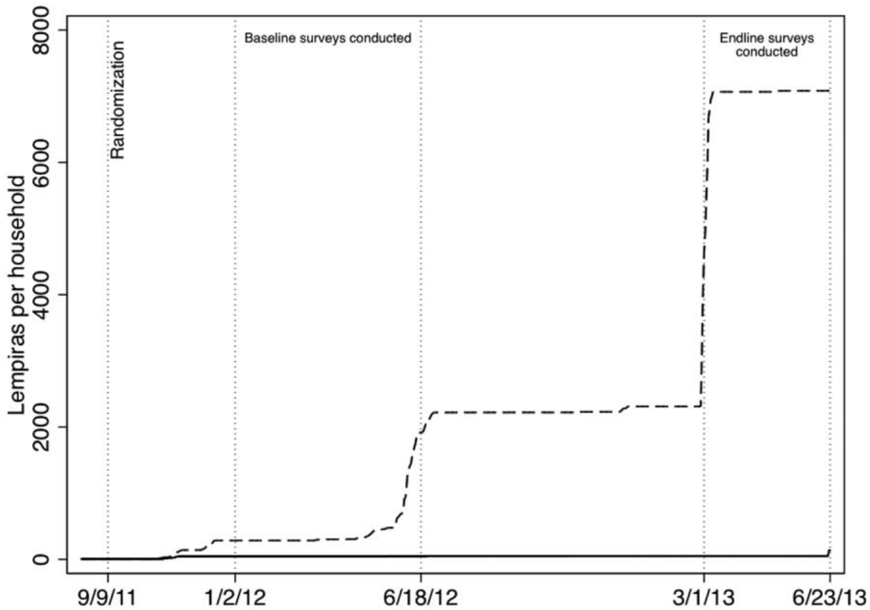

Figure 3. Cumulative payments to treatment and control group households. Dashed (solid) lines indicate payment received by households in the treatment (control) group of the survey.

representing 1/12 of the annual transfers. Given the chance that baseline survey response was influenced by early payments, our preferred specification includes no baseline control variables.

B. Full-Sample Results

1. Consumption, Poverty, Income, and Adult Labor Supply

Table 3 reports effects on measures of consumption and income. The estimates in model 1 are the unadjusted mean difference between households residing in treated and untreated villages, while models 2 and 3 add previously described controls. The table also reports the baseline and endline means of the outcome variable in the control group and the lower and upper bounds of a trimming exercise (Lee 2009).[19] The bounds do not overturn any of the main conclusions, and we do not discuss them further.

The per capita consumption of households in the treatment group was approximately 9% higher than the control group (the variables includes expenditures, as well as the estimated value of in-kind goods and services). The

[19] The attrition rate was always higher in the control group. To obtain the lower (upper) bound, we "attrited" cases from the top (bottom) of the outcome variable's distribution in the treatment group—until rates of attrition were same across groups—and reestimated the mean difference.

TABLE 2
MEAN PAYMENTS TO TREATMENT AND CONTROL GROUP HOUSEHOLDS

	Treatment Mean (SD)	Control Mean (SD)	Treatment Control Difference (SE)
Before household's baseline interview:			
Any payment (1/0)	.58	.06	.52
	(.49)	(.23)	(.04)
Number of payments	.58	.06	.52
	(.50)	(.23)	(.04)
Amount (100s of lempiras)	4.42	.46	3.97
	(3.96)	(1.87)	(.30)
Before household's endline interview:			
Any payment (1/0)	.91	.07	.83
	(.29)	(.26)	(.02)
Number of payments	1.86	.07	1.79
	(.91)	(.26)	(.05)
Amount (100s of lempiras)	69.25	.91	68.34
	(42.15)	(4.83)	(1.67)

Note. Payment data are from an administrative database. Number of households in the treatment (control) sample is 2,087 (2,049). Standard errors are adjusted for clustering within villages.

increase was similar for both food and nonfood consumption, while the effect on the share of consumption devoted to food was not statistically different from zero. Theory (and the Engel curve) predict declining food shares as incomes rise. The contrary result, frequently observed in evaluations, could be interpreted as evidence that transfers provided to female heads of household are allocated differently than regular income (Attanasio and Lechene 2010). We also find that the treatment increased the probability that households made any educational expenditure (by 1.7 percentage points) but had no statistically significant effects on the probability or amount of expenditures on alcohol or tobacco.

Treatment-group poverty was 3 percentage points lower than the control (while noting that control-group poverty actually rose from 77% to 81% between the baseline and endline).[20] The treatment also reduced the depth of poverty, as gauged by Foster-Greer-Thorbecke measures.[21] The poverty gap and its square were 0.036 and 0.033 lower than the control group, respectively, representing 9% and 14% decreases over the control-group means at endline. The results are similar for extreme poverty, although the dummy variable indicating extreme poverty is imprecisely estimated.

[20] We use the Instituto Nacional de Estadística's rural poverty lines for 2012 and 2013 (L 1,465 and L 1,529 per person per month, respectively). The extreme poverty lines are 1,098 and 1,146, respectively.

[21] The poverty gap for each observation is $[(z - c_i)/z]^a \times 1\{c_i < z\}$, where z is the poverty line, c_i is per capita consumption for each household i, and $a = 1$ (or $a = 2$ in the case of the squared poverty gap).

TABLE 3

EFFECTS ON HOUSEHOLD CONSUMPTION, POVERTY, AND INCOME

	Model				Control Group Mean		
	1	2	3	*N*	Baseline	Endline	Lee Bounds
Household consumption:							
ln(consumption per capita)	.089***	.088***	.093***	3,839	6.83	6.79	.053, .118
	(.033)	(.033)	(.027)				
ln(food consumption per capita)	.090***	.089***	.098***	3,835	6.45	6.40	.050, .122
	(.031)	(.031)	(.028)				
ln(nonfood consumption per capita)	.105*	.106*	.061	3,838	5.38	5.39	.064, .156
	(.054)	(.054)	(.039)				
Food share (proportion)	.002	.002	.008	3,839	.71	.70	−.002, .011
	(.007)	(.007)	(.007)				
Any school expenditure (1/0)	.017***	.017***	.014**	3,839	.96	.97	.017, .034
	(.006)	(.006)	(.007)				
ln(school expenditures)	.184***	.183***	.193***	3,723	3.66	3.81	.125, .278
	(.050)	(.049)	(.041)				
Any alcohol/tobacco expenditure (1/0)	−.013	−.014	−.013	3,839	.08	.07	−.029, −.012
	(.009)	(.009)	(.010)				
ln(alcohol/tobacco expenditures)	−.170	−.174	−.008	270	3.52	3.52	−.514, .137
	(.167)	(.164)	(.170)				
Consumption based poverty:							
Poor (1/0)	−.030**	−.031**	−.031**	3,922	.77	.81	−.031, −.027
	(.015)	(.015)	(.014)				
Poverty gap	−.036***	−.036***	−.031***	3,922	.36	.40	−.038, −.035
	(.014)	(.014)	(.010)				
Poverty gap^2	−.033***	−.033***	−.029***	3,922	.21	.23	−.035, −.032
	(.011)	(.011)	(.009)				
Extremely poor (1/0)	−.027	−.028	−.018	3,922	.62	.66	−.029, −.025
	(.020)	(.020)	(.016)				
Extreme poverty gap	−.037***	−.037***	−.032***	3,922	.25	.28	−.040, −.036
	(.013)	(.013)	(.010)				
Extreme poverty gap^2	−.030***	−.030***	−.027***	3,922	.13	.15	−.033, −.030
	(.009)	(.009)	(.008)				
Household income:							
ln(household income per capita)	.124**	.124**	.182***	3,737	6.41	6.64	.079, .171
	(.055)	(.054)	(.043)				
ln(household labor income per capita)	−.087	−.087	−.033	3,720	6.31	6.39	−.128, −.033
	(.063)	(.063)	(.053)				
Any nonlabor income (1/0) (excluding CCT)	.002	.001	.016	3,839	.47	.43	−.007, .009
	(.025)	(.025)	(.022)				
ln(household nonlabor income per capita) (excluding CCT)	−.003	−.002	−.020	1,644	3.85	4.18	−.084, .086
	(.109)	(.101)	(.093)				

TABLE 3 (*Continued*)

	Model				Control Group Mean		
	1	2	3	*N*	Baseline	Endline	Lee Bounds
Controls	No	Yes	Yes				
Dependent variable at baseline	No	No	Yes				

Note. Standard errors in parentheses are adjusted for clustering within villages. All specifications include a constant. Model 2 includes the variables in table 1 (using the household mean of female and age), dummy variables indicating missing values of independent variables, and dummy variables for departments and the week in which the follow-up survey was applied. Model 3 further includes the baseline value of the dependent variable and a dummy variable indicating missing values.
* Statistically significant at 10%.
** Statistically significant at 5%.
*** Statistically significant at 1%.

Household income per capita, from all sources, was approximately 12.4% higher in the treatment group.[22] There is no evidence that the treatment tended to lower labor income (via effects on labor supply) or nonlabor income (via crowding out of remittances or other donations). The coefficients on the former are negative but imprecisely estimated. Table 4 further reports effects on labor supply in separate samples of adult males and females. There are no effects on labor participation in the week before the survey or on adult labor income (conditional on any hours worked).[23] The labor supply results are broadly consistent with the literature on PRAF-II and other Latin American CCTs (Alzúa, Cruces, and Ripani 2013).

2. School Enrollment and Child Labor Supply

In the sample of children who were eligible for the education transfer, the treatment group was 3.8 percentage points more likely to be enrolled in school at the time of the endline survey (see table 5). Note that the net enrollment rate at baseline (based on retrospective responses for the 2011 school year) was 76% versus 69% at the endline. Thus, the treatment might have forestalled dropout among some students, in addition to encouraging enrollments among never-attenders or dropouts.

The former explanation is better supported by the data. Table 5 interacts the treatment dummy with four dummy variables indicating children who were not enrolled in 2011, enrolled in lower primary (grades 1–3), upper primary (grades 4–6), or lower secondary (grades 7 and 8). The treatment did

[22] Labor and nonlabor income during the past month, excluding cash transfers, was calculated from the household survey. We estimated monthly transfer income with administrative data by calculating the sum of all transfers in the year before the endline survey date and dividing by 12.
[23] The survey did not measure hours of work.

TABLE 4
EFFECTS ON ADULT LABOR SUPPLY AND LABOR INCOME

	Model				Control Group Mean		
	1	2	3	*N*	Baseline	Endline	Lee Bounds
Males age 21–65 at baseline:							
Worked ≥1 hour last week	.003	−.001	−.006	3,679	.938	.931	.002, .017
	(.010)	(.012)	(.011)				
ln(labor income)	−.041	.013	.034	3,311	7.514	7.485	−.099, .044
	(.068)	(.065)	(.057)				
Females age 21–65 at baseline:							
Worked ≥1 hour last week	−.028	−.002	−.001	4,128	.375	.357	−.041, −.021
	(.020)	(.020)	(.018)				
ln(labor income)	−.002	.016	−.021	1,907	6.528	6.379	−.087, .055
	(.095)	(.101)	(.090)				
Controls	No	Yes	Yes				
Dependent variable at baseline	No	No	Yes				

Note. Standard errors in parentheses are adjusted for clustering within villages. All specifications include a constant. Model 2 includes the variables in table 1, dummy variables indicating missing values of independent variables, and dummy variables for departments and the week in which the follow-up survey was applied. Model 3 further includes the baseline value of the dependent variable and a dummy variable indicating missing values.

not increase enrollments among unenrolled children, with coefficients close to zero. However, the probability of enrollment increased by 6.2 percentage points among students enrolled in grades 4–6 in 2011 (with less positive and precise coefficients for other grade levels). The transition from upper primary to lower secondary is when most rural students leave school (McEwan et al. 2015). The data in table 5 bear this out: 1/3 of upper-primary students in 2011 are not enrolled in the first half of the 2013 school year. While the point estimates are suggestive, *p*-values in table 5 suggest we cannot reject the null hypothesis that effects are jointly equal, even when comparing effects across unenrolled and the combined group of enrolled children.

Finally, table 5 examines whether enrollment effects differ by the baseline poverty of children. In model 1, the effect is only statistically significant for poor children, although the magnitude of the effect is similar (and less precisely estimated) for nonpoor children. In models 2 and 3, the addition of controls widens the gap between the point estimates and, in model 3, allows us to reject the null hypothesis that effects are equal.

Table 6 repeats the previous analyses using a dummy variable indicating whether children had any paid or unpaid work in the week before the endline survey. There are no full-sample effects. However, this appears to mask increased labor supply among children not enrolled at baseline (by 5.2 percentage points, or 10% of the control-group mean) and decreased labor supply among enrolled children (by 2.8 percentage points, or 19%), especially among those

TABLE 5
EFFECTS ON SCHOOL ENROLLMENT AT FOLLOW-UP

	Model				Control Group Mean	
	1	2	3	*N*	Baseline	Endline
Full sample:						
Treated	.038**	.044***	.044***	6,573	.76	.69
	(.016)	(.013)	(.012)			
Heterogeneity by enrollment in 2011:						
Treated × not enrolled	.009	.027	.027	6,012	0	.22
	(.026)	(.021)	(.021)			
Treated × enrolled grades 1–3	.020*	.040***	.040***		1	.95
	(.012)	(.013)	(.013)			
Treated × enrolled grades 4–6	.062**	.066***	.066***		1	.65
	(.028)	(.024)	(.024)			
Treated × enrolled grades 7 and 8	.044	.056	.056		1	.77
	(.040)	(.039)	(.039)			
p-value of *F*-test of joint equality	.45	.58	.58			
Treated × not enrolled	.009	.026	.026	6,566	0	.22
	(.026)	(.021)	(.021)			
Treated × enrolled	.042***	.049***	.049***		1	.82
	(.013)	(.013)	(.013)			
p-value of *F*-test of joint equality	.21	.28	.28			
Heterogeneity by baseline poverty:						
Treated × not poor	.049	.021	.011	6,566	.79	.74
	(.031)	(.024)	(.022)			
Treated × poor	.037**	.049***	.051***		.75	.67
	(.017)	(.014)	(.013)			
p-value of *F*-test of joint equality	.70	.28	.09			
Controls	No	Yes	Yes			
Dependent variable at baseline	No	No	Yes			

Note. Sample: age 6–17 at baseline, completed no more than eighth grade. Standard errors in parentheses are adjusted for clustering within villages. All specifications include a constant. Model 2 includes the variables in table 1, dummy variables indicating missing values of independent variables, and dummy variables for departments and the week in which the follow-up survey was applied. Model 3 further includes the baseline value of the dependent variable and a dummy variable indicating missing values. Models with treatment interactions always control for dummies indicating 2011 enrollment or poverty.
* Statistically significant at 10%.
** Statistically significant at 5%.
*** Statistically significant at 1%.

enrolled in grades 4–6. In all specifications, the *p*-values allow us to reject equality of effects across subgroups. Finally, there is no evidence of heterogeneity by baseline poverty status.

3. Child and Maternal Health

We next examine whether the use of health services increased in samples of children and mothers (see table 7).[24] Among infants, the percentage who had

[24] The age range of samples is imposed by age eligibility for questions on the baseline and endline surveys.

TABLE 6
EFFECTS ON PAID OR UNPAID WORK IN WEEK BEFORE FOLLOW-UP SURVEY

	Model				Control Group Mean	
	1	2	3	N	Baseline	Endline
Full sample:						
Treated	−.012	−.009	.000	6,598	.24	.23
	(.014)	(.012)	(.012)			
Heterogeneity by baseline enrollment:						
Treated × not enrolled	.052*	.046*	.048**	6,038	.47	.49
	(.028)	(.025)	(.024)			
Treated × enrolled grades 1–3	−.017	−.022	−.013		.11	.08
	(.014)	(.015)	(.014)			
Treated × enrolled grades 4–6	−.041*	−.035*	−.022		.25	.23
	(.023)	(.021)	(.020)			
Treated × enrolled grades 7 and 8	−.039	−.044	−.022		.27	.28
	(.044)	(.041)	(.040)			
p-value of *F*-test of joint equality	.05	.03	.06			
Treated × not enrolled	.052*	.046*	.048**	6,591	.47	.49
	(.028)	(.025)	(.023)			
Treated × enrolled	−.028**	−.026**	−.015		.17	.15
	(.013)	(.013)	(.012)			
p-value of *F*-test of joint equality	.01	.00	.01			
Heterogeneity by baseline poverty:						
Treated × not poor	−.031	−.008	.003	6,591	.22	.21
	(.028)	(.025)	(.023)			
Treated × poor	−.009	−.010	−.001		.25	.23
	(.014)	(.013)	(.012)			
p-value of *F*-test of joint equality	.44	.93	.88			
Controls	No	Yes	Yes			
Dependent variable at baseline	No	No	Yes			

Note. Sample: age 6–17 at baseline, completed no more than eighth grade. Standard errors in parentheses are adjusted for clustering within villages. All specifications include a constant. Model 2 includes the variables in table 1, dummy variables indicating missing values of independent variables, and dummy variables for departments and the week in which the follow-up survey was applied. Model 3 further includes the baseline value of the dependent variable and a dummy variable indicating missing values. Models with treatment interactions always control for dummies indicating 2011 enrollment or poverty.
* Statistically significant at 10%.
** Statistically significant at 5%.

been recently weighed increased by 16.4 percentage points (or 30% of the control-group mean), with somewhat larger point estimates in specifications with controls. We also find that parents of young children (0–3 years old at baseline) were 4 percentage points (44%) more likely to state that the reason for the child's last visit was to get a regular checkup. However, there was no effect on the percentage of young children with a complete set of immunizations (three doses of the pentavalent and oral polio vaccines and a single dose of the bacillus Calmette-Guérin and measles, mumps, and rubella vaccines).[25]

[25] The results are not sensitive to use of individual vaccine measures or narrower age groups (e.g., 12–23 months old at endline).

TABLE 7

EFFECTS ON THE USE OF HEALTH SERVICES BY CHILDREN AND MOTHERS

	Model				Control Group Mean		Lee Bounds
	1	2	3	*N*	Baseline	Endline	
Children age 0 at baseline:							
Weighed in 30 days before follow up	.164***	.228***	.228***	461	.566	.542	.142, .200
	(.050)	(.051)	(.049)				
Children age 0–3 at baseline:							
Reason for last visit to health center was a checkup (1/0)	.041**	.030*	.030*	1,999	.079	.094	.010, .045
	(.018)	(.017)	(.017)				
Complete immunizations (1/0)	.027	−.005	.008	2,189	.535	.771	.020, .053
	(.023)	(.023)	(.018)				
Women 12–49 (pre- and postnatal):							
Number of prenatal checkups during last or current pregnancy	.326	.240	.219	729	4.228	4.845	.129, .508
	(.202)	(.237)	(.232)				
Woman received tetanus shot before or during last/current pregnancy (1/0)	.052*	.056	.051	696	.716	.812	.032, .057
	(.031)	(.039)	(.038)				
Woman received advice about birth plan (1/0)	.030	.027	.024	692	.521	.548	.010, .048
	(.042)	(.052)	(.051)				
Woman received postnatal checkup in 10 days after birth (1/0)	.061	.031	.031	563	.539	.560	.037, .081
	(.049)	(.057)	(.057)				
Controls	No	Yes	Yes				
Dependent variable at baseline	No	No	Yes				

Note. Standard errors in parentheses are adjusted for clustering within villages. All specifications include a constant. Model 2 includes the variables in table 1, dummy variables indicating missing values of independent variables, and dummy variables for departments and the week in which the follow-up survey was applied. Model 3 further includes the baseline value of the dependent variable and a dummy variable indicating missing values. Prenatal sample includes women who were pregnant at baseline or follow-up and women who gave birth between January 2012 and July 2013. Postnatal sample includes women pregnant at baseline and women who gave birth between January 2012 and July 2013.

* Statistically significant at 10%.

** Statistically significant at 5%.

*** Statistically significant at 1%.

The substantial growth in control-group vaccination, from 54% to 77%, is consistent with Honduras' good coverage even in the absence of Bono 10,000.

The results are weaker for several measures of prenatal and postnatal care of mothers, including the number of prenatal checkups, tetanus immunization, receipt of advice about birth plans, and a postnatal checkup. In the relatively small samples, the signs of coefficients are uniformly positive but not statistically different from zero at 5%.

Finally, table 8 reports results for a small number of child health and nutritional outcomes, including parent-reported incidence of diarrhea and respiratory illness; *z*-score-based measures of child stunting, wasting, and underweight status; and anemia. The control-group means indicate high rates of child illness (51% reported respiratory problems in a 2-week recall period), stunting (30%), and anemia (45%). However, none of these measures were affected over the treatment period.[26]

C. Education Conditions

Table 9 assesses whether the number of children eligible for the education transfer mediates the size of effects on enrollment (panel A) and child labor (panel B). The regression in column 1, based on equation (2), shows an enrollment effect of 7.9 percentage points in one-eligible-child households, 4.4 percentage points with two, and smaller and statistically insignificant effects in households with three or four eligible children. The accompanying *p*-values allow us to reject the null hypothesis that effects are jointly equal to zero but not the null that they are jointly equal. Although suggestive, can the heterogeneity be explained by variables correlated with household structure?

Table 9 column 2 adds controls for household size, child age, birth order, and interactions with the treatment indicators (see above). The point estimate for children in one-eligible-child households is the same (and significant at 5%), although the coefficient on two-child households is no longer significant at 10%. Recalling earlier caveats, we split the sample into children residing in nonpoor and poor households. In the nonpoor sample, there is no evidence of any enrollment effects. In contrast, the significant coefficient for poor, one-child households is 14.1 percentage points. We can reject the null hypotheses that effects are jointly equal (at 6%), or jointly equal to zero (at 1%).

As a final exercise, we split the sample of poor households between children who were enrolled or not enrolled at baseline. Consistent with the full-sample estimates, there are no statistically significant coefficients for children who were not enrolled (although the magnitude of the point estimate in one-eligible-child households is not small, at 6.4 percentage points). The effects among poor children enrolled at baseline monotonically decline with the number of children, from 11.5 to 5.4 to 4.9 to 2.7 percentage points. One can reject the null hypothesis that the coefficients are jointly equal to zero. However, one cannot reject the null that they are jointly equal ($p = .40$) or that effects in one- and two-child households are equal ($p = .12$).

[26] All *z*-score measures exclude outliers on the basis of World Health Organization guidelines. The results are similar if one uses continuous measures of *z*-scores or child hemoglobin.

TABLE 8
EFFECTS ON CHILD HEALTH AND NUTRITION: AGE 0–3 AT BASELINE

	Model				Control Group Mean		
	1	2	3	*N*	Baseline	Endline	Lee Bounds
Child had diarrhea in 2 weeks before survey (1/0)	−.019	.004	.006	2,000	.225	.150	−.051, −.013
	(.019)	(.022)	(.021)				
Child had respiratory problem in 2 weeks before survey (1/0)	−.019	.002	.001	2,004	.508	.527	−.036, −.001
	(.027)	(.029)	(.029)				
Height-for-age *z*-score ≤2 (1/0)	−.008	−.013	−.023	1,830	.300	.273	−.041, .004
	(.028)	(.028)	(.023)				
Weight-for-height *z*-score ≤2 (1/0)	.007	.014**	.013**	1,824	.053	.016	−.012, .008
	(.006)	(.006)	(.006)				
Weight-for-age *z*-score ≤2 (1/0)	.007	.030*	.024	1,880	.116	.079	−.033, .010
	(.015)	(.018)	(.015)				
Anemic: hemoglobin <11 (1/0)	.010	.029	.028	1,791	.449	.413	−.018, .029
	(.029)	(.028)	(.028)				
Controls	No	Yes	Yes				
Dependent variable at baseline	No	No	Yes				

Note. Standard errors in parentheses are adjusted for clustering within villages. All specifications include a constant. Model 2 includes the variables in table 1, dummy variables indicating missing values of independent variables, and dummy variables for departments and the week in which the follow-up survey was applied. Model 3 further includes the baseline value of the dependent variable and a dummy variable indicating missing values.
* Statistically significant at 10%.
** Statistically significant at 5%.

One may yet be concerned that the patterns of the coefficients have alternate explanations. In table A2, there is an inverse relationship between the number of children and poverty and also between correlates of poverty such as mother's schooling. The correlation with mother's schooling is attenuated, but not eliminated, in the sample of poor households. However, the usual patterns of heterogeneity in CCT effects (Fiszbein and Schady 2009) suggest that further controls for poverty proxies and treatment interactions would only reinforce the pattern in the final column of table 9. But, there are evident precision trade-offs of making even further controls or using ever-shrinking samples.

Table 9 panel B reports estimates using a dummy variable indicating that children worked at least 1 hour in the week before the survey. Recall that table 6 reported a modest negative effect on the probability of working (2.8 percentage points), but only among children enrolled at baseline. It also reported increased labor participation among unenrolled children. The results in table 9 show that the largest estimated decline in child labor—11.7 percentage points—is for poor children, enrolled at baseline, in households with only one eligible child (while the coefficients are closer to zero in multichild

TABLE 9

HETEROGENEOUS EFFECTS BY FAMILY STRUCTURE ON CHILD ENROLLMENT AND LABOR

	Full Sample		Not Poor at Baseline	Poor at Baseline		
				Full Sample	Not Enrolled 2011	Enrolled 2011
	(1)	(2)	(3)	(4)	(5)	(6)
	A. Enrolled at follow up					
Treatment × one eligible child 6–18	.079***	.079**	−.048	.141***	.064	.115***
	(.027)	(.033)	(.050)	(.040)	(.081)	(.038)
Treatment × two eligible children 6–18	.044*	.030	.023	.031	−.030	.054**
	(.024)	(.022)	(.042)	(.023)	(.043)	(.024)
Treatment × three eligible children 6–18	.017	.021	−.042	.031	−.020	.049*
	(.028)	(.024)	(.072)	(.026)	(.037)	(.026)
Treatment × four eligible children 6–18	.021	.048	.171	.023	−.052	.027
	(.033)	(.039)	(.106)	(.042)	(.039)	(.050)
p-values of *F*-tests:						
Jointly equal	.34	.50	.21	.06	.68	.40
Jointly equal to 0	.03	.09	.34	.01	.62	.00
N	5,775	5,775	1,038	4,730	1,105	3,621

	B. Worked ≥1 hour					
Treatment × one eligible child 6–18	−.058**	−.062**	−.011	−.086**	.092	−.117***
	(.023)	(.031)	(.052)	(.037)	(.128)	(.028)
Treatment × two eligible children 6–18	−.024	−.016	−.026	−.012	.089	−.046*
	(.022)	(.021)	(.042)	(.024)	(.059)	(.026)
Treatment × three eligible children 6–18	.008	.016	.038	.007	.046	−.007
	(.024)	(.023)	(.079)	(.024)	(.047)	(.026)
Treatment × four eligible children 6–18	.019	.012	−.138	.033	.143**	−.011
	(.031)	(.041)	(.127)	(.043)	(.070)	(.053)
p-values of *F*-tests:						
Jointly equal	.09	.21	.66	.13	.70	.03
Jointly equal to 0	.08	.29	.74	.20	.10	.00
N	5,798	5,798	1,043	4,748	1,122	3,622
Dummies for number of eligible children 6–18	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	No	Yes	Yes	Yes	Yes	Yes

Note. Sample: age 6–17 at baseline, completed no more than eighth grade. Standard errors in parentheses are adjusted for clustering within villages. All specifications include a constant. Additional controls include dummies for age, birth order, and household size, as well as interactions between the treatment dummies and continuous age, birth order, and household size (see text for details).

* Statistically significant at 10%.

** Statistically significant at 5%.

*** Statistically significant at 1%.

households). It is plausible that the cash transfer—coupled with a binding education condition—discouraged children in one-child households from dropping out and working. The positive effect on labor participation among unenrolled children is particularly evident for households with four eligible children. One might attribute it to sampling noise, but it may reflect that some households encouraged unenrolled children to move from leisure to work in order to compensate for lost income from a child who did not drop out and work.

D. Health Conditions

Table 10 column 1 reports estimates of equation (4), for health service use among young children. In panel A, infants were 19.3 percentage points more likely to be weighed if they lived in a household with no child eligible for the education transfer. The point estimate is smaller and insignificant when there is one eligible child. Households with no older children receive a much smaller transfer, but it is explicitly labeled a health transfer, and the households must register in health centers. A similar result is observed for checkups (panel B) but not immunizations (panel C).

Subsequent columns add controls for household size, child age, birth order, and interactions with the treatment dummies. Table 10 panel A's estimates—using small samples of infants—are quite imprecise. Across all columns in panel B, the point estimates are largest (as well as statistically significant) in the absence of older children in the household, but *p*-values do not allow us to reject joint equality. Finally, panel C suggests no effects for any child.

Table 11 includes dependent variables measuring mothers' use of health services. It, too, is hampered by small sample sizes and reduced precision, but two consistent results emerge. In panel B, the only large and statistically significant effects on tetanus immunization are observed among mothers with no children eligible for the education transfer, and they are largest in poor households (14.1 percentage points). In panel D, mothers with no children eligible for the education transfer—especially poor ones—were more likely to receive a postnatal checkup in the 10 days after birth (18.4 percentage points).

V. Conclusion

This article analyzed a Honduran experiment in which the households in 150 poor villages (in a sample of 300) were given large cash transfers with associated conditions. Households received annual payments of L 10,000 (about \$500) if at least one child between age 6 and 18 enrolled in grades 1–9 (the "education transfer"). Eligible children residing in households with only one eligible child were always subject to the education conditions. In households with many eligible children, the chance of being subjected to a binding con-

TABLE 10

HETEROGENEOUS EFFECTS BY FAMILY STRUCTURE ON THE USE OF HEALTH SERVICES BY CHILDREN

	Full Sample		Not Poor at Baseline	Poor at Baseline
	(1)	(2)	(3)	(4)
	A. Weighed in 30 days before follow-up			
Treatment × zero eligible children 6–18	.193**	.140	−.287	.173
	(.085)	(.112)	(.400)	(.118)
Treatment × one eligible child 6–18	.128	.124	.199	.052
	(.086)	(.106)	(.275)	(.127)
p-values of *F*-tests:				
Jointly equal	.59	.92	.34	.51
Jointly equal to 0	.03	.21	.62	.29
N	256	256	59	197
	B. Reason for last visit to health center was checkup			
Treatment × zero eligible children 6–18	.119***	.132***	.175***	.114***
	(.031)	(.032)	(.060)	(.035)
Treatment × one eligible child 6–18	.031	.014	−.025	.016
	(.027)	(.034)	(.049)	(.040)
p-values of *F*-tests:				
Jointly equal	.02	.01	.01	.06
Jointly equal to 0	.00	.00	.02	.01
N	1,068	1,068	272	796
	C. Complete immunizations			
Treatment × zero eligible children 6–18	.035	.016	.029	.008
	(.036)	(.043)	(.079)	(.051)
Treatment × one eligible child 6–18	.045	.049	.095	.038
	(.039)	(.047)	(.104)	(.050)
p-values of *F*-tests:				
Jointly equal	.83	.60	.62	.66
Jointly equal to 0	.39	.55	.61	.75
N	1,068	1,068	272	796
Dummies for number of eligible children 6–18	Yes	Yes	Yes	Yes
Additional controls	No	Yes	Yes	Yes

Note. Standard errors in parentheses are adjusted for clustering within villages. All specifications include a constant. Sample in panel A includes children age 0 at baseline, living in households with zero or one eligible child age 6–18 at baseline. Sample in panels B and C includes children age 0–3 at baseline, living in households with zero or one eligible child age 6–18 at baseline. Additional controls include dummies for age, birth order, and household size, as well as interactions between the treatment dummies and continuous age, birth order, and household size (see text for details).
** Statistically significant at 5%.
*** Statistically significant at 1%.

dition was reduced. We leveraged this variation to assess the importance of education conditions.

Households received half that amount—the "health transfer"—if they included young children (or a pregnant or nursing mother) and those individuals registered in a health center. Young children and mothers were only subject to health conditions (and only received a transfer labeled health) if there was no child eligible for an education transfer.

TABLE 11

HETEROGENEOUS EFFECTS BY FAMILY STRUCTURE ON THE USE OF HEALTH SERVICES BY MOTHERS

	Full Sample		Not Poor	Poor
	(1)	(2)	(3)	(4)
	A. Number of prenatal checkups during last or current pregnancy			
Treatment × zero eligible children 6–18	.330	.157	−.054	.290
	(.398)	(.437)	(.759)	(.553)
Treatment × one eligible child 6–18	.271	.412	−.305	.286
	(.362)	(.412)	(.710)	(.512)
p-values of *F*-tests:				
Coefficients jointly equal	.91	.67	.80	1.00
Coefficients jointly equal to 0	.57	.58	.91	.74
N	385	385	115	269
	B. Woman received tetanus shot before or during last/current pregnancy			
Treatment × zero eligible children 6–18	.114**	.136**	.104	.141*
	(.055)	(.058)	(.097)	(.073)
Treatment × one eligible child 6–18	.053	.065	−.011	.113
	(.064)	(.073)	(.136)	(.100)
p-values of *F*-tests:				
Coefficients jointly equal	.44	.44	.48	.83
Coefficients jointly equal to 0	.10	.05	.56	.07
N	368	368	112	255
	C. Woman received advice about birth plan			
Treatment × zero eligible children 6–18	.052	.060	.114	.001
	(.079)	(.082)	(.155)	(.098)
Treatment × one eligible child 6–18	−.022	.028	.059	.005
	(.074)	(.085)	(.198)	(.104)
p-values of *F*-tests:				
Coefficients jointly equal	.47	.79	.81	.98
Coefficients jointly equal to 0	.75	.73	.75	1.00
N	365	365	112	252
	D. Woman received postnatal checkup in 10 days after birth			
Treatment × zero eligible children 6–18	.162**	.149	.248	.184*
	(.079)	(.094)	(.159)	(.110)
Treatment × one eligible child 6–18	.117	.144	−.106	.056
	(.083)	(.101)	(.165)	(.131)
p-values of *F*-tests:				
Coefficients jointly equal	.66	.98	.12	.48
Coefficients jointly equal to 0	.07	.11	.23	.21
N	291	291	89	202
Dummies for number of eligible children 6–18	Yes	Yes	Yes	Yes
Additional controls	No	Yes	Yes	Yes

Note. Standard errors in parentheses are adjusted for clustering within villages. All specifications include a constant. Sample in panels A–C includes women age 12–49 at baseline who were pregnant at baseline or follow up and women who gave birth between January 2012 and July 2013 (if the household contains zero or one eligible child 6–18 at baseline). Sample in panel D includes women pregnant at baseline and women who gave birth between January 2012 and July 2013 (if the households includes zero or one eligible child 6–18 at baseline). Additional controls include dummies for age and household size, as well as interactions between the treatment dummies and continuous age and household size (see text for details).

* Statistically significant at 10%.

** Statistically significant at 5%.

The weight of evidence suggests that conditions play a role in mediating the size of effects on enrollment, child labor participation, and the use of some health services (even after ruling out alternative explanations for observed heterogeneity). Specifically, we find that eligible children, residing in households with only one eligible child, were 7.9 percentage points more likely to be enrolled in school, with a nearly offsetting effect of −6.2 percentage points on labor participation. The effects are larger—11.5 and −11.7 percentage points, respectively—for poor children enrolled at baseline. The results are less conclusive in health, but we only find statistically significant effects on health service use in subsamples of young children and mothers subject to health conditions, including checkups among young children and two measures of pre- and postnatal health care.

The evidence from Progresa and Progresa-inspired CCTs in Latin America suggests that the imposition of conditions mediates the size of program effects on the use of education and health services (Schady and Araujo 2008; de Brauw and Hoddinott 2011; Attanasio et al. 2015). Our article further suggests that effects on enrollment are accompanied by a reduction in child labor. The collected results imply that the added cost of monitoring school or health conditions deserves careful analysis when designing transfer programs, depending on the policy objectives.

To illustrate this point, we estimated the per-child cost of the PRAF-II education transfer, relying on administrative costs in Caldés et al. (2006). Overall, PRAF-II cost US$(2001)39 for each child eligible for the education transfer.[27] This included $10 of deadweight loss associated with the transfer (but not the transfer itself) and $29 for administrative costs and associated deadweight loss. Of administrative costs, $6 was spent on monitoring and enforcing conditions. In the full sample of eligible children, the impact on enrollment was 8 percentage points (Galiani and McEwan 2013), with a cost-effectiveness ratio of approximately $5 for each percentage point gain.

We do not have similarly detailed administrative cost data on Bono 10,000. But imagine a revised PRAF-II, now with a per-household transfer of L 4,528 (10,000 in 2001 prices). In this case, the deadweight loss of transfers is

[27] To calculate the deadweight loss of transfers in PRAF-II, we used the 2001 census to calculate the number of children who received the L 800 education transfer in treated municipalities (assuming full take-up). The deadweight loss per eligible child is $10, assuming an exchange rate of 15 lempiras/dollar and a deadweight loss from taxation of 20% (Auriol and Warlters 2012). To calculate the administrative cost, we used total administrative costs from 1999 to 2001 (Caldés et al. 2006), excluding costs of the external impact evaluation and delivery of a separate treatment (grants to schools and health centers). We also reduced the costs in proportion to the number of children eligible for the education versus the health transfer (using 2001 census data and following Galiani and McEwan 2013).

$23 per eligible child.[28] Further assume that the new "treatment" incurs the same administrative costs as PRAF-II, except for a 50% reduction in the costs of monitoring conditions. Overall, "Bono 4,528" costs $49 for each eligible child, or $13 for each percentage point gained in enrollment (using the full-sample effect of 3.8 percentage points from table 5). It highlights the simple lesson that smaller payments combined with judiciously applied conditions may be a more cost-effective way of increasing schooling investments. Indeed, the cost-effectiveness ratio falls to $7 per point if we assume a "conditioned" impact of 7.9 percentage points (table 9) and double the costs of monitoring conditions.

Of course, the objectives of Bono 10,000 and most CCTs are much broader, encompassing short-run redistribution and long-run improvements in household welfare due to myriad human capital investments occurring in schools, health centers, neighborhoods, and households. It suggests the need to measure all outcomes and perform a full cost-benefit analysis. A growing literature has begun to measure the longer-run impacts of CCTs, and its findings are mixed, with stronger effects on attainment but weaker effects on measures of achievement.[29] Relatedly, the test-score effects of conditional in-kind transfers (such as free school meals) and health treatments (such as de-worming) are often small, despite large effects on school enrollment and participation (see the meta-analysis in McEwan 2015). This evidence highlights that the worth of CCTs may hinge on the quality of services provided to poor families, particularly in rural schools and health centers.

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[28] We used the 2001 census to calculate the number of households that received a L 4,528 transfer in treated villages (assuming full take-up under the Bono 10,000 rules). We assumed the same exchange rate and deadweight loss of taxation as above.

[29] A 10-year follow-up of a Nicaraguan CCT found substantial effects on both attainment and test scores (Barham, Macours, and Maluccio 2013). Almost 6 years after the Progresa treatment, older children exposed to education transfers gained 0.7–1 grades in school but with no effect on achievement tests (Behrman, Parker, and Todd 2009, 2011). Two years after the conclusion of a Cambodian program, adolescents completed 0.6 more grades but with no effects on test scores or employment outcomes (Filmer and Schady 2014). Two years after the conclusion of a Malawi program, the CCT treatment arm only affected the attainment of a subgroup of girls who were not attending school at baseline, with mixed effects on test scores (Baird, Chirwa, et al. 2015).

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