IDB WORKING PAPER SERIES No. IDB-WP-577

Do Education and Health Conditions Matter in a Large Cash Transfer?

Evidence from a Honduran Experiment

Fiorella Benedetti
Pablo Ibarrarán
Patrick McEwan

February 2015

Inter-American Development Bank
Social Protection and Health Division

Do Education and Health Conditions Matter in a Large Cash Transfer?

Evidence from a Honduran Experiment

Fiorella Benedetti
Pablo Ibarrarán
Patrick McEwan

IDB

Inter-American Development Bank

2015

Cataloging-in-Publication data provided by the
Inter-American Development Bank
Felipe Herrera Library
Benedetti, Fiorella.
Do education and health conditions matter in a large cash transfer? evidence from a Honduran experiment / Fiorella Bendetti, Pablo Ibarraran, Patrick McEwan.
p. cm. — (IDB Working Paper Series ; 577)
Includes bibliographic references.
1. Transfer payments—Honduras. 2. Education—Honduras. 3. Public health—Honduras. 4. Poor children—Honduras. I. Ibarraran, Pablo. II. McEwan, Patrick J. III. Inter-American Development Bank. Social Protection and Health Division. IV. Title. V. Series.
IDB-WP-577

http://www.iadb.org

scl-sph@iadb.org
www.iadb.org/ProteccionSocial

Do Education and Health Conditions Matter in a Large Cash Transfer?

Evidence from a Honduran Experiment[*]

Abstract: The paper analyzes a new Honduran conditional cash transfer experiment ("Bono 10,000") in which 150 poor villages (of 300) were treated. The transfers were much larger in size than an earlier experiment (Galiani & McEwan, 2013), but yielded smaller full-sample effects on school enrollment, child labor participation, and measures of health service use. One explanation is that Bono 10,000 did not apply conditions to children: only one child in eligible households was subject to the education condition, and young children and mothers were only subject to health conditions in the absence of older children. Consistent with this, we find a large effect on enrollment (and a nearly off-setting one on child labor) among "only" children, and smaller and insignificant effects on children in larger households. We only find significant effects on health service use among children and mothers in the absence of older children (despite a much smaller household transfer). The heterogeneity does not appear to be driven by correlated variables such as household size, child age, or poverty.

JEL Classification: C93, I15, I25, I38.

Keywords: Transfer payments, Honduras, education, public health, poor children, conditional cash transfers (CCTs), impact evaluation.

---

[*] We are grateful to many personnel of NORC at the University of Chicago and its Honduran partner, ESA Consultores, for their extraordinary efforts in survey design and implementation, especially Helmis Cárdenas, Carlos Echevarría, Fidel Ordóñez, Michael Reynolds, and Florencia Rodriguez, as well as Maria Deni Sánchez from the IDB's country office. We also thank Kristin Butcher, Dan Fetter, Sebastian Galiani, Phil Levine, Renán Rápalo, and Norbert Schady for their helpful comments, without implicating them for errors or interpretations.

Fiorella Benedetti and Pablo Ibarrarán are with the Inter-American Development Bank. Patrick J. McEwan is with Wellesley College.

1. 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 (Adato & Hoddinott, 2011; Fiszbein & Schady, 2009). The voluminous and often experimental evaluation literature is now the subject of many systematic reviews and meta-analyses. This literature shows that CCTs consistently increase school enrollment and attendance (Baird et al., 2014; Petrosino et al., 2012; Saavedra & Garcia, 2013), reduce child labor on the intensive and extensive margins (de Hoop & Rosati, 2014; Kabeer, Piza, & Taylor, 2012), and increase the use of preventive health services among mothers (Glassman et al., 2013) and children (Gaarder, Glassman, & Todd, 2010; Lagarde, Haines, & Palmer, 2007; Owusu-Addo & Cross, 2014).

An early Honduran experiment—included in the reviews—found that small per-child transfers of no more than $50 per year had substantial effects on increasing primary school enrollments (by 8 percentage points, or 12% of the control-group enrollment rate), reducing child labor force participation (by 3 p.p., or 30%), and increasing various measures of health-service use (Galiani & McEwan, 2013; Morris et al., 2004). Children between 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 visit health centers. Some conditions were weakly applied, especially school attendance and health center visits (Glewwe & Olinto, 2004; Morris et al., 2004). But, the costs of enforcing even minimal conditions were non-negligible. In 2001, they were 15% of administrative costs, while total administrative costs were 31% of the transfers (Caldés, Coady, & Maluccio, 2006).

An important question, in Honduras and other resource-constrained contexts, is whether the imposition and monitoring of conditions increases outcomes beyond that of an unconditional cash transfer. Cash alone may increase demand for schooling and/or health services via income effects, but a CCT decreases opportunity costs and could further occasion a substitution effect (Baird et al., 2014). Whether this occurs is an empirical question, analyzed in a growing body of papers.[1]

It has been most compellingly studied in randomized experiments with unconditional and conditional treatment arms.[2] However, none of this evidence is from Latin America, where researchers have relied on non-experimental—but perhaps exogenous—variation in the enforcement of conditions.[3] In Ecuador and Mexico, the effects on school enrollment were lower when some households believed that the cash transfers were unconditional, due to quirks in program implementation (de Brauw & Hoddinott, 2011; Schady & Araujo, 2008). In the Ecuador experiment, however, the effects on child labor were the same, regardless of households' beliefs about education conditionalities (Edmonds & Schady, 2012). In Colombia, health conditions were only enforced among children born before mothers registered for the CCT

[1] Baird 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 conditionalities' strength.

[2] Adding school-related conditions increased the impact of transfers on drop-out rates in Malawi (Baird, McIntosh, & Özler, 2011). In Burkina Faso, conditions increased school enrollments, but only among subgroups of girls, younger children, and lower-ability children (Akresh, de Walque, & Kazianga, 2013). The same experiment found that that health center visits only increased substantially among young children in the presence of conditions (Akresh et al., in press). In Zimbabwe, the positive effects on school attendance were similar across conditional and conditional 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., in press).

[3] 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).

(Attanasio, Oppedisano, & Vera-Hernández, in press). Health-center visits were substantially lower among children born after the registration date.

This paper assesses whether the strength of 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. The transfer size in Bono 10,000 was much larger than its predecessor, PRAF-II (Galiani & McEwan, 2013; Glewwe & Olinto, 2004). Most households received annual payments of 10,000 Lempiras (approximately $500), and $250 in some cases. The typical family would have received per-capita transfers equal to 18% of median per-capita consumption, provided they complied with conditionalities. Despite the larger transfers, the full-sample impacts were smaller than PRAF-II. As we will show, Bono 10,000 increased enrollments by about 4 percentage points (6% of the control group enrollment rate), and reduced child labor by 1.2 p.p. (5%), though the latter point estimate was not statistically significant at conventional levels. For health-service use, we find mixed results: young children in the treatment group are 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 will report a complete set of full-sample estimates, since the Bono 10,000 experiment has not been previously analyzed.)

One plausible explanation for differences is variability in the strength of conditions across the two experiments.[4] Unlike PRAF-II, the conditions were not applied uniformly across each

[4] 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. Section 2.3 compares samples used in the two experiments using the only common data set, the 2001 census, and finds that both have extremely high rates of extreme poverty in 2001: 89% and 84% in PRAF II and Bono 10,000, respectively.

child in a participating household. In Bono 10,000, households received $500 if *at least one 6-18 year-old* enrolled in and regularly attended school grades 1 to 9, implying that conditions were less likely to be applied to children with many siblings. Households received $250 if children under 6 (and pregnant or nursing mothers) regularly visited health centers, *but only in the absence of children between the ages of 6 and 18*. In the presence of an older child, the household transfer was doubled, but health conditions were no longer enforced, nor was it even labeled a health transfer.

We find that the effects on increasing enrollment and reducing child labor are strongest among "only" children between 6 and 18. Among these children, enrollment increased by 8.3 p.p., while labor supply decreased by 6 p.p. The effects were closer to zero and were not statistically distinguishable from zero in families with more children between the ages of 6 and 18. It is tempting to interpret this heterogeneity as the exclusive result of conditionalities, but it may be due to other factors. First, more 6-18 year-olds in the household implies a falling per-capita transfer and a higher mean age of eligible children. Second, larger households are usually poorer ones, a stylized fact also evident in our data.

We address the first concern by including fixed effects for discrete household sizes and student ages, and then interacting continuous household size and age terms with the (heterogeneous) treatment effects, as described in section 3.2. This is facilitated in our data by the fact that many households with different numbers of 6-18 year-olds nonetheless have the same number of total household members. In these estimates, the pattern of heterogeneity by number of 6-18 year-olds persists among households at the sample means of household size and child age.

Regarding the second concern, we argue that positive correlations between the number of children and poverty effectively stack the deck *against* finding larger effects among "only" children (and smaller ones in larger households). In Honduras and elsewhere, the literature typically finds larger effects among poor households (Fiszbein & Schady, 2009; Galiani & McEwan, 2013). Indeed, restricting the sample to poor households slightly reinforces the aforementioned pattern of heterogeneity by number of children 6 to 18. We conclude that the application of education conditionalities played an important role in increasing effects on enrollment and child labor participation.

The results are less clearcut in health because the application of health conditionalities only occurs in households with no children over the age of 5, which happens to be perfectly collinear with receipt of the smaller transfer of $250. Despite this, the only statistically significant effects on health-service use occur in such households. In poor households with no children between 6 and 18, the treatment increased the probability that a young child's last visit to a health center was a checkup by 7 percentage points, while the point estimate was smaller and statistically insignificant in households with one older child (as above, the model controls for household size, child age, and interactions with the treatment indicators). We find a similar pattern of results for two indicators of maternal health use: the receipt of tetanus immunization before or during a pregnancy, and the receipt of a postnatal checkup. These results could be generated by the differential application of conditionalities, though it is also plausible that simply labeling it a "health" transfer nudged households to seek medical care.

The paper makes two main contributions to the growing literature on the role of conditionalities. 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 RCTs relies on unintended variation (within experimental samples) in the understanding of conditions, but it focuses on education outcomes (de Brauw & Hoddinott, 2011; Schady & Araujo, 2008). The Ecuador experiment did not show any effects of households' beliefs about conditionalities on child labor (Edmonds & 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, Oppedisano, & Vera-Hernández, in press).

Worldwide, the most credible evidence on the role of conditions comes from African RCTs with unconditional and conditional treatment arms (Akresh et al., in press; Akresh, de Walque, & Kazianga, 2013; Baird, McIntosh, & Özler, 2011; Benhassine et al., in press; Robertson et al., 2013). The meta-analysis of Baird et al. (2014) shows 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 (large) body of Latin American experiments.

Second, and also related to external validity, this paper provides of 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 implementation of cash transfers, by the same government agency, in high-poverty communities. However, the size and structure of the cash transfers varied considerably, posing an interesting question as to why the effects of Bono 10,000 were attenuated. This paper's estimates suggest a plausible explanation: that enforcement of conditions attached to cash transfers are relevant in the Honduran context.

2. The Bono 10,000 Experiment

2.1. Background

The Honduran *Programa de Asignación Familiar* (PRAF), or Family Allowance Program, has distributed cash transfers to poor households since the early 1990s (Galiani & McEwan, 2013; Moore, 2008). The early phase, PRAF-I, distributed cash to families with young children and pregnant or nursing mothers, conditioning their receipt on school enrollment and health center visits. However, the putative conditionalities were not enforced, poverty targeting was weak, and the program was never rigorously evaluated (Moore, 2008).

A successor program, PRAF-II, is more familiar to economists. It identified the 70 poorest municipalities in Honduras (of 298), and distributed cash transfers to households in a random subset of 40, from 2000 to 2002 (Galiani & McEwan, 2013; Glewwe & Olinto, 2004; Morris et al., 2004). Children ages 6 to 12 were eligible for education transfers of about $50 per year per child if they (1) had yet to complete the 4th 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 & Schady, 2009). Moreover, PRAF weakly enforced the conditionalities. Enrollment (but not attendance) were enforced in the education transfer (Glewwe & 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 & McEwan, 2013). Health transfer eligibility increased the proportion of mothers with 5 or more prenatal appointments by 19 percentage points (a 38% gain over the control group), increased young childrens' 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).

2.2. The Bono 10,000 Treatment

In 2010, the newly-elected administration modified, expanded, and re-named the CCT. Under "Bono 10,000," PRAF offered payments of either 5,000 or 10,000 Lempiras per household per year (about $250 or $500, respectively). Transfers were to be paid in three cash installments by personnel of PRAF and the state-owned bank BANADESA. In the initial phase of program implementation, households were to receive the larger amount if they: (1) resided in a village declared as eligible by program administrators, based on poverty, (2) were poor as defined by a proxy means test, (3) had at least one child aged 6 to 18 years-old who had not completed the 9th grade, and (4) enrolled at least of the eligible children in school. As with PRAF-II, regular school attendance (80% of classes) was a nominal conditionality, but it was not regularly enforced. Households were to receive the smaller amount if they: (1) lived in an eligible village and passed a proxy means test, (2) had at least one child under the age of 6 and/or a pregnant or postpartum mother, but no eligible children between 6 and 18; and (3) regularly attended health centers, following Ministry of Health guidelines.

The treatment differed from PRAF-II in two regards. 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 per capita consumption is 10,789 Lempiras per capita per year (about $1.48 per day).[5] Given eligibility conditions, a typical household would have received 1,946 Lempiras per capita, or 18% of median consumption. This figure is closer to other Latin American CCT programs such as Progresa/Oportunidades (Fiszbein & 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 probability that eligible children were subject to conditions. Households with any number of children ages 6 to 18 (who had not completed 9th grade) were obligated to enroll only one child in school. This suggests that school-aged children in larger families had relatively lower probabilities of being subject to the education condition. Relatedly, households with children under 6 and/or pregnant women were subject to the health conditions only in the absence of older children. In the presence of older children, the health conditions did not apply, nor were they mentioned to recipients. Section 4 will describe how we leverage this variation to identify the effects of conditions on the use of education and health services.

2.3. Village Sample and Randomization

The government began implementing Bono 10,000 during the experiment's design phase, during 2010 and the first half of 2011. They initially targeted the poorest of Honduras' 3,727 *aldeas* or villages (which are themselves located within 298 municipalities and 18 departments).

[5] The headcount ratio is 77%, implying that the proxy means test allowed substantial leakage of non-poor households into the sample.

Given this constraint, the experimental sample was drawn from 816 slightly less poor villages not already treated.[6] On September 9, 2011, the treatment and control groups—each consisting of 150 villages—were randomly drawn from this group of 816 (see Figure 1).[7]

The shaded area in Figure 2 illustrates the 70 municipalities included in the earlier PRAF-II experiment because they ranked the lowest on a poverty proxy (Galiani & McEwan, 2013). They are dominated by traditionally poor and indigenous departments of western Honduras, especially Intibucá and Lempira. The 300 villages included in the Bono 10,000 experiment are outlined in black. Forty-nine, or 16%, are located in PRAF-II municipalities, but the rest are dispersed throughout the country.

To compare the household composition of each experiment, we use the 2001 census. Table A1 reports descriptive statistics for children ages 0 to 18. In PRAF-II municipalities, the average child's mother had 2.3 years of schooling, compared with 2.7 in the 816 villages eligible for the Bono 10,000 experiment, and 4.2 in ineligible villages. Using a 1999 household survey, we estimated 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 & McEwan, 2013). We then averaged across the predicted probabilities of extreme poverty in the census sample (Tarozzi & Deaton, 2009). This estimate of the extreme poverty rate was 89% in PRAF-II municipalities, 84% in villages eligible for the Bono 10,000 experiment, and 67% in the ineligible villages. The evidence confirms that the Bono 10,000

[6] The early treatment status of all villages cannot be reconstructed, but it appears to have relied on a mix of village-level extreme poverty estimates based on a poverty map, and more subjective criteria.

[7] 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 villages' treatment status was not publicly announced.

experiment included poor, rural villages distributed throughout the country, but not some of the poorest villages in western Honduras.

2.4. 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 a modest number of variables related to household structure, dwelling quality, and asset ownership. Second, the government applied a proxy means test, based on 5 variables, and constructed a list of nominally poor households.[8] Third, NORC at the University of Chicago randomly sampled approximately 15 poor households from each village's list of poor households.[9]

One village in the treatment group, and 3 in the control group refused to participate in the evaluation. 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 questionnaires, was applied over a shorter period between March and June 2013.

[8] 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 5 variables, including availability of electricity and sewer services, and a household asset index.
[9] In villages with more than 15 households, 15 were randomly chosen. In the 4 villages with fewer than 15, all were chosen.

2.5. Baseline Balance and Endline Attrition

Table 1 assesses whether observed socioeconomic variables are—as expected—balanced across households in the treatment and control villages. The sample includes children under 18 who appeared in the baseline survey.[10] The variables include measures of parental schooling and literacy, household structure, dwelling quality, and access to utilities. For now, we focus on variables that are plausibly invariant over short time periods that may coincide with early treatment. Across all variables, 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 Figure 1). These rates are slightly lower—88.2% and 83.3%, respectively—if one imputes 15 non-responding households for each of the 4 non-responding villages. To assess whether non-random attrition introduced observed differences across treatment and control groups, Table 1 reports descriptive statistics on baseline variables in the restricted sample of households responding to the endline survey. In general, the control-group means are similar even after removing attriting households. Moreover, the treatment-control differences are still small and not statistically significant. Despite this evidence, differential attrition raises the possibility of selection on unobservables and possible bias. Thus, our estimates will report bounds based on a trimming procedure (Lee, 2009).

[10] Note that these differences are based on a sample that already omits 4 non-responding villages and a small amount of non-responding households in the baseline survey of 296 villages (see Figure 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 nonetheless small in magnitude.

3. Empirical Approach

3.1. Main Estimates

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

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

where O_{ij} is the endline outcome of unit i—whether household, child, or mother—in village j, and T_j is the village-level 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 further present two specifications as robustness checks. The first controls for baseline socioeconomic variables from Table 1, dummy variables indicating any missing values of these variables,[11] and dummy variables indicating Honduran departments and the week in which the endline survey was conducted. The second specification adds the lagged value of the dependent variable, measured at baseline.

3.2. School Enrollment Conditions

Recall that poor households receive 10,000 Lempiras per year if: (1) they include any number of children ages 6 to 18 who have not completed 9th grade, and (2) at least one of them enrolls in school. It suggests that an eligible child's probability of being subject to conditions, and perhaps her outcome as well, depend on the number of children in the household. Thus, we

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

estimate (in the subsample of eligible children residing in homes with 1 to 4 children between 6 and 18):

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

where C_{ij} is the number of children between 6 and 18 in the household (at baseline) and $1\{\cdot\}$ is an indicator function. The β_k represent treatment effects in households with 1, 2, 3, or 4 children.

Suppose that $\hat{\beta}_k$—the marginal effect on enrollment—monotonically declines as k increases. It is tempting to conclude that children in larger households are less likely to enroll because of conditionalities, but there are alternate explanations. First, the per-capita transfer declines as the number of children increases (given per-household transfers of 10,000). Second, the mean age of 6-18 year-olds is higher when they reside in households with more children. (Table A2 shows that the average "only" child is 10.3 years old, compared with 11.6 in households with 4 children.) Third, family size is correlated with other observed and unobserved variables, such as poverty. Consistent with patterns in other developing countries, Table A2 shows that baseline poverty rates increase from 71% to 88% as the number of children increases from 1 to 4. To the extent that impacts are mediated by the size of per-capita transfers, child age, poverty, or correlated variables, then heterogeneity in equation (2) may not reflect the importance of conditionalities.

To address the first concern, we leverage the fact that per-capita transfers remain constant—even as the number of 6-18 year-olds 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:

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

where H_{ij} is the total household size of each child, $\overline{H}$ is the mean in the estimation sample, 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 6-18 (in a household of average size). In subsequent tables, the regressions include similar terms for child age: $\sum_{k=1}^{4} \lambda_k \cdot T_j \cdot 1\{C_{ij} = k\} \cdot (A_{ij} - \bar{A}) + \sum_{g=2}^{MAX(A_{ij})} \pi_g \cdot 1\{A_{ij} = g\}$, where A_{ij} is the child's age and $\bar{A}$ is the estimation sample mean.

It is still possible that heterogeneous effects are mediated by household poverty and its correlates. The accumulated evidence on CCTs, particularly in Honduras, suggests that larger enrollment effects will be observed in poorer households (Fiszbein & Schady, 2009; Galiani & McEwan, 2013). This would tend to stack the deck against finding larger enrollment effects in relatively richer, one-child households, than in relatively poorer households with more children. To address this, we report all estimates in subsamples of poor and non-poor households, using the consumption-based poverty estimate from the baseline survey.

3.3. Health Conditions

Households are only subject to the condition of regularly attending health centers if (1) they include at least one child under 6 or a pregnant or nursing mother, but (2) do not include children between 6 and 18. As a first step, therefore, we estimate (in the subsample of eligible young children or mothers who reside in homes with 0 or 1 child between 6 and 18):

$$O_{ij} = \beta_0 + \beta_1 \cdot T_j \cdot 1\{C_{ij} = 0\} + \beta_2 \cdot T_j \cdot 1\{C_{ij} = 1\} + \gamma \cdot 1\{C_{ij} = 1\} + \varepsilon_{ij} \quad (4)$$

where O_{ij} is a now a measure of health service use and C_{ij} still measures the number of children 6 to 18 in the household. To the extent that conditions matter, we might anticipate that $\hat{\beta}_1$ will be larger than $\hat{\beta}_2$. The same caveats still apply, and so we include interaction terms for household size and age, as above. Even so, note that the household transfer sharply increases from 5,000 to 10,000 with the addition of just one child 6 to 18 who enrolls in grades 1 to 9. This is hopelessly collinear with the application of the health condition, but we note that it dramatically stacks the deck against finding effects of stronger health conditions (presuming that demand for health services increases with income).

4. Results

4.1. Transfers to the Treatment and Control Groups

To assess how (and whether) Bono 10,000 was implemented in the household sample, we linked it to PRAF's administrative payments database. In Figure 3, the top panel's y-axis measures the proportion of households that had received *any* payment from PRAF, and the x-axis indicates dates after randomization. The solid line, corresponding to control-group households, shows that a very small percentage had received any payment by the date of the final endline survey (indicated by the vertical dotted line at June 23, 2013). Table 2 confirms that only 7% of the control-group households received any payment prior to the particular dates of their endline surveys; the mean payment in the control group was 91 Lempiras (less than $5).

In the treatment group, on the date the final *baseline* survey was applied, over 70% of the treatment group had received a single registration payment (see the top panel of Figure 3). Our conversations with PRAF personnel—confirmed by the data—indicated that these were one-time, unconditional "registration" payments to some households. The payments were either 417 or 833 Lempiras, representing 1/12 of the annual transfer of either 5,000 or 10,000. Table 2 confirms that 58% of the treatment group had received a single payment prior to application of that household's baseline survey (though the mean payment to treatment-group households was a modest 442 Lempiras, or $22). By the time each household's endline survey was conducted, 91% of the treatment-group households had received at least one payment, with an overall mean of just under 7000 Lempiras ($350) (see Table 2).

The results suggest that payments were widely distributed in the treatment group, with minimal crossover to the control group. However, the pre-baseline payments to the treatment group raise two concerns. First, some outcome measures—self-reported at the baseline—might have been influenced by small payments and the expectation of larger ones. This is especially true for outcomes that reference the week or month prior to the survey.[12] Second, it suggests that households—unblinded to their treatment status—may have varied incentives to mis-report baseline outcomes. Given this, our preferred estimates do not control for (potentially endogenous) baseline outcomes. The point is somewhat moot, however, because unconditional estimates based on equation (1) are generally precise and rarely sensitive to additional controls.

4.2. Full-Sample Results

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

Table 3 reports effects on common measures of consumption and income. The estimates in column (1) are the unadjusted mean difference between households residing in treated and untreated villages, while columns (2) and (3) add the controls discussed in section 4.1. Two

[12] For example, this is more likely with labor force participation (reported in the week prior to 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.

additional columns report the baseline and endline means of the outcome variable in the control-group. The final column reports the lower and upper bounds of the trimming exercise described by Lee (2009).[13] The bounds do not overturn any of the conclusions below, and so 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 variable includes expenditures, as well as the estimated value of in-kind goods and services). The increase was similar for both food and non-food consumption, while the effect on the share of consumption devoted to food was not statistically different from zero. Theory (and the Engel curve) predicts declining food shares as incomes rise. The contrary result, frequently observed in CCT evaluations, could be interpreted as evidence that transfers provided to female heads-of-household are allocated differently than regular income (Attanasio & 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 the control-group poverty rate actually rose from 77% to 81% between the baseline and endline).[14] The treatment also reduced the depth of poverty, as gauged by Foster-Greer-

---

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

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

Thorbecke measures.[15] The poverty gap and the squared poverty gap 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 qualitatively similar for extreme poverty, although the estimate on the dummy variable indicating extreme poverty is imprecisely estimated.

Household income per capita, from all sources, was approximately 12.4% higher in the treatment group.[16] There is no evidence that the treatment tended to lower labor income (via effects on labor supply) or non-labor 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 a dummy variable indicating any hours of work in the week prior to the survey, nor on adult labor income (conditional on any hours worked).[17] The labor supply results are broadly consistent with the literature on PRAF-II and other Latin American CCTs (Alzúa, Cruces, & Ripani, 2013).

4.2.2. School Enrollment and Child Labor Supply

In the sample of children who were eligible for the education transfer, treatment-group children were 3.8 percentage points more likely to be enrolled in school at the time of the endline (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

---

[15] The poverty gap for each observation is $\left(\frac{z-c_i}{z}\right)^a \cdot 1\{c_i < z\}$, 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).

[16] Labor and non-labor 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 prior to the endline survey date and dividing by 12.

[17] The survey did not measure hours of work.

forestalled drop-outs among some students, in addition to encouraging enrollments among never-attenders or drop-outs.

The former explanation is better supported by the data. Table 5 also 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 7-8). The treatment did not increase enrollments among unenrolled children, with coefficients very close to zero. However, the probability of endline 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., in press). The survey 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 column (1), the effect is only statistically significant for poor children, although the magnitude of the effect is similar (and less precisely estimated) for non-poor children. In columns (2) and (3), the addition of controls widens the gap between the point estimates and, in column (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 prior to the endline survey. There are no full-sample effects, at odds with evidence from PRAF-II (Galiani & McEwan, 2013). However, this appears to mask *increased* labor supply among children not enrolled at baseline (by about 5 percentage points, or 10% of the control-group mean), and decreased labor supply among enrolled children (by 2.8 percentage points, or 19%). As with school enrollment, the effect is concentrated among those 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.

4.2.3. Child and Maternal Health

We next examine whether the use of health services increased in samples of children and mothers.[18] Among infants less than a year old, the percentage who had 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 to 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 check-up. However, there was no effect on the percentage of young children with a complete set of immunizations, including 3 doses of the pentavalent and oral polio vaccines, and a single dose of the BCG and measles, mumps, and rubella vaccines.[19] The substantial growth in control-group vaccination, from 54% to 77%, is consistent with Honduras' widespread vaccine 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 small in magnitude and not statistically different from zero at 5%.

[18] The age-range of samples is imposed by age-eligibility for questions on the baseline and endline surveys.
[19] The results are not sensitive to use of individual vaccine measures and/or narrower age groups (e.g., 12 to 23 months-old at endline).

Lastly, 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 very 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 a one-year treatment period.[20]

4.3.1. Education Conditions

Table 9 assesses whether the number of children between 6 and 18 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 8.3 percentage points for "only" children, 6.3 percentage points in 2-child households, and smaller and statistically insignificant in households with 3 or 4 children. The accompanying *p*-values allow us to reject two null hypotheses that effects are jointly equal, or jointly equal to zero. The question remains as to whether heterogeneous effects can be explained by the relative strength of enrollment conditions or, perhaps, variables correlated with family size.

The next column's regression, based on equation (3), adds controls for household size, child age, and interactions with the treatment indicator. The point estimates are not substantially changed and the pattern of larger estimates for children with less or no siblings remains. Each coefficient's statistical significance, or lack thereof, is preserved (although both *p*-values for both F-tests now exceed 0.05). Recalling earlier caveats, we split the sample into children residing in non-poor and poor households. In the non-poor sample, there is no evidence of enrollment

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

effects in smaller households, and both *p*-values exceed 0.9. In contrast, the coefficients in the sample of poor households are consistent with full sample results: effects of 7.7 and 6.7 percentage points for 1- and 2-child households, respectively. Reduced precision, however, still does not allow us to reject the null hypothesis that effects are jointly equal.

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. Even so, the magnitude of the point estimate in 1-child households is appreciable, at 4.9 percentage points. The effects among poor, enrolled children monotonically decline with the number of children, and the effect on "only" children (8.6 percentage points) is the largest observed in any subgroup.

One might still 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). After restricting the sample to poor households, the inverse relationship between the number of children and mother's schooling is attenuated but not eliminated.[21] However, the usual patterns of heterogeneity in CCT effects, cited before, still imply that biases work against finding larger effects in households with fewer children.

Panel B reports estimates using a dummy variable indicating that children worked at least one hour in the week prior to the survey. Recall that Table 6 showed a modest negative effect on the probability of working (2.8 percentage points), but only among children enrolled at baseline. It also showed increased labor supply among unenrolled children, albeit at the margin of statistical significance. The results in Table 9 show that the largest estimated decline in child

[21] In the sample of poor households, the mean schooling of mothers with 1, 2, 3, and 4 children between 6 to 18 is 3.66, 3.59, 3.12, and 2.93, respectively.

labor—7.9 percentage points—is for poor children, enrolled at baseline, in households with only one child between 6 and 18 (while the coefficients monotonically increase in multi-child households). Among these children, the large reduction in child labor offsets a very similar increase in enrollment. Given declining enrollment rates in the control group (and rising rates of child labor), it appears that the cash transfer—coupled with binding conditions—discouraged children from dropping out and working. We also note that the positive effect on labor participation among unenrolled children is largely explained by children in households with 3 children between 6 and 18. In the absence of a compelling explanation for this anomaly, one might attribute it to sampling noise.

In summary, the results shown in Table 9 suggest that in households with fewer children the education conditionalities were more likely to be binding, and it is in those households where we observe significant impacts. It is possible that households with one or two kids are different from households with more kids in some unobserved way (not related to poverty or total household size). This could also mediate the size of treatment effects, but no obvious stories suggest themselves.

4.3.2. Health Conditions

Table 10 reports estimates of equation (4), using measures of child health service use. Regardless of the dependent variable, the first column suggests that point estimates are larger in households with no children over the age of 5. While these households are eligible for a smaller payment of 5,000, they are also subject to health conditionalities (and the transfer is explicitly labeled as a health transfer). In panels A and B, the larger estimates are statistically significant. In panel C, younger children are 5.5 percentage points more likely to have complete immunizations in households with no children 6-18, but it is not statistically distinguishable from zero. The results are especially surprising because the per-household payment doubles in households with school-aged children who enroll.

Subsequent columns add controls for household size, child age, and treatment interactions. Panel A's results—using the very small sample of infants—have sufficiently large standard errors to not justify strong conclusions. 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 a large and statistically significant increase in immunizations (19.3 percentage points), but only among non-poor households.

Table 11 includes 4 dependent variables measuring mothers' use of health services. It, too, is hampered by small sample sizes and reduced precision, but two strong results emerge. In Panel B, the only large and statistically significant effects on tetanus immunization are observed among mothers without children over 5 years old, and they are largest in poor households (19.9 percentage points). In Panel D, mothers with no children over 5—especially poor ones—were substantially more likely to receive a postnatal checkup in the 10 days following birth (25.4 percentage points). The pattern of point estimates in Panel C (birth plan advice) is consistent with other findings, but no coefficient is statistically significant.

5. Conclusions

This paper analyzed a large Honduran experiment, in which the households in 150 poor villages (in a sample of 300) were awarded conditional cash transfers. Under "Bono 10,000," households received annual payments of 10,000 Lempiras (about $500) if at least one child between the ages of 6 to 18 enrolled in grades 1 to 9 (the "education" transfer). Households received half that amount if they *only* included children under 6 (or a pregnant or nursing mother), and those individuals attended health center checkups (the "health" transfer).

In the first case, we noted that an "only" child in a household receiving the education transfer was obligated to enroll. In a household with many children between 6 and 18, the probability of being subject to the education condition was plausibly reduced. We leverage this non-experimental variation within the experiment to assess the importance of education conditions. In the second case, we observed that young children and mothers were only subject to health conditions (and only received a transfer labeled for health purposes) if there were no older children present in the household (although the imposition of health conditions was also accompanied by a halving of the transfer size).

We find evidence to suggest that conditions play a role in mediating the size of effects on enrollment, child labor participation, and use of health services (even after ruling out alternative explanations for observed heterogeneity in effects). Specifically, we find that "only" children, residing in treated households, were 8.3 percentage points more likely to be enrolled in school at the endline survey; the effects were smaller and not statistically different from zero in households with more children. The effects are similar in a sample of poor households, and when allowing for heterogeneous effects by household size and child age (all of which are correlated with the number of children between 6 and 18). We find nearly off-setting effects, of the opposite sign, on child labor participation. The results are less straightforward in health: perfect collinearity of conditions and the (halved) household transfer stacks the deck against finding larger effects on health service use among children and mothers subject to conditions. Even so, we only find statistically significant effects on health center use in subsamples of young children and mothers subject to health conditions, including checkups among young children and two measures of pre- and post-natal health care. There are no statistically significant effects in the presence of one child between 6 and 18 (despite a doubled household transfer).

The evidence from Progresa and Progresa-inspired CCTs in Latin America consistently suggests that the imposition of conditions mediates the size of program effects on the use of education and and health services (Attanasio, Oppedisano, & Vera-Hernández, in press; de Brauw & Hoddinott, 2011; Schady & Araujo, 2008). Our paper further suggests that effects on enrollment are accompanied by reduction in child labor. Combined with the weight of African RCTs (such as Baird et al., 2011) and a recent meta-analysis (Baird et al., 2014), it implies that the added cost of conditionalities might be justified (or, at least, deserves careful analysis in any policy design). It bears emphasis that the Honduran conditions applied in both PRAF-II and Bono 10,000—especially in education—are considerably weaker than their early description would suggest. In both cases, the enrollment condition was enforced, but regular attendance was not.

However, this literature (and our paper) still has little to say about whether the added social benefits of conditions are outweighed by their costs. In comparison to the voluminous literature that documents effects on the use of education and health services, there is a sparser and mixed literature on whether this is effectively translated into short- or longer-run effects on human capital. For example, the few studies that measure a test score outcome find small effects, on average (Baird et al., 2014).[22] This suggests that we must not neglect the supply side of social service delivery, particularly in rural schools and health centers. In fact, the earlier PRAF-II experiment was a fully factorial design that combined CCTs with targeted grants to schools and

[22] Similarly, the test-score effects of conditional in-kind transfers (such as free school meals) and health treatments (such as de-worming) are often small (or zero), despite large effects on school enrollment and participation (McEwan, in press).

health centers, but this component of the treatment was not fully implemented (Galiani & McEwan, 2013).

References

Akresh, R., de Walque, D., & Kazianga, H. (2013). *Cash transfers and child schooling: Evidence from a randomized evaluation of the role of conditionality.* Policy Research Working Paper No. 6340. Washington, DC: World Bank.

Akresh, R., de Walque, D., & Kazianga, H. (in press). Alternative cash transfer delivery mechanisms: Impacts on routine preventative health clinic visits in Burkina Faso. In S. Edwards, S. Johnson, & D. N. Weil (Eds.), *African Successes: Human Capital.* Chicago: University of Chicago Press.

Alzúa, M. L., Cruces, G., & Ripani, L. (2013). Welfare programs and labor supply in developing countries: Experimental evidence from Latin America. *Journal of Population Economics*, *26*, 1255-1284.

Attanasio, O., & Lechene, V. (2010). Conditional cash transfers, women and the demand for food. IFS Working Paper 10/17. London: Institute for Fiscal Studies.

Attanasio, O. P., Oppedisano, V., & Vera-Hernández, M. (in press). Should cash transfers be conditional? Conditionality, preventive care, and health outcomes. *American Economic Journal: Applied Economics.*

Baird, S., Ferreira, F. H. G., Özler, B., & Woolcock, M. (2014). Conditional, unconditional and everything in between: A systematic review of the effects of cash transfer programs on schooling outcomes. *Journal of Development Effectiveness*, *6*, 1-14.

Baird, S., McIntosh, C., & Özler, B. (2011). Cash or condition? Evidence from a cash transfer experiment. *Quarterly Journal of Economics, 126,* 1709–1753.

Barrera-Osorio, F., Bertrand, M., Linden, L. L., & Perez-Calle, F. (2011). Improving the design of conditional transfer programs: Evidence from a randomized trial. *American Economic Journal: Applied Economics, 3,* 167-195.

Benhassine, N., Devoto, F., Duflo, E., Dupas, P., & Pouliquen, V. (in press). Turning a shove into a nudge? A “labeled cash transfer” for education. *American Economic Journal: Economic Policy.*

Caldés, N., Coady, D., & Maluccio, J. A. (2006). The cost of poverty alleviation transfer programs: A comparative analysis of three programs in Latin America. *World Development, 34,* 818-837.

de Brauw, A., & Hoddinott, J. (2011). Must conditional cash transfer programs be conditioned to be effective? The impact of conditioning transfers on school enrollment in Mexico. *Journal of Development Economics, 96*, 359–370.

de Hoop, J., & Rosati, F. C. (2014). Cash transfers and child labor. *World Bank Research Observer*, *29,* 202-234.

Edmonds, E. V., & Schady, N. (2012). Poverty alleviation and child labor. *American Economic Journal: Economic Policy, 4,* 100-124.

Fiszbein, A., & Schady, N. (2009). *Conditional cash transfers: Reducing present and future poverty*. Washington, DC: World Bank.

Gaarder, M. M., Glassman, A., & Todd, J. E. (2010). Conditional cash transfers and health: Unpacking the causal chain. *Journal of Development Effectiveness, 2,* 6-50.

Galiani, S., & McEwan, P. J. (2013). The heterogeneous impact of conditional cash transfers. *Journal of Public Economics, 103,* 85-96.

Glassman, A., Duran, D., Fleisher, L., Singer, D., Sturke, R., Angeles, G., Charles, J., Emrey, B., Gleason, J., Mwebsa, W., Saldana, K., Yarrow, K., & Koblinsky, M. (2013). Impact of conditional cash transfers on maternal and newborn health. *Journal of Health, Population, and Nutrition, 31,* 48-66.

Glewwe, P., & Olinto, P. (2004). Evaluating the impact of conditional cash transfers on schooling: An experimental analysis of Honduras' PRAF program. Unpublished manuscript, University of Minnesota and IFPRI-FCND. Downloaded Nov. 26, 2012 from http://pdf.usaid.gov/pdf_docs/PNADT588.pdf

Kabeer, N., Piza, C., & Taylor, L. (2012). *What are the economic impacts of conditional cash transfer programmes? A systematic review of the evidence.* Technical report. London: EPPI-Centre, Social Science Research Unit, Institute of Education, University of London.

Lagarde, M., Haines, A., & Palmer, N. (2007). Conditional cash transfers for improving uptake of health interventions in low- and middle-income countries. *JAMA, 298,* 1900-1910.

Lee, D. (2009). Training, wages, and sample selection: Estimating sharp bounds on treatment effects. *Review of Economic Studies, 76,* 1071-1102.

McEwan, P. J. (in press). Improving learning in primary schools of developing countries: A meta-analysis of randomized experiments. *Review of Educational Research.*

McEwan, P. J., Murphy-Graham, E., Torres Irribarra, D., Aguilar, C., & Rápalo, R. (in press). Improving middle school quality in poor countries: Evidence from the Honduran Sistema de Aprendizaje Tutorial. *Educational Evaluation and Policy Analysis.*

Moore, C. (2008). *Assessing Honduras' CCT programme PRAF, Programa de Asignación Familiar: Expected and unexpected realities.* Country Study No. 15. International Poverty Center.

Morris, S. S., Flores, R., Olinto, P., & Medina, J. M. (2004). Monetary incentives in primary health care and effects on use and coverage of preventive health care interventions in rural Honduras: cluster randomized trial. *Lancet, 364,* 2030–2037.

Owuso-Addo, E., & Cross, R. (2014). The impact of conditional cash transfers on child health in low- and middle-income countries: A systematic review. *International Journal of Public Health, 59,* 609-618.

Petrosino, A., Morgan, C., Fronius, T. A., Tanner-Smith, E. E., & Boruch, R. F. (2012). Interventions in developing nations for improving primary and secondary school enrollment of children: A systematic review. *Campbell Systematic Reviews, 19.*

Puma, M. J., Olsen, R. B., Bell, S. H., & Price, C. (2009). *What to do when data are missing in group randomized controlled trials* (NCEE 2009-0049). Washington, DC: National Center for Education Evaluation and Regional Assistance, Institute of Education Sciences, U.S. Department of Education.

Robertson, L., Mushati, P., Eaton, J. W., Dumba, L., Mavise, G., Makoni, J….Gregon, S. (2013). Effects of unconditional and conditional cash transfers on child health and development in Zimbabwe: a cluster-randomised trial. *Lancet, 381,* 1283-1292.

Saavedra, J. E., & Garcia, S. (2013). *Educational impacts and cost-effectiveness of conditional cash transfer programs in developing countries: A meta-analyis.* Paper No. 2013-007. Los Angeles: USC Center for Economic and Social Research.

Schady, N., & Araujo, M. C. (2008). Cash transfers, conditions, and school enrollment in Ecuador. *Economía*, *8,* 43-70.

Tarozzi, A., & Deaton, A. (2009). Using census and survey data to estimate poverty and in-equality in small areas. *The Review of Economics and Statistics, 91,* 773–792.

Table 1: Mean individual and household variables among children ages 0 to 17

	Children ages 0-17 in households responding at baseline survey		Same children in households responding at follow-up survey	
	Control group mean (s.d.)	Treatment-Control difference (s.e.)	Control group mean (s.d.)	Treatment-Control difference (s.e.)
Female (1/0)	0.488	-0.012	0.487	-0.011
	(0.500)	(0.010)	(0.500)	(0.011)
Age at baseline (years)	8.722	-0.026	8.719	0.007
	(5.095)	(0.102)	(5.085)	(0.108)
Mothers' schooling (years)	3.585	-0.003	3.544	-0.011
	(2.824)	(0.125)	(2.770)	(0.127)
Mother is literate (1/0)	0.730	-0.008	0.728	-0.010
	(0.444)	(0.020)	(0.445)	(0.021)
Fathers' schooling (years)	3.439	-0.012	3.440	-0.014
	(2.798)	(0.128)	(2.781)	(0.132)
Father is literate (1/0)	0.729	-0.005	0.731	-0.009
	(0.444)	(0.021)	(0.444)	(0.021)
Household size	6.459	-0.110	6.465	-0.074
	(2.525)	(0.130)	(2.501)	(0.131)
Number of children ages 0-5	1.088	-0.045	1.091	-0.045
	(1.026)	(0.046)	(1.031)	(0.047)
Number of children ages 6-18	2.738	-0.053	2.733	-0.023
	(1.622)	(0.078)	(1.614)	(0.076)
Adults in household who are Lenca (proportion)	0.053	0.000	0.050	0.001
	(0.216)	(0.019)	(0.209)	(0.018)
Number of rooms in dwelling	3.310	-0.053	3.276	-0.008
	(1.407)	(0.068)	(1.395)	(0.071)
Dwelling has bathroom or latrine (1/0)	0.753	0.015	0.752	0.020
	(0.432)	(0.027)	(0.432)	(0.027)
Dirt floor in dwelling (1/0)	0.354	0.009	0.354	0.010
	(0.478)	(0.028)	(0.478)	(0.029)
Piped water in dwelling (1/0)	0.179	-0.017	0.181	-0.017
	(0.383)	(0.019)	(0.385)	(0.019)
Electricity in dwelling (1/0)	0.661	-0.026	0.672	-0.035
	(0.473)	(0.039)	(0.469)	(0.040)
Landline or cell phone access (1/0)	0.847	0.018	0.849	0.016
	(0.360)	(0.020)	(0.358)	(0.020)
Dwelling only accessible by footpath (1/0)	0.299	0.023	0.293	0.023
	(0.458)	(0.031)	(0.455)	(0.030)

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

Table 2: Mean payments to treatment and control group households

	Treatment mean (s.d.)	Control mean (s.d.)	Treatment-Control difference (s.e.)
Before households' baseline interviews:			
Any payment (1/0)	0.576	0.057	0.519
	(0.494)	(0.232)	(0.038)
Number of payments	0.576	0.057	0.519
	(0.494)	(0.232)	(0.038)
Amount (Lempiras)	442.399	45.530	396.869
	(396.443)	(187.147)	(29.781)
Before households' endline interviews:			
Any payment (1/0)	0.905	0.071	0.834
	(0.293)	(0.256)	(0.019)
Number of payments	1.862	0.071	1.791
	(0.906)	(0.256)	(0.045)
Total amount (Lempiras)	6,924.739	90.672	6,834.067
	(4,214.863)	(482.875)	(167.397)

Notes: The number of households in the treatment (control) group is 2,087 (2,049). Standard errors are adjusted for clustering within aldeas; all mean differences are statistically different from zero at 1%.

Table 3: Effects on household consumption, poverty, and income

	Model specification (1)	(2)	(3)	N	Control group mean: Baseline	Endline	Lee bounds
Household consumption							
ln(consumption per capita)	0.089***	0.088***	0.093***	3839	6.83	6.79	0.053,0.118
	(0.033)	(0.033)	(0.027)				
ln(food consumption per capita)	0.090***	0.089***	0.098***	3835	6.45	6.40	0.050,0.122
	(0.031)	(0.031)	(0.028)				
ln(non-food consumption per capita)	0.105*	0.106*	0.061	3838	5.38	5.39	0.064,0.156
	(0.054)	(0.054)	(0.039)				
Food share (proportion)	0.002	0.002	0.008	3839	0.71	0.70	-0.002,0.011
	(0.007)	(0.007)	(0.007)				
Any school expenditure (1/0)	0.017***	0.017***	0.014**	3839	0.96	0.97	0.017,0.034
	(0.006)	(0.006)	(0.007)				
ln(school expenditures)	0.184***	0.183***	0.193***	3723	3.66	3.81	0.125,0.278
	(0.050)	(0.049)	(0.041)				
Any alcohol/tobacco expenditure (1/0)	-0.013	-0.014	-0.013	3839	0.08	0.07	-0.029,-0.012
	(0.009)	(0.009)	(0.010)				
ln(alcohol/tobacco expenditures)	-0.170	-0.174	-0.008	270	3.52	3.52	-0.514,0.137
	(0.167)	(0.164)	(0.170)				
Consumption-based poverty							
Poor (1/0)	-0.030**	-0.031**	-0.031**	3922	0.77	0.81	-0.031,-0.027
	(0.015)	(0.015)	(0.014)				
Poverty gap	-0.036***	-0.036***	-0.031***	3922	0.36	0.40	-0.038,-0.035
	(0.014)	(0.014)	(0.010)				
Poverty gap squared	-0.033***	-0.033***	-0.029***	3922	0.21	0.23	-0.035,-0.032
	(0.011)	(0.011)	(0.009)				
Extremely poor (1/0)	-0.027	-0.028	-0.018	3922	0.62	0.66	-0.029,-0.025
	(0.020)	(0.020)	(0.016)				
Extreme poverty gap	-0.037***	-0.037***	-0.032***	3922	0.25	0.28	-0.040,-0.036
	(0.013)	(0.013)	(0.010)				
Extreme poverty gap squared	-0.030***	-0.030***	-0.027***	3922	0.13	0.15	-0.033,-0.030
	(0.009)	(0.009)	(0.008)				
Household income							
ln(household income per capita)	0.124**	0.124**	0.182***	3737	6.41	6.64	0.079,0.171
	(0.055)	(0.054)	(0.043)				
ln(household labor income per capita)	-0.087	-0.087	-0.033	3720	6.31	6.39	-0.128,-0.033
	(0.063)	(0.063)	(0.053)				
Any non-labor income (1/0) (excluding CCT)	0.002	0.001	0.016	3839	0.47	0.43	-0.007,0.009
	(0.025)	(0.025)	(0.022)				
ln(household non-labor income per capita) (excluding CCT)	-0.003	-0.002	-0.020	1644	3.85	4.18	-0.084,0.086
	(0.109)	(0.101)	(0.093)				
Controls	N	Y	Y				
Lagged dependent variable	N	N	Y				

Notes: *** indicates statistical significance at 1%; ** at 5%; * at 10%. Standard errors in parentheses are adjusted for clustering within aldeas. 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.

Table 4: Effects on adult labor supply and labor income

	Model specification (1)	(2)	(3)	N	Control group mean Base-line	End-line	Lee bounds
Males ages 21-65 at baseline							
Worked ≥1 hour last week	0.003	-0.001	-0.006	3679	0.938	0.931	0.002,0.017
	(0.010)	(0.012)	(0.011)				
ln(labor income)	-0.041	0.013	0.034	3311	7.514	7.485	-0.099,0.044
	(0.068)	(0.065)	(0.057)				
Females ages 21-65 at baseline							
Worked ≥1 hour last week	-0.028	-0.002	-0.001	4128	0.375	0.357	-0.041,-0.021
	(0.020)	(0.020)	(0.018)				
ln(labor income)	-0.002	0.016	-0.021	1907	6.528	6.379	-0.087,0.055
	(0.095)	(0.101)	(0.090)				
Controls	N	Y	Y				
Lagged dependent variable	N	N	Y				

Notes: *** indicates statistical significance at 1%; ** at 5%; * at 10%. Standard errors in parentheses are adjusted for clustering within aldeas. 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.

Table 5: Effects on school enrollment at follow-up (sample: ages 6 to 17 at baseline; completed no more than 8th grade)

	Model specification			N	Control group mean at:	
	(1)	(2)	(3)		Base-line	End-line
Full sample						
Treated	0.038**	0.044***	0.044***	6573	0.76	0.69
	(0.016)	(0.013)	(0.012)			
Heterogeneity by enrollment in 2011						
Treated*Not enrolled	0.009	0.027	0.027	6012	0	0.22
	(0.026)	(0.021)	(0.021)			
Treated*Enrolled grades 1-3	0.020*	0.040***	0.040***		1	0.95
	(0.012)	(0.013)	(0.013)			
Treated*Enrolled grades 4-6	0.062**	0.066***	0.066***		1	0.65
	(0.028)	(0.024)	(0.024)			
Treated*Enrolled grades 7-8	0.044	0.056	0.056		1	0.77
	(0.040)	(0.039)	(0.039)			
p-value of F-test of joint equality	0.45	0.58	0.58			
Treated*Not enrolled	0.009	0.026	0.026	6566	0	0.22
	(0.026)	(0.021)	(0.021)			
Treated*Enrolled	0.042***	0.049***	0.049***		1	0.82
	(0.013)	(0.013)	(0.013)			
p-value of F-test of joint equality	0.21	0.28	0.28			
Heterogeneity by baseline poverty						
Treated*Not poor	0.049	0.021	0.011	6566	0.79	0.74
	(0.031)	(0.024)	(0.022)			
Treated*Poor	0.037**	0.049***	0.051***		0.75	0.67
	(0.017)	(0.014)	(0.013)			
p-value of F-test of joint equality	0.70	0.28	0.09			

Notes: *** indicates statistical significance at 1%; ** at 5%; * at 10%. Standard errors in parentheses are adjusted for clustering within aldeas. 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.

Table 6: Effects on paid or unpaid work in week prior to follow-up survey (sample: ages 6 to 17 at baseline; completed no more than 8th grade)

	Model specification			N	Control group mean at:	
	(1)	(2)	(3)		Base-line	End-line
Full sample						
Treated	-0.012	-0.009	0.000	6598	0.24	0.23
	(0.014)	(0.012)	(0.012)			
Heterogeneity by baseline enrollment						
Treated*Not enrolled	0.052*	0.046*	0.048**	6038	0.47	0.49
	(0.028)	(0.025)	(0.024)			
Treated*Enrolled grades 1-3	-0.017	-0.022	-0.013		0.11	0.08
	(0.014)	(0.015)	(0.014)			
Treated*Enrolled grades 4-6	-0.041*	-0.035*	-0.022		0.25	0.23
	(0.023)	(0.021)	(0.020)			
Treated*Enrolled grades 7-8	-0.039	-0.044	-0.022		0.27	0.28
	(0.044)	(0.041)	(0.040)			
p-value of F-test of joint equality	0.05	0.03	0.06			
Treated*Not enrolled	0.052*	0.046*	0.048**	6591	0.47	0.49
	(0.028)	(0.025)	(0.023)			
Treated*Enrolled	-0.028**	-0.026**	-0.015		0.17	0.15
	(0.013)	(0.013)	(0.012)			
p-value of F-test of joint equality	0.01	0.00	0.01			
Heterogeneity by baseline poverty						
Treated*Not poor	-0.031	-0.008	0.003	6591	0.22	0.21
	(0.028)	(0.025)	(0.023)			
Treated*Poor	-0.009	-0.010	-0.001		0.25	0.23
	(0.014)	(0.013)	(0.012)			
p-value of F-test of joint equality	0.44	0.93	0.88			

Notes: *** indicates statistical significance at 1%; ** at 5%; * at 10%. Standard errors in parentheses are adjusted for clustering within aldeas. 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.

Table 7: Effects on the use of health services by children and mothers

	Model specification			N	Control group mean		Lee bounds
	(1)	(2)	(3)		Base-line	End-line	
Children age 0 at baseline							
Weighed in 30 days before follow-up	0.164***	0.228***	0.228***	461	0.566	0.542	0.142,0.200
	(0.050)	(0.051)	(0.049)				
Children ages 0-3 at baseline							
Reason for last visit to health center was a checkup (1/0)	0.041**	0.030*	0.030*	1999	0.079	0.094	0.010,0.045
	(0.018)	(0.017)	(0.017)				
Complete immunizations (1/0)	0.027	-0.005	0.008	2189	0.535	0.771	0.020,0.053
	(0.023)	(0.023)	(0.018)				
Women 12-49 (pre- and post-natal)							
Number of prenatal checkups during last or current pregnancy	0.326	0.240	0.219	729	4.228	4.845	0.129,0.508
	(0.202)	(0.237)	(0.232)				
Woman received tetanus shot prior to or during last/current pregnancy (1/0)	0.052*	0.056	0.051	696	0.716	0.812	0.032,0.057
	(0.031)	(0.039)	(0.038)				
Woman received advice about birth plan (1/0)	0.030	0.027	0.024	692	0.521	0.548	0.010,0.048
	(0.042)	(0.052)	(0.051)				
Woman received postnatal checkup in 10 days after birth (1/0)	0.061	0.031	0.031	563	0.539	0.560	0.037,0.081
	(0.049)	(0.057)	(0.057)				
Controls	N	Y	Y				
Lagged dependent variable	N	N	Y				

Notes: *** indicates statistical significance at 1%; ** at 5%; * at 10%. Standard errors in parentheses are adjusted for clustering within aldeas. 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. The prenatal sample includes women who were pregnant at baseline or follow-up, and women who gave birth between January 2012 and July 2013. The postnatal sample includes women pregnant at baseline, and women who gave birth between January 2012 and July 2013.

Table 8: Effects on child health and nutrition (ages 0 to 3 at baseline)

	Model specification			N	Control group mean		Lee bounds
	(1)	(2)	(3)		Base-line	End-line	
Child had diarrhea in 2 weeks prior to survey (1/0)	-0.019	0.004	0.006	2000	0.225	0.150	-0.051,-0.013
	(0.019)	(0.022)	(0.021)				
Child had respiratory problem in 2 weeks prior to survey (1/0)	-0.019	0.002	0.001	2004	0.508	0.527	-0.036,-0.001
	(0.027)	(0.029)	(0.029)				
Height-for-age z-score <-2 (1/0)	-0.008	-0.013	-0.023	1830	0.300	0.273	-0.041,0.004
	(0.028)	(0.028)	(0.023)				
Weight-for-height z-score <-2 (1/0)	0.007	0.014**	0.013**	1824	0.053	0.016	-0.012,0.008
	(0.006)	(0.006)	(0.006)				
Weight-for-age z- score <-2 (1/0)	0.007	0.030*	0.024	1880	0.116	0.079	-0.033,0.010
	(0.015)	(0.018)	(0.015)				
Anemic: hemoglobin <11 (1/0)	0.010	0.029	0.028	1791	0.449	0.413	-0.018,0.029
	(0.029)	(0.028)	(0.028)				
Controls	N	Y	Y				
Lagged dependent variable	N	N	Y				

Notes: *** indicates statistical significance at 1%; ** at 5%; * at 10%. Standard errors in parentheses are adjusted for clustering within aldeas. 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.

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
Panel A: Enrolled at follow-up						
Treatment*(1 child 6-18)	0.083***	0.063*	0.013	0.077**	0.049	0.086**
	(0.028)	(0.035)	(0.068)	(0.039)	(0.064)	(0.038)
Treatment*(2 children 6-18)	0.063**	0.052**	-0.007	0.067***	0.026	0.083***
	(0.025)	(0.024)	(0.049)	(0.025)	(0.048)	(0.023)
Treatment*(3 children 6-18)	-0.015	0.014	0.032	0.014	-0.037	0.035
	(0.027)	(0.024)	(0.055)	(0.026)	(0.038)	(0.025)
Treatment*(4 children 6-18)	0.042	0.032	0.036	0.023	-0.047	0.020
	(0.034)	(0.031)	(0.096)	(0.034)	(0.039)	(0.034)
p-values of F-tests:						
Jointly equal	0.03	0.53	0.95	0.30	0.39	0.29
Jointly equal to 0	0.00	0.13	0.97	0.05	0.48	0.00
N	5,514	5,514	1,019	4,488	1,047	3,437
Panel B: Worked ≥1 hour						
Treatment*(1 child 6-18)	-0.060**	-0.063*	-0.071	-0.063	0.017	-0.079**
	(0.025)	(0.034)	(0.057)	(0.039)	(0.117)	(0.037)
Treatment*(2 children 6-18)	-0.037	-0.032	-0.015	-0.035	0.055	-0.063**
	(0.023)	(0.023)	(0.046)	(0.025)	(0.060)	(0.026)
Treatment*(3 children 6-18)	0.026	0.013	0.011	0.009	0.143***	-0.031
	(0.022)	(0.020)	(0.058)	(0.022)	(0.051)	(0.024)
Treatment*(4 children 6-18)	-0.005	0.010	-0.056	0.022	0.055	0.017
	(0.029)	(0.032)	(0.111)	(0.034)	(0.063)	(0.039)
p-values of F-tests:						
Jointly equal	0.05	0.17	0.75	0.24	0.55	0.25
Jointly equal to 0	0.05	0.25	0.77	0.33	0.05	0.03
N	5,534	5,534	1,024	4,503	1,064	3,435
Dummies for # of children 6-18	Y	Y	Y	Y	Y	Y
Dummies for # of h.h. members	N	Y	Y	Y	Y	Y
Interactions with h.h. size	N	Y	Y	Y	Y	Y
Dummies for child age	N	Y	Y	Y	Y	Y
Interactions with child age	N	Y	Y	Y	Y	Y

Notes: *** indicates statistical significance at 1%; ** at 5%; * at 10%. Standard errors in parentheses are adjusted for clustering within aldeas. All specifications include a constant. Sample includes children aged 6 to 18 at baseline with fewer than 8 grades of schooling, living in household with 1 to 4 children ages 6 to 18 at baseline. See text for details on specifications that include interactions with household size and age.

Table 10: Heterogeneous effects (by family structure) on the use of health services by children

	Full Sample		Not poor at baseline	Poor at baseline
Panel A: Weighed in 30 days before follow-up				
Treatment*(zero children 6-18)	0.173*	0.084	-0.161	0.123
	(0.089)	(0.097)	(0.196)	(0.109)
Treatment*(1 child 6-18)	0.083	0.090	0.026	0.085
	(0.088)	(0.092)	(0.277)	(0.102)
p-values of F-tests:				
Jointly equal	0.47	0.96	0.58	0.80
Jointly equal to 0	0.10	0.38	0.71	0.36
N	241	241	53	188
Panel B: Reason for last visit to health center was checkup				
Treatment*(zero children 6-18)	0.123***	0.073**	0.099*	0.070**
	(0.032)	(0.030)	(0.053)	(0.035)
Treatment*(1 child 6-18)	0.029	0.041	0.036	0.049
	(0.029)	(0.030)	(0.044)	(0.037)
p-values of F-tests:				
Jointly equal	0.02	0.43	0.37	0.66
Jointly equal to 0	0.00	0.03	0.12	0.08
N	1,015	1,015	256	759
Panel C: Complete immunizations				
Treatment*(zero children 6-18)	0.055	0.050	0.193***	-0.007
	(0.036)	(0.045)	(0.069)	(0.053)
Treatment*(1 child 6-18)	0.019	0.015	-0.097	0.046
	(0.038)	(0.038)	(0.077)	(0.042)
p-values of F-tests:				
Jointly equal	0.47	0.52	0.00	0.40
Jointly equal to 0	0.29	0.53	0.01	0.53
N	1,110	1,110	276	824
Dummy for # of children 6-18	Y	Y	Y	Y
Dummies for # of h.h. members	N	Y	Y	Y
Interactions with h.h. size	N	Y	Y	Y
Dummies for child age	N	Y	Y	Y
Interactions with child age	N	Y	Y	Y

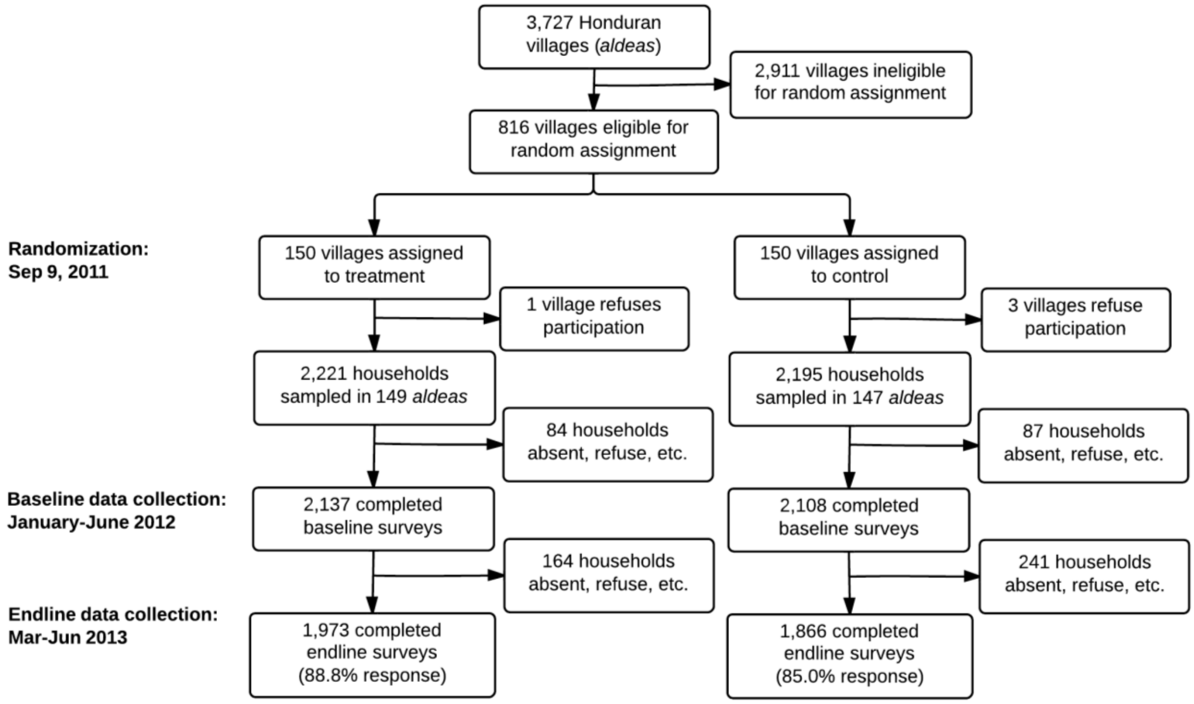
Notes: *** indicates statistical significance at 1%; ** at 5%; * at 10%. Standard errors in parentheses are adjusted for clustering within aldeas. All specifications include a constant. The sample in panel A includes children aged 0 at baseline, living in households with 0 to 1 children ages 6 to 18 at baseline. The sample in panels B and C includes children aged 0 to 3 at baseline, living in households with 0 to 1 children ages 6 to 18 at baseline. See text for details on specifications that include interactions with household size and age.

Table 11: Heterogeneous effects (by family structure) on the use of health services by mothers

	Full Sample		Not poor	Poor
Panel A: Number of prenatal checkups during last or current pregnancy				
Treatment*(zero children 6-18)	0.343	0.269	-0.116	0.429
	(0.424)	(0.472)	(0.904)	(0.607)
Treatment*(1 child 6-18)	0.361	0.431	-0.114	0.317
	(0.385)	(0.416)	(0.795)	(0.516)
p-values of F-tests:				
Coefficients jointly equal	0.98	0.79	1.00	0.89
Coefficients jointly equal to 0	0.48	0.51	0.98	0.64
N	361	361	109	251
Panel B: Woman received tetanus shot prior to or during last/current pregnancy				
Treatment*(zero children 6-18)	0.119**	0.144**	-0.008	0.199**
	(0.059)	(0.059)	(0.107)	(0.082)
Treatment*(1 child 6-18)	0.066	0.072	0.091	0.091
	(0.065)	(0.073)	(0.155)	(0.102)
p-values of F-tests:				
Coefficients jointly equal	0.53	0.44	0.59	0.46
Coefficients jointly equal to 0	0.10	0.03	0.84	0.02
N	344	344	106	237
Panel C: Woman received advice about birth plan				
Treatment*(zero children 6-18)	0.118	0.146	0.127	0.134
	(0.088)	(0.096)	(0.165)	(0.114)
Treatment*(1 child 6-18)	-0.071	-0.030	0.062	-0.082
	(0.074)	(0.081)	(0.187)	(0.096)
p-values of F-tests:				
Coefficients jointly equal	0.09	0.16	0.78	0.15
Coefficients jointly equal to 0	0.23	0.29	0.72	0.35
N	343	343	106	236
Panel D: Woman received postnatal checkup in 10 days after birth				
Treatment*(zero children 6-18)	0.236***	0.212**	0.182	0.254*
	(0.088)	(0.101)	(0.230)	(0.133)
Treatment*(1 child 6-18)	0.094	0.118	0.018	0.070
	(0.085)	(0.094)	(0.175)	(0.109)
p-values of F-tests:				
Coefficients jointly equal	0.23	0.48	0.55	0.31
Coefficients jointly equal to 0	0.02	0.06	0.73	0.11
N	273	273	115	190
Dummy for # of children 6-18	Y	Y	Y	Y
Dummies for # of h.h. members	N	Y	Y	Y
Interactions with h.h. size	N	Y	Y	Y
Dummies for child age	N	Y	Y	Y
Interactions with child age	N	Y	Y	Y

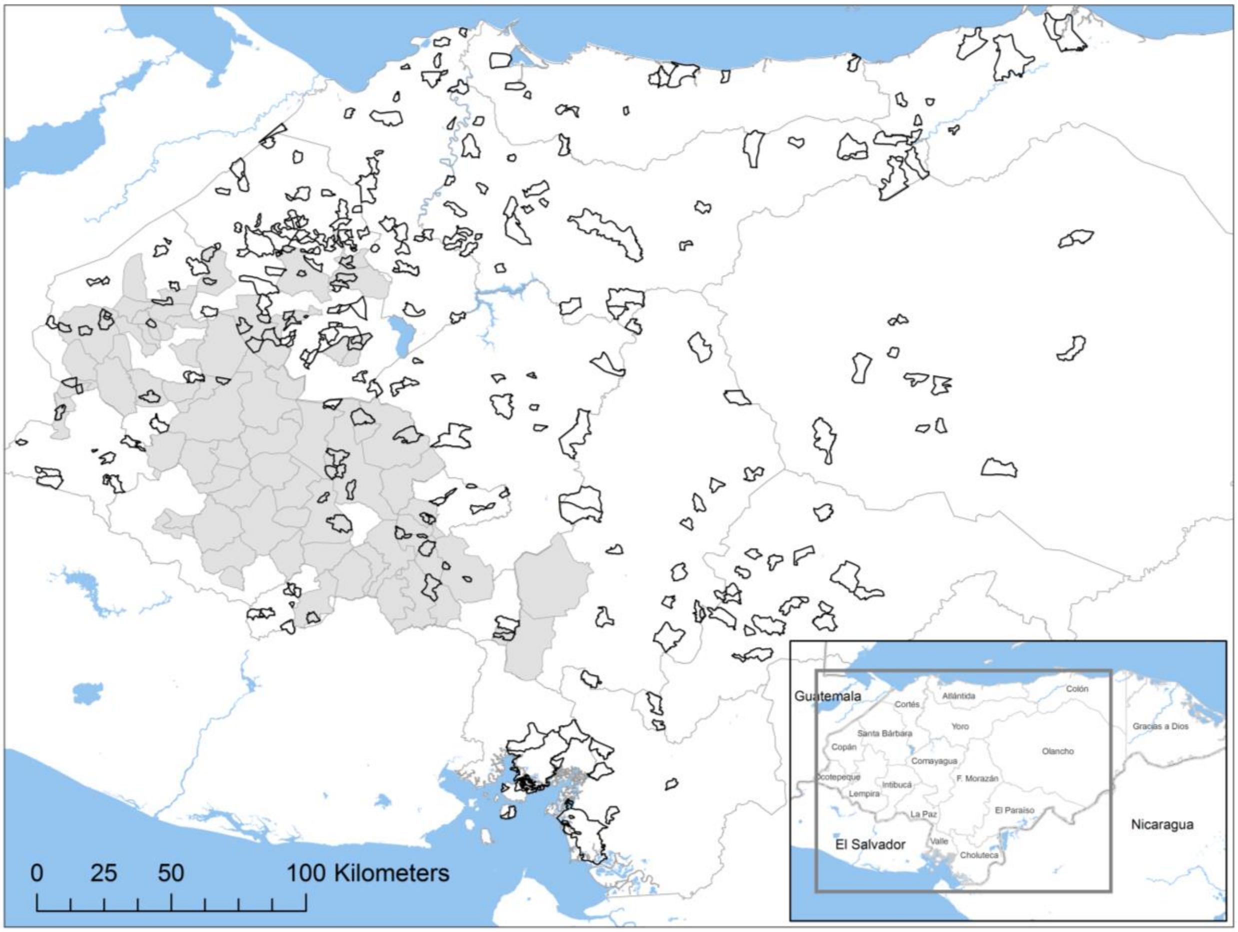
Notes: *** indicates statistical significance at 1%; ** at 5%; * at 10%. Standard errors in parentheses are adjusted for clustering within aldeas. All specifications include a constant. The sample in panels A-C includes women ages 12-49 at baseline who were pregnant at baseline or follow-up, and women who gave birth between January 2012 and July 2013 (if the household contains 0-1 children 6-18 at baseline). The sample in panel D includes women pregnant at baseline, and women who gave birth between January 2012 and July 2013 (if the households includes 0-1 children 6-18 at baseline).

Figure 1: Flow diagram of the Bono 10,000 experiment



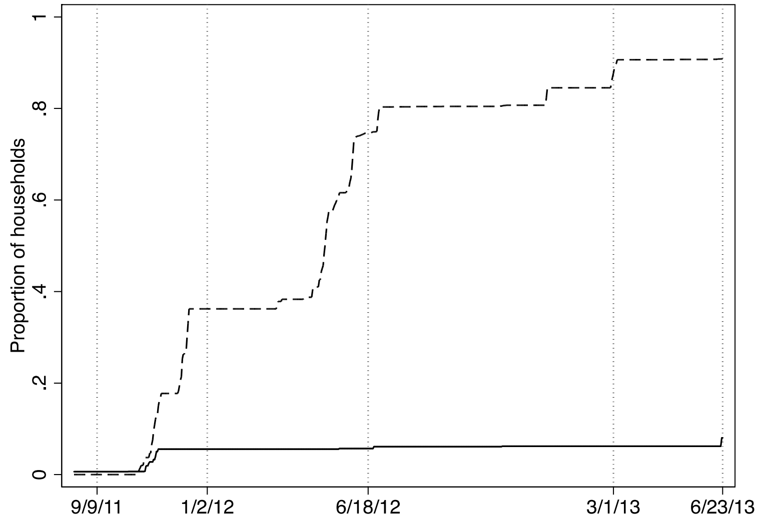
Notes: Follow-up response rates are calculated using the number of sampled households. If one further imputes 15 households for each non-responding village, then response rates are 88.2% and 83.3% for treatment and control groups, respectively.

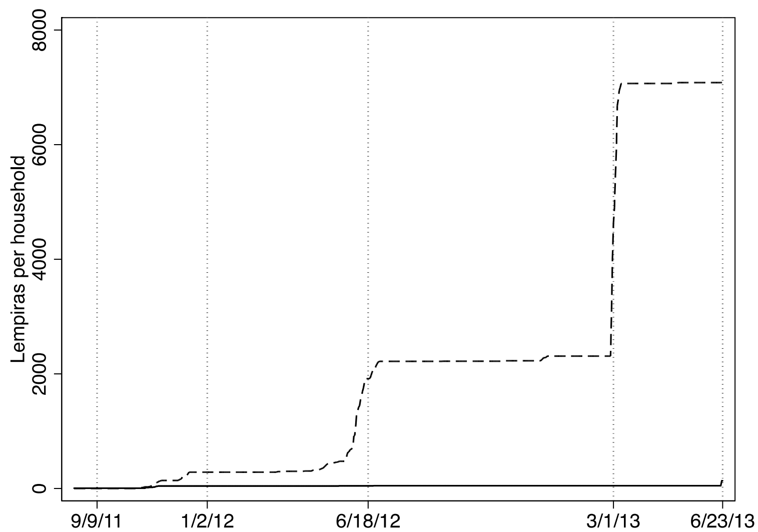
Figure 2: Sampled territories in the PRAF-II and Bono 10,000 experiments



Notes: Shaded areas indicate 70 municipalities sampled in the PRAF-II experiment in 2000 (Galiani & McEwan, 2013). Outlined areas indicate 300 villages (*aldeas*) sampled in the Bono 10,000 experiment in 2011.

Figure 3: Cumulative payments to treatment and control group households





Notes: Randomization occurred on September 9, 2011. Baseline data collection occurred between January 1, 2012 and June 18, 2012. Follow-up data collection occurred between March 1, 2013 and June 23, 2013. In the top panel, the dashed (solid) lines indicate the proportion of households that had received *any* payment by that date in the treatment (control) group. In the bottom panel, the lines indicates the average total payments received by households by that date.

Table A1: Descriptive statistics from 2001 census for children ages 0 to 17

	Municipalities in the PRAF-II experiment: mean (s.d.)	Villages (*aldeas*)			
		Ineligible for experiment: mean (s.d.)	Eligible for experiment: mean (s.d.)	Control group: mean (s.d.)	Treatment - control difference (s.e.)
Female (1/0)	0.489	0.493	0.486	0.486	-0.002
Age at baseline (years)	7.868	8.182	8.102	8.129	-0.000
	(5.051)	(5.081)	(5.002)	(5.014)	(0.057)
Lenca (1/0)	0.319	0.049	0.059	0.051	0.002
	(0.466)	(0.216)	(0.235)	(0.219)	(0.020)
Mothers' schooling (years)	2.320	4.248	2.727	2.777	-0.096
	(2.803)	(4.100)	(2.777)	(2.831)	(0.135)
Mother is literate (1/0)	0.575	0.743	0.648	0.647	-0.009
Fathers' schooling (years)	2.513	4.284	2.677	2.781	-0.271**
	(2.842)	(4.330)	(2.853)	(2.937)	(0.126)
Father is literate (1/0)	0.638	0.750	0.655	0.663	-0.032**
Enrolled in school (1/0)	0.547	0.657	0.587	0.584	0.008
Works outside home (1/0)	0.196	0.128	0.152	0.148	0.009
Selected departments:					
Comayagua (1/0)	0.173	0.047	0.061	0.074	-0.026
Intibucá (1/0)	0.212	0.026	0.070	0.078	-0.008
La Paz (1/0)	0.135	0.022	0.054	0.049	-0.003
Lempira (1/0)	0.294	0.050	0.008	0.002	-0.002
Santa Bárbara (1/0)	0.105	0.038	0.172	0.152	0.050
Predicted poverty	0.888	0.674	0.836	0.825	0.020
	(0.145)	(0.292)	(0.169)	(0.178)	(0.016)
Maximum N of children	356,106	2,589,206	394,417	79,786	
Number of municipalities	70	298	199	89	
Number of villages (*aldeas*)	704	2,906	816	150	

Notes: PRAF-II indicates aldeas that participated in PRAF-II experiment (2000-2002). Ineligible aldeas were not eligible for randomization in Bono 10000 experiment. Eligible aldeas were eligible for randomization. Treatment and control groups were selected from eligible aldeas. S.E. of difference are adjusted for aldea-level clustering. See text for details on calculation of predicted poverty.

Table A2: Descriptive statistics, by family structure

	Sample: children ages 6-18 at baseline who have not completed 9th grade				Sample: children ages 0-3 at baseline		Sample: women 12-49, prenatal	
	Number of children ages 6-18 in h.h				Number of children ages 6-18 in h.h		Number of children ages 6-18 in h.h	
	1	2	3	4	0	1	0	1
Poor at baseline (1/0)	0.713	0.798	0.827	0.880	0.725	0.783	0.674	0.725
Female (1/0)	0.479	0.480	0.459	0.479	0.480	0.492	1.000	1.000
Age at baseline (years)	10.330	11.070	11.320	11.550	1.467	1.429	23.320	24.450
	(3.46)	(3.19)	(3.14)	(3.17)	(1.09)	(1.10)	(4.04)	(6.05)
Mothers’ schooling (years)	3.950	3.787	3.304	2.986	5.241	4.248	5.466	4.693
	(3.02)	(2.81)	(2.63)	(2.42)	(3.20)	(2.92)	(3.13)	(2.95)
Mother is literate (1/0)	0.761	0.752	0.708	0.658	0.856	0.794	0.864	0.866
Fathers’ schooling (years)	3.571	3.433	3.348	2.924	4.732	3.897	4.638	3.951
	(2.96)	(2.77)	(2.99)	(2.51)	(3.04)	(2.99)	(3.02)	(2.91)
Father is literate (1/0)	0.729	0.734	0.715	0.672	0.818	0.781	0.812	0.788
Household size	4.213	5.160	6.275	7.346	3.870	5.097	3.503	4.463
	(1.41)	(1.48)	(1.52)	(1.73)	(1.16)	(1.79)	(1.17)	(1.54)
Number of children ages 0-5	0.778	0.693	0.730	0.753	1.548	1.576	1.184	1.000
	(0.81)	(0.84)	(0.88)	(0.92)	(0.65)	(0.77)	(0.67)	(0.83)
Adults in household who are Lenca (proportion)	0.029	0.037	0.048	0.045	0.034	0.046	0.025	0.031
	(0.16)	(0.18)	(0.21)	(0.20)	(0.18)	(0.20)	(0.15)	(0.17)
Number of rooms in dwelling	3.178	3.303	3.299	3.306	3.040	3.145	2.860	3.302
	(1.32)	(1.32)	(1.30)	(1.47)	(1.44)	(1.42)	(1.48)	(1.41)
Dwelling has bathroom or latrine (1/0)	0.770	0.787	0.761	0.783	0.713	0.761	0.687	0.741
Dirt floor in dwelling (1/0)	0.340	0.331	0.352	0.342	0.380	0.362	0.402	0.341
Piped water in dwelling (1/0)	0.196	0.188	0.168	0.148	0.168	0.167	0.179	0.210
Electricity in dwelling (1/0)	0.684	0.695	0.647	0.631	0.666	0.674	0.698	0.649
Landline or cell phone access (1/0)	0.853	0.848	0.880	0.852	0.807	0.825	0.804	0.868
Dwelling only accessible by footpath (1/0)	0.276	0.269	0.295	0.339	0.319	0.284	0.274	0.239
Maximum N	987	1,973	1,938	1,490	602	599	179	205

Notes: The prenatal sample includes women pregnant at baseline, and women who gave birth between January 2012 and July 2013 (if the households include 0-1 children 6-18 at baseline).