The heterogeneous impact of conditional cash transfers*

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Abstract: The Honduran PRAF experiment randomly assigned conditional cash transfers to 40 of 70 poor municipalities. Using census data, the paper shows that eligible children were more likely to enroll in school and less likely to work. Consistent with theory, effects were largest in the poorest experimental strata. Heterogeneity confirms the importance of judicious targeting to maximize the impact and cost-effectiveness of CCTs. Results from two regression-discontinuity designs are consistent with experimental results, and illustrate the common caveat that a local average treatment effect at an assignment discontinuity can be a misleading guide to the average treatment effect.

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1. Introduction

Conditional cash transfers (CCTs) have been extensively adopted in the last decade, especially in Latin America (Adato and Hoddinott, 2011; Schady and Fiszbein, 2009). The programs provide cash transfers to finance current consumption, but their receipt is conditional on behaviors such as regular school attendance or use of primary health services. Given the mounting evidence suggesting that households are constrained in their knowledge of the best course of action, social programs that encourage them to pursue desirable actions are potentially welfare enhancing (Banerjee and Duflo, 2011).

Experimental evaluations of CCTs in Mexico, Nicaragua, and Ecuador suggest that poor, school-aged children eligible for a CCT are more likely to enroll in school and to complete more grades.[1] This paper reanalyzes a Honduran experiment—conducted between 2000 and 2002—that showed statistically significant effects of 1-3 percentage points on education enrollment among primary-aged children eligible for a conditional cash transfer (Glewwe and Olinto, 2004). The *Programa de*

[1] Behrman and Parker (2010) review the effects of Latin American CCTs on education outcomes, while Fiszbein and Schady (2009) review broader CCT impacts on consumption and poverty, participation in education and health investments, and child and adult labor supply. The Progresa/Oportunidades experiment in Mexico showed short-run enrollment effects of less than one percentage point among primary children—with primary enrollment rates already exceeding 90%—but 6-9 percentage points among secondary school children (Schultz, 2004; Behrman et al., 2005; Skoufias, 2005). Almost six years after the treatment, older children exposed to the education transfers gained 0.7-1 more grades in school, but with no effects on achievement tests (Behrman et al., 2010, 2011). A Nicaraguan experiment found enrollment effects of 13 percentage points on primary-aged children after two years of exposure to treatment, with accompanying gains in attendance and grade advancement (Maluccio and Flores, 2005; Maluccio et al., 2010). In Ecuador, the *Bono de Desarrollo Humano* was randomly assigned to a treatment group of poor families, although administrative issues led nearly 42% of the control group to receive transfers (Schady and Araujo, 2008). Intention-to-treat estimates show that random assignment to the treatment group increased enrollment by 3 percentage points, and the instrumental variables estimates showed effects of 10 percentage points.

Asignación Familiar (PRAF) implemented two cash transfers to families: (1) an education transfer, in the amount of US$50-60 year, for children ages 6 to 12 who enrolled in and regularly attended grades 1 to 4, and (2) a health transfer of US$40-50 year for young children and pregnant mothers who regularly attended health centers. The original evaluation identified 70 of 298 municipalities with the lowest mean height-for-age z-scores, an available proxy of municipal poverty (IFPRI, 2000). The 70 municipalities were divided into 5 quintiles based on the mean height-for-age, and 8 of 14 municipalities in each stratum were randomly selected to receive the transfers.[2]

This paper uses the 2001 Honduran Census, rather than the official evaluation sample (Glewwe and Olinto, 2004; Morris et al., 2004). The census was applied in all 298 municipalities, about 8 months after the first of three transfers were distributed in late 2000 and just weeks after the second round of transfers (República de Honduras, 2002). We find that the Honduran CCT increases the enrollment of eligible children by 8 percentage points, a 12% increase over the control group enrollment rate. We also show that it reduces the supply of child labor outside the home by 3 percentage points (or 30%), and in-home child labor by 4 percentage points (or 29%). There is no evidence that full-sample treatment effects are biased, given balance across treatment and control groups in a range of observed individual and household variables not affected by the treatment.

Our paper makes several contributions to the CCT literature, facilitated by the large census samples not available in earlier evaluations. First, we exploit the stratified design to estimate treatment effects separately by experimental strata. The estimated effects on enrollment in the two poorest (or malnourished) strata are 18 and 10 percentage points, respectively. The effects on child labor supply outside the home are 8 and 5 percentage points and, on labor inside the home, 6 and 6

[2] Some municipalities were also assigned to receive direct investments in schools and health centers, but these were not implemented during the time of the official evaluation (Moore, 2008).

percentage points, respectively. Depending on the stratum, these represent percentage increases of 16-32% in enrollment, and decreases of 50-55% in work outside the home, and 38-46% in work inside the home. Strikingly, the effects in three richer (but still poor) strata are statistically indistinguishable from zero. We note that our main findings of heterogeneity are based on a feature of the original stratified design, addressing common concerns about data mining in subgroup analysis (Deaton, 2010).

An appendix to this paper shows that heterogeneity by a municipal income proxy is a prediction of a simple model of household schooling choice. Enrollment decisions of families are more sensitive to a fixed conditional transfer if one makes the uncontroversial assumption that utility is concave with respect to household consumption. The design of CCT experiments precludes careful tests of competing theoretical explanations of heterogeneity, but existing theory and mounting empirical evidence both reinforce the importance of considering heterogeneity during policy design.[3] In particular, CCTs should be adequately targeted in order to maximize their impact and cost-effectiveness. The results further highlight the relevance of carefully choosing proxy indicators to identify the poor (Coady et al., 1994; Alatas et al., 2010; and De Wacther and Galiani, 2006).

The paper's second contribution is to highlight the importance of considering treatment effect heterogeneity when designing impact evaluations, especially variants of the regression-discontinuity design (RDD). We do so by estimating treatment effects using two RDDs that exploit alternate control groups within the census data. The first RDD uses the original municipal-level targeting rule that identified 70 experimental municipalities (of 298) to participate on the basis of their low mean height-for-age. We show that this rule introduced sharp increases in the probability of treatment for children residing in municipalities close to the height-

[3] Treatment effect heterogeneity by income has been shown elsewhere in the CCT literature See Schady and Fiszbein (2009) for an overview, as well as Maluccio and Flores (2005), Filmer and Schady (2008), and Oosterbeek et al. (2008).

for-age cutoff. However, there are no consistent differences in outcomes between eligible children residing in municipalities on either side of the cutoff.

On the one hand, it confirms the robustness of the zero experimental effects measured among the "richest" stratum. On the other hand, it concretely illustrates a common caveat of RDDs: that local average treatment effects for subjects in the vicinity of assignment cutoffs may not accurately gauge average treatment effects among all subjects receiving the treatment.[4] It suggests that RDDs are less appropriate in circumstances in which theory or evidence imply treatment effect heterogeneity by the values of common assignment variables. In education and social program evaluation—often assigned using a proxy of poverty or academic achievement—the caveat seems especially germane.[5] A natural alternative to is to employ an assignment variable that does not plausibly interact with treatment effects. But these cases seem rare in the applied literature, if only because many RDDs are "recovered" from the explicit attempts of program designers to equitably and efficiently target social programs according to perceived need or potential benefits (Chay, McEwan, and Urquiola, 2005).

We analyze a second RDD that exploits discontinuities in treatment status created by municipal borders separating eligible children in treated villages from untreated children residing in a nearby village (but who do not reside in the control

[4] Oosterbeek et al. (2008) provide a similar illustration of this point in Ecuador, showing that large experimental enrollment effects are not observed when a discontinuity design is applied to a less poor sample. In higher education, a growing literature evaluates remedial programs for college students, assigned on the basis of low achievement (Butcher, McEwan, and Taylor, 2010; Martorell and McFarlin, 2011; Calcagno and Long, 2008). These RDDs show zero or very small effects on student outcomes, and all authors highlight that local average treatment effects are potentially misleading.

[5] Randomized experiments of education interventions often report larger treatment effects among poorer or lower-achieving subsamples (Schanzenbach, 2007; Banerjee et al., 2007).

municipalities of the original experiment).[6] It is the first such design, to our knowledge, that has been analyzed alongside a randomized experiment. It replicates the general pattern of results in the experimental sample, including positive effects on enrollment and negative effects on work outside the home in the two poorest strata, and zero effects in less-poor strata. The estimated enrollment effects are, however, smaller than in the experimental sample, perhaps because some of the poorest municipalities were necessarily excluded from the border-discontinuity sample (because their borders were fully circumscribed by other experimental municipalities). A falsification test confirms that there is no "effect" on eligible children who reside in one of the original experimental control municipalities, as compared with children in bordering municipalities that were not in the experimental sample.

As a third contribution, the paper finds no consistent evidence that children who are ineligible for education transfers (by virtue of having completed fourth grade) are affected by the municipal-level treatment, regardless of whether an eligible child lives in the same household. Modest effects are observed in just the poorest stratum, but this could easily be attributed to lax enforcement of grade-completion requirements for eligibility. The finding stands in contrast to the relatively large spillovers documented by Bobonis and Finan (2008) in the Progresa/Oportunidades experiment. We also find no evidence that CCTs affected adult female labor supply. A modest impact on adult male labor supply is confined to the richest stratum and is not replicated by the rule-based discontinuity design.

Finally, and not least, the paper provides a rare opportunity to replicate the results of a randomized experiment using a new source of data. Like this paper, Glewwe and Olinto (2004) find positive effects on child enrollment, although their point estimates are substantially smaller—1-3 percentage points vs. 8 percentage points in this paper—and they find no effects on child labor supply. As noted by the

[6] See, e.g., Black (1999), Cattaneo et al. (2009), and Dell (2010).

authors, a challenge in the original evaluation is that baseline data (but not follow-up data) were collected at different points in time for treated and untreated municipalities. Since the school year concluded during the staggered data collection, it introduced mechanical differences in baseline child enrollment and labor supply. We will suggest that commonly reported difference-in-differences estimates are biased towards zero. Even so, simple mean differences in child outcomes at follow-up are unbiased and consistent, and those estimates are comparable to our full-sample estimates, despite the use of different datasets.

Section 2 of the paper provides background on PRAF-II, the random assignment of treatments, and prior evaluation results. Section 3 describes features of the 2001 census data, while section 4 elaborates empirical strategy, including a straightforward experimental analysis and the two discontinuity designs. Section 5 describes the empirical results, and section 6 concludes.

2. PRAF in Honduras

A. Background

The *Programa de Asignación Familiar* (PRAF), or Family Allowances Program, started in the early 1990s. Its first phase, PRAF-I, distributed cash subsidies to families, including a *Bono Escolar* available to children in early primary school grades, and a *Bono Materno Infantil* available to pregnant mothers and families with young children. Subsidies were supposedly conditioned on regular school attendance and health center visits, and PRAF-I beneficiaries were identified by local civil servants, including teachers and health center employees. In practice, PRAF-I appears to have rarely enforced conditionalities, and the poverty targeting mechanism was applied haphazardly with substantial leakage to higher-income families (Moore, 2008). No credible impact evaluations were conducted.

In response to these shortcomings, PRAF-II was launched in the late 1990s with support from the Inter-American Development Bank (IDB).[7] It aspired to improve on PRAF-I in several ways, including: (1) improved enforcement of conditionalities for subsidy distribution; (2) a renewed emphasis on direct investments in schools and health centers alongside the distribution of cash subsidies; (3) an improved poverty targeting mechanism; and (4) a randomized evaluation design embedded within the project roll-out (IFPRI, 2000; Glewwe and Olinto, 2004; Morris et al., 2004).

B. PRAF-II Treatments

PRAF-II implemented two kinds of cash transfers to families. The education transfer, in the amount of 812 Lempiras/year (US$50-60), was available to children ages 6 to 12 who enrolled in and regularly attended grades 1-4 between the school year of March to November.[8] Children were not eligible if they had completed fourth grade, and up to 3 children per family were eligible to receive the transfer. A health transfer of 644 Lempiras/year (US$40-50) was available to children under 3 and pregnant mothers who regularly attended health centers. Families were eligible to receive up to 2 health transfers. In the first year of implementation, transfers were distributed on three occasions: late 2000, May-June 2001, and October 2001 (Morris et al., 2004). In practice, education enrollment (but not attendance) was enforced as a conditionality, while no health beneficiaries were suspended for failure to attend health centers (Morris et al., 2004).

PRAF-II planned to implement two kinds of direct interventions in education and health. The education interventions consisted of payments of approximately US$4,000 per year, depending on school size, to parent associations in primary

[7] For further background on PRAF and its variants, see Moore (2008) and IDB loan documents (BID, 2004).
[8] Our description of the treatments relies on Caldés et al. (2006). Other sources report quite similar but not identical amounts for the demand-side transfers (Glewwe and Olinto, 2004; IFPRI, 2000; BID, 2004; Morris et al., 2004).

schools (Glewwe and Olinto, 2004). The payments were conditioned on obtaining legal status and preparing a quality-improvement plan. The health interventions consisted of payments of approximately \$6,000 per year to local health centers, depending on the client base (Glewwe and Olinto, 2004). The health payments were conditioned on the formation of a health team (with members of the community and health personnel) and the preparation of a budget and proposal.

In fact, the distribution of education and health funds was extremely limited (Glewwe and Olinto, 2004; Moore, 2008). After two years of treatment, by late 2002, only 7% of the education funds were disbursed and 17% of health funds, and the formation of parent and community groups authorized to administer funds still faced legal hurdles (Moore, 2008). Based on her interviews, Moore (2008) concludes that "gauging the impact of the supply side incentives was virtually impossible, and only the impact of the demand side incentives could be correctly evaluated" (Moore, 2008, p. 14).

C. Experimental Sample and Random Assignment

The original evaluation design defined three treatment groups and one control group, henceforth referred to as G1, G2, G3, and G4.[9] G1 would receive demand-side transfers in education and health. G2 would receive transfers in addition to direct investments in education and health centers, while G3 would receive only direct investments. G4 would receive no PRAF-II interventions.

The unit of assignment was the Honduran municipality. To identify the sample of municipalities subject to random assignment, IFPRI (2000) ordered 298 municipalities from lowest to highest values of the mean height-for-age z-score of first-graders, obtained from the 1997 Height Census of First-Graders (Secretaría de Educación, 1997). Eligible municipalities had z-scores -2.304 or lower. Of 73 eligible municipalities, 3 were excluded because of distance and cost considerations,

[9] See IFPRI (2000), Glewwe and Olinto (2004), and Morris et al. (2004).

yielding a final sample of 70 municipalities, identified as the unshaded municipalities in Figure 1, panel A. The geographic concentration of child stunting produced a sample dominated by western Honduras.

IFPRI divided the 70 into five quintiles of 14 municipalities each, based on mean height-for-age. A stratified random assignment occurred on October 13, 1999 during a public event (IFPRI, 2000). Within each quintile, 4 municipalities were randomly assigned to G1, 4 to G2, 2 to G3, and 4 to G4. The final sample consisted of 20 municipalities in G1, 20 in G2, 10 in G3, and 20 in G4 (see Figure 1). The treatments in G1, G2, and G3 were to begin in late 2000 and proceed for two years. However, there is strong evidence that direct investments in G2 and G3, unlike cash transfers, were minimally implemented by the end of two years.

D. Prior Evaluations

IFPRI and associates collected baseline data in the 70 municipalities between mid-August and mid-December 2000, with a single follow-up survey in mid-May to mid-August 2002 (Glewwe and Olinto, 2004; Morris et al., 2004). The sample consisted of 5,748 households with 30,588 members. G1and G2 were surveyed from August to October, while G3 and G4 were surveyed from November to December (Glewwe and Olinto, 2004). The school year ends and agriculture work increases as the calendar year ends, perhaps introducing positive (negative) baseline differences in school enrollment (child labor) between G1-G2 and G3-G4. The follow-up data collection in 2002 was not staggered across treatment and control groups.

Glewwe and Olinto (2004) report statistically significant, difference-in-difference estimates on one-year enrollment outcomes of 0.8 percentage points (G1) and 2.1 percentage points (G2), each relative to G4.[10] The staggered baseline data collection suggests that these effects may be attenuated, as compared with simple

[10] See Table 14. The one-year results are based on retrospective data. Two-year difference-in-differences estimates are 2.6 and 0.7 percentage points in G1 and G2, respectively.

cross-sectional differences at follow-up. Indeed, the cross-sectional estimates after the first year are larger: 7.4 and 7.5 percentage points in G1 and G2, respectively, which will prove to be similar to our full-sample estimates. The effects of G3 relative to G4 are statistically insignificant. The authors find no statistically significant effects on child labor force participation.

Morris et al. (2004) analyze health outcomes, reporting that overall randomization appears to have produced baseline comparability across G1 to G4 in variables that are insensitive to the timing of the baseline survey, such as mother's literacy and child immunization rates.[11] The authors find statistically significant effects of G1 and G2 (relative to G4) on frequency of antenatal care, recent health center check-ups and growth monitoring, although measles and tetanus toxoid immunization were not affected. There were no impacts on any outcomes of G3 relative to G4.

3. Data

The 2001 Honduran Census was applied between July 28, 2001 and August 4, 2001 in all 298 municipalities (República de Honduras, 2002). This occurred approximately 8 months into the first year of treatment, after 2 of 3 transfer payments had occurred in G1 and G2. This paper uses the individual and household data, merged to municipal-level data on treatment group and strata membership. The census presents several advantages, compared with the earlier data: (1) the large samples allow for a more extensive consideration of heterogeneous treatment effects

[11] Two papers reanalyze the original data using additional dependent variables. Stecklov et al. (2007) report no statistically significant baseline differences between pooled samples of G1-G2 and G3-G4, including parental schooling and age, family size, and per-capita expenditures. The authors find that treatments in G1-G2 (relative to G3-G4) produced large increases in births or pregnancy in the past year (measured in 2002). They attribute this to the per-capita health transfer for pregnant women and young children. Alzúa et al. (2010) find no effects of PRAF-II on measures of adult labor supply.

than prior evaluations; (2) it contains large samples of children eligible for transfers as well as ineligible children, allowing us to test for spillover effects; and (3) the availability of national data facilitates the application of two regression-discontinuity designs using alternate control groups.

Table 1 reports descriptive statistics on the dependent and independent variables, while Table A1 provides full variable definitions. In all columns, the sample is limited to children eligible for the education transfer (ages 6 to 12 with incomplete fourth grade). The main dependent variables are (1) a dummy variable indicating current enrollment in any school, (2) a dummy variable indicating any labor force participation outside the home during the past week (where labor force participation includes paid or unpaid work in a business or farm), and (3) a dummy variable indicating that the individual worked *exclusively* inside the home on chores (thus reflecting a lower bound on actual rates of in-home labor).[12]

Independent variables include common individual variables such as age and gender, in addition to a dummy variable indicating self-identification as indigenous (*Lenca*).[13] Household variables include parent education and literacy, household structure, dwelling quality, service availability, and presence of costly assets like autos and computers. The first columns of Table 1 confirm that eligible children in the 70 experimental municipalities are relatively more disadvantaged than the national sample of eligible children. They are more likely to be indigenous; their parents have lower levels of schooling, literacy, and wealth; and they live in lower-quality dwellings.

The final columns of Table 1 compare variable means across G1, G2, G3, and G4. For each independent variable, we fail to reject the null hypothesis that means

[12] This restriction is imposed by the flow of the census questionnaire.
[13] Unlike Guatemala and other countries in Central and South America, this does not imply monolingual or bilingual status in any indigenous language.

are jointly equal across the four groups.[14] In contrast, the proportions of eligible children who are enrolled in school or work suggest higher enrollment and reduced work—both inside and outside the home—in G1 and G2, relative to G3 and G4. We reject the null hypothesis that the means are jointly equal at the 5 percent significance level. The enrollment results are broadly consistent with the cross-sectional results in Glewwe and Olinto (2004), but the child labor participation results are different. The next section describes an empirical framework to assess whether these findings are robust.

4. Empirical Strategy

A. Randomized Experiment

Given randomized assignment, the empirical strategy is straightforward. The initial specification is:

(1) $$O_{ijk} = \beta_0 + \beta_1 G1_{jk} + \beta_2 G2_{jk} + \beta_3 G3_{jk} + \delta_k + \varepsilon_{ijk}$$

where *O* is the binary school or labor outcome of child *i* in municipality *j* in experimental block (or stratum) *k*. The regression conditions on the treatment status dummy variables (G1, G2, and G3) (relative to the excluded control group, G4), and controls also for block dummy variables (δ_k). Henceforth, we refer to the quintile with the lowest mean height-for-age z-scores as Block 1, up to Block 5. We estimate the regression by ordinary least squares, clustering standard errors by municipality.

We estimate several variants of equation (1). First, we include individual and household controls to improve precision and further assess whether random assignment produced balance across treatment and control groups. Second, we estimate a simpler and ultimately preferred version of the regression:

(2) $$O_{ijk} = \beta_0 + \beta_1 D_{jk} + \delta_k + \varepsilon_{ijk}$$

[14] We regress each independent variable on dummy variables indicating G1, G2, and G3 (and 4 out of 5 strata dummies), and cluster standard errors at the level of municipality. The p-value is from a F-test of the null that coefficients on G1, G2, and G3 are jointly zero.

where *D* indicates children in the G1 or G2 experimental groups, relative to the pooled control group of G3 or G4. This decision rests on two sources of evidence. First, there is evidence from observers that the direct investments of G2 and G3 were not implemented, especially in the first half of 2001 school year and even by the end of IFPRI's two-year evaluation (Moore, 2008). Second, we test two null hypotheses that $\beta_1 = \beta_2$ and $\beta_2 = \beta_3$. Ultimately we fail to reject the former, and reject the latter. Moreover, like prior evaluations, we always report small and statistically insignificant estimates of β_3.

Subsequent specifications examine heterogeneity by: (1) interacting *D* with five experimental block dummy variables, to assess whether treatment effects vary by mean height-for-age; (2) interacting *D* with child-level variables indicating age, gender, and ethnicity, in the full sample and within subsamples defined by blocks. We also assess whether the effect on eligible children is, firstly, smaller when 4 or more eligible children reside within a household (recalling that administrative rules supposedly precluded more than 3 transfers per household) and, secondly, is smaller when there are no children ages 0-3 in the household (in a partial effort to assess whether children 6-12 are affected by health transfers to younger children).

Finally, we estimate equation (2) in two subsamples. First, we report estimates within the subsample of *ineligible* children, ages 6-12, which have completed fourth grade. This allows us to test for spillover effects of transfers. Using Mexico's Progresa data, Bobonis and Finan (2009) found that ineligible children's enrollment was responsive to the presence of treated children. Second, we estimate regressions using labor outcomes within subsamples of male and female adults, to assess whether there is an adult labor supply response to transfers. The literature on conditional cash transfers generally finds no evidence of adult labor supply responses (Fiszbein and Schady, 2009), although a Nicaraguan experiment found that men (and not women) reduced weekly hours worked by 6 (Maluccio and Flores, 2005).

B. Regression Discontinuity Using the Original Targeting Rule

The census data facilitate the application of two regression-discontinuity designs using alternate control groups. IFPRI chose the initial experimental sample by ordering 298 municipalities from lowest mean height-for-age z-score to highest. This variable, henceforth referred to as *HAZ,* is the municipal-level assignment variable in a regression-discontinuity design. Define a dummy variable $E_{ijk} = 1\{HAZ_{jk} \leq -2.304\}$, indicating individuals residing in 73 municipalities initially *eligible* for random assignment (among 298 nationally). Three municipalities were non-randomly excluded from random assignment because of distance and cost concerns. The random assignment further removed 30 municipalities (G3 and G4). Even so, individuals residing in municipalities with a *HAZ* just below -2.304 should have sharply higher probabilities of residing in a municipality with PRAF-II transfers, implying a fuzzy regression discontinuity design (Lee and Lemieux, 2010).

We restrict the sample to eligible children (ages 6-12 with incomplete fourth grade) residing in municipalities where $-h \leq (HAZ_{jk} + 2.304) < h.$ The bandwidth *h* specifies the size of the data window near the cutoff. We estimate the following first-stage regression:

(3) $$D_{ijk} = \alpha_0 + \alpha_1 E_{ijk} + f(HAZ_{jk}) + \nu_{ijk}$$

where the dummy variable *D* indicates children in G1 or G2 (relative to all who are not) and $f(HAZ_{jk})$ is a continuous function specified as a piecewise linear spline: $f(HAZ_{jk}) = \gamma_0 \times (HAZ_{jk} + 2.304) + \gamma_1 \times (HAZ_{jk} + 2.304) \times E_{ijk}$.

The parameter α_1 represents the increase in probability of treatment at the assignment cutoff. The reduced-form effect of eligibility on outcomes is estimated with:

(4) $$O_{ijk} = \beta_0 + \beta_1 E_{ijk} + f(HAZ_{jk}) + \varepsilon_{ijk}.$$

β_1/α_1, usually estimated via two-stage least squares, is the local average treatment effect among children in municipalities that were induced to be treated by virtue of falling just below the cutoff.

This would be straightforward to implement but for a practical complication: HAZ_{jk} is only observed for the 70 experimental municipalities. The 1997 height census is available in printed format for all 298 municipalities, but the document records only three municipal variables: (1) the proportion of children in a municipality with z-scores less than -3, (2) the proportion with z-scores between -3 and -2, and (3) the number of surveyed first-graders (Secretaría de Educación, 1997). To estimate HAZ_{jk} using these data, we regress the right-censored HAZ_{jk} on the two observed proportions and the interaction term, weighting by the number of surveyed first-graders.[15] We calculated a predicted value, $\widehat{HAZ}$, for 298 municipalities. In the sample of 70 experimental municipalities, $corr\left(HAZ, \widehat{HAZ}\right) = 0.96$.

We then replace HAZ with $\widehat{HAZ}$ in the prior equations. Given the introduction of additional noise in the value of the assignment variable, we anticipate that α_1 will be attenuated. However, it should still identify sharp and plausibly exogenous variation in the probability of being treated in G1 or G2. We further verify this by assessing whether baseline covariates, such as mother's schooling, do not vary sharply in the vicinity of the cutoff (Lee and Lemieux, 2010).

C. Regression Discontinuity Using Municipal Borders

Municipalities assigned to a treatment or control group frequently share borders with municipalities not in the experimental sample (see Figure 1, panel B). Indeed, households in close proximity—and perhaps similar in other regards, such as land quality and public services—may nonetheless have differential access to conditional cash transfers. The municipal boundaries create a sharp, multi-dimensional discontinuity in longitude-latitude space (Dell, 2010).

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[15] We use an interval regression estimator (Wooldridge, 2010, p. 783). Unobserved values of HAZ_{jk} were mostly right-censored at -2.304. However, three municipalities (the original "fuzzy" municipalities excluded for distance and cost considerations) were known to fall within the interval of -2.3862 and -2.3678, given the availability of the experimental municipalities' original rankings in our dataset.

Municipalities are subdivided into *aldeas* (villages) and *caseríos* (clusters of rural households, or hamlets). The latter are identified as points in geographic data.[16] We identify *caseríos* within a narrow band of all borders shared by experimental and non-experimental municipalities. Figure 1 (panel B) illustrates *caseríos* within 2 kilometers of borders, and eligible children within these *caseríos* constitute the border sample. We estimate the following regression:

$$O_{icb} = \beta_0 + \beta_1 D_{icb} + f(geographic\ location_{cb}) + \delta_b + \varepsilon_{ijk} \quad (5)$$

where the outcome of child *i* residing in *caserío c* near municipal border segment *b* is regressed on D_{icb}, an indicator that children reside in a G1 or G2 municipality.

The regression includes dummy variables, δ_b, indicating 33 municipal border segments. Children are assigned to a segment if they live in one of 33 municipalities assigned to G1 or G2, or if they live across its border in a non-experimental municipality. Seven of 40 municipalities in G1 and G2 are not included in the sample because their borders are circumscribed by other experimental municipalities; hence, there is no possibility of identifying a nearby control group. While a narrow bandwidth and border segement fixed effects may be sufficient for identification, the specification further includes a function of the *caserío's* geographic location. In a single-dimensional regression-discontinuity design, this would simply be distance from the border. Dell (2010) argues that a multi-dimensional RD should include a flexible function of longitude and latitude.[17] We report variants of both specifications.

As a falsification check, we assess whether eligible children in G3 and G4 municipalities have similar outcomes to nearby children in bordering non-experimental municipalities. To implement this, we re-estimate equation (5) in a sample of children whose *caseríos* are close to the borders of 22 control

[16] Geographic analyses use ArcGIS files obtained from the Infotecnología unit of the Ministry of Education.
[17] We include a quadratic in latitude and longitude.

municipalities in G3 or G4 (while excluding all G1 or G2 municipalities). We anticipate that the "effect" of residing in a G3 or G4 municipality should be zero.

Several features of the border-discontinuity design suggest that it will provide a conservative estimate of program effects, relative to the experimental sample. First, 7 of 40 municipalities in G1 or G2 contribute no observations to the sample. They are disproportionately (but not entirely) drawn from the poorer experimental blocks 1 and 2.[18] To the extent that treatment effects are larger in such municipalities, a full-sample estimate provides a conservative check on the robustness of experimental estimates (although we estimate effects separately by blocks 1-2 and blocks 3-5).

Second, it is plausible that untreated families in close proximity to a border would attempt to obtain transfers for their children by misrepresenting their residence. Although administrative checks were in place to prevent such instances, it would likely bias effects towards zero, to the extent that the census records such families in their original municipality. Third, the close proximity of treated and untreated households suggests a greater potential for spillover effects that could bias estimated towards zero, in the spirit of Miguel and Kremer (2004).

5. Results

A. Experimental Results

Table 2 describes the main experimental results. In panel A, column (1) shows that eligible students in the G1 and G2 experimental groups are, respectively, 10.1 and 7.4 percentage points more likely to attend school, relative to G4. The coefficient on G3 is small and statistically insignificant. Controlling for a full set of baseline variables in column 2 does not change the basic pattern of results: demand-side transfers increase enrollments by 7-8.3 percentage points, and direct investments appear to have no impact. In column (2), one cannot reject the null hypothesis that

[18] Recall that the stratified randomization assigned 8 municipalities to G1 or G2, in each of 5 experimental blocks. In the border sample, the poorest block 1 includes 5 such municipalities. Blocks 2 to 5 include, respectively, 6, 8, 7, and 7 municipalities.

the coefficients on G1 and G2 are equal, but one can reject the null, at 6%, that the coefficients on G2 and G3 are equal. Collectively, the evidence does not suggest that putative investments in G2 or G3 affected school enrollments.

Thus, regressions in panel B control for a single dummy variable *D* indicating that the observation belongs to one of the experimental groups G1 or G2. Conditional on baseline covariates, the enrollment of eligible children living in G1 or G2 increases by 8 percentage points. Given the improved precision, we henceforth focus on specifications that include a full set of controls. Columns (4) and (6) provide similar evidence for binary indicators of child labor supply (the sample sizes are smaller because the census excluded 6 year-olds from work-related questions). Overall, eligible children in treated municipalities are 3 percentage points less likely to work outside the home and 4 percentage points less likely to work exclusively on household chores inside the home.

The full-sample estimates are large. Consider that the percent of eligible children attending school in the groups G3 and G4 is 65%, the percent working outside the home is 10%, and the percent working inside the home is 14%.[19] Thus, in the full sample of eligible children, the cash transfer increases enrollment by approximately 12%, reduces work outside the home by 30%, and reduces work inside the home by 29%.

B. Heterogeneity by Experimental Block

Other research has found that treatment effects of a fixed CCT are larger among relatively poorer families (see, e.g., Maluccio and Flores, 2005, and Oosterbeek et al., 2008). A theoretical appendix to this paper shows that this result is a prediction of a model of household schooling choice. Suppose that heterogeneous households

[19] Appendix Table A2 reports means in the pooled sample of eligible children in G3 and G4, also dividing by experimental blocks. Henceforth we use these percentages to report effects as percent changes. It would obviously be more desirable to have a true baseline percentage.

are endowed with a child height-for-age (the poverty proxy used in PRAF-II), and that households' exogenous incomes are increasing in this endowment. This reflects the stylized fact that child height-for-age—a proxy for early nutritional deprivation—and incomes tend to be positively correlated (if not causally related). Household consumption is then determined by a family's choice between child schooling or work. Schooling incurs costs, in the form of foregone child wages, perhaps compensated by a positive conditional transfer. Families choose school or work to maximize utility that is a function of consumption, as well as the additional utility derived from sending children to school (interpreted as a "return").

Assuming a concave utility function with respect to consumption, the model predicts that the expected impact of offering a conditional cash transfer is higher among lower-income families—that is, among families with a lower child height-for-age. The intuition is that households with higher income have a smaller marginal utility of consumption, given their expected return from education. Thus, the transfer will have a smaller impact on their schooling decision.

Figure 2 presents visual evidence that the size of effects varies with *HAZ*, used to define experimental blocks 1 to 5. The panels graph fitted values of local linear regressions (bandwidth=0.3, rectangular kernel) that regress each dependent variable on *HAZ*. The dashed line reports fitted values from the pooled sample of eligible children in G1 and G2, and the solid line from children in G3 and G4. Vertical dotted lines indicate values that separate the blocks 1 to 5 (while the right-most line, at -2.304, indicates the eventual cutoff value for the rule-based regression-discontinuity design). The figure shows a pattern of larger treatment-control differences at lower values of *HAZ*, particularly in blocks 1 and 2.

Returning to Table 2, panel C reports regressions in which *D* is interacted with five block dummy variables. Focusing on columns that include a full set of controls, the results confirm that enrollment effects are larger in poorer blocks (17.8 and 10.4 percentage points in blocks 1 and 2, respectively), and smaller and statistically insignificant in blocks 3-5. One can reject the null hypothesis (at 10%) that effects are equal across blocks.

A similar pattern is observed for child work. In blocks 1 and 2, the rate of child work outside the home falls by 7.9 and 5 percentage points, respectively. We reject the null hypothesis that effects are equal across blocks at the 10% percent significance level. The rate of child work inside the home falls by 6.3 and 5.8 percentage points (although the null of coefficient equality cannot be rejected). The pattern of results is substantively similar in Panel D, where blocks 1-2 and 3-5 are analyzed as 2 groups rather than 5.

Overall, the results imply that PRAF-II's modest annual transfers of US$50-60 per child had very large effects in the poorest Honduran municipalities, both in increasing schooling and reducing child labor. In blocks 1 and 2, the point estimates imply 16-32% increases in enrollment, 50-55% decreases in work outside the home, and 38-46% decreases in work inside the home (Table A2 reports "baseline" values from G3-G4). The effects were not observed in relatively richer, though absolutely poor areas.

C. Ineligible Children and Adults

Table 3 limits the sample to children ages 6-12 who are ineligible for education transfers by virtue of already having completed fourth-grade. The sample contains no children ages 6-8 and less than 5% are 9 year-olds. To assess whether spillover effects occur within families or through another mechanism, we identify ineligible children who reside in households: (1) with no other children eligible for a health or education transfers; (2) with at least 1 child eligible for an education transfer; and (3) with at least one child eligible for a health or education transfer.

For all dependent variables, the full-sample estimates in odd columns show no evidence of spillover effects on ineligible children. The coefficients are small and statistically insignificant. There is some evidence that enrollment increases (panel A) and work outside the home declines (panel B) among ineligible children in block 1. The magnitude of the enrollment effect is about one-third the size of the effect in the sample of eligible children, and comparable or somewhat smaller for child labor. The relative stability of this effect across samples suggests that it is not driven by the presence other eligible children in the household. Beyond spillover effects, a plausible explanation is that program administrators subjectively loosened grade-related eligibility requirements for age-eligible children in the very poorest municipalities. Whatever the explanation, it is fair to conclude that evidence on spillovers is less compelling than evidence from the Progresa/Oportunidades experiment (Bobonis and Finan, 2009; Angelucci et al. 2010).

Table 4 reports estimates of labor supply regressions among male and female adults, dividing samples by the presence or absence of eligible children in the household. In the full sample, the only statistically significant findings reveal an increase of less than one percentage point, among males, in the probability of working only in the home. It is stable across samples, even when there are no children in the household eligible for health or education transfers.

When divided by block, there is little consistent evidence that labor supply of females or males is affected by the treatment. The most notable finding is that, among males in block 5, there is a decrease of about 5 percentage points in work outside the home when an eligible child is present in the home. We return to this finding in the rule-based discontinuity design, since the local average treatment effect in the vicinity of the cutoff should be informative about the magnitude of the average treatment effect in block 5.

D. Regression Discontinuity Using the Original Targeting Rule

A rule-based discontinuity design identifies effects in the vicinity of the cutoff that is also the right-hand bound of block 5. The experimental results suggest that the effect in the RDD will be zero. Figure 3 provides visual evidence on this point.

In each panel, the lines are fitted values from local linear regressions of the y-axis variable on $\widehat{HAZ}$ (re-centered such that 0 is the cutoff rather than -2.304). The y-axis variable in the upper-left panel is *D*, a visual analogue to equation (3). It suggests that an eligible child's probability of residing in a treated municipality increases sharply at the cutoff by perhaps 0.2. It is notably fuzzy because of (1) random assignment of municipalities to the treatment conditional on falling below the cutoff, and (2) the use of a noisier assignment variable $\widehat{HAZ}$. The upper-right panel suggests that the cutoff still provides credibly exogenous variation in *D*, since there is no visual evidence of a break in mother's schooling (nor is there in other background variables, not reported here).

The bottom panels are the visual reduced-form, representing equation (4). There is no evidence of a sharp change in enrollment. There is a small *increase* in work outside the home, but it remains to be seen whether this is robust to empirical specifications and precisely estimated. (A similar result, not shown here, holds for work only in the home.) Both panels illustrate a telltale reversal of the slope on either side of the cutoff, broadly consistent with experimental findings reported in Figure 2. Collectively, the panels suggest that the apparent absence of effects in the "richest" blocks is robust to alternate control groups.

Table 5 (panel A) reports first-stage estimates of equation (3) in three subsamples that apply progressively wider bandwidths. The point estimates confirm that the probability of treatment increased by 0.25-0.32, with the results insensitive to the inclusion of a full set of background controls. Only one estimate is significant at 5%, using the largest bandwidth and including controls. Panel B shows no evidence of statistically significant breaks in mother's schooling, consistent with the figure (and similar results hold for other background variables). Panels C-D generally show small point estimates that are stable to the inclusion of control variables. Finally, in results not reported here, we repeated all analyses for the adult labor supply outcomes. The reduced-form results showed no significant effects on female outcomes. Among males, the negative effect on labor supply in block 5 was not replicated; in fact, the small point estimates were of the opposite sign, small, and statistically significant at 5%.

The data provide no evidence to overturn our general understanding of program effects developed in the experimental analysis. The section also provides a concrete illustration of the frequent caveat accompanying discontinuity designs: that a local average treatment effect may not replicate the average treatment effect among all subjects treated by virtue of falling below (or above) a cutoff.[20] This is particularly true when theory predicts heterogeneity by the assignment variable, as in this paper's appendix.

E. Regression Discontinuity Using Municipal Borders

Table 6 reports results from the border discontinuity design. In panel A, B, and C, we use the sample of eligible children in *caseríos* on either side of the borders of 33 treated municipalities in G1 and G2. Columns (1) to (4) use a smaller sample within 2 kilometers of the border (as in Figure 1, panel B), while the final columns widen this bandwidth to 4 kilometers. Each regression controls for a function of geographic location—either a single variable measuring distance to the border ("Dist") or a quadratic in latitude and longitude ("Lat/Lon").[21]

[20] Oosterbeek et al. (2008) report a similar findings in Ecuador, with positive and significant enrollment effects in a poor, experimental sample, and statistically insignificant effects in a less-poor sample with a discontinuity design. Analyzing Progresa data, Buddelmeyer and Skoufias (2004) find inconsistent results. Using the fact that eligibility was determined by a proxy means test within localities, they estimated discontinuity effects local to these cutoffs. In an earlier round of data, these were zero or smaller than experimental estimates among the (poor) experimental sample. In a later round of follow-up data, the experimental and discontinuity effects were more comparable.

[21] We experimented with alternate functional forms, including higher-order polynomials, and the results did not appreciably change, likely because the narrow bandwidths already ensure a high degree of comparability across bordering *caseríos*.

In panel A, the variable *D*—an indicator of residence in G1 or G2—is interacted with dummy variables indicating block 1-2 or block 3-5. In this case, we assign untreated children to the block of their neighboring (and treated) municipality. While smaller than comparable point estimates from Table 2, the estimates replicate the existence of appreciable, statistically significant enrollment effects in blocks 1-2, but not blocks 3-5. The estimates are more precise when background controls are included, but the estimates are stable. In panel B, the results for work outside the home are similarly robust, with point estimates implying a 7-9 percentage points reduction in blocks 1-2, and no effect in blocks 3-5. In contrast, panel C fails to replicate the pattern of finding for work inside the home, although point estimates in blocks 1-2 are consistently negative.

Finally, panels D, E, and F conduct a falsification test among children in *caseríos* bordering municipalities in G3 and G4. To the extent that the discontinuity strategy is internally valid, these coefficients should not be statistically distinguishable from zero. That is always the case for school enrollment and work outside the home, and only one coefficient is significant for work inside the home.

F. Heterogeneity by Child and Household Characteristics

Returning to the experimental sample, Table 7 examines heterogeneity by age, gender, and ethnicity. In panels A-C, the variable *D* is interacted with dummy variables for each categories of an attribute, and regressions are estimated separately in the full sample, blocks 1-2, and blocks 3-5. The results support three main conclusions.[22] First, the absence of full-sample effects on child enrollment and labor supply in the "richer" blocks 3-5 is robust, even when treatment effects are allowed to vary by age, gender, and ethnicity. Second, the magnitude of enrollment effects is largest among younger children, while the reductions on work outside the home are

[22] In results not reported here, we replicated these specifications in the border discontinuity samples. With the exception of work in the home, the results largely replicate the pattern of findings in the experimental analysis.

largest among older children. Third, enrollment effects are similar by gender; however, boys drive the full-sample effect on work outside the home and girls drive the full-sample effects on work inside the home. Fourth, there is little consistent evidence that ethnicity plays a role in mediating treatment effects.

The final panels of Table 7 examine heterogeneity by two household attributes. According to program rules, no more than 3 education transfers are awarded to each household, even if the presence of 4 or more eligible children. We do not directly observe each child's participation, but individual effects should be attenuated in larger households since children have a reduced likelihood of receiving a transfer. Panel D suggests that is the case for enrollment. In blocks 1-2, for example, the effect is 12 percentage points for eligible children in household with 4 or more eligible children, versus 15 percentage points in households with fewer 1-3 eligible children (p-value=0.01). There is no strong evidence of a similar difference for child labor variables.

Panel E assesses whether the effects on children eligible for the education transfers are partly attributable to health transfers received on behalf of children ages 0-3 (recalling that a families were eligible to receive a maximum of 2). In the full sample, there is evidence that enrollment effects are less positive among eligible children in families without very young children (7 percentage points versus 8.6; p-value=0.05). Child labor effects are somewhat less negative when families have no young children. Overall, the magnitudes do not suggest that results among older children are entirely driven by a younger child's transfer.

6. Conclusions

This paper reanalyzes the Honduran PRAF-II experiment, using the 2001 census instead of the official evaluation sample. PRAF-II awarded cash transfers, conditional on school enrollment, to children ages 6-12 who had not completed fourth grade. Cash transfers were available in 40 randomly-chosen municipalities in an experimental sample of 70 poor municipalities. The 70 municipalities (of 298 total) were chosen because their mean height-for-age z-score of first-graders fell below a cutoff value of -2.304. In the full sample of eligible children, we find that residing in a treated municipality increased school enrollment by 8 percentage points, decreased work outside the home by 3 percentage points, and decreased work exclusively inside the home by 4 percentage points.

The full-sample results can be usefully compared to a randomized evaluation of Nicaragua's *Red de Protección Social,* also conducted during 2000-2002 (Maluccio and Flores, 2005). That program offered relatively more generous cash transfers that amounted to 27% of per capita household expenditures versus 9% in Honduras (Fiszbein and Schady, 2009). Eligibility for education transfers was similar (i.e., primary-aged children who had not completed fourth grade), and the baseline enrollment level of eligible children was similarly low (72%, compared with 65% in our data). Between 2000 and 2001, the program increased enrollment by 18.5 percentage points (26%) in the full evaluation sample, just over twice as larger as the Honduran estimates.[23] Taken together, the Nicaraguan and Honduran results demonstrate that CCTs are more effective than previously thought at increasing primary enrollments, especially when there is ample scope for doing so in extremely poor settings.

We also find substantial heterogeneity by the stratifying variable of mean municipal height-for-age, with full-sample effects mainly accounted for by municipalities in the 2 poorest (of 5) experimental strata. In these strata, enrollment increased by 10-18 percentage points, work outside the home decreased by 5-8 percentage points, and work inside the home decreased by 6 percentage points. We find little consistent evidence of spillovers to ineligible children and impacts on adult labor supply. Two regression-discontinuity designs, using alternate control groups, generally confirm the robustness of the findings. The rule-based discontinuity

[23] See Maluccio and Flores (2005), Table 4.8.

highlights the pitfalls of relying on local average treatment effects when theory and prior evidence imply heterogeneous responses to a treatment, depending on values of the assignment variable.

The heterogeneous results point to the importance of adequate targeting in order to maximize the impact and cost-effectiveness of CCTs. Caldés et al. (2006) report cost estimates for PRAF-II, suggesting a total administrative program cost of US$3,430,330 from 1999 to 2001 (excluding costs of the randomized evaluation and transfer payments). The 2001 census shows that 77,500 children were eligible for education transfers (6-12 year-olds with incomplete fourth grade in G1 and G2), implying a cost per child of US$44. Part of these costs covered administrative costs of delivering health transfers. Since there are 58,692 children eligible for health transfers (0-3 year-olds in G1 and G2), we proportionately adjust downward the program cost per child eligible for education transfers to US$25. Given full sample effects on enrollment of 8 percentage points (12%) and block 1 results of 18 percentage points (32%), the results suggest cost-effectiveness ratios of $0.79-$2.10 for a one-percent gain in enrollment. They are lower than comparable ratios for related interventions, summarized in Evans and Ghosh (2008), and would still be competitive even if costs were doubled.

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Table 1: Descriptive statistics in the sample of eligible children

	National sample		Experimental sample						
			All groups		G1	G2	G3	G4	
	Mean	N	Mean	N	Mean	Mean	Mean	Mean	p-value
Dependent variables									
Attends school	0.753	950,683	0.701	120,411	0.739	0.723	0.636	0.650	0.018
Works outside home	0.047	775,673	0.076	98,783	0.075	0.054	0.092	0.099	0.026
Works only in home	0.100	775,673	0.110	98,783	0.101	0.089	0.141	0.134	0.035
Independent variables									
Age	8.381	950,683	8.498	120,411	8.449	8.505	8.550	8.528	0.189
	(1.80)		(1.87)						
Female	0.481	950,683	0.483	120,411	0.484	0.483	0.483	0.483	0.918
Born in municipality	0.871	950,683	0.924	120,411	0.934	0.905	0.929	0.933	0.581
Lenca	0.053	950,683	0.319	120,411	0.391	0.266	0.336	0.286	0.317
Other	0.029	950,683	0.035	120,411	0.005	0.049	0.063	0.041	0.295
Father is literate	0.707	765,958	0.615	102,615	0.639	0.607	0.570	0.615	0.523
Mother is literate	0.699	878,677	0.548	111,418	0.564	0.551	0.530	0.529	0.445
Father's schooling	3.653	765,958	2.321	102,615	2.532	2.301	2.090	2.182	0.364
	(3.97)		(2.72)						
Mother's schooling	3.640	878,677	2.112	111,418	2.261	2.153	1.973	1.917	0.232
	(3.78)		(2.66)						
Dirt floor	0.434	936,249	0.719	118,697	0.726	0.724	0.728	0.698	0.893
Piped water	0.680	936,249	0.643	118,697	0.642	0.645	0.652	0.636	0.974
Electricity	0.475	936,249	0.144	118,697	0.146	0.156	0.096	0.151	0.848
Rooms in dwelling	1.682	948,056	1.405	120,321	1.435	1.416	1.402	1.352	0.101
	(0.90)		(0.72)						
Sewer/septic	0.413	948,056	0.305	120,321	0.346	0.297	0.287	0.269	0.312
Auto	0.090	948,056	0.038	120,321	0.040	0.034	0.050	0.035	0.162
Refrigerator	0.253	948,056	0.051	120,321	0.058	0.051	0.031	0.053	0.815
Computer	0.018	948,056	0.002	120,321	0.003	0.002	0.000	0.002	0.177
Television	0.373	948,056	0.076	120,321	0.090	0.072	0.047	0.078	0.781
Mitch	0.035	948,056	0.015	120,321	0.020	0.014	0.008	0.014	0.205
Household members	7.080	950,683	7.404	120,411	7.516	7.434	7.354	7.238	0.153
	(3.75)		(2.41)						
Household members, 0-17	4.427	950,683	4.785	120,411	4.852	4.820	4.770	4.655	0.261
	(3.16)		(1.92)						
Maximum N of children	950,683		120,411		38,435	39,065	14,154	28,757	
N of municipalities	298		70		20	20	10	20	

Source: 2001 Honduran Census and authors' calculations.
Notes: The sample includes children ages 6-12 who have not completed fourth grade. Standard deviations are in parentheses for continuous variables. The p-value in the final column is obtained by regressing each variable on three treatment group dummy variables and four of five block dummy variables—clustering standard errors by municipality—and testing the null hypothesis that coefficients on treatment group variables are jointly zero.

Table 2: Effects among eligible children

	Dependent variable					
	Attends school		*Works outside home*		*Works only in home*	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
G1	0.101**	0.083**	-0.031	-0.024	-0.040+	-0.032+
	(0.036)	(0.028)	(0.020)	(0.017)	(0.020)	(0.017)
G2	0.074*	0.070**	-0.045**	-0.043**	-0.047*	-0.045**
	(0.032)	(0.026)	(0.015)	(0.013)	(0.019)	(0.017)
G3	-0.013	-0.012	-0.008	-0.011	0.006	0.005
	(0.052)	(0.043)	(0.025)	(0.021)	(0.029)	(0.026)
Adjusted R^2	0.013	0.160	0.009	0.090	0.008	0.064
p-value (G1=G2)	0.469	0.646	0.455	0.208	0.713	0.390
p-value (G2=G3)	0.094	0.061	0.101	0.077	0.051	0.035
Panel B						
D	0.092**	0.080**	-0.035*	-0.030**	-0.045**	-0.040**
	(0.029)	(0.023)	(0.014)	(0.011)	(0.015)	(0.013)
Adjusted R^2	0.012	0.160	0.009	0.090	0.008	0.064
Panel C						
*D * Block 1*	0.221**	0.178**	-0.095**	-0.079**	-0.081**	-0.063*
	(0.055)	(0.044)	(0.025)	(0.022)	(0.029)	(0.027)
*D * Block 2*	0.108*	0.104*	-0.058*	-0.050*	-0.061*	-0.058**
	(0.053)	(0.041)	(0.028)	(0.020)	(0.024)	(0.019)
*D * Block 3*	0.048	0.047	-0.008	-0.011	-0.041	-0.039
	(0.053)	(0.045)	(0.020)	(0.016)	(0.040)	(0.036)
*D * Block 4*	0.010	0.016	0.007	0.001	-0.008	-0.011
	(0.043)	(0.041)	(0.030)	(0.029)	(0.026)	(0.026)
*D * Block 5*	0.052	0.044	-0.018	-0.009	-0.034	-0.031
	(0.067)	(0.046)	(0.028)	(0.021)	(0.038)	(0.028)
Adjusted R^2	0.019	0.163	0.013	0.093	0.009	0.065
p-value	0.049	0.071	0.038	0.061	0.402	0.542
Panel D						
*D * Blocks 1-2*	0.177**	0.150**	-0.080**	-0.068**	-0.073**	-0.061**
	(0.044)	(0.034)	(0.019)	(0.016)	(0.021)	(0.018)
*D * Blocks 3-5*	0.036	0.035	-0.006	-0.006	-0.027	-0.026
	(0.032)	(0.025)	(0.015)	(0.013)	(0.020)	(0.017)
Adjusted R^2	0.017	0.163	0.013	0.092	0.009	0.065
p-value	0.012	0.008	0.004	0.004	0.117	0.161
N	120411	120411	98783	98783	98783	98783
Controls?	No	Yes	No	Yes	No	Yes

Notes: ** indicates statistical significance at 1%, * at 5%, and + at 10%. Robust standard errors are in parentheses, adjusted for municipal-level clustering. All regressions include experimental block dummy variables. Optional controls include (1) the independent variables in Table 1 (with age-specific dummies and quadratic polynomials for other continuous variables), (2) dummy variables indicating the number of children eligible for the education transfer in a household, (3) dummy variables indicating the number of children eligible for the health transfer, and (4) dummy variables indicating missing values of the independent variables. Reported p-values refer to the null hypothesis that coefficients are equal.

Table 3: Effects among ineligible children

	Sample					
	No eligible child in HH		≥1 eligible for education transfer in HH		≥1 eligible for education or health transfer in HH	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: *Attends schools*						
D	-0.000		0.008		0.007	
	(0.015)		(0.011)		(0.011)	
*D * Block 1*		0.058		0.067**		0.067**
		(0.038)		(0.019)		(0.020)
*D * Block 2*		-0.001		-0.001		-0.009
		(0.044)		(0.019)		(0.019)
*D * Block 3*		0.012		-0.018		-0.021
		(0.029)		(0.017)		(0.018)
*D * Block 4*		0.000		-0.008		-0.005
		(0.025)		(0.023)		(0.021)
*D * Block 5*		-0.044+		-0.005		-0.003
		(0.024)		(0.019)		(0.021)
p-value		0.214		0.017		0.017
Panel B: *Works outside home*						
D	-0.006		-0.005		-0.004	
	(0.009)		(0.008)		(0.008)	
*D * Block 1*		-0.056*		-0.035+		-0.033+
		(0.026)		(0.018)		(0.018)
*D * Block 2*		-0.005		-0.005		-0.001
		(0.021)		(0.008)		(0.008)
*D * Block 3*		0.007		-0.001		-0.002
		(0.012)		(0.008)		(0.010)
*D * Block 4*		0.002		0.011		0.010
		(0.023)		(0.021)		(0.020)
*D * Block 5*		0.007		0.003		0.004
		(0.010)		(0.016)		(0.016)
p-value		0.057		0.069		0.068
		0.250		0.426		0.450
Panel C: *Works only in home*						
D	0.003		-0.001		0.000	
	(0.012)		(0.008)		(0.008)	
*D * Block 1*		-0.001		-0.020		-0.018
		(0.028)		(0.019)		(0.020)
*D * Block 2*		-0.015		0.013		0.012
		(0.032)		(0.017)		(0.014)
*D * Block 3*		-0.020		0.005		0.008
		(0.036)		(0.014)		(0.015)
*D * Block 4*		0.007		0.007		0.007
		(0.012)		(0.021)		(0.021)
*D * Block 5*		0.027		-0.004		-0.004
		(0.017)		(0.015)		(0.016)
p-value		0.652		0.715		0.778
N	3830	3830	16586	16586	18325	18325

Notes: *** indicates statistical significance at 1%, ** at 5%, and + at 10%. Robust standard errors are in parentheses, adjusted for municipal-level clustering. All regressions include the full set of controls described in the note to Table 2. Reported p-values refer to the null hypothesis that coefficients are equal.

Table 4: Effects among adults

	Sample											
	Males						Females					
	No eligible child in HH		≥1 eligible for educ. transfer in HH		≥1 eligible for educ. or health transfer in HH		No eligible child in HH		≥1 eligible for educ. transfer in HH		≥1 eligible for educ. or health transfer in HH	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: *Works outside home*												
D	-0.011		-0.013		-0.013		0.013		0.010		0.009	
	(0.010)		(0.008)		(0.008)		(0.019)		(0.019)		(0.019)	
*D * Block 1*		-0.035*		-0.023+		-0.021		0.032		0.033		0.026
		(0.017)		(0.013)		(0.014)		(0.027)		(0.026)		(0.023)
*D * Block 2*		0.017		0.006		0.005		0.057		0.040		0.037
		(0.030)		(0.020)		(0.017)		(0.109)		(0.096)		(0.099)
*D * Block 3*		0.015		0.028+		0.028+		-0.030*		-0.015		-0.010
		(0.016)		(0.016)		(0.016)		(0.014)		(0.015)		(0.014)
*D * Block 4*		-0.019		-0.018		-0.016		0.010		-0.011		-0.012
		(0.015)		(0.012)		(0.011)		(0.021)		(0.025)		(0.024)
*D * Block 5*		-0.018		-0.049*		-0.050*		0.010		0.007		0.010
		(0.025)		(0.021)		(0.021)		(0.028)		(0.025)		(0.023)
p-value		0.234		0.032		0.037		0.196		0.525		0.639
Panel B: *Works only in home*												
D	0.008+		0.009*		0.008*		-0.018		-0.008		-0.007	
	(0.004)		(0.004)		(0.004)		(0.019)		(0.019)		(0.019)	
*D * Block 1*		0.000		0.007		0.006		-0.032		-0.027		-0.018
		(0.009)		(0.010)		(0.011)		(0.028)		(0.030)		(0.027)
*D * Block 2*		0.005		0.000		0.000		-0.053		-0.035		-0.032
		(0.010)		(0.005)		(0.005)		(0.102)		(0.094)		(0.097)
*D * Block 3*		0.008		0.003		0.003		0.026		0.012		0.010
		(0.009)		(0.011)		(0.011)		(0.019)		(0.014)		(0.014)
*D * Block 4*		0.013+		0.015**		0.014**		-0.037		0.011		0.010
		(0.007)		(0.005)		(0.004)		(0.027)		(0.026)		(0.025)
*D * Block 5*		0.011		0.015*		0.016*		-0.008		-0.007		-0.010
		(0.009)		(0.006)		(0.006)		(0.028)		(0.025)		(0.023)
p-value		0.827		0.218		0.157		0.268		0.748		0.823
N	21707	21707	56107	56107	77683	77683	22567	22567	61760	61760	84344	84344

Notes: *** indicates statistical significance at 1%, ** at 5%, and + at 10%. Robust standard errors are in parentheses, adjusted for municipal-level clustering. All regressions include the full set of controls described in the note to Table 2. Reported p-values refer to the null hypothesis that coefficients are equal.

Table 5: Rule-based discontinuity effects among eligible children

	Bandwidth for *HAZ*					
	.3		.4		.5	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: *D*						
E	0.245	0.255	0.314+	0.320+	0.295+	0.299*
	(0.219)	(0.203)	(0.183)	(0.171)	(0.157)	(0.149)
N	192475	192475	246998	246998	341373	341373
Panel B: *Mother's schooling*						
E	0.000	--	0.207	--	-0.053	--
	(0.290)		(0.272)		(0.234)	
N	178149		228713		316598	
Panel C: *Attends school*						
E	-0.002	-0.010	0.011	-0.005	-0.016	-0.016
	(0.046)	(0.035)	(0.041)	(0.031)	(0.037)	(0.028)
N	192475	192475	246998	246998	341373	341373
Panel D: *Works outside home*						
E	0.013	0.017	0.012	0.018	0.017	0.017
	(0.022)	(0.020)	(0.018)	(0.016)	(0.017)	(0.015)
N	158619	158619	203306	203306	280762	280762
Panel E: *Works only in home*						
E	0.013	0.016	0.003	0.010	0.009	0.008
	(0.023)	(0.021)	(0.020)	(0.019)	(0.018)	(0.016)
N	158619	158619	203306	203306	280762	280762
Controls	No	Yes	No	Yes	No	Yes

Notes: *** indicates statistical significance at 1%, ** at 5%, and + at 10%. Robust standard errors are in parentheses, adjusted for municipal-level clustering. All regression include a piecewise linear spline of *HAZ*. Optional controls include the full set of controls described in the note to Table 2 (except for experimental block dummy variables).

Table 6: Border discontinuity effects among eligible children

	Caseríos +/- 2 km from border				*Caseríos* +/- 4 km from border			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: *Attends school*								
*G1/G2 * Blocks 1-2*	0.072	0.073+	0.072	0.071+	0.082+	0.085*	0.075+	0.080*
	(0.049)	(0.041)	(0.049)	(0.041)	(0.044)	(0.035)	(0.045)	(0.035)
*G1/G2 * Blocks 3-5*	0.012	0.026	0.011	0.023	0.006	0.038	0.005	0.035
	(0.032)	(0.031)	(0.029)	(0.030)	(0.032)	(0.029)	(0.031)	(0.029)
N	32180	32180	32180	32180	69840	69840	69840	69840
p-value	0.301	0.360	0.290	0.341	0.147	0.281	0.197	0.322
Panel B: *Works outside home*								
*G1/G2 * Blocks 1-2*	-0.065*	-0.065*	-0.067*	-0.067*	-0.084*	-0.085**	-0.083*	-0.085**
	(0.032)	(0.029)	(0.031)	(0.029)	(0.033)	(0.031)	(0.033)	(0.031)
*G1/G2 * Blocks 3-5*	0.007	0.002	0.007	0.002	0.012	0.003	0.015	0.006
	(0.010)	(0.009)	(0.011)	(0.009)	(0.011)	(0.010)	(0.013)	(0.012)
N	26496	26496	26496	26496	57531	57531	57531	57531
p-value	0.029	0.028	0.028	0.027	0.005	0.007	0.006	0.007
Panel C: *Works only inside home*								
*G1/G2 * Blocks 1-2*	-0.016	-0.015	-0.019	-0.019	-0.010	-0.009	-0.012	-0.012
	(0.021)	(0.018)	(0.022)	(0.019)	(0.019)	(0.016)	(0.018)	(0.015)
*G1/G2 * Blocks 3-5*	0.023	0.015	0.025	0.018	0.013	0.002	0.010	-0.001
	(0.018)	(0.017)	(0.017)	(0.017)	(0.016)	(0.016)	(0.017)	(0.016)
N	26496	26496	26496	26496	57531	57531	57531	57531
p-value	0.171	0.226	0.122	0.156	0.354	0.619	0.386	0.626
Panel D: *Attends school*								
*G3/G4 * Blocks 1-2*	-0.047	0.012	-0.030	0.023	-0.060	-0.033	-0.055	-0.029
	(0.053)	(0.043)	(0.053)	(0.043)	(0.037)	(0.029)	(0.037)	(0.029)
*G3/G4 * Blocks 3-5*	-0.025	-0.031	-0.030	-0.035+	-0.027	-0.022	-0.031	-0.024
	(0.026)	(0.019)	(0.028)	(0.019)	(0.021)	(0.019)	(0.025)	(0.020)
N	17687	17687	17687	17687	37555	37555	37555	37555
p-value	0.714	0.363	0.997	0.215	0.438	0.743	0.575	0.904
Panel E: *Works outside home*								
*G3/G4 * Blocks 1-2*	0.025	-0.009	0.018	-0.014	0.025	0.010	0.022	0.007
	(0.038)	(0.034)	(0.037)	(0.033)	(0.024)	(0.021)	(0.025)	(0.022)
*G3/G4 * Blocks 3-5*	-0.005	-0.010	-0.008	-0.012	0.005	0.001	0.003	-0.002
	(0.014)	(0.011)	(0.013)	(0.010)	(0.010)	(0.007)	(0.010)	(0.008)
N	14604	14604	14604	14604	30965	30965	30965	30965
p-value	0.447	0.974	0.486	0.966	0.433	0.695	0.439	0.685
Panel F: *Works only inside home*								
*G3/G4 * Blocks 1-2*	-0.019	-0.038+	-0.024	-0.041*	0.019	0.016	0.016	0.013
	(0.024)	(0.021)	(0.024)	(0.020)	(0.013)	(0.012)	(0.012)	(0.010)
*G3/G4 * Blocks 3-5*	0.006	0.007	0.001	0.001	0.021+	0.020+	0.017	0.015
	(0.016)	(0.014)	(0.015)	(0.013)	(0.013)	(0.012)	(0.013)	(0.011)
N	14604	14604	14604	14604	30965	30965	30965	30965
p-value	0.385	0.074	0.389	0.099	0.914	0.814	0.930	0.880
Controls?	No	Yes	No	Yes	No	Yes	No	Yes
Geographic control	Lat/Lon	Lat/Lon	Dist	Dist	Lat/Lon	Lat/Lon	Dist	Dist

Notes: *** indicates statistical significance at 1%, ** at 5%, and + at 10%. Robust standard errors are in parentheses, adjusted for municipal-level clustering. All regressions include fixed effects indicating border segments and geographic controls (see text for details). Optional controls include the full set of controls described in the note to Table 2 (except for experimental block dummy variables).

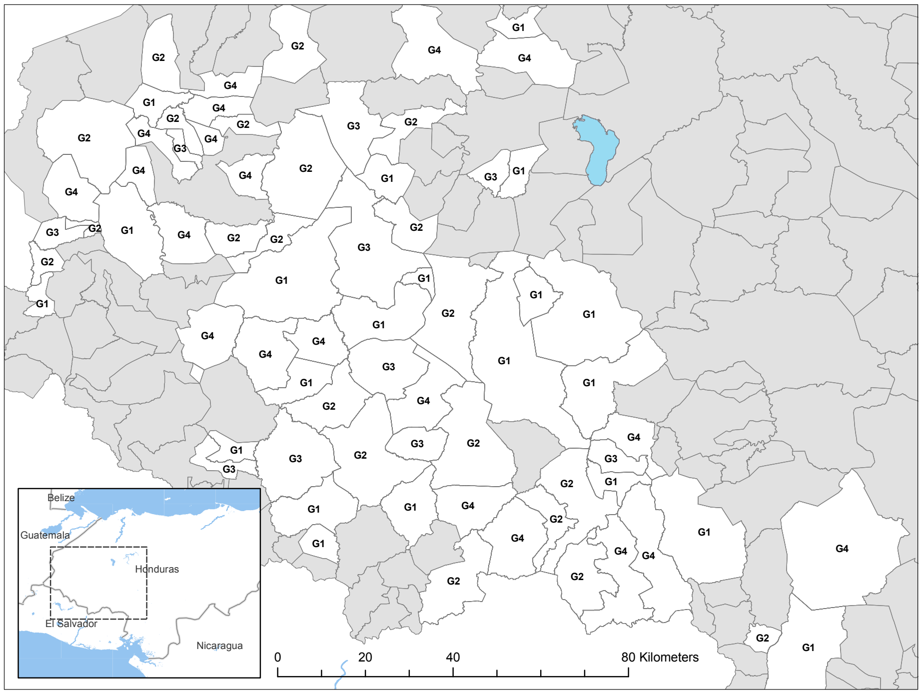
Table 7: Heterogeneity in effects among eligible children

	Dependent variable								
	Attends school			*Works outside home*			*Works only in home*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Age									
*D * Age 6*	0.085*	0.196**	0.008	--	--	--	--	--	--
	(0.033)	(0.049)	(0.034)						
*D * Age 7*	0.102**	0.183**	0.047	-0.023*	-0.060**	0.003	-0.050**	-0.079**	-0.031
	(0.027)	(0.036)	(0.029)	(0.011)	(0.016)	(0.011)	(0.017)	(0.024)	(0.021)
*D * Age 8*	0.071**	0.137**	0.028	-0.025*	-0.055**	-0.006	-0.035*	-0.054*	-0.024
	(0.023)	(0.033)	(0.025)	(0.010)	(0.015)	(0.010)	(0.014)	(0.021)	(0.017)
*D * Age 9*	0.056**	0.109**	0.021	-0.027**	-0.056**	-0.009	-0.035**	-0.060**	-0.022
	(0.019)	(0.026)	(0.022)	(0.009)	(0.013)	(0.011)	(0.012)	(0.017)	(0.015)
*D * Age 10*	0.063**	0.118**	0.026	-0.032*	-0.066**	-0.010	-0.033*	-0.058**	-0.017
	(0.023)	(0.034)	(0.024)	(0.013)	(0.017)	(0.016)	(0.014)	(0.019)	(0.019)
*D * Age 11*	0.107**	0.138**	0.088**	-0.047**	-0.093**	-0.019	-0.050**	-0.067**	-0.040+
	(0.026)	(0.040)	(0.032)	(0.017)	(0.024)	(0.021)	(0.015)	(0.018)	(0.020)
*D * Age 12*	0.083**	0.137**	0.044	-0.043+	-0.109**	0.000	-0.038*	-0.048+	-0.028
	(0.031)	(0.046)	(0.038)	(0.022)	(0.028)	(0.028)	(0.017)	(0.026)	(0.021)
p-value	0.000	0.118	0.000	0.171	0.013	0.264	0.023	0.018	0.317
Panel B: Gender									
*D * Female*	0.081**	0.155**	0.030	-0.013+	-0.023	-0.006	-0.057**	-0.092**	-0.035
	(0.023)	(0.032)	(0.023)	(0.008)	(0.014)	(0.008)	(0.021)	(0.028)	(0.027)
*D * Male*	0.079**	0.144**	0.037	-0.047*	-0.111**	-0.007	-0.024*	-0.034+	-0.018
	(0.024)	(0.038)	(0.026)	(0.020)	(0.029)	(0.023)	(0.010)	(0.017)	(0.012)
p-value	0.752	0.401	0.309	0.120	0.011	0.949	0.099	0.063	0.473
Panel C: Ethnicity									
*D * Lenca*	0.096**	0.157**	-0.016	-0.031*	-0.065**	0.022	-0.027+	-0.046*	0.007
	(0.032)	(0.036)	(0.029)	(0.015)	(0.014)	(0.019)	(0.016)	(0.020)	(0.019)
*D * Not Lenca*	0.073**	0.143**	0.044+	-0.030*	-0.071**	-0.013	-0.046**	-0.076**	-0.034+
	(0.022)	(0.035)	(0.025)	(0.012)	(0.019)	(0.013)	(0.015)	(0.019)	(0.019)
p-value	0.354	0.361	0.051	0.907	0.687	0.048	0.208	0.039	0.085
Panel D: Children eligible for education transfer									
*D * 1-3 children*	0.084**	0.152**	0.038	-0.030**	-0.068**	-0.006	-0.040**	-0.061**	-0.027
eligible	(0.023)	(0.034)	(0.024)	(0.011)	(0.015)	(0.013)	(0.013)	(0.018)	(0.017)
*D * ≥4 children*	0.041	0.122**	-0.011	-0.034*	-0.063**	-0.012	-0.039**	-0.070**	-0.021
eligible	(0.025)	(0.040)	(0.025)	(0.013)	(0.021)	(0.015)	(0.014)	(0.019)	(0.018)
p-value	0.000	0.011	0.000	0.379	0.558	0.140	0.800	0.241	0.299
Panel E: Children eligible for health transfer									
*D * ≥1 child 0-3*	0.086**	0.155**	0.035	-0.035**	-0.068**	-0.012	-0.044**	-0.065**	-0.030
	(0.025)	(0.034)	(0.027)	(0.012)	(0.017)	(0.015)	(0.014)	(0.019)	(0.018)
*D * No child 0-3*	0.070**	0.139**	0.031	-0.023*	-0.069**	0.002	-0.033*	-0.057**	-0.021
	(0.022)	(0.036)	(0.022)	(0.011)	(0.015)	(0.012)	(0.013)	(0.017)	(0.016)
p-value	0.046	0.196	0.661	0.025	0.880	0.041	0.111	0.368	0.266
Sample	Full	Blocks 1-2	Blocks 3-5	Full	Blocks 1-2	Blocks 3-5	Full	Blocks 1-2	Blocks 3-5
N	120411	44358	76053	98783	36261	62522	98783	36261	62522

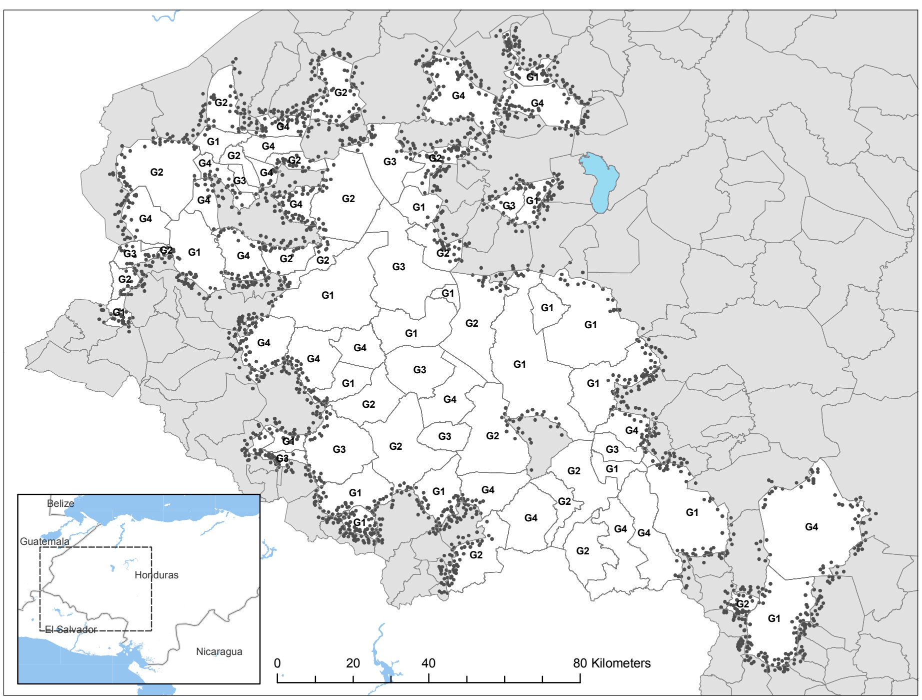
Notes: ** indicates statistical significance at 1%, * at 5%, and + at 10%. Robust standard errors are in parentheses, adjusted for municipal-level clustering. All regressions include the full set of controls described in the note to Table 2. Reported p-values refer to the null hypothesis that coefficients are equal.

Figure 1: Treated and untreated municipalities

Panel A: Seventy municipalities subject to random assignment

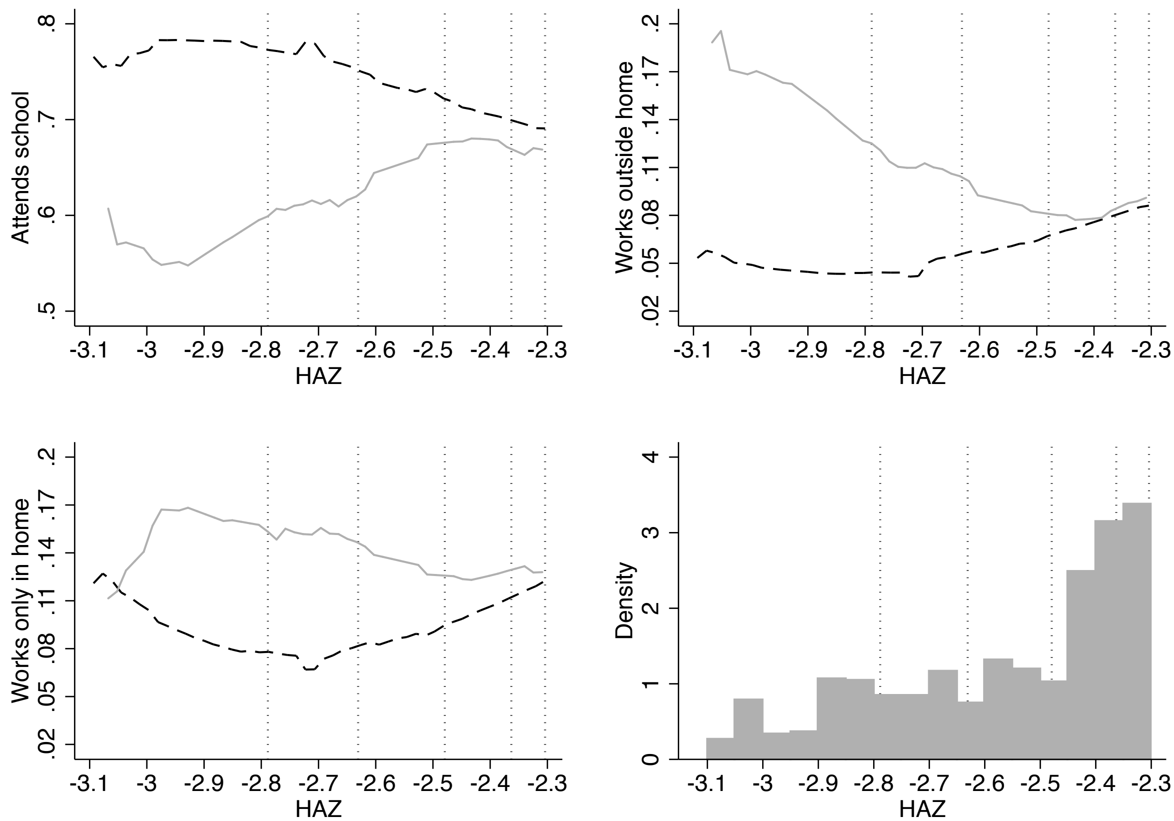


Panel B: *Caseríos* within 2 kilometers of municipal borders



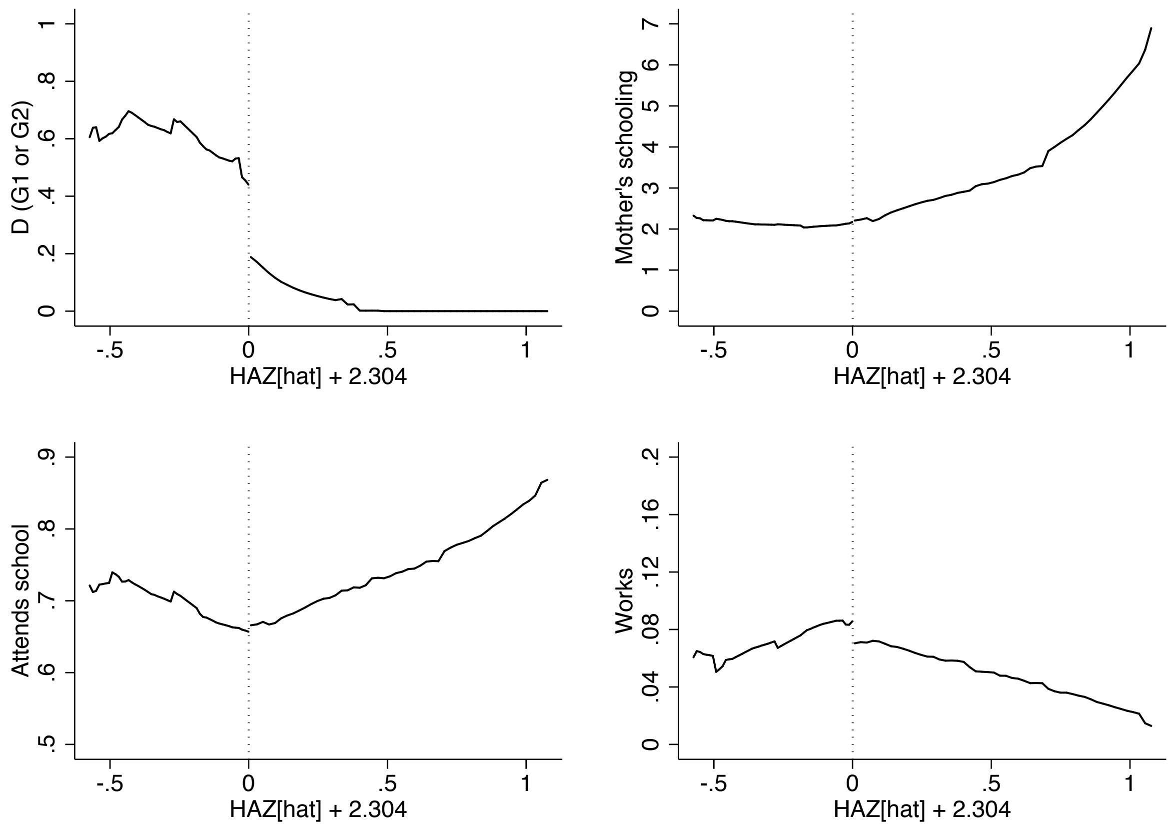
Notes: Unshaded municipalities were randomly assigned to receive cash transfers (G1), to receive transfers and direct investments (G2), to receive direct investments (G3), or to receive no treatment (G4). See text for details. Dots in panel B represent *caseríos* within 2 km of the municipal border.

Figure 2: Experimental treatment effects by block



Note: In the top panels, lines are fitted values from local linear regressions of the y-axis variable on the x-axis variable (bandwidth=0.3; rectangular kernel). The dashed line refers to the sample of eligible children in G1 and G2, and the solid line to eligible children in G3 and G4. Vertical dotted lines divide the 5 experimental blocks. The histogram applies a bin-width of 0.05 to the sample of eligible children.

Figure 3: Assignment discontinuity effects among eligible children



Note: In the top panels, lines are fitted values from local linear regressions of the y-axis variable on the x-axis variable (bandwidth=0.3; rectangular kernel). The dashed line refers to the national sample of eligible children (excluding G3 and G4), and the solid line to the national sample of eligible children (excluding G1 and G2). Vertical dotted lines separate the 5 experimental blocks.

Table A1: Variable definitions

	Variable definition (census question)
Dependent variables	
Attends schools	1=Currently enrolled in school; 0=not (F8).
Works outside home	1=Worked during past week, including self-employment, family business, and agricultural work; 0=not (F12, F13A01-04); only reported for ages 7 and up.
Works only in home	1=Worked during past week, exclusively in household chores; 0=not (F13B10); only reported for ages 7 and up.
Independent variables	
Age	Integer age on survey date (F3).
Female	1=Female; 0=male (F2).
Born in municipality	1=Born in present municipality; 0=not (F4A).
Lenca	1=Lenca; 0=not (F5).
Other	1=Other non-mestizo ethnicity/race (Garífuna, etc.); 0=not (F5).
Father is literate	1=Father is literate; 0=not (F7, F1, F2).
Mother is literate	1=Mother is literate; 0=not (F7, F1, F2).
Father's schooling	Years of father's schooling (F9, F1, F2).
Mother's schooling	Years of mother's schooling (F9, F1, F2).
Dirt floor	1=Dwelling has dirt floor; 0=not (B5).
Piped water	1=Dwelling has piped water from public or private source; 0=not (B6).
Electricity	1=Electric light from private or public source; 0=light from another source (ocote, etc.) (B8).
Rooms in dwelling	Number of bedrooms used by household (C1).
Sewer/septic	1=Household has toilet connected to sewer or septic system; 0=not (C5).
Auto	1=Household has at least one auto; 0=not (C7).
Refrigerator	1=Household has refrigerator; 0=not (C8a).
Computer	1=Household has computer; 0=not (C8g).
Television	1=Household has television; 0=not (C8e).
Mitch	1=After Hurricane Mitch, household member(s) emigrated; 0=not (E1).
Household members	Total individuals residing in household.
Household members, 0-17	Total individuals, ages 0-17, residing in household.

Table A2: Means of dependent variables in G3 and G4, by block

	Block 1	Block 2	Block 3	Block 4	Block 5	Full sample
Eligible children						
Attends schools	0.555	0.662	0.702	0.654	0.682	0.646
Works outside home	0.143	0.101	0.054	0.095	0.077	0.097
Works only in home	0.168	0.125	0.113	0.137	0.129	0.136
Eligible males						
Attends schools	0.548	0.665	0.687	0.641	0.667	0.636
Works outside home	0.227	0.152	0.094	0.144	0.130	0.153
Works only in home	0.094	0.077	0.071	0.075	0.070	0.078
Eligible females						
Attends schools	0.563	0.660	0.717	0.667	0.699	0.655
Works outside home	0.055	0.050	0.012	0.042	0.019	0.037
Works only in home	0.245	0.173	0.157	0.204	0.194	0.198
Adult males						
Works outside home	0.955	0.932	0.908	0.932	0.921	0.930
Works only in home	0.023	0.018	0.024	0.013	0.023	0.020
Adult females						
Works outside home	0.097	0.138	0.093	0.112	0.117	0.111
Works only in home	0.873	0.834	0.878	0.857	0.852	0.860

Note: Eligible children include children ages 6-12 who have not completed fourth grade. Adults include the male or female head of an eligible child's household and the spouse or partner. Means are taken within municipalities assigned to G3 or G4.

Appendix

A static model yields predictions that are consistent with our empirical results. We assume that each household has a child of school age endowed with a unit of time. The household decides whether to send the child to school or to work. If the child works, the household is able to increase its consumption. If the child attends school, we assume that the household derives utility from it.

Households are heterogeneous. Each household *i* is characterized by its exogenous income (y_i), the height-for-age of the child (h_i), and the utility derived from sending the child to school (s_i). For simplicity, we assume that income and height-for-age are linearly related: $y_i = \alpha h_i$ with $\alpha > 0$.[24] This conveys the idea that poorer households have children with poorer nutrition, as measured by their height-for-age. We assume that *h* and *s* are independent and uniformly distributed with support $[0, \bar{h}]$ and $[0, \bar{s}]$. There is only one good in the economy, whose price is normalized to 1. Finally, a child that works outside the home receives a salary $w(h_i)$, which is an increasing and differentiable function of *h* that satisfies $w(0) = 0$. Children can also attend school, which consumes their whole unit of time. We will represent the utility of attending school with s_i. For example, this variable could measure the expected return to education.[25]

Finally, assume that the household's utility function is quasi-linear in *s*.[26] Then the optimization problem of household *i* is given by:

$$\max_{d_i \in \{0;1\}} u(c_i) + d_i . s_i$$

---

[24] Our qualitative results should not change if we use another function, as long as it is increasing in *h*.

[25] For simplicity, we assumed that *s* and *h* are independent random variables. It could be argued that richer families face higher expected returns to education. This, if anything, would strengthen result 1, and should not change result 2 as long as other aspects of the distribution of *s*, like its variance, do not change.

[26] The central assumption here is that the utility function is separable in consumption and "returns to education".

$$c_i = \alpha.h_i + (1-d_i)w(h_i) + d_i.t$$

where d_i represent the binary schooling decision of the household, and *t* is a conditional cash transfer given by the government to each child that attends school. Define $B(h_i; s_i, t)$ as the difference between the expected utility from sending and not sending a child to school:

(A1) $\quad B(h_i, s_i, t) = u(\alpha.h_i + t) - u(\alpha.h_i + w(h_i)) + s_i$

Then household *i* will send the child to school if and only if $B(h_i, s_i, t) > 0$, or

(A2) $\quad s_i > u(\alpha.h_i + w(h_i)) - u(\alpha.h_i + t)$

Equation (A2) states that the benefits perceived from attending school have to be large enough to compensate the forgone present income. It is obvious from this equation that if the wage of the child is smaller than the transfer, then the family always prefers to send the child to school, even if their expected utility from education were zero. Given equation (A2), it is possible that for some values of *h* there is no household that finds it optimal to send the child to school. The following assumption rules out this situation even when $t = 0$:

Assumption 1: $\bar{s} \geq u\left(\alpha h + w\left(h\right)\right) - u\left(\alpha h\right)$ for all h.

Since s_i is a random variable independent of income, we can calculate the probability that a child in a family with income $\alpha\, h$ attends school and interpret it as the expected proportion of families with that income level that send their children to school:

(A3)

$$P(d_i = 1 | h_i, t) = P(s_i > u(\alpha h + w(h)) - u(\alpha h + t)] = \min\left\{\frac{\bar{s} + u(\alpha.h + t) - u(\alpha.h + w(h))}{\bar{s}}, 1\right\}$$

First consider the case where there is no conditional cash transfer (*t=0*). In this case, we have

$$P(d_i = 1 | h_i = h, t = 0) = \frac{\bar{s} + u(\alpha h) - u(\alpha h + w(h))}{\bar{s}}$$

$$and$$

$$\frac{\partial P(d_i = 1 | h_i = h, t = 0)}{\partial h} = \frac{1}{\bar{s}}\left[\alpha u'(\alpha h) - (\alpha + w'(h))u'(\alpha h + w(h))\right]$$

If u was a convex function, then u' would be an increasing function and hence $u'(\alpha h + w(h)) \geq u'(\alpha h)$, which implies that $\frac{\partial P(d_i = 1 | h_i = h, t = 0)}{\partial h} < 0$. Thus, a necessary condition to obtain $\frac{\partial P(d_i = 1 | h_i = h, t = 0)}{\partial h} > 0$ is a strictly concave function u. The following assumption provides a sufficient condition for a CES utility function:

Assumption 2: The utility function is CES, i.e. $u(c) = \frac{c^{1-\theta}}{1-\theta}$ for $\theta \neq 1$ and $u(c) = \ln c$ for $\theta = 1$.

Moreover, the following condition holds:

$$\theta > \bar{\theta}(h) = \frac{\ln\left(1 + \frac{w'(h)}{\alpha}\right)}{\ln\left(1 + \frac{w(h)}{\alpha h}\right)} \text{ for all } h.$$

Note that if $w(h) = wh$, then $\bar{\theta}(h) = 1$ for all h; if $w(h)$ is strictly concave, then $\bar{\theta}(h) < 1$ for all h; while if $w(h)$ is strictly convex, then $\bar{\theta}(h) > 1$.

Result 1: *If Assumptions 1 and 2 hold, then the derivative of the proportion of households that send their children to school, in the absence of a conditional cash transfer, is increasing in h.*[27]

The intuition behind this result is simple: given *s*, a family with higher income has a lower marginal utility of consumption, and thus will be more willing to send the child to school. Now, suppose the government implements a conditional cash transfer program with transfer level *t*. We have:

$$B(h_i, s_i, T) = u(\alpha h_i + T) - u(\alpha h_i + w(h_i)) + s_i$$

Define *h'* as the level of *h* that satisfies $w(h') = t$, assuming that $\underline{h} < h' < \overline{h}$. Then the families that have an income level lower than *αh'* always prefer to send the child to school when the CCT program is implemented. We will separate the analysis between families that meet this condition and families that do not. In the first case, the proportion of families that send their children to school if they have $h_i < h'$ will be

(A4) $$P(d_i = 1 | h_i \leq h'; t) = 1$$

In the second case, the proportion of families with income $\alpha\, h$ (with *h*>*h'*) that send their child to school when the CCT program is in place is given by:

(A5) $$P(d_i = 1 | h_i = h > h'; t) = P(s_i > u(\alpha, h + w(h)) - u(\alpha.h + T)]$$

$$= \frac{\bar{s} + u(\alpha h + T) - u(\alpha h + w(h))}{\bar{s}}$$

It is easy to prove that if a child goes to school when there is no transfer, then she will go when there is one. Finally, from (A3) to (A5) we can calculate the expected impact of the program at different income levels as the proportion of households that

[27] Note that this result is consistent with evidence that enrollments are lower among control group children in Block 1 (see Table A2).

send their children to school with the CCT program, but do not send the child without the program:

(A6)
$$
\begin{aligned}
& P[B(h,s_i 0) < 0 \wedge B(h,s_i;T) > 0] \\
& = P[u(\alpha.h + w(h)) - u(\alpha.h + T) < s_i < u(\alpha.h + w(h)) - u(\alpha.h)] \\
& = P(d_i = 1 | h_i = h;T) - P(d_i = 1 | h_i = h,0) \\
& = \begin{cases} \dfrac{u(\alpha h + w(h)) - u(\alpha h)}{\overline{s}} & \text{if } h \le h' \\ \dfrac{u(\alpha h + T) - u(\alpha h)}{\overline{s}} & \text{if } h > h' \end{cases}
\end{aligned}
$$

Result 2*: If the utility function is concave with respect to consumption and Assumption 1 holds, the expected impact of the program is decreasing in h.*

This result comes from taking the derivative of equation (A6) with respect to *h*. The intuition behind Result 2 is that households with higher income have a smaller marginal utility of consumption, given their expected returns from education. Thus, the transfer will have a smaller impact on their schooling decision.