The heterogeneous impact of conditional cash transfers in Honduras[1]

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1. Introduction

Conditional cash transfers (CCTs) have been extensively adopted in the last decade, particularly in Latin America. The programs are aimed at dealing simultaneously with current and permanent poverty reduction. They provide cash transfers to finance current consumption, but conditional upon certain behaviors such as school enrollment of children, or regular use of primary health services, especially by pre-school children and by pregnant women and nursing mothers. CCT impacts have been favorably evaluated with randomized, controlled trials, most notably in the case of Mexico's Progresa (Schultz, 2004; Skoufias, 2005), and subsequently in Nicaragua (Maluccio and Flores, 2005), Ecuador (Schady and Araujo, 2008), and Honduras (Glewwe and Olinto, 2004; Morris et al., 2004).[2]

This paper reassesses the causal evidence on the Honduran *Programa de Asignación Familiar* (PRAF-II) and its effects on schooling and child labor outcomes. PRAF-II 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. Previous work has conveyed the idea that the CCT produced small effects on enrollment (3 percentage points or less) and no effects on child labor, perhaps because transfers were a smaller percentage of per-capita household expenditures than other CCTs (Glewwe and Olinto, 2004; Fiszbein and Schady, 2009).

An official evaluation was conducted from 2000 to 2002 in 70 municipalities. After being stratified in 5 blocks based on a measure of the average height-per-age z-score of first-graders,

[2] Fiszbein and Schady (2009) provide a thorough overview of CCT impacts on poverty and participation in education and health investments; see especially their Tables 2-5.

40 municipalities were randomly selected to receive the transfers.[3] A key challenge in prior work is that baseline data (but not follow-up data) were collected at different points in time for treated and untreated municipalities, leading to baseline imbalance in schooling and labor outcomes (Glewwe and Olinto, 2004). This tended to reduce the size of difference-in-difference estimates, which are the most commonly reported in the literature (Fiszbein and Schady, 2009).

This paper reanalyzes the experiment with the 2001 Honduran Census. The census was applied in all 298 municipalities, about 7 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 use the full sample of individual and household data, merged to municipal-level data on treatment group and strata membership. The population data allow us to carefully examine treatment effect heterogeneity, and also to estimate spillover effects on ineligible children and labor supply effects on adults of eligible children. They also remove follow-up attrition as a threat to internal validity.

Finally, the data also facilitate the implementation of two regression-discontinuity designs, using alternate control groups, that we view as important robustness checks. The first exploits the original municipal-level targeting rule that chose 70 experimental municipalities (out of 298) to participate on the basis of low mean height-for-age z-scores, allowing us to estimate local effects at the assignment cutoff. In the second, we compare children who live near the borders of treated municipalities to untreated children near the same borders (but who do not reside in the control municipalities of the original experiment). It is a variant of a multi-dimensional, border-discontinuity design described by Dell (2010) and previously implemented in the education

[3] 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).

literature (Black, 1999). It is the first such design, to our knowledge, that has been analyzed alongside a randomized experiment.

Using the census data, we show that the stratified randomization resulted in well-balanced treatment and control groups, and that the previous finding of imbalance in key baseline outcome variables was almost certainly a statistical artifact (see also Moore, 1998). We find that the Honduran CCT increases the enrollment of eligible children by 8 percentage points, a 12% increase over the control group percentage. It also 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%).

These effects are much larger than commonly supposed, but they are even larger in relatively poorer municipalities, as measured by the stratum of the height-for-age z-score. Indeed, all the significant effects are concentrated in the poorest two quintiles of the distribution of this indicator. The effects on enrollment in these quintiles are 18 and 10 percentage points, respectively, and smaller and statistically insignificant in others. Much the same pattern is observed for child work. The effects on child labor supply outside the home are 8 and 5 percentage points and, on labor inside the home, 6 and 6 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. Further dividing by gender shows quite similar enrollment effects, but larger percentage point gains for boys in work outside the home, and for girls in work inside the home. This result is very important since it implies that the relatively small transfers of PRAF-II had a very large effect in the poorest areas of the country, both in increasing schooling and reducing child labor, but not in relatively richer, but absolutely poor areas. It highlights the importance of targeting the transfers in order to maximize their impact and cost-effectiveness.

The rule-based regression-discontinuity design finds zero effects among eligible children at the assignment cutoff of mean height-for-age z-scores used to assign municipalities to the experimental sample. It confirms the robustness of the zero experimental effects measured among higher quintiles of municipalities, and also illustrates a common caveat of discontinuity designs: that local average treatment effects near the cutoff may not accurately reflect average treatment effects among all subjects assigned to the treatment. Using still another control group, the border-discontinuity design replicates the general pattern of significant effects on enrollment and work outside the home among the poorest quintiles of municipalities, but zero effects among other municipalities (though it does not replicate the negative effects on work inside the home).

Finally, we do not find strong evidence that CCTs have consistent or large effects on samples of similarly-aged children who are ineligible by virtue of having completed fourth grade, regardless of whether an eligible child lives in the same household. Modest effects are observed in just the poorest quintile of experimental municipalities, but this could easily be attributed to lax enforcement of grade-completion requirements for eligibility. Similarly, we find no evidence that CCTs affected adult female labor supply. A very small impact on adult male labor supply is confined to the richest quintile and is not replicated by the rule-based discontinuity design.

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 robustness checks. 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 version, 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).[4] 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; Stecklov et al., 2007).

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

[4] For further background on PRAF and its variants, see Moore (2008) and IDB loan documents (BID, 2004).

and regularly attended grades 1-4 between the school year of March to November.[5] 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/year, depending on school size, to parent associations in primary 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/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

[5] 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).

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.[6] 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) rank-ordered all 298 municipalities, from lowest to highest, by the mean height-for-age z-score of enrolled 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, yielding a final experimental 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. Prior to the baseline, IFPRI (2000) estimated that 70.5% of households in the 70 municipalities fell below an extreme poverty line of 6,462 Lempiras per capita per year.[7]

[6] See IFPRI (2000), Glewwe and Olinto (2004), and Morris et al. (2004).
[7] The data are drawn from the Survey of Expenditures and Livelihoods conducted in May-July 1999 (IFPRI, 2000). Using the 2004 ENCOVI survey, World Bank (2006) reports an extreme poverty line of 6,120 Lempiras per capita per year.

The government 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, or experimental block, 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 its contractors 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. Unfortunately the G1and G2 groups were surveyed from August to October, while the G3 and G4 groups were surveyed from November to December (Glewwe and Olinto, 2004). The school year ends and agriculture work increases as the calendar year ends, likely introducing spurious baseline differences in school enrollment and work of children in G1-G2 and G3-G4. Specifically, one would anticipate a higher proportion enrolled and a lower proportion working among children in G1-G2 who were surveyed earlier. 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. The cross-sectional estimates after the first year are much larger: 7.4 and 7.5

percentage points, respectively (these will prove to quite similar to our own full-sample estimates). The authors attribute the smaller difference-in-difference estimates to the aforementioned baseline differences in student outcomes.[8] The effects of G3 relative to G4 are statistically insignificant. Finally, the authors find no statistically significant effects on child labor force participation.

Two other papers analyze health and fertility outcomes in the IFPRI dataset. Morris et al. (2004) report that overall randomization appears to have produced baseline comparability across G1 to G4 in a variety of variables that are insensitive to the timing of the baseline survey, such as mother's literacy and health outcomes like child immunization rates. 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.

Similarly, 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.

3. Data

A. Sample

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 7 months into

[8] Reviews typically cite the smaller difference-in-difference effects (Fiszbein and Schady, 2009).

the first year of treatment, after 2 of 3 transfer payments had occurred in G1 and G2. This paper uses the full set of 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 than prior evaluations; (2) it improves upon sample attrition by virtue of including the entire population in the follow-up; (3) it contains large samples of children eligible for transfers as well as ineligible children, allowing us to test for spillover effects; and (4) the availability of national data facilitates the application of two regression-discontinuity designs using alternate control groups.

There are a few limitations. First, it is a short-term follow-up, although it occurred after 2 of 3 transfer payments were made in 2001. Second, the census form only contains binary measures of school enrollment and labor force participation, and no health outcomes. Third, there is no baseline data, although we are also able to demonstrate (like Morris et al., 2004 and Stecklov et al., 2007 in the IFPRI data) that there is balance across treatment and control groups in a wide range of individual and household variables not directly affected by the treatments.

B. Variables

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, and less than fourth grade complete). 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).[9]

Independent variables include common individual variables such as age and gender, in addition to a dummy variable indicating self-identification as indigenous (*Lenca*).[10] 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 are jointly equal across the four groups.[11] 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 develops an empirical framework to assess whether the basic findings are robust.

4. Empirical Strategy

[9] This restriction is imposed by the flow of the census questionnaire.

[10] Unlike Guatemala and other countries in Central and South America, this does not imply monolingual or bilingual status in any indigenous language.

[11] We regress each independent variable on dummy variables indicating G1, G2, and G3 (as well as 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-G3 are jointly zero.

A. Randomized Experiment

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

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

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 Block 1 as the quintile of 14 municipalities with the lowest mean height-for-age z-scores, 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 a complete set of 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:

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

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 in equation (1): $\beta_1 = \beta_2$ 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 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, who have already 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 availability of census data facilitate the application of two regression-discontinuity strategies that allow robustness checks using alternate control groups. As section 2 described, 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,* can be interpreted as a 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 excluded from random assignment because of distance and cost concerns. The stratified random assignment removed 30 additional municipalities, but falling below the cutoff should still occasion sudden increases in the probability of treatment with cash transfers. This implies a fuzzy regression discontinuity design (Lee and Lemieux, 2008).

We first restrict the sample to eligible children (ages 6-12 with incomplete fourth grade) residing in municipalities where $-h \leq (HAZ_{jk} + 2.304) < h$; h is a bandwidth specifying the size of the data window near the cutoff, and we report estimates using several different ones. We then 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* still 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: $f(HAZ_{jk}) = \gamma_0 \times (HAZ_{jk} + 2.304) + \gamma_1 \times (HAZ_{jk} + 2.304) \times E_{ijk}$. In equation (3), $\hat{\alpha}_1$ represents the sharp increase in probability of treatment with cash transfers at the assignment cutoff. Reduced-form effects on outcomes can be calculated by replacing the dependent variable with a student outcome:

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

$\hat{\beta}_1 / \hat{\alpha}_1$, usually estimated via two-stage least squares, is the local average treatment effect: that is, the effect among children in municipalities that were induced to be treated by virtue of falling just below the cutoff. In practice, this does not include the very poorest municipalities that had little chance of obtaining a mean *HAZ* close to the cutoff.

This would be straightforward to implement but for a practical complication: the original HAZ_{jk} is 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 below -3, (2) the proportion with z-scores between -3 and -2, and (3) the number of surveyed first-graders (Secretaría de Educación, 1997). To obtain an estimate of HAZ_{jk} using these data, we estimate a municipal-level interval regression of the censored HAZ_{jk} on the observed proportions and their interaction term, weighting the regression by the number of first-graders.[12] 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 a small amount of noise in the value of the assignment variable, we might anticipate that $\hat{\alpha}_1$, the estimated increase in probability of treatment near the cutoff, is further attenuated. However, it should still identify sharp and plausibly exogenous variation in the probability of being treated in G1 or G2. We can 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 often share borders with municipalities not in the experimental sample (see Figure 1). Indeed, households in close

[12] 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.

proximity—and perhaps similar in other regards, such as land quality and public services—may nonetheless have differential access to conditional cash transfers.[13] These municipal boundaries create a sharp, multi-dimensional discontinuity in longitude-latitude space (Dell, 2010).

Municipalities are subdivided into *aldeas* (villages) and *caseríos* (clusters of rural households, or hamlets). The latter are identified as points in government geographic data.[14] 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 controls for 33 dummy variables, δ_b, with each dummy indicating treated and untreated children on either side of a G1 or G2 border (note that 7 municipalities are excluded because their borders are circumscribed by other experimental municipalities).

The specification further include 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) persuasively argues that a multi-dimensional RD should include a flexible function of longitude and latitude.[15] We report variants of both specifications. Finally, we re-estimate equation (5) in a sample of children whose *caseríos* border 22 control municipalities in G3 or

[13] In general, public services such as education are provided at a higher territorial authority, the Department.
[14] This paper's geographic analyses rely on ArcGIS files obtained from the Infotecnología unit of the Ministry of Education.
[15] We include a quadratic in latitude and longitude.

G4. Doing so provides a useful falsification test, since the "effect" of residing in a control 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.[16] 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 estimate differences towards zero, in the spirit of Miguel and Kremer (2004).

6. 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

[16] 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 2-5 include, respectively, 6, 8, 7, and 7 municipalities.

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 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 statistical evidence provides little support for the notion that putative direct investments in G2 or G3 affected school enrollments.

Thus, in panel B, columns (1) and (2) control for a single dummy variable *D* indicating that the observation belongs to one of the experimental groups G1 or G2 (relative to G3 or G4). 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 (3) to (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 the treatment groups G1 or G2 are 3 percentage points less likely to work outside the home (panel B, column (4)), and 4 percentage points less likely to work exclusively on household chores inside the home.

The full-sample point estimates are quite 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%.[17] 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%.

[17] 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.

B. Heterogeneity by Experimental Block

Figure 2 presents visual evidence that the magnitude of effects varies with values of *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 hat 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 that coefficients are jointly equal at the 7 percent confidence level. In panel D, the regression in column (2) further combines blocks 3-5 in a single group, but that coefficient is not statistically significant.

Much the same pattern is observed for child work in columns (4) and (6). The effects in blocks 1 and 2 are, respectively, 7.9 and 5 percentage points on work outside the home, and smaller and insignificant in higher ranked blocks. Again, we reject the null hypothesis that effects are equal across blocks at the 6 percent significance level. A similar pattern is observed for work inside in the home in column (6), with effects of 6.3 and 5.8 percentage points in blocks 1 and 2 (although the null of coefficient equality across all blocks cannot be rejected in this case).

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, depending on the block (see Table A2). These gains are even larger than full sample estimates just reported. Contrary to conventional wisdom, the results imply that PRAF-II's modest annual transfers of US$50-60 per child had very large effects in the poorest 10% of Honduran municipalities, both in increasing schooling and reducing child labor. The effects were not observed in relatively richer, though absolutely poor areas. This might easily be explained if children in poor areas face lower costs of schooling—whether direct or opportunity costs—and hence are more likely to switch from labor to school even with a small incentive. It might also be the case if the labor-supply and schooling income elasticities are larger in poorer areas.

C. Heterogeneity by Child and Household Characteristics

The existence of heterogeneity by experimental block suggests that individuals might also respond differently to a uniform subsidy payment. Panel A-C in Table 3 examine heterogeneity by age, gender, and ethnicity, respectively. In each panel, *D* is interacted with dummy variables for all categories of an attribute, and regressions are estimated separately in the full sample, blocks 1-2, and blocks 3-5.

In panel A, column (1), one can reject the null hypothesis that treatment effects on enrollment are similar in age groups, although effects are not monotonic. The pattern is somewhat clearer in the blocks 1-2 in column (2), with effects as large as 18-19 percentage points among younger students (with a p-value of 0.12). Blocks 3-5 show an anomalous positive effect among 11 year-olds, but nothing else to overturn the main conclusion that effects are smaller among children in these blocks. For work outside the home the largest absolute reductions are among 11-12 year-olds in the poorer blocks, with effects of 9-11 percentage points (p-value=0.01). Even so, the implied percentage reductions are actually somewhat larger among younger ages: 60-65% for 7-9 year-olds, compared with 39-57% for 10-12 year-olds. In the sample of eligible children, the rate of work outside the home rise from 6% at age 7 (in G3 and G4) to 28% at age 12. The pattern for work only in the home is slightly different. In blocks 1-2, the null of equality is rejected at p=0.02, but coefficients lie between 5-8 percentage points. In percentage terms, effects are somewhat larger for younger children, from 44-54% for 7-9 year-olds, and 20-43% for 10-12 year-olds.

Panel B shows no evidence of differential effects on enrollment by gender, although the final columns suggest substantial gender differences in response to work. In blocks 1-2, the reduction in work outside the home is 11 percentage points for boys versus a statistically insignificant 2 percentage points for girls. For work only in the home, it is a marginally significant reduction of 3 percentage points for boys, and 9 for girls, again in blocks 1-2. In the sample of eligible children in G3 or G4 of blocks 1-2, the rates of work are similarly reversed for each variable, with 20% of boys working outside the home (versus 5% for girls), and 22% of girls working only inside the home (versus 9% for boys). Thus, percentage changes are relatively similar in each case. Panel C show little consistent evidence that ethnicity plays a role in mediating treatment effects. The only notable effect is somewhat larger effect on reducing work in the home among non-Lenca children (p-value=0.04).

Panel D tests a basic hypothesis regarding program eligibility. 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 effects should be attenuated if children have a reduced likelihood of receiving a transfer within a larger household. The coefficient in column (1) and (2) suggest that is the case for enrollment. In blocks 1-2, for example, the effect is 12 percentage points for children in larger families, versus 15 percentage points when 1-3 eligible children reside in the household (p-value=.01). There is no strong evidence of a similar difference for child labor variables.

Finally, Panel E assesses whether the effects on children eligible for the education transfers is 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 smaller among eligible children in families without small children (7 percentage points versus 8.6; p-value=0.05), and also for child work. In blocks 1-2, however, these differences are attenuated in percentage terms and rendered statistically insignificant. The enrollment effects are 13.9 versus 15.5 percentage, with a p-value of 0.2).

D. Ineligible Children and Adults

We next assess whether individuals other than eligible children modify their behavior in response to the transfers. Table 4 limits the sample to children ages 6-12 who are ineligible for education transfers by virtue of already having completed fourth-grade. As expected, the sample contains no children ages 6-8 and less than 5% are 9 year-olds. To partly assess whether spillover effects occur within families or through another mechanism, we further identify ineligible children who reside in a household (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.

In panels A, B, and C, the full-sample estimates in odd columns uniformly 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 declines (panel B) among ineligible children in block 1. The magnitude of the enrollment effect is about one-third the size of the eligible sample's, and comparable or somewhat smaller for child labor. The relative magnitudes of point estimates across samples suggest that it is not driven by the presence other eligible children in the household. Besides spillover effects, a plausible explanation is that program administrators subjectively loosened grade-related eligibility requirements for age-eligible children in the very poorest municipalities. We have no direct evidence on this point. Other explanations, such as census measurement error in school variables, might imply that we might also observe effects in block 2, but that it not the case. Whatever the explanation, it is fair to conclude that evidence on spillovers is far less compelling than reported findings of Bobonis and Finan (2009) in Mexico.

Table 5 reports estimates of labor supply regressions among male and female adults, again dividing samples by the presence or absence of eligible children in the household. In the full sample, the only statistically significant findings reveal a small increase, among males, in the probability of working only in the home. However, it is quite stable across samples, even when there are no children in the household eligible for health or education transfers. Among males in block 5, there is a reduction of 5 percentage points in work outside the home, a small reduction of just over 5 percent. There is an increase of 1.5 percentage points in work inside the home; this is only evident when eligible children are present in the household. Overall, the results provide no evidence of effects on female labor supply, and weak evidence of small effects on male labor supply in a subsample of the data. With the exception of Nicaragua, these findings are consistent with the broader literature on conditional cash transfers in Latin America (Fiszbein and Schady, 2009; Maluccio and Flores, 2005).

E. Regression Discontinuity Using the Original Targeting Rule

The prior sections suggested that enrollment and child labor effects in blocks 3-5 were zero (or at least small and statistically indistinguishable from zero). A rule-based discontinuity design identifies effects in the vicinity of the cutoff that bounds block 5. Thus, this section is largely an attempt to confirm a zero or small effect in relatively richer (but still poor) municipalities, using municipalities to the right of the cutoff as an alternate control group.

Figure 3 provides preliminary graphical evidence. In each panel, the lines are fitted values from local linear regressions of the y-axis variable on $\overline{HAZ}$ (re-centered such that 0 is the cutoff). 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 does increase sharply at the cutoff. Though over 0.2, it is notably fuzzy because of (1) random assignment conditional on falling below the cutoff, and (2) the use of a noisier assignment variable, $\overline{HAZ}$ (indeed, the fuzziness to the right of the discontinuity is entirely due to the latter). 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, perhaps, a small *increase* in work outside the home, but it remains to be seen whether this is robust and precisely estimated (a similar result holds for work only in the home). Both panels illustrate a tell-tale reversal of the slope on either side of the cutoff. Collectively, the panels suggest that the apparent absence of effects in richer blocks is robust to alternate control groups.

In Table 6, panel A report 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%.

While imprecision renders the analysis unconvincing as a stand-alone evaluation, 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.

F. Regression Discontinuity Using Municipal Borders

Table 7 reports results from the border discontinuity analysis. In panel A-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)-(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"). 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.

Panel D, E, and F repeat the same analyses 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.

Table 8 replicates regressions testing for heterogeneity by child attributes. With the exception of work in the home, the results largely replicate the pattern of findings in the experimental analysis. First, the effects are statistically significant in blocks 1-2, but not blocks 3-5. Second, increases in enrollment are relatively larger among younger children, and reductions in work outside the home are relatively larger among older children (panel A). Third, similar differences between boys and girls are observed for work variables, as in the experimental analysis (panel B). Fourth, unlike the experimental analysis, the Lenca and non-Lenca points estimates are different, with larger effects among indigenous groups (panel C). Fifth, effects on eligible children are attenuated when there are more than 3 eligible children in a household (panel D). Sixth, the effects are somewhat smaller in households without younger children (panel E), implying that income from health transfers plays a small role in explaining effects among children eligible for education transfers.

7. Conclusions

This paper reported a reanalysis of the Honduran PRAF-II experiment, using the 2001 census instead of the official evaluation data. PRAF-II awarded cash transfers, conditional on school enrollment, to children ages 6-12 who had not completed fourth grade. Cash transfers were applied in 40 randomly-chosen municipalities in an experimental sample of 70 poor municipalities, chosen because the mean height-for-age z-score of first-graders fell below a cutoff.

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. These effects are mainly accounted for 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. These represent increases of 16-32% in enrollment, and decreases of 50-55% in work outside the home, and 38-46% in work inside the home. On the other hand, we find minimal 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 (although the border-discontinuity design yields smaller point estimates).

The new results can also be compared to the randomized evaluation of Nicaragua's *Red de Protección Social,* also conducted during 2000-2002 (Maluccio and Flores, 2005). The program offered relatively more generous cash transfers, with similar conditionalities, 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 was similar (72%, compared with 65% in our data). Between 2000 and 2001, the program increased enrollment by 18.5 p.p. (26%) in the full evaluation sample, just over twice as larger as the Honduran estimates.[18]

The results highlight 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 program cost of US$3,430,330 from 1999 to 2001 (excluding costs of the randomized evaluation and transfer payments). The census shows that 77,500 children ages 6-12 had not completed fourth grade in G1 and G2, a cost per child of US$44. Part of these costs covered administrative costs of delivering health transfers. Since there are 58,692 children ages 0-3, we proportionately adjust downward the program cost per child eligible for education transfers to US$25. Given full sample results of 8 p.p. (12%) and block 1 results of 18 p.p. (32%), the results suggest cost-effectiveness ratios of $0.79-$2.10 for a one-percent gain. They

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

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: 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:									
*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:									
*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.

Table 4: 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 5: 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 6: 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 7: 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).

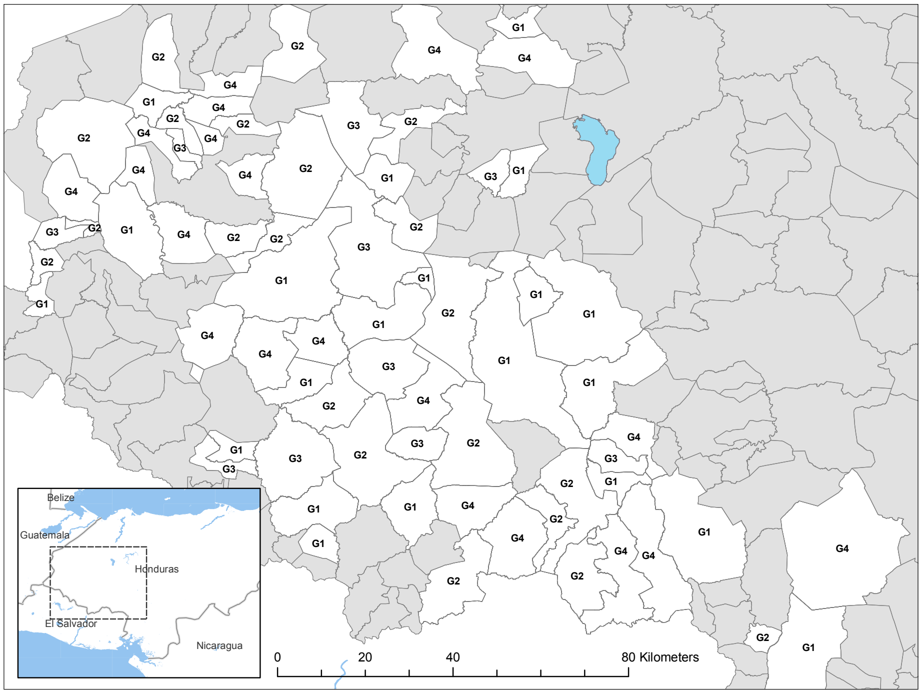
Table 8: Heterogeneity in border discontinuity effects among eligible children

	Dependent variable					
	Attends school		*Works outside home*		*Works only in home*	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Age						
*G1/G2 * Age 6*	0.075	0.047	--	--	--	--
	(0.066)	(0.033)				
*G1/G2 * Age 7*	0.122*	0.051	-0.097*	-0.002	-0.013	0.014
	(0.048)	(0.039)	(0.041)	(0.009)	(0.015)	(0.023)
*G1/G2 * Age 8*	0.125*	0.032	-0.103*	0.004	-0.027	0.011
	(0.050)	(0.038)	(0.039)	(0.010)	(0.019)	(0.019)
*G1/G2 * Age 9*	0.068	0.028	-0.070*	0.000	-0.030	0.000
	(0.045)	(0.031)	(0.033)	(0.010)	(0.021)	(0.016)
*G1/G2 * Age 10*	0.090+	0.031	-0.097*	0.002	-0.038+	0.002
	(0.050)	(0.030)	(0.042)	(0.013)	(0.021)	(0.015)
*G1/G2 * Age 11*	0.101*	0.057*	-0.110*	-0.005	-0.020	-0.027+
	(0.045)	(0.026)	(0.043)	(0.013)	(0.025)	(0.016)
*G1/G2 * Age 12*	0.071	0.031+	-0.147**	0.016	0.003	-0.008
	(0.058)	(0.018)	(0.053)	(0.018)	(0.032)	(0.015)
p-value	0.085	0.177	0.057	0.697	0.461	0.035
Panel B: Gender						
*G1/G2 * Female*	0.092*	0.039	-0.058*	-0.003	-0.041	0.003
	(0.042)	(0.029)	(0.027)	(0.009)	(0.031)	(0.022)
*G1/G2 * Male*	0.101*	0.040	-0.139*	0.006	-0.005	0.000
	(0.046)	(0.029)	(0.051)	(0.016)	(0.011)	(0.012)
p-value	0.674	0.954	0.032	0.627	0.264	0.880
Panel C: Ethnicity						
*G1/G2 * Lenca*	0.138*	0.079	-0.119**	0.004	-0.027	0.000
	(0.056)	(0.053)	(0.043)	(0.022)	(0.026)	(0.023)
*G1/G2 * Not Lenca*	0.078+	0.034	-0.090*	0.001	-0.021	0.002
	(0.039)	(0.026)	(0.035)	(0.009)	(0.014)	(0.015)
p-value	0.054	0.244	0.075	0.903	0.712	0.914
Panel D:						
*G1/G2 * 1-3 children*	0.101*	0.042	-0.100*	0.002	-0.023	0.001
eligible	(0.043)	(0.028)	(0.037)	(0.009)	(0.017)	(0.016)
*G1/G2 * ≥4 children*	0.049	0.017	-0.092*	-0.000	-0.024	0.005
eligible	(0.045)	(0.032)	(0.037)	(0.013)	(0.020)	(0.015)
p-value	0.006	0.039	0.528	0.742	0.913	0.699
Panel E:						
*G1/G2 * ≥1 child 0-3*	0.109*	0.043	-0.103*	0.001	-0.024	0.002
	(0.046)	(0.031)	(0.040)	(0.011)	(0.019)	(0.019)
*G1/G2 * No child 0-3*	0.075+	0.035	-0.094**	0.003	-0.021	0.002
	(0.038)	(0.027)	(0.034)	(0.009)	(0.015)	(0.012)
p-value	0.027	0.509	0.558	0.702	0.765	0.993
Sample	Blocks 1-2	Blocks 3-5	Blocks 1-2	Blocks 3-5	Blocks 1-2	Blocks 3-5
N	18182	51658	14845	42686	14845	42686

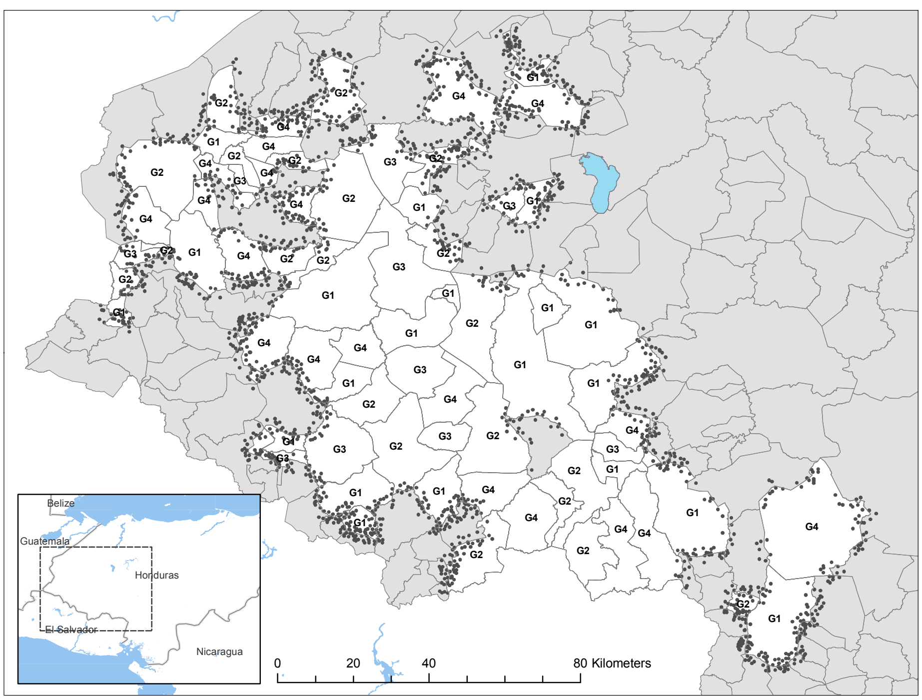
Notes: *** indicates statistical significance at 1%, ** at 5%, and + at 10%. Robust standard errors are in parentheses, adjusted for municipal-level clustering. All regressions include fixed effects indicating border segments and geographic controls (see text for details). Optional controls include the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

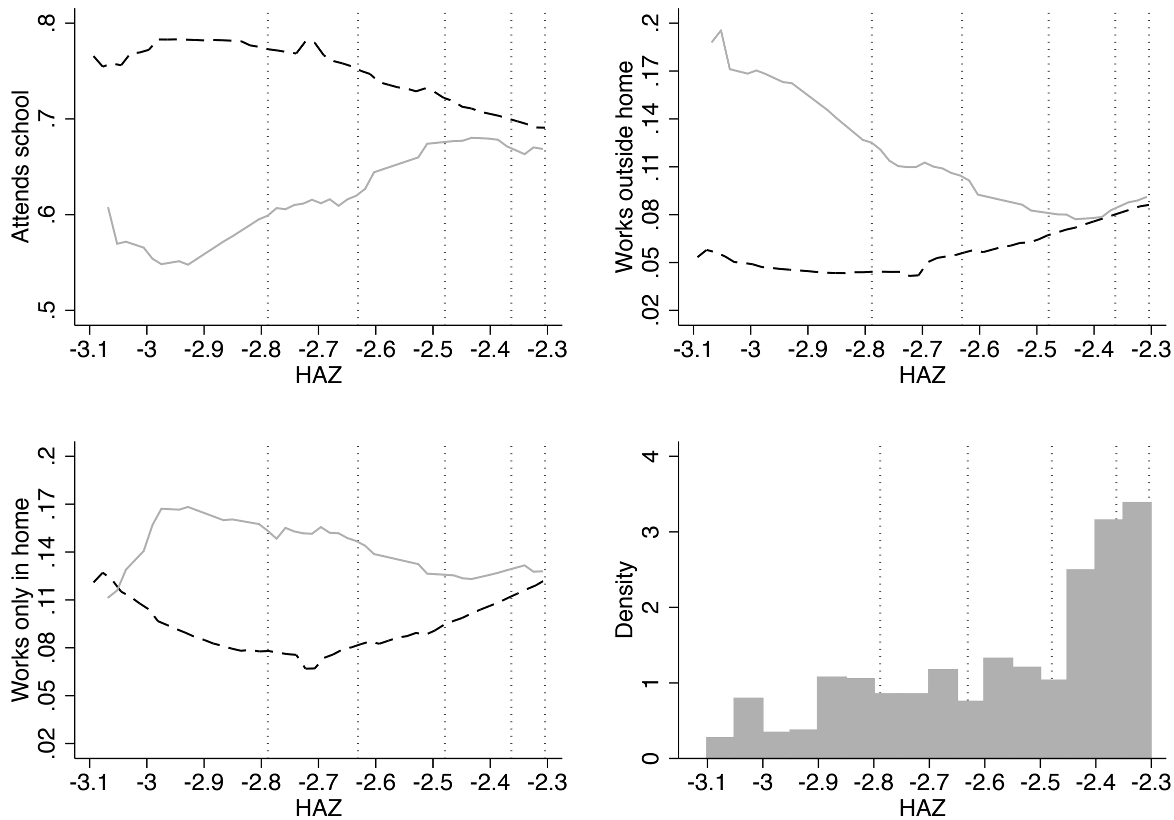


Panel B: *Caseríos* within 2 kilometers of municipal borders



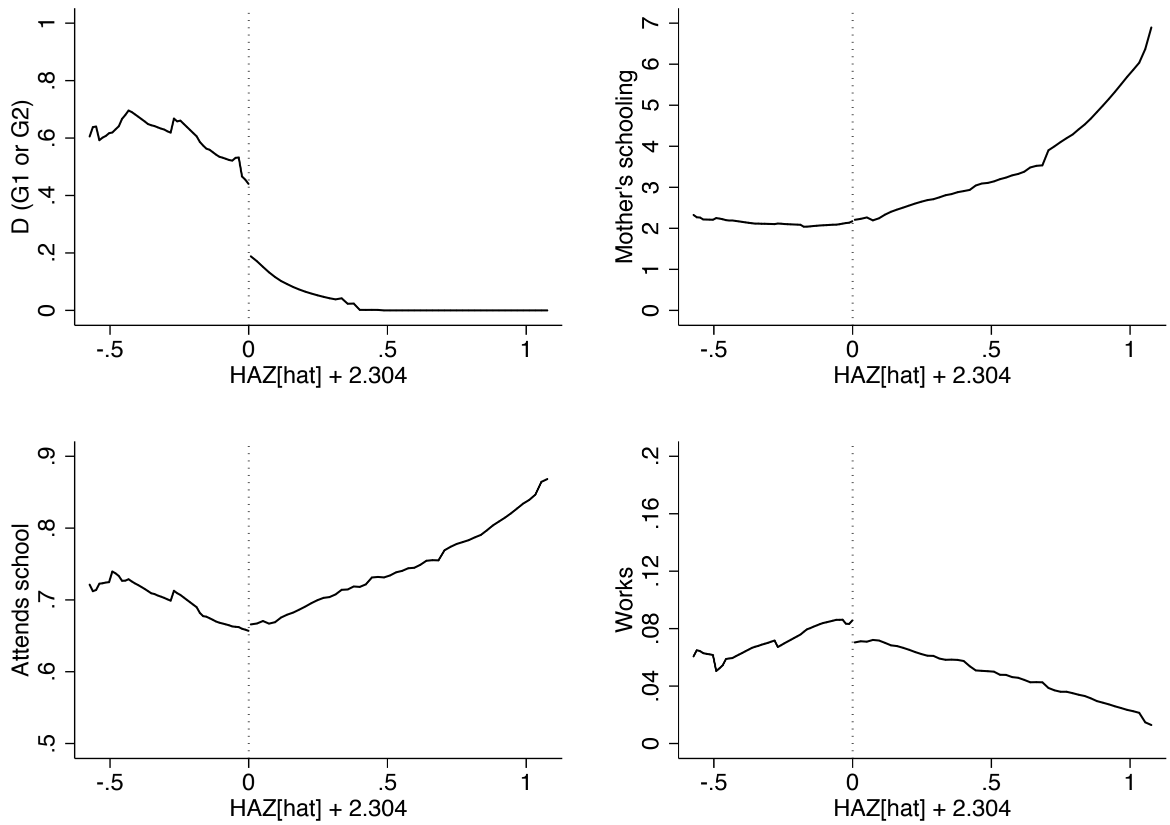
Notes: Unshaded municipalities were randomly assigned to receive cash transfers (G1), to receive transfers and direct investments (G2), to receive direct investments (G3), or to receive no treatment (G4). See text for details. Dots in panel B represent *caseríos* within 2 km of the municipal border.

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)
<u>Dependent variables</u>	
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.
<u>Independent variables</u>	
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.