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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, within five strata defined by a poverty proxy. Using census data, we show that eligible children were 8 percentage points more likely to enroll in school and 3 percentage points less likely to work. The effects were much larger in the two poorest strata, and statistically insignificant in the other three (the latter finding is robust to the use of a separate regression-discontinuity design). Heterogeneity confirms the importance of judicious targeting to maximize the impact and cost-effectiveness of CCTs. There is no consistent evidence of effects on ineligible children or on adult labor supply.

1. Introduction

Conditional cash transfers (CCTs) have been extensively adopted in the last decade, especially in Latin America (Adato and Hoddinott, 2011; Fiszbein and Schady, 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).

Randomized experiments in Latin America consistently find that poor, school-aged children eligible for a CCT are more likely to enroll in school and to complete more grades (Behrman and Parker, 2011; Fiszbein and Schady, 2009).[1] The increased school attainment is accompanied by declines in child labor supply (Edmonds and Schady, 2012).[2] This paper conducts a new analysis of the impact of a Honduran CCT on child enrollment and work. Between 2000 and 2002, the *Programa de Asignación Familiar* (PRAF) implemented two cash transfers: (1) an education transfer of about US$50 per year, for each child between 6 and 12 who enrolled in and regularly attended grades 1 to 4; and (2) a health transfer of about US $40 per year for each child under 3 or pregnant mother who regularly visited a health center. Of 298 Honduran municipalities, a randomized experiment included 70 with the lowest mean height-for-age z-scores, a proxy of municipal poverty (IFPRI, 2000). The 70 municipalities were divided into 5 quintiles based on mean height-for-age, and 8 of 14 municipalities in each quintile were randomly selected to receive transfers.[3]

This paper uses the 2001 Honduran Census, rather than the official evaluation data.[4] The census was conducted 8 months after the first transfer was distributed and just weeks after the second round of

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[1] The Progresa 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., 2009, 2011). The Nicaraguan RPS 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, a CCT 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. Finally, a CCT targeted at poor urban adolescents in Bogotá increased attendance and re-enrollment in secondary school (Barrera-Osorio et al., 2011).

[2] Skoufias and Parker (2001) found that Progresa reduced work among 12–17 year-old boys by 3–5 percentage points and 2 percentage points among girls. The Nicaraguan experiment found declines of 3–5 percentage points (Maluccio and Flores, 2005). In the Ecuadorean experiment, paid and unpaid work declined by 10 and 19 percentage points, respectively, among adolescents (Edmonds and Schady, 2012). Finally, the Colombian experiment found that hours worked declined by a third (Barrera-Osorio et al., 2011).

[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).

[4] Using the official evaluation data, Glewwe and Olinto (2004) analyze child school and work outcomes; we discuss their findings in Section 5. Morris et al. (2004) analyze health outcomes, finding statistically significant effects of CCTs on the frequency of antenatal care, recent health center check-ups and growth monitoring. Measles and tetanus immunization were not affected. Stecklov et al. (2007) find that CCTs produced large increases in births or pregnancy in the past year (measured in 2002), which they attribute this to the per-capita health transfer for pregnant women and young children. Alzúa et al. (forthcoming) find no effects of CCTs on adult labor supply.

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transfers. Using individual census data matched to municipal-level treatment data, 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 further show that it decreases the proportion of children who work outside the home by 3 percentage points (or 30%), and decreases the proportion who work inside the home 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. 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 work outside the home are 8 and 5 percentage points and, on work 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. Strikingly, the effects in three richer (but still poor) strata are statistically indistinguishable from zero. To assess the robustness of the latter finding, we leverage the regression-discontinuity design implied by the formula used to select the 70 experimental municipalities. Though imprecise, the point estimates are consistent with the absence of effects in the "richest" stratum.

Other research tends to find larger effects on enrollment when eligible children are in poorer households.[5] However, it is important to note that our main findings of treatment heterogeneity are based on a feature of the original stratified design, addressing concerns about potentially arbitrary subgroup analysis using experimental data (Deaton, 2010). Collectively, the results highlight the importance of carefully choosing proxy indicators to identify and target the poor (Coady et al., 2004; Alatas et al., 2012; De Wachter and Galiani, 2006).

Second, 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. A modest improvement in enrollment occurs in just the poorest stratum, but this could be attributed to lax enforcement of grade-completion requirements for eligibility. The finding contrasts with the relatively large positive spillovers on secondary school enrollment of children in ineligible households in the Progresa experiment (Bobonis and Finan, 2009).[6] It is important to note, however, that Progresa transfers were much larger: 27% of pretransfer consumption vs. 7% in Honduras (Fiszbein and Schady, 2009). We also find no evidence that CCTs affected adult female labor supply. A modest impact on adult male labor supply is confined to the two richer strata and is not replicated by the discontinuity design.

Third, and not least, the paper provides a rare opportunity to replicate the results of a social experiment using a new source of data. Using a household survey collected in late 2002, Glewwe and Olinto (2004) found that school enrollment in 2001 was 7 percentage points higher in municipalities treated with CCTs (see Section 5.1). Unlike this paper's results, they found that child work was only 0.5 percentage points lower in CCT municipalities, and statistically insignificant. However, the confidence interval is consistent with reductions even larger than those reported in this paper. Alzúa et al. (forthcoming) also analyzed the household survey and, like this paper, found no effects on adult labor supply. Our paper is the first to consider the issue of heterogeneity across the experimental strata, as well as spillover effects on ineligible children.

Section 2 of the paper provides background on PRAF-II and the CCT treatment, as well as its randomized assignment. Section 3 describes features of the 2001 census data, while Section 4 describes the empirical strategy. Section 5 describes the empirical results, and Section 6 concludes.

2. PRAF in Honduras

2.1. Background

The *Programa de Asignación Familiar* (PRAF), or Family Allowances Program, started in the early 1990s.[7] 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). 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).

2.2. PRAF-II treatments

PRAF-II implemented two kinds of cash transfers. The education transfer, in the amount of 800 Lempiras per year (about US$50), was available to each child between 6 and 12 who enrolled in and regularly attended grades 1 to 4 between the school year of February and November.[8] Children were not eligible if they had already completed fourth grade. A health transfer of 644 Lempiras per year (about US$40) was available to children under 3 and pregnant mothers who regularly attended health centers. Households were eligible to receive up to 3 education transfers and up to 2 health transfers. In practice, Glewwe and Olinto (2004) report that education enrollment (but not attendance) was enforced as a conditionality. Although families regularly deposited health center attendance slips, no health beneficiaries were suspended for failure to attend health centers (Morris et al., 2004). During the two years of implementation, transfers were distributed in November 2000, May-June 2001, October 2001, and late 2002 (see Fig. 1). The transfers were locally distributed as cash by PRAF personnel (Moore, 2008).

Just before PRAF-II was implemented, the median annual expenditure per capita of households in experimental municipalities was 3846 Lempiras, which was well below the extreme poverty line of 6462 Lempiras per year, or about US$1.20 per day (IFPRI, 2000). The headcount ratio was 71%. Using census data from 70 experimental municipalities, in concert with eligibility rules, we further estimated that the average household was eligible for 1,127 Lempiras annually, or 182 Lempiras per capita.[9] This is only about 5% of the median per capita expenditure, on the low side of other CCT programs in the

[5] For a review of theory and evidence, see Fiszbein and Schady (2009). For related empirical evidence, see Maluccio and Flores (2005), Filmer and Schady (2008), and Oosterbeek et al. (2008).

[6] Barrera-Osorio et al. (2011) find that untreated siblings in a Colombian program actually have lower attendance and enrollment.

[7] For details on PRAF-I and its successors, see BID (2004) and Moore (2008).

[8] Our description of the treatments relies on Morris et al. (2004). Other sources report very similar but not identical amounts for the demand-side transfers (Caldés et al., 2006; Glewwe and Olinto, 2004; IFPRI, 2000; BID, 2004).

[9] This may understate the amount because the census does not include data on one eligibility criterion: whether women are currently pregnant.