

The impact of (unconditional) cash transfers on school enrolment: Evidence from Ecuador*

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ABSTRACT. Evaluations of conditional cash transfer programs in several Latin American countries indicate that such programs have substantial positive effects on school enrolment. It is unclear, however, whether it is the cash transfer itself, or the conditionality that matters most. This paper presents fresh evidence from a cash transfer program in Ecuador. Unlike programs in other countries, the transfers are **un**conditional. Using a regression discontinuity design, we find a precisely estimated zero effect of eligibility on school enrolment. This suggests that the success of other programs should be attributed to the requirement that children attend school.

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Key words: cash transfers, school enrolment, regression discontinuity

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1. Introduction

Conditional cash transfer programs provide cash transfers to poor families conditional on the children of these families attending school and/or visiting health care centers. The attractiveness of these programs is the potential to combine short-term and long-term poverty reduction. The cash transfers reduce short-term poverty, while long term poverty will be reduced if children of poor families acquire human capital.

A number of countries in Latin America have implemented conditional cash transfer programs to combat poverty. Countries that have adopted such programs include Brazil (in 1995), Mexico (1997), Honduras (1998), Nicaragua (2000), Costa Rica, Colombia (2001), Argentina, Uruguay, Chile and Jamaica. Rawlings and Rubio (2003) and Caldés et al. (2004) provide overviews of the various programs.

Some of these programs have been assessed through impact evaluation studies. These studies show substantial positive effects of conditional cash transfers on school enrolment. The programs in Mexico and Nicaragua have been evaluated using randomized field experiments. In Mexico enrolment rates at the secondary level increased from 67% to around 75% for girls and from 73% to around 78% for boys (Schultz 2004). In Nicaragua the program was targeted to pupils up to fourth grade in primary school. The program increased the enrolment rate for this group by 18 percentage points (Maluccio and Flores 2004).¹

Other programs have been evaluated using non-experimental research designs. Duryea and Morrison (2004) used propensity score matching to evaluate the program in Costa Rica, and find a 5 to 9 percentage points increase in the probability of attending school. Attanasio et al. (2006) have evaluated the program in Colombia using propensity score matching in a difference-in-differences framework. They find an increase in school enrolment of 5 to 7 percentage points for 14 to 17 years old.

Given these successes of conditional cash transfer programs, one may ask whether it is the cash transfer itself that enhances school enrolment, or that the requirement that children attend school is the driving force. If the cash transfers themselves are sufficient, resources can be saved by abandoning costly monitoring of school attendance. Moreover, such a finding shows the importance of liquidity constraints for school enrolment. On the other hand, if cash transfers do not matter, this indicates that liquidity constraints are not the source of low school attendance. Finally, if families behave differently under conditional and unconditional cash transfer

¹ The program in Honduras will also be evaluated through a randomized field experiment. Results are not yet available.

programs, this indicates that the government reduces families' welfare by making the cash transfers conditional. This is only justified if families behave sub-optimally.

De Brauw and Hoddinott (2007) attempt to disentangle the cash transfer from the school attendance requirement by exploiting the fact that some treated families in Mexico did not receive the forms needed to monitor the attendance of their children at school. They find that the absence of such forms reduced the likelihood of children attending school, suggesting that the requirement matters. Since the reason for not receiving forms is unknown, it is unclear whether the two types of families can be compared.

This paper takes a different approach. We investigate the impact of the cash transfer program in Ecuador using a regression discontinuity design. Unlike the programs implemented in other countries, this program does not require children of treated families to attend school. We assume that if the program in Ecuador would have been a normal conditional cash transfer program, it would have produced effects similar to those in other Latin American countries. This implies that if we find that the unconditional cash transfers in Ecuador have effects of the same magnitude as the conditional cash transfers in other countries, we interpret this as the school attendance requirement having no effect. Likewise, if we find that unconditional cash transfers have no impact on school enrolment then we conclude that all effects of conditional cash transfers should be attributed to the school attendance requirement.

At the start of the program in Ecuador some television programs mentioned the obligation of parents to send children to school in order to receive the transfer. The obligation was, however, never put into practice. Schady and Araujo (2006) use individuals' unawareness of the absence of the requirement to identify the effect of the requirement and find a positive effect. It is questionable, however, whether badly informed families are comparable to others. Moreover, unawareness depends on children's school attendance. If a child does not attend school, the parents learn that this is not a requirement for the cash transfer, introducing a reversed causality.²

The remainder of the paper is organized as follows. The next section describes the program in Ecuador in more detail and provides information about the specific context. Section 3 describes the empirical approach adopted in this paper. Section 4 describes the data. Section 5 presents and discusses the empirical results. Section 6 summarizes and concludes.

² Studies that take an entirely different – structural – approach to disentangle the effects of the cash transfer and of the conditionality include Attanasio et al. (2005), Bourguignon et al. (2003), De Janvry and Sadoulet (2006) and Todd and Wolpin (2003). All these studies conclude that the conditionality explains the bulk of the effects.

2. Program and context

Ecuador is a lower-middle income country, characterized by high poverty levels and high inequality. During the last decades education levels have gone up. For example, between 1982 and 1990, enrolment increased from 68.6% to 88.9% for primary schools and from 29.5% to 43.1% for secondary schools. Moreover, the average number of years of schooling of the population aged 24 years or older increased from 6.7 to 7.3 between 1990 and 2001. Despite these improvements, the country faced a serious problem with school enrolment during the 1990s. In 2001 enrolment at primary and secondary levels stagnated around the values of 1990. This disappointing performance contrasts with aspirations. The 1990s was the decade of “Education for All”, and Ecuador subscribed to several international declarations emphasizing the importance of education. In addition, at the end of the 1990s, the Ecuadorian government engaged in new programs aiming to improve access to primary education and school achievements. Paradoxically, educational inputs showed remarkable improvements during the same period. The pupil-teacher ratio for primary education declined from 30 in 1990 to 23 in 2001.

In 1998 the government in Ecuador launched a program called *Bono Solidario*. This program started as a safety net to compensate poor families for the elimination of gas and electricity subsidies. Initially the program used a self-targeting strategy directed at mothers with earnings below USD 40, people with disabilities and senior citizens. While the immediate political justification for this program was to compensate the poor for losses in their purchasing power caused by statutory increases in (heavily subsidized) petroleum and natural gas prices, the program quickly took on a life of its own, becoming the government's largest social expenditure outside of education, with total transfers equal to about one percent of the GDP (Vos et al., 2001). The transfer was modest, but non-trivial by Ecuadorian standards. At the time that the program started, mothers received about USD 15 per month, and senior citizens and people with disabilities received USD 7.50. On average, the share of *Bono* income in total household expenditures was 11 percent in 1999. During 2000, the program reached around 1.2 million beneficiary households, representing about 45 percent of Ecuadorian households.

Vos et al. (2001) evaluated *Bono Solidario* using propensity score matching. They report a positive impact of around 5 percentage points on school enrolment, although no significant impact was found on poverty indicators. Using an instrumental variables approach, León and Younger (2004) find that the program had very minor, yet significant positive effects on children's nutritional status. As instruments they use dummy variables for whether the household has people of retirement age, has a worker in the formal sector, has a mother of children younger

than 18 and a measure of the time that it takes to reach the bank branch, where the *Bono* is collected.

At the end of the 1990s the government implemented another program called *Beca Escolar*. This program consisted of transfer of USD 5 per child (up to two children per household) conditional on these children to enroll in school and attend at least 90% of the school days. This program has never been evaluated.

In 2003 the two programs were reformulated and incorporated under a new program called *Bono de Desarrollo Humano* (BDH). The main objective of this program is to improve the formation of human capital among poor families in Ecuador. The program has two components: education and health. The education component aims at children from the ages of 6 to 15 to enroll in school and attend at least 90% of the school days. The health component aims at children under 6 years old to attend health centers for medical check-ups. Unlike other programs in Latin America, up until 2006 the program had no mechanisms to verify attendance in school and in health care centers. Families are not taken off program rosters if their school-aged children are not enrolled in school or fail to attend classes regularly. Consequently, the program is best characterized as an unconditional cash transfer program instead of a conditional cash transfer program.

BDH uses an individual targeting strategy to select beneficiaries based on a poverty index. This index identifies potential beneficiaries of social programs by classifying families according to their unmet basic needs. The poverty index is computed using non-linear principal components analysis. Families pertaining to the poorest two quintiles receive the benefit. Currently, the program consists of a cash transfer of USD 15 per family per month. The annual budget of the program reached USD 190 million in 2004 (around 1% of GDP).

3. Empirical approach

When implementing the cash transfer program, the government of Ecuador decided to evaluate the program's impact through a regression discontinuity design. The initial design of the program established two different amounts: USD 15 for families in the lowest quintile and USD 11.5 for in the second quintile.³ The difference around the 40th percentile can be exploited to estimate the impact of the cash transfer per se, while the difference around the 20th percentile can be exploited to estimate the impact of different amounts of the cash transfer.

³ The cutoff point between these quintiles on the poverty index was 42.87, while the cutoff point between the second and third quintiles was 50.65.

Once the research was designed and the baseline survey was conducted, the government decided to grant all families in the bottom two quintiles US\$ 15. Due to this, the design no longer permits evaluation of the impact of different amounts of the transfer. Instead it was decided to use a randomized design to evaluate the impact of those around the 20th percentile of the poverty index. Potential beneficiaries around this point were randomly assigned to treatment and control. Schady and Araujo (2005) use this experimental design for their evaluation. We discuss their findings in more detail below where we compare them to our findings.⁴

This paper exploits the remaining of the original evaluation design, namely the discontinuity around the 40th percentile, in a regression discontinuity design. The identifying assumption is that conditional on a flexible function of the poverty index and other observables, eligibility for treatment is random for families with a poverty index close to the 40th percentile. More formally, we will estimate equations of the following type using instrumental variables.

$$Y_{i,t} = X_{i,t-1}\beta + f(P_{i,t-1}) + \delta T_{i,t} + u_{i,t} \quad (1)$$

Where Y is school enrolment which takes a value of 1 if a child is enrolled and 0 otherwise, X is a vector of individual, household and community level characteristics, $f(P)$ is a flexible function (a third degree polynomial) of the poverty index, T is an indicator variable taking the value of 1 if the person receives the treatment and 0 otherwise, and u the error term. Subscript i indicates the child, t indicates the time period when the follow-up survey was conducted, $t-1$ refers to the baseline period.

In a standard regression discontinuity design one compares observations just below and just above the cutoff. We do this by restricting the analysis to observations that have their poverty index within a certain range around the cutoff. Widening this range increases the number of observations, but makes at the same time the treatment and control group more different. By presenting results for different ranges around the cutoff we examine the sensitivity of our results in this regard.

It turns out that not all families that receive the transfer meet the poverty index requirement. Likewise not all families that meet this requirement received the transfer. This implies that the design is not a sharp regression discontinuity design but is instead a fuzzy design. There is not a deterministic relation between the poverty index and treatment but a probabilistic

⁴ The two papers by Schady and Araujo (2005, 2006) have almost identical titles and have much overlap. It is unclear whether these are two versions of the same paper or two different papers. The 2005 version/paper reports results on child labor and heterogeneous treatment effects. The 2006 version/paper focuses in more detail on the awareness of the unconditionality.

one. To address this we apply an instrumental variables approach where receipt of the cash transfer is instrumented by eligibility. This means that we will estimate a first stage equation in which the endogenous variable T in equation (1) is instrumented by the dummy variable eligibility (Z), which takes value 1 if the poverty index is below the cutoff and 0 otherwise. The identifying assumption is then that $E(Z_{i,t} \cdot u_{i,t} | X_{i,t-1}, P_{i,t-1}) = 0$.

Since we have pre-intervention and post-intervention measures of outcomes, we can also combine the regression discontinuity design with a first difference approach. To this end we estimate equations of the following form:

$$\Delta Y_{i,t} = X_{i,t-1} \beta_{\Delta} + f_{\Delta}(P_{i,t-1}) + \delta_{\Delta} T_{i,t} + \Delta u_{i,t} \quad (2)$$

Where ΔY is the change in school enrolment which takes a value of 1 if a child is enrolled at t and not enrolled at $t-1$, of 0 if the enrolment status is the same at t and $t-1$, and of -1 if a child is enrolled at $t-1$ but not at t . Specification (2) allows changes of Y to be affected by X and $f(P)$.

In addition to equations (1) and (2) we will also present results from reduced form estimations. These equations have a similar specification as equations (1) and (2), except that T is replaced by Z . The reduced form equations recover the effect of the intention to treat (ITT).

4. Data

To exploit the discontinuity in eligibility around the poverty index of 50.65, families with a poverty index between 47.65 and 53.65 were drawn in four out of twenty-two provinces in the country.⁵ The sampling scheme used a two-stage procedure. Within the provinces, parishes were randomly drawn and, within these parishes, a random sample of households was taken. Households who had previously received transfers from the *Bono Solidario* and the *Beca Escolar* programs were excluded to avoid contamination of the estimation results. Finally, the sampling scheme selected only households with at least one child aged between 6 and 15 years old at the time of the baseline survey.⁶

The survey includes one record for each household member including their gender, age and relation with the head of the household. The survey also contains information on the level of

⁵ These provinces are Carchi, Imbabura, Cotopaxi and Tungurahua, which are located in the *Sierra* (highlands) region.

⁶ The baseline survey was conducted between June and August 2003, the follow-up survey was carried out between January and March 2005.

schooling, the parents' level of schooling, marital status, and language spoken by all household members. For children aged between 5 to 17 years, the survey includes information on current enrolment (level and grade), causes in the case of no enrolment, and other variables related to the type of school the child attends, as well as some variables related to labor market status. Finally, the survey includes a complete module of household expenditures, which replicates the structure of the 1999 Ecuador LSMS. One important feature of the survey is that it includes all 27 variables that were used to construct the poverty index, as well as an indicator that takes the value of 1 if the person belongs to a household that receives the cash transfer, and 0 otherwise.

Attrition is low; 96% of the households interviewed at the baseline were interviewed again in the follow-up survey. No significant differences are found between households who were and were not interviewed. Attrition can introduce biases when correlated with treatment status (Angrist, 1997). A regression of an attrition indicator on treatment status has a coefficient of 0.0012 (s.e. 0.11), indicating that attrition will not bias our results.

The sample is restricted to children aged 5 to 17 years when they live in households that responded to the follow-up survey. This results in a sample of 2,384 children in 1,221 households.⁷ Table 1 presents descriptive statistics for eligible and ineligible children/households. It does this separately for two samples. Columns 1-3 pertain to the full sample of children/households who have a poverty index at most three points different from the 40th percentile cutoff. Columns 4-6 are for the restricted sample of observations whose poverty index is at most one point different from the cutoff.

Some of the variables listed in table 1 serve as an input in the construction of the poverty index or are highly correlated with the poverty index. This is the case for head of household being indigenous, log of per capita expenditures and parents' education. It is therefore not surprising that we find significant differences for these variables between the groups below and above the cutoff. This suggests that treatment and control groups in this research design may be too different to compare. Recall, however, that the identifying assumption of the regression discontinuity design is that there are no systematic differences between treatment and control groups conditional on covariates (including a flexible function of the poverty index). Hence, differences in observed characteristics need not invalidate the research design.

When we restrict the sample to observations no more than one point from the cutoff, the eligible and non-eligible groups become more similar on most variables. This is evidenced by the *p*-values in column 6. For the poverty index, per capita consumption, and head of household

⁷ Data on all key variables are available for all households in the sample, with the exception of parental education, which is missing in some cases.

being indigenous we still find (hardly surprising) significant differences. But these differences are reduced in size. The significant differences on parents' education have vanished. On the other hand, there appears now to be a significant difference in age between observations above and below the cutoff in the restricted sample. Omitting this variable as a control would bias the impact estimates upwards since older children are less likely to attend school.

The results in table 1 show that enrolment rates in Ecuador are around 0.85. Any impact estimate should be regarded relative to current enrolment rates, since there are obvious ceiling effects. Countries with similar enrolment rates in Latin America include: Brazil, Chile, Paraguay, Dominican Republic and Honduras.

5. Results

First stage

The first thing that we need to establish is the (first stage) effect of eligibility of the cash transfer on actual receipt (treatment) of it. Out of a total of 537 families that were not eligible, 41 (8%) received the cash transfer. Out of 684 families that were eligible, 178 (26%) didn't receive the cash transfer. Hence for 18% of the families, eligibility-status and treatment-status do not coincide.⁸

Figure 1 plots the relation between the poverty index, eligibility and the probability of treatment. The discontinuity in the probability of treatment at the eligibility cutoff is evident. Closely around the point where the poverty index equals 50.65, the probability of treatment drops by around 60 percentage points. Notice further that the relation between actual receipt and the poverty index is almost flat at both sides of the cutoff. This indicates that the probability of treatment is independent of the poverty index conditional on the eligibility index.

Table 2 shows these findings more formally for various specifications of the first stage relationship. The top panel contains the results for the full sample. Column (1) contains no control variables, column (2) adds controls for background characteristics (see table 1), and column (3) adds a third degree polynomial of the poverty index. Even in this latter specification, the coefficient of eligibility status is not lower than 0.64, and is always very significantly different from zero. The F-value for the instrument is never below 148. The flatness of the relation between treatment and poverty index at both sides of the cutoff is expressed by the low

⁸ In comparison, in the (experimental) data analyzed by Schady and Araujo (2005) the comparable percentage equals 31%.

F-value for a joint test on the significance of the three poverty index terms. We cannot reject the hypothesis that conditional on other variables, the joint effect of these three terms equals zero.

The bottom panel of table 2 reports results for the same first stage specifications when the sample is restricted to children in families that are no more than 1 point from the poverty rate cutoff. Point estimates are very similar to those for the full sample: effects are very significant, F-values for significance of the instrument are never below 97, and the poverty index (polynomial) has - conditional on eligibility status - no significant impact on treatment.

Reduced form

Table 3 shows the reduced form results for the full sample. We present results for different specifications corresponding to those in the previous tables. The top panel reports results for the levels specification, while the bottom panel reports results from specification in which the dependent variable is measured in first differences. In all specifications the point estimates are small and never significantly different from zero.

Going from the first to the last columns we observe that adding more control variables makes the point estimate less negative or more positive. Differences between the point estimates in the different columns are, however, insignificant. For the results in the final columns we tested for the joint significance of the poverty index polynomial. We cannot reject that the joint effects of these three terms equals zero. On the basis of efficiency considerations, we should therefore prefer the results in column (2). The standard error on the impact estimate in that column is substantially smaller than the standard error on the impact estimate in the final column.

Also for the restricted sample, none of the estimates in table 4 differs significantly from zero. In this sample, adding more control variables makes the estimated impacts less positive or more negative. Like in the larger sample, we cannot reject the hypothesis that the joint effects of the three poverty index terms equals zero. Hence, for reasons of efficiency we prefer the results in the second column to those in the third.

Figure 2 illustrates the reduced form without any controls using data from the full sample. There appears to be no impact of eligibility status on school enrolment. This confirms the findings from tables 3 and 4.

In the current application the reduced form results have a clear policy interpretation. These estimates show the effect on the group that the program was intended to serve (the effect of the intention to treat). Our preferred estimates for the full sample give point estimates equal to 0.002 and -0.003 . These estimates are fairly precisely measured (se. 0.015). An increase in school enrolment as small as 3 percentage points can therefore be excluded with 95% probability as

impact estimate. Also for the restricted sample the intention to treat effects are small. The point estimates equal 0.022 (s.e. 0.025) and 0.007 (s.e. 0.024)

IV

Tables 5 and 6 report the IV results for the full sample and the restricted sample, respectively. Point estimates are equal to the reduced form estimates (in tables 3 and 4) divided by the first stage coefficient in the corresponding column (in table 2). None of the impact estimates is significantly different from zero, implying that we cannot reject the hypothesis that receipt of the cash transfer has no impact on school enrolment.

For both samples we cannot reject that the joint effect of the three poverty index terms equals zero and therefore prefer the outcomes presented in the second columns. For the results obtained using the full sample this excludes – with 95% probability – that receipt of the cash transfer raises school enrolment by more than 4.6 (levels specification) and 3.9 (first difference specification) percentage points. For the restricted sample the respective figures are 10.4 and 8.2.

Our findings on the effect of the cash transfer can be compared with those reported by Schady and Araujo (2005). As mentioned before, these authors use data from an experiment in which potential beneficiaries around the 20th percentile were randomly allocated to treatment and control. They report a significantly positive average effect, but this effect is concentrated among the poorest in their sample. For instance, they report a significant effect equal to 0.066 (s.e. 0.022) for children from families with below 20th percentile (median in their sample) per capita expenditures and an insignificant effect equal to 0.012 (s.e. 0.022) for children from families with above 20th percentile (median in their sample) per capita expenditures. The children in our sample come from families around the 40th percentile of per capita expenditures. Taken together, this clearly suggests that the effect of the cash transfer is heterogeneous and is larger for poorer families. Our estimate of a zero effect should thus not be interpreted as an estimate of the average effect of the program but rather as an estimate of the effect for children from families close to the 40th percentile cutoff.

What did they do with the cash?

The results presented so far establish that the unconditional cash transfers in Ecuador do not have a significant impact on school enrolment. In this subsection we address the question how the families that received the cash transfer spent it. This is relevant for its own right. Is the transfer spent in a way that also (potentially) benefits the children in the families? Moreover, by looking

at alternative outcomes we examine whether the research design applied in this paper is able to detect any impact.

Table 7 reports results for five spending categories, separately for the full sample and the restricted sample and for levels and changes specifications. All effect estimates are obtained from specifications that also include the full set of background characteristics and a control for the poverty index. The results for the restricted sample reveal that receipt of the cash transfer leads to more food expenditures and more school related expenditures.⁹ Food expenditures go up by 25 percentage points, school expenditures by 73 percentage points. While the cash transfer does not increase school attendance, it better equips those who attend. Part of the cash transfer is thus spent in a way that potentially raises children's human capital.

6. Summary and discussion

Various evaluation studies of cash transfer programs in Latin America that condition receipt of the transfer on children attending school, all find that such programs have substantial positive effects on school enrolment. The evidence comes both from studies that use data from randomized field experiments as well as from studies that use non-experimental designs.

This paper evaluates the effects of a cash transfers program in Ecuador where receipt of the transfers does not depend on children attending school. The design of the program includes a regression discontinuity. Families with a score on a poverty index equal to or below the 40th percentile are eligible for the transfers; families with a score above the 40th percentile on that index are not eligible. Although eligibility does not perfectly predict actual receipt of the cash transfer, there is a sharp drop in the probability to receive treatment at the 40th percentile. We exploit this feature of the program's design to instrument receipt of treatment.

We find a rather precisely estimated zero effect of the cash transfer on school enrolment. Combined with the evidence from another study from Ecuador that looks at the effects for groups close to the 20th percentile, this suggests that the effects of the cash transfer on school enrolment is heterogeneous and increases with poverty. Our estimate of a zero effect should thus not be interpreted as an estimate of the average effect of the program but rather as an estimate of the effect for children from families close to the 40th percentile cutoff. The policy implication of our zero effect finding is therefore not that the program should be abandoned altogether but rather that it should be considered to lower the eligibility threshold.

⁹ School related expenditures include transportation, uniforms, tuition fees, textbooks and other school materials, and parents' contributions to school expenditures.

This estimated impact of unconditional cash transfers in Ecuador contrasts with the estimates in previous studies of the impact of conditional cash transfers in other Latin American countries. The study of Maluccio and Flores (2004), which is based on a randomized experiment in Nicaragua, finds effects on enrolment equal to 0.26 for children from the 21 percent poorest families (extremely poor), equal to 0.12 for children from families from the next 24 percent poorest families (poor), and equal to 0.05 for children from families between the 45th and 66th percentile on the poverty scale. Our estimate of zero for families around the 40th percentile can probably best be compared with their estimates of 0.12 and 0.05. The differences in effects is suggestive evidence that not the cash transfer itself but the requirement to send children to school is the driving factor for the success of these programs.

De Brauw and Hoddinott (2007) and Schady and Araujo (2006) have used other approaches to disentangle the effects of the cash transfer per se and the requirement that children attend school. Both studies impose a conditional independence assumption that can be questioned. Both paper, however, also reach the same conclusion as we do, namely that the conditionality is decisive.

The fact that families behave differently under conditional cash transfers than under unconditional cash transfers implies that families reach higher utility levels without the conditioning. The requirement that children should attend school is therefore only justified if there is a clear belief that families behave sub-optimally.

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Figures

Figure 1: First stage relation

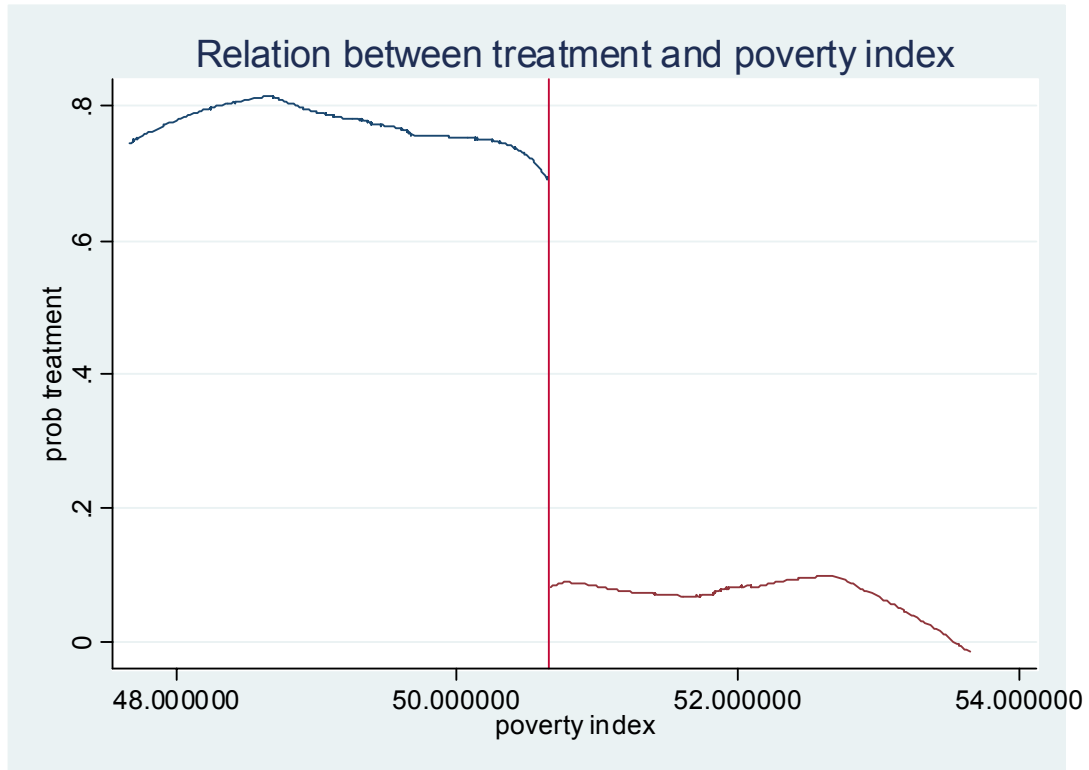


Figure 2: Reduced form relation

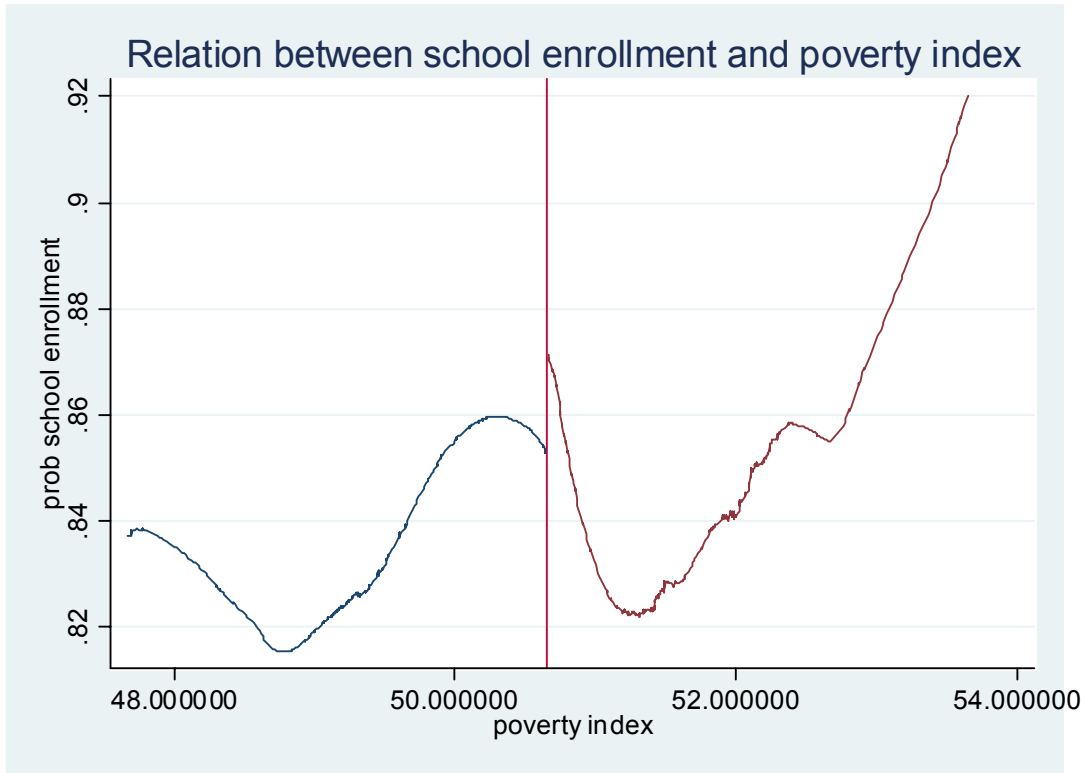


Table 1: Descriptive statistics by eligibility status, different discontinuity samples

| Variable | <i>DS±3points</i> | | | <i>DS±1point</i> | | |
|--------------------------------------|-------------------|--------------|---------|------------------|--------------|---------|
| | Eligible | Not eligible | p-value | Eligible | Not eligible | p-value |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| School enrolment pre intervention | 0.85 | 0.86 | 0.625 | 0.87 | 0.84 | 0.291 |
| Child's age | 11.91 | 12.00 | 0.498 | 11.82 | 12.31 | 0.009 |
| Child is female | 0.53 | 0.52 | 0.787 | 0.51 | 0.55 | 0.258 |
| Log of per capita expenditures | 2.92 | 3.07 | 0.000 | 2.94 | 3.01 | 0.034 |
| Poverty index | 49.42 | 51.88 | 0.000 | 50.17 | 51.11 | 0.000 |
| Father's education | 5.68 | 6.16 | 0.000 | 5.91 | 5.94 | 0.840 |
| Mother's education | 5.28 | 5.92 | 0.000 | 5.69 | 5.41 | 0.210 |
| Head of household is male | 0.85 | 0.87 | 0.307 | 0.84 | 0.83 | 0.562 |
| Head of household is indigenous | 0.09 | 0.06 | 0.002 | 0.08 | 0.04 | 0.025 |
| Head of household can read and write | 0.94 | 0.96 | 0.161 | 0.94 | 0.94 | 0.693 |
| Household size | 5.63 | 5.58 | 0.422 | 5.69 | 5.61 | 0.449 |
| Number of children | 1394 | 990 | | 636 | 394 | |

Table 2: First stage results

| <i>Variable</i> | <i>(1)</i> | <i>(2)</i> | <i>(3)</i> |
|---------------------------------|-------------------|------------|------------|
| | Full Sample | | |
| Eligibility status | 0.694*** | 0.681*** | 0.648*** |
| | (0.022) | (0.022) | (0.053) |
| R squared | 0.469 | 0.510 | 0.511 |
| F-value for instrument | 1030.0*** | 955.8*** | 148.9*** |
| F-value for poverty index terms | | | 0.06 |
| | Restricted Sample | | |
| Eligibility status | 0.680*** | 0.670*** | 0.635*** |
| | (0.034) | (0.034) | (0.064) |
| R squared | 0.436 | 0.513 | 0.513 |
| F-value for instrument | 404.3*** | 393.3*** | 97.3*** |
| F-value poverty index terms | | | 0.23 |
| Controls | None | X | X, f(P) |

Note: Robust standard errors in brackets. *** indicates significance at the 1% level. Number of observations equals 2384/1030 for full/restricted sample.

Table 3: Reduced form results, full sample

| <i>Variable</i> | <i>(1)</i> | <i>(2)</i> | <i>(3)</i> |
|-----------------------------|-------------------|------------|------------|
| | Levels | | |
| Eligibility status | -0.009 | 0.002 | 0.013 |
| | (0.017) | (0.015) | (0.035) |
| R squared | 0.000 | 0.289 | 0.289 |
| F-value poverty index terms | | | 0.06 |
| | First Differences | | |
| Eligibility status | -0.002 | -0.003 | 0.026 |
| | (0.015) | (0.015) | (0.034) |
| R squared | 0.000 | 0.123 | 0.124 |
| F-value poverty index terms | | | 1.12 |
| Controls | None | X | X, f(P) |

Note: Robust standard errors in brackets. Number of observations equals 2384.

Table 4: Reduced form results, restricted sample

| <i>Variable</i> | <i>(1)</i> | <i>(2)</i> | <i>(3)</i> |
|-----------------------------|-------------------|------------|------------|
| | Levels | | |
| Eligibility status | 0.041 | 0.022 | -0.048 |
| | (0.027) | (0.025) | (0.047) |
| R squared | 0.003 | 0.290 | 0.293 |
| F-value poverty index terms | | | 1.61 |
| | First Differences | | |
| Eligibility status | 0.018 | 0.007 | -0.023 |
| | (0.023) | (0.024) | (0.046) |
| R squared | 0.001 | 0.155 | 0.156 |
| F-value poverty index terms | | | 0.31 |
| Controls | None | X | X, f(P) |

Note: Robust standard errors in brackets. Number of observations equals 1030.

Table 5: IV results, full sample

| <i>Variable</i> | <i>(1)</i> | <i>(2)</i> | <i>(3)</i> |
|-----------------------------|-------------------|------------|------------|
| | Levels | | |
| Eligibility status | -0.013 | 0.003 | 0.019 |
| | (0.025) | (0.022) | (0.056) |
| R squared | 0.000 | 0.289 | 0.288 |
| F-value poverty index terms | | | 0.06 |
| | First Differences | | |
| Eligibility status | -0.003 | -0.004 | 0.043 |
| | (0.022) | (0.022) | (0.055) |
| R squared | 0.000 | 0.123 | 0.123 |
| F-value poverty index terms | | | 0.95 |
| Controls | None | X | X, f(P) |

Note: Robust standard errors in brackets. Number of observations equals 2384.

Table 6: IV results, restricted sample

| <i>Variable</i> | <i>(1)</i> | <i>(2)</i> | <i>(3)</i> |
|-----------------------------|-------------------|------------|------------|
| | Levels | | |
| Eligibility status | 0.061 | 0.032 | -0.076 |
| | (0.040) | (0.037) | (0.076) |
| R squared | 0.000 | 0.291 | 0.283 |
| F-value poverty index terms | | | 1.53 |
| | First Differences | | |
| Eligibility status | 0.026 | 0.011 | -0.036 |
| | (0.034) | (0.036) | (0.072) |
| R squared | 0.000 | 0.155 | 0.155 |
| F-value poverty index terms | | | 0.30 |
| Controls | None | X | X, f(P) |

Note: Robust standard errors in brackets. Number of observations equals 1030.

Table 7: Treatment effects on log expenditures, both samples

| <i>Variable</i> | <i>Food</i> | <i>School</i> | <i>Nonfood</i> | <i>Housing</i> | <i>PC</i> |
|-------------------|-------------|---------------|----------------|----------------|-----------|
| Full sample | | | | | |
| Levels | 0.057 | 0.213 | -0.163 | -0.110 | 0.008 |
| | (0.093) | (0.196) | (0.163) | (0.087) | (0.083) |
| First Differences | 0.143 | -0.029 | 0.057 | 0.059 | 0.118 |
| | (0.119) | (0.279) | (0.214) | (0.103) | (0.106) |
| Restricted sample | | | | | |
| Treatment status | 0.245* | 0.464 | -0.184 | -0.038 | 0.094 |
| | (0.144) | (0.328) | (0.259) | (0.127) | (0.127) |
| First Differences | 0.292 | 0.726* | 0.239 | 0.181 | 0.287 |
| | (0.184) | (0.449) | (0.346) | (0.160) | (0.164) |

Note: Robust standard errors in brackets. * indicates significance at the 10% level. Number of observations equals 2384 for full sample and 1030 for restricted sample.