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The Impact of Cash Transfers on School Enrollment:

Evidence from Ecuador

Hessel Oosterbeek
Juan Ponce
Norbert Schady

The World Bank
Development Research Group
Human Development and Public Services Team
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Abstract

This paper presents evidence about the impact on school enrollment of a program in Ecuador that gives cash transfers to the 40 percent poorest families. The evaluation design consists of a randomized experiment for families around the first quintile of the poverty index and of a regression discontinuity design for families around the second quintile of this index, which is the program's eligibility threshold. This allows us to compare results from two different credible identification methods, and to investigate whether the impact varies with families' poverty level. Around the first quintile of the poverty index the impact is positive while it is equal to zero around the second quintile. This suggests that for the poorest families the program lifts a credit constraint while this is not the case for families close to the eligibility threshold..

The Impact of Cash Transfers on School Enrollment: Evidence from Ecuador[*]

Hessel Oosterbeek

Amsterdam School of Economics Tinbergen Institute

Juan Ponce

FLACSO-Ecuador

Norbert Schady

World Bank

1. Introduction

Many countries in Latin America provide poor families with conditional cash transfers. The first country that adopted such a program was Brazil in 1995. Other countries include Mexico (1997), Honduras (1998), Nicaragua (2000), Costa Rica, Colombia (2001), Argentina, Uruguay, Chile, and Jamaica.[1] Conditional cash transfer programs provide poor families with cash conditional on their children attending school and/or visiting health care centers. The attractiveness of such 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.

The effectiveness of some of these programs has been assessed through rigorous impact evaluation studies. These studies show substantial positive effects of conditional cash transfers on school enrollment. The programs in Mexico and Nicaragua have been evaluated using randomized field experiments. In Mexico enrollment rates at the secondary school 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 enrollment rate for this group by 18 percentage points (Maluccio and Flores 2004).[2]

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 enrollment of 5 to 7 percentage points for 14 to 17 year-olds.

In this paper we evaluate the impact of a cash transfer to the poorest 40 percent families on school enrollment in Ecuador. While the program aims at increasing school attendance and visits to health care centers, the program does not impose any explicit requirement for children of treated families to attend school or visit health centers. An important consideration for the Ecuadorian government not to impose such requirements is that the administrative burden of monitoring attendance is high. Moreover, this requires careful consideration of incentive problems in the education system: interviews with teachers indicated that if they would be responsible for administering attendance, they might be inclined to report children to be present while they were actually not in school. A similar concern motivated Duflo and Hanna (2006) to

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[1] Rawlings and Rubio (2003), Caldés et al. (2004), and World Bank (2008) provide overviews of the various programs.
[2] The program in Honduras will also be evaluated through a randomized field experiment. Results are not yet available.

use cameras with a tamper-proof date and time function, to monitor teacher attendance in India when teachers were provided financial incentives for attendance.

While the formal rules of the program make it an unconditional program, this appears not to be the case in the perception of a substantial part of the potential beneficiaries. Before the actual implementation of the program there was a publicity campaign, which mentioned the need for households to enroll their children in school and take them to health care centers. Some surveys indicate that 1/3 of the beneficiaries state that they believe that the transfers are conditional, so that they will probably respond to the program as if it poses explicit requirements with regard to school enrollment and visits to health care centers.

An interesting feature of the design of the program's impact evaluation is that it consists of a randomized experiment and of a regression discontinuity design. In the experiment 1309 families around the first quintile of the poverty index were randomly assigned to treatment and control groups. For the regression discontinuity design data were collected from 1221 families around the second quintile of the poverty index, which is the program's threshold for eligibility. Hence our estimates pertain to groups at two different locations of the poverty distribution, thereby giving insight into the potential heterogeneity of the program's impact. If the cash transfer lifts a credit constraint, it is likely that the impact is larger among poorer families. Moreover, since school enrollment prior to the program's implementation is lower among poorer families there is also more scope for an increase in enrollment among these families.

Our empirical findings show that there are indeed heterogeneous treatment effects according to this pattern. School enrollment of children in families around the first quintile increases by about 10 percentage points in response to the cash transfer, while school enrollment of children in families around the second quintile is unaffected by the program. Our findings suggest that the program's effectiveness can be enhanced by lowering the poverty threshold for program eligibility, so that unresponsive families are no longer covered (i.e. do not receive a windfall).

The experimental design using data from families around the first quintile has been analyzed before in a recent paper by Schady and Araujo (2008). Although their empirical approach differs somewhat from the approach adopted in this paper, the findings are qualitatively similar; they too find that school enrollment goes up by about 10 percentage points for families around the first quintile that receive the cash transfer. The main novelty of the current paper is that we present the findings from the randomized experiment along with the fresh evidence from the regression discontinuity design, and that we compare and interpret the findings from both designs.

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 approaches adopted in this paper. Section 4 describes the data. Section 5 presents and discusses the empirical results. Section 6 summarizes and concludes.

2. Program and context

Ecuador is a lower-middle income country, characterized by high poverty levels and high inequality. Between 1982 and 1990, enrollment increased from 68.6% to 88.9% for primary schools and from 29.5% to 43.1% for secondary schools. Despite an expansion of educational inputs in the 1990s, enrollment stagnated in that decade.

Compulsory schooling in Ecuador starts at the age of 5 and ends at the age of 14. This covers one year of pre-school, six years of primary school and three years of basic secondary school. Direct costs of schooling for parents include the following items: (i) uniforms; (ii) a (not so) voluntary contribution of around US$ 20 per year; (iii) school books; and (iv) transportation costs.[3]

The cash transfer program we evaluate in this paper is called the *Bono de Desarrollo Humano* (BDH), and was launched in 2003.[4] It consists of a payment to the poorest 40 percent of families with children. The transfer equals US$ 15 per family per month and is independent of the number of children. The amount of US$ 15 should be compared to average monthly expenditures in the target group of around US$ 100. Whether a family belongs to the 40 percent poorest families depends on their score on a poverty index. The poverty index is computed using non-linear principal components analysis based on 27 variables including household assets and housing characteristics (television, car, telephone, electricity, water, etc.), characteristics of head of household and her/his partner (schooling, ethnicity, illiteracy, labor market status, etc.), children's characteristics and household size.

The main stated objective of the 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

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[3] The new government that started in 2007 eliminated the "voluntary" contribution and plans to provide free books and uniforms to children from poor families.
[4] The program incorporated two previous smaller programs aimed at the very bottom of the poverty distribution. See Vos et al (2001) and León and Younger (2004) for evaluations of one of these programs.

are not taken off program rosters if their school-aged children are not enrolled in school or fail to attend classes regularly.

3. Empirical approach

To evaluate the impact of the Ecuadorian cash transfer program, we take advantage of two elements included in the design of the program during its initial stage: a randomized social experiment of families around the first quintile of the poverty index (EXP) and a regression discontinuity design (RDD) created by the program's eligibility threshold around the second quintile of the poverty index.[5] This will in principle produce credible estimates of the impact of the cash transfer at different points of the poverty distribution. The identifying assumption for the experimental design is that assignment to treatment and control groups is random. This assumption can be verified by comparing the two groups in terms of their observable characteristics. The identifying assumption for the regression discontinuity design 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 second quintile.

More formally, we will estimate different versions of the following equation:

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

Where Y is school enrollment which takes a value of 1 if a child is enrolled and 0 otherwise, T is an indicator variable taking the value of 1 if the person receives the treatment 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, 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. For all the results we report heteroscedasticity robust standard errors that are clustered at the family level.[6]

The effect of interest is δ. When assignment in the experiment is truly random, controlling for observables should not affect the estimates. In the regression discontinuity design, controlling for a flexible function of the underlying variable (poverty index) can be vital, depending on the (local) relationship between this variable and the outcome of interest.

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[5] When implementing the cash transfer program, the government of Ecuador planned to evaluate the program's impact only through a regression discontinuity design. The initial design of the program established two different amounts: US$ 15 for families in the lowest quintile and US$ 11.5 for those in the second quintile. Once the research was designed and the baseline survey was conducted, the government, however, decided to grant all families in the bottom two quintiles US$ 15. The regression discontinuity around the first quintile was replaced by the randomized experiment.

[6] Clustering at the parish level instead of family level does not change our findings.

It turns out that not all families that received the transfer were eligible, and vice versa. In the experiment some families that were assigned to control did receive the transfer and some families that were assigned to treatment did not receive it. Likewise, in the regression discontinuity design some families that should not have received the transfer did get it, while some other families that were eligible for the transfer did not receive it. There is thus not a deterministic relation between eligibility (assignment to treatment, poverty index) and actual receipt of treatment, but a probabilistic one. To address the potential biases caused by this contamination, we apply an instrumental variables approach where actual 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 for eligibility (Z), which takes the value 1 if the respondent is eligible for treatment (assignment to treatment or poverty index below the cutoff) and 0 otherwise. The identifying assumption is then that $E(Z_{i,t} \cdot u_{i,t} \mid X_{i,t-1}, P_{i,t-1}) = 0$.

Since we have pre-intervention and post-intervention measures of outcomes at our disposal, we can also combine the experimental and regression discontinuity designs with a before-after approach. To this end we estimate equations of the following form:

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

Where ΔY is the change in school enrollment which takes a value of 1 if a child is enrolled at t and not enrolled at t-1, of 0 if the enrollment 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 effects of the intention to treat (ITT) for the two samples.[7]

4. Data

The experiment and the RD design were both implemented in four out of twenty-two provinces in the country.[8] 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. The sampling scheme for both designs selected only households who had

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[7] Note that the ITT for the two designs has an entirely different interpretation. We come back to that after presenting the empirical findings

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

at least one child aged 6 to 15 at the time of the baseline survey. A baseline survey was conducted between June and August 2003 and a follow-up survey was carried out between January and March 2005.

The sample for the experiment consists of households with a poverty index between the 13th percentile and the 28th percentile. One-half of the households in this sample were randomly assigned to the treatment group that was eligible for the cash transfer, and the other half was assigned to the control group that was not eligible for the transfers during the period of the evaluation. These two groups are the lottery winners and the lottery losers respectively.

To exploit the discontinuity in eligibility around the program's eligibility threshold of the second quintile of the poverty index, families with a poverty index between the 33rd percentile and the 47th percentile were sampled. In that design families with their value of the poverty index between the 33rd percentile and the threshold (40th percentile) are just eligible for receipt of the cash transfer. Families with a value on the poverty index between the threshold and the 47th percentile are just ineligible for receipt of the transfer.

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 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 enrollment during the current school year (level and grade). Finally, the survey includes a complete module of household expenditures, which replicates the structure of the 1999 Ecuador LSMS.

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), suggesting 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 3,004 children in 1,309 families in the experiment, and of 2,384 children in 1,221 households in the RDD study.[9] Table 1 presents descriptive statistics for eligible and ineligible children/households in both groups. Columns 1-3 pertain to the RDD sample of children/households who have a poverty index just below or just above the program's threshold. Columns 4-6 are for the experimental sample around the first quintile of the poverty index.

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[9] 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.

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 just below and above the cutoff in the RDD. This suggests that treatment and control groups in this design may be too different to compare. Recall, however, that the identifying assumption of the RDD 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 do not invalidate the RDD.

With the genuine random assignment in the experimental study, we expect no significant differences for any of the observables in table 1. This is true for all variables in table 1, with two exceptions. Somewhat surprisingly, we find a significant difference between the two groups on the poverty index. The absolute difference between the eligible and non-eligible groups is, however, rather small. We believe that controlling for a flexible function of the poverty index will undo any biases due the apparent deviations from the randomized assignment. (And as our results in the next section show, our impact estimates are very similar whether we control for the poverty index or not.) Furthermore the randomization favored a bit families living in rural areas, as they were more likely to win the lottery. Here too, we believe that controlling for the urban area dummy in combination with canton fixed effects will eliminate any biases related to this composition difference. And again this is supported by the fact that the estimation results are not sensitive to the inclusion of these control variables.

The results in table 1 also show substantial differences between the RDD sample and the EXP sample. For most variables these differences reflect the differences between families around the first and around the second quintile of the poverty index, and hence these differences are qualitatively similar to the differences between the eligible and non-eligible groups in the RDD. Noticeable is the substantial difference in enrollment rates at baseline. This is close to 0.75 around the first quintile and close to 0.85 around the second quintile. Our impact estimates should be regarded relative to these current enrollment rates, since there are obvious ceiling effects. Many countries in Latin America have very similar enrollment rates, including 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. Among the winners of the experiment's lottery, 529 of 677 households (78%) received cash transfers. Among the losers of the lottery, 264 out of 632 households (42%) erroneously received transfers. Likewise, out of a total of 537 families that were just above the second quintile of the poverty index in the RDD, 41 (8%) received the cash transfer. And out of 684 families that were eligible in the RDD because their poverty index was just below the second quintile, 178 (26%) did not receive the cash transfer. Hence for 31% of the families in the experiment and for 18% of the families in the RDD, eligibility-status and treatment-status do not coincide.

Figure 1 plots the relation between the poverty index and the probability of actual treatment for the treatment and control groups in both designs (EXP and RDD) separately. At the right hand side of the figure the discontinuity in the probability of treatment around the cutoff in the RDD sample is evident. Closely around the second quintile of the poverty index, 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 in this sample the probability of treatment is independent of the poverty index conditional on the eligibility status. The left hand part of the figure shows the same relations for the winners and losers in the experiment. Winners are clearly more likely to actually receive treatment than losers, but it is clear that a substantial fraction of the losers also receive treatment. Moreover, the two lines at the left hand part of the figure indicate that the difference in the probability of actual treatment between winners and losers increases with the poverty index. This shows that in the EXP-sample the probability of actual treatment is higher for poorer families.

Table 2 shows these findings more formally for various specifications of the first stage relationship for the two samples. The top panel contains the results for the RDD 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.[10] 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 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.

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[10] Results are the same when we include the poverty index in linear or quadratic form.

The bottom panel of table 2 reports results for the same first stage specifications for the EXP-sample. For this sample the point estimates are about half the size of those for the RDD-sample. Nevertheless the first stage results are still very significant, and while inclusion of the poverty index terms cannot be rejected, the effect of eligibility of treatment is hardly affected by it.

Administrative problems appear to have been the main cause for the high non-compliance to the assigned treatment status in the experiment. The persons responsible for the actual payment of the cash transfer to winners (and not to losers) initially did not respect the lists of winners and losers that were sent to them. Only after some time did they take it seriously. This suggests an important practical lesson for conducting randomized social experiments with the involvement of local civil servants/bureaucrats. The higher rate of compliance to eligibility status in the RDD suggests that in some circumstances, this might be a more effective evaluation scheme than a randomized experiment.

Reduced form

The first three columns in table 3 show the reduced form results for the EXP-sample. We present results for different specifications corresponding to those in the previous table. 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 the level specifications the point estimates are close to 0.03 and in the first difference specifications they are slightly above 0.04. With one exception these estimates are significantly different from zero.

Going from the first to the third column we observe that adding more control variables does not change the estimates, as it should not given randomized assignment. Not much precision is gained by including extensive sets of control variables. For the results in the third column we tested for the joint significance of the poverty index polynomial.[11] We reject that the joint effects of these three terms equal zero.

The last three columns of table 3 report the reduced form results for the RDD-sample. In all specifications the estimates are small and never significantly different from zero. Going from the fourth to the sixth column we observe that adding more controls 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 column we tested for the joint significance of the poverty index polynomial.[12] We cannot reject that the joint effects of these three terms equal

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[11] Again results are the same when we include the poverty index in linear or quadratic form.
[12] Again results are the same when we include the poverty index in linear or quadratic form.

zero. On the basis of efficiency considerations, we should therefore prefer the results in column (5). 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. Based on our preferred first difference specification in the second column, we can rule out a program impact on school enrollment exceeding 2.7 percentage points for children in families around the second quintile of the poverty index, with 95% likelihood.

Figure 2 illustrates the reduced form results for the specification without any controls using data for both samples. The right hand part clearly shows the absence of any impact in the RDD sample. In anything, it even seems that at the cutoff, eligibility for treatment has a slight negative impact. The left hand side shows the relation between the poverty index and school enrollment for winners and losers of the lottery. Evidently, children of families that won the lottery are more likely to be enrolled in school than children in families that lost the lottery.[13]

IV

Table 4 reports the IV results for the two samples. Point estimates are equal to the reduced form estimates (in table 3) divided by the first stage coefficient in the corresponding column (in table 2). The impact estimates for the EXP-sample are around 0.09 for the levels specification and around 0.12 for the first difference specification (columns 1-3). This implies that actual receipt of the cash transfer raises school enrollment by 9 to 12 percentage points for children in families around the first quintile of the poverty index. None of the impact estimates for the RDD-sample are significantly different from zero, implying that we cannot reject the hypothesis that receipt of the cash transfer has no impact on school enrollment for children in families around the second quintile of the poverty index (columns 4-6).

These IV-estimates make very prominent the difference in impact the cash transfer has for families at different points of the poverty index. Average monthly expenditures amount to US$ 104 for families around the first quintile and US$ 125 for families around the second quintile. For the first group the extra US$ 15 per month has an impact and school enrollment goes up from around 75% to around 85%. For the second group the extra US$ 15 has no impact and school enrollment remains around 85%. Apparently, extra financial resources are helpful to increase school enrollment from 75% to 85%. To increase school enrollment beyond that level, extra cash appears not to matter.

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[13] From the figure it appears that the impact of winning in this group is larger for those with a high value of the poverty index. This is, however, a result of the lowess estimation being sensitive for outliers located at the ends of the graph.

Comparing the EXP and RDD results

Thus far we have presented and discussed the results from the experiment and the RDD as if they are entirely comparable. Assuming that the RDD approximates the conditions of randomized assignment, the results from the two designs would be comparable if there would be full compliance. That is: if intended treatment (eligibility) and actual treatment coincide. This is, however, not the case. In this subsection we discuss how this might affect the interpretation and comparability of the results from the two designs.

The ITT estimates are based on a comparison of the outcomes for children from eligible and ineligible families. In the RDD 82% of the sample received the treatment they were intended to receive, while this percentage is only 69% in the experiment. If the compliance rate in the EXP would be as high as in the RDD and if treatment has a non-negative effect on school enrollment, then the ITT estimates of the EXP would be higher than those reported in table 3. Consequently, due to the different compliance rates the difference in ITT estimates of the two designs is underestimated.

Notice that the policy relevance of the ITT's of the two designs is different. In the RDD it is really the intention not to provide cash transfers to families above the second quintile. In the EXP it is not be the intention of the policy makers to permanently withhold treatment from families that have been assigned to the control group.

Of greater policy relevance for the experimental design are the IV-estimates. The IV-estimates divide the ITT-estimates by the difference in the probabilities of actual receipt of the cash transfer between eligible and ineligible observations. This estimator is usually interpreted as the local average treatment effect (LATE): it is the treatment effect measured on the compliers. Compliers are those (unidentifiable) observations that receive the cash transfer because they won the lottery (in the EXP) or because they are just below the eligibility threshold (in the RDD). Due to the different compliance rates and the (probably) different reasons for compliance in the two designs, it is difficult to compare the LATE-estimates across designs.

Are the groups that deviated from their assigned eligibility status systematically different across the two samples? To gain some insight in this, table 5 reports the results of regression of actual treatment status on background characteristics, separately for the EXP and RDD-samples and for the eligible and ineligible groups within these samples. The first column in this table pertains to observations in the EXP that lost the lottery. The probability that a person in that ineligible group receives treatment decreases with the poverty index (poorer people are more likely to receive treatment) and increases when the mother lives in the same house. The second

column shows that the probability that lottery winners in the EXP actually receive treatment is higher when children were enrolled in school at baseline and is also higher in rural areas than in urban areas. A higher compliance rate in the EXP would relocate some families from the (Z=0, T=1) group to the (Z=0, T=0) group thereby lowering the poverty level in the latter group. At the same time it would relocate some families from the (Z=1, T=0)-group to the (Z=1, T=1) group, thereby increasing the share of children with lower enrollment levels at baseline in the latter group. If poverty and low enrollment at baseline are both negatively correlated with lower enrollment rates, then the enrollment levels of both the treated and the control groups are upward biased.[14] But since the low compliance rate is mainly due to the high take-up are of transfers among ineligible families, and higher compliance rate in the EXP-design would probably lead to a larger relocation in this group, and thereby to a larger impact estimate of the program.

Columns 3 and 4 repeat this for the eligible and ineligible groups in the RDD. Families just below the threshold are more likely to actually receive the cash when the mother lives in the house (this is expectable since the money is provided to the mother). Moreover, in both groups (eligible and ineligible) living in an urban area significantly reduces the chances of collecting the cash. It is not clear a priori whether, and if so in which direction, these composition effects would bias the RDD-estimates.

6. Summary and discussion

This paper evaluates the impact on school enrollment from a program in Ecuador that gives cash transfers to the 40 percent poorest families. Using data from a randomized experiment and from a regression discontinuity design, we find heterogeneous effects of the program on school enrollment. Around the first quintile of the poverty index the cash transfer of US$ 15 per month increases school enrollment from 75% to 85%. Around the second quintile the cash transfer has no impact and school enrollment remains 85%. This suggests that for the poorest families in Ecuador the program lifts a credit constraint while this is not the case for families close to the eligibility threshold.

Increasing school enrollment is one of the main goals of the cash transfer program in Ecuador. Our findings suggest two different avenues to enhance the program's effectiveness. Because children in families close to the program's eligibility cutoff are not affected by the

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[14] Put differently, the poorest households were likely to receive transfers no matter whether they lost or won the lottery. These households are therefore unlikely to be compliers in the language of Angrist, Imbens and Rubin (1996). A similar argument holds for households whose children were enrolled at baseline—they were more likely to receive transfers than those households whose children were not enrolled at baseline, no matter what their lottery status.

program, it might be considered to lower the threshold. Our results are however not more informative about the optimum threshold level other than that it should be somewhere between the first and second quintile of the poverty index. Alternatively, it might be considered to impose an explicit requirement for children to be enrolled in school to qualify for the transfer.

Recently the Ecuadorian government decided to double the amount of the cash transfers from US$ 15 to US$ 30. Given the findings reported in this paper, it is doubtful whether this increase will have an impact on school enrollment.[15] It will not have an impact for children in families close to the program's threshold. These families are already unresponsive to receipt of the first US$ 15, so the next US$ 15 will only have a smaller impact. But also children in families around the first quintile are unlikely to respond to the increase in the transfer. The first US$ 15 already made their enrollment levels catch-up with that of children from families around the second quintile of the poverty index. The results for the children from families around the second quintile suggest that something different than cash is needed to boost the enrollment rate above 0.85.

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[15] For a discussion of the likely impacts on health and cognitive development outcomes, see Paxson and Schady (2008).

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Figure 1: First stage relation

Relation between poverty index and actual treatment
by design and group

Prob Treatment

Control RDD — Treated RDD
Control EXP — Treated EXP
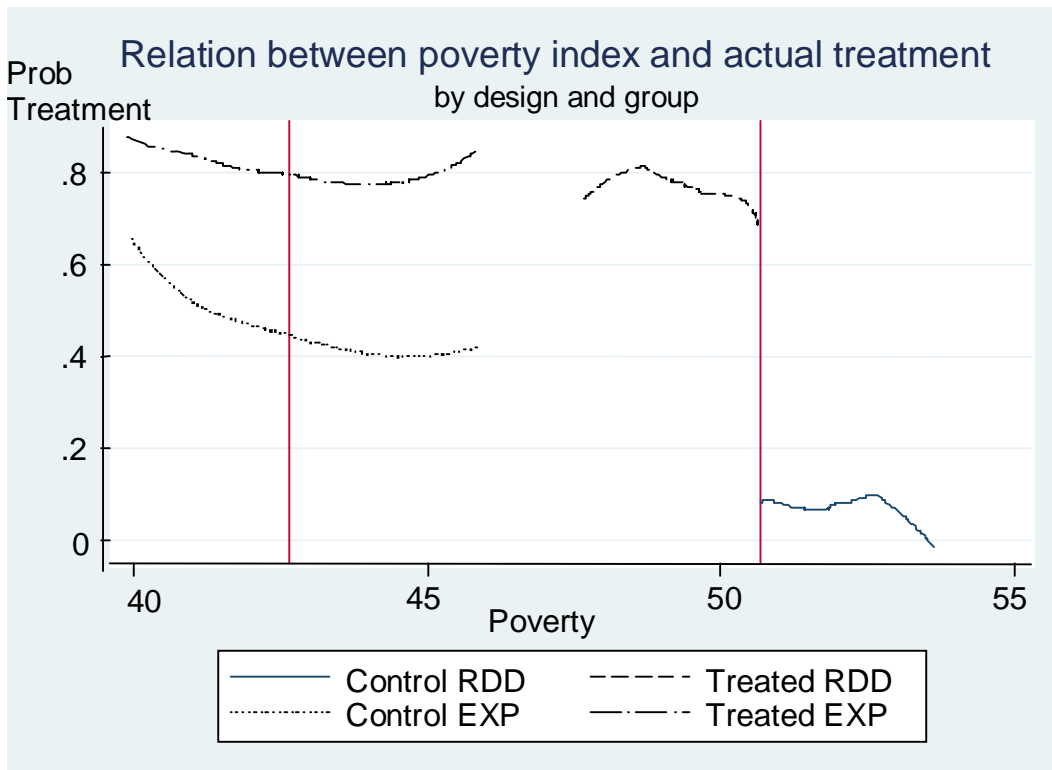
Figure 2: Reduced form relation

Relation between poverty index and school enrollment
by design and group

Prob Enroll

Control RDD — Treated RDD
Control EXP — Treated EXP

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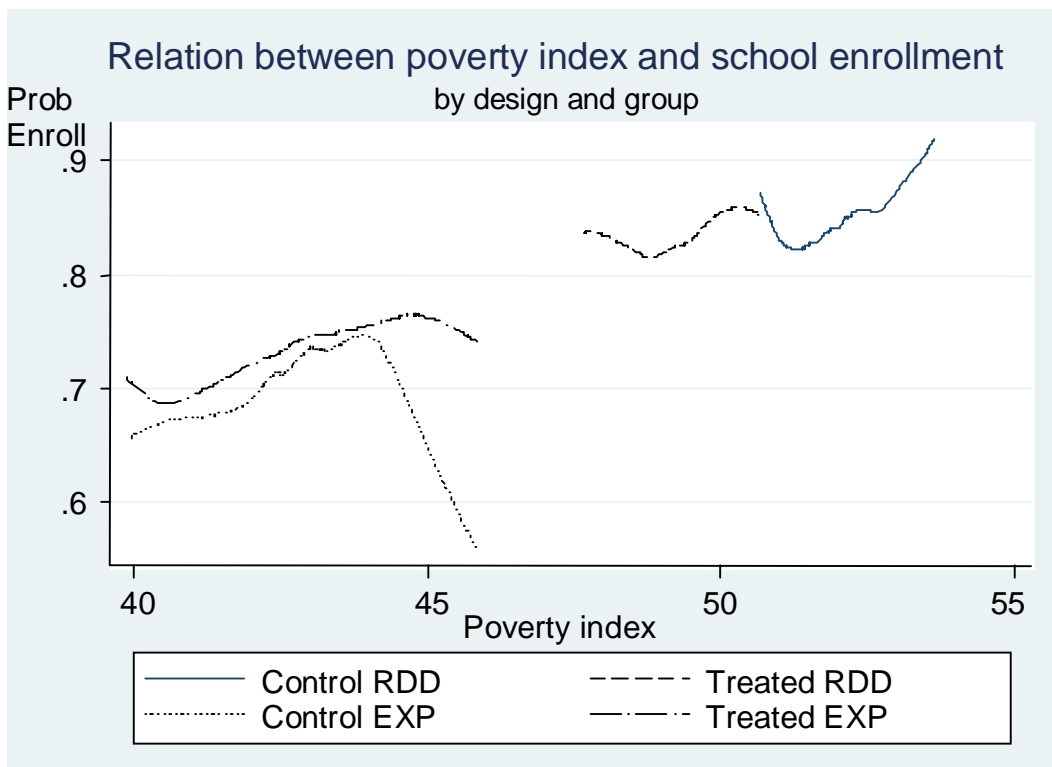
Table 1: Descriptive statistics by sample and eligibility status

	RDD			EXP		
Variable	Eligible	Not eligible	p-value	Eligible	Not eligible	p-value
	(1)	(2)	(3)	(4)	(5)	(6)
School enrollment pre intervention	0.85	0.86	0.625	0.75	0.77	0.426
Child's age	11.9	12.0	0.498	11.4	11.4	0.592
Child is female	0.53	0.52	0.787	0.49	0.51	0.187
Log of per capita expenditures	2.92	3.07	0.000	2.69	2.72	0.259
Poverty index	49.4	51.9	0.000	43.0	42.8	0.001
Father's education	5.68	6.16	0.000	4.76	4.65	0.281
Mother's education	5.28	5.92	0.000	3.84	3.75	0.381
Father lives at home	0.79	0.77	0.209	0.83	0.82	0.864
Mother lives at home	0.90	0.85	0.000	0.93	0.94	0.251
Head of household is male	0.85	0.87	0.307	0.88	0.87	0.218
Head of household is indigenous	0.09	0.06	0.002	0.17	0.17	0.024
Head of household can read and write	0.94	0.96	0.161	0.84	0.88	0.006

Household size	5.63	5.58	0.422	6.36	6.28	0.255
Urban area	0.51	0.51	0.992	0.47	0.53	0.001
Number of children	1394	990		1567	1437	

Table 2: First stage results

Variable	(1)	(2)	(3)
	RDD		
Eligibility status	0.694***	0.681***	0.648***
	(0.022)	(0.022)	(0.053)
F-value for instrument	1030***	956***	149***
F-value for poverty index terms			0.06
	EXP		
Eligibility status	0.347***	0.358***	0.362***
	(0.028)	(0.027)	(0.027)
F-value for instrument	155***	175***	183***
F-value poverty index terms			2.42*
Controls	None	X	X, f(P)

Note: Standard errors in brackets are heteroscedasticity robust and clustered at family level. ***/* indicates significance at the 1%/10% level. Number of observations equals 2384/3004 for RDD/EXP sample. X includes: dummies for child's age, dummy for child's gender, dummies for (potential) grade levels, consumption, parents' education, dummies for parents being present, dummy for gender of head of household, dummy for ethnicity of head of household, dummy for head of household being illiterate, household, size, dummy for urban/rural area, canton dummies.

Table 3: Reduced form results

	EXP			RDD		
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
			Levels			
Eligibility status	0.029	0.033**	0.031**	-0.009	0.002	0.013
	(0.019)	(0.015)	(0.015)	(0.017)	(0.015)	(0.035)
F-value poverty index terms			4.16**			0.06
			First Differences			
Eligibility status	0.042***	0.044***	0.044***	-0.002	-0.003	0.026
	(0.016)	(0.015)	(0.015)	(0.015)	(0.015)	(0.034)
F-value poverty index terms			2.42*			1.12
Controls	None	X	X, f(P)	None	X	X, f(P)

Note: Standard errors in brackets are heteroscedasticity robust and clustered at family level. ***/**/* indicates significance at the 1%/5%/10% level. Number of observations equals 3004 in EXP and 2384 in RDD . See also the note of table 2.

Table 4: IV results

	EXP			RDD		
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
			Levels			
Eligibility status	0.084	0.092**	0.086**	-0.013	0.003	0.019
	(0.054)	(0.043)	(0.042)	(0.025)	(0.022)	(0.056)
F-value poverty index terms			4.67***			0.06
			First Differences			
Eligibility status	0.120***	0.124***	0.120***	-0.003	-0.004	0.042
	(0.046)	(0.042)	(0.041)	(0.022)	(0.022)	(0.055)
F-value poverty index terms			2.63*			0.95
Controls	None	X	X, f(P)	None	X	X, f(P)

Note: Standard errors in brackets are heteroscedasticity robust and clustered at family level. ***/**/* indicates significance at the 1%/5%/10% level. Number of observations equals 3004 in EXP and 2384 in RDD . See also the note of table 2.

Table 5: Determinants of actual treatment by sample and eligibility status

	EXP		RDD	
Variable	Ineligible	Eligible	Ineligible	Eligible
	(1)	(2)	(3)	(4)
School enrollment pre intervention	-0.036	0.075*	-0.051	-0.006
	0.046	0.040	0.037	0.046
Poverty index	-0.035**	-0.015	0.003	-0.021
	0.015	0.011	0.016	0.022
Child is female	0.011	0.002	-0.024	0.005
	0.024	0.019	0.015	0.023
Father's education	0.017	0.001	-0.001	-0.001
	0.010	0.009	0.005	0.007
Mother's education	0.007	0.004	-0.004	0.007
	0.009	0.007	0.005	0.006
Father lives at home	0.013	-0.056	-0.033	0.161
	0.097	0.070	0.043	0.094
Mother lives at home	0.172**	0.135	0.021	0.141**

	0.085	0.084	0.035	0.080
Head of household is male	0.056	-0.007	-0.011	-0.103
	0.104	0.073	0.050	0.093
Head of household is indigenous	0.071	-0.020	-0.062	-0.030
	0.072	0.062	0.041	0.078
Head of household can read and write	0.040	0.014	-0.029	0.130
	0.072	0.061	0.075	0.084
Household size	0.017	0.000	0.035***	0.012
	0.013	0.011	0.012	0.011
Urban area	-0.061	-0.120**	-0.068*	-0.128**
	0.061	0.048	0.040	0.051
N	1437	1567	990	1394
R squared	0.177	0.115	0.178	0.111

 Note: Standard errors in brackets are heteroscedasticity robust and clustered at family level.
***/**/* indicates significance at the 1%/5%/10% level. Number of observations equals 3004 in
EXP and 2384 in RDD . See also the note of table 2.