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Abstract

Throughout Latin America, conditional cash transfer (CCT) programs play an important role in social policy. These programs aim to influence the accumulation of human capital, as well as reduce poverty. In terms of educational outcomes, a number of impact evaluation studies have shown that such programs have led to an increase in school enrollment, ensured regular school attendance and led to a reduction in child labor. Theoretically, such cash transfer programs may also be expected to exert a positive impact on students' test scores, but related empirical evidence is scarce. Accordingly, this paper evaluates the impact of a cash transfer program, the *Bono de Desarrollo Humano* of Ecuador, on students' cognitive achievements. The paper uses a regression discontinuity strategy to identify the impact of the program on second grade cognitive achievement. Regardless of the specification used, we find no impact of the program on test scores, suggesting that attempts at building human capital, as measured by cognitive achievement, require additional and alternative interventions.

JEL codes: I38, I28

Key words: Cash transfers, test scores, regression discontinuity

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I. Introduction

Demand-side interventions play an important role in education policy in Latin America. Broadly, two types of policies have been implemented in the region - conditional cash transfer programs (CCT) and school vouchers. CCT programs started during the 1990s and the main idea of these programs is to provide money to poor families, conditional on enrollment and regular attendance of their children in school and regular visits to health centers, where their growth is monitored and they receive nutritional supplements. In the long run, these programs seek to influence the accumulation of human capital, especially amongst youth and children, as a means of breaking the inter-generational cycle of poverty. In the short run, CCT programs aim to reduce poverty by increasing the income of poor families.

A number of CCT programs operating in Latin American countries have been evaluated. While details appear later, on the education front, a majority of the studies have found that CCT programs boost school enrollment and ensure regular school attendance. While these are clearly the first steps required to ensure a higher level of educational attainment and achievement, if CCT programs are to ensure that students accumulate adequate human capital to break the cycle of poverty, then a focus on enrollment is not enough. From a policy perspective it is important to examine whether such programs also increase cognitive achievement.[1] Higher cognitive achievement as captured by test scores, are likely to ensure that a child stays in school for a longer duration and are also correlated with labor market success. There is a limited literature on developing countries which shows that cognitive achievement increases wages and tends to have larger effects than schooling attainment.[2] While the link between the level of test scores and earnings may be confounded with a number of other factors, a more recent literature focuses on gains in tests scores and earnings. For example, Jencks and Phillips (1999) show that math test

[1] While this paper focuses on cognitive outcomes, it is possible that such programs boost non-cognitive skills which in turn have a bearing on earnings and related outcomes (see Heckman et al. 2006). The effect of CCT programs on fostering non-cognitive skills is an unexplored research area.

[2] See Boissiere, Knight and Sabot (1985) for work on urban Kenya and Tanzania; Alderman, Behrman, Ross and Sabot (1996) for work on Pakistan; Lavy, Spratt and Leboucher (1997) for work on Morocco.

scores gains between 10^{th} and 12^{th} grade exert a positive impact on educational attainment and also exert a positive impact on earnings nearly a decade after students graduated from high school. Rose (2006) shows that employed women who gained one standard deviation more than average on math test scores between 8^{th} and 12^{th} grade, experience, on average, a 9 percent increase in earnings. Her results also show that for women, gains in test scores influence the probability of finding employment.[3]

Theoretically, such programs are likely to influence students' cognitive achievements in several ways. On the one hand, on average, there could be a *positive impact* because CCT programs increase attendance rates and higher attendance is likely to lead to higher test scores.[4] Cash transfer program induced increases in household incomes may be expected to lead to increased food consumption and better nutrition which in turn should translate into higher levels of cognitive achievement. Several evaluations have shown that these programs are associated with a reduction in the probability that a child works which again maybe expected to exert a positive impact on test scores. On the other hand, these programs may also have a *negative effect* on average test scores. Increases in school enrollment may translate into congested classrooms, which in turn may negatively affect cognitive achievement. Furthermore, if the program encourages less able students to enroll, then changes in student-body composition may lead to a decline in average test scores. Whether, on average, such programs exert a net positive or negative effect is an empirical question.

While there are a number of studies that have examined the effect of CCT programs on enrollment, child work and other outcomes, the number of studies evaluating the effect of the CCT program on cognitive achievements is scarce. Therefore, the

[3] A number of authors have used developed country data to examine the impact of the level of test scores on earnings. For example, based on US data, Murnane, Willett, and Levy (1995) show that the importance of mathematics test scores in predicting earnings grew during the 1970s and 1980s. For the same time period, Bedard and Ferrall (2003) use international data to compare test scores distributions at age 13 with the distribution of subsequent wages and conclude that the trends in the two distributions are related.

[4] Bedi and Marshall (2002) discuss the link between school attendance and test scores in Honduras. In particular, they report that an increase in school attendance by 5 days increases grade 2 mathematics and Spanish test scores by about 1.5 points.

contribution of this paper is to evaluate the impact of the Ecuadorian cash transfer program (*Bono de Desarrollo Humano-BDH*) on students' cognitive achievements. In particular, the paper exploits the manner in which the *BDH* is allocated and relies on a regression discontinuity (RD) approach to identify the impact of the program on second grade cognitive achievement.

The next section presents a review of the main demand side interventions and their impact on educational outcomes in Latin America. The third section presents a country background and a program description. The fourth section outlines the empirical approach. The fifth section presents the data while the sixth presents the results. The final section concludes.

II. Conditional cash transfer programs in Latin America

The first Latin American CCT program started in Brazil in 1995. Other early experiences include Mexico's program which started operations in 1997, Honduras in 1998 and Nicaragua in 2000. Soon other countries in the region followed suit.

A number of studies have examined the impact of these programs on school enrollment, attendance, nutrition and child work. In particular, experimental designs have been used to examine the impact of the CCT programs in Mexico (Skoufias, 2000; Schultz, 2004; Behrman, Sengupta and Todd, 2005) and Nicaragua (Maluccio and Flores, 2004). In the case of Mexico, Schultz (2004) reports that at the primary school level, where enrolment rates before program implementation were between 90 and 94 percent, the program had a small positive impact with an increase in enrolment of between 0.8 to 1.18 percentage points for boys and 0.92 to 1.27 percentage points for girls. At the secondary level, where initial enrolment rates were 67 (73) percent for girls (boys), the program increased enrolment rates for girls (boys) by 9.2 (6.2) percentage points. Other program effects, as reported in Skoufias (2000) include, in 1999, a 13 percent increase in median food expenditure, improvement in child health (children aged 0-5 were 12 percent less likely to be ill), and reduction in child stunting. Behrman, Sengupta and Todd (2005) also report a program induced increase in enrollment as well as lower dropout and repetition rates. However, in terms of cognitive achievement, Behrman, Sengupta and Todd (2000) find that after almost a school year and a half of exposure there is no impact of the program on test scores.

In Nicaragua, Maluccio and Flores (2004) show that the CCT program increased school enrollment amongst children in the age group 7 to 13 by 18 percentage points, led to a 23 percentage point increase in attendance (during the previous month) and reduced the incidence of child work by 5 percentage points. In addition, the program led to a 5 percentage point reduction in stunting amongst children aged 0 to 5.

Other CCT programs have been evaluated using non-experimental methods. For example, Duryea and Morrison (2004) use regression analysis and propensity score matching to evaluate Costa Rica's *Superémonos* program. Their propensity score estimates show that the program increased school attendance for children in the group 13 to 16 by 5 to 8.7 percentage points but did not have any effect on their work patterns. The effect of the program on school performance as measured by the probability of passing a grade indicated a 5 percentage point increase for program participants but was not robust to changes in estimation method.

While there are differences across countries, in general, it appears that CCT programs have led to substantial increases in school enrollment. The programs have also led to increases in school attendance and in several cases also led to reductions in child work, increases in food expenditure and improvements in health outcomes. The effect of such programs on measures of school performance such as test scores has not yet been extensively researched.

III. Country background and program description

Ecuador is a lower-middle income country, characterized by high levels of poverty and inequality. Regarding education, the country witnessed sharp improvements in enrolment rates in the 1980s, with the net enrolment rate at the primary and secondary level increasing from 68.6 and 29.5, in 1982 to 88.9 and 43.1, in 1990 respectively. However, between 1990 and 2001, net enrolment rates for both primary and secondary levels stagnated and in 2001 were at the level achieved in 1990. Educational achievement fell and according to the Ecuadorian System of Educational Achievements Measurement, during the second half of the 1990s, test scores for mathematics and language, which are marked out of 20, decreased from 9.7 and 10.7 to 8.5 and 9 respectively for the second grade of primary education. A similar deterioration was observed for students in other grades.

Towards the end of the 1990s, in a bid to boost school enrollment amongst the poorer segments of the population and to raise achievement the Ecuadorian government launched a conditional cash transfer program (*Beca Escolar*) and a school-meal program. The *Beca Escolar* program consisted of transferring US$5 per month per child (upto two children per household), conditional on a child being enrolled in school and maintaining a monthly attendance of 90 percent. At about the same time (in 1998), a program (*Bono Solidario*) was launched to compensate poor families for the elimination of gas and electricity subsidies.

In 2003 the *Bono Solidario* was reformulated and became a CCT. The program was renamed *Bono de Desarrollo Humano* (*BDH*) and incorporated both the *Bono Solidario* and the *Beca Escolar*. The main objective of the new program is to improve the formation of human capital among poor families in Ecuador. Education and health are the two components of the program. The education component requires children from the ages of 6 to 15 to enroll in school and to attend at least 90 percent of school days in a month. The health component requires children under the age of six to attend health centers for bimonthly medical check-ups where their growth and development is monitored and they receive nutritional supplements and immunization.

To select beneficiaries, the program uses an individual targeting strategy based on a proxy-means test. In particular, program participation is based on an index called Selben, or system of selection of beneficiaries of social programs. Selben identifies potential beneficiaries of social programs by classifying households according to an unmet basic needs index computed using non-linear principal components analysis.[5] Families in quintiles 1 and 2, that is, families with a Selben score of less than 50.65 are eligible to participate in the program. The eligibility cutoff score of 50.65 has been set by the government and beneficiaries receive a cash transfer of US$15 per month, per family which may be compared with the average monthly expenditure of US$100 amongst target group families. In 2004, the annual budget of the program was US$190 million (around 1 percent of GDP) and the program covered 1.1 million households or 40 percent of the population.

The effects of these programs have been examined by a number of authors. For instance, Vos et al. (2001) use propensity score matching to show that *Bono Solidario* leads to a 5 percentage point increase in school enrolment. León and Younger (2007) use an instrumental variable approach and report that the *Bono Solidario* had a statistically significant but small positive effect on children's nutritional status. Turning to the *BDH*, based on an experimental evaluation design, Schady and Araujo (2006) find that the program increased school enrollment for children in the age group 6 to 17 by about 10 percentage points and reduced child work by about 17 percentage points. Ponce (2008) (2008) refines these findings to show that the enrollment effect is heterogeneous and that the increase in enrollment is restricted to children around quintile 1 (poorest families) while enrollment for children from families around quintile 2 is unaffected by the program. While there are no enrollment effects, his analysis shows that program beneficiaries around quintile 2 (that is, in the neighborhood of the eligibility cutoff score of 50.65) experience a 25 percentage point increase in food expenditure and a 46 to 73 percentage point increase in educational expenditure as compared to non-beneficiaries.[6]

These existing results provide guidance on the mechanism through which the program may be expected to exert an effect on test scores. On average, the increase in enrollment and regular attendance maybe expected to translate into higher levels of

[5] The index is scaled from 0 (poorest) to 100 (richest). More details on the construction of the Selben index are provided later on in the text.

[6] School related expenditures include outlays on transportation, uniforms, tuition fees, text-books and other school materials.

achievement, although the negative effects of school congestion cannot be ruled out. For those close to the cutoff score, there is no enrollment increase and no corresponding congestion effect, and the increase in food expenditure and especially educational expenditures - indicating regular school attendance and additional spending on educational materials - may be expected to lead to increases in learning outcomes.

IV. Empirical strategy

As discussed above, while there is ample evidence that programs such as the *BDH* have been successful at raising enrollment and attendance as well as in some cases reducing child work and improving nutritional status of children, whether such outcomes also translate into higher levels of learning as measured by gains in test scores is not clear. To isolate the effect of the program on students' test scores, we begin with the following educational production function:

$$Y_i = X_i\beta + \sum_{j=1}^{3}\theta_j S_i^j + \alpha T_i + u_i \, , \qquad (1)$$

where Y_i is the outcome variable (test scores), T_i is an indicator variable that equals 1 if a child lives in a family receiving the *BDH* and 0 otherwise, X_i is a vector of individual, household, school and teacher characteristics, S_i indicates the Selben index which enters (1) as a third degree polynomial and u_i is an unobserved error term.[7] Since program participation is not random and purposively targets the poor, it is likely that T_i is negatively correlated with the error term u_i, and OLS estimates of α, the main parameter of interest, are likely to be downward biased.

[7] A potential pitfall of the RD approach is that it assumes that the relationship between the outcome variable and the variable that determines treatment is known. If one assumes the wrong functional form, estimates can be biased because of model misspecification. If, for example, the relationship is non-linear around the cutoff, but the function is specified as linear, then the estimated treatment effect may simply pick up any underlying non-linearity in the function (see Matsudaira, 2008). To deal with this problem we use a third degree polynomial of the Selben index. The choice of the third degree polynomial was based on a model selection approach using the Akaike and Schwarz information criteria. For each of the outcomes we fitted models such as (1) with one to five polynomial terms of the Selben index. In the case of both outcomes, both the information criteria suggested that a second or a third degree polynomial would be appropriate. The results presented in the paper are based on a third degree polynomial. We repeated the entire analysis using a second degree polynomial in the Selben index. Results based on a second degree polynomial are not substantially different from the results presented in the paper and are available in a supplemental appendix.

To tackle this problem, we exploit the *BDH's* targeting mechanism and rely on a regression discontinuity (RD) strategy to isolate the causal effect of the program.[8] As stated earlier, program participation is based on the Selben index and is intended only for families scoring less than 50.65 (S_0). This allocation mechanism generates a highly non-linear relationship between treatment status and the Selben index. Figure 1 illustrates this relationship and shows that as the Selben index declines (moving leftwards along the x-axis) there is an increase in the probability of being treated with a sharp spike at the cutoff point of 50.65.[9] Households with a Selben index of less than 50.65 are about 10 percentage points more likely to be in the treatment group as compared with households that have a Selben index of just above 50.65. As illustrated in the figure, the non-linear relationship between the Selben index and treatment status provides exogenous variation in treatment status which may be used to identify the causal effect of the program.

If individuals were assigned to treatment *solely* on the basis of the assignment variable, that is, all those above the cutoff point (S_0) do not receive the treatment (T_i=0 if $S_i > S_0$), whereas all those who lie below do (T_i=1 if $S_i \leq S_0$) then T would be deterministic and would depend only on the score in the Selben index. Under such circumstances ("sharp" discontinuity design), assuming that unobserved characteristics vary continuously around the cutoff with the observable characteristics used to determine treatment, the program allocation rule replicates random assignment of individuals to treatment status around the cutoff point. Accordingly, individuals lying within an arbitrarily small interval above and below the cutoff point are likely to have similar observed and unobserved characteristics and, restricting the sample to those just below and just above the cutoff and

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8 The regression discontinuity approach proposed here has often been used to evaluate the effects of educational interventions. Recent examples include Van der Klaauw (2002) and Jacob and Lefgren (2004).

9 Figure 1 is obtained from locally weighted sum of squares regressions (lowess) of treatment status on the Selben index. Following a suggestion by Imbens and Lemieux (2008), two separate lowess regressions are estimated on either side of the Selben cutoff point of 50.65 and the predicted probabilities of treatment from these two regressions are plotted versus the Selben index.

comparing test scores of children on either side of the cutoff is likely to yield unbiased program effects.[10]

In this case it is unlikely that program participation is a deterministic function of the assignment rule. As shown in Table 1 there is a fair degree of "fuzziness" in program assignment. For about 66 percent of the sample (1721/2595) eligibility and program status match, but there are 673 individuals (26 percent) who are eligible but do not receive the program and 201 individuals (8 percent) who are not eligible but do receive the program.[11] Thus, assignment to treatment status depends on the Selben index in a stochastic manner. To estimate the treatment effect in the presence of fuzzy discontinuity, following Hahn et al. (2001), we adopt an IV approach. Program participation, or the first stage equation, is treated as a function of an instrument (Z_i), a third degree polynomial of the Selben index (S_i) and other variables (X_i). The instrument is based on the decision rule and takes the value of 1 for those scoring below the cutoff in the Selben index (50.65) and the value of 0 for those scoring above the cutoff. This first stage equation may be written as:

$$T_i = X_i \delta + \sum_{j=1}^{3} \lambda_j S_i^j + \gamma Z_i + w_i \tag{2}$$

Since the instrument is based on the assignment rule it is likely to be correlated with program participation. However, we also need to assume that unobserved characteristics that determine student test performance are not correlated with the instrument, that is, we assume, $E(Z_i \cdot u_i \mid X_i, S_i) = 0$. If this assumption holds then consistent program estimates may be obtained by estimating,

$$Y_i = X_i \beta + \sum_{j=1}^{3} \theta_j S_i^j + \alpha \hat{T}_i + u_i , \tag{3}$$

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[10] That is, OLS estimates of an equation such as $Y_i^{RS} = X_i^{RS} \beta_{rs} + \delta_{rs} S_i + \alpha_{rs} T_i^{RS} + u_i$, where *RS* indicates arbitrarily restricted samples above and below the cutoff point are likely to yield unbiased estimates of the program.

[11] Leakage occurs mainly because some households who received benefits under earlier intitiatives continued to receive benefits through the *BDH* program, although based on the Selben index they were no longer eligible. On the other hand eligible households who did not participate in village-level meetings at the time that the Selben was originally being calculated, although eligible, do not receive the *BDH*.

where $\hat{T}$ is obtained from (2).[12] Estimates based on (3) provide the average treatment effect for those around the discontinuity point, that is, it is the treatment effect for those whose participation has been influenced by the assignment rule (instrument). This effect is usually termed the local average treatment effect.

IV.2 Reproducing the Selben index

The implementation of the RD design is based on the idea that the researcher has information on the Selben index and therefore on program eligibility. However, while the post-program data does have information on outcomes and several other characteristics and we know whether families are program participants or not we do not know each families score in the Selben index and nor do we have information in the post-program data on characteristics at the time that the Selben index was actually developed and used to determine program participation. Thus, in order to implement the RD strategy and replicate the assignment process the first step is to reproduce the Selben index using the post-program data.

The original Selben index was constructed using non-linear principal components analysis and a combination of 27 variables. These variables can be classified into the following groups: infrastructure (6 variables), demographic characteristics of household members (9 variables), educational characteristics of household members (4 variables), and household assets (8 variables). The index is scaled from 0 to 100. As already mentioned, families scoring below 50.65 were eligible to receive the benefit, while families scoring above 50.65 were ineligible. While the Selben is constructed using 27 variables, the post-program data that we have has information on only 20 of the 27 variables.

For the construction of the original version of the index, researchers from the Technical Secretariat of the Social Cabinet used the 1999 Living Standards Measurement Survey (LSMS). To replicate the index, we worked with the same survey (LSMS 1999) using only the 20 variables available in our post-program data. Using the same statistical

[12] Later on in the text we examine the validity of this assumption.

procedure (non-linear principal components), we re-estimated the index to obtain the new weights for the restricted set of 20 variables and created a quasi-selben index.[13] A regression of the Selben index on the quasi-selben index shows that the original Selben index can be computed based on the quasi-selben index on the basis of the following equation:

$$\underset{}{Selben} = \underset{(0.14312)}{9.159029} + \underset{(0.0032)}{0.925} * quasi\_selben \qquad (4)$$

Standard errors are in parentheses. The R-squared of the regression is 0.93.

Finally, with the new weights for the restricted set of 20 variables and using the post-program data we computed the quasi-selben index, while equation (4) was used to obtain the Selben index for each family in the post-program data set.

V. Data

The data used in this paper were gathered between November 2004 and February 2005, which is about a year and a half after the launch of the *BDH* program, by the Latin America Faculty of Social Sciences (*Facultad Latinoamericana de Ciencias Sociales, FLACSO-Ecuador)*. The fieldwork to gather data was intensive and covered the rural areas of the country and the capital Quito and utilized three different instruments. Standardized tests in mathematics and language were conducted to gather information on cognitive achievement from students in second and fourth grades and for each child the research team obtained information on school and teacher's characteristics and household variables.[14] The test scores, as well as school and teacher questionnaires, were filled out in the school, while the household questionnaire was filled out at the child's home.

The second grade sample includes 2,588 children (1,469 in the treatment and 1,119 in the control group). The school questionnaire contains information on school infrastructure, the number of teachers, the number of students, the number of classrooms,

[13] The weights attached to each of the variables and details on the variables are available in a supplemental appendix.

[14] This paper present results only for the second grade. The results for the fourth grade are similar and are available on request.

availability of books, computers and other school inputs. The teacher questionnaire was applied to the teachers in charge of mathematics and language and the survey obtained information on the teacher's education, experience, the type of contract (hired by the Ministry of Education or by the school), and the number of training courses attended during the last four years. The household questionnaire contains information on a wide range of household assets, income and expenditure, educational and demographic composition.

Table 1 presents selected descriptive statistics based on the complete sample, conditional on beneficiary status. As the table shows, there are substantial differences between beneficiaries and non-beneficiaries. Beneficiaries have lower test scores (about 10 percent lower) in mathematics and language and live in families with less educated heads of household. Regarding school characteristics, the percentage of children enrolled in schools with just one teacher, in schools belonging to the indigenous system and the percentage of children attending schools with a part-time principal, is higher amongst beneficiaries as compared to non-beneficiaries.[15] While there are no statistically significant differences in terms of access to books and learning guides, there are differences in favor of non-beneficiaries in terms of access to computers and the internet and school infrastructure.[16] Turning to teacher characteristics, once again, non-beneficiaries are more likely to be taught by teachers with a superior level of education, as well as by teachers contracted by the Ministry of Education.

To summarize, based on these descriptive statistics, it is clear that children living in non-beneficiary families have higher cognitive achievements, they belong to families with a higher socioeconomic status, and attend better schools, as compared to beneficiaries. These

[15] The Ecuadorian schooling system consists of two independent components - the Indigenous system, and the Hispanic system. Most indigenous students are enrolled in indigenous schools, where Quechua and Spanish are taught. Schools with one teacher are generally located in the poorer areas of the country. A full-time principal implies that the principal takes care of administrative issues and has no teaching responsibilities.

[16] This index is scaled from 0 to 5, and was computed using indicator variables that take the value of 1, if a characteristic is present and 0 otherwise. The index is based on access to teacher housing, potable water, electricity, bathrooms and playgrounds.

differences are consistent with the targeting strategy of the program and suggest that a simple comparison of test scores between beneficiaries and non-beneficiaries is unlikely to yield credible program estimates, and as in the regression discontinuity approach proposed here, credible program estimates are likely to be obtained only after controlling for differences in characteristics between program beneficiaries and non-beneficiaries.

VI. Results

Table 3 displays OLS estimates of the effect of the *BDH* program on tests scores. The table contains four specifications. Specification 1 includes child characteristics (sex, age and a third-degree polynomial of the Selben index). Specification 2 includes, in addition, household variables indicating whether the head of household is illiterate, indigenous, and female, as well as a set of variables that captures household composition (including the number of individuals in the household in different age groups). Specification 3 expands the specification and includes school characteristics that may have a bearing on cognitive achievements (indicator variables for urban, enrollment in a Hispanic school, enrollment in a school with one teacher or a multi-grade school, whether the school has a full time principal, access to computers, access to the internet, and the number of textbooks and learning guides per student), as well as characteristics of the teacher instructing children in mathematics and language (age, sex, education level and training, and type of contract). Finally, specification 4 includes canton (an administrative sub-region of Ecuador) fixed effects. While we present estimates based on all four specifications, the discussion focuses mainly on the most complete specification (that is, specification 4).

As shown in Table 1, on average (unconditional mean), non-beneficiaries have about a one point advantage over program beneficiaries in Mathematics and language test scores. The various estimates in Table 3 suggest that a large part of this gap in the case of mathematics and almost the entire gap in the case of language may be attributed to differences in observable characteristics. Moving along the table from left to right, there is a decline in the test score advantage for beneficiaries. However, despite this decline, based on the estimates in Table 4-specification 4, *prima facie* it appears that program participation is associated with a reduction in mathematics tests scores of about one-third of a point while there is no effect of program participation on language test scores.

Validity of the RDD

While the preceding OLS estimates control for a variety of observed characteristics, as argued earlier, they do not control for endogeneity of program participation. To control for this we exploit the program's allocation mechanism and create an instrument which allows us to obtain IV estimates of the effect of the program on test scores. First stage estimates of program participation using program eligibility as an instrument, are provided in Table 4. Across the four specifications there is a clear effect of eligibility on program participation. Consistent with Figure 1, regardless of the specification, program eligibility is associated with a 9-11 percentage point increase in the probability of receiving the program. In all cases, the partial R^2 of the excluded instrument is different from zero and the *F*-statistic on the excluded instrument is statistically significant at at least the 5 percent level with values ranging from 4.76 to 7. Overall, these figures support the idea that the assignment rule is correlated with program participation and that it is a relevant instrument.

While the requirement that the instrument and treatment status should be correlated seems to be satisfied, a remaining question is whether the instrument is uncorrelated with the error term in the test score equation. More broadly, this question may be posed in terms of enquiring whether the cutoff (locally) randomizes treatment eligibility? As discussed above, the instrument is a non-linear function of the Selben index and identifying information comes from the non-linearity imposed by the program design. Given this structure, there seems to be little reason to expect that after controlling for observable characteristics and a flexible functional form of the Selben index, an arbitrarily imposed cutoff point (over which families have no control) in the Selben index should be correlated with unobserved characteristics that determine test scores.

Notwithstanding this argument, there are reasons why the assumption of no correlation between the instrument and error term may be violated. For instance, households may be able to manipulate the Selben index by underreporting their assets and thereby increasing their chances of receiving the benefit. If households are able to do this then samples on either side of the cutoff may not be comparable and local randomization breaks down. If such manipulation were taking place, it should be manifested in terms of a discontinuity in the density of the Selben index around the cutoff point. An examination of the density of the Selben index does not display signs of discontinuity (see Figure 2). More specifically, the number of observations 1, 2, and 3 points above (below) the cutoff are 80 (53), 149 (106), and 207 (178), respectively. If the Selben index was being manipulated then we would expect a considerably higher number of cases just below the cutoff (to enhance eligibility) as compared to above the cutoff. However, in this case there are fewer observations immediately below the cutoff. More formally, following McCrary's (2008) argument and the approach used by Matsudaira (2008), we test for discontinuity in the density of the Selben index by regressing the log of the fraction of observations with each value of the Selben index on a linear term in the Selben index and a dummy equal to one for observations below the cutoff of 50.65. The coefficient on the dummy is statistically insignificant (*p*-value 0.321), indicating that there are no statistically evident discontinuities in the neighborhood of the cutoff.

An implication of the idea that the allocation rule creates local randomization at the cutoff point is that all preset observable and unobservable characteristics (that is, fixed at the time of the creation of the Selben index) should be similar for those children/households scoring just above and just below the cutoff point. While we can never be certain that the unobservable characteristics satisfy this condition, the validity of this assumption can be explored by examining differences in the means of the preset observable characteristics around the cutoff point. Accordingly, we examined differences in the means of various observable characteristics ± 1, 2, 3 points around the cutoff. These differences are reported in Table A1. As the table shows, for the most part, these differences are small and statistically insignificant. Under the null hypothesis that all student characteristics and in all sub-samples are balanced, we may expect about 5 to 10 percent of the estimates to be statistically different from each other. In this case 3 of the 30 estimates or 10 percent are significant.

Overall, there is no evidence of manipulation of the Selben index and local differences in observed characteristics are small. This provides support for the assumption that unobservable characteristics are balanced and that the assignment rule is a valid instrument.

IV estimates

IV estimates of the effect of *BDH* on test scores are provided in Table 5. At first glance these estimates look implausibly large, positive and statistically significant. However, estimates based on the most comprehensive specification (Table 5, specification 4) display that for both mathematics and language the effect of the program on test scores is statistically insignificant. Although, insignificant, as compared to the OLS estimates, the IV estimates indicate that there is a positive relationship between program participation and test scores or put somewhat differently, there seems to be no evidence that program participation has a negative effect on test scores (as indicated by the OLS estimates). The larger IV estimates also suggest that there is a negative correlation between the errors in the test scores and program participation equation and that in the absence of controls for differences in unobserved characteristics between program beneficiaries and non-beneficiaries, there would be a tendency to underestimate the impact of the program on test scores.

While the IV estimates are not statistically significant, their size warrants additional discussion. The IV estimate of the program on test scores is the ratio of the differences in average test scores and increase in the probability of participation (controlling for other variables) between individuals whose participation has been influenced by the assignment rule and those unaffected by the assignment rule.[17] The IV estimate depends on the marginal effect of the program on the group whose probability of participation is affected

[17] The IV estimate of the program on test scores is the ratio of the reduced form coefficients on *T* in the test scores and participation equations. That is, using the estimates displayed in Tables 4 and 6 (specification 4), we have for mathematics, 4.899= 0.486/0.097 and for language, 0.402 = 0.039/0.097.

by the assignment rule. If the assignment rule affects a group with a high marginal return from the program then the IV estimate, which is the average treatment effect for those affected by the assignment rule, may be quite large. In this case the large size of the IV estimate suggests that the group of individuals who are around the cutoff point experience a large increase in test scores, although the effect is not precise.

An alternative possibility is that the large size of the estimated test score effect and the large standard errors are driven by a weak instrument, that is, the instrument and the endogenous variable are weakly correlated. For example, Bound et al. (1995) and Stock et al. (2002) point out that if instruments are weak then IV point estimates and hypothesis tests are unreliable. As discussed earlier and as shown in the first-stage regressions, the correlation between treatment status and the instrument is non-zero and does not seem to be unduly weak. More formally, to examine the effect of weak instruments, following Stock et al. (2002) we computed the Anderson-Rubin (AR) statistic, in lieu of the *t*-statistic on the treatment dummy (α in equation 3). The AR statistic is an alternative to the standard *t*-statistic and is robust to weak instruments. In the case of Math (Table 5, specification 4) the *p*-value of the AR statistic is 0.207 as opposed to a *p*-value of 0.271 for the *t*-statistic. In the case of language, the *p*-value on the *t*-statistic and on the AR statistic is 0.916. These statistics suggest that the lack of a statistically significant program impact is not driven by a weak instrument.

VII. Concluding remarks

Throughout Latin America, CCT programs play an important role in social policy. These programs aim to reduce poverty and to promote accumulation of human capital. On the educational front, several papers have shown the substantial impact of these programs on boosting school enrollment and ensuring regular school attendance. While these are the first steps to enhance educational attainment, if the aim of these programs is to build human capital and break the cycle of poverty then a focus beyond enrollment, on learning and gains in cognitive skills may also be required. While there is a considerable body of work on the effect of CCT on enrollment and attendance, their effects on learning as measured by effects on test scores has not been extensively examined.

This paper contributed to the body of work on the impact of cash transfer programs by using information from Ecuador and by focusing on the effect of the program on test scores. We exploited the program's design and used an arguably credible empirical strategy to show that the *BDH* does not have a statistically significant positive impact on test scores amongst those close to the program eligibility threshold. Although methodologies differ, the lack of an impact is similar to Behrman et al. (2000), who find no impact of a CCT program on test scores in Mexico.

We analyze the effect of the program on test scores a year and a half into the program and it is possible that going forward, the program may well exert a positive effect on learning. Alternatively, nutritional interventions through the *BDH* program for children in the age group 0-5 may lead to increases in learning. These effects have yet to be evaluated.

Nevertheless, a consideration of the results reported here along with the existing body of work on the effect of such programs highlights the strengths and potential limitations of such programs. While such interventions are successful at getting children to enroll and attend school and lead to increased household expenditure, expecting that such programs will also result in higher levels of learning is at the very least, premature. Rather than focusing only on the demand-side, alternative and additional programs that also consider the supply-side, for example, getting teachers to come to school may have a larger bearing on learning than demand-oriented cash transfer programs. [18]

[18] A recent report based on a nationally representative teacher tracking survey conducted in Ecuador in 2002, reports a teacher absenteeism rate of 14 percent (see http://siteresources.worldbank.org/DEC/Resources/37912_Ecuador.Teacher.Absenteeism.August13.2004.pdf, accessed on July 21, 2008.

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Table 1
Assignment rule and treatment status

	Selben score		
Treatment status	More than 50.65	Less than 50.65	Total
Non-beneficiaries	450	673	1,123
Beneficiaries	201	1,271	1,472
Total	651	1,944	2,595

Table 2: Descriptive statistics for selected variables

Child and Household Characteristics	**Non-beneficiaries (Std. Dev.)**	**Beneficiaries (Std. Dev.)**	**Difference (Std. Err.)**
Mathematics, second grade (out of 20)	9.393	8.494	0.899*
	(5.097)	(5.200)	(0.204)
Language, second grade (out of 20)	11.130	10.217	0.912*
	(4.765)	(4.773)	(0.189)
Score in Selben index	44.33	36.57	7.754*
	(14.48)	(11.85)	(0.517)
Dummy sex (1=female)	0.491	0.480	0.011
	(0.500)	(0.499)	(0.0198)
Household head is indigenous	0.356	0.564	-0.208*
	(0.479)	(0.495)	(0.0193)
Household head is illiterate	0.125	0.177	-0.0517*
	(0.331)	(0.382)	(0.0143)
Household head is female	0.131	0.127	0.004
	(0.338)	(0.333)	(0.013)
Number of persons aged less than 6	1.138	1.410	-0.271*
	(0.0474)	(0.0434)	(0.065)
Number of persons aged 6 to 17	3.771	4.383	-0.613*
	(3.191)	(0.086)	(0.129)
School characteristics			
Children attending schools with one teacher (%)	0.136	0.195	-0.0587*
	(0.343)	(0.396)	(0.0148)
Children attending Hispanic schools (%)	0.710	0.583	0.127*
	(0.453)	(0.493)	(0.019)
Children residing in Quito (%)	0.250	0.101	0.148*
	(0.433)	(0.301)	(0.0143)
Children attending schools with full-time principal (%)	0.237	0.102	0.135*
	(0.426)	(0.303)	(0.014)
Learning guides per child	0.0596	0.0571	0.002
	(0.222)	(0.200)	(0.008)
Children attending schools with computers (%)	0.694	0.542	0.152*
	(0.460)	(0.498)	(0.019)
Number of books per pupil	1.523	1.811	-0.288
	(3.996)	(3.992)	(0.158)
Children attending schools with access to internet (%)	0.108	0.0461	0.062*
	(0.311)	(0.209)	(0.010)
Index of school infrastructure (out of five)	3.717	3.546	0.171*
	(0.853)	(1.032)	(0.038)
Teacher characteristics			
Female teacher	0.626	0.577	0.0494*
	(0.483)	(0.494)	(0.019)
Age of teacher	37.63	37.24	0.381
	(10.14)	(10.44)	(0.408)
Educated to the superior level	0.764	0.696	0.0685*
	(0.424)	(0.459)	(0.0176)
Ministry of education contract	0.792	0.752	0.0395*
	(0.406)	(0.431)	(0.017)
Number of training courses received by teachers	6.611	7.415	-0.804*
	(6.981)	(10.318)	(0.357)
Number of cases [a]	1123	1472	

Notes: * Significant at 1 percent level, ** significant at 5 percent level, *** significant at 10 percent level. [a] For Math (language) test scores the number of observations are 1119 (1118) for non-beneficiaries and 1469 (1471) for beneficiaries.

Table 3
OLS estimates of BDH on Test Scores

Mathematics	Specification 1	Specification 2	Specification 3	Specification 4
T	-0.507**	-0.449***	-0.628*	-0.326***
	(0.219)	(0.219)	(0.213)	(0.202)
R^2	0.021	0.029	0.111	0.278
N	2588	2588	2588	2588
Language				
T	-0.254	-0.195	-0.228	-0.038
	(0.198)	(0.198)	(0.191)	(0.184)
R^2	0.054	0.06	0.15	0.247
N	2589	2589	2589	2589

Notes: Standard errors in parentheses are corrected for heteroskedasticity. *Significant at 1 percent level, ** significant at 5 percent level, *** significant at 10 percent level.

Table 4
Participating in BDH

Variable	**Specification 1**	**Specification 2**	**Specification 3**	**Specification 4**
Below cutoff point of 50.65 (Z)	0.101**	0.111*	0.093**	0.097**
	(0.042)	(0.042)	(0.043)	(0.042)
R^2	0.113	0.121	0.138	0.191
Partial R^2 of excluded instrument	0.0025	0.0030	0.0021	0.0024
F-statistic on excluded instrument	5.79**	7.00*	4.76**	5.3**
N	2595	2595	2595	2595

Notes: Standard errors in parentheses are corrected for heteroskedasticity. *Significant at 1 percent level, ** significant at 5 percent level, *** significant at 10 percent level.

Table 5
IV Estimates of BDH on test scores

Mathematics	**Specification 1**	**Specification 2**	**Specification 3**	**Specification 4**
T	16.457*	13.710*	8.679	4.899
	(8.107)	(6.534)	(6.088)	(4.507)
N	2588	2588	2588	2588
Language				
T	10.064*	8.15*	2.04	0.402
	(5.6953)	(4.675)	(4.233)	(3.866)
N	2589	2589	2589	2589

Notes: Standard errors in parentheses are corrected for heteroskedasticity. *Significant at 1 percent level, ** significant at 5 percent level, *** significant at 10 percent level.

Table 6
Reduced form test score estimates

Mathematics	**Specification 1**	**Specification 2**	**Specification 3**	**Specification 4**
Below cutoff point of 50.65 (Z)	1.690*	1.548*	0.822*	0.486
	(0.431)	(0.432)	(0.418)	(0.390)
N	2588	2588	2588	2588
R^2	0.024	0.032	0.109	0.277
Language				
Below cutoff point of 50.65 (Z)	1.021*	0.912*	0.191	0.039
	(0.392)	(0.393)	(0.384)	(0.371)
N	2589	2589	2589	2589
R^2	0.055	0.061	0.15	0.247

Notes: Standard errors in parentheses are corrected for heteroskedasticity. *Significant at 1 percent level, ** significant at 5 percent level, *** significant at 10 percent level.

Table A1
Descriptive statistics for selected variables around cutoff

Child and Household Characteristics	**Difference ± 1 (Std. Err)**	**Difference ± 2 (Std. Err.)**	**Difference ± 3 (Std. Err.)**
Dummy sex (1=female)	-0.035	0.027	0.053
	(0.088)	(0.064)	(0.051)
Age of child	-0.096	0.007	-0.009
	(0.202)	(0.139)	(0.107)
Household head is indigenous	0.048	-0.031	-0.014
	(0.074)	(0.053)	(0.041)
Household head is illiterate	-0.076***	-0.021	-0.007
	(0.044)	(0.033)	(0.026)
Household head is female	-0.108	-0.010	-0.014
	(0.068)	(0.051)	(0.041)
Number of persons aged less than 6	0.108	0.010	-0.020
	(0.166)	(0.118)	(0.089)
Number of persons aged 6 to 17	0.904*	-0.021	-0.322
	(0.334)	(0.29)	(0.255)
Number of persons aged 18 to 44	0.325	0.014	-0.128
	(0.203)	(0.148)	(0.121)
Number of persons aged 45 to 64	-0.083	-0.136	-0.144***
	(0.116)	(0.093)	(0.077)
Number of persons aged 65 and older	-0.057	-0.040	0.018
	(0.069)	(0.044)	(0.036)

Notes: * Significant at 1 percent level, ** significant at 5 percent level, *** significant at 10 percent level.

Figure 1

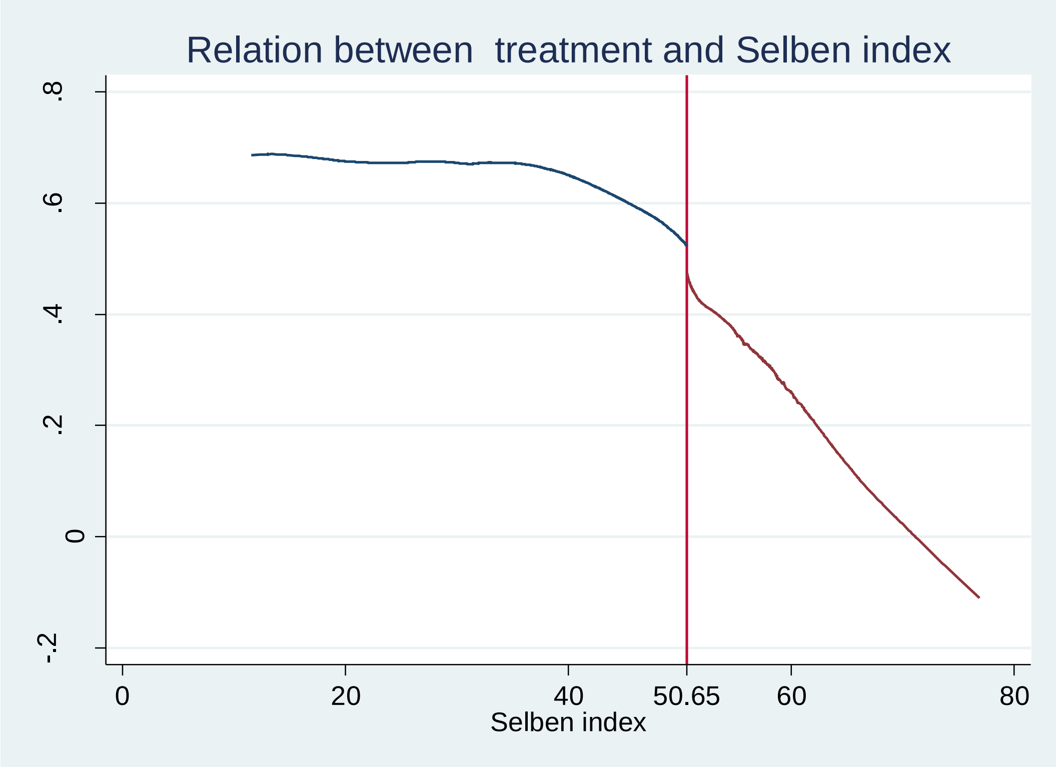

Figure 2
Examining discontinuities around the cutoff