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FACULTAD LATINOAMERICANA DE CIENCIAS SOCIALES - SEDE ECUADOR

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*Profesor investigador del Programa de Economía de FLACSO – Sede Ecuador y PhD© del Instituto de Estudios Sociales (ISS) de la Haya (este documento es parte de la disertación doctoral). E-mail: jponce@flacso.org.ec. Agradezco los útiles comentarios y sugerencias de María Caridad Araujo, Arjun Bedi, Paúl Carrillo, Hessel Oosterbeek , Norbert Schady, Rob Vos y Steve Younger.

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cooreconomia@flacso.org.ec

ABSTRACT

This paper evaluates the impact of a conditional cash transfer program on school enrolment in Ecuador. By combining several methodological approaches, such as regression discontinuity, difference in difference, and matching estimates, the paper finds no significant impact of the program on school enrolment.

One possible explanation of this result is that Ecuador, unlike most Latin America countries that have conditional cash transfer programs, has no enforced the conditions to receive the transfer. In this sense, the program is not a conditional cash transfer, but a cash transfer. However, results remain robust to different specifications, even after controlling for a proxy variable that takes into account the application of the conditionality.

1 INTRODUCTION

This paper analyses the impact of a conditional cash transfer (CCT) program on school enrollment in Ecuador by using a regression discontinuity design. As in the majority of Latin American countries, conditional cash transfer programs play an important role in social policy in Ecuador. The main objective of this kind of programs is to improve human capital accumulation among poor people, especially the young and children, as a means to breaking the inter-generational cycle of poverty. In the short term, CCT programs seek to increase family income among poor families. In this way, conditional cash transfer programs try to reduce both temporary and structural poverty; by targeting the transfer to poor households, those programs alleviate short-term poverty, while by linking the transfer to investments in human capital those programs address long-run poverty.

The main idea of CCT programs is to provide money to poor families conditional on investments in human capital such as sending children to school or bringing them to health centers on a regular basis. In this sense, typically two lines of human capital intervention are common to CCT programs: education and health.

CCT programs appear in response to the insufficiency of supply-side interventions in promoting access of poor people to basic services. Although supply-side interventions increase the availability and quality of schools and health centers, those are not enough to guarantee the access of poor people to education and health services. To have access to such services, poor people face some costs -such as information, transportation and opportunity costs- that generally can not be afforded. As a consequence, the poor have been traditionally excluded from basic services. Conditional cash transfer programs attack this problem by targeting transfers to poor households and by conditioning these transfers on attendance at school and health clinics.

Regarding education, most of the CCT programs in Latin America focus on improving the access of children to school. Impact evaluation studies of the majority of CCT programs show an important impact on school enrollment.

The paper is organized as follows. The first part presents a review of the main CCT programs in Latin America, as well as their impact evaluation results. The second part characterizes the *Bono de Desarrollo Humano* of Ecuador. The third part describes the data used and introduces some descriptive statistics. In the fourth part, the methodological approach is introduced. Results and discussion are presented in the fifth part. The last part concludes.

2 CONDITIONAL CASH TRANSFER PROGRAMS IN LATIN AMERICA

The first Latin America CCT program started in 1995 in Brazil under the government of the Distrito Federal of Brasilia (*Bolsa Escola*). The second experience of a CCT program is *Progresa* (now re-named O*portunidades*) from Mexico that started operations in 1997. After that, most countries in the region implemented CCT programs. Honduras restructured a safety net program into a CCT program in 1998 (*Programa de Asignacion Familiar PRAF*). Nicaragua started its CCT program in 2000 (*Red de Protección Social*). Costa Rica has also a CCT program called S*uperémonos*. Colombia started its CCT program in 2001 (*Familias en Acción*). Argentina (F*amilias por la Inclusion Social*), Uruguay (P*royecto 300*), Chile (*Chile Solidario*), and Jamaica (*Programa de Avance Mediante la Salud y la Educacion*) are among the countries that have a conditional cash transfer program[1].

Most of those programs have impact evaluation studies, and in the majority of cases important impacts on school enrollment, as well as other components of human capital, were found. This part focuses on the impact of CCT on school enrollment. Three CCT have been evaluated using an experimental design. *Progresa* from Mexico, *Red de Protección Social* from Nicaragua, and *PRAF* from Honduras.

The basic idea of an experimental study is to compare two population groups of observation that have no systematic differences except that one group receives the treatment ("treatment group") and the other does not ("control group"). The simplest method is to take a sample of the population of potential beneficiaries and randomly divide the sample into "treatment" and "control" groups ("randomization"). Differences in the variables of interest across the two groups are unbiased estimates of the effect of the treatment.

In all three cases communities, which were potential beneficiaries, were randomly assigned into control and treatment groups. In the case of Mexico, at the primary school level, where the enrollment rates before program implementation were between 90 and 94 percent, statistical methods that control for the age and family background of children as well as community characteristics reveal that *Progresa* succeeds at increasing the enrollment rate of boys by 0.74 to 1.07 percentage points and of girls by 0.96 to 1.45 percentage points (Schultz, 2004). At the secondary level, where the initial enrollment rates before *Progresa* were 67% for girls and 73% for boys, the

[1] See Rawlings and Rubio (2003), Caldés, Coady, and Maluccio (2004), and Villatoro (2005) for reviews.

increase in enrollment effects for girls range from 7.2 to 9.3 percentage points and for boys from 3.5 to 5.8 percentage points. (Schultz, 2004). In Nicaragua, the program was directed to children up to the fourth grade of primary school. Results of the impact evaluation study show an increase in enrollment of 18 percentage points in favor of the treatment group when compared with the control group. In addition there was an even larger increase, 23 percentage points, in the percentage of children regularly attending school (during the previous month). In tandem with increased enrollment, the percentage of children aged 7 to 13 who were working declined by 5 percentage points and was half as large in treatment areas (6%) as in control areas (13%). (Maluccio and Flores, 2004). Finally, Honduras has planned an experimental evaluation of its programs. Unfortunately results are still not available.

On the other hand, some CCT has been evaluated using less robust methodologies than experimental designs. As an example, the program *Superémonos* from Costa Rica was evaluated using a propensity score matching procedure. Results show an increase in the probability of attending school of 5 percentage points in 2001, and 8.7 percentage points in 2002. (Duryea and Morrison. 2004). Attanasio et, al. (2006) by using a difference in difference combined with a propensity score matching found a significant and positive impact of *Familias en Acción* on school enrollment by 5 to 7 percentage point among children form 14 to 17 years old in rural Colombia.

3 COUNTRY BACKGROUND AND PROGRAM DESCRIPTION

Ecuador is a lower-middle income country. In 2004, its per capita GDP was 1,435 in constant 2000 US dollars. The country is characterized by high poverty levels (around 61% using the criteria of unmet basic needs according to the 2001 population census), as well as high inequality (the Gini coefficient of consumption was 0.47 in the 1999 LSMS). The Ecuadorian economy was dollarized in January 2000. After that, two trends can be observed. On the one hand, some improvements on poverty reduction because of wage and salaries appreciation are observed, and, on the other hand, a permanent increase in inequality is present.

Regarding education, the country has made important improvements in its education results in the last few decades. As an example, the average number of years of schooling of the population aged more than 24 years old continued to increase from 6.7 to 7.3 between 1990 and 2001. The net enrollment rate, for primary and secondary level, increased from 68.6 and 29.5 in 1982 to 88.9 and 43.1 in 1990 respectively. Despite such improvements, during the 1990s the country faced a serious problem with school enrollment. In this sense, in 2001 the net enrollment rates for both primary and secondary levels remain stagnated around the values of 1990 (90.1% and 44.6% respectively). This disappointing performance contrasts with educational goals. The decade of the 1990s was the decade of “Education for All”, and Ecuador also subscribed to several international declarations emphasizing the importance of education. In addition, at the end of the 1990s, the Ecuadorian government engaged in new programs aiming at improving the access to primary education and school achievements: cash transfer programs (the “Bono Solidario” and “Beca Escolar”) and a School-meal program (“Programa de Alimentacion Escolar”) were established and expanded, among other objectives in order to increase access to schooling of the poorer segments of the population. (Vos and Ponce, 2005). Paradoxically, educational inputs showed remarkable improvements during the same period. The pupil-teacher ratio for primary education declined from 30 in 1990 to 23 in 2001.

The “Bono Solidario” program started in 1998 as a safety net to compensate poor families for the elimination of gas and electricity subsidies. Initially the program used a self-targeting strategy directed to mothers with earnings below US$ 40 dollars, the disabled and elderly people. While the immediate political justification for this program was to compensate the poor for losses in their real purchasing power caused by statutory increases in (heavily subsidized) petroleum and natural gas prices, the program quickly took on a life of its own, becoming the government's largest social expenditure outside of education, with total transfers equal to about one percent of GDP (León, Vos, and Brborich, 2001). By comparison, public education and health expenditures account for two-and-a-half and a bit less than one percent of GDP, respectively. The transfer was modest, but non-trivial by Ecuadorian standards. At the time that the program started, mothers received 100,000 sucres per month, about US$15, and the elderly and handicapped received 50,000. In April of 1999, those amounts were increased by 50%, mostly to account for high inflation. On average, the share of *Bono* income in total household expenditures was 11 percent in 1999. During 2000 the program reached around 1.2 million beneficiary households, representing about 45 percent of Ecuadorian households.

An impact evaluation conducted by Vos et., al., (2001) using a matching comparison strategy showed a positive impact of around 5 percentage points on school enrollment, although no significant impact was found on poverty indicators. Another study, conducted by León and Younger (2004), using an instrumental variable approach show that the program has quite modest but significant effects on children's nutritional status.

In 2003 the program was reformulated and became a CCT. The program was renamed as *Bono de Desarrollo Humano* (BDH). The main objective of the program is to improve the formation of human capital among the poor of Ecuador. The program has two components: education and health. The education component requires children from 6 to 15 years old to enroll in school and to attend at least 90% of school days in a month. The health component requires children under six years old to attend health centers for medical check-ups bimonthly. Unlike to other CCT programs in Latin America, until now, the BDH has no mechanisms to verify the conditionality. In this way, households are not taken off program rosters if their school-aged children are not enrolled in school or fail to attend classes regularly.

To select the beneficiaries the program uses an individual targeting strategy based on a proxy mean test computed by Selben (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 nonlinear principal components analysis[2]. Families pertaining to quintiles 1 and 2 receive the benefit (those are families that score less than 50.65 in the Selben index). At this moment around 90% of the beneficiaries of the program have the corresponding score in the Selben index. The difference is composed of families that do not have the Selben survey, but receive the program since its initial implementation. The government expects to finish the retargeting process during 2006. Currently the program consists of a cash transfer of US$ 15 per month and per family. The annual budget of the program reached US$ 190 million dollars in 2004 (around 1% of the GDP).

An impact evaluation conducted by Schady and Araujo (2005) used an experimental design. Families above and below the first Selben quintile threshold[3] were randomly assigned to treatment and control groups. After verifying the validity of the experiment, the paper uses a difference in difference strategy and concludes that the program has positive effects on enrollment (around 10 points), and negative effects on child work (about 17 percentage points). Two additional findings of this research are remarkable. First, the study shows considerable and significant heterogeneity of program effects along various dimensions. Especially, larger impacts are found among older children and among poorer households, while no significant effects are found among the less poor of the sample. Second, the paper finds an important role of conditionality. Among households that believe that children must attend schools to receive the benefit the impact is larger, and it is significant even among the less poor. (Schady and Araujo, 2005).

[2] The index is scaled from 0 to 100; 0 for the poorest and 100 for the richest.
[3] Those are the extremely poor.

4 DATA AND DESCRIPTIVE STATISTICS

The study was built around a regression discontinuity design, and uses the fact that families scoring above the cutoff point in the Selben index do not receive the BDH (group A), while families scoring below do receive the BDH (group B). Accordingly, families around the cutoff point in the Selben index[4] were drawn from the Selben rosters of four of twenty two provinces in the country: Carchi, Imbabura, Cotopaxi and Tungurahua. All four provinces are located in the highlands region. The sampling scheme uses a two-stage process. Within the provinces, parishes were randomly drawn and, within these parishes, a random sample of households was taken. Households who had previously received transfers from the *Bono Solidario* program were excluded because it was unclear how earlier transfers made by this program could have affected schooling decisions. Finally, the sample selected only households with at least one child aged from 6 to 15 years old at the time they were surveyed by the Selben.

The Technical Secretariat of the Social Cabinet contracted the *Universidad Católica del Ecuador* (PUCE) to carry out the base-line and the follow-up surveys[5]. The base-line survey was taken between June and August 2003, while the follow-up survey was carried out between January and March 2005.

The survey instrument includes a register of every household member, their names and id number (when available), as well as their sex, age and relationship with the head of the household. In addition, the survey takes information on schooling level, parents' level of education, marital status, and language spoken by all household members. For children aged between 5 to 17 years, the instrument has information on current enrollment (level and grade), causes in case of no enrollment, and other variables related to the type of school the child attend. In addition an employment module for those children is also included. Finally, the instrument includes a complete module of household expenditures, which very closely follows the structure of the 1999 Ecuador LSMS. One important feature of the survey is that it includes all the variables that are used to construct the Selben index, as well as an indicator that takes the value of 1 if the person pertains to a household that receives the HDB, and 0 otherwise.

The follow-up survey also includes a module on access to and perception of the HDB program. In this module households were asked whether they know of the HDB

[4] The cutoff point in the Selben index is 50.65. Families between 47.65 and 53.65 in the Selben index were selected.
[5] This process had the financial and technical support of the World Bank.

program, how they had heard of it, if appropriate, and whether they receive HDB transfers. In addition, the respondents were asked whether they believed that household had to comply with any requirements or conditions to receive the transfers.

The attrition rate over the two surveys is low: 95.9 percent of households at the baseline were re-interview in the follow up survey. In addition, no significant differences are found between households who were and were not re-interviewed in log per capita expenditures. Attrition can introduce bias when it is correlated with treatment status (Angrist, 1997). A regression of a dummy variable for households who were and were not re-interviewed on a dummy for study groups ("control" group A=0, or "treatment" group B=1) has a coefficient of 0.0012, with a robust standard error of 0.11. The previous show that attrition is not a possible source of bias in this research.

The sample was limited to children aged from 6 to 17 years, and to households that were re-interview in the follow-up survey. There were 2,384 children aged 6-17, and 1,463 household that were interviewed in both surveys. Data on all key variables are available for all households in the sample, with the exception of parental education, which is missing in a small number of cases.

In the sample, families from group A (that do not receive treatment) score from 50.66 to 53.64 in the Selben index, while families from group B (that receive the treatment) score from 47.66 to 50.64. As was said, the cutoff point is 50.65.

Descriptive statistics for both groups are introduced in table 1. As expected, group A has a significantly higher score in the Selben index than group B. In addition, significant differences between group A and B are found in the log of percapita expenditure, father's education, mother's education, ethnicity of household head, and household composition. No significant differences are found in children's school enrollment, age and sex of children, sex of household head, literacy of household head, and household size.

One potential problem in this research is that study groups (A and B) were contaminated. From a total of 617 families of group A, 49 (8 percent) received the benefit. In this case the contamination was minimal, and responded to some administrative error of program operators. Regarding to the treatment group, only 544 (72 percent) received the program from a total of 760 families. Lack of information, some costs associated with the inscription to receive the transfer, and stigma associated with being a BDH recipient are all plausible explanations in this case.

Table 1 also presents descriptive statistics for those that in fact receive and do not receive the transfer. In this case there are significant differences in child's age, the log of per-capita expenditure, score in the Selben index, father's and mother's education, household size and household composition. No significant differences are found in child' enrollment, the sex of children, and sex, ethnicity, and literacy of household head.

In this regard, there appear to be clear differences between transfer recipients and non-recipients especially regarding to education level. It seems that households with higher education levels are more likely to participate in the program.

5 METHODOLOGY

Several methodological approaches are used to estimate the program impact. First, as was said, the research uses the fact that families scoring below the cutoff point in the Selben index receive the treatment, while families scoring above do not. The cutoff point in the Selben index is 50.65. This kind of selection process allows one to use a regression discontinuity (RD) design. The RD is a quasi-experimental design that uses the fact that the probability of receiving treatment changes discontinuously as a function of one or more underlying variables. (Hahn, Todd and Van der Klaauw, 2001)[6]. One can start with the following equation to identify the program impact on school enrollment.

$$Y_{it} = X_{it-1}\chi_1 + \chi_2 S_i + \chi_3 T_i + u_{it} \qquad (1)$$

where Y is the outcome variable taking the value of 1 if the child is enrolled at the follow-up survey, X is a vector of individual, household and community level characteristics at base line[7], S is the score in the Selben index, and T is an indicator variable taking the value of 1 if the person receives the treatment and 0 otherwise. Assuming that unobservable characteristics do not vary discontinuously around the cutoff, the assignment rule into the program provides exogenous variation in the treatment. One can thus identify the impact of the program by simply comparing children pertaining to families scoring just below and above the cutoff point in the Selben index. (Jacob and Lefgren, 2002). The fundamental assumption behind the RD design is that unobserved characteristics vary continuously around the cutoff point (S_0). This assumption may not hold if individuals can influence their position relative to the cutoff. In the case of the BDH, families know nothing about the way in which the Selben index is computed.

One can find in the literature two types of RD design. First, the "sharp design", where individuals are assigned to treatment solely on basis of an observed continuous variable (S), called the selection or assignment variable: those who fall above the cutoff

[6] Some examples of studies using RD design can be found in Thistlethaite and Campbell (1960), Shahidur Khandker (1998), Sandra Black (1999), Angrist and Lavy (1999), Hahn, Todd and Van der Klaauw (1999), Van der Klaauw (2002), Jacob and Lefgren (2004), Chay, McEwan, and Urquiola. (2005).

[7] Baseline characteristics are used because program can affect some of these characteristics, in this sense using follow up characteristics can bias estimates.

point do not receive the treatment (T_i=0 if S_i>=S_0), whereas those who fall below the cutoff point do receive the treatment (T_i=1 if S_i<S_0). In this case, T is deterministic and depends on some observable variable. T_i=f(S_i). Where S_i takes on a continuum of values and the point S_0, where the function f(S) is discontinuous, is assumed to be known. In this case, OLS estimates of equation (1) will be unbiased.

In some cases, however, treatment may be partly determined by other factors, leading to a "fuzzy" discontinuity. For example, there are some people scoring above the cutoff that receives the HDB, and some people scoring below the cutoff that do not receive the program. Even in this case, as the probability of treatment changes discontinuously at the cutoff, it is possible to determine the treatment effect by comparing mean outcomes of individuals in a narrow range on either side of the cutoff. In this case one needs to correct the estimation for the probability of receiving the treatment, and instrument treatment status with the initial assignment into the treatment and control group. (Hahn, Todd and Van der Klaauw, 2001). The reduced form equation in this case is:

$$Y_{it} = X_{it-1}\chi_1 + \chi_2 S_i + \chi_3 Z_i + u_{it} \tag{2}$$

where T is instrumented by Z. Z being an indicator variable that takes the value of 1 if the person scores below the cutoff point, and the value of 0 if the person scores above the cutoff. In this case, the first stage is:

$$T_i = X_{it-1}\gamma_1 + \gamma_2 S + \gamma_3 Z_i + w_i \tag{3}$$

One pitfall of the IV approach is that it assumes that the relationship between the outcome variable and the variable that determines treatment is known. If, for example, the relationship is not linear around the cutoff, but one specifies the function as linear, then the estimated treatment effect may simply pick up any underlying non-linearity in the function. In this sense, it seems reasonable to use several functional forms of the Selben index in estimating equation (2). The robustness of estimates is checked by including second and third order polynomials of the Selben index in equation (2).

In addition, one can estimate the program impact using non-parametric techniques. In this case, the program impact, α, under the fuzzy design, can be estimated by:

$$\alpha = \frac{Y^{+} - Y^{-}}{T^{+} - T^{-}} \qquad (4)$$

where $Y^{+} \cong \lim_{S \to S^{+}{}_{0}} E[Y_i \mid S_i = S]$, $Y^{-} \cong \lim_{S \to S^{-}{}_{0}} E[Y_i \mid S_i = S]$, and $T^{+} \cong \lim_{S \to S^{+}{}_{0}} E[T_i \mid S_i = S]$, and $T^{-} \cong \lim_{S \to S^{-}{}_{0}} E[T_i \mid S_i = S]$.

As was said, T is the treatment variable that takes the value of 1 if the person receives the treatment and 0 otherwise[8]. The limits in equation (2) can be estimated by nom-parametric techniques. The main idea of equation (2) is to compare the output of individuals nearly above the cutoff point (Y^+) with the output of individuals nearly below the cutoff point (Y^-), correcting by the probability of receiving treatment of those nearly above the cutoff point (T^+), and of those nearly below the cutoff point (T^-).

Regarding non-parametric estimates, it is widely known in the literature the poor boundary performance of standard Kernel estimators. Because of that, this paper will use local linear regression to estimate the limits of equation (4). This estimation technique has better boundary properties (Hahn, Tod, and Van der Klaauw, 1999; Han, Todd and Van der Klaauw, 2001; and Fan, 1992), as well as high asymptotic efficiency (Fan, 1992). However, one potential pitfall of non-parametric estimators is that those are sensitive to different bandwidths. For this reason one should present results of equation (4) for different bandwidths.

Estimates of equations (1) and (4) represent the effect of treatment on the treated. In this case, due the fact that a RD sample is used, those estimates are local average treatment effects (LATE). On the other hand, estimates of equation (2) represent the intent to treat (ITT) effect. In this case one uses the initial assignment to control (group B) and treatment group (group A) as the treatment variable.

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[8] See Hahn, Todd and Van der Klaauw (2001) for more details.

The second methodological strategy is a difference in difference (DD) approach[9]. Because of the original design of this research, what one has is a combination of a DD approach applied to an RD sample. In this case, one compares the situation of treatment and control group before the starting of the program with the situation after certain period of program application. As was said, the follow up survey was taken one and a half year after the baseline. Changes in enrollment status (between the base-line and the follow-up surveys) can be used to estimate program effects. The following options are found. First, children enrolled at base-line. The dependent variable in this case takes the value of 1 if the child was enrolled at base-line and dropped out between the base line and the follow-up survey; and the value of 0 if the child stayed enrolled at the follow-up.

$$Y_{it} - Y_{it-1} = X_{it-1}\beta_1 + \beta_2 T_i + e_{it} \mid (Y_{it-1} = 1) \qquad (5)$$

The parameter β_2 measures differences between those receiving the program and those not receiving in dropping out of school. Because of the RD design of the sample, β_2 is the LATE of school dropout.

Another possibility is new enrollment. In this case the sample is reduced to those who were not enrolled at the base line. The dependent variable takes the value of 1 if the child enrolled at the follow-up, and the value of 0 if the child stayed not enrolled.

$$Y_{it} - Y_{it-1} = X_{it-1}\delta_1 + \delta_2 T_i + \eta_{it} \mid (Y_{it-1} = 0) \qquad (6)$$

The parameter δ_2 measures differences between the treatment and the control group in the probability of being enrolled at the follow up survey (new enrollment). It measures the LATE of new enrollment.

Like under the RD case, as a source of exogenous variation, one can use as instrument the initial assignment into the study groups; scoring below the cutoff point (treatment group B), and above the cutoff point (control group A). In this case, T_i can be instrumented by Z_i. Focusing on equation (5), the reduced form is:

[9] There are many studies that use a difference in difference strategy. Some examples of studies using a difference in difference design to evaluate CCT programs are: Schultz (2004), Maluccio and Flores (2004), Angrist et, al. (2002), Rouse (1998), Kling et al. (2005), Schady and Araujo (2006).

$$Y_{it} - Y_{it-1} = X_{it-1}\beta_1 + \beta_2 Z_i + e_{it} \mid (Y_{it-1} = 1) \qquad (7)$$

In this case one gests the ITT on school drop-out. The same applies for new enrollment. In those cases the first stage equation is the same as equation (3). The estimate γ_3 of equation (3) is 0.647, with a robust standard error of 0.027, so the second stage estimates of program impact can be expected to be roughly one and a half times (1/0.647) larger than the corresponding intent to treat estimates from the reduced form equations.

Finally, to check the robustness of estimates the impact of the program on new enrollment and school dropout was also estimated using non-parametric techniques. In this case one can use a matched double difference (Galasso and Ravallion, 2004). Always it is important to remember that one is working with an RD sample. Focusing on new enrollment, as an example, one has the following equation:

$$DD = (1/n)\sum_{i=1}^{n}[(Y_{it}^{1} - Y_{it-1}^{1}) - (Y_{it}^{0} - Y_{it-1}^{0})] \qquad (8)$$

where n is the number of cases, Y^1 yields for those receiving the treatment and Y^0 for those not-receiving the treatment. For each individual who receives the treatment the nearest neighbor (in terms of the propensity score) is matched. So, a one to one matching with replacement is used, using only the region of common support (Heckman, Ichimura and Todd, 1998). In this case, because of the sample design, one also gets a LATE.

Finally, it is important to mention that unlike other CCT programs of Latin America, the BDH has not established mechanisms to control the compliance by households. As was said, data in the follow up survey included some questions about conditionality. In this case one can see whether the un-enforced conditions may have affected household schooling decisions. According to this, one can differentiate the program impact among those who believe the program has conditions versus among those who believe the program has no conditions. A comparison between those groups seems important to evaluate possible effect of enforcing the conditionality of the program in the future.

6 RESULTS

6.1 Regression discontinuity design

6.1.1 Parametric Results

Table 2 presents results for estimates of equation (1). In this case several specifications are introduced. First, the second column uses just the Selben index as a control variable. The third column includes controls for age and sex of the child. The fourth column includes as additional controls baseline measures of log per capita expenditures, school grade dummies (dummy variable that takes the value of 1 if the child was enrolled in a given grade at baseline), father's and mother's education (in years), dummy variables for whether father and mother of the child live at home, dummy variables for whether the head of household was male, illiterate, or indigenous, a measure of household size, a set of controls of household composition, and a rural dummy. Finally, the last column includes, in addition to extended controls, 27 canton-level fixed effects. On the other hand, different functional forms of the control function (Selben index) are introduced across rows. The third row introduces a linear form, the sixth line a quadratic form, and the ninth a cubic form. In all cases, no significant effects are found. Results remain the same if the sample is reduced to those cases scoring from 49.6 to 51.6 in the Selben index. See appendix A.

As was said, those estimates represent LATE of the program. Table 3 presents the reduced form estimates (equation 2), which are the intent to treat (ITT), and table 4 introduces 2SLS instrumentig T by Z. The same specifications are used, and no significant impact of the BDH on enrollment is found. Appendixes B and C present the same results for the reduced sample. Again, no significant impact of the program is found.

6.1.2 Non-parametric results

As was said in the methodological part, local linear regression, a la Fan (Fan 1992) was used to estimate equation (4), using different bandwidths[10]. In all cases, the

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[10] The optimal bandwidth, in the different RD samples, is around 0.3.

estimates of program impact are not different from zero. See table 5. The previous confirm the results obtained under the parametric estimation.

6.2 Difference in difference and matching results

Table 6 present estimates of equations (5) and (6), as well as results for matching estimates of equation (8). The second column presents the number of cases. The third column introduces basic controls (age, and sex of children). The fourth column includes extended controls (as defined above), and the last column includes extended controls and cantonal fixed effects. In both cases, in new enrollment as well as in school drop out, no significant effects are found. Tables (7) and (8) present the same results for reduced form and 2SLS estimates respectively, and the conclusion is the same: no significant effects. In sum, results show no significant effects of the program on new enrollment or in school drop out.

6.3 Conditions and cash transfers

As was said, data in the follow up survey included some questions about conditionality. In this case one can see whether the un-enforced conditions may have affected household schooling decisions. To do this, the survey was divided into two groups. The first group includes those that think that recipients of the BDH have to comply with some conditions regarding to children schooling to receive the transfer ("informed"). The other group formed by people that think that no condition is required to receive the transfer ("uninformed").

Tables 9 and 10 show estimates of equation 5 and 6, as well as matching estimation of equation (8) for informed and uninformed respectively. In both cases no significant program effects are found on school drop out and new enrollment. Tables form 11 to 14 present reduced form, and 2SLS estimates for informed and uninformed separately. In both cases, no significant effects are found.

Tables 15 to 17 present the results for reduced form and non-both parametric estimates of equations (2) and (4) respectively for informed and uninformed. Results show no significant program effects.

6.4 Discussion

As was said, the paper by Schady and Araujo (2005) found a significant effect of the BDH on school enrollment. One important difference that could explain the divergence in the results is that, while Schady and Araujo use families around the threshold of the first quintile and second quintile to randomly assign beneficiaries and non-beneficiaries, we use families around the threshold of the second and third quintile. In this sense, their sample consists of extremely poor people, while our sample consists of less poor people. In addition, as was said, Schady and Araujo found heterogeneous impact effects. When they divide the sample according to the consumption level, they find that enrollment effects are concentrated among the extremely poor. They find no effect on the less poor households of their sample.

As the sample used in our research refers to families around the cutoff point of the Selben index (second quintile, or the less poor of the poor), results seems consistent with those found by Schady and Araujo. Interestingly, however, they find significant impacts even among the less poor when the sample is split among those who believe in the existence of conditions to receive the transfer. In our case, we do not find any significant effect among those who believe that conditions are required (again, among persons around the second quintile threshold). The conclusion in this case is that among the families from quintile 1 (the extremely poor), the application of the conditionality could reinforce the impact of the program and extend them to the less poor. However, among the families from quintile 2 (near to the cutoff point), the program would have no impacts on enrollment, even if the conditionality will be enforced.

The household survey of 2003 allows one to reconstruct the Selben index, and has information on who receives and who do not the BDH. In addition, the survey has a very large number of observations. With these antecedents, this research carried out a RD analysis to evaluate the impact of the BDH on school enrollment. The logic used in the analysis was as similar as possible to the used above (to estimate equations 1 and 2). Results for the reduced form estimates can be seen in annex D. The conclusion is the same. No significant effect of the HDB is found among those very close to the cutoff.

7 CONCLUSIONS

Conditional cash transfer programs play an important role in promoting children's access to school in some Latin American countries. Empirical evidence based on impact evaluations studies show important effects of those programs on school enrollment.

Ecuador has made important improvements in its education results in the last few decades. However, during the 1990s the net enrollment rates, for both primary and secondary school levels, remained stagnated. One key policy directed to improve school access is a CCT program called *Bono de Desarrolo Humano.* This paper evaluates the impact of this program on school enrollment. Results show no significant impacts among the less poor of the poor. However, results from other research show important impacts, on school enrollment as well as on child work, among the extremely poor.

As a consequence, the *Bono de Desarrollo Humano* of Ecuador can be an important policy instrument to improve access to school among the extremely poor. No empirical evidence is found of any impact among the less poor.

Enforcing conditionality can improve CCT programs efficiency. This result is significant among the extremely poor. In our case, apparently the enforcement of conditionality has not any significant effect on school enrollment among the less poor.

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Table 1
Descriptive statistics at base line

Descriptive statistics at baseline Complete sample	Group A	Group B	Difference	Untreated	Treated	Difference
Fraction of children enrolled	0.8489	0.8411	0.0079	0.8041	0.8069	-0.0028
	(0.0107)	(0.0094)	(0.0143)	(0.0099)	(0.0106)	(0.0146)
Child age	12.7274	12.6534	.07404	12.8186	12.5438	0.2748*
	(0.0906)	(0.074)	(0.1165)	(0.0810)	(0.0847)	(0.1174)
Child is female	0.5171	0.5292	-0.0121	0.5242	0.5278	-0.0037
	(0.0138)	(0.0118)	(0.0182)	(0.0125)	(0.0134)	(0.0184)
Log of percapita expenditure	3.0743	2.9212	0.1531*	3.0638	2.8912	0.1726*
	(0.0157)	(0.0127)	(0.0199)	(0.0144)	(0.0141)	(0.0202)
Score in Selben index	51.8735	49.4262	2.4473*	51.2403	49.6138	1.627*
	(0.0202)	(0.0185)	(0.0276)	(0.0319)	(0.0273)	(0.0427)
Father's education	6.5923	6.0162	0.5761*	6.4393	5.9460	0.4933*
	(0.0927)	(0.0716)	(0.1153)	(0.0809)	(0.0790)	(0.1140)
Mother's education	5.9686	5.3649	0.6037*	5.7902	5.3054	0.4848*
	(0.0993)	(0.0786)	(0.1251)	(0.0886)	(0.0869)	(0.1248)
Head of household is male	0.8535	0.8500	0.0035	0.8506	0.8576	-0.0070
	(0.0098)	(0.0085)	(0.0129)	(0.0089)	(0.0094)	(0.0130)
Head of household is indegenous	0.0585	0.0949	-0.0365*	0.0734	0.0875	-0.0140
	(0.0065)	(0.0069)	(0.0098)	(0.0065)	(0.0076)	(0.0099)
Head of household can read and write	0.9415	0.9365	0.005	0.9353	0.9465	-0.0112
	(0.0065)	(0.0058)	(0.0087)	(0.0062)	(0.0060)	(0.0087)
Household size	5.5634	5.6174	-0.05401	5.4708	5.7881	-0.3173*
	(0.0516)	(0.0383)	(0.0629)	(0.0425)	(0.0478)	(0.0638)
Number of household members age 0-5	0.3781	0.3056	0.0725*	0.3296	0.3471	-0.0175
	(0.0178)	(0.0134)	(0.0219)	(0.0150)	(0.0164)	(0.0222)
Number of household members age 6-17	2.4966	2.7770	-0.2804*	2.4909	2.8626	-0.3717*
	(0.0294)	(0.0280)	(0.0412)	(0.0268)	(0.0322)	(0.0416)
Number of household members age18-44	1.7775	1.7708	0.0067	1.7696	1.8019	-0.0323
	(0.0319)	(0.0247)	(0.0397)	(0.0284)	(0.0282)	(0.0402)
Number of household members age45-64	0.7692	0.6657	0.1034*	0.7439	0.6797	0.0641**
	(0.0221)	(0.0192)	(0.0293)	(0.0200)	(0.0222)	(0.0299)
Number of household members older than 65	0.1420	0.0983	0.0437*	0.1368	0.0969	0.0399*
	(0.0108)	(0.0085)	(0.0136)	(0.0101)	(0.0092)	(0.0138)

Reduced sample (form 49.6 to 51.6)	Group A	Group B	Difference	Untreated	Treated	Difference
Fraction of children enrolled	0.8253	0.8606	-0.0353*	0.8263	0.8681	-0.0418**
	(0.0182)	(0.0131)	(0.0219)	(0.0159)	(0.01430)	(0.0214)
Child age	13.0340	12.5557	0.4783*	13.0135	12.4707	0.5428*
	(0.1395)	(0.1125)	(0.1793)	(0.1212)	(0.1320)	(0.179)
Child is female	0.5369	0.5116	0.0252341	0.5435	0.5033	0.0402862
	(0.0217)	(0.0175)	(0.0279)	(0.0193)	(0.0202)	(0.0279)
Log of percapita expenditure	3.0204	2.9227	0.0977*	3.0165	2.8940	0.1225*
	(0.026)	(0.0189)	(0.0316)	(0.023)	(0.0211)	(0.0318)
Score in Selben index	51.1056	50.1568	0.9487*	50.8308	50.2143	0.6165*
	(0.0115)	(0.0102)	(0.0157)	(0.0192)	(0.0145)	(0.0244)
Father's education	6.4277	6.3078	0.119892	6.4140	6.1758	0.2381173
	(0.1372)	(0.1135)	(0.1794)	(0.1218)	(0.1229)	(0.1733)
Mother's education	5.6389	5.7139	-0.0750383	5.7039	5.5250	0.178937
	(0.1578)	(0.1172)	(0.1935)	(0.1346)	(0.1314)	(0.1884)

Head of household is male	0.8185	0.8446	-0.0260277	0.8183	0.8567	-0.0384**
	(0.0168)	(0.0127)	(0.0208)	(0.0149)	(0.0142)	(0.0207)
Head of household is indegenous	0.0454	0.0808	-0.0354*	0.0601	0.0700	-0.0099
	(0.0090)	(0.0095)	(0.0139)	(0.0092)	(0.0103)	(0.0138)
Head of household can read and write	0.9263	0.9364	-0.0100765	0.9174	0.9560	-0.0386086
	(0.0113)	(0.0085)	(0.0140)	(0.0107)	(0.0083)	(0.0137)
Household size	5.5312	5.6891	-0.1579**	5.4444	5.8827	-0.4383*
	(0.0770)	(0.0577)	(0.0948)	(0.0615)	(0.0728)	(0.0948)
Number of household members age 0-5	0.3648	0.3635	0.0013	0.3198	0.4169	-0.0971*
	(0.0292)	(0.0217)	(0.0358)	(0.0241)	(0.0268)	(0.0359)
Number of household members age 6-17	2.4745	2.7613	-0.2868*	2.4775	2.8388	-0.3613*
	(0.0463)	(0.0399)	(0.0620)	(0.0409)	(0.0467)	(0.062)
Number of household members age18-44	1.7826	1.8556	-0.0729605	1.7477	1.9414	-0.1936*
	(0.0513)	(0.0348)	(0.0598)	(0.0434)	(0.0406)	(0.0597)
Number of household members age45-64	0.8015	0.6083	0.1932*	0.7658	0.6107	0.1550*
	(0.0344)	(0.0272)	(0.0437)	(0.0303)	(0.0322)	(0.0442)
Number of household members older than 65	0.1078	0.1004	0.0074	0.1336	0.0749	0.0587*
	(0.016)	(0.0120)	(0.0199)	(0.0161)	(0.0116)	(0.0201)

Standard errors in parenthesis

Table 2

OLS estimates of equation (1)

Dep var: enrollment at follow-up	**No controls**	**Basic Controls**	**Extended controls**	**Extended Controls (cfe)**
Linear				
T	0.0183024	0.0008313	0.0046002	0.0018849
	(0.0207)	(0.0200)	(0.0178)	(0.0179)
Quadratic				
T	0.0189244	0.0011972	0.0046321	0.0015713
	(0.0209)	(0.0202)	(0.0178)	(0.0180)
Cubic				
T	0.0164276	-0.0014129	0.0008566	-0.0015595
	(0.0216)	(0.0208)	(0.0184)	(0.0185)
No_cases	2384	2384	2384	2384

Standard errors are in parenthesis and corrected by heteroskedasticity and within-sibling correlations.
* Significant at 1 percent level, ** significant at 5 percent level, and *** significant at 10 percent level.

Table 3

Reduced form estimates of equation 2
Complete sample

Dep var: enrollment at follow-up	**No controls**	**Basic Controls**	**Extended controls**	**Extended Controls (cfe)**
Linear				
Z	0.0421708	0.0244554	0.0254691	0.0183653
	(0.0303812)	(0.0295383)	(0.0265609)	(0.0270105)
Quadratic				
Z	0.0455965	0.0266109	0.0261497	0.0176383
	(0.0311898)	(0.0303132)	(0.0270748)	(0.0275839)
Cubic				
Z	0.0522325	0.0283971	0.0203181	0.0111613
	(0.0396327)	(0.0385309)	(0.0342263)	(0.0348596)
No_cases	2384	2384	2384	2384

Standard errors are in parenthesis and corrected by heteroskedasticity and within-sibling correlations.
* Significant at 1 percent level, ** significant at 5 percent level, and *** significant at 10 percent level.

Table 4

2SLS estimates equation (1 and 2).
Instrumenting T by Z.

Dep var: enrollment at follow-up	**No controls**	**Basic Controls**	**Extended controls**	**Extended Controls (cfe)**
Linear				
T=Z	0.0651834	0.0380595	0.0399884	0.0288979
	(0.0469)	(0.0459)	(0.0418)	(0.0425)
Quadratic				
T=Z	0.0702225	0.0412824	0.0410301	0.0277112
	(0.0480)	(0.0470)	(0.0425)	(0.0433)
Cubic				
T=Z	0.081619	0.0455604	0.0330453	0.0172236
	(0.0619)	(0.0657)	(0.0519)	(0.0548)
No_cases	2384	2384	2384	2384

Standard errors are in parenthesis and corrected by heteroskedasticity and within-sibling correlations.
* Significant at 1 percent level, ** significant at 5 percent level, and *** significant at 10 percent level.

Table 5

Nom-parametric estimates of equation (2)

	Bandwidth			
RD sample	**Optimal**	**0.2**	**0.3**	**0.4**
From 49.6 a 51.6	0.0598	0.0490	0.0543	0.0485
	(0.0379)	(0.0294)	(0.0292)	(0.0279)
From 48.6 a 52.6	-0.0043	-0.0023	0.0003	-0.0041
	(0.0275)	(0.0238)	(0.0243)	(0.0224)
From 47.6 to 53.6	-0.0171	-0.0150	-0.0161	-0.0226
	(0.0268)	(0.0242)	(0.0225)	(0.0242)

Asymptotic standard errors in parenthesis.

Table 6

OLS and matching estimates of eqs 5 and 6

Dependent Variable	Number of cases	Basic Controls	Extended controls	Extended controls and canton fixed effects	Matching
New enrollment	352	0.0782667	0.1166863	0.1154*	0.17819096
		(0.0516)	(0.0556)	(0.0559)	(0.20566)
Drop_out	2032	0.0169679	0.0118729	0.014222	0.02434194
		(0.0127)	(0.0127)	(0.0128)	(0.02260)

Standard errors are in parenthesis and corrected by heteroskedasticity and within-sibling correlations.
* Significant at 1 percent level, ** significant at 5 percent level, and *** significant at 10 percent level.

Table 7

Reduced form estimates of eqs 5 and 6

Dependent Variable	Number of cases	Basic Controls	Extended controls	Extended controls and canton fixed effects
New enrollment	352	0.0639653	0.0581947	0.0665829
		(0.0538)	(0.0562)	(0.0573)
Drop_out	2032	0.0137226	0.006195	0.0098172
		(0.0129)	(0.0132)	(0.0133)

Standard errors are in parenthesis and corrected by heteroskedasticity and within-sibling correlations.
* Significant at 1 percent level, ** significant at 5 percent level, and *** significant at 10 percent level.

Table 8

2SLS estimates of eqs 5 and 6

Dependent Variable	Number of cases	Basic Controls	Extended controls	Extended controls and canton fixed effects
New enrollment	352	0.1007031	0.0959788	0.1042992
		(0.0842)	(0.0920)	(0.0895)
Drop_out	2032	0.0195038	0.0089402	0.0143085
		(0.0183)	(0.0190)	(0.0193)

Standard errors are in parenthesis and corrected by heteroskedasticity and within-sibling correlations.
* Significant at 1 percent level, ** significant at 5 percent level, and *** significant at 10 percent level.

Table 9

OLS and matching estimates of eqs 5 and 6

Compliers

Dependent Variable	Number of cases	Basic Controls	Extended controls	Extended controls and canton fixed effects	Matching
New enrollment	78	0.0228854	0.105365	0.0482206	0.4
		(0.1067)	(0.1518)	(0.2140)	(0.24495)
Drop_out	536	-0.0215537	-0.0166594	-0.0249421	0.03843466
		(0.0319)	(0.0301)	(0.0347)	(0.06614)

Standard errors are in parenthesis and corrected by heteroskedasticity and within-sibling correlations.
* Significant at 1 percent level, ** significant at 5 percent level, and *** significant at 10 percent level.

Table 10

OLS and matching estimates of eqs 5 and 6

No-compliers

Dependent Variable	Number of cases	Basic Controls	Extended controls	Extended controls and canton fixed effects	Matching
New enrollment	261	0.0737823	0.1031847	0.1101647	0.0550482
		(0.0626)	(0.0683)	(0.0691)	(0.20178)
Drop_out	1464	0.0333**	0.0268***	0.0253853	-0.00121133
		(0.0154)	(0.0151)	(0.0156)	(0.02972)

Standard errors are in parenthesis and corrected by heteroskedasticity and within-sibling correlations.
* Significant at 1 percent level, ** significant at 5 percent level, and *** significant at 10 percent level.

Table 11

Reduced form estimates of eq. 7

Compliers

Dependent Variable	Number of cases	Basic Controls	Extended controls	Extended controls and canton fixed effects
New enrollment	78	-0.0226799	0.1162274	-0.0268728
		(0.1227)	(0.1817)	(0.2862)
Drop_out	536	-0.016191	-0.0051925	-0.016599
		(0.0324)	(0.0326)	(0.659)

Standard errors are in parenthesis and corrected by heteroskedasticity and within-sibling correlations.
* Significant at 1 percent level, ** significant at 5 percent level, and *** significant at 10 percent level.

Table 12

Reduced form estimates of eq. 7

No-compliers

Dependent Variable	Number of cases	Basic Controls	Extended controls	Extended controls and canton fixed effects
New enrollment	261	0.0626106	0.035644	0.0289291
		(0.0657)	(0.0682)	(0.0722)
Drop_out	1464	0.0255***	0.0174023	0.0185673
		(0.0148)	(0.0154)	(0.0155)

Standard errors are in parenthesis and corrected by heteroskedasticity and within-sibling correlations.
* Significant at 1 percent level, ** significant at 5 percent level, and *** significant at 10 percent level.

Table 13

2SLS estimates of eqs 5 and 6

Compliers

Dependent Variable	Number of cases	Basic Controls	Extended controls	Extended controls and canton fixed effects
New enrollment	78	-0.0345119	0.1945861	-0.0397611
		(0.1882)	(0.3009)	(0.4233)
Drop_out	536	-0.0201491	-0.006478	-0.0213725
		(0.0403)	(0.0407)	(0.0483)

Standard errors are in parenthesis and corrected by heteroskedasticity and within-sibling correlations.
* Significant at 1 percent level, ** significant at 5 percent level, and *** significant at 10 percent level.

Table 14

2SLS estimates of eqs 5 and 6

No-compliers

Dependent Variable	Number of cases	Basic Controls	Extended controls	Extended controls and canton fixed effects
New enrollment	261	0.1009748	0.0613452	0.0474175
		(0.1058)	(0.1168)	(0.1179)
Drop_out	1464	0.0396***	0.0272698	0.02988
		(0.0229)	(0.0242)	(0.0250)

Standard errors are in parenthesis and corrected by heteroskedasticity and within-sibling correlations.
* Significant at 1 percent level, ** significant at 5 percent level, and *** significant at 10 percent level.

Table 15

Reduced form estimates of eq 3

Informed

Dep var: enrollment at follow-up	No controls	Basic Controls	Extended controls	Extended Controls (cfe)
Linear				
Z	0.0893	0.0790	0.0810	0.1160***
	(0.0596)	(0.0607)	(0.0594)	(0.0639)
Quadratic	0.0667	0.0649	0.0598	0.1116
Z	(0.0701)	(0.0709)	(0.0683)	(0.0747)
Cubic	0.0909	0.0780	0.0628	0.1220
Z	(0.0801)	(0.0807)	(0.0750)	(0.0806)
No_cases	614	614	614	614

Standard errors are in parenthesis and corrected by heteroskedasticity and within-sibling correlations.
* Significant at 1 percent level, ** significant at 5 percent level, and *** significant at 10 percent level.

Table 16

Reduced form estimates of eq 3

Uniformed

Dep var: enrollment at follow-up	No controls	Basic Controls	Extended controls	Extended Controls (cfe)
Linear				
Z	0.0235	0.0080	0.0065	-0.0033
	(0.0358)	(0.0349)	(0.0312)	(0.0316)
Quadratic	0.0271	0.0107	0.0080	-0.0024
Z	(0.0359)	(0.0350)	(0.0314)	(0.0318)
Cubic	0.0301	0.0108	0.0019	-0.0086
Z	(0.0466)	(0.0453)	(0.0402)	(0.0407)
No_cases	1725	1725	1725	1725

Standard errors are in parenthesis and corrected by heteroskedasticity and within-sibling correlations.
* Significant at 1 percent level, ** significant at 5 percent level, and *** significant at 10 percent level.

Table 17

Nom-parametric estimates of eq 4 Informed and uninformed				
	Several bandwidths (Informed)			
RD sample	Optimal	0.2	0.3	0.4
From 49.6 a 51.6	0.0882	0.0989	0.0859	0.0885
	(0.0677)	(0.0748)	(0.0704)	(0.0633)
From 48.6 a 52.6	0.0474	0.0600	0.0458	0.0444
	(0.0531)	(0.0555)	(0.0578)	(0.0501)
From 47.6 to 53.6	0.0551	0.1392	0.0602	0.0631
	(0.0615)	(0.2948)	(0.0727)	(0.1718)
	Several bandwidths (Uninformed)			
RD sample	Optimal	0.2	0.3	0.4
From 49.6 a 51.6	0.0385	0.0386	0.0341	0.0386
	(0.0338)	(0.0384)	(0.0329)	(0.0326)
From 48.6 a 52.6	-0.0159	-0.0100	-0.0155	-0.0179
	(0.0252)	(0.0281)	(0.0234)	(0.0248)
From 47.6 to 53.6	-0.0565	-0.0490	0.0133	-0.0572
	(0.0233)	(0.0267)	(0.0308)	(0.0235)

Asymptotic standard errors in parenthesis.

Appendix A

OLS estimates of eq. 1

Reduced sample

Dep var: enrollment at follow-up	No controls	Basic Controls	Extended controls	Extended Controls (cfe)
Linear				
T	0.0309469	0.0124155	0.0054147	0.0072657
	(0.0294)	(0.0279)	(0.0253)	(0.0267)
Quadratic				
T	0.0261143	0.0084663	0.0032752	0.0050689
	(0.0302)	(0.0286)	(0.0258)	(0.0273)
Cubic				
T	0.0259473	0.0095588	0.0032986	0.0050982
	(0.031)	(0.0294)	(0.0258)	(0.0273)
No_cases	1030	1030	1030	1030

Standard errors are in parenthesis and corrected by heteroskedasticity and within-sibling correlations.
* Significant at 1 percent level, ** significant at 5 percent level, and *** significant at 10 percent level.

Appendix B

Reduced form estimates. Eq. 2

Reduced sample

Dep var: enrollment at follow-up	**No controls**	**Basic Controls**	**Extended controls**	**Extended Controls (cfe)**
Linear				
Z	0.0097165	-0.0248022	-0.0498586	-0.0390426
	(0.0477)	(0.0466)	(0.0426)	(0.0442)
Quadratic				
Z	-0.0088975	-0.0430234	-0.0626907	-0.0528016
	(0.0509)	(0.0496)	(0.0454)	(0.0472)
Cubic				
Z	-0.0086076	-0.0427308	-0.0624608	-0.0525552
	(0.0508)	(0.0496)	(0.0453)	(0.0471)
No_cases	1030	1030	1030	1030

Standard errors are in parenthesis and corrected by heteroskedasticity and within-sibling correlations.
* Significant at 1 percent level, ** significant at 5 percent level, and *** significant at 10 percent level.

Appendix C

2SLS estimates of eq. 1

Reduced sample

Dep var: enrollment at follow-up	**No controls**	**Basic Controls**	**Extended controls**	**Extended Controls (cfe)**
Linear				
T=T_hat	0.0156022	-0.0401052	-0.0793944	-0.0626966
	(0.0764)	(0.0760)	(0.0690)	(0.0719)
Quadratic				
T=T_hat	-0.0144619	-0.0700191	-0.1003387	-0.0843748
	(0.0830)	(0.0822)	(0.0738)	(0.0762)
Cubic				
T=T_hat	-0.0139876	-0.0695328	-0.0999537	-0.0839795
	(0.0829)	(0.0821)	(0.0737)	(0.0761)
No_cases	1030	1030	1030	1030

Standard errors are in parenthesis and corrected by heteroskedasticity and within-sibling correlations.
* Significant at 1 percent level, ** significant at 5 percent level, and *** significant at 10 percent level.

Appendix D

IV_ Reduced form. Household survey, 2003.

RD sample from 49.6 to 51.6

Dep var: enrollment at follow-up **Linear**	**No controls**	**Basic Controls**	**Extended controls**	**Extended Controls (cfe)**
Z	0.0821***	0.0539	0.0503	0.0595
	(0.0462)	(0.0416)	(0.0400)	(0.0442)
Quadratic				
Z	0.080***	0.0552	0.0513	0.0600
	(0.0464)	(0.0419)	(0.0403)	(0.0440)
Cubic				
Z	0.0801***	0.0552	0.0513	0.0600
	(0.0464)	(0.0419)	(0.0403)	(0.0440)
No_cases	**1338**	**1338**	**1338**	**1338**

RD sample from 48.6 to 52.6

Dep var: enrollment at follow-up **Linear**	**No controls**	**Basic Controls**	**Extended controls**	**Extended Controls (cfe)**
Z	0.0032	-0.0145	-0.0071	-0.0027
	(0.0326)	(0.0297)	(0.0289)	(0.0286)
Quadratic				
Z	0.0084	-0.0099	-0.0027	0.0014
	(0.0326)	(0.0297)	(0.0289)	(0.0287)
Cubic				
Z	0.0379	0.0182	0.0263	0.0384
	(0.0438)	(0.0397)	(0.0385)	(0.0395)
No_cases	**2714**	**2714**	**2714**	**2714**

RD sample from 47.6 to 53.6

Dep var: enrollment at follow-up **Linear**	**No controls**	**Basic Controls**	**Extended controls**	**Extended Controls (cfe)**
Z	-0.0211	-0.0258	-0.0239	-0.0208
	(0.0265)	(0.0242)	(0.0236)	(0.0238)
Quadratic				
Z	-0.0175	-0.0231	-0.0213	-0.0171
	(0.0264)	(0.024)	(0.0236)	(0.0238)
Cubic				
Z	0.0067915	-0.0072392	-0.0021943	0.0053528
	(0.0352)	(0.0320)	(0.0312)	(0.0313)
No_cases	4092	4092	4092	4092