The Effects of User Fee Reductions on Enrollment

Evidence from a quasi-experiment

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Abstract. This paper evaluates the impact of a fee reduction program launched by the city of Bogota in 2004. The program is targeted using a proxy-mean index (the SISBEN index), such that the probability that households benefit from the fee reduction is a discontinuous function of their SISBEN score. This allows us to implement a regression discontinuity design which, as we discuss below, can yield rigorous and credible estimates of this program's effect. To our knowledge, Bogota is the first setting in which such a design has been applied to evaluate a fee reduction program. Our results suggest that the program had a significant impact. We find that the fee reductions offered to individuals of Sisben levels 1 and 2 have a positive effect on enrollment in primary grades for students in Sisben 1, and in high school grades for Sisben 2. Specifically, the estimates suggest that Gratuidad raises the probability of enrollment for primary-aged Sisben 1 students by about 3 percent, and for high school-aged Sisben 2 students by about 6 percent. Importantly, these positive effects seem to be larger for at-risk students, and to not vary by gender. While there are groups for which we find no effect (e.g. students of age to attend high school grades in Sisben 1) the overall pattern of results suggests the program had a significant impact.

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1. Introduction

There is concern that children in developing countries do not invest sufficiently in human capital, and that public policy should therefore aim to remove barriers to such investment. One potential barrier that has received attention is that of school fees—payments that public schools in many countries require from even low income children.[1]

Proponents of school fee reduction programs have argued that these charges can deter enrollment, particularly for potentially credit constrained low income households. Their proposals typically mandate that schools reduce direct charges, and that they be compensated through fixed per-student public subsidies. In support of such initiatives, they point to increases in school enrollment in countries that have eliminated school fees (Oxfam, 2001, 2002; Al-Samarrai and Zaman, 2000; MacJessie, 2002).[2]

In contrast, other authors make the case that both efficiency and equity may be advanced, and enrollment may even increase, if otherwise under-funded schools can charge prices tailored to individuals' willingness and ability to pay (Bird, undated; Thobani, 1984; Hillman and Jenknerm, 2002; Jimenez, 1990).

The empirical literature has not made clear headway on estimating the causal effects of fee reductions. There is extensive work on the consequences of conditional cash transfers, notably on *Progresa* in Mexico, which provided a randomized and therefore very credible design.[3] In our reading of the literature, however, there is little well-identified evidence on the effects of fee reductions on enrollment. For instance, most previous work compares enrollment before and after fee reductions without controlling for other factors.

To address this gap, we consider the *Gratuidad* fee reduction initiative, introduced in 2004 by the municipal government of Bogotá, Colombia. The program is targeted

[1] There appears to be no consensus definition of user fees. Reddy and Vandemoortele (1996) define these as "contributions to cost by individual users in the form of a charge per unit of service consumed, typically in the form of cash." They present evidence on the magnitude of user fees in different countries. See also Bentaouet-Kattam and Burnett (2002) and Tomasevski (2003).

[2] In the cases of Malawi and Kenya, such programs induced important increases in enrollment that proved difficult to sustain over time, in part because massive student entry seems to have strained school systems and reduced educational quality

[3] See for instance Schultz, T.P. (2004). For a recent review, see Parker, Rubalcava and Teruel (2005).

using the *Sisben* index, a scheme which attempts to identify the most vulnerable households in Colombia. The scheme uses data from a proxy-means survey to assign households a single *Sisben* score that is then used to classify them into six categories. For instance, households with scores below a cutoff score of 11 are given an index of 1, those between 11 and a cutoff of 22 receive a 2. The *Gratuidad* program provides varying levels of fee reductions to children from households in the poorest (lowest) two *Sisben* categories, such that the probability that households benefit from the fee reduction is a discontinuous function of their *Sisben* score.

These discrete changes provide an excellent opportunity to estimate the causal effects of the difference in school fees with a regression discontinuity (RD) design. First, the scoring system used to classify households is very fine, greatly raising the likelihood that households that either just make or just miss the cut-off for each Sisben category are similar—the key assumption in an RD approach. Second, the scoring formula is kept secret, making it very difficult for families near the cut-offs to manipulate their eventual scores. Finally, the *Sisben* survey's substantial sample size allows us to estimate the differences at the discontinuities with great precision.

Using this approach, our results suggest that the program did have a significant impact. We find that the fee reductions offered to individuals of *Sisben* levels 1 and 2 have a positive effect on enrollment in primary grades for students in *Sisben* 1, and in high school grades for *Sisben* 2. Specifically, the estimates suggest that *Gratuidad* raises the probability of enrollment for primary-aged *Sisben* 1 students by about 3 percent, and for high school-aged *Sisben* 2 students by about 6 percent. Importantly, these positive effects seem to be larger for at-risk students, and to not vary by gender. While there are groups for which we find no effect (e.g. students of age to attend high school grades in *Sisben* 1) the overall pattern of results suggests the program had a significant impact.

Additionally, we find evidence that enrollment responses are sensitive to the size of the subsidy, with results related to a strand of the literature estimates demand elasticities of changes in user fees (Gertler and Glewwe, 1990; Birdall and Orivel, 1996; Mingar and Tan, 1986; Jimenez, 1990; and Reddy and Vandemoortele, 1996).[4] From a policy

[4] These articles typically find elasticities greater than negative one, such that the percentage change in the quantity is less than the percentage change in the price. Gertler and Glewwe (1990) find elasticities that

standpoint, these results should make feasible further work that may productively inform modifications to the program's parameters.

The remainder of the paper is organized as follows. Section 2 describes the characteristics of the program and section 3 describes the estimation strategy. Section 4 describes the data and Section 5 presents the results. Section 6 concludes.

2. The program

a. The *Sisben* Scheme

The *Sisben* is an instrument used to focalize social assistance. The system was first implemented in 1994 with a survey of 62 questions about households' infrastructure, demographics and human capital characteristics. In its initial conception, the *Sisben* was used to target public health insurance. By 2000, the its classification covered close to 27 million individuals

Between 2003 and 2005, the *Sisben* scheme was updated for three main reasons: first, some of the variables changed dramatically since 1994; second, there was evidence of signficant errors of inclusion and exclusion (see Castañeda, 2005); third, there was a perception that in some areas, particularly small rural town, the instrument was being manipulated by local authorities.

The new *Sisben* includes 74 questions, and reaches about 32 million individuals, with survey questions divided into six chapters

a. Location and identification: rural/urban area; identification of the household and of each member
b. Infrastructure of the house, including floor, wall, and ceiling materials.
c. Services of the house: collection of solid waste, phone connection, cooking fuel, among others.

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range between -0.05 and -0.61 in Peru. The estimates are lower the closer the household is located to the school. Birdall and Orivel (1996) present estimates for Mali that range between -0.016 and -0.98. Mingat and Tan (1986) find a price elasticity of -0.03 for Malawi. Additionally, these papers generally find that elasticities are higher for low income households.

d. Demographics: household structure, civil status, age of all members, number of children below 6 years old, whether the head is single or not.
e. Health and Education: whether each member is covered by health insurance; years of schooling for members 12 years or older; enrollment status for individuals 18 years or younger
f. Income and occupation: working status of each member of the household and monthly income of each member.

The scheme uses data from a proxy-means survey to assign households a single *Sisben* index that runs from 0 to 100, and to then place them into six different levels, 1 being the most poor, and 6 the richest. The *Sisben* index number that households receive is a function of their *Sisben* score, calculated using data from a survey. For instance, households with scores below a cutoff score of 11 are given an index of 1, those between 11 and a cutoff of 22 receive a 2. [5]

According to our dataset, within Bogota the new *Sisben* reached 4.030.628 individuals in 1.110.588 households, 18% of them are 18 or younger, and 24% belong to Sisben level 1; 38% belong to level 2 and 37% to level 3. The rest (less than 1%) are distributed in levels 4 to 6.

b. Gratuidad Program

In Colombia, municipalities are in charge of regulating the fees charged by public schools. As a result, every year the government of the city of Bogotá issues a resolution that stipulates which items schools may charge for, as well as aspects like the maximum fee they can set for each.

Table 1 summarizes these items, which fall under the headings of: academic fees, complementary services, periodic charges, and others. Academic fees are for tuition and board. In general, these are zero for basic education (grades 1 through 9) and positive for high school (grades 10 and 11). Complementary services cover items such as report cards, school handbooks, ID cards, pedagogical materials, maintenance, and field trips.

[5] The remaining categories and bounds are: level 3—22 and 43, level 4—43 and 65, level 5— 65 and 79, and level 6—above 79 points. For further detail on the *Sisben* instrument, see Vélez et al (1999) and Castañeda (2005).

Periodic charges cover transportation and food services.[6] Usually these are zero for public schools, as the municipality itself provides transportation and some school meals. Finally, other charges cover the replacement of items such as ID cards and handbooks, as well as graduation (for grade 11) and certification materials.

Table 2 describes the magnitude of these fees in the city of Bogotá.[7] It reports households' self-reported average total educational expenditure for primary (grades1 to 5), secondary (grades 6 to 9), and high school (grades 10 and 11) education for households classified in *Sisben* 1 to 4.[8]

In general, schooling-related expenses increase with households' *Sisben* level, and with the grades children enroll in. These expenses are equivalent to between 7 and 29 monthly dollars, which in turn represent between 6 and 25 percent of the minimum wage. Thus, particularly for lower income households in Bogotá, the direct costs of schooling are non-trivial, and might potentially deter enrollment. Indeed, using household survey data, Barrera and Dominguez (2006) point out that 37 percent of those school-aged children not enrolled reported this was due to the high costs of schooling.

The *Gratuidad* program addressed this situation by reducing the fees for complementary services, and, for grades 10 and 11, those for academic fees as well. At present, the extent to which students benefit from these reductions is a function of their *Sisben* level. As stated above, the *Sisben* index, which determines students' *Sisben* level seeks to summarize households' welfare based on their demographic characteristics (e.g. number of children), their educational levels (e.g. household heads' schooling attainment), their employment situation (e.g., the ratio of workers to minors), and their housing infrastructure (e.g. building materials and access to utilities). Based on these characteristics, households are classified into six *Sisben* levels. For instance, households with the lowest welfare (and scores between 0 and 11) are assigned to level 1; those with scores between 11 and 22 are assigned to level 2, and so on.

[6] In general "food services" comprises a full lunch, usually contracted and offered by the school to students who want to pay for the option. In contrast, board is usually a light meal provided to schools by the local government.

[7] Table 2 is based on Fedesarrollo (2005) and Barrera and Domínguez (2006)

[8] We do not report costs for *Sisben* 5 or 6 because the survey on which the data are based (*Encuesta de Calidad de Vida*) is targeted towards poor neighborhoods, and so contains few observations for households belonging to these levels.

All children in Grade 0, regardless of their *Sisben* level, benefit from the elimination of complementary service charges. As Table 3 shows, beyond grade 0, only children in levels 1 and 2 are entitled to the fee reduction. For basic education (grades 1-9) *Sisben* 1 children enjoy a 100 percent reduction of complementary service charges, while those in *Sisben* levels 2 and above receive no reduction. For high school (grades 10-11), *Sisben* 1 children benefit from the elimination of both academic and complementary services fees, while *Sisben* 2 households receive roughly a 50 percent reduction; households in levels 3 and higher receive no benefit. The second row in Table 3 provides the number of children eligible for enrollment in each cell according to the *Sisben* dataset.

The first two rows of Table 3 are sufficient to describe the present day *Gratuidad* program. The lower four rows, however, are necessary because initially (in 2005) the program was not allocated using children's *Sisben* but rather their *Estrato* index—a variable which classifies households according to the block on which they reside. This index goes from 1 to 6 (with 1 again containing the poorest households), and is used to allocate cross-subsidies that originate in water and energy services.[9]

The *Estrato* index was used initially because when the resolution that enacted *Gratuidad* was passed in September 2004, the questionnaire for the *new Sisben* index had not been administered to the whole potential target population. By October 2005, its coverage of was high enough, and the system started using the *Sisben* instead.

The change in the targeting instrument involved a transition period. Students that initially benefited from fee reductions due to their *Estrato* classification had a grace period until March 31, 2006 to show that they belonged to *Sisben* 1 or 2; otherwise, they lost their benefits.

Thus, as Table 3 shows, some students who received discounts in 2005 did not in 2006. Conversely, there are individuals who did not receive discounts in 2005 (on account of belonging to *Estrato* 2 or higher), but began doing so in 2006 (on account of their *Sisben* classification). As we discuss below, we take these features into account below, by controlling for the fact that the program temporarily "grandfathered" beneficiaries selected based on their *Estrato*.

[9] For a more thorough description of the *Estrato* system, see Gomez-Lobo and Contreras (2003).

3. Identification

As usual, the challenge in credibly evaluating a program like *Gratuidad* is to identify groups which in the absence of the program would have had similar levels and changes in their enrollment rates. Then, if one receives the program and the other does not, the differences in their outcomes can be attributed to the fee reduction itself.

We use a regression discontinuity (RD) design to try to achieve this goal. This approach acknowledges from the outset that the groups that benefit from *Gratuidad* and those do not might be rather different—indeed, the whole *Sisben* scheme is designed to identify the most vulnerable households. The key is noting that whether or not students benefit from the discount is a discrete function of their score.

For illustration, consider the situation at the high school (grades to 10-11) level. As Table 3 shows, beneficiaries with scores from 0 to 11 (*Sisben* 1) receive full discounts for complementary services and academic fees, whereas those with scores between 11 and 22 (*Sisben* 2) receive close to 50 percent discounts. Individuals with scores above 22 (*Sisben* 3 and higher) receive no discount. Thus, at 11 and 22, the discounts individuals experience are a discontinuous function of their scores.

Suppose that in contrast other characteristics that affect enrollment are continuously related to the score at these points, in other words, that even unmeasured traits that influence enrollment are similar for students just above and below the cutoff scores. Under this assumption, discrete differences in attendance rates between treated and untreated students close to the cutoff can be attributed to the fee reductions. For example, students with scores of 21.5 might provide an adequate control group for students with scores of 22.5—i.e., ideally close to the cutoffs the RD design resembles a (localized) randomized experiment.[10]

Specifically, if y_i is an enrollment outcome, then the idea is to explore whether it is related to receiving the fee reduction (captured by a dummy, G, for *Gratuidad*), while controlling for a smooth function of the score $f(S_i)$, where i indexes individuals:

$$y_i=\alpha+\beta G_i+f(S_i)+\varepsilon_i$$

[10] See Hahn, Todd, and van der Klaauw (2001), van der Klaauw (2002), and Lee (2005).

If *f(Si)* is correctly specified, then β will consistently estimate the effect of the program. Alternately, one can estimate this equation within arbitrarily narrow bands close to the cutoff point.

As we show below, the *Gratuidad* fee reduction program satisfies two conditions that are important for this strategy to make sense. First, the program has well defined cut-off points, i.e., it produces sharp discontinuities; and second, the density of observations around these discontinuities is sufficient to generate reasonably accurate estimates.

Even with these conditions satisfied, the similarity between students on either side of the cutoff is ultimately the key identifying assumption. Below, we verify that it holds for observable characteristics. Finally, note that Table 3 makes clear that there are two relevant cutoff *Sisben* scores (at 11 and 22) for high school, and one (at 11) for basic education (grades 1-9). Because all students receive the discounts for grade 0, no RD-type design is possible in that case.

4. The data

The information we use comes from two sources. First, there is data collected directly through the *Sisben* survey. These are at the individual level and include demographic characteristics such as gender, age, household composition, pregnancy, and marital status. They also cover educational attainment (grades completed), type of enrollment (public/private), labor force participation, and income. The survey identifies individuals by name and national ID number, and includes address and telephone information. These data were collected in 2004 and 2005. Second, we use administrative enrollment records kept by the District Education Department (henceforth, SED)—these are current as of October 2006, and also include individual ID numbers and address and phone information.

The two data sets are merged using a master data set, created by the SED, that includes a matching variable constructed using information on individuals' ID numbers,

their names, addresses, and phone numbers. The two datasets do not merge perfectly for several reasons. First, some individuals in the *Sisben* survey are enrolled in private school, and therefore do not appear in the administrative files, which cover only public schools. Additionally, students who have left Bogotá, or those who made mistakes in reporting their ID numbers or names, will not be matched. In the event, the merge rate is close to 60 percent. Table 4 presents descriptive statistics for both datasets.

Column 1 in Table 4 contains descriptive statistics for enrollment (in the first row), and also for a number of student and household-level characteristics. Standard deviations are in parentheses, and sample sizes in brackets. For later reference, columns 2-7 present similar statistics restricting the sample to given grade and *Sisben* index levels.

5. Results

A. First stage

Figure 1 shows how students' *Sisben* index level is related to their *Sisben* score. The Figure plots individual level data, where the scores, as in the original data, can take on up to two decimals. We only plot data for students in the first three *Sisben* levels—these account for the majority (about 99%) of all observations, and they are the ones we focus on the for the RD estimates below, since students with index levels of 3 or higher receive no discounts.

The Figure suggests that students' *Sisben* score is an excellent predictor of their index. For completeness, Column 1 in Table 5 reports a regression of students' *Sisben* index on a piecewise linear spline of their score, as in van der Klaauw (2002). The first two dummy variables indicate whether students' scores are greater than or equal to 11 and 22, the cutoff scores for index levels 2 and 3, respectively. Their coefficients thus provide direct estimates of the average increase in the index that takes place in the vicinity of those breaks. Consistent with the visual evidence in Figure 1, they suggest that the index jumps by essentially one point at each threshold. These estimates are highly significant, even though standard errors are clustered by score values in view of

the fact that the *Sisben* score is not a fully continuous variable.[11] The R^2 statistics exceed 0.999, showing that in very few cases are students index levels not those which their scores would suggest.[12] Columns 2 and 3 refer to students eligible for grades 1-9 or 10-11, respectively, grade ranges we will focus on below. The key coefficients are again essentially equal to one, and the fit essentially unchanged.

Figure 1 and Table 5 show that at least at a first pass level, the *Gratuidad* program provides a sharp RD design. This should not be surprising to the extent that the same agency calculates the score and assigns the index value. Nonetheless, concerns could remain if households had been able to influence their *Sisben* score either directly by hiding assets when surveyors arrive or, indirectly by lobbying with program administrators. Indeed concerns this was going on were central in motivating the update of the *old Sisben* during 2003 and 2005. The generalized perception is that partly by keeping the scoring formula secret, the *new Sisben* has substantially reduced the scope of such behaviors, particularly in the major cities.

Nevertheless, if such "gaming" behaviors had persisted, they could generate non-random sorting across the cutoff scores, invalidating a seemingly strong RD design. The intuition is that such behaviors would result in that students just to the right and to the left of the cutoff scores would no longer being comparable—for instance, if more motivated households sought to influence their scores, and those less motivated or less well connected did not.[13]

We have two reasons to believe this is not a major concern. First, if households had the ability to influence their score, or if the agency had sought to increase the ranks of those eligible for the fee reductions, one might well expect to see "stacking" of observations at scores of 11 or 22, since households with scores equal to or smaller than these cutoffs get *Sisben* 1 and 2 classifications (respectively), thereby earning the right to

[11] See Lee and Card (2004).
[12] As we show below, excluding these cases has essentially no effect on our key estimates, which is not surprising given how few they are.
[13] For theoretical and practical illustrations of this issue, see Lee (2004), McCrary (2005), and Urquiola and Verhoogen (2006).

benefit from the fee reduction (depending on the grade level). Specifically, we would expect to see significantly more students with scores of 11, say, than of 11.01.[14]

Figure 2 presents a histogram of the number of individuals observed at each *Sisben* score, suggesting there is essentially no evidence of stacking. For instance, while there are 277 children whose households had scores of 11, and who therefore *just* made the *Sisben* 1 category, there are 250 children whose households had scores of 11.01, thereby just missing it. Similarly, while there are 144 children with scores of 22, 224 have scores of 22.01.

Second, if households and/or administrators had been able to manipulate the running variable, one would expect student characteristics to change discretely at the cutoff points, but we find little evidence of this, as detailed in the next section.

B. Continuity checks

The identifying assumption underlying the RD design is that only individuals' treatment status varies discretely at the cutoff scores that determine access to the program—all other characteristics, observable and unobservable, should be smoothly related to the *Sisben* score at these points.

While it is impossible to verify this for all potential student traits, we can consider those observable variables available in the *Sisben* survey. Given the sample size with which we have to work, these differences can be estimated very precisely, but all of them are small–especially when considered in relationship to the correlation between these variables an enrollment provided in the Column 8 of Table 4.

Figure 3 begins to present evidence on this by showing how students' household per capita income relates to their *Sisben* score. It presents fitted values of a locally weighted regression of per capita income on the score. The solid line describes the results for students in basic grades and *Sisben* 1 and 2, and the dashed line presents the results for students in high schools and *Sisben 1 to 3.* As one would expect, per capita income is

[14] Using school data from Chile, Urquiola and Verhoogen (2006) provide an illustration of such "stacking" and the non-random sorting and biases it can result in.

indeed increasing in the score, but visually there is no break in this relationship at the cutoff scores.

Table 6 presents the corresponding statistical evidence for this variable as well as for a host of other individual and household level traits. It first considers students that are either in *Sisben* 1 or 2 and eligible to enroll in grades 1-9. Columns 1 and 2 present the point estimates of a regression of students' household per capita income on a dummy that indicates whether they have scores lower than or equal to 11—whether they are *Sisben* 1 rather than 2. It also includes a cubic in the *Sisben* score. The estimates restrict the sample to students whose score was within 1 and 0.25 points of the cutoff level—11 in this case. Thus, here the comparisons are carried out among students with scores between 10 and 12 points, and 10.75 and 11.25 points, respectively. This restricts the sample to about 10 and 2.5 percent of the total *Sisben* 1 and 2.

In these two specifications, the key coefficient of the regression of household per capita income (first row) becomes statistically insignificant and approaches zero. By column 2, it suggests that the difference in per capita income between students just above and below the cutoff is equivalent to only about one tenth of one percent of a standard deviation in per capita income, suggesting that at least along this observable dimension, these sets of students might provide a useful treatment/control comparison.

Columns 3-4 and 5-6 present similar evidence for high school—for the *Sisben* 1-2 and 2-3 samples, respectively. This reflects, as discussed in Section 2, that at the high school level *Sisben* 2 students are also eligible for the fee reductions, although the magnitude of the discounts is different (Table 3). For both of these samples it is also the case that for individuals close to the cutoffs, the differences in per capita income become statistically insignificant, although the magnitude of the points estimates is not as small as in the basic education case.

A relevant point is that of course the high school cutoff analyzed in columns 3-4 (Sisben 1/2) occurs at a lower point in the score than that considered in columns 5-6 (Sisben 2/3)—11 vs. 22 points. This implies that down the line, these two cutoffs will identify local treatment effects for potentially rather different populations. For instance, the descriptive statistics in Table 4 show that the individuals around the second cutoff have per capita income about 22 percent higher than those around the first, and that their

household heads have on average one extra year of schooling. As expected, therefore, in general the results around the second cutoff will refer to wealthier students.

Besides per capita income, the data provide a number of additional characteristics. For instance, Figure 4 presents analogous evidence where the comparison variable is the schooling achieved by household heads. The visual evidence again suggests no clear breaks at the cutoff scores—in fact the segments describing the fitted values of regressions run within each segment essentially meet at the cutoff. Similarly, Figure 5 presents the evidence where the comparison variable is the type of school in which the student was enrolled at the baseline (last row, Table 6). The figure suggests differences of very small magnitude, as the Table 6 confirms.

Finally, the differences between individuals across the cutoffs for the rest of variables showed in Table 6 are generally small. For instance, in some cases the point estimates for the baseline enrollment rates are very close to zero (the first row in Panel B) Despite the large sample sizes (ranging from about a thousand to 39 thousand children), these are never statistically significant. On the other hand, the sample sizes are often sufficiently large that even some small differences in other variables are significant—in this case about 14 out of 64 point estimates.

C. Effects on enrollment

Figure 6 introduces the enrollment results for both basic education (solid line) and high school education (dashed line). As in previous figures, it displays the probability of enrollment in public school as a function (using a local linear polynomial estimator) of children's *Sisben* score.

If *Gratuidad* had an effect we would expect to see a discontinuity at the first cutoff in the solid line in Figure 6, and at both cutoffs for the dashed line. Only the first and the last of these expectations are borne out, namely, there is a discontinuity for basic grades of about 3 percentage points at the first discontinuity (and none at the second); and one slightly larger between *Sisben* 2 and 3 at the high school level. Contrary to the expectation, the discontinuity for high school grades between *Sisben* 1 and 2 is almost zero.

Table 7 presents the statistical version of these results. For each grade and *Sisben* range combination, it displays the results for the full sample (with and without a cubic in the *Sisben* score), and then results within a band of 1 point around the relevant cutoff. For completeness, the results within a band of 0.25 points are presented in the Appendix. The table presents the results for different populations: all students; students attending public or private school at the baseline; attending school or not at baseline; students with households in *Estrato* 1 or 2; male or female students; and age appropriate or non-age appropriate students.

Columns 1-3 suggest that the discounts offered to students in *Sisben* levels 1 and 2 have had a significant impact on enrollment in basic grades. The specification that uses all the data (column 1, row 1) suggests that *Sisben* 1 students are about 2.8 percent more likely to enroll in school. Adding a cubic in the *Sisben* score (column 2) reduces the point estimate only slightly, and it is still significant at the 99 percent level. Similarly, restricting the sample to those students whose *Sisben* scores are within one point of the cutoff (column 3) still produces a point estimate of about 2.9, in this case significant at the 5 percent level.

The results at the high school level, where there are two relevant cutoff levels, are somewhat more mixed. For *Sisben* 1 and 2 individuals, columns 4-6 suggest the program had no consistent effect on enrollment. Columns 7-9 do suggest a significant positive effect for Sisben 2 and 3 children. While the effect is large and always statistically significantly different from zero, the point estimates are less stable than those observed for basic education. These differences raise the possibility of heterogeneous impacts of the program across different subpopulations. It is possible, for example, that the fifty-percent subsidy is not sufficiently high to cover the different costs faced by families near the Sisben 1 and 2 transition, but is sufficiently high for those families near the Sisben 2 and 3 transition. However, there is also a composition effect involved as well. Most of the students at the transition between Sisben 2 and 3 are from *Estrato* 2 which means that they were not affected by the grandfathering like those at the break between Sisben 1 and 2. The difference in school fees is thus larger for the former students.

The second and third rows of Table 7 disaggregate students depending on whether or not they were attending a private or public school at baseline. This distinction is

important in order to test for the possibility that students may switch between sectors in response to the change in relative prices, rather than changing their enrollment status. If this were the case, then we would expect to see a strong response from students previously enrolled in a private school and no response from students previously enrolled in a public school. As Table 7, column 2 shows, the program for basic education has a point estimate of 2.9, significant at the 10% level for students that were enroll in public school at baseline, whereas a non significant point estimate for students previously enrolled in private institutions. The estimates for high school, and discontinuities between Sisben 1–2 and 2-3, show that students for both private and public schools are almost equally likely to enroll in public schools. In short, the results instead suggest that the discounts have encouraged students who otherwise would not have enrolled in school to do so.

Rows four and five divide the data into students who were attending and not attending school when surveyed by the SISBEN. These results are important to see if the program encouraged students to return to school. As Table 4 shows, the percentage of students enrolled at the moment of the baseline was 89.9 for the full sample. However, it seems that the program increases the probability of enrollment for those already enrolled in basic education at the baseline–a statistically significant point estimated of 2.8–and for high school in the cutoff between *Sisben* 2 and 3–a point estimate of 6.0.

The results also suggest that students respond to the size of the subsidy. Students who were registered as *Estrato* 1 were given the full subsidy through the beginning of the academic year, and thus did not face as sharp a difference in the cost of education as those students who were classified as *Estrato* 2. The sixth and seventh rows of Table 7 disaggregate the results by the students' *Estrato* classification. While there are very few *Estrato* 1 students with SISBEN scores large enough to put them near the SISBEN 2 and 3 cut-off, the results for basic students show a 3.7 percentage point response for students in *Estrato* 2 and an insignificant 1.6 percent response from students in *Estrato* 1. Also, for students in grades 10 and 11, *Sisben 2* and 3, students in households in *Estrato* 2 have a point estimate of 6, significant at the 10% level, whereas the students in *Estrato 1* have an non significant point estimate.

Additionally, Table 7 breaks down the individual results by student demographic characteristics. Rows 8 and 9 divide the sample by gender. For basic education, most of the point estimates are positive and boys have a statistically significant 4.3 percentage point response, whereas the point estimator for females is not statistically significant. In contrast, females have a statistically significant 7.6 percentage point response in high school grades, and the estimator for male is non significant. In short, the results suggest a non-linear pattern regarding gender.

Finally, the Table shows results by students who are within a year of being in the appropriate grade given their age, and those students who are older. The results suggest that the program benefits at-risk students (e.g., old for grade) more than those who are not lagging behind. Results for students who are behind are displayed in row 10, revealing a statistically significant 4.8 percent response from children in basic education and a 7.7 percent response from those in media. Students who are age appropriate for their grades show no response and a much smaller (and statistically insignificant) 2.9 percent response respectively.

7. Conclusion

Several governments are interested in eliminating the barriers to enrollment that the direct costs of schooling may entail. In this paper we have sought to estimate the effects of one such effort—that implemented by Bogotá's municipal government. The manner in which the program was implemented renders feasible a regression discontinuity design to evaluate its impact. Subject to several conditions for which we find support in the data, this design can yield credible estimates of the program's causal effect, and in this case is strengthened by large sample sizes given the city's size.

Our preliminary results suggest that the program had a significant impact. The estimates suggest that the program raises the probability of enrollment for basic-aged *Sisben* 1 students by about 3 percent, and for high school-aged *Sisben* 2 students by about 6 percent. Importantly, these positive effects seem to be larger for at-risk students. While there are groups for which we find no effect, the overall pattern of results suggests

the program had a significant impact. The program also seems to display a substantial degree of heterogeneous impacts for different populations.

It is important to further elaborate on these results and their policy implications. For instance, we find evidence that students' enrollment responses are sensitive to the size of the implied subsidies—a result that will make feasible further work that might productively inform modifications to the program's parameters.

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Table 1: School fees in the city of Bogotá

Category	Frequency	Items
1. Academic fees	Annual or monthly	Registration
		Board
2. Complementary services	Annual, monthly,	Report cards
	or when event takes place	School handbook
		ID cards
		Pedagogical materials
		Maintenance of infraestructure
		Field trips
3. Periodic charges	Monthly	Transportation
		Food services
4. Other	When event takes place	Certifications
		Replacement of ID cards
		Replacement of school handbook
		Graduation fees

Source: Resolution No. 2693, Sept., 2003; No. 4670, Oct. 2004; and No. 4465, Oct. 2005

Table 2: Educational spending in Bogotá

Sisben level	Grades 1-5		Grades 6-9		Grades 10-11	
	Median (in dollars)	% of min salary	Median (in dollars)	% of min salary	Median (in dollars)	% of min salary
Sisben 1	7	6.3	10	8.3	15	12.7
Sisben 2	8	7.2	11	9.4	14	12.3
Sisben 3	10	8.5	13	11.5	11	9.8
Sisben 4	15	13.3	12	10.1	29	24.8

Source: Fedesarrollo (2005) based on the *Encuesta de Calidad de Vida 2003*.

Table 3: Percentage fee reduction due to *Gratuidad* by grade, *Sisben* and *Estrato* level

		Basic education, grades 1-9 (Complementary services)			High School, grades 10-11 (Complementary services and academic fees)		
		Sisben level (2006)			Sisben level (2006)		
		1 (scores 1-11)	2 (scores 11-22)	3-6 (scores above 22)	1 (scores 1-11)	2 (scores 11-22)	3-6 (scores above 22)
Estrato (2005)	One	100%	0%*	0%*	100%	~50%*	0%*
		(92,474)	(21,867)	(88)	(19,342)	(3,884)	(17)
	Two	100%**	0%	0%	100%**	~50%*	0%
		(12,372)	(269,726)	(51,780)	(3,615)	(55,781)	(8,799)
	Three	100%**	0%	0%	100%**	~50%*	0%
		(104,846)	(291,593)	(51,868)	(22,957)	(59,665)	(8,816)

Note: The number of children whose age renders them eligible for enrollment is in parenthesis
* For *Estrato* 1 students, the SED provided subsidies at the 2005 level up to March 31, 2006. If these students cound not demonstrate they belonged to *Sisben* 1, they lost their reductions.
** Being in *Estrato* 2 or 3 in 2005, these students did not benefit from reductions, but began enjoying them in 2006 once the *Sisben* classification came into use.

Table 4: Descriptive statistics

Variable	Full Sample	Basica (grades 1-9) Sisben 1 to 2		High sch. (grades 10-11) Sisben 1 to 2		High sch. (grades 10-11) Sisben 2 to 3		Correlation with Enrollment
		Full sample	± 1 pt. from cutoff	Full sample	± 1 pt. from cutoff	Full sample	± 1 pt. from cutoff	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Household variables								
Household income per capita	98.0	85.9	69.5	88.1	72.5	107.9	146.3	-0.088
	(72.3)	(53.0)	(42.4)	(54.7)	(43.8)	(74.0)	(78.7)	(0.00)
Household income	485.8	440.8	392.1	452.3	407.7	524.4	666.0	0.009
	(340.1)	(284.7)	(252.6)	(290.4)	(261.4)	(350.6)	(381.2)	(0.00)
Number of people in hhld.	5.3	5.4	5.9	5.4	5.9	5.1	4.7	-1.892
	(2.0)	(2.0)	(2.1)	(2.0)	(2.1)	(1.8)	(1.4)	(0.08)
Number of children under 6	0.37	0.41	0.49	0.31	0.38	0.26	0.19	-2.067
	(0.63)	(0.66)	(0.72)	(0.60)	(0.66)	(0.53)	(0.44)	(0.12)
Number of children under 18	2.6	2.7	3.1	2.6	2.8	2.4	2.1	1.871
	(1.3)	(1.4)	(1.5)	(1.4)	(1.5)	(1.2)	(1.0)	(0.09)
Household head yrs. of sch.	6.6	6.2	4.7	5.8	4.6	6.8	8.7	-0.703
	(3.5)	(3.2)	(2.5)	(3.1)	(2.5)	(3.4)	(3.2)	(0.02)
Age of household head	43.5	42.8	43.2	46.2	46.3	46.2	46.0	0.002
	(10.2)	(10.5)	(9.8)	(9.5)	(8.9)	(9.5)	(8.8)	(0.01)
Household head works	0.82	0.81	0.82	0.80	0.81	0.81	0.83	1.576
	(0.39)	(0.39)	(0.38)	(0.40)	(0.39)	(0.39)	(0.37)	(0.18)
Household head is single	0.26	0.27	0.25	0.30	0.29	0.28	0.21	-3.884
	(0.44)	(0.44)	(0.43)	(0.46)	(0.46)	(0.45)	(0.41)	(0.16)
Panel B: Individual variables								
Enrolled at baseline	0.898	0.89	0.85	0.88	0.81	0.93	0.97	1.727
	(0.30)	(0.31)	(0.36)	(0.32)	(0.39)	(0.26)	(0.16)	(0.27)
Employed	0.00	0.00	0.00	0.02	0.02	0.01	0.00	-14.866
	(0.07)	(0.03)	(0.03)	(0.13)	(0.16)	(0.11)	(0.07)	(1.98)
Own income	0.6	0.1	0.1	2.4	3.3	1.8	0.8	0.016
	(11.7)	(5.7)	(5.6)	(22.9)	(26.8)	(20.5)	(12.8)	(0.01)
Age	12.0	11.0	11.1	17.0	17.0	17.0	16.9	-2.831
	(3.4)	(2.6)	(2.6)	(0.8)	(0.8)	(0.8)	(0.8)	(0.05)
Years of schooling	3.4	2.3	2.0	6.9	6.2	7.3	7.8	4.349
	(2.9)	(2.2)	(2.1)	(2.0)	(2.0)	(1.8)	(1.5)	(0.05)
Male	0.51	0.51	0.51	0.51	0.52	0.50	0.51	-0.142
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.13)
Enrolled in public school	0.69	0.69	0.71	0.78	0.75	0.76	0.69	26.97
	(0.46)	(0.46)	(0.45)	(0.42)	(0.43)	(0.43)	(0.46)	(0.18)
Estrato 1	0.25	0.28	0.32	0.28	0.25	0.06	0	4.005
	(0.44)	(0.45)	(0.47)	(0.45)	(0.43)	(0.23)	(0.05)	(0.16)
N	570,648	388,238	39,646	118,481	13,949	97,450	8,097	

Note: Average values. Standard deviations are in parenthesis in columns (1) - (7), and standard errors in column (8)

Table 5: First stage regressions

	Full sample (1)	Students eligible for basic education (grades 1-9) (2)	Students eligible for high school education (grades 10-11) (3)
$1\{S_{i>} 11\}$	1.00 ***	1.00 ***	1.00 ***
	(0.00)	(0.00)	(0.00)
$1\{S_i > 22\}$	1.00 ***	1.00 ***	1.00 ***
	(0.00)	(0.00)	(0.00)
Sisben score (S_i)	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)
$(S_i - 11)*1\{S_i > 11\}$	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)
$(S_i - 22)*1\{S_i > 22\}$	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)
N	570,648	439,773	130,875
R^2	0.9997	0.9997	0.9998

Table 6: Continuity checks for household and individual level variables

	Grades 1-9 Sisben 1 to 2		Grades 10 and 11 Sisben 1 to 2		Grades 10 and 11 Sisben 2 to 3	
	Within band of		Within band of		Within band of	
	1 point	0.25 points	1 point	0.25 points	1 point	0.25 points
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Household variables						
Household income per capita	-0.4	-0.1	-1	-3.6	-0.4	7.9
	(1.2)	(2.4)	(2.0)	(4.0)	(4.8)	(9.8)
Household income	-10.1	4.9	-30.0**	-48.6**	-21.7	-0.4
	(6.9)	(14.2)	(11.9)	(24.1)	(23.1)	(46.3)
Number of people in the hh	-0.1**	0	-0.3***	-0.4*	-0.3***	-0.2
	(0.1)	(0.1)	(0.1)	(0.2)	(0.1)	(0.2)
Number of children under 6	0	0	0	0	0	-0.1
	(0.0)	(0.0)	(0.0)	(0.1)	(0.0)	(0.1)
Number of children under 18	-0.1	0.2*	-0.2**	-0.1	-0.1*	-0.3**
	(0.0)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)
Household head yrs of sch.	0	0.4**	0	0.3	0.4*	1.2***
	(0.1)	(0.1)	(0.1)	(0.2)	(0.2)	(0.4)
Age of household head	-0.7**	-1.7***	-0.5	-2.3***	-0.4	0.1
	(0.3)	(0.6)	(0.4)	(0.8)	(0.5)	(1.1)
Household head works	0	0	0	0	0	0
	(0.0)	(0.0)	(0.0)	(0.0)	(0.0)	(0.0)
Household head is single	0.0**	0	0.0**	0.1	0	0.1
	(0.0)	(0.0)	(0.0)	(0.0)	(0.0)	(0.0)
Panel B. Individual variables						
Enrolled at baseline	0	0	0	0	0	0
	(0.0)	(0.0)	(0.0)	(0.0)	(0.0)	(0.0)
Employed	0	0	0	-0.0*	0	0
	(0.0)	(0.0)	(0.0)	(0.0)	(0.0)	(0.0)
Own income	-0.2	-0.3	0.9	-1.2	-0.3	-1
	(0.2)	(0.2)	(1.2)	(2.7)	(0.8)	(1.2)
Age	0.1	0.1	0	0	-0.1	0
	(0.1)	(0.1)	(0.0)	(0.1)	(0.0)	(0.1)
Years of schooling	0	0	0.1	0	0.2**	0
	(0.1)	(0.1)	(0.1)	(0.2)	(0.1)	(0.2)
Male	0	0	0	0	-0.1**	0.1
	(0.0)	(0.0)	(0.0)	(0.0)	(0.0)	(0.1)
Attends public school	0.0*	0	0	0	0	-0.1**
	(0.0)	(0.0)	(0.0)	(0.0)	(0.0)	(0.1)
N	39646	9411	13949	3374	8097	1901

Note: The table reports the coefficient of regressions of each characteristic on a dummy indicating whether individuals have a Sisben score below the corresponding cutoff (11 for columns 1-4, and 22 to columns 5-6.) The regressions include a cubic term in the score, and standard errors are in parentheses.

Table 7: *Gratuidad* and the probability of enrollment

	Grades 1 to 9 - Sisben 1 and 2			Grades 10 and 11 - Sisben 1 and 2			Grades 10 and 11 - Sisben 2 and 3		
	Full sample		Band of One point	Full sample		Band of One point	Full sample		Band of One point
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
All students	2.8***	2.4***	2.9**	-5.3***	0.6	0.7	10.9***	2.1**	6.1**
	(0.2)	(0.4)	(1.4)	(0.3)	(0.7)	(2.3)	(0.5)	(0.9)	(3.0)
	388238	388238	39646	118481	118481	13949	97450	97450	8097
Attending public school	1.1***	2.3***	2.9*	-3.2***	2.6***	2.9	-0.9	1	5.4
	(0.2)	(0.5)	(1.6)	(0.4)	(0.8)	(2.6)	(0.6)	(1.1)	(3.5)
	269415	269415	28014	92153	92153	10565	74329	74329	5582
Attending private school	12.3***	-0.1	5.1	10.5***	-3.8	-5.5	13.2***	0.6	5.9
	(0.7)	(1.4)	(5.4)	(1.2)	(2.6)	(8.9)	(0.7)	(1.5)	(4.6)
	46467	46467	2624	12225	12225	760	15674	15674	2301
Not attending at baseline	1.5***	0.4	2.3	-2.4***	-2.9**	-6.2*	1.3	10.6***	-1.9
	(0.5)	(1.1)	(3.2)	(0.6)	(1.3)	(3.4)	(2.4)	(4.1)	(13.6)
	42642	42642	6076	13856	13856	2583	7258	7258	206
Attending at baseline	4.8***	1.8***	2.8*	0.2	2.1***	2.3	13.8***	2.3**	6.0*
	(0.2)	(0.4)	(1.5)	(0.4)	(0.8)	(2.5)	(0.5)	(0.9)	(3.1)
	345596	345596	33570	104625	104625	11366	90192	90192	7891
Estrato 1	-0.6	-0.5	1.6	-9.6***	-4.2***	-5.5	33.7***	23.3	14.6
	(0.4)	(0.8)	(2.4)	(0.7)	(1.5)	(4.6)	(10.1)	(16.9)	(51.1)
	111852	111852	12664	33446	33446	3493	5521	5521	23
Estrato2	-4.5***	-0.8	3.7**	-14.7***	-1.3	2.4	10.5***	1.8**	6.0**
	(0.5)	(0.8)	(1.7)	(0.7)	(1.2)	(2.6)	(0.5)	(0.9)	(3.0)
	276386	276386	26982	85035	85035	10456	91929	91929	8074
Males	2.1***	2.8***	4.3**	-6.5***	0.6	-0.1	9.6***	1.2	4.7
	(0.3)	(0.6)	(1.9)	(0.4)	(1.0)	(3.1)	(0.7)	(1.3)	(4.2)
	197394	197394	20212	60093	60093	7243	49118	49118	4136
Females	3.4***	2.0***	1.5	-3.9***	0.6	1.7	12.3***	3.2**	7.6*
	(0.3)	(0.6)	(1.9)	(0.5)	(1.0)	(3.3)	(0.7)	(1.3)	(4.4)
	190844	190844	19434	58388	58388	6706	48332	48332	3961
Old for grade	2.8***	1.9***	4.8***	-5.2***	-0.1	-0.1	9.0***	1.6	7.7**
	(0.2)	(0.5)	(1.7)	(0.3)	(0.8)	(2.4)	(0.6)	(1.1)	(3.7)
	206495	206495	24445	95645	95645	12481	73263	73263	5376
Age appropriate	1.9***	3.2***	-0.7	4.3***	2	10.5	16.7***	4.2***	2.9
	(0.3)	(0.7)	(2.2)	(1.0)	(2.1)	(7.3)	(0.8)	(1.6)	(5.3)
	177423	177423	14933	22507	22507	1445	23845	23845	2687
Cubic in score	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes

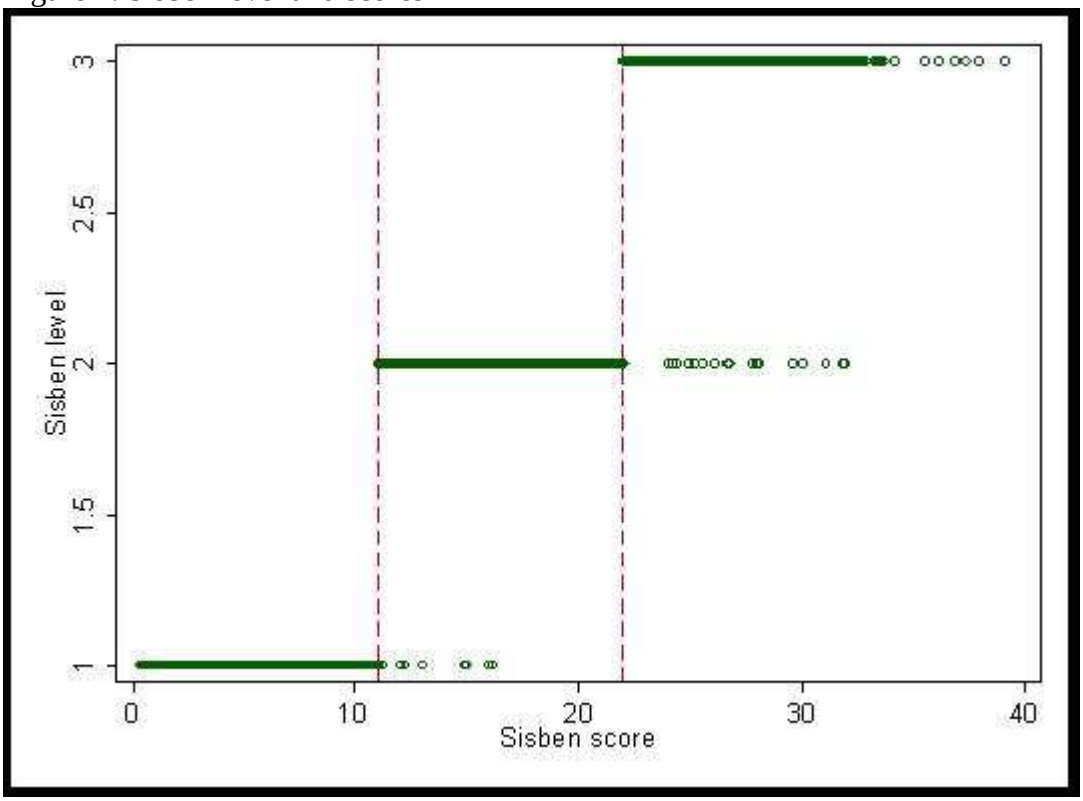
Note: The table reports the coefficients on the *Sisben* dummy variables. Standard errors are in parentheses; and the number of observations are below them.

Appendix. Effects of the program on the probability of enrollment. Band of 0.25 points

	Grades 1 to 9 Sisben 1 and 2	Grades 10 and 11 Sisben 1 and 2	Grades 10 and 11 Sisben 2 and 3
All students	3.9	-7.8*	-1.5
	(2.8)	(4.6)	(6.2)
	9411	3374	1901
Attending public school	2.9	-6.7	3.6
	(3.2)	(5.4)	(7.1)
	6626	2523	1335
Attending private school	22.2**	-16	10.6
	(10.5)	(16.1)	(9.8)
	586	189	513
Not attending at baseline	3.7	-8.3	-42.8
	(6.8)	(6.5)	(33.2)
	1518	652	52
Attending at baseline	4.4	-7.7	-0.2
	(3.0)	(5.2)	(6.3)
	7893	2722	1849
Estrato 1	3.8	-14.3	-469.6***
	(4.9)	(9.1)	(90.9)
	3029	795	6
Estrato2	3.5	-6.3	-1.1
	(3.4)	(5.3)	(6.2)
	6382	2579	1895
Males	2.6	-12.6**	-11.4
	(4.0)	(6.2)	(8.5)
	4713	1779	993
Females	5.2	-1.2	10.6
	(3.9)	(6.9)	(9.1)
	4698	1595	908
Old for grade	2.8	-8.2*	3.5
	(3.5)	(4.8)	(7.5)
	5902	3051	1259
Age appropriate	4.6	3.4	-13.9
	(4.7)	(16.6)	(11.2)
	3464	320	636
Cubic in score	Yes	Yes	Yes

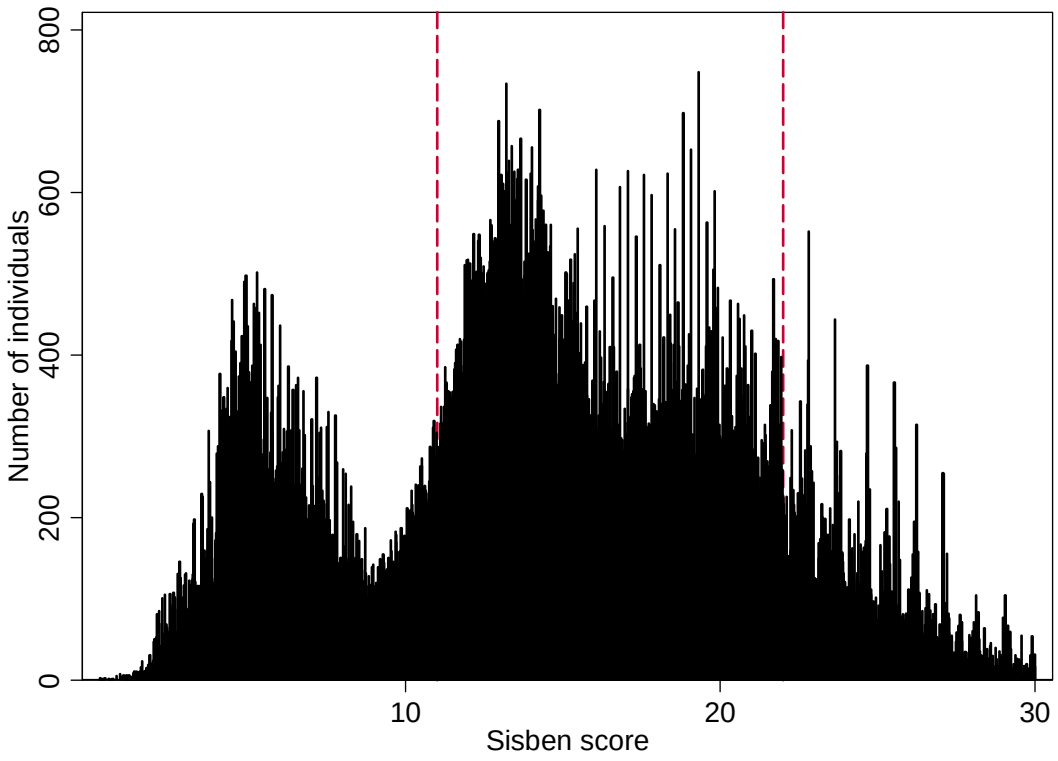
Note: The table reports the coefficient of the Sisben dummy variable.
Standard errors are in parentheses, and the number of observations below them.

Figure 1. Sisben level and scores



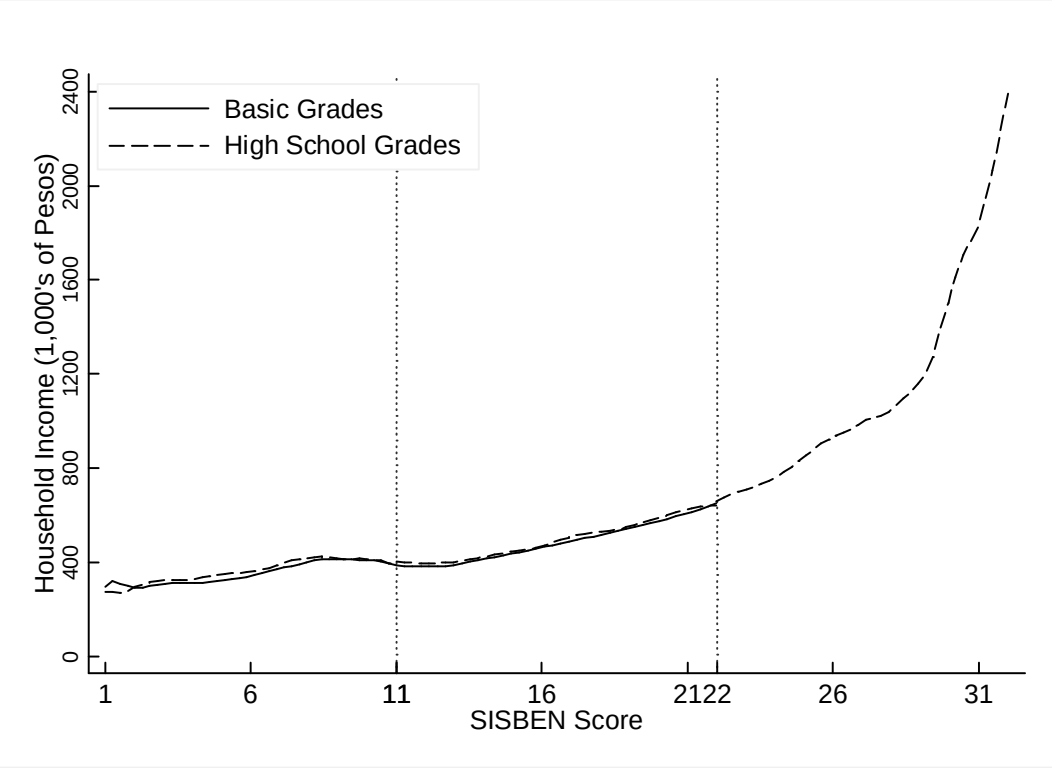
Note: The Figure plots individual Sisben index level against their households' score. The dotted lines indicate the critical scores between Sisben index levels

Figure 2. Histogram of Sisben scores



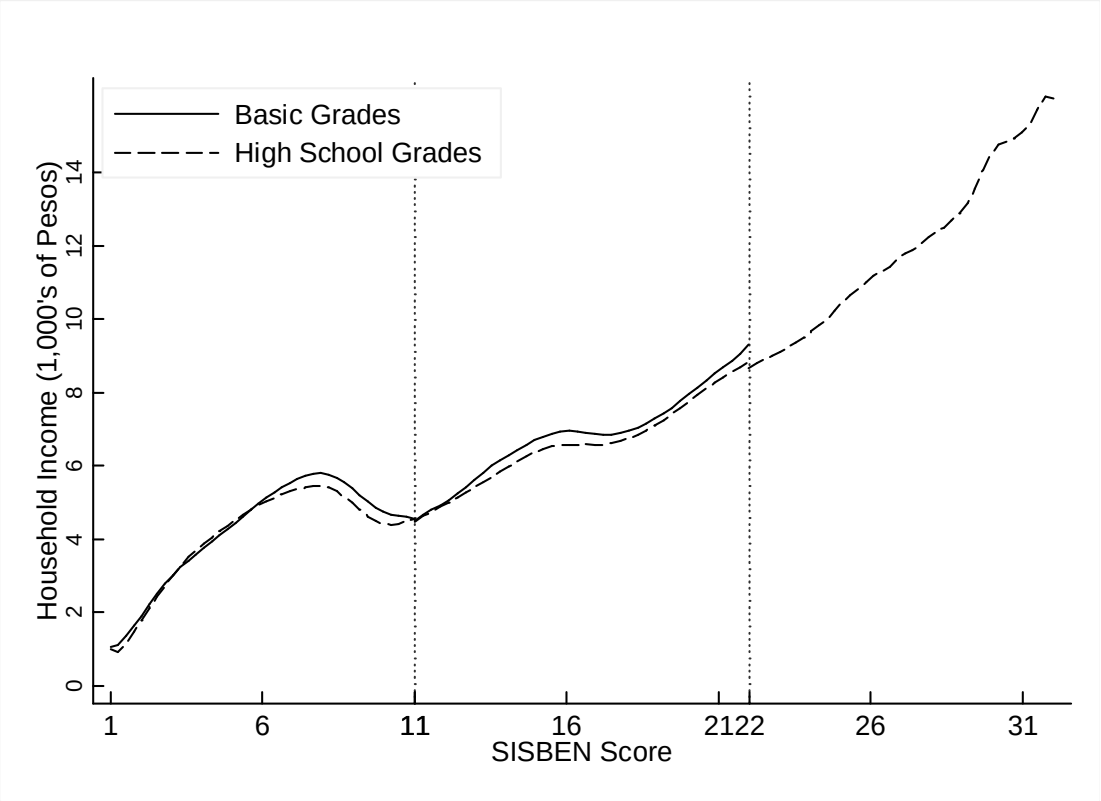
Note: the figure plots a histogram of Sisben scores, which in the administrative data we use are calculated with two decimals. The dotted lines indicate the critical scores between Sisben levels

Figure 3. Household income and Sisben score



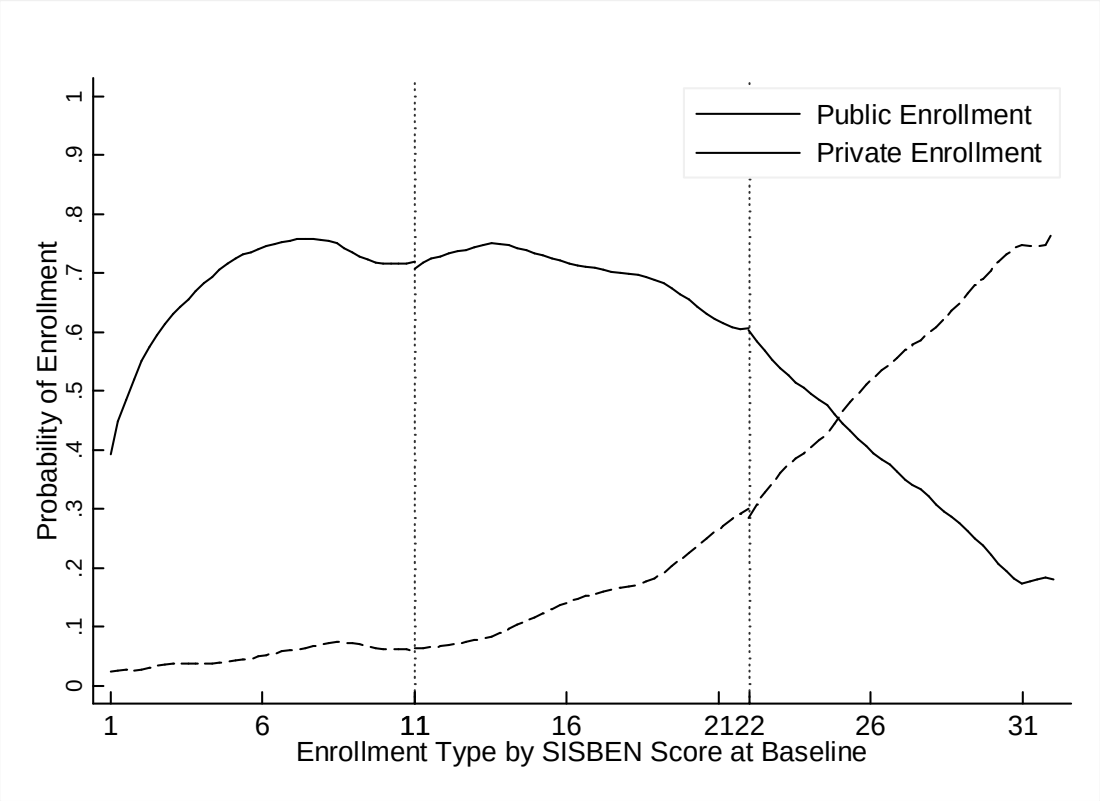
Note: The Figure uses individual level data and plots the fitted values of locally weighted regressions of students' per capita income on their households' Sisben score.

Figure 4. Household heads' years of schooling and the Sisben score



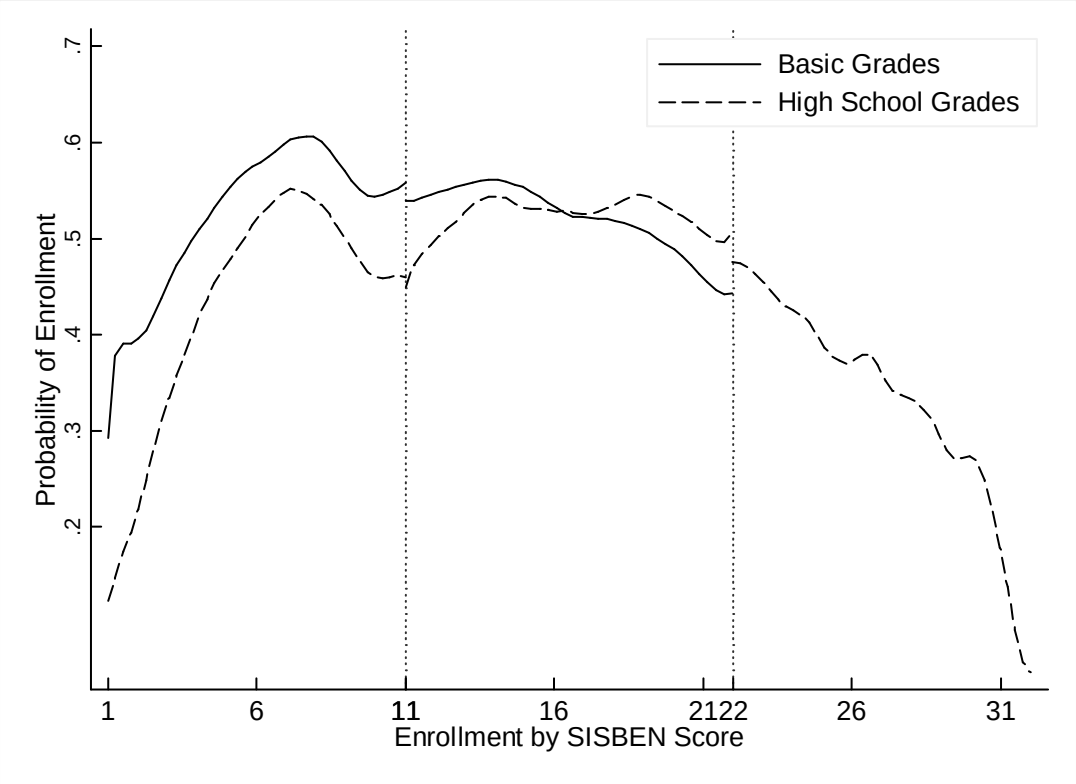
Note: The Figure uses individual level data and plots the fitted values of locally weighted regressions of Household heads' years of schooling on their households' Sisben score.

Figure 5. Enrollment by type of institution and Sisben score



Note: The Figure uses individual level data and plots the fitted values of locally weighted regressions of students' type of institution on their households' Sisben score.

Figure 6. Enrollment and the Sisben score



Note: The Figure uses individual level data and plots the fitted values of locally weighted regressions of students' type of institution on their households' Sisben score.