THE IMPACT OF CONDITIONAL CASH TRANSFERS TO HIGH SCHOOL STUDENTS ON DROPOUT RATES: AN EVALUATION OF MEXICO'S PROBEMS

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January 2013

Abstract

This article presents the first impact evaluation of PROBEMS, a government program of conditional cash transfers to students from poor families in public high schools in Mexico. Beneficiaries receive between 500 and 1,000 pesos (between 40 and 80 US dollars) per month if they remain as students in good standing. The exact amount of the transfer is a function of gender, grade, previous GPA, and type of school. Discontinuities in the program structure provide quasi experiments at the extensive margin (being a beneficiary or not) as well as at the intensive margin (a greater or smaller transfer). The impact of the program is evaluated using those discontinuities and administrative records from program applications of 10th-graders, a list of them who took the national test ENLACE in 12th grade, and enrollment records from a group of technological high schools. The results show that PROBEMS' conditional cash transfers reduce dropout rates, but the magnitude of the impact estimate depends on the sample and the econometric specification used. The impact attributable to the program as a percentage of the number of dropouts can be of up to 20%.

JEL classification: I21, I28, I38, O12, O15.

Keywords: scholarships; conditional cash transfer programs; program evaluation; regression discontinuity design; PROBEMS; Mexico.

[*] This work has benefitted from the comments of Thania de la Garza, Curtis Huffman, Mathis Wagner, Janet Zamudio and seminar participants at El Colegio de México and Instituto Tecnológico Autónomo de México. The author is grateful to José Antonio Ramírez, Luis Miguel García, and Erick Javier Sánchez from PROBEMS for providing the data used for the evaluation and explaining the details of the program operation, and to Bernardo García-Bullé for his programming assistance. All errors and omissions are responsibility of the author. Correspondence can be sent to pablo@uchicago.edu.

I. Introduction

Education supply at the high school level in Mexico has expanded in the last two decades. [1] Between 1990 and 2010 the number of high schools grew 143% and the number of high school teachers grew 92%. [2] In the same period, population of high school age (15 to 19 years old) only grew 8%. [3] Thus, access has increased as supply has outpaced potential demand. However, many students who start high school still drop out.

Currently more than a third of students who start high school in Mexico do not finish.[4] Many of them drop out of school to start working. According to a national survey, 43.4% of adult males and 23.5% of adult females who dropped out of high school did it "to start working to help sustain their families or themselves." [5] In other words, a large fraction of those who truncated their education at the high school level faced an opportunity cost that was too high.

One way of reducing high school dropout rates is with conditional cash transfers (CCT). Mexico's Oportunidades (originally called PROGRESA) was a pioneer in the use of transfers conditional on school attendance. There are now CCT programs in many countries aimed at the reduction of school dropout rates (see Cameron 2009, Dearden el al. 2009 and Glewwe and Kassouf 2012). PROBEMS is one such program and this article presents its first impact evaluation.

PROBEMS started in 2007 and by 2011 it had nearly half a million beneficiaries. The program structure has discontinuities that allow a quasi-experimental impact evaluation. The program can affect the decision to drop out of school in two margins, one extensive and one intensive. The extensive margin is given by whether a student receives the CCT or not. The intensive margin is given by the amount of the CCT. For the evaluation I use administrative records from the applications of 10^{th} grade students (those starting high school) who applied for the benefit in the summers of 2009 and 2010. I combine program application records with data from two sources that are informative on whether the students were still in school the following years: the records of who took the national standardized test ENLACE in 12^{th} grade, and enrollment data from a group of technological high schools.

PROBEMS administrative records are confidential. PROBEMS officials made them available to the author to evaluate the impact of the program. The author was not compensated in any form by PROBEMS or the Ministry of Education for performing the evaluation, other than retaining the rights to publish the results.

[1] High school education in Mexico comprises grades 10, 11 and 12, and includes some technical education that might not allow continuing with college education (known as medium professional studies or *estudios de profesional medio*).

[2] Secretaría de Educación Pública (2012a).

[3] Secretaría de Salud (2012).

[4] In the academic year 2010-11 terminal efficiency was 63.3%. Secretaría de Educación Pública (2012b).

[5] Encuesta Nacional de Capacitación y Empleo 2001, INEGI.

II. Institutional Background

After the successful experience of Oportunidades in terms of schooling outcomes (see Schultz 2004, Berhman, Parker and Todd 2011, and Berhman, García-Gallardo, Parker, Todd and Vélez-Grajales 2012), the Mexican government decided to extend the benefits of the program to high school students in 2001 and in 2007 implemented PROBEMS, a CCT program complementary to Oportunidades exclusively targeted at reducing high school dropout rates.[6] PROBEMS was designed to modify the incentives of teenagers from low-income households attending public high schools and who are not Oportunidades beneficiaries, giving cash to them if they remain students in good standing.

Every year the Ministry of Education announces the guidelines to become a beneficiary of PROBEMS. Student participation is voluntary. If a student wants to receive the CCT, she must fill out an application once she is registered at a public high school. Once a student becomes a beneficiary, she continues receiving the CCT the following academic years of high school as long as she remains in good standing—beneficiaries do not need to apply for the CCT every year.

When applying for the CCT students fill out a questionnaire in which they report information on socioeconomic characteristics of their household (e.g. if the dwelling has running water, the materials the dwelling is made of, if the family owns a car, etc.) Applicants also report a list of all household members, their earnings and other characteristics such as whether they are beneficiaries of other government programs.

After receiving the applications, PROBEMS officials validate them through several steps. Applications should be completely filled out. Applicants must be registered at a public high school. They must not be beneficiaries of Oportunidades or Prepa Sí—PROBEMS officials look up their applicants in the lists of beneficiaries of those programs. [7] Applicants must have a household per capita income below the official patrimonial poverty line. [8] Based on the socioeconomic information reported by applicants, PROBEMS officials filter those below the poverty line and then build a socioeconomic index. With the index they determine the priority of every applicant to receive the benefit. The intention of the program is to give the CCT to the neediest valid applicants.

During the time PROBEMS has been in operation the formula to compute the socioeconomic index that determines applicants' priority has changed. In 2009 it was computed as a poverty index, weighting 13 variables such as a dummy for whether the family had a refrigerator, and dummy if at least one family member had social security benefits. Household income was not among those variables. In 2010 the socioeconomic index was

[6] PROBEMS is the acronym for High School Scholarship Program or *Programa de Becas de Educación Media Superior*.

[7] Prepa Sí is a local government program that gives cash transfers to students in public high schools in Mexico City. See http://www.prepasi.df.gob.mx/

[8] The official patrimonial poverty line is computed monthly by the National Council for the Evaluation of the Social Development Policy. In September 2009 it was 1,431 pesos per month in rural areas and 2,132 pesos in urban areas. In 2010, the amounts were 1,423 and 2,118 pesos, respectively.

computed as the household per capita income. In 2011 and 2012 the formula included both the household per capita income and other household characteristics.

Because of budget reasons, not all valid applicants become beneficiaries. To determine who receives the CCT among the valid applicants in a given year, program officials consider three elements: the amount of the transfers that applicants would receive, the amount needed to cover the transfers to beneficiaries grandfathered from the previous year, and the appropriations for the current fiscal year.

The amounts of PROBEMS transfers are defined according to the gender, grade and GPA of the student, together with the type of institution attended (technical versus general high school). In the academic years 2009-10 and 2010-11 the transfers were determined by Table 1. The transfer amount is a step function of grade and GPA in the previous grades. For 10^{th}-graders GPA corresponds to grades 7 to 9, for 11^{th}-graders it corresponds to 10^{th} grade, and for 12^{th} grader it corresponds to 11^{th} grade. Students report their GPA in the applications and PROBEMS authorities later verify it against high school records. In 10^{th} grade there are only two GPA-based categories, with a discontinuity at 8.0. A difference of one tenth of a point in the GPA, from 7.9 to 8.0, implies a difference in the transfer ranging from 24 to 33%, depending on gender and type of school. In 11^{th} and 12^{th} grades there are five GPA-based categories. The maximum difference in amounts ranges between 60 and 81% (from the lowest to the highest GPA). The amount of PROBEMS' transfers was determined to roughly match the amounts given by Oportunidades. [9]

PROBEMS officials rank applicants based on the socioeconomic index they build, from more to less need. Based on that ranking and using the amount of the transfers in Table 1 they can compute how many applicants could become beneficiaries given different budgets. The budget actually allocated to the applicants in one year is the difference between the program appropriations and the amount needed to cover the transfers to the beneficiaries grandfathered from the previous year. Given the appropriations and the grandfathered beneficiaries, the budget available to cover new applicants is fixed. With that fixed budget PROBEMS officials allocate the benefit to applicants, giving priority to the neediest.

The fixed budget together with the need-based prioritization imply that there is a level of the socioeconomic index that works as a cutoff: applicants below that level become beneficiaries and applicants above it do not. That discontinuity allows an impact evaluation at the extensive margin: there are applicants virtually identical at both sides of the cutoff. Some become beneficiaries and some do not. The comparison of those two groups is informative of the impact of the program.

The combination of the discontinuity induced by the budget insufficiency and the discontinuity in the amounts of the transfer associated with different GPAs allows an evaluation at the intensive margin. We can measure the impact of a greater transfer by comparing the beneficiaries and non-beneficiaries both around the transfer discontinuity.

[9] Oportunidades' transfers per student enrolled in high school range between 510 and 660 pesos per month. See Berhman, Parker and Todd (2011), Table 1.

It is important to make clear that when students apply for the benefit they do not know the formula to be used to compute the socioeconomic index. They do not know what the appropriations of the program are, how many grandfather beneficiaries there are or how many applicants there are in the same year.

Not all students in public high schools apply for the benefit. There probably is self-selection into the application. It is possible that some students rule themselves out, foreseeing that their socioeconomic status would make them ineligible. It is also possible that some are not aware of the program or the application requirements. Table 2 shows the number of students in public high schools in Mexico, the number of PROBEMS applicants, and the number of beneficiaries from 2007 to 2011. Column A shows total enrollment in public high schools. In the period shown, enrollment grew 15%, from 3.1 to 3.5 million students. Column B shows the number of PROBEMS applicants every year. As mentioned above, previous years' beneficiaries do not have to apply for the benefit anew—they keep it as long as they remain in good standing. Column C shows the number of applicants in that year that became new beneficiaries. Column D shows the total number of beneficiaries, both new and grandfathered from the previous year.

Columns E, F, and G in Table 2 present several ratios. Column E shows the fraction of applicants who became beneficiaries. At the beginning of the program all applicants became beneficiaries. In subsequent years the fraction has stayed below 0.5. Column F shows the fraction of all students in public high schools who are beneficiaries of PROBEMS. In the period shown the maximum is 0.14, and it was reached in 2010. Column G shows an upper bound for the fraction of all students in public high schools who applied for the benefit in that year, or that had it the year before and could keep it. That fraction could be interpreted as an indicator of the demand for the benefit. In the denominator is the total number of students in public high schools. In the numerator is the number of applicants plus the number of grandfathered beneficiaries.[10] Even though the indicator suggests an increasing demand, the maximum in the period is 0.27. Those levels suggest self-selection into the application process.

Program beneficiaries receive the transfer bimonthly (12 months per grade) through an "electronic purse" to which only the Ministry of Education can make deposits. Students have an ATM card that can also be used to pay at commercial establishments with point-of-sale terminals. The fact that it is the student who receives the transfer distinguishes PROBEMS from Oportunidades and other CCT programs in an essential way. The recipient is not the mother or the head of household but the teenage student. In fact, parents might not be aware that their child is receiving a transfer. Because of this distinction, the impact of PROBEMS is expected to differ from the impact of other CCT programs.

There are theoretical and empirical studies on how transfers can have different effects depending on which household member is the recipient (see Alderman et al. 1995). That is the premise of Oportunidades and other CCT programs. They require the mother to be the recipient of the transfer. However, if the aim of the program is to change the

[10] It is an upper bound because some of the grandfathered beneficiaries graduated, some fell behind, and some of dropped out.

behavior of a teenage student, probably a direct transfer to the student provides a different incentive than a transfer to the mother.

III. Data

In order to evaluate the impact of the program it is necessary to have information on dropout rates for both beneficiaries and non-beneficiaries. However, after the application process PROBEMS only keeps track of beneficiaries. Thus, the evaluation requires data from other sources. Two different sources were used.

The first source is the national standardized test ENLACE given in March 2012.[11] Since ENLACE is given to 12th-graders, a student enrolled in 10th grade in September of 2009 should have taken ENLACE in March of 2012. Thus, the information on whether PROBEMS applicants in 10th grade in 2009 took ENLACE in 2012 is a proxy of whether applicants were in school as regular students nearly three years later, and therefore can be used as a measure of retention.

The second source is the enrollment data from a group of high schools administered by the Direction General for Technological and Industrial Education (DGETI) of the Ministry of Education. PROBEMS officials arranged for DGETI authorities to produce data on the enrollment status of 10th grade students who applied for the benefit in the summer of 2010. The enrollment status was provided at four different points in time: September 2010, March 2011, September 2011 and March 2012.

The use of ENLACE has the advantage that it allows the analysis of most 10th grade applicants in 2009, and it provides a proxy of whether they were about to graduate from the same school three years later. Its disadvantage is that taking ENLACE is a noisy measure of enrollment or attendance, since it is not a mandatory test. The enrollment data from DGETI have the advantage that they indicate whether applicants were enrolled or not in subsequent periods. Their disadvantage is that their results are limited to applicants in DGETI schools, and they might not be representative of the rest. Next I describe in more depth the two sources used for the evaluation.

Applications from 2009 and data from ENLACE 2012

ENLACE is taken by students in 12th grade in the month of March—a few weeks before the last year of high school ends. In principle, all students are supposed to take ENLACE. However, some schools have decided not to participate, preventing their students from taking it.[12] Although it is not an infallible indicator of whether students were enrolled and attending classes at that moment or of whether they graduated, ENLACE is a proxy that can be used for most applicants.

[11] ENLACE is the acronym for National Evaluation of Academic Achievement in School Centers. Since 2008 it has been given to 12th graders.

[12] That is the case of Universidad Nacional Autónoma de México's (UNAM) high schools. See the notes "La prueba ENLACE no se aplicará a bachilleres de la UNAM: autoridades" in *La Jornada*, December 7, 2007, and "UNAM rechaza participar en la prueba ENLACE" in *El Universal*, March 25, 2009.

In 2012 ENLACE was taken by students that were in 10th grade in 2009—or earlier if they repeated grade. Thus, in order to match the data from ENLACE 2012 with the data from PROBEMS applications I had to focus on 10th grade applicants from 2009.

The data used in the evaluation were obtained from the Ministry of Education's ENLACE results Internet site.[13] On that site it is possible to verify whether a student took the test or not. Only those who took ENLACE have a "folio number" assigned. To report whether a student has a folio number or not the website requires the population registry number (CURP) and the school ID number. The CURP and the school ID are available in PROBEMS' applications data. However, in 2009 not all schools had a school ID assigned. Those schools are excluded from the analysis.

Table 3 shows the process followed to arrive at the sample used in the evaluation, starting from all applicants in 2009. In that year, a total of 339,486 students applied for PROBEMS benefits. Only 234,319 had valid applications. Of all validated applicants, 135,869 were in 10th grade, and 126,367 were in schools with an ID—those are the applicants that I could look up in the ENLACE results website. Finally, of those applicants, 111,590 were in schools where at least one student took ENLACE in 2012. With that last filter schools not participating in ENLACE are ruled out of the analysis. The sample used for the evaluation contains 70,584 beneficiaries and 41,006 applicants rejected because of budget insufficiency.

To determine the priority of the validated applicants to become beneficiaries, in 2009 PROBEMS officials used a socioeconomic index computed as a linear combination of 13 variables reported by applicants—or built based on those variables. [14] The index was built as a measure of poverty, thus a higher index in principle means greater need and also should imply ceteris paribus a greater propensity to dropout. Figure 1 shows the distribution of applicants according to their socioeconomic index and their beneficiary status. Applicants who received the benefit in September of 2009 according to PROBEMS administrative records were considered beneficiaries. Only the 111,590 students reported in the right-most column in Table 3 are shown. As Figure 1 shows, the cutoff for the socioeconomic index is not sharp. There are applicants with a high poverty index who are not beneficiaries as well as applicants with a low poverty index who are beneficiaries. PROBEMS officials have not offered an explanation for the apparent violation.[15]

Since the cutoff is not sharp, to empirically determine its location I applied an algorithm to minimize the number of noisy observations, that is, those that violate the allocation rule based on a sharp cutoff. The result was a cutoff at a value of 0.08. The number

[13] http://enlace.sep.gob.mx/

[14] The 13 variables are: dummies indicating whether the household has dirt floor, does not have gas stove, does not have refrigerator, does not have washing machine, and does not have DVD player; dummies for municipalities with high and very high degree of marginalization; a dummy indicating that no household member has state-provided retirement benefits and medical, disability and life insurance ("*seguridad social*"); number of people age 15 or older with incomplete elementary education; number of people age 12 or older who work; number of people 5 to 15 years old not attending school; number of people per bedroom; number of people under 15 or over 65 years old divided by the rest. All variables are given a positive weight.

[15] Current officials were not in charge of the program in 2009.

of noisy observations based on that cutoff is 2,852 (equivalent to 2.6% of the observations): 1,660 are beneficiaries with a poverty index below 0.08 and 1,192 are non-beneficiaries with a poverty index equal to or greater than 0.08.

The metric of evaluation is whether the student dropped out based on the ENLACE proxy. If the student did not take ENLACE in 2012 in the same school where she applied for the benefit in 2009 (when she was in 10^{th} grade), then I consider her to have dropped out. Obviously, the definition is not free from problems. First, ENLACE is not mandatory. Someone might have not taken it because she was sick and skipped school. Second, the student might have switched schools. Third, the student might have fallen behind (and therefore not taken ENLACE in 2012) without actually dropping out. Those caveats should be borne in mind when interpreting the results.

Figure 2 shows dropout rates according to the poverty index and the applicants' beneficiary status. Circles correspond to non-beneficiaries and squares to beneficiaries. The size of the circles and squares represents the number of observations. Two facts are noteworthy. First, dropout rates seem lower for beneficiaries. The average dropout rate is 40.9% for beneficiaries, whereas for non-beneficiaries it is 46.2%. Second, within each group, the poverty index seems negatively associated with dropout rates: more poverty is associated with lower dropout rates.

GPA in previous grades is one of the main predictors of dropout rates. GPA summarizes several cognitive and non-cognitive skills of students. Figure 3 shows average dropout rates by GPA (grades 7 to 9). GPA is expressed in one-tenth categories (the scale ranges from 6 to 10). As in the previous figure, the size of the circles and squares represents the number of observations. The vertical line shows the discontinuity in the amount of the transfer. Beneficiaries with a GPA greater than or equal to 8.0 receive a greater transfer. Among applicants with a GPA below 8.0, the dropout rate is 61.3% for beneficiaries and 65.3% for non-beneficiaries. Among applicants with a GPA equal to or above 8.0, the dropout rate is 35.6% for beneficiaries and 39.2% for non-beneficiaries.

Table 4 shows descriptive statistics for the applicants in the sample. Males and females, and beneficiaries and non-beneficiaries, are presented separately. They all come from PROBEMS self-reported application questionnaires. Only a few variables among those available are displayed. Dropout rates for beneficiaries are lower than for non-beneficiaries: 43.3 versus 48.9% for males, and 38.9 versus 43.8% for females. At the same time, beneficiaries have lower household per capita income and better GPA. They live in households with more members. A smaller fraction of beneficiaries' households owns a car, a washing machine or a microwave oven. Except for the GPA, the averages shown suggest dropout rates should be greater for beneficiaries than for non-beneficiaries.

Applications from 2010 and DGETI enrollment records

DGETI administers over 400 high schools around the country that simultaneously grant a high school diploma and a technical degree.[16] Thanks to a request by PROBEMS officials, DGETI shared with me enrollment information for the applicants in 10th grade from 2010. The enrollment data consists of dummy variables indicating whether each applicant was enrolled at four points in time: September 2010, March 2011, September 2011, and March 2012. Those dummies were used to create a proxy of dropout rates to evaluate the program.

Table 5 shows the process used to arrive at the sample of applications from 2010 and DGETI data. In 2010 there were 426,805 applicants, and only 287,568 were validated. Of all valid applicants, 174,261 were in 10th grade. 44,275 of them were in DGETI schools: 28,360 were beneficiaries and 15,915 were rejected because of budget insufficiency.

In 2010 PROBEMS officials used the applicants' household per capita income as the socioeconomic index to define their priority. Household per capita income was computed based on the number of household members and the income of each of them, as reported by applicants. Ceteris paribus a greater household per capita income should be associated with a lower probability of dropping out. Figure 4 shows the distribution of validated applicants in 10th grade in DGETI schools (the 44,275 applicants in column 3 of Table 5).

Two facts are noteworthy in Figure 4. First, there is bunching in some income categories. Bunching can be explained because of the way applicants answer the application questionnaire. They are asked to report the income of each household member. They probably do not know it exactly and they speculate. Some figures come up more frequently than others. In particular, multiples of 1,000 or 500 are more frequent. [17] When adding up the income reported for several household members and dividing it by the number of members it is common to observe more frequently some figures than others.

Second, the allocation rule seems to be violated. There are beneficiaries with a greater income than some non-beneficiaries and vice versa. PROBEMS officials have explained that the apparent violation is due in part to special treatment of some applicants based on commitments by the federal government to support certain groups. However, no explanation has been given for applicants with a low socioeconomic index not receiving the benefit. For evaluation purposes, the sample is restricted to an interval of household per capita income from 100 to 900 pesos. The logic of the depletion of the budget resulting in a discontinuity seems to apply well in that interval. Column 4 in Table 5 shows the applicants in the interval: 26,712 validated applicants in 2010 in 10th grade in DGETI schools reported a household per capita income between 100 and 900 pesos per month. Of those applicants, 11,990 were beneficiaries and 14,722 were rejected because of budget insufficiency.

[16] Those schools are Centros de Bachillerato Tecnológico, Industrial y de Servicios (CBTIS) and Centros de Estudios Tecnológico, Industrial y de Servicios (CETIS).

[17] In the empirical literature it has been observed that people speculate in round numbers, which produces bunching. For instance, Dominitz and Manski (1996) asked students in the US about their yearly earnings expectations, and the answers show bunching in multiples of 5,000 dollars.

To empirically determine the location of the cutoff within the interval from 100 to 900 pesos an algorithm was used. The algorithm minimizes the number of observations that violate the rule assumed. The result was a cutoff at 500 pesos. There are 511 observations that violate that rule (equivalent to 1.9% of the sample): 340 have an income below 500 pesos and are non-beneficiaries, and 171 have an income greater than or equal to 500 pesos and are beneficiaries.

For the evaluation, I consider a student to have dropped out by a point in time if she was not enrolled at that moment. Since there is information on enrollment at four points in time, there are dropout measures at four moments: September 2010, March 2011, September 2011, and March 2012. Those moments roughly correspond to 0, 6, 12 and 18 months after starting high school, and I refer to them that way throughout. The definition of dropout is not perfect. In principle, a student not enrolled at some moment can enroll later. Thus, a student might appear as a dropout 6 months after starting high school, but not after 12 or 18 months. At the same time, enrollment alone is not informative of whether the student fell behind or not. Student can stay in school repeating a grade. The results should be interpreted with those caveats.

For illustrative purposes I present figures using the 12-month definition of dropout. Figure 5 shows the dropout rates 12 months after starting high school by household per capita income in 10-peso categories and beneficiary status. The status of an applicant was determined as beneficiary if she received a transfer in September 2010 according to PROBEMS administrative records. The circles correspond to non-beneficiaries and the squares to beneficiaries. The size of the circles and squares represents the number of observations. Several facts are noteworthy. First, bunching takes place in several income categories far from the cutoff. Second, bunching around 500 pesos happens just above the cutoff, at the value that leaves applicants without the benefit. That suggests that if there is strategic behavior, it is not very effective. Third, dropout rates are similar along the interval displayed. That suggests household per capita income probably is a very noisy measure, and the noise results in an apparent flat relationship between income and dropout rates. Fourth, dropout rates are slightly lower for beneficiaries (17.5%) than for non-beneficiaries (19.0%).

Figure 6 shows dropout rates at 12 months according to the last GPA registered in the application and the beneficiary status. GPA is expressed in one-tenth categories (in a 6 to 10 point scale). As before, the size of the circles and squares represents the number of observations. The vertical line shows the discontinuity in the amount of the transfer at 8.0. Among students with a GPA below 8.0, dropout rate is 31.4% for beneficiaries and 35.4% for non-beneficiaries. Among students with a GPA equal to or above 8.0, dropout rate is 12.7% for beneficiaries and 13.8% for non-beneficiaries.

Table 6 shows descriptive statistics for some variables in the sample based on what applicants report in their application questionnaires. Males and females, and beneficiaries and non-beneficiaries, are presented separately. They all come from PROBEMS self-reported non-verified application questionnaires. Among males, dropout rates are lower for beneficiaries. Among females, beneficiaries' and non-beneficiaries' dropout rates are

similar, and in some cases are higher for beneficiaries. On average, beneficiaries have a lower household per capita income, and they live in households with more members. Beneficiaries' GPA is slightly lower and a smaller fraction of them live in a household with a car, washing machine or microwave oven. Statistics in Table 6 suggest that ceteris paribus beneficiaries should have a higher dropout rate because they are poorer.

IV. Empirical strategy

To evaluate the impact of PROBEMS at the extensive margin I use a Regression Discontinuity Design (RDD), and at the intensive margin I use Difference in Differences (DD). The RDD is applied estimating the coefficient on a dummy variable indicating the discontinuity in the beneficiary status that in principle is the result of the cutoff in the socioeconomic index. The DD is applied estimating the coefficient on the interaction of two dummies: one for a GPA greater than or equal to 8.0 and the other for the beneficiary status. In the DD, the first difference is between GPAs around 8.0, and the second difference is between beneficiaries and non-beneficiaries.

The specifications for the estimation can be interpreted as linear probability models. The dependent variable is a dummy that is 0 if the applicant is still in school and 1 if she dropped out. On the right-hand side of the equation is a dummy for the treatment (the beneficiary status) together with the running variable and other controls. At the extensive margin, the impact is estimated with the equation:

$$d_i = \beta X_i + \theta_{j(i)} + \delta x_i + \gamma x_i^2 + \lambda s_i + u_i \qquad (1)$$

Subscripts i and j(i) indicate the student and the school she was at when she applied for the benefit, respectively, d is a dummy indicating whether the student dropped out or not, X is a set of controls (GPA, household and dwelling characteristics, student's age, distance to school, etc.), θ is a school fixed effect, x is the running variable, and s is the treatment (a dummy indicating if the student is a beneficiary). The coefficient λ is the effect of the treatment at the discontinuity.

At the intensive margin, the impact is estimated adding to equation (1) two right-hand side variables: a dummy for a GPA greater than or equal to 8.0, and the interaction of that dummy with the treatment dummy. The specification for the DD estimation is:

$$d_i = \beta X_i + \theta_{j(i)} + \delta x_i + \gamma x_i^2 + \pi s_i + \mu g_i + \rho(s_i \times g_i) + v_i \qquad (2)$$

g is a dummy indicating whether the students' GPA in grades 7 to 9 was greater than or equal to 8.0. In equation (2) the coefficient π is the impact of the baseline transfer and ρ is the impact of the additional amount of the transfer associated with a higher GPA. In other words, ρ is the impact at the intensive margin: the impact attributable to a larger transfer. Thus, for students with a GPA greater than or equal to 8.0 the impact is given by $\pi+\rho$.

Since students respond the questionnaire used to determine the socioeconomic index, they could influence the allocation of the treatment—their answers are not verified. How-

ever, they do not seem to lie flagrantly. As shown in Tables 4 and 6, applicants report incomes greater than zero. They also report having basic appliances and services at home. The important issue for the application of the empirical strategy is whether applicants can manipulate with precision the allocation into the treatment, and that is something they cannot do. The allocation rule is not known with precision because students do not know the yearly appropriations for the program, the number of applicants and the number of beneficiaries grandfathered, nor the socioeconomic index to be used to establish priorities. Given the imprecision with which they can affect it, allocation of the treatment around the cutoff is similar to what would result from randomization (see Lee and Lemieux 2010).

Since in 2009 and 2010 the allocation of the benefit was not the result of a sharp cutoff, I apply the empirical strategy in three different ways. First, I run OLS on the whole sample. Second, I run OLS excluding from the sample those observations that violate the allocation rule based on the cutoff determined empirically. Third, I apply a "fuzzy RDD", i.e. I run instrumental variables using the allocation rule as an instrument for the status of the beneficiary. The allocation rule applied to applicant i can be defined as the variable r_i, which equals 1 if the socioeconomic index of i is below the cutoff and 0 otherwise. Thus, in equation (1) s_i is instrumented with r_i, and in equation (2) s_i and $s_i \times g_i$ are instrumented with r_i and $r_i \times g_i$. The interpretation of the IV approach is that the cutoff does not determine the applicants' status deterministically but probabilistically: the probability of becoming a beneficiary changes discontinuously when crossing the threshold given by the cutoff (see Lee and Lemieux 2010).

For the evaluation I considered that an applicant was "treated" if she received the transfer right after she started 10^{th} grade (the first year of high school). It could well be that some students lose the benefit as they fail courses and fall behind. The evaluation does not draw a distinction between beneficiaries based on what happened after the first year. Among beneficiaries, losing the benefit after having it is endogenous. Therefore, only the status at the start is considered in the analysis.

In terms of the moment when dropout rates are measured, it is expected that the analyses of different moments imply different outcomes. If we look at the effect during the first academic year it might be too soon for differences to arise. However, if we consider the effect at graduation it might be too late because even beneficiaries could have found it optimal not to finish high school. Figure 7 shows two hypothetical paths of dropout rates among two groups of students. Each line represents the fraction the group that remains enrolled at time t. If we consider a point like t_1, the gap between the two groups is still small because even in the groups more prone to drop out, students do not abandon school right away. If we consider a point like t_3 the gap is also small because the dropout rate is also high for those less prone to dropout—sooner or later all "drop out" because nobody stays in school forever. At a point like t_2 the gap would be greater. In practical terms the impact of PROBEMS at one point in time alone can be misleading—false negatives can arise even when there is an impact. It is important to measure the impact on the trajectory. For that reason the evaluation using DGETI data at 0, 6, 12 and 18 months after

starting 10th grade and the data from ENLACE towards the end of 12th grade is not redundant.

A concern when applying an RDD is self-selection or sorting into the treatment. If there were self-selection into the treatment the status as beneficiary would probably be correlated with some characteristics of the student or her household even controlling for the running variable. In order to compute those correlations one can run regressions in which the dependent variable is a characteristic of the student or her household and the explanatory variables are the running variable and the beneficiary status. Tables 7 and 8 show the results of 31 such regressions.

Table 7 corresponds to the applications from 2009. The running variable is the poverty index. In 24 of the 31 regressions the coefficient on the running variable is significant at 95% confidence. However, 12 cases have a potentially worrying sign and 12 cases they have the opposite. For instance, partial correlation between beneficiary status and having a refrigerator or a TV set is positive, whereas the partial correlation with having a microwave oven or a DVD player is negative.

Table 8 corresponds to the applications from 2010. The running variable is household per capita income. In this case only 7 coefficients are significant at 95% confidence and only 2 have a potentially worrying sign. The 2010 allocation process seems to provide a better quasi-experiment, probably due to greater noise in the running variable.

In addition to the controls shown in Tables 7 and 8, as controls in evaluations I include dummies for the materials of the ceiling and the walls (asbestos, cardboard, concrete, brick, etc.), and the floor (tile, cement, dirt, wood, etc.) Those variables are qualitative in nature. They were introduced as controls with a dummy for each category.

In principle, the inclusion of as many controls as available might seem desirable in the RDD. However, it might also create a problem for the estimation of the impact of the program. Household characteristics are dummies and some of them are highly correlated with each other. Their inclusion might be spuriously absorbing the impact of the transfer. In order to avoid that problem one possibility is to restrict the model relying on economic theory. For instance, theory would say that poverty (household per capita income) could have a positive (negative) relation with the probability of dropping out. We could impose that restriction on the model explicitly. Under the assumption that the coefficient on poverty (household per capita income) should have a positive (negative) sign, we could restrict the coefficient to zero and obtain that way a lower bound for the estimate of the impact. Figure 8 presents a graphical description.

Figure 8 assumes an underlying negative relationship between the running variable (income or the inverse of the poverty index) and the probability of dropping out. Applicants with lower income (or in greater need) are the beneficiaries. By omitting the running variable from the regression we restrict its coefficient to be zero. Since the treatment is given to those more likely to dropout, the estimate of the treatment effect at the discontinuity is a lower bound for the true parameter. [18]

[18] If the underlying relationship between income and the probability of dropping out was positive, then the opposite would hold: we would get an upper bound for the effect.

The following section presents the results of several specifications with different sets of controls. The specifications with fewer controls can be interpreted as restricted versions of more general models. Given that in some cases economic theory predicts a signs for the relation between some variables and the probability of dropping out, their omission could be expected to result in lower bounds for the impact.

Among the controls considered are school fixed effects. Figure 9 shows the distribution of students in the sample of applicants from 2009 according to the dropout rate of the school. Figure 10 shows the distribution of students in the sample of applicants from 2010. The two charts show that dropout rates vary across schools. The convenience of including school fixed effects is debatable. Their inclusion should be interpreted as a robustness check.

V. Results

There are several reasons why the results using the applications from 2009 and the data from ENLACE might differ from the results using the applications from 2010 and the data from DGETI. First, the pool of applicants is different since they are enrolled in different schools. The ENLACE data include 2,219 public high schools, among which are the 469 DGETI schools. Second, the criterion to allocate the benefit differed across the two years. In 2009 a poverty index was used to determine the priority of applicants (its calculation did not include household income), whereas in 2010 the priority was determined based on household per capita income alone. Third, the metric of impact is different in the two analyses. ENLACE is a proxy of whether the student was at the same school three years later, without falling behind. DGETI enrollment data indicate whether the student was enrolled in the same school 0, 6, 12 and 18 months after starting high school (10^{th} grade), regardless of whether she fell behind. Fourth, it is possible that self-selection changed as more students became aware of it. As shown in Table 2, from 2009 to 2010 the number of students in high school grew 3.5% whereas the number of applicants grew 26.0%.

Applications from 2009 and data from ENLACE 2012

Table 9 shows the results of the regressions run separately for males and females in three panels. Panel I shows the results of running OLS on the full sample. Panel II shows OLS results excluding the noisy observations (those who apparently violate the allocation rule based on a cutoff for the poverty index at 0.8). Panel III shows the results using the allocation rule as an IV on the full sample. The three panels present specifications with different sets of controls.

Specification A does not include any control and therefore is equivalent to a comparison of means. Specification B includes the running variable (the poverty index) as the only control. Specification C includes the 13 variables used to compute the poverty index as controls. Specification D adds to the set of controls household per capita income and its square. Specification E adds a polynomial of degree three on the GPA. Specification F includes household and dwelling characteristics (running water, building materials, etc.)

as well as the age of the student. Specification G includes school fixed effects. Finally, specification H includes a dummy for a GPA greater than or equal to 8.0, and the interaction of that dummy with the beneficiary status.

In specifications A though G for males the impact estimated by OLS ranges from -5.59 to -0.46 percentage points using the full sample. Excluding the noisy observations the estimates range from -5.47 to -0.29 percentage points. Using IV on the full sample the estimates range between -5.27 and -0.13 percentage points. The results for females are similar: using OLS on the full sample they range between -4.96 and -0.03, excluding the noisy observations they range between -4.99 and -0.20, and using IV on the full sample they range between -5.06 and -0.22. Coefficients in specifications A through E are significant at 95% confidence in one-sided tests. In specifications F and G the coefficients are negative but not significant.

Specification H includes the same controls as G but distinguishes the impact of the greater amount for students with a GPA greater than or equal to 8.0 (the intensive margin). The IV estimation includes two instruments: a dummy for the cutoff and its interaction with the dummy for a GPA greater than or equal to 8.0. [19] In the three panels the estimates are similar: the impact of a greater transfer is not significant for students with a GPA below 8.0, but it is significant for students with a GPA greater than or equal to 8.0. The estimates of the impact for those students (the intensive margin) range between -1.99 and -1.73 percentage points for males, and between -2.13 and -2.11 for females.

Considering as a baseline the dropout rate among non-beneficiaries shown in Table 4 (48.9% for males and 43.8% for females), the estimates in specifications A through G imply a reduction in the number of dropouts ranging from 0.3 to 11.4% for males and from 0.1 to 11.6% for females. For the intensive margin estimates we can use as a baseline the dropout rates among non-beneficiaries with a GPA greater than or equal to 8.0, which are 39.6% for males and 38.9% for females. In that case, the impact at the intensive margin on the number of dropouts is a reduction that ranges between 4.3 and 5.8% for males and between 5.4 and 5.5% for females.[20]

Applications from 2010 and DGETI enrollment records

Tables 10 through 13 present the results using the applications from 2010 and the enrollment data from DGETI schools. Table 10 shows the results using a definition of dropout based on the data on enrollment right after starting 10th grade. Tables 11, 12 and 13 show the results using the data on enrollment 6, 12 and 18 months after starting 10th grade, respectively. The tables have a layout similar to Table 9. Panel I shows OLS results on the full sample. Panel II shows OLS results excluding noisy observations (those who violate

[19] The IV regression includes school fixed effects. For estimation purposes deviations from the mean were considered. In principle, standard errors should be corrected. However, for large samples the correction implies negligible differences. The significance shown is not corrected in panel III specification H.

[20] One could also use as baseline the dropout rates of beneficiaries with a GPA below 8.0, which are 60.8% for males and 62.1% for females. In that case the reduction in the number of dropouts ranges between 2.8 and 3.3% for males and 3.4% for females.

the assumed allocation rule). Panel II shows IV results on the full sample. The instrument is the allocation rule based on a cutoff for the household per capita income at 500 pesos.

Specification A does not include any controls. Specification B only includes as controls household per capita income and its square. Specification C adds a polynomial of degree three on GPA. Specification D adds household and dwelling characteristics together with other student traits. Specification E adds school fixed effects. Finally, specification F includes a dummy for a GPA greater than or equal to 8.0 as well as the interaction of that dummy with the status of the beneficiary.

Results in Table 10 show that there is no significant impact on whether the student was enrolled or not by the start of the 10^{th} grade. Table 11 shows some negative estimates for using enrollment six months later, but they are not significant in all cases. Table 12 shows the results using enrollment 12 months later, that is, at the start of the 11^{th} grade. Point estimates in specification A through E are negative for males and females, but some are not significant.

Point estimates for males in specifications A through E in Table 12 range between -4.39 and -2.61 percentage points using OLS on the full sample, between -3.28 and -2.40 excluding noisy observations, and between -2.71 and -1.59 using IV on the full sample. Estimates for females range between -2.54 and -0.43 using OLS on the full sample, between -2.18 and -0.38 excluding noisy observations, and between -2.22 and -0.35 using IV on the full sample. At the intensive margin, results in specification F show that the increase in the transference associated with a higher GPA does not have much of an impact. The estimates are not significant.

Using as a baseline the dropout rates in Table 6 (21.5% for males and 16.7% for females), the estimates in specifications A through E imply a reduction in the number of dropouts that ranges between 7.4 and 20.4% for males and between 2.1 and 15.2% for females. Specification F—the most flexible—implies a reduction in the number of dropouts between 11.4 and 18.3% for males and between 12.5 and 13.8% for females.

Estimates in Table 13 (18 months after the start of high school) show a weaker impact. Only the OLS specification with the full sample results in statistically significant estimates, but not in all cases. When the results are significant, they are negative. A greater impact at 12 months in comparison to 6 and 18 suggests that non-beneficiaries tend to stay the whole academic year and then not enroll the next.

Dropout rates in Mexico City

An obvious concern regarding the results is the definition of dropout used. It is possible that some students did not abandon their studies but switched schools. The results would be biased if non-beneficiaries were switching school because they did not receive the transfer. In that scenario the impact of the program would be overestimated. In order to explore that possibility we can compare dropout rates in two zones: Mexico City and the rest of the country (excluding the State of Mexico, which neighbors Mexico City and shares the metropolitan area). Public high schools in the metropolitan area of Mexico City (MAMC) participate in a centralized mechanism to allocate students to schools

known as COMIPEMS.[21] Students wanting to enroll at a public high school in the MAMC have to take an exam and list their preferred schools (up to 20). According to their test results and their preferences, students are allocated to a school. Those with the highest scores get their first option. Students with lower scores get their most preferred option subject to availability. Location of the school is not a factor. The relevant part of the COMIPEMS mechanism is that if a student wants to switch to another public high school she has to take the same test again and hope to get a score high enough to end up at her desired school. The exam is given once a year. Thus, the strategy of trying to switch schools in the MAMC is costly. That is why little mobility across schools would be expected. An implication is that those who stop attending a public high school in the MAMC most likely are dropping out and not switching schools. Under that premise we can compare dropout rates in Mexico City and the rest of the country, excluding the State of Mexico.

Table 14 shows the dropout rates in schools located in the Federal District (DF), for beneficiaries and non-beneficiaries. The table also shows the dropout rates for the rest of the country excluding the State of Mexico. Two facts are noteworthy. First, dropout rates are higher in DF. Second, using the applications from 2009 and the ENLACE data, the difference in dropout rates between beneficiaries and non-beneficiaries is larger in absolute value for the rest of the country. If instead we consider the applications from 2010 and the DGETI data at 12 months, the difference in absolute value is larger for DF.

The first fact suggests that dropout rates measured outside of DF do not seem artificially high. If we assume that as a consequence of the COMIPEMS mechanism switching from one public high school to another is uncommon in DF, ceteris paribus we would expect the dropout rates to be higher in the rest of the country. However, we observe the opposite.

The second fact seems to imply that there is heterogeneity in the impact across regions, school systems, and the criteria used in different years. When using the applications from 2009 and the ENLACE data we observe a greater difference in dropout rates between beneficiaries and non-beneficiaries in the rest of the country. At the same time, when using the applications from 2010 and the DGETI data we observe a greater difference in DF.

VI. Discussion of findings

The evaluation of PROBEMS suggests that the program reduces high school dropout rates. The results from two separate evaluations (using different samples, criteria to allocate the benefit and metrics of impact) provide evidence that dropout rates are lower as a result of the CCT. The magnitude of the estimated impact varies across specifications, and in some cases it is not significant. The more flexible specifications (with the most controls) point at reductions attributable to the program in the number of dropouts three year later that range between 4.3 and 5.5% for those with a GPA above 8.0 (applications from

[21] COMIPEMS is the acronym for Metropolitan Commission of Public High Schools.

2009). In the case of the applications from 2010, the reduction in the number of dropouts 12 months after starting high school at a DGETI institution ranges between 11.4 and 18.3%.

It should be borne in mind that the definition of dropout used could be confusing the abandonment of high school with switches to different schools or with the postponement of education. Strictly speaking, the impact should be interpreted as dropout from the schools where they enrolled in 10^{th} grade.

A concern about the results is the quality of the information. First, applicants can lie when they fill out the application questionnaire used to determine the socioeconomic index. Reporting false information in the questionnaire does not seem to be a big problem. A study commended by PROBEMS and carried out by the Autonomous University of the State of Mexico found that the information reported in the questionnaire is reasonably reliable. [22] But even if applicants were lying, only a systematic pattern of lies would bias the results (e.g., if the students less prone to drop out were also the more prone to lie to get the transfer.) However, there are no reasons to believe that lying follows a systematic pattern affecting the estimation.

Second, the information reported by applicants might be imprecise or include measurement error. Applicants might report information without actually knowing the true answers, especially when it comes to the income of all household members. Measurement error might be one of the reasons why there seems to be no relationship between the income and dropout rates at both sides of the discontinuity in Figure 5. That does not demerit the analysis. On the contrary, it might reduce any sorting into the treatment, making the RDD with the applications from 2010 more similar to an experiment.

Third, there is the possibility of fraud. Administrative records could be reported fraudulently by school authorities with the intention of collecting the transfers intended for the students. [23] PROBEMS officials performed an audit and only detected irregularities in administrative issues and not in falsely reporting beneficiaries as students in good standing when they are not. [24]

A relevant question is how the impact of PROBEMS compares to the impact of Oportunidades. The impacts would be expected to differ for four reasons. First, the target population of the programs is different. By definition, Oportunidades' beneficiaries are excluded from PROBEMS. Second, Oportunidades gives more than just cash transfers. It involves interventions in terms of health and it is targeted to all members of the house-

[22] The study consisted of a home visit to 20,751 applicants from 2010. Visitors verified the information on dwelling and household characteristics. Inconsistencies between what the applicant reported and the visitor saw were found in less than 20% of cases.

[23] It is not difficult to imagine a fraud in which, in spite of the beneficiary not being enrolled nor attending school, the principal could confiscate the ATM card, get the PIN from the student, make up grades, and enroll her the next academic year, all just to collect the transfer.

[24] An audit in 2011 consisted of visits to 18 schools in six states (Campeche, Guanajuato, Jalisco, Michoacán, Sonora and Yucatán) to verify the information from 360 beneficiaries. The schools and the beneficiaries were randomly determined. The audit detected anomalies in the information reported on morning versus afternoon attendance, grade, GPA, etc. However, all beneficiaries were found. No evidence on false records making inactive or irregular students appear as regular was found.

hold. PROBEMS only includes a cash transfer to high school students. Third, Oportunidades includes transfers in grades before high school. Thus, its impact at the high school level is conditional on having had the benefit in previous grades. Fourth, PROBEMS gives the transfer directly to the students. The design of Oportunidades and many other CCT programs assumes that who receives the transfer matters. That same argument implies that the effects of PROBEMS and Oportunidades should probably differ.

With the above caveats in mind, impact estimates of Oportunidades for beneficiaries in high school age differ from those of PROBEMS. Berhman et al. (2012, Table 4) estimate the impact on enrollment for girls and boys 15-18 years old (regardless of grade) after one and two years of the program. When they use differences in differences, their estimates are not statistically different from zero. When they use difference in difference propensity score-matching they find a statistically significant impact only for boys after one year, but not after two. Thus, PROBEMS might be having an impact greater than Oportunidades at the high school level.

In 2012 the Mexican government launched a program parallel to PROBEMS that seeks to extend the coverage of CCT under the name Síguele (which can be translated as "Go on.") Síguele has a budget twice that of PROBEMS and is targeted to students that do not receive PROBEMS benefits. The transfers are equal to the PROBEMS base amount: 500 pesos per month. A reasonable prediction is that the average impact of Síguele will be more modest than PROBEMS', for its beneficiaries will have higher household incomes, making the CCT less effective.

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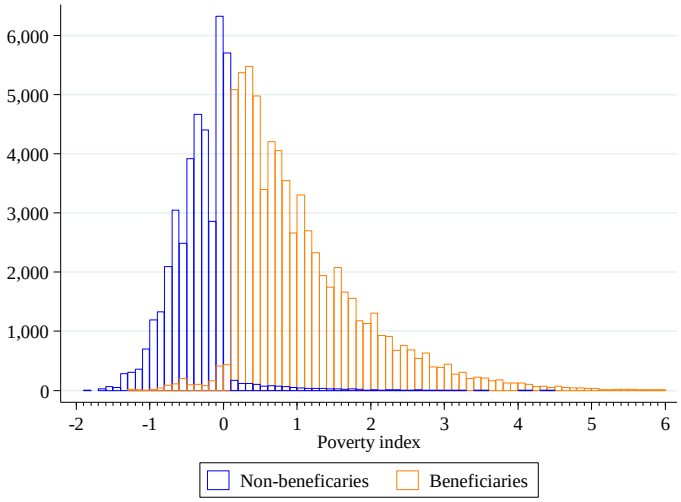
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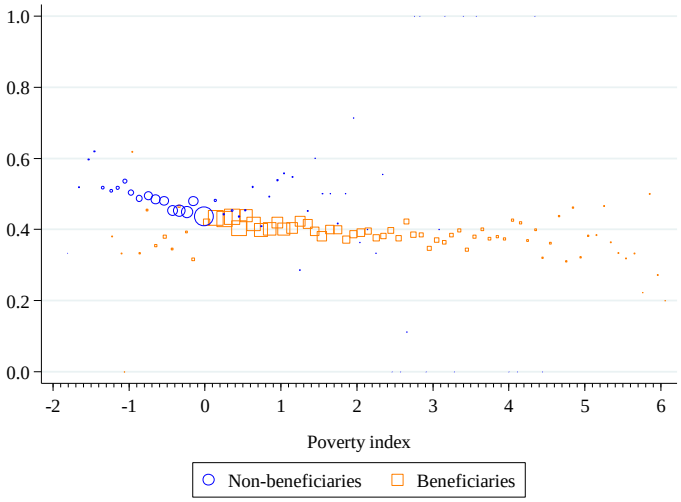
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Figure 1. Distribution of validated applicants from 2009 by poverty index and beneficiary status



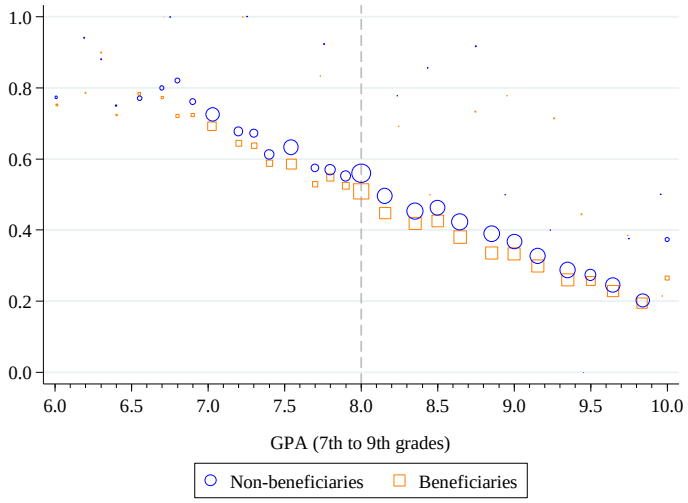
Note: includes 111,590 applicants in 10th grade in high schools with a known school ID and at least one applicant in 10th grade in 2009 with an ENLACE folio number in 2012.

Figure 2. Dropout rates using applications from 2009 and ENLACE data by poverty index and beneficiary status



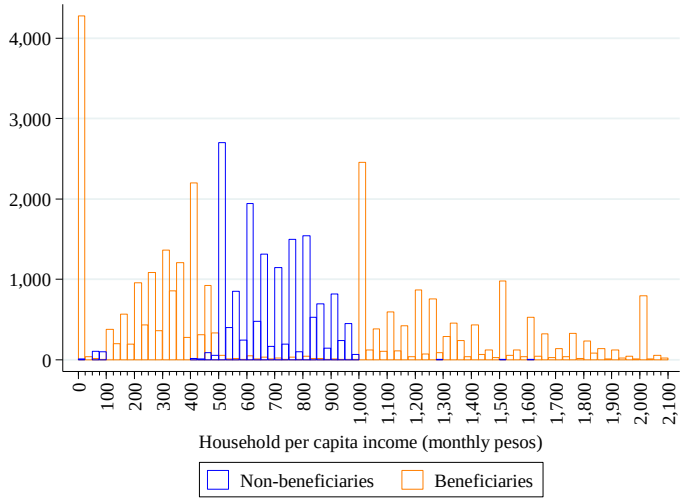
Note: includes 111,590 applicants in 10th grade in high schools with a known school ID and at least one applicant in 10th grade in 2009 with an ENLACE folio number in 2012.

Figure 3. Dropout rates using applications from 2009 and ENLACE data by GPA and beneficiary status



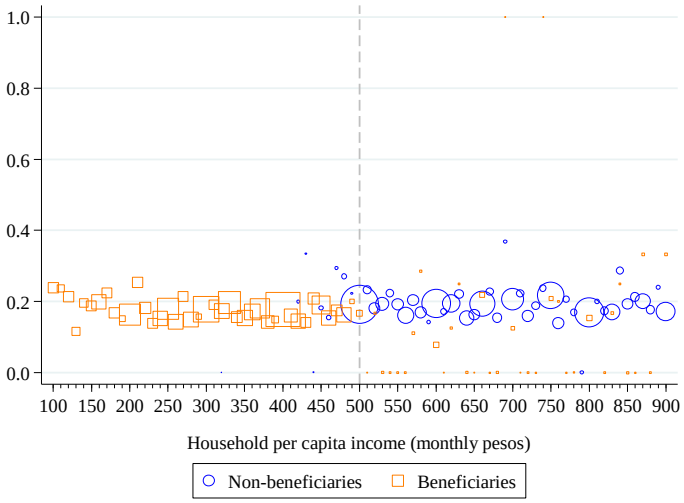
Note: includes 111,518 validated applicants in 10th grade with GPA in high schools with a known school ID and at least one applicant in 10th grade in 2009 with an ENLACE folio number in 2012. GPA in grades 7 to 9.

Figure 4. Distribution of validated applicants from 2010 by household per capita income and beneficiary status



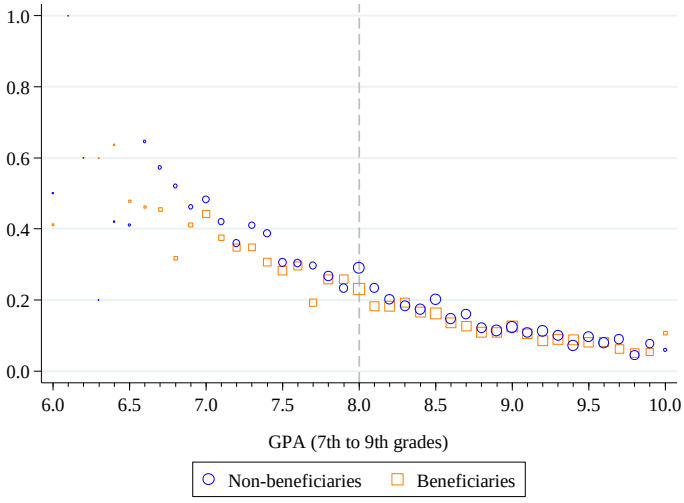
Note: includes 44,275 validated applicants in 10th grade in DGETI schools in 2010.

Figure 5. Dropout rates using applications from 2010 and DGETI data by household per capita income and beneficiary status



Note: includes 26,712 validated applicants in 10th grade in DGETI schools in 2010 with a household per capita income between 100 and 900 pesos per month.

Figure 6. Dropout rates using applications from 2010 and DGETI data by GPA and beneficiary status



Note: includes 26,616 validated applicants with GPA in 10th grade in DGETI schools in 2010 with a household per capita income between 100 and 900 pesos per month. GPA in grades 7 to 9.

Figure 7. Differences in dropout rates are not monotone

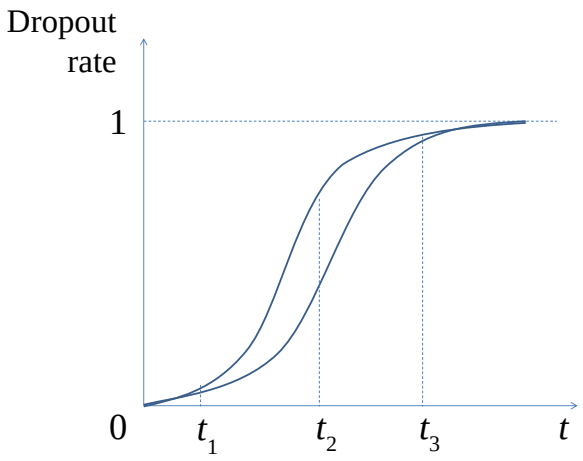


Figure 8. Omission of the running variable with a negative relationship with the outcome and the treatment

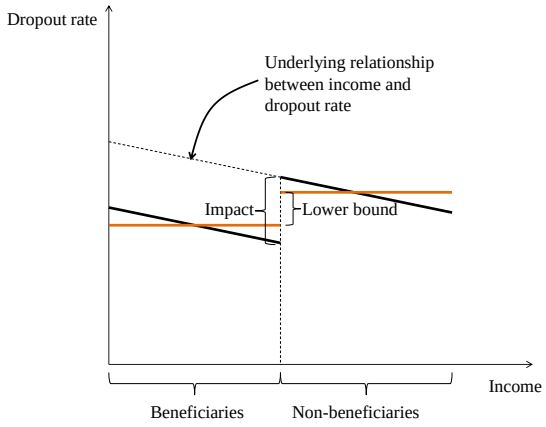
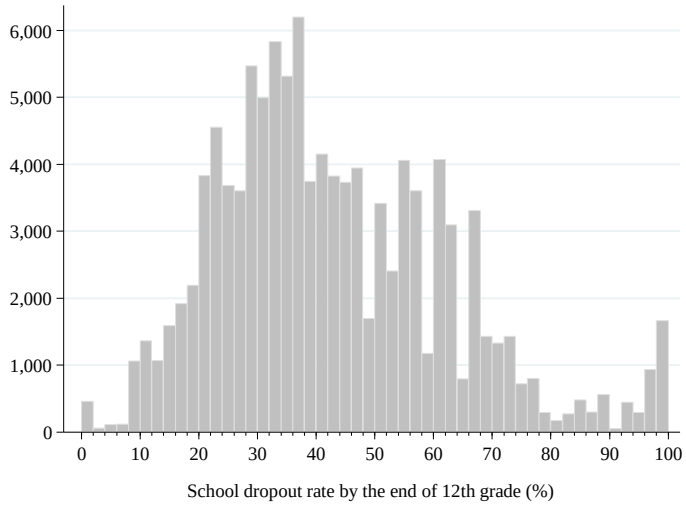
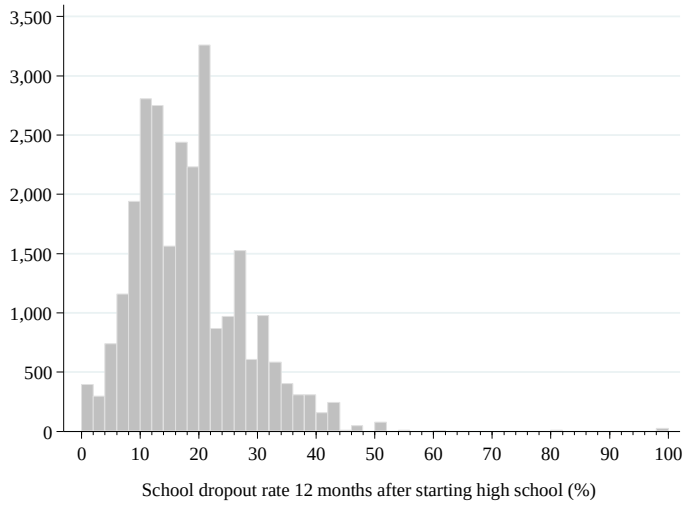


Figure 9. Distribution of applicants from 2009 by dropout rate in their school using ENLACE data



Note: includes 111,590 applicants in 10th grade in 2,219 high schools with a known school ID and at least one applicant in 10th grade in 2009 with an ENLACE folio number in 2012.

Figure 10. Distribution of applicants from 2010 by dropout rate in their school using DGETI data 12 months after starting high school



Note: includes 26,712 validated applicants in 10th grade in 469 DGETI schools in 2010 with a household per capita income between 100 and 900 pesos per month.

Table 1. PROBEMS transfers in 2009 and 2010, monthly pesos

Grade and GPA in previous grade/level	Regular high school		Technological high	
	Males	Females	Males	Females
10^{th} grade				
6.0 a 7.9	500	525	500	525
8.0 a 10.0	625	650	650	700
11^{th} grade				
6.0 a 7.9	525	575	525	575
8.0 a 8.9	650	700	700	750
9.0 a 9.4	850	900	850	900
9.5 a 9.7	900	950	900	950
9.8 a 10.0	950	1000	950	1000
12^{th} grade				
6.0 a 7.9	575	625	575	625
8.0 a 8.9	700	750	750	790
9.0 a 9.4	850	900	850	900
9.5 a 9.7	900	950	900	950
9.8 a 10.0	950	1000	950	1000

Source: Operating Rules of PROBEMS, Diario Oficial de la Federación, December 31, 2008 and December 29, 2009. Transfers are made for 12 months in the academic year through bimonthly deposits.

Table 2. Students in public high school and applicants and beneficiaries of PROBEMS

Year	Students in public high schools (thousands)	Applicants in the year (thousands)	New beneficiaries (thousands)	Total beneficiaries (thousands)	C/B	D/A	$(B+D_{t-1})/A$
	A	B	C	D	E	F	G
2007	3,094	250	250	250	1.00	0.08	0.08
2008	3,198	292	115	352	0.39	0.11	0.17
2009	3,336	339	150	427	0.44	0.13	0.21
2010	3,454	427	187	478	0.44	0.14	0.25
2011	3,543	476	141	449	0.30	0.13	0.27

Source: SEP and PROBEMS.

Table 3. Process to arrive to sample used in the analysis: Applicants from 2009 and ENLACE data

Applicants from 2009	All applicants	Applicants in 10th grade	Applicants in 10th grade in schools with known ID	Applicants in 10th grade in schools with known ID and at least one applicant in 10th grade in 2009 with an ENLACE folio number in 2012
Total	339,486	192,556	176,148	138,994
Validated	234,319	135,869	126,367	111,590
Beneficiaries†	149,668	85,485	79,990	70,584
Rejected because of budget insufficiency ‡	84,651	50,384	46,377	41,006
Rejected by no non-budget reasons	105,167	56,687	49,781	27,404

† Received a transfer in September 2009. ‡ Did not receive a transfer in September 2009.

Table 4. Sample descriptive statistics using applications from 2009 and ENLACE data

Variable	Males		Females	
	Non-beneficiaries N=19,065	Beneficiaries N=32,275	Non-beneficiaries N=21,941	Beneficiaries N=38,309
	Mean (std. dev.)	Mean (std. dev.)	Mean (std. dev.)	Mean (std. dev.)
Dropout rate (%)	48.9 (50.0)	43.3 (49.5)	43.8 (49.6)	38.9 (48.7)
Household per capita income (monthly $)	641 (436)	530 (422)	631 (430)	509 (409)
GPA	8.21 (0.84)	8.33 (0.81)	8.64 (0.78)	8.71 (0.75)
Year of birth	1993.82 (0.93)	1993.86 (0.88)	1993.98 (0.77)	1993.99 (0.76)
Household members	3.59 (1.16)	5.31 (1.62)	3.59 (1.16)	5.40 (1.66)
Minutes to school	24.7 (13.9)	24.7 (14.0)	24.8 (13.9)	24.9 (13.9)
Households with (%):				
Car	24.0 (42.7)	16.4 (37.0)	21.1 (40.8)	15.1 (35.8)
Washing machine	55.1 (49.7)	27.0 (44.4)	51.8 (50.0)	24.7 (43.1)
Microwave oven	40.5 (49.1)	22.4 (41.7)	39.2 (48.8)	21.4 (41.0)

Note: GPA is only recorded for 19,047 male non-beneficiaries, 32,260 male beneficiaries, 21,931 female non-beneficiaries, and 38,280 female beneficiaries.

Table 5. Process to arrive to sample used in the analysis: Applicants from 2010 and DGETI data

Applicants from 2010	All applicants	Applicants in 10th grade	Applicants in 10th grade in DGETI schools	Applicants in 10th grade in DGETI schools with a household per capita income from $100 to $900
Total	426,805	246,634	59,924	30,911
Validated	287,568	174,261	44,275	26,712
Beneficiaries†	186,583	114,204	28,360	11,990
Rejected because of budget insufficiency‡	100,985	60,057	15,915	14,722
Rejected by no non-budget reasons	139,237	72,373	15,649	4,199

† Received a transfer in September 2010. ‡ Did not receive a transfer in September 2010.

Table 6. Sample descriptive statistics using applications from 2010 and DGETI data

Variable	Males		Females	
	Non-beneficiaries N=6,862	Beneficiaries N=5,352	Non-beneficiaries N=7,861	Beneficiaries N=6,637
	Mean (std. dev.)	Mean (std. dev.)	Mean (std. dev.)	Mean (std. dev.)
Dropout rate (%):				
At the start (Sep. 2010)	4.9 (21.6)	4.6 (21.0)	4.7 (21.2)	4.9 (21.5)
6 months later (Mar. 2011)	13.3 (34.0)	11.4 (31.8)	10.0 (30.0)	10.1 (30.1)
12 months later (Sep. 2011)	21.5 (41.1)	18.9 (39.2)	16.7 (37.3)	16.3 (36.9)
18 months later (Mar. 2012)	23.5 (42.4)	22.0 (41.4)	19.2 (39.4)	19.3 (39.4)
Household per capita income (monthly $)	671 (125)	335 (116)	668 (126)	329 (114)
GPA	8.35 (0.86)	8.30 (0.83)	8.76 (0.76)	8.70 (0.78)
Year of birth	1994.99 (0.75)	1994.95 (0.76)	1995.06 (0.65)	1995.04 (0.68)
Household members	3.38 (1.34)	3.87 (1.48)	3.44 (1.36)	3.97 (1.48)
Minutes to school	22.28 (13.2)	22.47 (12.9)	22.13 (13.2)	22.03 (13.0)
Households with (%):				
Car	20.6 (40.4)	16.8 (37.4)	19.3 (39.4)	15.3 (36.0)
Washing machine	43.7 (49.6)	36.5 (48.1)	41.9 (49.3)	35.2 (47.8)
Microwave oven	28.5 (45.1)	21.5 (41.1)	27.4 (44.6)	21.5 (41.1)

Note: GPA is only recorded for 6,829 male non-beneficiaries, 5,333 male beneficiaries, 7,834 female non-beneficiaries, and 6,620 female beneficiaries.

Table 7. Regressions of on running variable and beneficiary status: applications from 2009

Dependent variable	Explanatory variable								R^2
	Poverty index				Beneficiary		Constant		
	Linear		Square						
Last GPA	0.03	***	-0.01	***	0.06	***	8.45	***	0.004
Piped water	-7.30	***	0.14	***	2.74	***	90.93	***	0.041
Water inside the dwelling	-10.86	***	0.38	***	1.86	***	81.86	***	0.054
Bathroom for the family	-6.39	***	0.20	***	1.12	**	79.86	***	0.019
Agua Running water	-13.00	***	0.53	***	1.61	***	76.36	***	0.066
Electricity	-2.03	***	-0.01		1.10	***	97.28	***	0.011
Blender	-8.04	***	0.26	***	2.56	***	88.02	***	0.039
Stove	-15.06	***	0.23	***	7.07	***	93.86	***	0.192
Radio	-2.81	***	0.44	***	-2.17	***	59.56	***	0.004
Sewing machine	-3.12	***	0.38	***	-0.09		13.59	***	0.006
DVD player	-17.17	***	1.98	***	-3.56	***	45.56	***	0.095
Fan	-6.94	***	0.16	**	2.12	***	43.78	***	0.016
AC	-2.19	***	0.14	***	-1.14	***	8.04	***	0.009
Antenna	-3.73	***	0.37	***	-0.80	**	11.83	***	0.013
Motorcycle	-0.28	***	0.01		-0.03		2.09	***	0.000
Pickup truck	-0.94	***	0.02		0.99	***	7.77	***	0.001
Microwave oven	-11.79	***	1.13	***	-3.52	***	35.73	***	0.059
Refrigerator	-15.14	***	0.38	***	3.46	***	92.64	***	0.164
Water heater	-9.73	***	0.74	***	-2.03	***	34.31	***	0.040
Stereo	-8.85	***	0.97	***	-1.56	***	26.39	***	0.034
TV set	-6.17	***	0.00		2.76	***	95.09	***	0.051
Washing machine	-16.69	***	1.90	***	-7.59	***	47.43	***	0.112
Heater	-0.62	***	0.09	***	-0.28	**	1.66	***	0.003
Telephone	-10.90	***	0.62	***	-0.03		50.75	***	0.039
Water tank	-7.59	***	0.56	***	-0.71		45.12	***	0.019
Car	-4.87	***	0.24	***	-0.41		20.82	***	0.014
Tractor	-0.11	*	0.04	***	0.13		0.55	***	0.000
Minutes to school	2.73	***	0.15	*	-2.54	***	43.55	***	0.002
Year of birth	-0.04	***	0.00	**	8.01	***	1993.9	***	0.002
Female	0.82	**	-0.12		-0.17		53.80	***	0.000
Household members	1.21	***	-0.06	***	0.17	***	3.99	***	0.473

P values for two-sided tests: * p<0.05; ** p<0.01; *** p<0.001. N=111,590, except for the GPA regression where N=111,518.

Table 8. Regressions of on running variable and beneficiary status: applications from 2010

Dependent variable	Explanatory variable								R^2
	Household per capita income-500, in 1000s				Beneficiary		Constant		
	Linear		Square						
Last GPA	0.06		-0.03	*	-0.03		8.57	***	0.001
Piped water	11.14	***	-0.89	*	1.12	*	92.43	***	0.005
Water inside the dwelling	15.75	***	-0.41		-0.18		83.50	***	0.008
Bathroom for the family	11.63	***	-0.74		0.13		82.05	***	0.004
Agua Running water	21.22	***	-1.15		-0.11		77.94	***	0.011
Electricity	3.11	***	-0.40	**	0.67	**	98.87	***	0.002
Blender	14.70	***	-1.87	***	0.92		87.38	***	0.006
Stove	9.22	***	-1.57	***	0.13		95.63	***	0.007
Radio	12.42	***	1.05		1.59		49.68	***	0.002
Sewing machine	4.27	*	1.03	*	0.48		10.61	***	0.001
DVD player	22.76	***	2.82	***	0.55		36.03	***	0.010
Fan	15.44	***	-1.06		2.48	*	42.24	***	0.002
AC	6.75	***	0.29		0.29		5.77	***	0.003
Antenna	3.45	*	0.95	*	-1.01		9.34	***	0.002
Motorcycle	0.64		-0.11		0.06		1.89	***	0.000
Pickup truck	3.04	*	-0.24		0.74		7.06	***	0.000
Microwave oven	19.80	***	1.97	**	0.35		23.68	***	0.009
Refrigerator	13.97	***	-1.44	***	0.74		90.99	***	0.008
Water heater	24.18	***	0.30		-0.62		30.20	***	0.013
Stereo	10.98	***	1.48	*	-0.89		19.86	***	0.005
TV set	9.03	***	-0.72	*	0.12		94.57	***	0.006
Washing machine	26.17	***	1.56	*	1.93		37.61	***	0.010
Heater	4.89	***	-0.42	*	2.06	***	0.79	***	0.002
Telephone	21.46	***	0.01		-0.85		53.98	***	0.009
Water tank	6.95	**	1.79	*	-4.61	***	49.70	***	0.006
Car	20.66	***	0.47		3.12	***	16.16	***	0.007
Tractor	-0.64	*	-0.02		-0.20		0.47	***	0.000
Minutes to school	-3.28	**	0.70	*	-1.37	**	30.22	***	0.001
Year of birth	0.13	***	-0.03	**	1.36		1995.0	***	0.001
Female	-8.64	**	0.79		-0.93		54.51	***	0.001
Household members	-1.02	***	0.18	***	0.17	***	3.51	***	0.039

P values for two-sided tests: * p<0.05; ** p<0.01; *** p<0.001. N=26,712, except for the GPA regression where N=26,616.

Table 9. Impact on dropout rates by the third year of high school

Explanatory variable	Dependent variable: 0 if took ENLACE in 2012 at the same school, 100 if not															
	A		B		C		D		E		F		G		H	
I. OLS on full sample																
Males																
Beneficiary	-5.59	***	-3.25	***	-4.03	***	-3.93	***	-1.89	***	-0.46		-0.53		0.63	
GPA ≥ 8.0															2.24	*
GPA ≥ 8.0 × ben.															-1.72	*
R^2	0.003		0.004		0.008		0.008		0.093		0.122		0.251		0.251	
Observations	51,340		51,340		51,340		51,340		51,307		51,307		51,307		51,307	
Females																
Beneficiary	-4.96	***	-2.20	***	-2.97	***	-2.92	***	-0.97	*	-0.03		-0.21		1.57	
GPA ≥ 8.0															0.20	
GPA ≥ 8.0 × ben.															-2.11	*
R^2	0.002		0.003		0.007		0.008		0.081		0.109		0.254		0.254	
Observations	60,250		60,250		60,250		60,250		60,211		60,211		60,211		60,211	
II. OLS excluding noisy observations																
Males																
Beneficiary	-5.47	***	-2.53	***	-3.34	***	-3.24	***	-1.46	**	-0.29		-0.37		0.89	
GPA ≥ 8.0															2.52	*
GPA ≥ 8.0 × ben.															-1.89	*
R^2	0.003		0.004		0.008		0.008		0.093		0.121		0.251		0.252	
Observations	49,975		49,975		49,975		49,975		49,943		49,943		49,943		49,943	
Females																
Beneficiary	-4.99	***	-1.99	***	-2.85	***	-2.77	***	-1.06	*	-0.20		-0.43		1.34	
GPA ≥ 8.0															0.02	
GPA ≥ 8.0 × ben.															-2.09	*
R^2	0.002		0.003		0.007		0.008		0.081		0.110		0.255		0.255	
Observations	58,763		58,763		58,763		58,763		58,726		58,726		58,726		58,726	
III. IV on full sample																
Males																
Beneficiary	-5.27	***	-2.17	***	-3.19	***	-3.09	***	-1.34	*	-0.13		-0.20		1.14	
GPA ≥ 8.0															2.41	*
GPA ≥ 8.0 × ben.															-1.99	*
R^2	0.003		0.004		0.008		0.008		0.093		0.122		0.095		0.095	
Observations	51,340		51,340		51,340		51,340		51,307		51,307		51,307		51,307	
Females																
Beneficiary	-5.06	***	-1.86	***	-2.76	***	-2.69	***	-0.98	*	-0.22		-0.54		1.25	
GPA ≥ 8.0															0.22	
GPA ≥ 8.0 × ben.															-2.13	*
R^2	0.002		0.003		0.007		0.008		0.081		0.109		0.088		0.088	
Observations	60,250		60,250		60,250		60,250		60,211		60,211		60,211		60,211	

P-values for one-sided tests: * p<0.05; ** p<0.01; *** p<0.001. Robust standard errors. A: no controls. B: the only control is the poverty index. C: the only controls are the 13 variables that determine the poverty index. D: in addition to the controls in C, it includes household per capita income (linear and square). E: controls in D plus GPA in grades 7 to 9 (linear, square and cube). F: controls in E plus household/dwelling characteristics. G and H: controls in F plus school fixed effects. In estimates (panel III) in G and H were performed on demeaned data, thus the R-square is not comparable with specifications A to F.

Table 10. Impact on dropout rates using enrollment at the start

Explanatory variable	Dependent variable: 100 if enrolled at the start of 10th grade, 0 if not					
	A	B	C	D	E	F
I. OLS on full sample						
Males						
Beneficiary	-0.30	-0.43	-0.36	-0.44	0.26	0.50
GPA ≥ 8.0						-0.03
GPA ≥ 8.0 × ben.						-0.36
R^2	0.000	0.000	0.001	0.010	0.073	0.073
Observations	12,214	12,214	12,162	12,162	12,162	12,162
Females						
Beneficiary	0.13	-0.27	-0.27	-0.22	0.30	0.85
GPA ≥ 8.0						-0.32
GPA ≥ 8.0 × ben.						-0.65
R^2	0.000	0.000	0.002	0.005	0.062	0.062
Observations	14,498	14,498	14,454	14,454	14,454	14,454
II. OLS excluding noisy observations						
Males						
Beneficiary	-0.15	0.26	0.42	0.34	0.55	0.74
GPA ≥ 8.0						0.02
GPA ≥ 8.0 × ben.						-0.30
R^2	0.000	0.000	0.002	0.010	0.073	0.073
Observations	11,969	11,969	11,917	11,917	11,917	11,917
Females						
Beneficiary	0.20	-0.08	-0.05	0.00	0.09	0.73
GPA ≥ 8.0						-0.46
GPA ≥ 8.0 × ben.						-0.76
R^2	0.000	0.000	0.002	0.006	0.063	0.063
Observations	14,232	14,232	14,188	14,188	14,188	14,188
III. IV on full sample						
Males						
Beneficiary	0.01	0.63	0.80	0.72	0.83	0.95
GPA ≥ 8.0						-0.10
GPA ≥ 8.0 × ben.						-0.18
R^2	0.000	0.000	0.001	0.010	0.073	0.073
Observations	12,214	12,214	12,162	12,162	12,162	12,162
Females						
Beneficiary	0.25	0.03	0.09	0.17	0.14	0.87
GPA ≥ 8.0						-0.21
GPA ≥ 8.0 × ben.						-0.88
R^2	0.000	0.000	0.002	0.005	0.062	0.062
Observations	14,498	14,498	14,454	14,454	14,454	14,454

P-values for one-sided tests: * p<0.05; ** p<0.01; *** p<0.001. Robust standard errors. A: no controls. B: the only control is the household per capita income (linear and square). C: controls in B plus applicant's GPA (linear, square and cube). D: controls in C plus household/dwelling characteristics. E and F: controls in D plus school fixed effects.

Table 11. Impact on dropout rates using enrollment 6 months later

Explanatory variable	Dependent variable: 100 if enrolled 6 months later, 0 if not					
	A	B	C	D	E	F
I. OLS on full sample						
Males						
Beneficiary	-1.94 ***	-3.08 **	-3.21 ***	-3.07 **	-1.91 *	-3.15 *
GPA ≥ 8.0						-1.20
GPA ≥ 8.0 × ben.						1.86
R^2	0.001	0.001	0.049	0.057	0.119	0.119
Observations	12,214	12,214	12,162	12,162	12,162	12,162
Females						
Beneficiary	0.09	-0.02	-0.20	-0.11	0.59	1.07
GPA ≥ 8.0						1.08
GPA ≥ 8.0 × ben.						-0.55
R^2	0.000	0.000	0.033	0.040	0.091	0.091
Observations	14,498	14,498	14,454	14,454	14,454	14,454
II. OLS excluding noisy observations						
Males						
Beneficiary	-1.62 **	-1.70	-1.19	-1.13	-1.22	-2.29
GPA ≥ 8.0						-0.98
GPA ≥ 8.0 × ben.						1.61
R^2	0.001	0.001	0.050	0.058	0.119	0.119
Observations	11,969	11,969	11,917	11,917	11,917	11,917
Females						
Beneficiary	0.07	-0.17	-0.06	0.05	0.10	0.71
GPA ≥ 8.0						1.03
GPA ≥ 8.0 × ben.						-0.72
R^2	0.000	0.000	0.034	0.041	0.092	0.092
Observations	14,232	14,232	14,188	14,188	14,188	14,188
III. IV on full sample						
Males						
Beneficiary	-1.30 *	-1.05	-0.40	-0.34	-0.58	-1.47
GPA ≥ 8.0						-0.94
GPA ≥ 8.0 × ben.						1.35
R^2	0.001	0.001	0.048	0.057	0.118	0.119
Observations	12,214	12,214	12,162	12,162	12,162	12,162
Females						
Beneficiary	0.05	-0.23	0.03	0.21	0.03	0.58
GPA ≥ 8.0						1.12
GPA ≥ 8.0 × ben.						-0.64
R^2	0.000	0.000	0.033	0.040	0.091	0.091
Observations	14,498	14,498	14,454	14,454	14,454	14,454

P-values for one-sided tests: * p<0.05; ** p<0.01; *** p<0.001. Robust standard errors. A: no controls. B: the only control is the household per capita income (linear and square). C: controls in B plus applicant's GPA (linear, square and cube). D: controls in C plus household/dwelling characteristics. E and F: controls in D plus school fixed effects.

Table 12. Impact on dropout rates using enrollment 12 months later

Explanatory variable	Dependent variable: 100 if enrolled 12 months later, 0 if not											
	A		B		C		D		E		F	
I. OLS on full sample												
Males												
Beneficiary	-2.61	***	-3.94	***	-4.39	***	-4.08	***	-3.41	**	-3.94	*
GPA ≥ 8.0											-0.03	
GPA ≥ 8.0 × ben.											0.82	
R^2	0.001		0.001		0.078		0.093		0.156		0.156	
Observations	12,214		12,214		12,162		12,162		12,162		12,162	
Females												
Beneficiary	-0.43		-2.14	*	-2.54	**	-2.32	*	-1.81	*	-2.30	
GPA ≥ 8.0											1.29	
GPA ≥ 8.0 × ben.											0.59	
R^2	0.000		0.000		0.070		0.087		0.139		0.139	
Observations	14,498		14,498		14,454		14,454		14,454		14,454	
II. OLS excluding noisy observations												
Males												
Beneficiary	-2.40	***	-3.28	**	-2.74	*	-2.38	*	-2.78	*	-3.24	*
GPA ≥ 8.0											0.19	
GPA ≥ 8.0 × ben.											0.71	
R^2	0.001		0.001		0.078		0.093		0.157		0.157	
Observations	11,969		11,969		11,917		11,917		11,917		11,917	
Females												
Beneficiary	-0.38		-2.18	*	-2.07	*	-1.82		-1.95	*	-2.30	
GPA ≥ 8.0											1.29	
GPA ≥ 8.0 × ben.											0.42	
R^2	0.000		0.000		0.071		0.087		0.139		0.139	
Observations	14,232		14,232		14,188		14,188		14,188		14,188	
III. IV on full sample												
Males												
Beneficiary	-2.18	**	-2.71	*	-1.94		-1.59		-2.10		-2.45	
GPA ≥ 8.0											0.13	
GPA ≥ 8.0 × ben.											0.54	
R^2	0.001		0.001		0.077		0.092		0.156		0.156	
Observations	12,214		12,214		12,162		12,162		12,162		12,162	
Females												
Beneficiary	-0.35		-2.22	*	-1.83		-1.47		-1.79		-2.08	
GPA ≥ 8.0											1.40	
GPA ≥ 8.0 × ben.											0.36	
R^2	0.000		0.000		0.070		0.087		0.139		0.139	
Observations	14,498		14,498		14,454		14,454		14,454		14,454	

P-values for one-sided tests: * p<0.05; ** p<0.01; *** p<0.001. Robust standard errors. A: no controls. B: the only control is the household per capita income (linear and square). C: controls in B plus applicant's GPA (linear, square and cube). D: controls in C plus household/dwelling characteristics. E and F: controls in D plus school fixed effects.

Table 13. Impact on dropout rates using enrollment 18 months later

Explanatory variable	Dependent variable: 100 if enrolled 18 months later, 0 if not					
	A	B	C	D	E	F
I. OLS on full sample						
Males						
Beneficiary	-1.49 *	-2.68 *	-3.17 **	-2.89 *	-2.06	-1.56
GPA ≥ 8.0						0.96
GPA ≥ 8.0 × ben.						-0.75
R^2	0.000	0.000	0.089	0.111	0.166	0.166
Observations	12,214	12,214	12,162	12,162	12,162	12,162
Females						
Beneficiary	0.09	-1.71	-2.15 *	-2.01 *	-1.26	-1.79
GPA ≥ 8.0						0.02
GPA ≥ 8.0 × ben.						0.63
R^2	0.000	0.000	0.078	0.095	0.144	0.144
Observations	14,498	14,498	14,454	14,454	14,454	14,454
II. OLS excluding noisy observations						
Males						
Beneficiary	-1.25	-1.92	-1.30	-0.87	-1.23	-0.53
GPA ≥ 8.0						1.23
GPA ≥ 8.0 × ben.						-1.04
R^2	0.000	0.000	0.091	0.112	0.167	0.167
Observations	11,969	11,969	11,917	11,917	11,917	11,917
Females						
Beneficiary	0.13	-1.70	-1.56	-1.36	-1.34	-1.64
GPA ≥ 8.0						-0.01
GPA ≥ 8.0 × ben.						0.36
R^2	0.000	0.000	0.079	0.096	0.144	0.144
Observations	14,232	14,232	14,188	14,188	14,188	14,188
III. IV on full sample						
Males						
Beneficiary	-1.00	-1.24	-0.36	0.06	-0.34	0.58
GPA ≥ 8.0						1.29
GPA ≥ 8.0 × ben.						-1.37
R^2	0.000	0.000	0.089	0.110	0.166	0.166
Observations	12,214	12,214	12,162	12,162	12,162	12,162
Females						
Beneficiary	0.14	-1.91	-1.45	-1.15	-1.30	-1.37
GPA ≥ 8.0						0.29
GPA ≥ 8.0 × ben.						0.08
R^2	0.000	0.000	0.078	0.095	0.144	0.144
Observations	14,498	14,498	14,454	14,454	14,454	14,454

P-values for one-sided tests: * p<0.05; ** p<0.01; *** p<0.001. Robust standard errors. A: no controls. B: the only control is the household per capita income (linear and square). C: controls in B plus applicant's GPA (linear, square and cube). D: controls in C plus household/dwelling characteristics. E and F: controls in D plus school fixed effects.

Table 14. Dropout rates in DF and the rest of the country excluding the State of Mexico

	Non-beneficiaries	Beneficiaries	Difference
Applicants from 2009 and ENLACE 2012 data			
Schools in DF			
Dropout rate (%)	62.8	63.1	0.3
Observations	4,888	3,469	
Rest of schools outside of State of Mexico			
Dropout rate (%)	42.5	38.5	-4.0
Observations	28,106	58,307	
Applicants from 2010 and DGETI data			
Schools in DF			
Dropout rate (%)	28.3	22.7	-5.6
Observations	937	547	
Rest of schools outside of State of Mexico			
Dropout rate (%)	17.4	16.7	-0.7
Observations	12,687	10,836	