Universidad de los Andes
Escuela de Gobierno
Alberto Lleras Camargo

No. 50
Octubre 2017
ISSN 2215 – 7816 (En línea)

Documentos de Trabajo

Escuela de Gobierno Alberto Lleras Camargo

Information Policies and Higher Education Choices Experimental Evidence from Colombia

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Serie Documentos de Trabajo 2017

Edición No. 50

ISSN 2215 – 7816 (En línea)

Edición digital

Octubre de 2017

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Information Policies and Higher Education Choices
Experimental Evidence from Colombia*

Leonardo Bonilla† Nicolas L. Bottan‡ Andrés Ham§

Abstract

Governments have recently invested in online information systems that provide labor market statistics and financial aid options to help students make higher education choices. This paper uses a randomized controlled trial to study the extent to which this information influences students' understanding of the potential wage premium associated with various college degrees; performance on tests that are key in college admissions; and subsequent decisions about whether and where to enroll in college. We collect data on more than 6,000 students across 115 public schools in Bogotá, Colombia. Students in 58 schools were given a 35-minute presentation that provides labor market and funding information: average earning premiums upon completing college, available financial aid options to cover costs, and the importance of test scores for admission and financing. Results indicate that students learn about financial aid but do not change their generally inflated beliefs about earnings associated with college degrees. Test scores and college enrollment are unchanged by the treatment, although we find evidence that the intervention leads more students to choose to attend selective colleges.

Key words: information, higher education, schooling demand, Colombia.

Resumen

Varios gobiernos han establecido sistemas de información en línea que proveen estadísticas laborales y opciones de financiaciamiento para ayudar a los alumnos a tomar mejores decisiones sobre educación superior. Este trabajo utiliza un experimento aleatorio para estudiar cuánto dicha información afecta: el conocimiento de los alumnos sobre los beneficios salariales esperados de distintas carreras universitarias; su desempeño en las pruebas SABER 11; y sus decisiones sobre matrícula universitaria. Recolectamos datos sobre más de 6.000 alumnos en 115 colegios distritales en Bogotá, Colombia. En 58 colegios, los alumnos recibieron una charla de 35 minutos con información sobre los beneficios esperados de la educación superior, opciones de financiamiento para pagar la universidad y la importancia de las notas y pruebas para la admisión y acceso a becas. Los resultados indican que los alumnos obtienen mayor conocimiento sobre ayuda financiera, pero no cambian sus percepciones infladas sobre los salarios esperados al obtener un título de educación superior. El tratamiento no afecta el desempeño en las pruebas SABER 11 ni la tasa de matrícula, aunque sí encontramos evidencia que la intervención motiva a los alumnos a inscribirse en instituciones más selectivas.

Palabras clave: información, educación superior, demanda educativa, Colombia.

*We would like to thank the Department of Economics at the University of Illinois for financial support. Special thanks are due to Richard Akresh and Martin Perry for their guidance, advice, and encouragement. We are grateful to the Secretary of Education of Bogotá for its interest in this project and authorization to visit the schools, to ICFES and the Ministry of Education for the exit exam data and enrollment data sets, and to the field work team. Earlier versions benefited from useful comments by Geoffrey Hewings, Dan Bernhardt, Adam Osman, Rebecca Thornton, Marieke Kleemans, Seema Jayachandran, Paul Glewwe, Walter McMahon, Julian Cristia, Felipe Barrera-Osorio, Francisco Gallego, Oscar Mitnik, Alejandro Ganimian, Guillermo Cruces, Cristian Pop-Eleches, and participants at various seminars and conferences. This project was reviewed and approved in advance by the Institutional Review Board for the protection of human subjects of the University of Illinois at Urbana-Champaign (IRB #13570). All remaining errors and omissions are our sole responsibility.

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1 Introduction

Motivated by the increasing demand for higher education and the complexity of the labor market-matching process, in recent years numerous governments have created Labor Market Observatories. These institutions aim to improve the information available to high school students deciding whether to pursue higher education, and for universities determining what programs to offer. Such institutions typically compile and publish online statistics on average starting wages and employment rates obtained from administrative records for recent graduates by college, degree, and field. Similarly, funding programs have made significant efforts to provide more accurate information to students and families through their websites. Despite the recent interest in and resources allocated to implementing these online information systems, relatively little evidence exists regarding the role they play in high school students' college decisions.

In this paper we explore the extent to which this information influences students' knowledge, beliefs and decisions about college, test scores, and the pursuit of higher education. We conduct a randomized controlled trial involving more than 6000 senior high school students, most of them from low-income families, in 115 public schools in Bogotá, Colombia. A month after the beginning of the 2013 school year, young Colombian college graduates gave a 35-minute presentation to 11th-grade students in 58 schools. The presentations covered three main topics: i) average statistics on the earning premiums associated with graduating from college, and mean salary differences for some colleges, degrees, and fields; ii) the availability of student loan programs for financing higher education; and iii) the importance of exit exam scores for college admission and obtaining financial aid. We collect survey data that measure students' knowledge of funding programs, as well as their beliefs on average earnings for different levels of education. The survey data are then matched to administrative records that contain information on high school exit exam scores and college enrollment (i.e., whether the student enrolled in college, their institution, degree, and field).

At baseline, students in our sample are mostly unaware of the existence of the Labor Observatory, and they are misinformed about the average earnings of college graduates. Students tend to overestimate the average returns to four-year college degrees by almost 100%, which is consistent with other studies (e.g., Gamboa and Rodríguez, 2014). Students have greater baseline knowledge of the existence of the main funding program (the national student loan institution - ICETEX, for its acronym in Spanish) than of the city-wide program (FESBO, for its acronym in Spanish). The average effects of the presentation on students' knowledge and earning beliefs, measured five months after the intervention, are modest. Student awareness of the Labor Observatory and the city loan program are unaffected. The treatment does increase their familiarity with the national loan program by around 6.6%. We also find that the intervention does not significantly change earning beliefs, a result that is robust to different definitions of student perceptions.

Matching our survey data to administrative data, we are able to observe students' subsequent performance on the high school exit exam, and their decisions on higher education enrollment. Overall, we do not find statistically significant effects on either test scores or enrollment. Because we have enough statistical power to detect even small effects on knowledge, beliefs, test scores, and higher education choices, we are confident that our estimates capture the average effects of such information on these outcomes. Though our intervention does not increase overall enrollment in higher education, evidence suggests that students who attend the presentations enroll in more selective colleges. Though small in magnitude (between 0.5-0.6 percentage points), this effect is economically significant and robust. It represents an increase of approximately 50% of the control group mean. Furthermore, we report p-values adjusted for multiple hypothesis testing using a Bonferroni correction that accounts for correlation among outcomes in a group (Aker et al., 2012). Therefore, we are confident that our estimates do not capture spurious correlations or may be driven by specification choice.

We also evaluate whether some students benefit from information more than others. Selected attributes include: gender, family income, direction of error in baseline beliefs (underestimating or overestimating), students' perceived academic ranking, perceived self-efficacy, risk aversion, and perceived likelihood of college enrollment. Overall, we do not find evidence of differential effects of the information treatment.

Our study contributes to a growing literature that explores how information affects educational choices. Given the success of similar programs in increasing high school enrollment at very low costs (Nguyen, 2008; Jensen, 2010; Dhaliwal et al., 2011), interest has grown in examining their effectiveness in the transition to higher education. Decisions at this level, however, are more complex. On one hand, college represents a major financial investment, and students have limited information regarding its costs and the funding options that may be available to them. On the other, education premiums vary dramatically by college and degree. Many studies use randomized controlled trials in developing countries where they provide information on funding (Loyalka et al., 2013; Dinkelman and Martínez, 2014), returns to higher education (Rao, 2016), or both (Hastings et al., 2015; Avitabile and De Hoyos Navarro, 2015; Busso et al., 2016). Our paper contributes to this literature by examining the effect of a less targeted intervention, which is better suited to examining the effects of information prepared by Labor Observatories on the population of public high school students. Additionally, our study takes into account not only the effect on student perceptions and test scores, but also the effect on students' actual enrollment in college, and the characteristics of choices they make regarding higher education.

Our findings confirm that even though misinformation is a problem among potential college entrants, information policies do not improve the probability of college enrollment (Pekkala-Kerr et al., 2015; Fryer Jr., 2016; Hastings et al., 2015; Busso et al., 2016). These findings cast doubts on the effectiveness of information policies to increase the demand for college. We do find, however, evidence of positive and significant effects in the intensive margin. That is, better-informed students choose more selective colleges. These results are particularly interesting since the program achieved comparable results to Hastings et al. (2015) and Busso et al. (2016) using a less targeted or personalized program. This suggests that simple communication strategies can be effective —though only a fraction of students may actually benefit from information gains.

The remainder of this paper is organized as follows. Section 2 provides background on Colombia's higher education system. Section 3 outlines the experimental framework and describes the informational intervention based on Labor Observatory data. Section 4 presents the data and empirical strategy. Section 5 reports average and heterogeneous impacts of the intervention. Section 6 concludes by discussing our findings and potential directions for future research.

2 Higher education in Colombia

The Colombian higher education system consists of 327 colleges that differ in the degrees they grant, their administration, and prestige. They offer vocational (two-year) and academic (four-year) degrees across 55 different fields of study. In the Bogotá metropolitan area, vocational degrees are granted in 92 technical/technological institutes, while 40 universities supply most of the academic programs. There are 23 public and 109 private institutions. Six of the 10 most selective universities in Colombia are located in Bogotá.[1]

Each institution has its own admissions criteria. Most colleges use a merit-based system contingent on educational performance and a minimum test score on the SABER 11 high school exit exam, but thresholds vary widely across institutions and fields. The majority of students take the exit exam at least once, though it is not always required to graduate from high school.[2] However, passage of the exam is a requirement for college entry and, as described below, a determining factor for financial aid.

Higher education in Colombia is not free, and tuition costs are markedly different across colleges. On the one hand, students in public institutions pay tuition under a progressive system based on family income. Costs to enroll in public colleges can be as low as 0.1 of the monthly earnings of a worker who received Colombia's monthly minimum wage, about $29 per semester

[1] The best universities in Colombia are ranked based on their students' average performance on university exit exams, the SABER PRO. In 2012, six institutions in Bogotá were in the top-10: Universidad de los Andes, Universidad Nacional, Universidad del Rosario, Universidad Externado, Universidad de la Sabana, and Pontificia Universidad Javeriana. Universidad Nacional is the only public institutions ranked in the top-10.

[2] Students are allowed to retake the SABER 11 exam for a fee of US$21.

(or \$58 per year).[3] On the other hand, tuition costs at top-tier private universities may rise to a level that is 13.2 times the monthly minimum wage, or roughly \$3,800 per semester (\$7,600 a year).

Two funding sources are available to college students in Bogotá. The Colombian Public Student Loans Institution (ICETEX, for its acronym in Spanish) runs the largest (national) student loan program in the country. It funds vocational, academic, and postgraduate studies in Colombia and abroad. Approximately 22% of college students in Bogotá received funding from this source during 2013. Zero-interest loans for low-income students introduced in 2003 have not only increased enrollment rates but also had positive effects on academic performance, dropout rates, and labor outcomes (Melguizo et al., 2016). Bogotá's Secretary of Education offers a second funding option for low-income students educated in the city's public schools: the Fund for Higher Education of Bogotá (FESBO, for its acronym in Spanish). This fund has two financing options. The first targets high-achieving students, and offers loans for degrees in any college, degree, and field. The second provides non-targeted loans to students who pursue vocational degrees. In both cases a fraction of the debt can be forgiven upon degree completion.

To obtain a student loan, students must fulfill several requirements: They must be able to demonstrate that they are Colombian citizens. They must have a letter showing that they have been admitted to an accredited college. And, they must have achieved a minimum threshold score on the SABER 11 high school exit exam.[4] In addition, all applications must be backed by an approved co-signer —a stipulation that is particularly binding for low-income families because a proposed co-signer must pass a credit check, and must provide evidence of sufficient financial capacity to repay the full loan. In this sense, the Colombian system contrasts with Chile, for example, and other countries, where the government often backs student loans without requiring co-signatories.[5]

[3]Hereafter, we express monetary variables in monthly minimum wages, a commonly used measure in Colombia. The 2013 monthly minimum wage was 535,600 Colombian Pesos (roughly 288 US dollars).

[4]Specific requirements on test scores have changed over time, since the national loan program offers different loan types. See http://www.icetex.gov.co/dnnpro5/en-us/cr%C3%A9ditoeducativo/pregrado.aspx for more information.

[5]See González-Velosa et al. (2015) for a detailed comparison of higher education systems in Chile and Colombia.

The average benefits of higher education in the labor market are also heterogeneous. Figure 1 plots mean monthly earnings for college graduates during their first three years after graduating, as well as the interquartile range of these salaries (25th and 75th percentiles). Differences in average salaries and their spread are sizable across colleges, degrees, and fields. For example, mean earnings for recent graduates from public institutions are 2.0 minimum wages versus 2.9 minimum wages for private college graduates. The interquartile range shows that private college graduates in the 25th percentile earn more than public institution graduates at the 75th percentile. On average, earnings are higher for individuals with academic degrees, from top-10 institutions, and fields that are classified as Science, Technology, Engineering and Mathematics (STEM).[6] Earnings inequality is substantial when comparing salaries between prestigious colleges and fields.

Given this heterogeneity in the Colombian higher education system, the government started an online college information site, the Labor Observatory for Education in 2005 (http://www.graduadoscolombia.edu.co). Its mission is to "provide valuable information about the relevance of educational investment and help students make higher education decisions". It is one of the longest running labor observatories in Latin America, pre-dating similar initiatives in Mexico and Chile. The website provides statistics on average starting wages for college graduates, information about how long it takes them to gain employment, and a picture of labor demand patterns across fields and regions. We will study the level of students' awareness of the Labor Observatory, and whether an informational intervention that uses data from this source affects their beliefs, test scores, and higher education choices.

3 Experimental setting

In order to answer our research question, we conduct a randomized controlled trial in Bogotá. Our population of interest is the universe of public school students enrolled in their final year of high school (11th grade). These individuals face a disadvantage in access to higher education relative

[6]STEM fields include agronomy, animal sciences, veterinary medicine, medicine, bacteriology, biology, physics, mathematics, chemistry, geology, business, accounting, economics, and all engineering programs.

to pupils that attend private schools (see Supplementary Material Table A.1). Most students in the public school system come from low socioeconomic backgrounds. They consistently perform worse than their private-school peers on the SABER 11 exit exam, which results in lower levels of college enrollment. Public school students have particularly low representation in prestigious colleges, academic degree programs, and STEM fields.

Using administrative data, we select a representative sample of 120 public school shifts out of the 570 that offer an academic track.[7] These institutions are all mixed-sex high schools with at least 20 students enrolled in 11th grade the year before our intervention. Half of the selected schools are randomly assigned to receive an informational talk detailing earning premiums by college, degree, and field; and discussing funding opportunities based on data from the Labor Observatory. The remaining schools serve as our comparison group. Despite making numerous attempts, we were unable to visit five schools, yielding a final sample of 115 schools of which 57 are assigned to treatment and 58 to control. Overall, schools in our sample cover almost all neighborhoods in Bogotá, with treatment and control schools relatively spread out over the city (see Supplementary Material Figure A.1).

Since participating schools are heterogeneous in size, we interviewed senior students in at most two classrooms per school. In schools with more than two classrooms, we randomly selected two of them to take part in our study. Our sample consists of all students attending school on the days of our visits during the baseline survey.

The timing of our intervention is summarized in Figure 2. In line with the public institutions' academic cycle, which begins in February and ends in December, we arranged our visits at certain key points over the 2013 school year. The baseline survey and the intervention took place during March, about a month after the beginning of the school year. The follow-up visit took place in August, just before students took the high school exit exam. We use administrative data to measure test scores and enrollment outcomes, which are described in the next section.

[7]Most public high schools in Bogotá have two shifts: morning and afternoon. Each shift has different students and, most importantly, different teachers and staff. Hence, each school-shift may be considered as an independent educational institution. In what follows, we refer to school-shifts as schools.

During baseline visits to all schools students took a survey designed for this study. After the surveys were collected, visits concluded in control schools. In each treatment school, young Colombian college graduates delivered a 35-minute presentation to students.[8] The presentation covered three main topics: i) showing average statistics on the earning premiums associated with graduating from college, the mean salary differences between selected colleges, degrees, and fields, and the websites where students could find this information on their own; ii) the availability of student loan programs for financing higher education; and iii) the importance of exit exam scores for college admission and obtaining financial aid.

The first topic began by showing statistics on the average monthly earnings for individuals with incomplete (0.9 minimum wages) and complete secondary (1.1 minimum wages). These values were then compared to the mean monthly salary for individuals who have completed higher education, differentiating by vocational (2.0 minimum wages) and academic degrees (2.9 minimum wages).[9] Next, we introduced students to two websites where they could find very detailed information on the labor market outcomes of college graduates: the Labor Observatory and *Finanzas Personales*.[10] Using these resources, students were shown how to find information on average wages by means of examples (e.g. geographer v. geologist at the same institution, and medicine at different universities), the supply of degrees and fields across institutions, and the average employment probabilities by college, degree, and field.

The second part of the talk focused on the two main funding programs available to students in Bogotá: the national student loan program and the city loan program. For each program, we provided basic information regarding benefits, application requirements, and deadlines. Students were encouraged to visit each program's website to collect more information on their own. We emphasized the fact that college education can be affordable, even if students choose a relatively expensive university.

[8]We opted for local college graduates based on findings in Nguyen (2008), where information provided by role models is shown to be more effective in comparison to information provided by researchers.

[9]Reference earnings for incomplete and complete secondary were estimated from household survey data for 2011.

[10]This site is maintained by *Semana* publications, one of the leading media groups in Colombia. Its information system is based on data from the Labor Observatory presented in a user-friendly way. The page is located at http://www.finanzaspersonales.com.co/calculadoras/articulo/salarios-profesion-para-graduados/45541.

In the final portion of the talk, we highlighted the importance of good performance on the SABER 11 exit exam. As mentioned in the previous section, this test is a determinant factor for admission in most colleges, and certain minimum scores are required to obtain funding. Students were allowed time for questions and were given a one-page handout summarizing the main points of the talk, which also provided links to all websites mentioned during the presentation.[11]

4 Data and estimation strategy

4.1 Data

We employ two sources of data in our analysis: surveys and administrative records. Students in selected schools answered a baseline and follow-up questionnaire. The baseline survey was completed by 6,601 students, and asked about demographics, family background, socioeconomic status, educational history, knowledge and beliefs about the higher education system, aspirations, and attitudes towards risk. The follow-up survey was completed by 5,503 students in the same schools. It followed up on baseline questions about knowledge, beliefs, and aspirations. Given the lower response rates in the follow-up survey, we test for selective attrition. There is no indication that baseline and follow-up respondents differ by treatment status (Supplementary Material Table A.2).[12] Observed attrition is likely due to absences on the day of our second visit because we are able to match most of the baseline sample to administrative data collected at the end of the year.

Next, we match administrative records to our experimental sample in order to measure exit exam performance and college enrollment after the intervention. We use data from the Colombian Institute for the Evaluation of Higher Education (ICFES, for its acronym in Spanish). These data contain students' raw scores on the high school exit exams across eight different subject areas, and for their overall performance. They also gather information on demographics, family background, and socioeconomic attributes. When constructing individual and household-level

[11]The original and translated copy of this handout may be found in the Supplementary Material.

[12]In unreported results, we also compared observable characteristics across respondents and non-respondents and found no significant differences between both samples.

controls we use administrative data, replacing any missing information from our surveys. The matching rate with ICFES data is approximately 95.7%, and shows no significant differences across treatment and control groups (column 2 of Table A.2). Enrollment information is provided by the National Information System for Higher Education (SNIES, for its acronym in Spanish). These data track students in our sample who enrolled in college. The data identify the students' institution, degree, and field. About 95.4% of our baseline sample with valid test scores are matched to SNIES data (column 3 of Table A.2).

In order to explore whether our information treatment affects knowledge and beliefs, test scores, and higher education choices; we focus on three sets of variables:

1. *Knowledge and beliefs*. In both baseline and follow-up surveys, we asked students to indicate whether they were familiar with the Labor Observatory website and available funding programs (national and city) by indicating Yes/No. We construct dummy variables that equal to one if Yes and zero otherwise.

   Next, we sought to understand students' beliefs about expected earnings across different levels of education. Specifically, we asked: "How much do you think the average individual who recently began to work earns per month (in minimum wages) in each of the following situations? a) completes high school but does not go to college, b) completes a vocational degree, and c) completes an academic degree." The options range from one to "10 or more" minimum wages. Using these responses, we construct *perceived earning errors* for vocational and academic degrees as the percentage error in beliefs relative to earning estimates that were provided to students during the talk. These measures are similar to those used in Hastings et al. (2015).

2. *Test scores*. We examine the effect of our intervention on students' overall, math, and language scores (the two most heavily weighted fields).[13] Since our matched data contain

[13]The official weights are: mathematics (3), language (3), social sciences (2), biology (1), physics (1), chemistry (1) and philosophy (1).

raw scores, these values are standardized to have mean zero and standard deviation of one with respect to the control group for ease of interpretation.

3. *Higher education choices*. We first measure enrollment with a dummy variable equal to one if a student enrolled in any higher education program, and zero otherwise (*College Enrollment*). Then, we take advantage of data on the institution, degree, and field of enrollment in order to study whether the intervention influenced program choice. We define four dummy variable outcomes indicating whether the student is pursuing an *Academic Degree*, enrolled in a *Private College*, enrolled in a *Top-10 College*, and enrolled in a program of study classified as a *STEM field*.

Table 1 presents baseline statistics for student attributes, school characteristics and knowledge and belief outcomes across treatment and control schools. The final column presents p-values for the hypothesis that means are equal across groups, which are estimated by OLS clustering standard errors at the school level. Both groups are statistically identical before the intervention, indicating that our randomization was successful. A joint test for the significance of student and school variables on the likelihood of attending a treatment school indicates that they are uncorrelated (p-value of 0.239).

Individuals in our sample are almost 18 years old, and 47.3% are male. Students mainly come from low socioeconomic backgrounds: only 16.5% of their parents have completed college and 68% report that family income lies below two minimum wages ($576 per month). About 17% are employed while attending high school. To measure academic self-concept, we asked students to rank their academic performance relative to their peers on a Likert-scale from 1 to 10 where the latter is the highest value. As a measure of self-efficacy, students rated how often they achieved their goals (from 1 to 10, where 1 is never and 10 is always). Individuals above the median response are classified as having a high level of academic self-concept and self-efficacy, while those below the median constitute the group with low levels of these attributes. Given that risk aversion has been found to play an important role in educational decisions, students were asked to play a game at baseline.[14] The resulting classification indicates that 85% of our sample is “risk averse.” We also asked students about their perceived probability of college enrollment. Over 84% reported that they were likely to enroll.

4.2 Estimation strategy

Given the random assignment of treatment status, we quantify the effect of providing Labor Observatory information to public school students in Bogotá using cross-sectional or difference-in-difference regressions depending on whether outcomes are observed once (as in administrative data) or twice (in our survey data).

For cross-sectional outcomes that are only observed after the intervention (e.g. test scores and higher education choices) we estimate:

$$y_{is,t=1} = \alpha + \beta T_s + \theta X_{is,t=0} + u_{is,t=1} \quad (1)$$

where $y_{is,t=1}$ is the outcome for student i attending school s at the follow-up, $t = 1$. We include an intercept, α, and control for baseline student and household-level attributes (male, age, age squared, family income, and parental education), school characteristics (average scores on the exit exam in previous years, whether the school has a computer lab, shift indicators, and school size), and neighborhood fixed effects in $X_{is,t=0}$. Given that take-up depends on the level of attention placed by students, β captures the intent-to-treat effect of the informational intervention. $u_{is,t=1}$ is a mean-zero error term assumed to be uncorrelated with the treatment indicator since it was randomly assigned. Equation (1) is estimated by Ordinary Least Squares (OLS)[15] with clustered standard errors at the school level.

For outcomes available at both baseline and follow-up (e.g. knowledge and beliefs), we employ two specifications. First, we estimate Equation (1), but also include the baseline outcome

[14]Students face the following hypothetical scenario: They were just hired for a new short-term job and can choose between a fixed salary or a lottery in which earnings are determined by a coin flip. By varying the optimistic scenario payment, we classify students in a scale from one to four where one is extremely risk averse and four is risk loving. We consider students to be risk averse if she is classified one or two.

[15]We also estimate Probit regressions. Because results are largely unchanged, we only show the OLS estimates.

as a control. This ANCOVA approach may provide additional power when autocorrelation in outcomes is low (McKenzie, 2012). Second, we estimate a difference-in-difference specification with student-level fixed effects:

$$y_{ist} = \alpha Post + \beta(T_s \times Post) + \mu_i + u_{ist} \tag{2}$$

where $Post$ is a dummy variable that equals one after information exposure and zero otherwise. α estimates changes in the outcome over time and μ_i is a student-specific effect that controls for time-invariant characteristics. Again, β is our coefficient of interest, which measures the intent-to-treat effect of the information treatment. Standard errors are clustered at the school-level.

Given that we are testing whether the intervention affected multiple outcomes, we adjust p-values for multiple hypothesis testing using a Bonferroni correction that accounts for correlation among outcomes in a group, following Aker et al. (2012).[16] We distinguish between three groups of outcomes when calculating adjusted p-values: knowledge and beliefs (5 outcomes), test scores (3 outcomes), and higher education choices (5 outcomes).

5 Results

5.1 Average effects

We present descriptive statistics for knowledge and beliefs in panels C and D of Table 1. While only 7.7% of students are aware of the existence of the Labor Observatory, funding programs are better known. Almost 70% of students express familiarity with the national loan program and 17.5% with the city loan program. Students in our sample tend to overestimate college earnings. They believe average monthly earnings for vocational and academic graduates exceed observed salaries by about 64% and 95%, respectively. Figure 3 plots the distribution of these errors to study belief dispersion. Individual perceptions are not far from actual vocational earnings, with

[16]The adjusted p-value for the k-th variable in a group of K different outcomes is given by $1-(1-p_k)^{K^{1-r_{-k}}}$ where p is the usual p-value and r_{-k} is the average correlation among all other outcomes excluding k.

76.2% reporting beliefs within one standard deviation of the true salary. Beliefs regarding earnings for those with academic degrees are more dispersed: 43.1% of students are within one standard deviation, 46.2% between one and three standard deviations, and 10.7% more than three standard deviations of the actual salary levels. Knowledge and belief outcomes are balanced at baseline. These results are consistent with evidence for Colombia (Gamboa and Rodríguez, 2014) and other countries (Hastings et al., 2015; Pekkala-Kerr et al., 2015; Avitabile and De Hoyos Navarro, 2015; Busso et al., 2016; McGuigan et al., 2016; Rao, 2016).

The effects of the information treatment on knowledge and beliefs are reported in Table 2. Panel A reports ANCOVA regressions and Panel B presents difference-in-difference estimates with individual fixed-effects. On average, the informational talk did not change student awareness of the Labor Observatory. Results are robust to specification choice and are further confirmed by adjusting p-values for multiple hypothesis testing. The intervention does have a positive and significant effect on student knowledge of the national loan program. Average awareness increases by at least 4.6 percentage points, or 6.6% of the baseline mean. This estimate remains significant even after correcting for multiple testing, with p-values between 0.009 and 0.051. Our estimates also indicate that knowledge of the city loan program was unaffected.

In addition, we find that beliefs about earnings are unchanged for the sample. ANCOVA estimates are close to zero and not statistically significant, while difference-in-difference coefficients are slightly positive but also not significant at any conventional level. In fact, adjusted p-values are close to one, indicating that the information treatment does not change students' expectations about the average earnings for college graduates. Perhaps students are considering as a reference point, their own potential earnings, rather than those of an average individual. To explore this possibility, we compare reported beliefs with salaries for students' aspired careers. In the survey, they were asked to list their ideal college, degree, and field. Using this alternate comparison point, we find similar results (see Supplementary Material Table A.3). We also estimate effects on aspirations using the same criteria (ideal college, degree, and field), and find no impact (Supplementary Material Table A.4).

Next, we explore whether the intervention affected students' performance on the exit exam, and their higher education choices. We present estimates from cross-section regressions for students interviewed at the baseline. These estimates are matched to the administrative records (full sample) and students observed in both in-school surveys who are successfully matched to administrative data (balanced sample).

Students in our sample took the SABER 11 exit exam five months after the intervention. We focus on overall performance and scores on the two most heavily weighted subjects: mathematics and language.[17] Columns 1 to 3 of Table 3 present the average effects of our treatment on standardized test scores. Overall, there is no evidence that students adjust their effort on the exam. The estimated coefficients are consistently positive for mathematics, ranging from 0.045 to 0.065 of a standard deviation, though statistically insignificant (e.g., the adjusted p-value is 0.144 in panel B).

Finally, we examine whether our intervention influenced higher education choices of students. College enrollment rates for our sample are 44%, which includes both vocational and academic programs. The majority pursue vocational degrees (34.6%), and the rest choose academic careers (9.6%). Few public school students attend private institutions (15%) and top-10 colleges (1.1%). Only about 5.2% opt for careers in STEM fields. Columns 4 to 8 of Table 3 show treatment effect estimates for these outcomes. The estimated coefficients for the effect on enrolling in college is zero and precisely estimated for either sample. The estimates for academic degree, private colleges and STEM field are all positive but not statistically significant. However, we do find a positive and statistically significant effect on the likelihood of enrolling in a top-10 college. The estimated effects lie between 0.5 and 0.6 percentage points, depending on the sample, and remain statistically significant at the 10% level after adjusting for multiple testing. Though small in magnitude, this impact is economically significant. It represents an increase of approximately 50% with respect to the control group's average.

[17] Estimated treatment effects for other subjects show no effects.

Overall, our findings suggest that providing high school students with information on higher education has no effects on enrollment, and small positive effects on the intensive margin, where some students enroll in more selective colleges. Even though we find statistically insignificant effects on most other dimensions, it is important to note that our experimental design has enough statistical power to detect small effects. In Supplementary Material Table A.5 we show the minimum detectable effects given our experimental design. For example, we could have detected modest effects of our treatment on knowledge of the Labor Observatory (of 1 percentage point), perceived academic errors (12 percentage points), and enrollment in an academic degree (2 percentage points).

Our results are consistent with other studies. Most work analyzing information treatments finds no effect of disclosing information on college enrollment (Booij et al., 2012; Oreopoulos and Dunn, 2013; Pekkala-Kerr et al., 2015; Wiswall and Zafar, 2015; Fryer Jr., 2016; McGuigan et al., 2016). Intensive-margin effects on college type are comparable to interventions that focus on students who are already applying to college, and who have a high probability of enrollment (Hoxby and Turner, 2013; Hastings et al., 2015). This suggests that the extra cost inherent to a more targeted intervention may not be warranted given our findings. This is important given that targeting high achievers may not be politically feasible in many developing countries, particularly in Latin America, where public support for redistribution to foster social mobility is widespread (Alesina and La Ferrara, 2005). At the same time, opting for a top-10 college may have large implications on long-run earnings (conditional on graduating). Recall from Figure 1 that students who graduate from a top-10 college in Colombia earn significantly more than non-top college students (one minimum wage more on average). Therefore, while simple communication strategies may not lead more individuals to attend college, it does seem to affect the colleges choices of those who do enroll.

5.2 *Heterogeneous effects*

Even though we find modest effects of providing information on average, some students may have benefited more than others from our intervention. We test for differential effects across student- and household-level attributes. These include: gender, family income, direction of error in baseline beliefs (underestimating or overestimating), students' perceived academic ranking, perceived self-efficacy, risk aversion, and perceived likelihood of college enrollment. These results should be interpreted as suggestive, because the data and experiment were not stratified by these characteristics. Despite this limitation, the analysis may provide further insight into whether and how information programs work.

Depending on the outcomes, we estimate cross-section or difference-in-difference regressions that interact a dummy variable for each group with the treatment indicator and all other right-hand side variables. This procedure estimates differential effects for the informational intervention but also allows the coefficients on included controls to vary across groups. Regressions are estimated by OLS with clustered standard errors at the school-level. We adjust all p-values for multiple hypothesis testing using the procedure in Aker et al. (2012), also accounting for the fact that we calculate three coefficients: the reference group treatment effect, an interaction, and their sum.

Table 4 presents estimates for knowledge and belief outcomes by gender, family income, and direction of perceived errors at baseline. Results for knowledge of the Labor Observatory are mostly insignificant and do not differ across groups. Estimated coefficients on familiarity with the national loan program are significant for boys and students who underestimate college returns at baseline, but are not statistically different from effects on girls and students overestimating returns. We find no differential effects on knowledge of the city loan program. Effects on beliefs are mostly insignificant, although we note that students who underestimate at baseline seem to adjust their earning expectations upwards. However, these estimates are imprecise. In the Appendix, we also report effects by self-perception, self-efficacy, risk aversion, and perceived likelihood of enrollment (see Table A.6). Similar to our results above, only coefficients on knowledge of the national loan program are significant. Students with low perceived academic ranking and high self-efficacy report increased awareness of this funding program. However, we cannot reject that these coefficients are different from the estimates for students with high perceived ranking and low self-efficacy, respectively.

We present the differential effects for test scores in the first three columns of Table 5. Overall, we do not find any heterogeneous impact across gender, income, and baseline error direction. There is some suggestive evidence that the treatment improved language scores for students in the high self-efficacy group, and the difference is significant compared to individuals with low perceived self-efficacy (see Table A.7). The analysis finds no other differences across baseline attributes. We also explore potential heterogeneity across the score distribution in test scores by estimating quantile specifications of our cross-sectional regressions. Figure A.2 in the Appendix shows no evidence of heterogeneous impact across the distribution of test scores.

The remainder of Table 5 presents heterogeneous effects for higher education choices. There are no subgroup differences across the selected characteristics, including self-perception and risk aversion (Table A.7). Some coefficients are positive, but most are statistically insignificant. While we found positive and robust average effects on enrollment in a top-10 college, there is no indication from our data that certain students enrolled in these institutions more than others.

Overall, these results suggest that in addition to modest average effects, the information treatment had no differential impact on students.

6 Conclusion

Government efforts have provided access to online information regarding labor market outcomes associated with earning a college degree, and funding options available to students pursuing higher education. This paper evaluates the effects of an information treatment involving face-to-face presentation that provides information from Colombia's Labor Observatory and two programs that provide student loans. We use a randomized controlled trial to assess the effects on high school students' knowledge about the likely earnings premiums associated with various higher education choices, performance on tests that are a key part of the admissions process, and higher education enrollment choices that students ultimately make. We find modest effects of providing information. Students do not increase their knowledge of the Labor Observatory or significantly change their beliefs, but do become more familiar with the national student loan program. However, test scores and college enrollment are unaffected. We also explore the possibility that some students benefit more than others from receiving the information, but we find little evidence to suggest that is the case. Our findings are consistent with other studies that assess similar programs in different settings (Pekkala-Kerr et al., 2015; Fryer Jr., 2016; Hastings et al., 2015; Busso et al., 2016). We interpret these results as evidence that, contrary to the success of information programs in increasing secondary school enrollment (Nguyen, 2008; Jensen, 2010), campaigns to raise awareness for government-maintained information systems do not have significant effects on student test scores or on higher education enrollment.

Beyond the effect on enrollment, our intensive margin estimates suggest that some students do benefit from additional information. In fact, students in treated schools are more likely to enroll in top-10 colleges. Interestingly, these results were obtained with less targeted and personalized programs than Hastings et al. (2015) and Busso et al. (2016), suggesting that simple communication strategies, in the spirit of Jensen (2010), can be effective as well. There are other aspects of the informational campaigns at the higher education level that require further research. For instance, who should be targeted by information policies? What is the relevant information for higher education decisions? When is the best time to disclose this knowledge? We are hopeful that the growing literature to which our study contributes will seek answers to these unanswered questions to help governments improve their information policies, and, ultimately, improve students' educational choices in a way that, facilitates upward social and economic mobility.

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Table 1. Baseline characteristics and outcomes for experimental sample

	Control		Treatment		Difference
	Mean	(SD)	Mean	(SD)	p-value
A. Student attributes					
Male	0.475	(0.499)	0.472	(0.499)	0.831
Age	17.639	(0.925)	17.663	(0.942)	0.504
Parent completed secondary	0.398	(0.489)	0.392	(0.488)	0.719
Parent completed higher education	0.176	(0.381)	0.155	(0.362)	0.270
Family income (<1 minimum wage)	0.136	(0.343)	0.151	(0.358)	0.289
Family income (1-2 minimum wages)	0.538	(0.499)	0.539	(0.499)	0.941
Family income (>2 minimum wages)	0.320	(0.467)	0.307	(0.461)	0.589
Student works	0.164	(0.370)	0.176	(0.381)	0.352
Perceived high academic ranking	0.424	(0.494)	0.395	(0.489)	0.128
Perceived high self-efficacy	0.350	(0.477)	0.355	(0.479)	0.749
Risk averse	0.857	(0.350)	0.845	(0.362)	0.374
Perceived in likelihood of enrollment	0.841	(0.366)	0.844	(0.363)	0.832
B. School characteristics					
Number of students (2010-2012)	95.264	(48.292)	92.349	(31.826)	0.718
SABER 11 score (2010-2012)	0.160	(0.216)	0.118	(0.275)	0.381
Morning shift	0.647	(0.478)	0.625	(0.484)	0.811
Afternoon shift	0.330	(0.470)	0.359	(0.480)	0.748
Single shift	0.023	(0.150)	0.016	(0.125)	0.803
School has computer lab	0.969	(0.173)	0.958	(0.201)	0.749
C. Knowledge					
Knows Labor Observatory	0.072	(0.258)	0.082	(0.274)	0.200
Knows National Loan Program	0.699	(0.459)	0.688	(0.463)	0.646
Knows City Loan Program	0.181	(0.385)	0.168	(0.374)	0.254
D. Perceived earning errors					
Vocational	0.656	(0.978)	0.617	(0.939)	0.308
Academic	0.976	(0.834)	0.923	(0.834)	0.120
Total number of students	3,224			3,377	
Total number of schools	58			57	

Source: Authors' calculations from surveys matched to administrative data.

Notes: Using date of birth, we compute each student's age on December 31, 2013. The number of students is the average number of individuals who sat for the SABER 11 exam in each year from 2010-2012. SABER 11 scores are standardized with respect to each year's national average. The last column presents the p-value of the difference in the attribute between treatment and control groups calculated by regression with clustered standard errors at the school-level. A joint significance test for student and school variables accepts that these characteristics are unable to explain the likelihood of attending a treatment school, with an estimated p-value of 0.239.

Table 2. Average effects on knowledge and beliefs

	Knowledge			Perceived earning errors	
	Labor Observatory	National Loan Prog.	City Loan Prog.	Vocational	Academic
A. ANCOVA					
Treatment	0.008	0.049***	0.016	-0.002	0.001
	(0.007)	(0.016)	(0.012)	(0.027)	(0.029)
Adjusted p-value	0.761	0.009	0.608	1.000	1.000
Observations	5,080	5,365	5,112	5,121	5,169
B. Difference-in-differences					
Treatment × Post	-0.005	0.046**	0.007	0.037	0.035
	(0.010)	(0.018)	(0.014)	(0.038)	(0.035)
Adjusted p-value	0.978	0.051	0.986	0.844	0.832
Observations	10,556	10,861	10,591	10,599	10,656
Baseline mean	0.077	0.694	0.175	0.636	0.949

Source: Authors' calculations from survey data.

Notes: Each column and panel correspond to separate OLS regressions. Panel A presents coefficients of ANCOVA regressions that control for student and household-level attributes (male, age, age squared, family income, and parental education), school characteristics (average scores on exit exam in previous years, has computer lab, shift indicators, and school size), and neighborhood fixed effects. Panel B presents coefficients for difference-in-difference regressions that control for individual fixed-effects. Standard errors in parentheses are clustered at school-level. We report adjusted p-values for multiple hypothesis testing using a Bonferroni correction that accounts for correlation among outcomes in a group (see Section 4.2 for details).

* Significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3. Average effects on test scores and higher education choices

	Test scores			Higher education choices				
	Overall score	Math	Language	College enrollment	Academic degree	Private college	Top-10 college	STEM field
A. Full sample								
Treatment	-0.002	0.045	-0.004	0.004	0.008	0.013	0.005**	0.005
	(0.038)	(0.042)	(0.033)	(0.022)	(0.008)	(0.012)	(0.003)	(0.006)
Adjusted p-value	0.997	0.343	0.952	0.997	0.754	0.593	0.086	0.872
Observations	6,318	6,318	6,318	6,298	6,298	6,298	6,298	6,298
B. Balanced sample								
Treatment	0.019	0.065	0.011	-0.001	0.010	0.012	0.006**	0.006
	(0.039)	(0.041)	(0.035)	(0.023)	(0.008)	(0.013)	(0.003)	(0.006)
Adjusted p-value	0.858	0.144	0.826	1.000	0.601	0.719	0.082	0.779
Observations	5,427	5,427	5,427	5,414	5,414	5,414	5,414	5,414
Mean Control Group				0.438	0.096	0.150	0.011	0.052

Source: Authors' calculations from surveys matched to administrative data.

Notes: Each column and panel correspond to separate OLS regressions that control for student and household-level attributes (male, age, age squared, family income, and parental education), school characteristics (average scores on exit exam in previous years, has computer lab, shift indicators, and school size), and neighborhood fixed effects. Panel A presents results for students interviewed at baseline that are matched to the administrative records (full sample) and Panel B for students observed in both in-school surveys who are matched to administrative data (balanced sample). Standard errors in parentheses are clustered at school-level. We report adjusted p-values for multiple hypothesis testing using a Bonferroni correction that accounts for correlation among outcomes in a group (see Section 4.2 for details).

* Significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4. Heterogeneous effects on knowledge and beliefs

	Knowledge			Perceived earning errors	
	Labor Observatory	National Loan Prog.	City Loan Prog.	Vocational	Academic
A. Gender					
Female	-0.012	0.033	-0.005	0.047	0.068
	(0.013)	(0.023)	(0.019)	(0.053)	(0.046)
Male	0.002	0.060*	0.021	0.025	-0.003
	(0.015)	(0.024)	(0.019)	(0.042)	(0.042)
p-value (Female=Male)	0.998	0.963	0.969	1.000	0.891
Observations	10,556	10,861	10,591	10,599	10,656
B. Family income					
Low (≤2 MW)	-0.003	0.051	0.004	0.020	0.032
	(0.011)	(0.021)	(0.016)	(0.048)	(0.039)
Middle (>2 MW)	-0.009	0.035	0.013	0.073	0.045
	(0.016)	(0.024)	(0.025)	(0.047)	(0.051)
p-value (Low=Middle)	1.000	0.997	1.000	0.996	1.000
Observations	10,556	10,861	10,591	10,599	10,656
C. Perceived earning errors (academic)					
Under or equal	-0.010	0.162**	0.080	0.195	0.119
	(0.040)	(0.051)	(0.048)	(0.100)	(0.090)
Over	-0.006	0.038	0.002	0.025	0.022
	(0.011)	(0.019)	(0.015)	(0.037)	(0.035)
p-value (Under=Over)	1.000	0.141	0.738	0.603	0.978
Observations	10,147	10,422	10,178	10,318	10,417

Source: Authors' calculations from survey data.

Notes: Each column and panel correspond to separate difference-in-difference regressions that interact a dummy variable for each group with the treatment indicator and all controls. Standard errors in parentheses are clustered at school-level. Reported significance levels and p-values are adjusted for multiple hypothesis testing using a Bonferroni correction that accounts for correlation among outcomes in a group (see Section 4.2 for details).
* Significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5. Heterogeneous effects on test scores and higher education choices

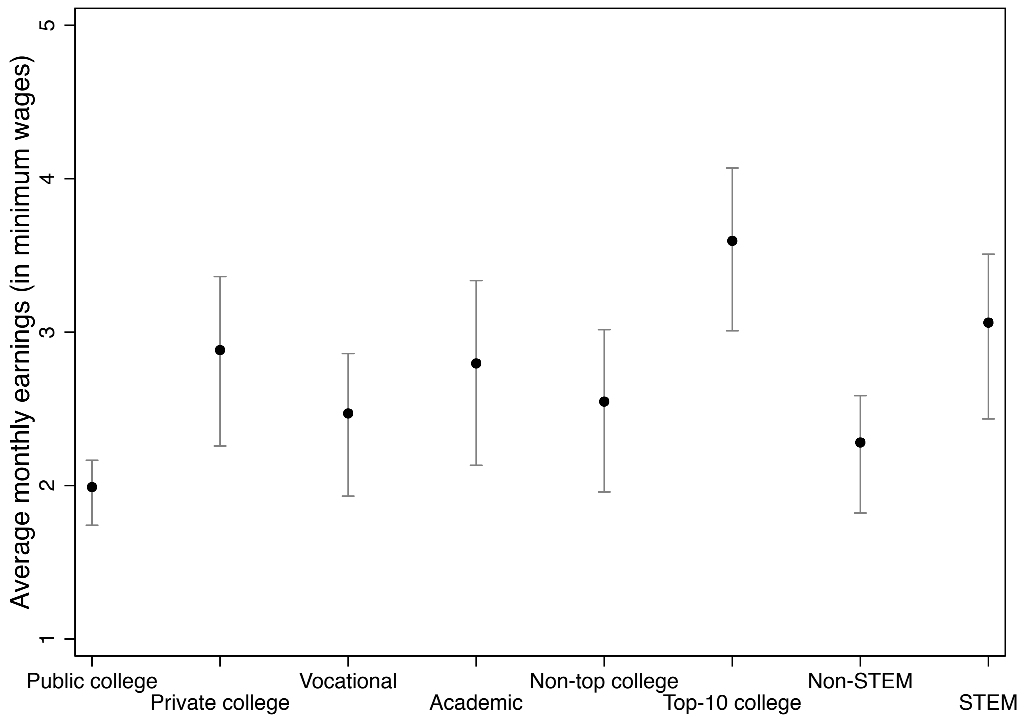
	Test scores			Higher education choices				
	Overall score	Math	Language	College enrollment	Academic degree	Private college	Top-10 college	STEM field
A. Gender								
Female	-0.030	0.029	-0.045	-0.014	0.007	0.004	0.006	0.001
	(0.043)	(0.047)	(0.041)	(0.026)	(0.015)	(0.003)	(0.011)	(0.007)
Male	0.030	0.063	0.043	0.025	0.021	0.007	0.011	0.008
	(0.048)	(0.050)	(0.041)	(0.024)	(0.014)	(0.004)	(0.013)	(0.010)
p-value (Female=Male)	0.632	0.677	0.133	0.659	0.961	0.991	1.000	0.998
Observations	6,318	6,318	6,318	6,298	6,298	6,298	6,298	6,298
B. Family income								
Low ($\leq$2 MW)	-0.022	0.022	-0.017	0.006	0.024	0.004	0.013	0.005
	(0.042)	(0.043)	(0.039)	(0.023)	(0.011)	(0.003)	(0.009)	(0.007)
Middle ($>$2 MW)	0.042	0.096	0.026	0.000	-0.010	0.009	-0.002	0.004
	(0.049)	(0.055)	(0.046)	(0.027)	(0.021)	(0.006)	(0.016)	(0.013)
p-value (Low=Middle)	0.541	0.221	0.591	1.000	0.492	0.974	0.986	1.000
Observations	6,318	6,318	6,318	6,298	6,298	6,298	6,298	6,298
C. Perceived earning errors (academic)								
Under or equal	0.004	0.081	0.033	0.056	0.027	-0.001	0.030	0.032
	(0.099)	(0.099)	(0.094)	(0.045)	(0.034)	(0.008)	(0.033)	(0.018)
Over	-0.008	0.043	-0.011	-0.006	0.010	0.006	0.004	0.002
	(0.037)	(0.041)	(0.034)	(0.022)	(0.013)	(0.003)	(0.009)	(0.007)
p-value (Under=Over)	1.000	0.853	0.826	0.636	0.998	0.959	0.994	0.620
Observations	6,021	6,021	6,021	6,003	6,003	6,003	6,003	6,003

Source: Authors' calculations from surveys matched to administrative data.

Notes: Each column and panel correspond to separate OLS regressions that interact a dummy variable for each group with the treatment indicator and all controls. Standard errors in parentheses are clustered at school-level. Reported significance levels and p-values are adjusted for multiple hypothesis testing using a Bonferroni correction that accounts for correlation among outcomes in a group (see Section 4.2 for details).

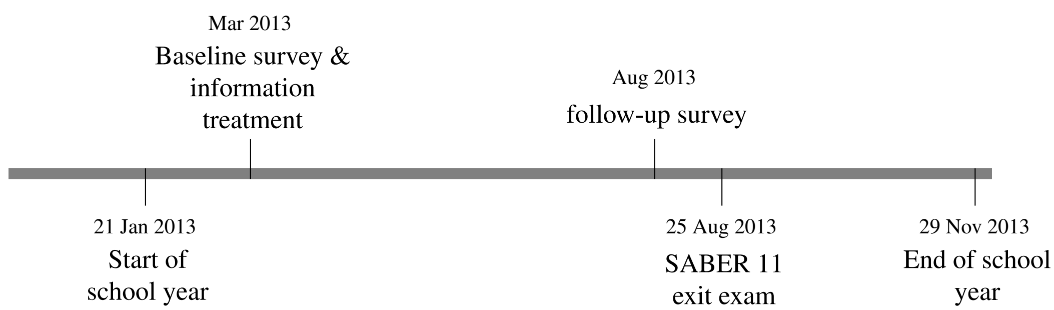
* Significant at 10%; ** significant at 5%; *** significant at 1%.

Figure 1. Average monthly earnings of recent college graduates



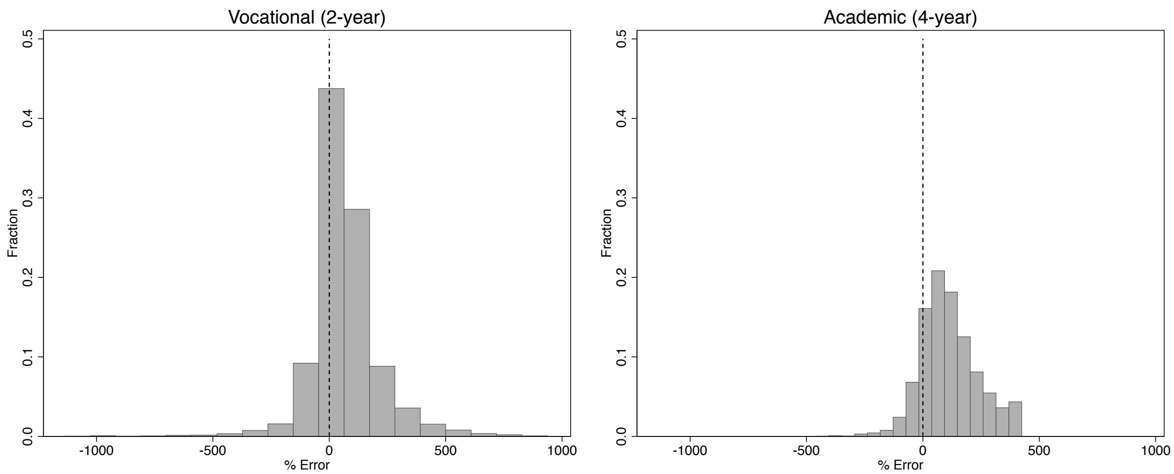
Source: Authors' elaboration from Labor Observatory data.
Notes: Monthly earnings are expressed in minimum wages (1 minimum wage ≈ $288 US dollars), and correspond to the average entry-level salaries for recent graduates by college, level, and field (in the first three years). The lower and upper bounds identify the 25th and 75th percentiles.

Figure 2. Intervention timing and primary data collection



Source: Authors' elaboration.

Figure 3. Distribution of perceived earning errors at baseline



Source: Authors' elaboration from survey data.
Notes: We calculate perceived earning errors as the difference between perceived and actual earnings divided by actual earnings. Let y^j denote earnings, with $j = \{\text{actual,perceived}\}$. Errors are calculated as $(y^{perceived} - y^{actual})/y^{actual}$.

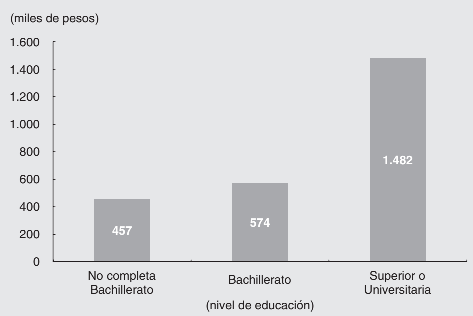
A Supplementary Material

Student Handout

¡La educación superior paga!

La relación entre estudios e ingresos

La educación superior es un factor determinante de la situación económica y por tanto la calidad de vida de las familias. En el siguiente gráfico se presentan los salarios promedio por nivel educativo en Bogotá.



Fuente: Encuesta de Hogares 2011, DANE

Como se puede observar, mayor educación se traduce en salarios más altos. Sólo con terminar el Bachillerato se pasa de ganar 457.000 a 574.000 por mes. El salto es más evidente para aquellos con un título de nivel superior, ya que el salario promedio mensual crece a 1.482.000. Estas estadísticas presentan un mensaje claro: vale la pena estudiar.

¿Cómo puedo averiguar cuanto ganaría en la carrera que a mí me interesa?

Es probable que usted ya tenga una idea sobre las carreras que le interesarían y la institución donde quisiera realizar estos estudios. Si es así, ¿hay alguna manera de saber cuánto puede esperar ganar en su situación específica?

Existen dos lugares donde pueden consultar el salario promedio de los graduados por institución y carreras. Estas son:

1. Calculadora de salarios promedios para graduados: www.finanzaspersonales.com.co

Esta página cuenta con una herramienta que le permite consultar el salario promedio por región, institución educativa, programa de estudio y género de las personas que obtuvieron su título entre 2001-2011.

¿Cómo funciona?

- Acceda al enlace y busque la *Calculadora de Salario por profesión para Graduados*
- Escoja la región donde quiere realizar la búsqueda (por ejemplo, Bogotá)
- Seleccione la institución donde quiere realizar sus estudios y el programa que planea cursar

2. Observatorio laboral del Ministerio de Educación: www.graduadoscolombia.edu.co

Está página también provee información sobre los salarios promedios de personas con título de educación superior para toda Colombia. Además, le permite conocer las perspectivas laborales del programa de estudio de su interés.

¿Cómo funciona?

- Acceda al enlace y busque el botón rojo que dice *Sistema de información del Observatorio Laboral*.
- Si quiere conocer el número de graduados por carrera, acceda a la pestaña que dice "Perfil nacional". Después, escoja el departamento donde planea estudiar y obtendrá los datos de graduados por área de estudio.

Si desea saber cuántos individuos en su área de interés tienen un empleo formal (cotizando a la seguridad social) y cuanto ganan en promedio vaya a ¨Vinculación laboral recién graduados¨. Aquí tiene la opción de buscar por institución o por carrera.

Recuerde que estas páginas le permiten conocer el salario promedio de los profesionales graduados en su área de interés.

¿Qué necesito para entrar a la Universidad y la carrera que me interesa?

1. Buenos resultados académicos: Uno de los criterios más importantes a la hora de buscar admisión a una institución de educación superior es el rendimiento académico. Muchas instituciones utilizan el puntaje del ICFES (SABER 11), y otras instituciones como la Universidad Nacional que tienen su propio examen de admisión. En cualquier caso, estudiar aumenta las posibilidades de ser admitido y también las posibilidades de acceder a becas o financiación.

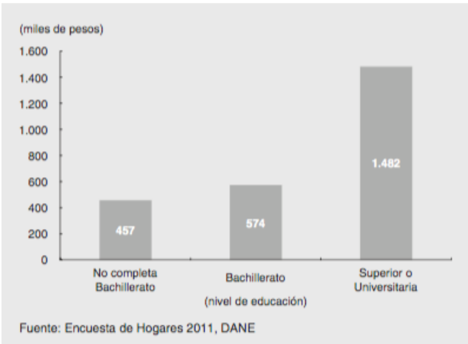
2. Financiación: Existen varias maneras de financiar la educación superior en Colombia. En general, tendrán preferencia los alumnos de escasos recursos y buen desempeño académico. Las siguientes son algunas opciones a tener en cuenta:

- Becas proveídas por cada institución por mérito académico y/o escasos recursos. Consulte las políticas de beca ya que estas son diferentes para cada institución.
- ICETEX: http://www.icetex.gov.co
- Secretaría de Educación de Bogotá (Banco de cupos, Fondo de Financiamiento de Educación Superior de Bogotá): http://www.sedbogota.edu.co/index.php/educacion-superior.html

Post-secondary education pays!

The relation between studies and income

Higher education is a determining factor of wages and the quality of life of families. The following figure presents average wages by level of completed education in Bogotá:



Clearly, more education is related with higher wages. By only finishing high school, wages move from 457,000 to 574,000 pesos each month. The difference is even more marked for those with a college degree, since their average monthly wage increases to 1,492,000. These statistics present a clear pattern: studying is worth it.

How can I learn about how much people earn who finished the degree I'm interested in?

It is very likely that you already have a good idea about the degrees and institutions where you would like to pursue your studies. If this is true, is there a way to know how much I could expect to earn?

There are two places where you can obtain information on average wages for graduates by institution and degree. These are:

1. Average wage calculator for graduates: www.finanzaspersonales.com.co

This website counts with a tool that allows to calculate average wages by region, institution, degree and gender of people who graduated between 2001 and 2011.

How does it work?

- Visit the website and search for *Wage calculator by degree for Graduates.*
- Select the region where you are interested in searching (e.g. Bogotá)
- Select the institution and the degree you are interested in evaluating

2. Labor Observatory of the Ministry of Education: www.graduadoscolombia.edu.co

This website also provides information about average wages for the whole country. Additionally, you can learn about the labor prospects for your degree of interest

How does it work?

- Visit the website and click on the red button reading *Information System of the Labor Observatory*
- If you would like to know the number of graduates by degree, click on the "National Profile" tab. Next, select the department where you plan to study and you will find data on graduates by degree.

If you are interested in the number of individuals who pursued your degree of interest who have a formal job (paying social security) and how much they earn on average, select "*labor link of recent graduates"*. Here you have the option to search by institution and degree.

Remember that these websites allow to learn about the average wages of recent graduates for your degree of interest.

What will I need to enroll in a University and in my degree of interest?

1. **Good academic results**: One of the main criteria for admissions in University if academic performance. Many institutions use the ICFES (SABER 11) score, and other institutions like the National University also have their own admissions test. Nevertheless, studying will increase the probability of being admitted and also of obtaining financial aid or financing.
2. **Financing:** There are many ways to finance higher education in Colombia. In general, financing institutions have preferences for students of low income and good academic performance. The following are some organizations to keep in mind:

- Scholarships provided by each institution according to academic merit of financial need. Consult the scholarship policies for each institution given that they may differ.
- ICETEX: http://www.icetex.gov.co
- Secretary of Education in Bogotá (FDFESBO): http://www.sedbogota.edu.co/index.php/ed- ucacion-superior.html

Table A.1. Descriptive statistics for universe of students in Bogotá, by public and private schools

	Public schools		Private schools		Difference
	Mean	(SD)	Mean	(SD)	p-value
A. Student attributes					
Male	0.458	(0.498)	0.492	(0.500)	0.007
Age	17.641	(0.873)	17.648	(0.907)	0.825
Parent completed secondary	0.395	(0.489)	0.288	(0.453)	0.000
Parent completed higher education	0.156	(0.363)	0.580	(0.494)	0.000
Family income ($<$1 minimum wage)	0.144	(0.351)	0.028	(0.165)	0.000
Family income (1-2 minimum wages)	0.559	(0.497)	0.246	(0.431)	0.000
Family income ($>$2 minimum wages)	0.297	(0.457)	0.726	(0.446)	0.000
B. SABER 11 exit exam					
Overall Score	0.138	(0.841)	0.864	(1.192)	0.000
Math	0.046	(0.884)	0.708	(1.231)	0.000
Language	0.156	(0.870)	0.702	(1.060)	0.000
C. Higher education choices					
Enrolled	0.426	(0.495)	0.571	(0.495)	0.000
Academic degree (4-year)	0.098	(0.298)	0.370	(0.483)	0.000
Vocational degree (2-year)	0.328	(0.469)	0.201	(0.400)	0.000
Public College	0.278	(0.448)	0.147	(0.354)	0.000
Private College	0.148	(0.355)	0.424	(0.494)	0.000
Top-10 College	0.011	(0.106)	0.160	(0.366)	0.000
STEM field	0.054	(0.227)	0.211	(0.408)	0.000
Total number of students	37,787		37,068		
Total number of schools	570		790		

Source: Authors' calculations from administrative data.

Notes: These statistics include the universe of public and private schools offering an academic track. SABER 11 exam scores are standardized with respect to the national average. The last column presents the p-value for a difference in means test between public and private schools.

Table A.2. Attrition diagnostics

	Surveys: Baseline to Follow-Up	Baseline survey to ICFES	Baseline survey to ICFES-SNIES
	(1)	(2)	(3)
A. Attrition Rates			
Baseline N	6,601	6,601	6,601
Final N	5,503	6,323	6,303
Attrition Rate	0.166	0.043	0.046
B. Random attrition tests (OLS)			
Treatment	0.015	-0.012	-0.012
	(0.027)	(0.013)	(0.014)

Source: Authors' calculations from surveys matched to administrative data.
Notes: Standard errors in parentheses are clustered at the school-level.
* Significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.3. Average effects on perceived earning errors with alternative reference point

	Vocational		Academic	
Reference earnings by:	College, degree & field	Public/private college, degree & field	College, degree & field	Public/private college, degree & field
A. ANCOVA				
Treatment	0.009	-0.001	0.010	-0.010
	(0.024)	(0.023)	(0.038)	(0.037)
Adjusted p-value	0.829	0.989	0.884	0.893
Observations	2,782	3,972	2,802	4,009
B. Difference-in-differences				
Treatment × Post	0.033	0.039	0.038	0.049
	(0.029)	(0.028)	(0.040)	(0.040)
Adjusted p-value	0.356	0.228	0.444	0.297
Observations	5,691	8,152	5,715	8,196
Baseline mean	0.096	0.217	0.944	1.147

Source: Authors' calculations from survey data.

Notes: Each column and panel correspond to separate OLS regressions. Panel A presents coefficients of ANCOVA regressions that control for student and household-level attributes (male, age, age squared, family income, and parental education), school characteristics (average scores on exit exam in previous years, has computer lab, shift indicators, and school size), and neighborhood fixed effects. Panel B presents coefficients for difference-in-difference regressions that control for individual fixed-effects. Standard errors in parentheses are clustered at school-level. We report adjusted p-values for multiple hypothesis testing using a Bonferroni correction that accounts for correlation among outcomes in a group (see Section 4.2 for details).

* Significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.4. Average effects on educational aspirations

	Aspirations				
	College enrollment	Academic degree	Private college	Top-10 college	STEM field
A. ANCOVA					
Treatment	0.002	0.010	0.004	0.010	0.015
	(0.003)	(0.017)	(0.020)	(0.013)	(0.013)
Adjusted p-value	0.982	0.967	1.000	0.909	0.757
Observations	5,503	5,503	5,503	5,503	5,503
B. Difference-in-differences					
Treatment × Post	-0.001	0.003	-0.004	0.004	0.007
	(0.004)	(0.016)	(0.023)	(0.013)	(0.014)
Adjusted p-value	1.000	1.000	1.000	0.997	0.983
Observations	11,006	11,006	11,006	11,006	11,006
Baseline mean	0.983	0.228	0.449	0.877	0.410

Source: Authors' calculations from survey data.

Notes: Each column and panel correspond to separate OLS regressions. Panel A presents coefficients of ANCOVA regressions that control for student and household-level attributes (male, age, age squared, family income, and parental education), school characteristics (average scores on exit exam in previous years, has computer lab, shift indicators, and school size), and neighborhood fixed effects. Panel B presents coefficients for difference-in-difference regressions that control for individual fixed-effects. Standard errors in parentheses are clustered at school-level. We report adjusted p-values for multiple hypothesis testing using a Bonferroni correction that accounts for correlation among outcomes in a group (see Section 4.2 for details). * Significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.5. Minimum detectable effects

Outcome	MDE in standard deviations	MDE in percentage points
Knowledge		
Knows Labor Observatory	0.0520	0.0138
Knows National Loan Program	0.1219	0.0562
Knows City Loan Program	0.0535	0.0203
Perceived earnings error		
Vocational	0.0955	0.1441
Academic	0.1019	0.1272
Test scores		
Overall score	0.1981	
Math	0.2154	
Language	0.1869	
Higher education choices		
College enrollment	0.1043	0.0518
Academic degree	0.0953	0.0281
Private college	0.0888	0.0317
Top-10 college	0.0389	0.0040
STEM field	0.0690	0.0153

Source: Author's calculations from survey and administrative data.

Notes: These calculations follow Duflo et al. (2008). We assume 50 students per school (6000/115), calculate intra-cluster correlations from the data and set the test level at 0.10 and statistical power at 0.80.

Table A.6. Heterogeneous effects on knowledge and beliefs, perceptions and risk aversion

	Knowledge			Perceived earnings error	
	Labor Observatory	National Loan Prog.	City Loan Prog.	Vocational	Academic
A. Perceived academic ranking					
Low	0.000	0.065**	0.008	0.042	0.002
	(0.012)	(0.022)	(0.019)	(0.045)	(0.039)
High	-0.016	0.016	0.002	0.029	0.077
	(0.015)	(0.024)	(0.021)	(0.051)	(0.049)
p-value (Low=High)	0.986	0.520	1.000	1.000	0.826
Observations	10,480	10,780	10,514	10,524	10,578
B. Perceived self-efficacy					
Low	0.004	0.024	0.015	0.041	0.009
	(0.011)	(0.022)	(0.017)	(0.044)	(0.040)
High	-0.025	0.083***	-0.013	0.027	0.076
	(0.016)	(0.024)	(0.024)	(0.058)	(0.053)
p-value (Low=High)	0.697	0.230	0.977	1.000	0.958
Observations	10,473	10,773	10,504	10,514	10,571
C. Risk aversion					
Low	-0.048	0.047	0.085	0.013	-0.072
	(0.028)	(0.042)	(0.037)	(0.102)	(0.078)
High	0.004	0.047	-0.002	0.031	0.043
	(0.010)	(0.019)	(0.015)	(0.041)	(0.037)
p-value (Low=High)	0.529	1.000	0.221	1.000	0.858
Observations	10,194	10,487	10,229	10,248	10,300
D. Perceived likelihood of enrollment					
Low	-0.030	0.098	-0.009	0.112	0.112
	(0.022)	(0.039)	(0.039)	(0.095)	(0.083)
High	0.002	0.038	0.010	0.031	0.024
	(0.011)	(0.020)	(0.016)	(0.039)	(0.038)
p-value (Low=High)	0.854	0.720	1.000	0.997	0.985
Observations	10,083	10,372	10,118	10,137	10,196

Source: Authors' calculations from survey data.

Notes: Each column and panel correspond to separate difference-in-difference regressions that interact a dummy variable for each group with the treatment indicator and all controls. Standard errors in parentheses are clustered at school-level. Reported significance levels and p-values are adjusted for multiple hypothesis testing using a Bonferroni correction that accounts for correlation among outcomes in a group (see Section 4.2 for details).

* Significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.7. Heterogeneous effects on test scores and higher education choices, perceptions and risk aversion

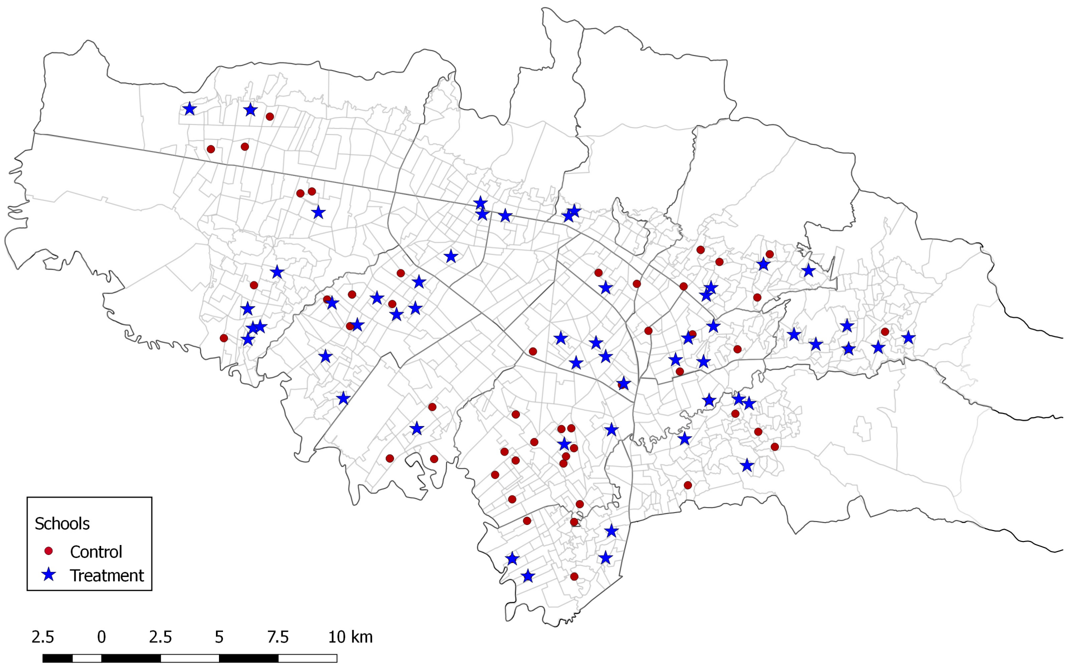
	Test scores			Higher education choices				
	Overall score	Math	Language	College enrollment	Academic degree	Private college	Top-10 college	STEM field
A. Perceived academic ranking								
Low	0.015	0.066	-0.010	0.005	0.016	0.005	0.007	0.004
	(0.045)	(0.049)	(0.042)	(0.025)	(0.013)	(0.003)	(0.009)	(0.007)
High	-0.002	0.038	0.024	0.007	0.008	0.006	0.011	0.008
	(0.047)	(0.050)	(0.043)	(0.027)	(0.018)	(0.005)	(0.015)	(0.011)
p-value (Low=High)	0.993	0.768	0.692	1.000	0.999	1.000	1.000	1.000
Observations	6,268	6,268	6,268	6,248	6,248	6,248	6,248	6,248
B. Perceived self-efficacy								
Low	-0.034	0.032	-0.052	0.003	0.014	0.004	0.002	0.000
	(0.044)	(0.049)	(0.039)	(0.023)	(0.013)	(0.003)	(0.011)	(0.008)
High	0.076	0.091	0.094*	0.005	0.012	0.008	0.021	0.013
	(0.048)	(0.051)	(0.046)	(0.027)	(0.017)	(0.005)	(0.012)	(0.010)
p-value (Low=High)	0.103	0.423	0.011	1.000	1.000	0.973	0.892	0.871
Observations	6,257	6,257	6,257	6,237	6,237	6,237	6,237	6,237
C. Risk aversion								
Low	0.020	0.081	0.039	0.031	0.019	0.016	0.032	0.032
	(0.085)	(0.090)	(0.074)	(0.039)	(0.024)	(0.009)	(0.018)	(0.015)
High	-0.012	0.035	-0.015	-0.002	0.011	0.004	0.005	0.002
	(0.040)	(0.041)	(0.036)	(0.022)	(0.013)	(0.003)	(0.009)	(0.007)
p-value (Low=High)	0.992	0.762	0.668	0.950	1.000	0.720	0.719	0.200
Observations	6,085	6,085	6,085	6,066	6,066	6,066	6,066	6,066
D. Perceived likelihood of enrollment								
Low	-0.039	0.014	-0.057	0.015	0.004	-0.001	0.008	-0.001
	(0.056)	(0.058)	(0.062)	(0.032)	(0.016)	(0.003)	(0.014)	(0.008)
High	0.001	0.044	0.002	0.005	0.015	0.007	0.010	0.007
	(0.039)	(0.044)	(0.036)	(0.023)	(0.014)	(0.003)	(0.010)	(0.007)
p-value (Low=High)	0.941	0.831	0.539	1.000	0.994	0.313	1.000	0.980
Observations	6,023	6,023	6,023	6,004	6,004	6,004	6,004	6,004

Source: Authors' calculations from surveys matched to administrative data.

Notes: Each column and panel correspond to separate OLS regressions that interact a dummy variable for each group with the treatment indicator and all controls. Standard errors in parentheses are clustered at school-level. Reported significance levels and p-values are adjusted for multiple hypothesis testing using a Bonferroni correction that accounts for correlation among outcomes in a group (see Section 4.2 for details).

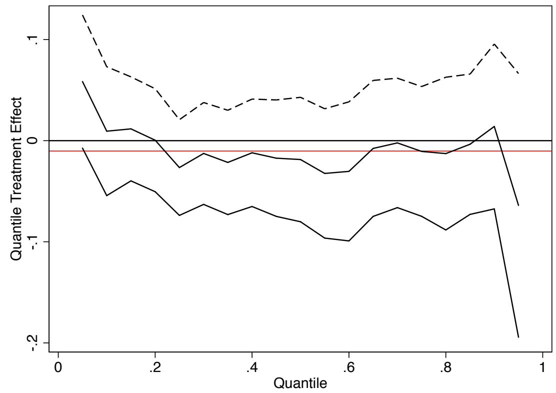
* Significant at 10%; ** significant at 5%; *** significant at 1%.

Figure A.1. Geographic distribution of 115 treatment and control schools

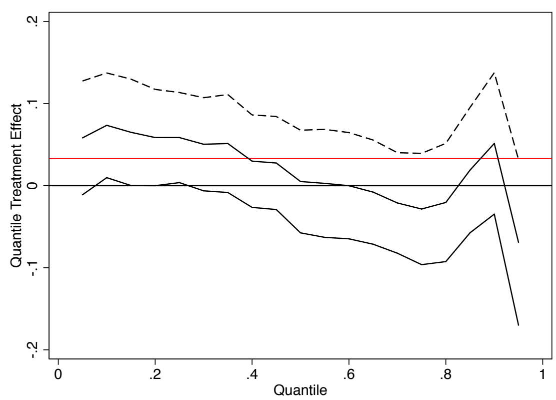


Source: Authors' elaboration from Secretary of Education's school census and survey data.

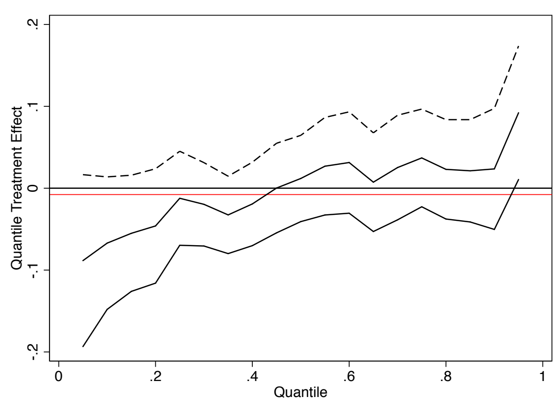
Figure A.2. Quantile treatment effects for SABER 11 test scores



Overall score



Math



Language

Source: Authors' elaboration from surveys matched to administrative data.
Notes: 90% Confidence intervals in black dashed/red dotted lines. OLS estimate in red line.

Universidad de los Andes
Escuela de Gobierno Alberto Lleras Camargo

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Reconocimiento como Universidad: Decreto 1297 del 30 de mayo de 1964.
Reconocimiento personería jurídica: Resolución 28 del 23 de febrero de 1949 Minjusticia.