Medium- and Long-Term Educational Consequences of Alternative Conditional Cash Transfer Designs: Experimental Evidence from Colombia[1]

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October 2016

Abstract
We show that three Colombian conditional cash transfer (CCT) programs for secondary school improve educational outcomes eight to twelve years after random assignment relative to a control group. Forcing families to save a portion of the transfers until they make enrollment decisions for the next academic year increases on-time enrollment in secondary school, reduces dropout rates, and promotes tertiary enrollment and completion in the long-term. Traditional stipends improve on-time enrollment and high school exit exam completion rates in the medium-term but do not affect tertiary outcomes in the long-term. A stipend that directly incentivizes tertiary enrollment promotes secondary school on-time enrollment and enrollment in lower quality tertiary institutions in the medium but not the long-term.

JEL codes: C93, I21, I38
Keywords: Conditional Cash Transfer, medium-term effects, long-term effects, tertiary education, randomized controlled trial, Bogota Colombia

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[1] Acknowledgements: As in the original paper, we are grateful to the Secretary of Education of Bogota (SED) for their cooperation in and financial support of the original experiment, as well as for providing administrative records for this study. We are also grateful to Fedesarrollo for financial and technical assistance. Several individuals provided research assistance at various stages of the project's development: Luis Omar Herrera was instrumental in assisting us with the medium-term administrative data. Camilo Dominguez, Megan Thomas, and Ricki Sears Dolan also provided assistance with the data analysis. Richard Murnane and Katja Vinha provided valuable comments. Linden acknowledges financial support from the National Science Foundation's Award SES-1157691 and Saavedra from the National Institute of Health RCMAR Grant P30AG043073.
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I. Introduction

Conditional cash transfers (CCT) are one of the most prevalent and fastest growing social assistance programs in the developing world. Today, over fifty countries worldwide operate CCTs, more than twice the number in 2008 (World Bank 2014a). Much of the research on CCTs has documented short-term educational impacts on outcomes such as enrollment, attendance and dropout rates (for reviews see Baird et al. 2014; Garcia & Saavedra 2016; Fiszbein & Schady 2009). There is very limited evidence, however, on the long-term educational effects of CCTs (notable exceptions include Filmer & Schady 2014; Barham et al. 2013; Baez & Camacho 2011; Behrman et al. 2010).[2] This paper provides experimental estimates of medium- and long-term effects —between eight and twelve years after initial receipt— relative to a pure experimental control group that throughout the period does not receive transfers. It is also the first to document experimentally the effects on tertiary enrollment and how these medium- and long-term impacts may vary with program design.

In 2005, the government of Bogota, Colombia in collaboration with a subset of co-authors and others, randomized differently structured conditional cash transfers targeted at socioeconomically disadvantaged secondary school students (Barrera et al., 2011). Using various sources of administrative data, we conduct an assessment of the effects of alternative payment structures on students' continued secondary school enrollment, completion of the high school exit exam, and tertiary education enrollment and completion.

[2] Filmer and Schady (2014) employs an RD design to estimate effect of a three-year CCT offer to secondary school students in Cambodia to show increases in secondary school grade attainment (no impacts on test-scores, employment or earnings). Baez & Camacho (2011) and Behrman, Todd & Parker (2010) employ non-experimental research designs for Colombia's Familias en Accion and Mexico's Oportunidades. Barham et al. (2013) use the randomized phase-in of Nicaragua's Red de Proteccion Social program.

We experimentally compare two payment structures relative to a control group that receives no transfer. The first is a standard CCT payment scheme that provides a fixed bimonthly transfer conditional on secondary school enrollment and continued school attendance (the "basic" treatment). The second is identical to the basic treatment, except it forces families to save close to one-third of the stipend each month until the time at which families must make enrollment decisions for the next academic year (the "savings" treatment). Separately, we evaluate, relative to a different control group, another variant of the payment structure in which students receive a monetary incentive for secondary school graduation and tertiary enrollment (the "tertiary" treatment).

The savings and basic treatments both generate medium-term benefits, although the alternative structure of the savings treatment generally proves to be more effective in the long-term. Across all students, the savings treatment improves performance on more and longer-term outcomes. It increases the probability of on-time enrollment three years after the start of the experiment by 3.5 percentage points, due mostly to a 3.2 percentage point reduction in the probability that students drop out. For upper secondary students (grades nine through eleven) the savings treatment increases tertiary enrollment in universities in the medium-term (eight years after randomization) by 3.6 percentage points (base is 31 percent), and in the long-term (twelve years after randomization) by 2.8 percentage points (base is 40 percent). This persistent effects on tertiary enrollment from the savings treatment suggests that, at least in the context of Bogota's CCT program, the main operating mechanism is not the program's conditionality. This result is consistent with evidence from various other contexts (Baird et al. 2011; Baird et al. 2014).

We can rule out that the concentration of tertiary education effects among upper secondary students in the middle term in the savings treatment is driven solely by cohort composition. We present suggestive evidences that also (partially) rules out that transfers in the savings treatment increase tertiary enrollment by (sorely) relaxing liquidity constraints. These two pieces suggest that, by targeting upper secondary students and potentially waiting until ability is better revealed, the program may be operating through a "scholarship" model that rewards students who successfully transition from lower to upper secondary and may be more inclined to pursue tertiary education.

The traditional basic treatment increases on-time enrollment in secondary school and, while the savings treatment only causes improvements for upper secondary students in the medium-term, the basic treatment increases the probability of taking the secondary graduation exam for all students. We find no effect of the basic treatment on tertiary enrollment, either in the medium- or long-term. We strongly reject equality of long-run effects between the basic and savings treatments.

The tertiary treatment has effects similar to the savings treatment in the medium-term. It improves on-time enrollment in secondary school by 2.2 percentage points by reducing dropout rates by 3.6 percentage points. It does not affect graduation, but it improves enrollment in tertiary institutions by 5.8 percentage points (*vis-à-vis* a control group mean of 31 percent) eight years after enrolling in the program. In contrast with the savings treatment, however, the tertiary treatment induces applicants to enroll in lower quality tertiary education institutions. This result may be related to the tertiary treatment's high power incentives to enroll in institutions of higher education. However, these tertiary enrollment effects dissipate in the long-term.

This paper contributes to two strands of literature. First, we contribute to recent work on the effects of savings constraints on educational investments (see for example, Karlan and Linden 2014; Benhassine et al. 2013). In particular, our results complement those of Karlan and Linden (2014) who show that weaker savings commitments, which do not require families to spend money on specific types of goods, can improve educational outcomes.

Second, we make a unique contribution to the voluminous literature on conditional cash transfers. We build on Barrera et al. (2011) to highlight the importance of structuring transfers in a way that alleviates savings constraints. We document that with a revenue-neutral modification in the timing of transfers to a standard CCT design, students can be induced to enroll in tertiary education, unlike standard CCT designs, which only promote compliance with transfer conditions.[3]

The rest of the paper is organized as follows. In Section II we describe the background and experimental intervention. In Section III we explain the research design and data sources. We discuss the internal validity of the experiment in Section IV and present results in Section V. Section VI concludes.

II. Program Background, Experimental Design and Prior Evidence on Short Term Impacts

In 2005, Colombia's capital city Bogota established the Conditional Subsidies for School

[3] Others have documented that variation in conditions and program design affect short term educational outcomes of CCTs. For example, De Brauw & Hoddinott (2011) and Baird et al. (2011) test the role of conditionality. Chaudhury &Parajuli 2010; Fiszbein & Schady, 2009; Filmer & Schady, 2011; Fernald et al. (2008) test the importance of transfer size. Benhassine et al. (2013) explore the role of soft vs. strong commitments. Benhassine et al. (2013) and Barder & Gertler (2009) explore the role of recipient identity.

Attendance (“Subsidios Condicionados a la Asistencia Escolar”) pilot program in an effort to increase student retention, reduce dropout rates and ameliorate child labor among low-income secondary school students. The Secretary of Education of the City (Secretaria de Educacion del Distrito, SED) implemented the program in San Cristobal and Suba, two of the poorest localities in the city.[4]

The program is a variant of traditional CCTs —such as Mexico’s PROGRESA/Oportunidades— focusing only on educational investments among secondary school students. As such, it does not include the health or nutritional components that typically accompany CCTs that target younger children.[5] Unlike many other educational conditional cash transfer programs, the SED intended this pilot to be a policy experiment in which it would test three alternative treatment variations. In all treatments, students were required to attend at least 80 percent of school days during each payment period.[6] Students would be removed from the program if they twice failed to matriculate to the following grade, failed to reach the attendance target in two successive payment periods or were expelled from school.

Eligibility was based on several criteria. Applicants had to have finished the fifth (San Cristobal) or eighth (Suba) grade (last grade of primary) and they had to be enrolled in a secondary school. The applicant’s family had to demonstrate that they had been designated as impoverished based on the national poverty assessment tool, SISBEN.[7] Applicants also had to present a valid national identification card (which the vast majority

[4] There are twenty localities in Bogota.

[5] Reviews of these programs are Baird et al. (2013); Saavedra & Garcia (2012); Fiszbein & Schady (2009).

[6] Payments were made on a bimonthly basis. As a result, student had to achieve 80 percent attendance over a two-month period to receive payment.

[7] Families had to present their SISBEN card and be ranked in the lowest or second to lowest of the system’s six categories.

of students have) to validate their poverty status at the time of registration. Finally, to prevent families from moving to obtain eligibility, only families classified by the SISBEN system as living in San Cristobal or Suba prior to 2004 were eligible to participate.

In San Cristobal, eligible secondary school students entering upper and lower secondary school (grades six through eleven) were randomly assigned to the basic treatment, the savings treatment or a control group. In the basic treatment, which is similar to Mexico's PROGRESA/Oportunidades program, participants were paid about $30 every two months via a dedicated debit card from one of Colombia's major banks as long as they complied with the program conditions. Conditional on full compliance with the attendance requirements, the total annual value of the transfer amounted to $150, which was slightly more than the average $125 that families reported spending each year on educational expenses (Barrera-Osorio et al. 2011).

The savings treatment was designed to be a revenue-neutral experimental variant of the basic treatment.[8] Compared to the basic treatment, the payment structure in the savings treatment differed. In the savings treatment, instead of receiving $30 for reaching the attendance target over two months, students were paid $20, while the remaining $10 was held in a bank account. The accumulated funds—up to $50 per school year for fully compliant students—was then made available to families during the period in which students prepared to enroll for the next academic year. This savings treatment differs from the basic intervention in that it could potentially provide a means of bypassing short-term liquidity constraints when paying enrollment expenses.

[8] Both treatments are exactly revenue-neutral in the absence of inflation. In practice, inflation during the 2005/2006 period was 5.6% (World Bank, 2014c).

In Suba, eligible secondary school students entering upper secondary school (grades nine through eleven) were randomly assigned to a "tertiary" treatment group or a control group. As in the savings treatment, participants in the tertiary treatment were paid a basic transfer of $20 every two months but they were also eligible for a secondary school graduation and tertiary enrollment incentive. Students who successfully graduated from secondary school became eligible to receive a lump-sum transfer of $300. Students received the funds immediately upon documenting enrollment in a tertiary education institution.[9] If students failed to enroll in higher education, they still received the transfers, but were penalized by having to wait a year. Therefore, the incentive in the "tertiary" treatment is just the delay of payment, not whether the payment is made. While cost-equivalent to the basic treatment for students going through six years of secondary education, the tertiary treatment ends up being more generous than the basic treatment because in practice —due to an administrative decision from part of the SED— it was offered only to students that were, at baseline, three years or less from graduation. [10]

Assignment in both localities was contingent on over-subscription. To ensure oversubscription, the SED advertised the program through posters, newspapers ads, radio clips, loudspeakers in cars, churches and community leaders, including school principals and priests.[11] Interested applicants had to register during a 15-day window between late

[9] The transfer for post-secondary enrollment represents about 70 percent of the average first year cost in a technical post-secondary institution (Barrera-Osorio et al. 2011).

[10] Applicants in grades six through eight in Suba were assigned to either a control group or the basic treatment. As in Barrera-Osorio et al (2011), we omit the results for this subsample. However, they are similar to the treatment effects of the basic treatment for grades six through eight in San Cristobal in Tables 3-6, except that the effect on dropout rates is statistically significant. These results are available upon request.

[11] The transfers were advertised as incentives to participate in school, with an annual value equal to at least the annual value of the basic treatment, so that families were not aware at the time of registration of the existence of different treatments.

February and early March 2005. Program registration took place in various schools at the two localities.

The SED guaranteed in 2005 funding for 7,984 students in total: 6,851 in the basic-savings experiment in San Cristobal and 1,133 in the tertiary experiment in Suba. In total, 13,433 eligible applicants registered in the two localities: 10,907 in San Cristobal and 2,526 in Suba. Barrera-Osorio et al. (2008, 2011) created a stratified randomization algorithm that SED implemented in publicly held lotteries in each locality on April 4 2005. The algorithm stratified by locality (San Cristobal, Suba), school type (Public/Private), grade (6th-11th) and gender. A team of economists from Universidad Nacional in Bogota verified the validity of the algorithm prior to its implementation as well as compliance with the (random) assignment results during the public lottery events.

Barrera-Osorio et al. (2011) document that one year after randomization of students into treatments, all treatments significantly increase school attendance relative to control conditions. In addition, the savings and tertiary treatments increased grade re-enrollment in secondary education relative to control, unlike the basic treatment, which had no effect. Similarly, the savings and tertiary treatments increased tertiary enrollment after one year of treatment for students who were enrolled in grade eleven at baseline.

The Secretary of Education preserved the integrity of random assignment. This implies that control students did not receive any treatment throughout their secondary school enrollment. It also implies that school grade at baseline maps directly into years of exposure to the various treatments. Under perfect compliance with program conditions, treated individuals in grade 11 at baseline received one year of treatment whereas individuals in grade 6 at baseline received up to 6 years of treatment. Hence, impact

estimates broken-down by grade at baseline represent impacts of different years of exposure to treatment as well as potential treatment effect heterogeneity by grade or age.

III. Data and Estimation Methods

A. Data

Instead of generating a new follow-up survey, we combine administrative data sources with the original experimental sample to track medium-term educational outcomes[12]:

1. Program registration data: This dataset contains identification numbers for the 13,433 eligible applicants in the two experiments, which we use to match with the other data sets described below. It also includes information on the school and grade in which students enrolled at the time of the lottery.
2. SISBEN: At baseline, we matched applicant records to Colombia's *Sistema de Identificación y Clasificación de Potenciales Beneficiarios para Programas Sociales* (SISBEN) also known as the census survey of the poor. We matched one hundred percent of applicant records to the 2003-2004 census. We use the data as baseline socio-demographic controls because all of it—including household composition, assets, and income—was collected prior to the randomization.
3. Secondary school enrollment records: To measure secondary school enrollment, we use annual administrative data from SED.[13] The data are similar to those used in Barrera-Osorio et al. (2011), but include information from 2006-2008.[14] These data

[12] We attempted a follow-up survey of lottery applicants in 2012 and, during pilot phase, obtained responses from less than a third of the sample. For this reason, we did not pursue this strategy any further.
[13] The data include enrollment information for all public schools and most private schools in the city. The few non-participating private schools are not an issue for our study. Although we are unable to distinguish between schools who do not report and schools who report but do not have any enrolled students in our sample, only 55 students (0.4 percent of the sample) attend schools in this group in 2006.
[14] The data for 2006 is an alternate version of the data used to measure 2006 enrollment in Barrera-Osorio (2011). The earlier data set had been cleaned more thoroughly by the SED than the current data sets but was

include an indicator for whether or not a student is enrolled as well as information on the students' grade level, allowing us to measure grade repetition. As shown in Barrera-Osorio et al. (2011), the match rate with the program registration data is high—over 90 percent—and there is no difference in the probability of matching records between research groups.[15]

4. ICFES: We use administrative data from Colombia's centralized secondary school exit examinations—ICFES (*Instituto Colombiano para la Evaluacion de la Educacion*). ICFES registration is a good proxy for secondary school graduation since over 95 percent of all secondary school students take the exam (Bettinger et al. 2014; Angrist, Bettinger and Kremer 2006). Given the timing of the original lottery and data availability only through 2012, we match applicant records to the universe of test-takers from 2005 to 2012, a maximum of eight years after the beginning of the treatment.
5. SPADIES: To track tertiary enrollment, we use data from the Colombian Ministry of Education's Sis*tema de Prevención y Análisis de la Deserción en Instituciones de Educación Superior* (SPADIES). SPADIES is an individual-level panel dataset that since 1998 has tracked students from their first year of college enrollment until their degree receipt. SPADIES is similar to the National Student Clearinghouse in the United States, covering 95 percent of the post-secondary population in Colombia. SPADIES contains information on the timing and university of student's initial

only available for 2006. That said, the treatment effect estimates are very similar to those from the earlier data set, as we note below. The data used to match the two versions of the enrollment data to the program registration data is the same.

[15] We also demonstrate in Appendix A that the main results for the ICFES and SPADIES data sets are robust to limiting our sample to just those students for which enrollment data is available.

enrollment and the type of institution. Higher quality institutions are classified as either universities or vocational schools, while lower quality institutions remain unclassified.[16]

We use two cuts of the SPADIES data one that covers collegiate pathways from 2005 up to 2012—up to eight years after the start of the program or "medium-term" and one that covers collegiate pathways up to 2016—up to twelve years after the start of the program or "long-term." The long-term SPADIES dataset is particularly useful to track collegiate outcomes for students who began the program in early secondary grades (i.e. grades 6-8 in 2005).

To match registration records to ICFES and SPADIES data we followed a four-step algorithm:

1. Exact match on student ID number, name, and date of birth;
2. For those not matched in (i), exact match on ID and date of birth;
3. For those not matched in (i) or (ii), exact match on ID and names;
4. For those not matched in (i), (ii), or (iii), match on name and date of birth.

Table 1 displays the match rates among the enrollment, ICFES and SPADIES data. Enrollment match rates in 2006 are very similar to those in Barrera-Osorio et al. (2011). Without grade repetition and dropping out, we would expect that a sixth of the sample graduates each year (approximately 17 percent). The actual reduction in matches in 2007 and 2008 is consistent with the expected repetition and dropout rates (Panel A of

[16] The data also include information that will allow us to follow students through to graduation. However, this will be a topic for future work when data is available beyond 2012. Since our youngest students were in grade six in 2005, they would not graduate form a university until 2014 at the earliest. And of course, it will likely will take a few years longer given that many of them have already been held back at least once in secondary school.

Table 1).

Match rates to ICFES and SPADIES data across all students are similar to those among comparable individuals in Bogota (Panel A of Table 1). Based on representative survey data from Colombia's 2010 Encuesta de Calidad de Vida (ECV), we calculate that among low-income 18- to 25-year olds in Bogota who have completed primary school, 72 percent report having completed secondary school. This is very similar to the 69 percent rate we find among applicants for taking the ICFES test in the San Cristobal (basic and savings) experiment. Similarly, among these individuals in the ECV, 21 percent have completed some college, which is exactly the SPADIES match rate in the San Cristobal (basic and savings) experiment. The rates also align to those reported in Bettinger et al. (2016). The match rates for the tertiary experiment are higher for both ICFES (0.84) and SPADIES, medium-term (0.37) and long-term (0.45, Panel A, Column 3, Table 1).

B. Estimation Strategy

Given random assignment, we estimate causal treatment effects by comparing average outcome levels across treatment groups. To maximize precision, we do this in a regression framework that also controls for pre-treatment applicant characteristics:

$$Y_{ij}=b_0 + \boldsymbol{b_t'Treatment_i} + \boldsymbol{b_t'X_i} + e_{ij} \qquad (1)$$

where Y_{ij} is an outcome variable for applicant i in school j, and $\boldsymbol{Treatment_i}$ is a vector of indicator variables for the treatment group to which the applicant was assigned. We initially estimate Equation (1) separately for each experiment, so the vector $\boldsymbol{Treatment_i}$ in the San Cristobal sample includes indicators for the basic and savings treatment and in the Suba sample it includes an indicator for the tertiary treatment.

The vector $\boldsymbol{X_i}$ contains the set of demographic characteristics. It includes four asset/wealth indexes (possessions, access to utilities, ownership of durable goods, and the physical infrastructure of the child's home), age, gender, years of education at registration, grade indicators, and a range of household characteristics (whether the head of the household is single, head's age, head's years of education, number of people in the household, number of children in the household, socioeconomic stratum classification, SISBEN score, and monthly income). In our preferred specification we also include school fixed effects, so that only variation within schools in treatment assignment identifies the parameters of interest. We cluster all standard errors at the school level.

In some specifications, we also pool estimates from the San Cristobal and Suba samples. To do this, and given that the Suba experiment only cover grades nine through eleven, we restrict the sample to applicants in grades nine through eleven at baseline and include a district fixed effect to account for mean level differences, such as differences in the probability of treatment assignment between samples. Recall, however, that the San Cristobal and Suba are two independent experiments. Hence, while we can only experimentally estimate the causal effect of the tertiary treatment of Suba's experiment, we can only identify its relative effect compared to the basic and savings treatments (in San Cristobal) using a rich set of socio-demographic controls and school-level fixed effects rather than purely random variation. Pooling the San Cristobal and Suba samples for grades nine through eleven is empirically justified given the similarities in baseline characteristics across the two groups of students (Barrera-Osorio et al. 2008 presents a detailed comparison).

IV. Internal Validity

The potential threats to internal validity are limited. First, Barrera et al. (2011) validate compliance with the randomization protocol by showing that the applicants assigned to each treatment group were comparable at baseline. Second, the centralized administrative records obviate concerns of low response rates or differential attrition because they include the universe of students.

Potential sample selection issues stemming from differential data quality across treatment and control groups are unlikely for a number of reasons. First, in all of our matches we employ the original identification data reported at baseline. As we show next, there are no differences across groups in the availability of identifying information at baseline. Second, the research team put in place strict protocols for data-entry, cleaning and coding of the experimental sample dataset. Third, enrollment data is centralized at SED and to our knowledge there is no differential treatment in student records across experimental groups or localities. Specifically information for individuals in each treatment group is not differentially updated. Fourth, the Ministry of Education centrally manages the ICFES high school graduation and SPADIES college enrollment databases and the Ministry does not have access to the individual information of the original sample of the experiment.

Nevertheless, differential availability of identifying information needed to match records to the administrative data could pose a problem for internal validity. To assess this threat, we analyze the availability of the four variables we use to match the data form the original experiment in Barrera et al. (2011) to the administrative data described above: students' last names, first names, national ID numbers, and birthdays. First, we find that

very little information is missing. The data include birthdays and first names for all students, and national ID numbers and complete last names for 99.4 and 97.8 percent of the students respectively. For variables in which information is missing, we then show in Table 2 (using Equation (1)) that the availability of this information is evenly distributed between the various research groups. Finally in Appendix B, we show that the characteristics of students for whom we have information are balanced across the treatment groups. These results suggest a high level of internal validity.

V. Results

A. Secondary Enrollment and Graduation

We start by documenting effects on students' secondary school enrollment (Table 3). We use grade information to create an indicator variable for whether or not students are enrolled "on time" in each academic year 2006, 2007 and 2008.[17] For each student, the indicator variable for on-time enrollment is set to one if the student has not dropped out and has not been held back.[18]

In the basic and savings experiment we find that, relative to control conditions, the basic treatment increases on-time enrollment by 2.4 percentage points (*vis-à-vis* a control group average of 51%). This difference is statistically significant at the ten-percent level with full controls (column three of Table 3). Relative to the control group, the savings treatment increases students' on-time enrollment by 3.5 percentage points, a difference that

[17] In Colombian public schools and non-elite private schools, the academic year runs from February through December.

[18] Specifically, we consider a student as enrolled on time if the student is enrolled and (analysis year – 2005) = (grade in analysis year – grade at baseline). For example, a student in grade six in 2005 would be expected to be in grade seven in 2006, grade eight in 2007, and grade nine in 2008.

is statistically significant at the one-percent level (column three). The estimate on the tertiary treatment is also positive (2.2 percentage points) and statistically significant at the ten-percent level (column six). All estimates are robust to the alternative specifications presented in columns one, two, four and five.

To compare across the experiments, we restrict the sample to students in upper secondary school and pool the samples. These results are presented in column seven. For these older students, the effect of the basic treatment falls and the effect of the savings treatment remains unchanged. The result is a statistically significant difference in treatment effects (p-value is 0.06). We cannot, however, reject equality between either of these treatments and the tertiary treatment. Finally, in column eight, we present the results for lower secondary students and find treatment effects for the basic treatment that are now on par with those of the savings treatment.

To understand the drivers of the on-time enrollment results, we estimate effects for the basic and savings experiment (Panel A) and the tertiary experiment (Panel B) on other aspects of enrollment in Table 4. First, we estimate the effects for each year of data on whether students are enrolled regardless of being held back in columns one through three.[19,20] By this measure, we find that the savings treatment significantly increases enrollment. The basic treatment and the tertiary treatment have uniformly positive effects, but these are not consistently statistically significant. The results are similar when for the on-time enrollment by year outcome (columns four through six). With on-time enrollment,

[19] For these estimates, we exclude students who would have graduated had they not been held back. So, for example, the estimates for 2006 exclude students enrolled in grade eleven at registration in 2005.

[20] The estimates for 2006 also give us a chance to compare the results using the new data set to the results obtained from the previous data. The estimated treatment effects similar to those found in Barrera et al. (2011): 0.009 for the basic treatment and 0.034 for the savings treatment with standard errors of 0.010 and 0.011 respectively.

the standard errors on the treatment effect for the savings treatment are small enough in 2007 and 2008 for the effects to be statistically significant.

In columns seven and eight, we disaggregate the overall on-time enrollment effect observed in Table 3 by separately measuring whether students were held back or dropped out. The on-grade enrollment effect is largely explained by a reduction in dropouts. Although we find no effect from the basic treatment, dropout rates fall by 3.2 percentage points in the savings treatment and 3.6 percentage points in the tertiary treatment. We find no effect from any treatment on students being held back in secondary school.

Next, we assess the treatment effects on the probability that students took the ICFES secondary school exit exam (Table 5). Overall, only the basic treatment increases exam-taking by 2.2 percentage points for all students (column three), and the results are again consistent across specifications. However, when we compare the three treatments simultaneously using just students who were in upper secondary school at enrollment, we again observe differences in effects by secondary school level. First, we find similar effects for both the basic and savings treatment for students in upper secondary, and the effect for the savings treatment is statistically significant at the ten-percent level. However, despite the small coefficient on the tertiary treatment indicator, we cannot reject equality between either the basic or savings and the tertiary treatment effects. For lower secondary students, we do find a larger effect for the basic treatment than the savings, but neither effect is statistically significant.

In order to test for heterogeneity by baseline characteristics, we estimate Equation (1) with interactions between the treatment variables and two baseline characteristics (student gender and income of the household) for two outcomes (on-time enrollment and

the probability of a student taking the ICFES). We do not find evidence of heterogeneous effects by gender or baseline income.[21]

B. Medium-term Tertiary Enrollment

In this section we document the treatment effects on students' tertiary education enrollment (Table 6) using SPADIES medium-term data (up to 2012). This dataset will capture with higher probability college pathways for individuals who were in upper grades at the beginning of the experiment.

The savings treatment increases the probability of ever enrolling in a tertiary institution by 1.5 percentage points (vis-à-vis a base rate of 21 percent), statistically significant at the ten-percent level. The effect of the basic treatment is positive but small, not statistically significant but indistinguishable from the savings treatment effect (columns 1-3). The tertiary treatment estimate—in the specification with full controls—is 5.7 percentage points (vis-à-vis a base rate of 35 percent). The effects are again similar for all specifications.

When we restrict the sample in the basic and savings experiments to those students who were in grades nine through eleven at registration, the treatment effect for the savings treatment is 3.6 percentage points (column seven). In this sample, the difference between the savings and basic treatment is statistically significant at the one-percent level. We are also able to reject the null hypothesis of equality between the tertiary enrollment effects

[21] Results are available upon request.

and the basic effects, but not between the savings and tertiary treatments.[22, 23] For students in lower secondary, we find no effects for either the basic or the savings treatments.

Disaggregating the results by institution, we do, however, find differences in the types of schools in which the tertiary and savings treatments cause students to enroll (Table 7). For upper secondary students, the primary tertiary enrollment effect of the savings treatment is to encourage enrollment in universities rather than vocational schools or the lower quality unclassified schools. The tertiary treatment, on the other hand, seems to solely encourage enrollment into the unclassified schools. It may be that the high-powered incentives encourage students to enroll more indiscriminately. We find no significant differences for younger students.[24]

C. Long-Term Tertiary Enrollment and Completion

To estimate long-term effects, we use the SPADIES dataset up to the year 2016. To the extent that they are college-bound, this long-term dataset enables us to observe collegiate pathways for students from all grades at the time of randomization —year 2005—.

[22] When we estimate the model used in column three of Table 6 interacting the treatment effects with grade level at registration, we obtain an estimate of the coefficient on the interaction term of 1.3 percentage points per grade level for the savings treatment (p-value of 0.012) and on the main treatment coefficient of -9.1 (p-value of 0.031). This suggests that the savings treatment effect for students in grade six at registration is small and negative (-1.3 percentage points), while for those in grade eleven at registration it is large and positive (5.2 percentage points). For the basic treatment the interaction and main effect estimates are 0.001 and statistically insignificant. We do not find a similar pattern for taking the ICFES exam. The interactions effects are small and insignificant. For on-time enrollment, the treatment effect for the savings treatment is constant across grades while the basic treatment declines for older students.

[23] For the tertiary treatment, we have significantly fewer grade levels to exploit. However, we do find that the treatment effect on tertiary enrollment increases by 4.7 percentage points per grade (p-value of 0.036) over a base treatment effect of -0.402 (p-value of 0.063). The effects for on-time enrollment and the exit exam do not vary with grade.

[24] We test heterogeneous effects on tertiary enrollment by gender and baseline income. Like the estimation for secondary enrollment and graduation, we fail to accept heterogeneous effects. Results are available upon request.

Table 8 presents the effects of the various treatments on long-term tertiary enrollment rates. Overall, effects of the basic and savings treatments are positive, but small in magnitude and never statistically significant. For the tertiary treatment effects are comparatively large although also insignificant. When we restrict the sample to upper secondary grades, the effect of the savings treatment is 2.8 percentage points, significant at the 10 percent level (Column 7, Table 8).

Given that the long-term SPADIES dataset allows enough time for potential enrollment and graduation of students from all grades at the beginning of the experiment, we focus on two additional outcomes: on-time tertiary enrollment (Table 9) and graduation (Table 10). We calculate these outcomes unconditional on tertiary enrollment; e.g. a person who did not enroll in tertiary has a value of zero for on-time enrollment and for graduation.

Focusing on all grades, neither the basic nor the savings treatment increase on-time tertiary enrollment in the long-term (Columns 1-3, Table 9). The tertiary treatment, by contrast, increases on-time long-term tertiary enrollment by 3.1 percentage points (base is 24 percent, Columns 4-6, Table 9). When we restrict the sample to focus on upper secondary students, both the savings and the tertiary treatment increase on-time tertiary enrollment in the long-term by 3.9 and 3.2 percentage points, respectively (Column 7, Table 9). On-time tertiary enrollment effects are close to zero or negative and always insignificant for lower secondary students. The fact that using the long-term SPADIES dataset we continue to find no tertiary enrollment effects for lower secondary students suggests that the treatments—particularly the savings and tertiary treatments—interact with students' grade progression through secondary school. We explore potential reasons

for why might be the case below in subsection E.

None of the three treatments affect tertiary graduation in the full sample (Columns 1-6, Table 10). When we focus on upper secondary grades, we find positive effects on tertiary graduation that are marginally statistically significant for the basic and for the savings treatments (Column 7, Table 10). These effects are of the order of magnitude or 1.6-1.9 percentage points, from a base of 10%. We find no effects of the various treatments on tertiary graduation among lower secondary students (Column 8, Table 10).

D. Joint Hypothesis Tests

The purely experimental results thus far suggest that the savings treatment has larger effects than the basic treatment on upper secondary grade enrollment and tertiary education enrollment. Similarly results are consistent with the tertiary treatment producing larger effects on tertiary enrollment than the basic treatment. Given the number of outcomes we analyze, we conduct a joint hypothesis test of the treatment effect for each treatment and the difference between the basic and savings treatment. In the medium-term we focus on on-time enrollment in secondary school; taking the secondary school exit exam; and tertiary enrollment. In the long-term we focus on on-time enrollment in secondary school, taking the secondary school exit exam, long-term tertiary enrollment; on-time tertiary enrollment; and tertiary graduation. All estimates are performed using Equation (1) with a Seemingly Unrelated Regressions model. The results are presented in Table 11.

In the medium-term we find that the overall effects of all treatments are statistically significant (Column 1, Table 11). The p-values on the savings and tertiary treatments are

less than one-percent. The savings treatment is also statistically significant at the ten-percent level with a p-value of 0.078.

To test for differences between the savings and basic treatments, we use the same model, but allow for differential effects by secondary school level. Specifically, we include an indicator variable for students having been enrolled in upper secondary at the time of registration.[25] Overall, we can reject the equality of treatment effects with a p-value of 0.019. This result, however, seems to be driven by upper secondary students, which is consistent with the observed differences in the previous sections. We can reject equality with a p-value of 0.057 for upper secondary students, but for lower secondary students, the p-value is just 0.312.

In the long-term, we find that the savings treatment is the only treatment for which we can consistently reject the null hypothesis of zero effects on all educational outcomes (p-value of 0.001, Column 2, Table 11). Furthermore, as in the medium-term test, we reject the hypothesis of equality of effects from basic versus secondary treatment (p-value 0.029). When we separate the test for lower and upper grades, it seems that lower-grades drive the difference in results between savings and basic treatments (p-value of 0.047). The respective test for upper secondary grades has a p-value of 0.147.

Demotivation effects—control students responding negatively to treatment—could potentially explain effects between students assigned to treatment and control. However, demotivation effects are inconsistent with the differential effects we find across treatments. In the short term evaluation of the program, Barrera-Osorio et al (2008) found *positive* peer effects on attendance rates on the network of (untreated) friends. A more recent estimate

[25] The p-value on a joint test of the significance of the interaction term is 0.0634.

of peer effects of the program in the short run (Dieye et al, 2015) finds small positive net effect of treatment on non-treated friends for the attendance outcome. If anything, these peer effect results work against our findings.

E. Discussion

The evidence presented so far indicate that the savings treatment dominates the basic treatment both in the middle and long-run. Also, there are clear effects on tertiary outcomes from the savings treatment. These effects cannot be product of conditionalities, since the savings treatment was not conditional on tertiary outcomes.

A potential effective mechanism for the observed effects operates thought the budget constrain: both savings and tertiary treatments can reduce the budget constrain for the relatively poor households at the beginning of the enrollment of tertiary education. This is compatible with the positive effects of on-time enrollment in tertiary education.

As discussed before, we run models that allow for heterogeneity in effects by the bassline income of individuals. Under the assumption that the effects are driven by budget constraints, we expect to see higher effects from low-income individuals, vis-à-vis control groups. We used two proxies of income (the SISBEN score and a measure of income of the household captured in the SISBEN questionnaire). Both measures are correlated among them and they are positively correlated with the main outcomes. For any of the specifications ran, the interaction between (any) treatment and (any) measurement of income was significant. Moreover, all the coefficients were close to zero and with precision. Results are available upon request.

Second, we run, within each treatment group, correlational models between enrollment in tertiary education and income. Among treatment one, income is systematically positively correlated with enrollment; e.g., individuals in treatment one with relatively higher income tend to enrolled more in tertiary education than individuals with relatively lower income. The correlational coefficient between medium-term enrollment and (standardized) income is 0.0193, with a t-value of 3.10, while the coefficient for the long-term enrollment is 0.018 (t-value 2.69). In contrast, the correlation between income and medium-term enrollment among people with savings treatment is 0.0099 (t-value of 0.47) and 0.0068 (t-value of 0.58). The tertiary treatment shows similar results than the secondary treatment.

VI. Conclusion

This paper contributes new evidence to the small literature on long-term effects of CCT program on student outcomes. Building on the original design of Barrera-Osorio et al. (2011) which experimentally manipulates the transfer payment structure and combining additional administrative data sources we show that a revenue-neutral modification that commits families to save a portion of transfers induces students to enroll in tertiary education. This contrasts with the standard CCT payment structure, which only seems to promote educational investments derived from compliance with transfer conditions. A third payment structure that heavily incentivizes tertiary enrollment is effective at encouraging students to enroll in tertiary institutions, but most of the increase is enrollment at lower quality schools.

The differential secondary graduation and tertiary enrollment effects between the savings and basic treatments among upper secondary school students are consistent with

models in which younger students more heavily discount the future. To the extent that this differential discounting is true, the savings treatment may not motivate students in lower secondary grades to progress in the educational ladder as strongly as it does for students in upper secondary.

We strongly reject equality of the medium-term educational impacts of the basic and savings treatments. This difference suggests that, at least among socioeconomically disadvantaged students in Bogota, savings constraints are a barrier to educational attainment. These savings constraints seem to be particularly binding in the transition from secondary to tertiary school, when families presumably need to cover significant, lumpy expenditures. Since the savings treatment does not condition transfers on tertiary enrollment and students make tertiary enrollment decisions after they have stopped receiving transfers, impacts estimates on tertiary enrollment among this group cannot be explained by program's conditionalities.

In theory, standard CCTs could also induce tertiary education investments, despite being outside the period of conditionality. For example, they could signal the importance of educational investments, make education a more salient investment to families or help reveal ability through persistent school enrollment. It seems, however, that while some of these mechanisms may encourage enrollment in lower grades (see, for example, Benhassine et al., 2013), they may be insufficient for helping families bridge the gap to tertiary enrollment.

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Table 1: Match Rates for ICFES and SPADIES Data Sets

	Experiments		
	Both	Basic and Savings	Tertiary
	(1)	(2)	(3)
Panel A: All Students			
Secondary Enrollment			
2006	0.624	0.64	0.559
2007	0.482	0.524	0.304
2008	0.311	0.376	0.033
ICFES Exit Exam	0.716	0.688	0.836
Tertiary Enrollment (SPADIES)			
Medium Term (up until 2012)	0.243	0.213	0.373
Long Term (up until 2016)	0.345	0.322	0.445
Panel B: Upper Secondary (Grades 9-11)			
ICFES Exit Exam	0.81	0.795	0.836
Tertiary Enrollment (SPADIES)			
Medium Term (up until 2012)	0.32	0.29	0.373
Long Term (up until 2016)	0.405	0.382	0.445
Panel C: Lower Secondary (Grades 6-8)			
ICFES Exit Exam	0.617	0.617	
Tertiary Enrollment (SPADIES)			
Medium Term (up until 2012)	0.163	0.163	
Long Term (up until 2016)	0.283	0.283	

Notes: This table displays the match rates between the original registration data and the three administrative data sets used to analyze educational outcomes. The administrative secondary enrollment data covers the period of 2006 through 2008. For the ICFES exit exam data and the SPADIES data we restrict analyses to the years 2005-2012. To match registration records to ICFES and SPADIES data we followed a four-step algorithm: i) Exact match on student ID number, name, and date of birth; ii) For those not matched in (i), exact match on ID and date of birth; iii) For those not matched in (i) or (ii), exact match on ID and names; iii) For those not matched in (i), (ii), or (iii), match on name and date of birth.

Table 2: Differences in the Probability of Available Matching Information

	Any ID Number (1)	Last Name (2)
Panel A: Basic and Savings Treatment		
Basic Treatment	0.002	-0.004
	(0.001)	(0.004)
Savings Treatment	0.002*	-0.008**
	(0.001)	(0.004)
N	10,947	10,947
R^2	< 0.01	< 0.01
Control Mean	0.99	0.98
H_0: Basics vs. Savings		
F-Stat	2.09	0.99
p-value	0.15	0.32
Panel B: Tertiary Treatment		
Tertiary Treatment	< 0.001	0.003
	(< 0.001)	(0.006)
N	2,544	2,544
R^2	< 0.01	< 0.01
Control Mean	1.00	0.98

Notes: This table presents estimates of the differences in the probability that the indicated information is available for matching using Equation (1) with no control variables. Birthdate and first names are not included because the information is available for all students. Standard errors are clustered at the school level. Statistical significance at the one-, five- and ten-percent level is indicated by ***, ** and * respectively.

Table 3: On-Time Enrollment

	Basic and Savings Treatment			Tertiary Treatment			Upper Secondary	Lower Secondary
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Basic Treatment	0.028**	0.028**	0.024*				0.004	0.035**
	(0.013)	(0.012)	(0.013)				(0.017)	(0.015)
Savings Treatment	0.044***	0.041***	0.035***				0.035***	0.034***
	(0.010)	(0.010)	(0.009)				(0.013)	(0.012)
Tertiary Treatment				0.026*	0.025**	0.022*	0.022*	
				(0.016)	(0.012)	(0.013)	(0.012)	
N	9,937	9,937	9,937	2,345	2,345	2,345	6,320	5,962
R^2	< 0.01	0.14	0.19	< 0.01	0.14	0.24	0.22	0.14
Control Mean	0.51	0.51	0.51	0.72	0.72	0.72	0.68	0.42
Socio-Demographic Controls	No	Yes	Yes	No	Yes	Yes	Yes	Yes
School Fixed Effects	No	No	Yes	No	No	Yes	Yes	Yes
Grades at Registration	All	All	All	All	All	All	9-11	6-8
H_0: Basics = Savings								
F-Stat	1.56	1.02	0.94				3.52	0.01
p-value	0.21	0.31	0.33				0.06	0.94
H_0: Basic = Tertiary								
F-Stat							0.68	
p-value							0.41	
H_0: Savings = Tertiary								
F-Stat							0.49	
p-value							0.48	

Notes: This table presents estimates of the treatment effects on on-time enrollment. Students are considered to be enrolled "on-time" if they have not dropped out and have not been held back. All coefficients are estimated using Equation (1) with the indicated control variables. Standard errors are clustered at the school level. Statistical significance at the one-, five- and ten-percent level is indicated by ***, ** and * respectively.

Table 4: Enrollment Outcomes

	Enrollment in Any Grade			On-Time Enrollment				
	2006	2007	2008	2006	2007	2008	Held back	Dropout
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Basic and Savings Treatment								
Basic Treatment	0.012	0.016	0.014	0.019	0.023**	0.018*	-0.009	-0.018
	(0.011)	(0.013)	(0.016)	(0.012)	(0.010)	(0.010)	(0.008)	(0.012)
Savings Treatment	0.031***	0.033***	0.034***	0.038***	0.036***	0.028***	-0.007	-0.032***
	(0.010)	(0.012)	(0.012)	(0.010)	(0.010)	(0.009)	(0.007)	(0.010)
N	9,010	7,601	5,962	9,937	9,937	9,937	9,937	9,937
R^2	0.15	0.16	0.18	0.24	0.34	0.38	0.06	0.20
Control Mean	0.68	0.65	0.57	0.54	0.43	0.30	0.13	0.38
H_0: Basics vs. Savings								
F-Stat	2.46	1.07	2.63	1.87	0.98	1.20	0.05	1.49
p-value	0.12	0.30	0.11	0.17	0.32	0.28	0.82	0.22
Panel B: Tertiary Treatment								
Tertiary Treatment	0.040**	0.044		0.027**	0.019*		0.005	-0.036***
	-0.019	-0.028		-0.013	-0.011		-0.009	-0.014
N	1,747	930		2,345	2,345		2,345	2,345
R^2	0.23	0.24		0.47	0.59		0.11	0.25
Control Mean	0.72	0.69		0.50	0.25		0.05	0.23

Notes: This table presents estimates of the treatment effects on the indicated enrollment measures. "Enrollment in Any Grade" indicates enrollment regardless of whether or not a student was held back. These estimates exclude students who should have graduated had they not been held back. (For example, the estimates for 2006 exclude all students enrolled in grade eleven at registration in 2005.) "On-Time Enrollment" indicates that a student is enrolled and has not been held back as of the indicated year. All coefficients are estimated using Equation (1) with the indicated control variables. Standard errors are clustered at the school level. Statistical significance at the one-, five- and ten-percent level is indicated by ***, ** and * respectively.

Table 5: Taking the ICFES Exam

	Basic and Savings Treatment			Tertiary Treatment			Upper Secondary	Lower Secondary
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Basic Treatment	0.020*	0.023**	0.022**				0.021	0.02
	(0.010)	(0.010)	(0.010)				(0.016)	(0.014)
Savings Treatment	0.011	0.012	0.01				0.028*	0.001
	(0.012)	(0.011)	(0.011)				(0.017)	(0.013)
Tertiary Treatment				0.011	0.01	0.007	0.005	
				(0.014)	(0.015)	(0.015)	(0.014)	
N	10,947	10,947	10,947	2,544	2,544	2,544	6,905	6,586
R^2	< 0.01	0.10	0.12	< 0.01	0.04	0.10	0.09	0.11
Control Mean	0.68	0.68	0.68	0.83	0.83	0.83	0.80	0.61
Socio-Demographic Controls	No	Yes	Yes	No	Yes	Yes	Yes	Yes
School Fixed Effects	No	No	Yes	No	No	Yes	Yes	Yes
Grades at Registration	All	All	All	All	All	All	9-11	6-8
H_0: Basics vs. Savings								
F-Stat	1.50	2.12	2.48				0.28	2.68
p-value	0.22	0.15	0.12				0.59	0.10
H_0: Basic vs. Tertiary								
F-Stat							0.44	
p-value							0.51	
H_0: Savings vs. Tertiary								
F-Stat							0.91	
p-value							0.34	

Notes: This table presents estimates of the treatment effects on an indicator for whether or not a student took the ICFES exit exam. All coefficients are estimated using Equation (1) with the indicated control variables. Standard errors are clustered at the school level. Statistical significance at the one-, five- and ten-percent level is indicated by ***, ** and * respectively.

Table 6: Tertiary Enrollment

	Basic and Savings Treatment			Tertiary Treatment			Upper Secondary	Lower Secondary
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Basic Treatment	0.009	0.01	0.01				0.007	0.012
	(0.009)	(0.009)	(0.009)				(0.016)	(0.013)
Savings Treatment	0.016*	0.017*	0.015*				0.036**	0.006
	(0.009)	(0.009)	(0.009)				(0.014)	(0.013)
Tertiary Treatment				0.058***	0.059***	0.057***	0.058***	
				(0.020)	(0.019)	(0.021)	(0.021)	
N	10,947	10,947	10,947	2,544	2,544	2,544	6,905	6,586
R^2	< 0.01	0.06	0.08	< 0.01	0.05	0.10	0.09	0.06
Control Mean	0.21	0.21	0.21	0.35	0.35	0.35	0.31	0.16
Socio-Demographic Controls	No	Yes	Yes	No	Yes	Yes	Yes	Yes
School Fixed Effects	No	No	Yes	No	No	Yes	Yes	Yes
Grades at Registration	All	All	All	All	All	All	9-11	6-8
H_0: Basics vs. Savings								
F-Stat	1.06	0.97	0.55				6.43	0.56
p-value	0.31	0.33	0.46				0.01	0.46
H_0: Basic vs. Tertiary								
F-Stat							3.23	
p-value							0.07	
H_0: Savings vs. Tertiary								
F-Stat							0.72	
p-value							0.40	

Notes: This table presents estimates of the treatment effects on an indicator for whether or not a student enrolled in a tertiary institution. All coefficients are estimated using Equation (1) with the indicated control variables. Standard errors are clustered at the school level. Statistical significance at the one-, five- and ten-percent level is indicated by ***, ** and * respectively.

Table 7: Effects of Treatments on Tertiary Enrollment by Institution Type

	Basic and Savings				Tertiary			
	Any	University	Vocational	Unclassified	Any	University	Vocational	Unclassified
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Grades 9-11								
Basic Treatment	0.009	0.014	-0.004	-0.001				
	(0.015)	(0.013)	(0.009)	(0.006)				
Savings Treatment	0.034**	0.025*	0.007	0.001				
	(0.014)	(0.014)	(0.008)	(0.008)				
Tertiary Treatment					0.057***	-0.002	0.019	0.040***
					(0.021)	(0.016)	(0.013)	(0.010)
N	4,361	4,361	4,361	4,361	2,544	2,544	2,544	2,544
R^2	0.09	0.07	0.04	0.04	0.10	0.08	0.08	0.08
Control Mean	0.28	0.15	0.10	0.03	0.35	0.21	0.10	0.04
H_0: Basics vs. Savings								
F-Stat	4.89	0.84	1.09	0.20				
p-value	0.03	0.36	0.30	0.66				
Panel B: Grades 6-8								
Basic Treatment	0.012	-0.005	0.012	0.005				
	(0.013)	(0.008)	(0.008)	(0.004)				
Saving Treatment	0.006	0.001	0.004	0.001				
	(0.013)	(0.009)	(0.006)	(0.003)				
N	6,586	6,586	6,586	6,586				
R^2	0.06	0.05	0.05	0.02				
Control Mean	0.16	0.10	0.04	0.02				
H_0: Basics vs. Savings								
F-Stat	0.56	0.90	1.68	0.92				
p-value	0.46	0.34	0.20	0.34				

Notes: This table presents estimates of the treatment effects on an indicator for whether or not a student enrolled the indicated types of tertiary institutions. Higher quality institutions are classified as either universities or vocational training programs, while lower quality programs remain unclassified. All coefficients are estimated using Equation (1) with the indicated control variables. Standard errors are clustered at the school level. Statistical significance at the one-, five- and ten-percent level is indicated by ***, ** and * respectively.

Table 8: Tertiary Enrollment: Long Term (SPADIES 2016)

	Basic and Savings Treatment			Tertiary Treatment			Upper Secondary	Lower Secondary
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Basic Treatment	0.004	0.007	0.006				0.007	0.00632
	(0.011)	(0.010)	(0.010)				(0.018)	(0.013)
Savings Treatment	0.002	0.004	0.002				0.028*	-0.012
	(0.010)	(0.009)	(0.009)				(0.016)	(0.010)
Tertiary Treatment				0.034	0.034	0.026	0.027	
				(0.022)	(0.021)	(0.022)	(0.022)	
N	10,947	10,947	10,947	2,544	2,544	2,544	6,905	6,586
R^2	0.00	0.05	0.08	0.00	0.05	0.10	0.09	0.08
Control Mean	0.32	0.32	0.32	0.43	0.43	0.43	0.40	0.29
Socio-Demographic Controls	No	Yes	Yes	No	Yes	Yes	Yes	Yes
School Fixed Effects	No	No	Yes	No	No	Yes	Yes	Yes
Grades at Registration	All	All	All	All	All	All	9-11	6-8
H_0: Basics vs. Savings								
F-Stat	0.06	0.10	0.18				1.85	2.15
p-value	0.81	0.75	0.67				0.17	0.14
H_0: Basic vs. Tertiary								
F-Stat							0.50	
p-value							0.48	
H_0: Savings vs. Tertiary								
F-Stat							0.00	
p-value							0.99	

Notes: This table presents estimates of the treatment effects on an indicator for whether or not a student enrolled in a tertiary institution, cut up to 2016. All coefficients are estimated using Equation (1) with the indicated control variables. Standard errors are clustered at the school level. Statistical significance at the one-, five- and ten-percent level is indicated by ***, ** and * respectively.

Table 9: On time enrollment, tertiary education (SPADIES 2016)

	Basic and Savings Treatment			Tertiary Treatment			Upper Secondary	Lower Secondary
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Basic Treatment	0.003	0.00538	0.00461				0.01	0.001
	(0.009)	(0.009)	(0.009)				(0.015)	(0.011)
Savings Treatment	0.008	0.00946	0.00762				0.039***	-0.01
	(0.009)	(0.008)	(0.009)				(0.014)	(0.011)
Tertiary Treatment				0.034**	0.036**	0.031*	0.032*	
				(0.017)	(0.017)	(0.018)	(0.018)	
N	10,947	10,947	10,947	2,544	2,544	2,544	6,905	6,586
R^2	0.00	0.03	0.05	0.00	0.04	0.09	0.08	0.06
Control Mean	0.18	0.18	0.18	0.24	0.24	0.24	0.21	0.18
Socio-Demographic Controls	No	Yes	Yes	No	Yes	Yes	Yes	Yes
School Fixed Effects	No	No	Yes	No	No	Yes	Yes	Yes
Grades at Registration	All	All	All	All	All	All	9-11	6-8
H_0: Basics vs. Savings								
F-Stat	0.45	0.34	0.18				4.27	2.09
p-value	0.50	0.56	0.67				0.04	0.15
H_0: Basic vs. Tertiary								
F-Stat							0.71	
p-value							0.40	
H_0: Savings vs. Tertiary								
F-Stat							0.11	
p-value							0.75	

Notes: This table presents estimates of the treatment effects on an indicator for whether or not a student enrolled on time in a tertiary institution. It is constructed using a 2 year window (after graduation) and it is not conditional on enrollment. All coefficients are estimated using Equation (1) with the indicated control variables. Standard errors are clustered at the school level. Statistical significance at the one-, five- and ten-percent level is indicated by ***, ** and * respectively.

Table 10: Teritary Graduation (SPADIES 2016)

	Basic and Savings Treatment			Tertiary Treatment			Upper Secondary	Lower Secondary
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Basic Treatment	0.004	0.005	0.00586				0.0162*	0.00106
	(0.005)	(0.005)	(0.005)				(0.010)	(0.006)
Savings Treatment	0.009	0.01	0.0104				0.019*	0.00553
	(0.007)	(0.007)	(0.007)				(0.011)	(0.007)
Tertiary Treatment				0.012	0.012	0.011	0.011	
				(0.014)	(0.014)	(0.014)	(0.014)	
N	10,947	10,947	10,947	2,544	2,544	2,544	6,586	6,586
R^2	0.00	0.02	0.04	0.00	0.03	0.09	0.06	0.03
Control Mean	0.06	0.06	0.06	0.11	0.11	0.11	0.10	0.05
Socio-Demographic Controls	No	Yes	Yes	No	Yes	Yes	Yes	Yes
School Fixed Effects	No	No	Yes	No	No	Yes	Yes	Yes
Grades at Registration	All	All	All	All	All	All	9-11	6-8
H_0: Basics vs. Savings								
F-Stat	0.87	0.78	0.60				0.08	0.38
p-value	0.35	0.38	0.44				0.77	0.54
H_0: Basic vs. Tertiary								
F-Stat							0.09	
p-value							0.77	
H_0: Savings vs. Tertiary								
F-Stat							0.22	
p-value							0.64	

Notes: This table presents estimates of the treatment effects on an indicator for whether or not a student graduated from a tertiary institution (not conditional on tertiary enrollment). All coefficients are estimated using Equation (1) with the indicated control variables. Standard errors are clustered at the school level. Statistical significance at the one-, five- and ten-percent level is indicated by ***, ** and * respectively.

Table 11: Joint Hypothesis Tests

Hypothesis	Medium Term (1)	Long Term (2)
H_0: Basic = 0		
Chi^2	6.811	8.253
p-value	0.078	0.143
H_0: Savings = 0		
Chi^2	21.703	21.683
p-value	< 0.001	0.001
H_0: Tertiary = 0		
Chi^2	15.581	7.311
p-value	0.001	0.198
H_0: Basic = Savings		
All Students		
Chi^2	15.185	20.057
p-value	0.019	0.029
Upper Secondary (Grades 9-11)		
Chi^2	7.513	8.18
p-value	0.057	0.147
Lower Secondary (Grades 6-8)		
Chi^2	3.566	11.209
p-value	0.312	0.047

Notes: This table presents the results of joint hypothesis tests for treatment effects on different outcomes, depending on the temporal horizont. For the middle term effects, the three primary outcome variables are on-time secondary enrollment, taking the ICFES exit exam from high school, and enrollment in a tertiary institution in the medium term (Column 1). For the long term effects, the outcomes are on-time secondary enrollment, taking the ICFES exit exam from high school, long term enrollment in a tertiary institution, on-time tertiary enrollment and tertiary graduation (Column 2). All coefficients for the tests are estimated using Equation (1) in a Seemingly Unrelated Regressions model. The equations used to estimate the coefficients for the tests of equality between the basic and savings treatments also include indicator variables for secondary school level (i.e. whether students were enrolled in upper secondary at registration). Standard errors are clustered at the school level. Statistical significance at the one-, five- and ten-percent level is indicated by ***, ** and * respectively.

Appendix A: Subjects Missing Enrollment Data

As noted in Section III, the information required to match the enrollment data to our original registration data was not available for all students. As a result, the sample of students used to estimate the treatment effects for enrollment outcomes is slightly smaller than the full sample used to estimate the treatment effects for the other outcomes. Excluding those students, however, does not significantly change the estimates. In Tables A1 and A2, we replicate Tables 5 and 6 using only those students for whom information was available for the enrollment match. The estimated treatment effects are consistent with those estimated using the entire sample.

Table A1: Taking the ICFES Exam, Excluding Students Missing Enrollment Matching Information

	Basic and Savings Treatment			Tertiary Treatment			Upper Secondary	Lower Secondary
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Basic Treatment	0.023**	0.024**	0.024**				0.025	0.019
	(0.011)	(0.011)	(0.011)				(0.019)	(0.014)
Savings Treatment	0.016	0.015	0.013				0.027	0.005
	(0.011)	(0.011)	(0.011)				(0.017)	(0.013)
Tertiary Treatment				0.018	0.016	0.015	0.013	
				(0.015)	(0.014)	(0.014)	(0.013)	
N	9,937	9,937	9,937	2,345	2,345	2,345	6,320	5,962
R^2	< 0.01	0.17	0.19	< 0.01	0.06	0.13	0.11	0.20
Control Mean	0.68	0.68	0.68	0.84	0.84	0.84	0.81	0.61
Grades at Registration	All	All	All	All	All	All	9-11	6-8
H_0: Basics vs. Savings								
F-Stat	0.62	1.18	1.6				0.01	1.87
p-value	0.43	0.28	0.21				0.92	0.17
H_0: Basic vs. Tertiary								
F-Stat							0.24	
p-value							0.62	
H_0: Savings vs. Tertiary								
F-Stat							0.35	
p-value							0.56	

Notes: This table presents estimates of the coefficients presented in Table 5, while omitting subjects without sufficient information to match to the enrollment data. Standard errors are clustered at the school level. Statistical significance at the one-, five- and ten-percent level is indicated by ***, ** and * respectively.

Table A2: Studying at any Tertiary Institution, Excluding Students Missing Enrollment Matching Information

	Basic and Savings Treatment			Tertiary Treatment			Upper Secondary	Lower Secondary
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Basic Treatment	0.014	0.013	0.014				0.012	0.015
	(0.009)	(0.008)	(0.009)				(0.016)	(0.014)
Savings Treatment	0.018*	0.018**	0.016*				0.038***	0.005
	(0.009)	(0.008)	(0.009)				(0.013)	(0.013)
Tertiary Treatment				0.066***	0.068***	0.066***	0.065***	
				(0.021)	(0.020)	(0.021)	(0.021)	
N	9,937	9,937	9,937	2,345	2,345	2,345	6,320	5,962
R^2	< 0.01	0.07	0.09	< 0.01	0.07	0.12	0.12	0.07
Control Mean	0.21	0.21	0.21	0.35	0.35	0.35	0.31	0.16
Grades at Registration	All	All	All	All	All	All	9-11	6-8
H_0: Basics vs. Savings								
F-Stat	0.27	0.27	0.08				3.55	0.96
p-value	0.60	0.61	0.78				0.06	0.33
H_0: Basic vs. Tertiary								
F-Stat							3.56	
p-value							0.06	
H_0: Savings vs. Tertiary								
F-Stat							1.24	
p-value							0.27	

Notes: This table presents estimates of the coefficients presented in Table 6, while omitting subjects without sufficient information to match to the enrollment data. Standard errors are clustered at the school level. Statistical significance at the one-, five- and ten-percent level is indicated by ***, ** and * respectively.

Appendix B: Internal Validity

Despite the small differences between groups and the high rates at which the information is available, we assess whether or not information is differentially available for particular types of students. Tables B1 and B2 conduct the analysis for students with valid ID information and full last name, respectively. For each, we use the control variables to compare the average composition of subjects for which the respective information is available in each research group. All of the differences in these tables are practically very small, and none are statistically significant. These results are consistent with the low levels of missing information and the individual, student-level randomization.

Table B1: Comparison of Students with Valid ID's

	Basic-Savings				Tertiary	
Demographic Variable	**Control Average**	**Basic-Control**	**Savings-Control**	**Basic-Savings**	**Control Average**	**Tertiary-Control**
Panel A: Indexes of Household Assets						
Possessions	1.896	0.066	0.033	0.033	1.941	-0.044
	(1.099)	(0.020)	(0.023)	(0.024)	(1.019)	(0.043)
Utilities	4.654	-0.017	0.062	-0.079	4.848	0.049
	(1.418)	(0.030)	(0.031)	(0.034)	(1.315)	(0.041)
Durable Goods	1.373	-0.027	0.006	-0.032	1.635	0.015
	(0.896)	(0.019)	(0.021)	(0.022)	(0.858)	(0.034)
Physical Infrastructure	11.657	-0.052	0.041	-0.094	12.142	-0.053
	(1.756)	(0.035)	(0.029)	(0.040)	(1.486)	(0.064)
Panel B: Individual Characteristics						
Age	14.374	0.092	-0.064	0.156	15.666	-0.066
	(5.293)	(0.106)	(0.143)	(0.170)	(4.230)	(0.194)
Gender	0.495	0.005	-0.005	0.010	0.454	-0.009
	(0.500)	(0.012)	(0.010)	(0.010)	(0.498)	(0.018)
Years of Education	5.612	-0.025	-0.004	-0.021	7.428	-0.051
	(1.855)	(0.038)	(0.050)	(0.041)	(1.344)	(0.052)
Grade	8.084	-0.004	-0.002	-0.002	9.849	-0.002
	(1.626)	(0.035)	(0.048)	(0.042)	(0.792)	(0.028)
Panel C: Household Characteristics						
Single Head	0.297	0.017	0.010	0.006	0.271	0.008
	(0.457)	(0.010)	(0.010)	(0.012)	(0.445)	(0.016)
Age of Head	45.917	-0.081	0.136	-0.217	46.211	0.252
	(10.271)	(0.176)	(0.228)	(0.212)	(8.591)	(0.286)
Years of Ed, Head	5.654	-0.103	-0.170	0.068	5.940	-0.124
	(2.940)	(0.078)	(0.066)	(0.066)	(2.936)	(0.096)
People in Household	5.416	-0.042	-0.020	-0.022	5.158	-0.006
	(2.005)	(0.046)	(0.050)	(0.040)	(1.775)	(0.068)
Member under 18	2.569	0.029	0.015	0.015	2.310	0.045
	(1.354)	(0.032)	(0.026)	(0.028)	(1.199)	(0.055)
Panel D: Poverty Measures						
Strata	1.445	-0.010	0.022	-0.032	1.632	-0.002
	(0.828)	(0.017)	(0.019)	(0.018)	(0.767)	(0.028)
SISBEN Score	11.771	-0.121	-0.027	-0.094	13.450	0.041
	(4.647)	(0.085)	(0.115)	(0.096)	(4.333)	(0.176)
Household Income	366.398	-4.474	0.368	-4.842	399.592	4.131
(1,000 Pesos)	(240.865)	(5.642)	(5.912)	(6.214)	(236.795)	(7.924)

Note: This table presents a comparison of students in each of the listed research groups for whom a valid ID is available for matching. Columns one and five contain the average characteristics of the respective control students while columns two, three, four, and six contain the average difference between the respective control students and treatment students. Panel A contains indices of household assets (positive values indicate wealthier families). Panel B contains individual student characteristics, and Panel C contains characteristics of the students' families. Panel D contains poverty measures available in the SISBEN data set. This includes the "strata" number which is a geographic measure of poverty as well as the SISBEN score which is a continuous score used to classify households for various social programs. All coefficients are estimated using Equation (1) with no control variables. Standard errors are clustered at the school level. Statistical significance at the one-, five- and ten-percent level is indicated by ***, ** and * respectively.

Table B2: Comparison of Students with Valid Last Names

	Basic-Savings				Tertiary	
Demographic Variable	**Control Average**	**Basic-Control**	**Savings-Control**	**Basic-Savings**	**Control Average**	**Tertiary-Control**
Panel A: Indexes of Household Assets						
Possessions	1.896	0.066	0.033	0.033	1.941	-0.044
	(1.099)	(0.020)	(0.023)	(0.024)	(1.019)	(0.043)
Utilities	4.654	-0.017	0.062	-0.079	4.848	0.049
	(1.418)	(0.030)	(0.031)	(0.034)	(1.315)	(0.041)
Durable Goods	1.373	-0.027	0.006	-0.032	1.635	0.015
	(0.896)	(0.019)	(0.021)	(0.022)	(0.858)	(0.034)
Physical Infrastructure	11.657	-0.052	0.041	-0.094	12.142	-0.053
	(1.756)	(0.035)	(0.029)	(0.040)	(1.486)	(0.064)
Panel B: Individual Characteristics						
Age	14.374	0.092	-0.064	0.156	15.666	-0.066
	(5.293)	(0.106)	(0.143)	(0.170)	(4.230)	(0.194)
Gender	0.495	0.005	-0.005	0.010	0.454	-0.009
	(0.500)	(0.012)	(0.010)	(0.010)	(0.498)	(0.018)
Years of Education	5.612	-0.025	-0.004	-0.021	7.428	-0.051
	(1.855)	(0.038)	(0.050)	(0.041)	(1.344)	(0.052)
Grade	8.084	-0.004	-0.002	-0.002	9.849	-0.002
	(1.626)	(0.035)	(0.048)	(0.042)	(0.792)	(0.028)
Panel C: Household Characteristics						
Single Head	0.297	0.017	0.010	0.006	0.271	0.008
	(0.457)	(0.010)	(0.010)	(0.012)	(0.445)	(0.016)
Age of Head	45.917	-0.081	0.136	-0.217	46.211	0.252
	(10.271)	(0.176)	(0.228)	(0.212)	(8.591)	(0.286)
Years of Ed, Head	5.654	-0.103	-0.170	0.068	5.940	-0.124
	(2.940)	(0.078)	(0.066)	(0.066)	(2.936)	(0.096)
People in Household	5.416	-0.042	-0.020	-0.022	5.158	-0.006
	(2.005)	(0.046)	(0.050)	(0.040)	(1.775)	(0.068)
Member under 18	2.569	0.029	0.015	0.015	2.310	0.045
	(1.354)	(0.032)	(0.026)	(0.028)	(1.199)	(0.055)
Panel D: Poverty Measures						
Strata	1.445	-0.010	0.022	-0.032	1.632	-0.002
	(0.828)	(0.017)	(0.019)	(0.018)	(0.767)	(0.028)
SISBEN Score	11.771	-0.121	-0.027	-0.094	13.450	0.041
	(4.647)	(0.085)	(0.115)	(0.096)	(4.333)	(0.176)
Household Income	366.398	-4.474	0.368	-4.842	399.592	4.131
(1,000 Pesos)	(240.865)	(5.642)	(5.912)	(6.214)	(236.795)	(7.924)

Note: This table presents a comparison of students in each of the listed research groups for whom a valid last name is available for matching. Columns one and five contain the average characteristics of the respective control students while columns two, three, four, and six contain the average difference between the respective control students and treatment students. Panel A contains indices of household assets (positive values indicate wealthier families). Panel B contains individual student characteristics, and Panel C contains characteristics of the students' families. Panel D contains poverty measures available in the SISBEN data set. This includes the "strata" number which is a geographic measure of poverty as well as the SISBEN score which is a continuous score used to classify households for various social programs. All coefficients are estimated using Equation (1) with no control variables. Standard errors are clustered at the school level. Statistical significance at the one-, five- and ten-percent level is indicated by ***, ** and * respectively.