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MEDIUM- AND LONG-TERM EDUCATIONAL CONSEQUENCES OF
ALTERNATIVE CONDITIONAL CASH TRANSFER DESIGNS:
EXPERIMENTAL EVIDENCE FROM COLOMBIA

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

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ABSTRACT

We show that three Colombian conditional cash transfer (CCT) programs for secondary schools improve educational outcomes eight and 12 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. Traditionally structured bimonthly transfers improve on-time enrollment and high school exit exam completion rates in the medium term, but do not affect long-term tertiary outcomes. A delayed transfer that directly incentivizes tertiary enrollment promotes secondary school on-time enrollment and enrollment—only in lower-quality tertiary institutions—in the medium term but not the long term.

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Introduction

Conditional cash transfers (CCTs) are one of the most prevalent and fastest-growing social assistance programs in the developing world. The number of countries operating CCTs worldwide doubled between 2008 and 2016 (Garcia & Saavedra 2016). 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). This paper provides experimental estimates of medium- and long-term effects – between eight and 12 years after initial receipt – relative to a pure experimental control group that throughout the period does not receive transfers. To the best of our knowledge, this is the first paper to document experimentally the effects on tertiary enrollment and how these medium- and long-term impacts may vary with program design.¹

In 2005, the government of Bogota, Colombia, in collaboration with a subset of co-authors and practitioners, randomized three types of CCT payment structures targeted at aiding socioeconomically disadvantaged students—ages 14 to 16 at baseline—attend and complete secondary schooling (Barrera *et al.*, 2011).

In one experiment, we compare two payment structures relative to a control group that receives no transfer. In the first payment structure, families are forced to save one-third of a bimonthly conditional transfer until the following academic year (the “savings” treatment). The second payment structure is a standard CCT payment scheme providing a

¹ There is very limited evidence on the long-term educational effects of CCTs. Filmer and Schady (2014) employed an RD design to estimate effect of a three-year CCT offer to secondary school students in Cambodia to show increases in grade attainment (no impacts on test-scores, employment or earnings). Baez & Camacho (2011) and Behrman *et al.* (2010) employed non-experimental research designs for Colombia’s *Familias en Accion* and Mexico’s *Oportunidades*. Barham *et al.* (2013) used the randomized phase-in of Nicaragua’s *Red de Proteccion Social* program.

bimonthly transfer conditional on enrollment and attendance (the “basic” treatment). In another experiment we evaluate, relative to a different control group, another variant of the payment structure in which students receive a conditional bimonthly transfer and a monetary incentive for secondary graduation and tertiary enrollment (the “tertiary” treatment).

We combine randomization data with various sources of national administrative data on secondary and tertiary education enrollment and completion, eight and 12 years after randomization—when students are between 26 and 28 years old. We document that the “savings” treatment affects more and longer-term educational outcomes—particularly tertiary enrollment—than the “basic” treatment. The “tertiary treatment,” on the other hand, promotes tertiary enrollment only in lower-quality tertiary institutions—a potentially unintended consequence of the “tertiary” treatment’s high-powered incentives to comply with the transfer’s conditions.

Across upper and lower secondary students, the savings treatment increases the probability of on-time secondary school enrollment by 3.5 percentage points (base is 51 percent), due mostly to a 3.2 percentage point reduction in the probability that students drop out (base is 38 percent). For upper secondary students (grades nine through 11), 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 (12 years after randomization) by 2.8 percentage points (base of 40 percent).² Tertiary enrollment effects of the “savings” treatment concentrate on university enrollment.

² These findings are consistent with those of Karlan and Linden (2014), who show educational outcomes can be improved by weaker savings commitments not requiring families to spend money on specific types of goods.

The “basic” treatment increases on-time enrollment in secondary school and the probability of taking the secondary graduation exam by 2.2 percentage points (base is 68 percent). The “basic” treatment does not affect students’ likelihood of enrolling in tertiary education 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 (base is 72 percent) by reducing dropout rates by 3.6 percentage points (base of 23 percent). Secondary graduation is not affected, but enrollment only in lower-quality tertiary institutions improves by 5.8 percentage points (base of 31 percent) eight years after enrolling in the program.

The “tertiary” treatment’s tertiary enrollment effects are entirely consistent with the incentives induced by the transfer conditions. The “savings” treatment’s effects on tertiary enrollment—particularly among upper secondary students—are not the result of the transfer’s conditionality. First, unlike the “tertiary” treatment, the “savings” treatment does not incentivize tertiary enrollment. Second, a dynamic model of educational decisions would predict that the transfer’s conditionality has small (or even negative) effects of the program on student effort for upper secondary students, for whom the prospect of future transfers is negligible—as is the case in the “savings” treatment.

Tertiary education effects among upper secondary students in the “savings” treatment are also not likely the result of cohort composition or relaxation of liquidity constraints. Instead, we conjecture that by targeting upper secondary students and potentially waiting until ability is better revealed, the program may be operating through

a “scholarship” model – rewarding students who successfully transition from lower to upper secondary and may be more inclined to pursue tertiary education. With only one experiment, however, we cannot make a definitive statement about the mechanisms at play.

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; present results in Section V; and conclude in Section VI.

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

In 2005, Colombia’s capital city Bogota established the Conditional Subsidies for School Attendance (*Subsidios Condicionados a la Asistencia Escolar*) pilot program 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 out of the 20 in Bogotá.

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 typically accompanying CCTs targeting younger children.³ Unlike many other conditional cash transfer programs, the SED intended this pilot to be a policy

³ Reviews of these programs are Baird *et al.* (2013), Garcia & Saavedra (2016) and Fiszbein & Schady (2009).

experiment in which it would test three alternatives. In all treatments, students are required to attend at least 80 percent of school days during each payment period.⁴ Students are removed from the program if they twice failed to matriculate to the following grade, fail to reach the attendance target in two successive payment periods or are expelled from school.

Eligibility is based on several criteria. Applicants must have finished the fifth (San Cristobal) or eighth (Suba) grade (last grade of primary) and they must be enrolled in a secondary school. The applicant's family must demonstrate they had been designated as impoverished based on the national poverty assessment tool, SISBEN.⁵ Applicants also must present a valid national identification card (which almost all students in Bogota have) to validate their poverty status at the time of registration. Finally, to prevent families from moving to obtain eligibility, eligible to participate are only families classified by the SISBEN system as living in San Cristobal or Suba prior to 2004.

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

⁴ 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.

⁵ Families had to present their SISBEN card and be ranked in the lowest two categories of the system's six.

The savings treatment is designed to be a revenue-neutral experimental variant of the basic treatment.⁶ Compared to the basic treatment, the payment structure in the savings treatment differs. In the savings treatment, instead of receiving \$30 for reaching the attendance target over two months, students are paid \$20, with the remaining \$10 held in a bank account. The accumulated funds – up to \$50 per school year for students in full compliance – are 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 can potentially provide a means of bypassing short-term liquidity constraints when paying enrollment expenses.

In Suba, eligible secondary school students entering upper secondary school (grades nine through 11) randomly are assigned to a “tertiary” treatment group or a control group. As in the savings treatment, participants in the tertiary treatment are paid a basic transfer of \$20 every two months but they also are eligible for incentives through secondary school graduation and tertiary enrollment. Students successfully graduating from secondary school are eligible for a lump-sum transfer of \$300. Students receive the funds immediately upon documenting enrollment in a tertiary education institution.⁷ If students fail to enroll in higher education, they still receive the transfers but only after a year’s wait. Therefore, the (dis-)incentive in the “tertiary” treatment is only the delay of payment, not whether the payment was made. While cost-equivalent to the basic treatment for students going through six years of secondary education, the tertiary treatment is more generous than the basic treatment because in practice – due to an

⁶ Both treatments are exactly revenue-neutral in the absence of inflation. In practice, inflation during the 2000-06 period was 5.6 percent (World Bank, 2014c).

⁷ 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).

administrative decision from part of the SED – it was offered only to students that were, at baseline, three years or less from graduation.⁸

Assignment in both localities was contingent on oversubscription. 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.⁹ Interested applicants had to register during a 15-day window in February-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 public lotteries in each locality on April 4, 2005. The algorithm stratified by locality (San Cristobal or Suba), school type (public or private), grade (6th through 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 lotteries.

Barrera-Osorio *et al.* (2011) documented that one year after randomization of students into treatments, all treatments significantly increased 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

⁸ 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 the effect on dropout rates is statistically significant. These results are available upon request.

⁹ 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.

had no effect. Similarly, the savings and tertiary treatments increased tertiary enrollment after one year of treatment for students who were enrolled in grade 11 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 effecting heterogeneity by grade or age.

II. 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:¹⁰

1. Program registration data: This dataset contains identification numbers for the 13,433 eligible applicants in the two experiments, which we used to match with the other data sets described below. It also includes information on the school and grade in which students were enrolled at the time of the lottery.
2. SISBEN: At baseline, we matched applicant records to Colombia's 2003-04 *Sistema de Identificación y Clasificación de Potenciales Beneficiarios para*

¹⁰ 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.

Programas Sociales (SISBEN), also known as the census survey of the poor. We use the data as baseline socio-demographic controls because it was collected prior to the randomization.

3. Secondary school enrollment records: To measure secondary school enrollment, we used annual administrative data from SED.¹¹ The data are similar to those used in Barrera-Osorio *et al.* (2011) but includes information from 2006-08.¹² These data includes an indicator for whether a student was 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.¹³
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 because more than 95 percent of all secondary school students take the exam (Bettinger *et al.* 2016; Angrist *et al.* 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 2005-12, a maximum of eight years after the beginning of the treatment.

¹¹ The data includes 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 did not report and schools who reported but did not have any enrolled students in our sample, only 55 students (0.4 percent of the sample) attended schools in this group in 2006.

¹² 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 but was only available for 2006. 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.

¹³ 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.

5. SPADIES: To track tertiary enrollment, we use data from the Colombian Ministry of Education’s *Sistema 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, similar to the National Student Clearinghouse in the United States, covers 95 percent of the post-secondary population in Colombia. SPADIES contains information on the timing and university of students’ initial enrollments and the types of institution. Higher-quality institutions are classified as either universities or vocational schools, while lower-quality institutions remain unclassified.¹⁴ We use two cuts of the SPADIES data: one that covers collegiate pathways from 2005-12 – up to eight years after the start of the program, or “medium-term;” and one that covers collegiate pathways up to 2016 – up to 12 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 follow 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 name;
4. For those not matched in (i), (ii) or (iii), match on name and date of birth.

¹⁴ 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 from 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 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 (approximately 17 percent) graduates each year. The actual reduction in matches in 2007-08 is consistent with the expected repetition and dropout rates (Panel A of 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 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 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:

$$Y_{ij}=b_0 + \mathbf{b}_t' \mathbf{Treatment}_i + \mathbf{b}_x' \mathbf{X}_i + e_{ij} \quad (1)$$

where Y_{ij} is an outcome variable for applicant i in school j , and $Treatment_i$ 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 $Treatment_i$ 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 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, household head's age, household 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 pool estimates from the San Cristobal and Suba samples. To do this, and given that the Suba experiment only covers grades nine through 11, we restrict the sample to applicants in grades nine through 11 at baseline and include a district-fixed effect to account for mean level differences, such as disparities in the probability of treatment assignment between samples. Recall, however, that the San Cristobal and Suba experiments are independent of each other. Hence, just as 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-fixed effects rather than purely random variation. Pooling the San Cristobal and Suba samples for grades nine through 11 is empirically justified given the similarities in baseline characteristics across the two groups of students (Barrera-Osorio *et al.* 2008 presents a detailed comparison).

III. 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 many reasons. First, in all 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 used to match the data from 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, respectively, 99.4 and 97.8 percent of them. For variables in which information is missing, we show in Table 2 (using Equation (1)) that the availability of this information is evenly distributed across 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.

IV. 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 students are enrolled "on time" in each academic year 2006, 2007 and 2008.¹⁵ For each student, the indicator variable for on-time enrollment is set to one if the student has not dropped out

¹⁵ In Colombian public schools and non-elite private schools, the academic year runs from February through December.

and has not been held back.¹⁶

In the basic and savings experiment we find that the basic treatment increases on-time enrollment by 2.4 percentage points (base of 51 percent). This difference is statistically significant at the 10 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 is statistically significant at the 1 percent level (column three). The estimate on the tertiary treatment also is positive (2.2 percentage points) and statistically significant at the 10 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 (column seven, Table 3). For these older students, the effect of the basic treatment falls while 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 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

¹⁶ Specifically, we consider a student as enrolled on time if the student is enrolled and $(\text{analysis year} - 2005) = (\text{grade in analysis year} - \text{grade at baseline})$. 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.

three.^{17,18} By this measure, 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 for the on-time enrollment by year outcome (columns four through six). With on-time enrollment, 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, by separately measuring whether students were held back or dropped out. The on-grade enrollment effect is largely explained by a reduction in dropouts. Dropout rates fall by 3.2 percentage points in the savings treatment and 3.6 percentage points in the tertiary treatment. None of the treatments affect the likelihood of 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 again are consistent across specifications. The basic and savings treatments have comparable effects among students in upper secondary at baseline, and the effect for the savings treatment is statistically significant at the 10 percent level. Despite the small estimate for the tertiary treatment, we cannot reject equality between either the basic or savings and the tertiary treatment effects. For lower secondary students, the basic

¹⁷ For these estimates, we exclude students who would have graduated had they not been held back. So, for example, the estimates for 2006 excluded students enrolled in grade 11 at registration in 2005.

¹⁸ The estimates for 2006 also provides the opportunity to compare the results using the new data set to the results obtained from the previous data. The estimated treatment effects are 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.

treatment estimate is larger than the estimate for the savings treatment although neither effect is statistically significant.

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 (results not shown, available upon request).

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 captures 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 (base rate of 21 percent), statistically significant at the 10 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 (base rate of 35 percent).

Among students who were in grades nine through 11 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 1 percent level. We also can reject the null hypothesis of equality of tertiary enrollment

effects between tertiary and basic treatments, but not between savings and tertiary treatments.^{19, 20}

Students in the savings and tertiary treatments enroll in different types of tertiary education institutions (Table 7). For upper secondary students, the savings treatment encourages enrollment in universities (rather than vocational schools or the lower-quality unclassified schools). The tertiary treatment, on the other hand, solely encourages enrollment in unclassified tertiary institutions. It may be that the high-powered incentives encourage students to enroll more indiscriminately.

Basic and savings treatments do not increase tertiary enrollment in the medium term for lower secondary students. The difference in tertiary enrollment effects is not due to differences in cohort composition across lower and upper secondary grades. Point estimates (not shown, available upon request) are unchanged when we limit our sample to only include students in San Cristobal who were either in grade 9 or greater at baseline or who successfully reached grade 9 thereafter.

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 the students are college-bound, this long-term dataset enable us to observe

¹⁹ When estimating 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 11 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.

²⁰ For the tertiary treatment, we have significantly fewer grade levels to exploit. However, we do find 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.

collegiate pathways for students from all grades at the time of randomization.

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 grade levels, the basic and savings treatments do not 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 restricting 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 absence of tertiary enrollment effects for lower secondary students suggests that the treatments—particularly savings and tertiary—interact with students’ grade progression through secondary school. We explore

potential reasons for why might be the case below in subsection E below.

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

One potential mechanism for observed effects operates through relaxation of liquidity constraints. Both savings and tertiary treatments may be able to relax liquidity constraints for relatively poor households when they make tertiary education enrollment decisions. If this were the mechanism at play we would expect to see stronger effects among the poorest participants. We find no evidence of an interaction effect between any of the treatments and two alternative definitions of income. Estimates of the interaction term are precisely estimated zeros (results not shown, available upon request).

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. 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 estimations are performed using Equation (1) with a Seemingly Unrelated Regressions model. The results are presented in Table 11.

In the medium term, overall effects of all treatments are statistically significant (Column 1, Table 11). The p-values on the savings and tertiary treatments are less than 1 percent. The savings treatment also is statistically significant at the 10 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.²¹ Overall, we reject the equality of treatment effects with a p-value of 0.019. Upper secondary students drive this result – consistent with documented differences in previous sections. We reject equality with a p-value of 0.057 for upper secondary students – but for lower secondary students, the p-value is 0.312.

In the long term, we find that the savings treatment is the only treatment for which we 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 savings treatment (p-value 0.029). When we separate the test for lower and upper grades, lower grades drive the difference in results between basic and savings treatments (p-value of 0.047). The respective test for upper secondary grades has a p-value of 0.147.

²¹ The p-value on a joint test of the significance of the interaction term is 0.0634.

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

V. Conclusion

This paper contributes new evidence to the small literature on long-term effects of CCT programs on students' educational outcomes. Building on the original design of Barrera-Osorio *et al.* (2011), which experimentally manipulated the transfer payment structure, and combining additional administrative data sources, we find students are induced to enroll in tertiary education through a revenue-neutral modification that commits families to save a portion of transfers. This contrasts with the standard CCT payment structure, which only promotes educational investments derived from compliance with transfer conditions. A third payment structure that incentivizes tertiary enrollment is effective at encouraging students to enroll in tertiary institutions, but the increase is due to enrollment at lower-quality schools, which is a potentially unintended consequence of the high-powered incentives to comply with conditions.

The savings treatment, on the other hand, does not condition transfers on tertiary enrollment and students make tertiary enrollment decisions after they stop receiving

transfers, suggesting that impact estimates on tertiary enrollment among this group cannot be explained by compliance with conditions. Furthermore, the concentration of tertiary enrollment effects among upper secondary students in the savings treatment is inconsistent with predictions on the role of conditions in dynamic models of educational decisions. These models (e.g. Dubois, de Janvry & Sadoulet 2012) suggest that transfer conditions generate minimal incentives for students in upper secondary school to exert effort—particularly beyond the period of conditionality—since the prospect of future transfers is negligible.

The differential secondary graduation and tertiary enrollment effects between the savings and basic treatments among upper secondary school students may be consistent with a “scholarship model” of transfers. By targeting upper secondary students and potentially waiting until ability is better revealed, the program may be rewarding students who successfully transition from lower to upper secondary and may be more inclined to pursue tertiary education. However, with only one experiment we cannot make definitive statements and future research may be able to shed more precise light on the mechanisms at play.

For instance, standard CCTs could potentially induce educational investments beyond the period of conditionality by revealing ability and returns to effort 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), at least in the case of Bogota’s CCT program, 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 (1)	Basic and Savings (2)	Tertiary (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 (0.001)	-0.004 (0.004)
Savings Treatment	0.002* (0.001)	-0.008** (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 (< 0.001)	0.003 (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	Lower
	(1)	(2)	(3)	(4)	(5)	(6)	Secondary	Secondary
Basic Treatment	0.028** (0.013)	0.028** (0.012)	0.024* (0.013)				0.004 (0.017)	0.035** (0.015)
Savings Treatment	0.044*** (0.010)	0.041*** (0.010)	0.035*** (0.009)				0.035*** (0.013)	0.034*** (0.012)
Tertiary Treatment				0.026* (0.016)	0.025** (0.012)	0.022* (0.013)	0.022* (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			Held back	Dropout
	2006	2007	2008	2006	2007	2008		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Basic and Savings Treatment								
Basic Treatment	0.012 (0.011)	0.016 (0.013)	0.014 (0.016)	0.019 (0.012)	0.023** (0.010)	0.018* (0.010)	-0.009 (0.008)	-0.018 (0.012)
Savings Treatment	0.031*** (0.010)	0.033*** (0.012)	0.034*** (0.012)	0.038*** (0.010)	0.036*** (0.010)	0.028*** (0.009)	-0.007 (0.007)	-0.032*** (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** -0.019	0.044 -0.028		0.027** -0.013	0.019* -0.011		0.005 -0.009	-0.036*** -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 (0.009)	0.01 (0.009)	0.01 (0.009)				0.007 (0.016)	0.012 (0.013)
Savings Treatment	0.016* (0.009)	0.017* (0.009)	0.015* (0.009)				0.036** (0.014)	0.006 (0.013)
Tertiary Treatment				0.058*** (0.020)	0.059*** (0.019)	0.057*** (0.021)	0.058*** (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 (1)	University (2)	Vocational (3)	Unclassified (4)	Any (5)	University (6)	Vocational (7)	Unclassified (8)
Panel A: Grades 9-11								
Basic Treatment	0.009 (0.015)	0.014 (0.013)	-0.004 (0.009)	-0.001 (0.006)				
Savings Treatment	0.034** (0.014)	0.025* (0.014)	0.007 (0.008)	0.001 (0.008)				
Tertiary Treatment					0.057*** (0.021)	-0.002 (0.016)	0.019 (0.013)	0.040*** (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 (0.013)	-0.005 (0.008)	0.012 (0.008)	0.005 (0.004)				
Saving Treatment	0.006 (0.013)	0.001 (0.009)	0.004 (0.006)	0.001 (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 (0.011)	0.007 (0.010)	0.006 (0.010)				0.007 (0.018)	0.00632 (0.013)
Savings Treatment	0.002 (0.010)	0.004 (0.009)	0.002 (0.009)				0.028* (0.016)	-0.012 (0.010)
Tertiary Treatment				0.034 (0.022)	0.034 (0.021)	0.026 (0.022)	0.027 (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 (0.009)	0.00538 (0.009)	0.00461 (0.009)				0.01 (0.015)	0.001 (0.011)
Savings Treatment	0.008 (0.009)	0.00946 (0.008)	0.00762 (0.009)				0.039*** (0.014)	-0.01 (0.011)
Tertiary Treatment				0.034** (0.017)	0.036** (0.017)	0.031* (0.018)	0.032* (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: Tertiary Graduation (SPADIES 2016)

	Basic and Savings Treatment			Tertiary Treatment			Upper	Lower
	(1)	(2)	(3)	(4)	(5)	(6)	Secondary	Secondary
Basic Treatment	0.004 (0.005)	0.005 (0.005)	0.00586 (0.005)				0.0162* (0.010)	0.00106 (0.006)
Savings Treatment	0.009 (0.007)	0.01 (0.007)	0.0104 (0.007)				0.019* (0.011)	0.00553 (0.007)
Tertiary Treatment				0.012 (0.014)	0.012 (0.014)	0.011 (0.014)	0.011 (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	Long Term
	(1)	(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 horizon. 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	Lower
	(1)	(2)	(3)	(4)	(5)	(6)	Secondary	Secondary
Basic Treatment	0.023** (0.011)	0.024** (0.011)	0.024** (0.011)				0.025 (0.019)	0.019 (0.014)
Savings Treatment	0.016 (0.011)	0.015 (0.011)	0.013 (0.011)				0.027 (0.017)	0.005 (0.013)
Tertiary Treatment				0.018 (0.015)	0.016 (0.014)	0.015 (0.014)	0.013 (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	Lower
	(1)	(2)	(3)	(4)	(5)	(6)	Secondary	Secondary
Basic Treatment	0.014 (0.009)	0.013 (0.008)	0.014 (0.009)				0.012 (0.016)	0.015 (0.014)
Savings Treatment	0.018* (0.009)	0.018** (0.008)	0.016* (0.009)				0.038*** (0.013)	0.005 (0.013)
Tertiary Treatment				0.066*** (0.021)	0.068*** (0.020)	0.066*** (0.021)	0.065*** (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

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

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