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

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Abstract

In 2005 the city of Bogota, Colombia, introduced three conditional cash transfer programs for secondary schooling, randomly assigning socioeconomically disadvantaged students to different payment structures. We show, through administrative data, that forcing families to save one-third of the transfer increases long-term human capital accumulation by means of additional tertiary education – which is not incentivized –, casting doubt on conditionalities as a driving mechanism. Directly incentivizing on-time tertiary enrollment does no better than forcing families to save a portion of the transfer. Whereas forcing families to save increases enrollment in four-year universities, incentivizing tertiary enrollment only increases enrollment in low-quality colleges.

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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 2017). 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 2017; Fiszbein & Schady 2009). This paper provides experimental estimates of medium- and long-term educational 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 city of Bogota, Colombia, introduced three CCT programs for secondary schooling. Eligible socioeconomically disadvantaged students – ages 14 to 16 at baseline – were randomly assigned to different payment structures. In these CCTs, students receive cash transfers conditional on enrollment and attendance until they graduate from secondary school. We document through various administrative data sources the educational trajectories of program participants up until they are 26 to 28 years of age.

In one experiment, students are randomly assigned to three conditions. In the first condition, families receive a bimonthly transfer conditional on enrollment and attendance (the "basic" treatment) through the end of secondary school. This condition closely

[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*-the national CCT program-- and Mexico's *Oportunidades* respectively. Barham *et al.* (2013) used the randomized phase-in of Nicaragua's *Red de Proteccion Social* program to present effects on labor outcomes.

resembles the payment structure of most CCT programs worldwide (Garcia & Saavedra 2017). In the second condition, families are forced to save one-third of a bimonthly conditional transfer until the following academic year (the "savings" treatment) also through the end of secondary school. In the third condition, families do not receive transfers (control).

Our first key result is that forcing families to save one-third of the transfer for at least a year during the incentivized period of secondary schooling (the "savings" treatment) increases long-term human capital accumulation by means of additional tertiary education – which is not incentivized.[2] In contrast, the traditional CCT payment structure (the "basic" treatment) does not increase tertiary education enrollment. Two implications follow from this result. First, a simple modification to the timing of CCT payments that does not affect the government's budget constraint can have sizeable economic returns. Consider, for instance, that the (Mincerian) return to an additional year of tertiary education in Colombia exceeds 20 percent (Montenegro & Patrinos 2014).[3] Second, this result casts doubt on the role of conditionalities as a driving mechanism because tertiary education in the "savings" treatment is not directly incentivized.[4]

In a second (simultaneous) experiment, secondary school students are randomly assigned to one of two conditions. In the first condition, families receive a conditional bimonthly transfer through the end of secondary school and a monetary incentive for

[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.

[3] See Bazzi et al. 2015 for an estimation of the role of timing in a large CCT program in Indonesia.

[4] A dynamic model of educational decisions (e.g. DuBois, de Janvry & Sadoulet 2012) would predict that the transfer's conditionality has small (or even negative) effects on student effort the closer students approach secondary school graduation upon which the prospect of future transfers is negligible.

secondary graduation and on-time tertiary enrollment (the "tertiary" treatment). In the second condition, families do not receive transfers (control).

Our second key result is that the delayed transfer that directly incentivizes on-time tertiary education (the "tertiary" treatment) does no better than the "savings" treatment and, in fact, the point estimates suggest possibly weaker overall tertiary enrollment effects. Moreover, whereas the "savings" treatment increases tertiary enrollment in four-year universities, the "tertiary" treatment only increases tertiary enrollment in low-quality colleges, providing a cautionary tale for the use of high-powered incentives in education.

While our findings are not necessarily generalizable, they are consistent with various pieces of evidence from other contexts. Consistent with our first key finding that conditions alone cannot explain long-term human capital improvements in the "savings" treatment, evidence suggests that effects of conditional and unconditional cash transfer (UCT) programs in the developing world are generally indistinguishable from each other (e.g. Baird et al. 2014), and that adding conditions to a labeled cash transfer for education does not improve educational outcomes (Benhassine *et al.* 2015). Consistent with our second key finding that directly incentivizing tertiary education (the "tertiary" treatment) only promotes enrollment in the medium term and in low-quality tertiary institutions, some evidence suggests that student incentives often have no impacts on achievement (e.g. Fryer 2011) or may even generate negative unintended consequences (e.g. Lepper, Greene & Nisbett 1973).

The rest of the paper is organized as follows. In Section I, we describe the background and experimental intervention. In Section II, we explain the research design, timeline of events, data sources and internal validity of the experiments. We present results in Section III; and conclude in Section IV.

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

A. Program Description

In 2005, Colombia's capital city Bogota introduced 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 districts out of the 20 in Bogota.[5]

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.[6] At the national level, Colombia has another CCT program (*Familias en Accion*) that started in 2001 in rural areas. Between 2007 and 2010 the program was expanded to cities, including Bogota. We discuss in detail below the implications of this expansion for the internal validity of the research design in Bogota and integrity of the experimental control groups. To summarize, however, we find little interaction between the national CCT program and the CCT program in Bogota, which suggests that the expansion of the national CCT program did not compromise the

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[5] Despite targeting the poor, the fact that the program was implemented in Bogota, Colombia's capital and most prosperous city, may limit generalizability to other more disadvantaged settings.

[6] Reviews of these programs are Baird *et al.* (2014), Garcia & Saavedra (2017) and Fiszbein & Schady (2009).

internal validity of our experimental sample.

In the San Cristobal District, the Secretary of Education of Bogota introduced one experiment—the "Basic/Savings" experiment (Figure 1, top panel). In the Basic/Savings experiment, eligible secondary school students entering secondary grades six through 11 at baseline were randomly assigned to one of three groups, "basic" treatment, "savings" treatment, and control:

Basic treatment. Similar to many CCTs worldwide, in the basic treatment participants are paid $30 every two months via a dedicated debit card from one of Colombia's major banks conditional on enrolling in school and attending at least 80 percent of school days during the payment period. For a fully compliant student, transfers in the basic treatment can total $150 per school year, which is slightly more than the $125 the average eligible family reports spending each year on educational outlays (Barrera-Osorio *et al.* 2011).[7] Students in the basic treatment only receive transfers through the duration of secondary school.

Savings treatment. The savings treatment is a revenue-neutral experimental variant of the basic treatment.[8] In the savings treatment, instead of receiving $30 for meeting the school attendance target over two months, students are paid $20, with the remaining $10 held in a bank account if they attend at least 80 percent of school days during each payment period. 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

[7] According to household surveys, monthly expenses were between $13/month and $22/month in 2004/2005. The main items in educational expenses for low-income households in 2005 were enrollment fees, uniforms and school materials (90% of the cost). See Barrera-Osorio et al. 2008 for more details.
[8] 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).

prepare to enroll for the next academic year. The payment structure of the savings treatment can potentially provide a means of bypassing short-term liquidity constraints when paying enrollment expenses. Students in the savings treatment only receive transfers through the duration of secondary school.

Basic/Savings control group. Students in this control group never receive monetary transfers in the Basic/Savings experiment.

In the Suba District, the Secretary of Education of Bogota simultaneously introduced a second experiment—the "Tertiary" Experiment (Figure 1, top panel). In the Tertiary Experiment, eligible secondary school students entering upper secondary grades nine through 11 were randomly assigned to one of two groups, "tertiary" treatment or control:

Tertiary treatment. Participants in the tertiary treatment are paid a basic transfer of $20 every two months if they attend at least 80 percent of school days during each payment period, and are eligible for a lump-sum transfer of $300 if they successfully graduate from secondary school and enroll in a tertiary education institution for the next academic year.[9] If students fail to enroll in tertiary education, they still receive the lump-sum transfer, but only after a year's wait. The (dis-)incentive in the "tertiary" treatment is only the delay of payment for not enrolling in tertiary education, not whether the payment is made. The tertiary treatment is cost-equivalent to the basic treatment for students going through six years of secondary education. In practice however, the tertiary treatment is more generous than the basic treatment because – due to an administrative decision from

[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).

part of the SED – it was offered only to students that were, at baseline, in grades nine through eleven, which is three years or less from graduation.[10]

Tertiary control group. Students in this control group never receive monetary transfers in the Tertiary experiment.

In addition to being enrolled in secondary school at the time of application, eligible families in the two experiments had to demonstrate they had been designated as socioeconomically disadvantaged, as determined by being classified in levels I or II of the SISBEN national poverty assessment tool collected in 2004 in the city.[11] Applicants had to also present at registration a valid national identification card **(**which almost all students in Bogota have**)** to validate their poverty status against the SISBEN registry. To prevent families from moving to obtain eligibility, only families classified by the SISBEN system as living in San Cristobal prior to 2004 could apply for the Basic/Savings experiment, and only those living in Suba prior to 2004 could apply for the Tertiary experiment.[12] In both experiments, students are removed from the program if they twice fail to matriculate to the following grade, fail to reach the attendance target in two successive payment periods or are expelled from school.

B. *Experimental design and institutional verification of experimental integrity*

In each experiment, random assignment to the various treatment and control groups

[10] Applicants in grades six through eight in the Tertiary experiment 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. Results are similar to the treatment effects of the basic treatment for grades six through eight in the Basic/Savings experiment in Tables 3-6, except for the effect on secondary dropout, which is statistically significant in the Tertiary experiment.

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

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

was contingent on oversubscription.[13] 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. In 2005, the SED guaranteed funding for 6,851 students in the Basic/Savings experiment and 10,907 eligible applicants registered.[14] In the Tertiary experiment, funding was allocated for 1,133 students and 2,526 eligible applicants registered. Barrera-Osorio *et al.* (2008, 2011) created a stratified randomization algorithm that SED implemented in public lotteries in each district on April 4, 2005.[15] Applicants across randomization groups in both experiments were comparable at baseline along observable characteristics, providing evidence for compliance at baseline with the randomization protocol (Barrera *et al.* 2011).

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 (Figure 1). Hence, impact estimates broken down by grade at baseline combine impacts of different years of exposure to treatment with potential treatment effect heterogeneity by grade or age.

[13] To ensure oversubscription, SED advertised the program through posters, newspapers ads, radio clips, loudspeakers in cars, churches and community leaders, including school principals and priests.
[14] 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
[15] The algorithm stratified by district (San Cristobal or Suba), school type (public or private), grade at registration (six through 11) 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.

In 2007, during operation of the Basic/Savings and Tertiary experiments in the San Cristobal and Suba districts of Bogota, respectively, Colombia's national government started expanding its national CCT program *Familias en Accion*, which is of similar characteristics as the basic treatment in the Basic/Savings experiment. One potential concern is that the national CCT expansion into urban areas might have compromised the integrity of the Basic/Savings and Tertiary experiments. Three pieces of institutional evidence help allay this concern.

First, as mentioned before, city of Bogota authorities preserved the original experimental groups in the Basic/Savings and Tertiary experiments of the *Subsidios Condicionados a la Asistencia Escolar* program for the whole period of analysis.

Second, conversations with high-level city officials at the time indicate that Bogota's local government was reluctant to rollout the national *Familias en Accion* CCT program throughout the city. The national government, in response, introduced *Familias en Accion* initially in one poor suburb of the city (*Soacha*) that it is not under the jurisdiction of the local city. *Familias en Accion* began the strongest expansion in 2010. In 2010, most participants (treatment and control) in the Basic/Savings and Tertiary experiments would have likely already completed secondary school, which made them ineligible for *Familias en Accion* (Figure 1, bottom panel).

Third, unlike the Basic/Savings and Tertiary experiments, which target socioeconomically disadvantaged students classified in levels I and II of the SISBEN poverty assessment tool, *Familias en Accion* targeted only students classified in level I of the SISBEN tool. As we show in the results section, all of our main results remain unchanged when we restrict the analysis sample to include only students classified in level

II of SISBEN. These three pieces of evidence suggest that the expansion into Bogota of the *Familias en Accion* national CCT program does not compromise the integrity of the Basic/Savings and Tertiary experiments during our period of analysis.[16]

Barrera-Osorio *et al.* (2011) documented effects of the program one year after randomization of students into treatments. In general, 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 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.

II. Administrative Data Sources and Estimation Strategy

A. Administrative Data Sources

We combine the original program registration and randomization records with four administrative data sources to track educational outcomes over time, as Figure 1 indicates. Program registration records contain information for the 13,433 eligible applicants on the experiment (Basic/Savings, Tertiary), randomization group (Basic treatment, Savings treatment, Basic/Savings control; Tertiary treatment, tertiary control), school, and grade in which students were enrolled at the time of the lottery. From the program registration records we also obtain names and identification numbers, which we used to match to the

[16] In 2012, the Colombian government introduced *Jovenes en Accion*, a complementary training program to *Familias en Accion* through which *Familias en Accion* beneficiaries graduating from secondary school could receive training scholarships at Colombia's national job training agency (SENA). To the extent that students in the Basic/Savings and Tertiary experiments were ineligible for *Familias en Accion*, they would have also been ineligible for the *Jovenes en Accion* training scholarships.

following four administrative data sources:[17]

1. *National poverty assessment tool (SISBEN).* At baseline, we matched applicant records to Colombia's 2003-04 *Sistema de Identificación y Clasificación de Potenciales Beneficiarios para 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.

2. *Administrative secondary school enrollment records from the city of Bogota.* To measure secondary school enrollment, we used annual administrative data from SED.[18] The data are similar to those used in Barrera-Osorio *et al.* (2011) but include information from 2006-08 (Figure 1, bottom panel).[19] These data include an indicator for whether a student was enrolled as well as information on the student's 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 across research groups.[20] As Figure 1 indicates, these secondary enrollment records enable us to capture the full academic progression through secondary school for all students in the Tertiary experiment

[17] To match registration records to the ICFES secondary exit exam dataset and the SPADIES tertiary education dataset, we follow 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 name; iv) For those not matched in (i), (ii) or (iii), match on name and date of birth.

[18] 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.

[19] 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.

[20] As we later demonstrate, results are robust to limiting our sample to just those students with available secondary school data.

regardless of the grade in which they enroll at baseline, and to capture the full academic progression through secondary school for all students in the Basic/Savings with the exception of those who enroll at baseline in grade 6 or 7. For these students, assuming that they progress on time and do not drop out, we may miss the last one or two years of secondary enrollment with available enrollment data. In these data we define four outcome measures: secondary school enrollment (any grade, on-time progression), being held back and dropping out of secondary school.[21]

3. *Secondary exit exam (ICFES) database*. 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). We match applicant records to the universe of test-takers in 2006-12. The timespan of the secondary exit exam database allows us to track secondary graduation for program participants from all grades at baseline in the Basic/Savings and Tertiary experiments assuming on-time progression. (Figure 1, bottom panel). For program participants with delayed progress through secondary school, we can track secondary graduation with up to a six-year delay among those in grade 11 at baseline (Basic/Savings and Tertiary experiments), and up to a two-year delay among those in grade 6 at baseline (Basic/Savings experiment, Figure 1). In these data we define one secondary schooling outcome: taking the ICFES exam, which is our proxy for secondary school graduation.
4. *Tertiary education database (SPADIES)*: To track tertiary education outcomes, we use

[21] On-time enrollment is set to one if the student has not dropped out and has not been held back.

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 and excludes students enrolled in Colombia's National Job Training Agency (SENA) programs. SPADIES contains information on the timing and university of students' initial enrollments and the types of institutions. Higher-quality institutions are classified as either universities – roughly equivalent to four-year colleges – or vocational schools, while low-quality tertiary institutions remain unclassified.[22] We use two cuts of the SPADIES data: one that covers collegiate pathways from 2006-12 – up to seven years after the start of the program, or "medium-term;" and one that covers collegiate pathways up to 2016 – up to 11 years after the start of the program, or "long-term" (Figure 1, bottom panel).

Figure 1 illustrates the importance of using two cuts of the *SPADIES* data. A student in grade 6 at baseline in the Basic/Savings experiment will graduate on time from secondary school in 2011. If she delays tertiary enrollment for one or two years, which is not uncommon among low-income college-bound students, we will not capture her collegiate pathways with the 2012 cut of the *SPADIES* data. We will also likely not capture tertiary completion for any grade cohort in the 2012 cut of *SPADIES.* As such, the 2012

[22] 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.

SPADIES cut is best suited to track short- and medium-term tertiary enrollment outcomes for students in upper secondary grades at baseline (Basic/Savings and Tertiary experiments). The 2016 cut of *SPADIES* allows us to capture on-time and delayed tertiary enrollment decisions for all grades at baseline, particularly for students in lower secondary grades at baseline (Basic/Savings experiment) as well as tertiary completion outcomes for students in upper secondary grades at baseline (Basic/Savings and Tertiary experiment).

In the tertiary education database we define four outcomes. In the 2012 *SPADIES* cut we define medium-term tertiary enrollment. In the 2016 *SPADIES* cut we define long-term tertiary enrollment at any point in time, on-time tertiary enrollment (up to two years post-secondary school graduation) and tertiary graduation.

Table 1 shows match rates to the secondary enrollment, secondary exit exam (*ICFES*) and tertiary education (*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, Table 1).

[INSERT TABLE 1 HERE]

Match rates to the secondary graduation and tertiary education databases across all students are similar to those among comparable individuals in Bogota (Panel A of Table 1). The match rates for the Tertiary experiment are higher for both the secondary graduation database (0.84), as well as for the tertiary education, medium term (0.37) and long term data (0.45, Panel A, Column 3, Table 1).[23]

Importantly, the administrative secondary graduation (*ICFES*) and tertiary education (*SPADIES)* datasets have nationwide coverage. The nationwide coverage of these datasets minimizes attrition because as long as we have identifying information for the majority of applicants—which we do for 99 percent of applicants (Table 2)—we can track relevant outcomes. In particular, note that not showing up in the secondary graduation database conditional on having valid identifying information likely implies that the student did not graduate from secondary school. Similarly, not showing up in the tertiary education database conditional on having valid identifying information likely implies that the student did not enroll in tertiary education. So in these datasets, our main outcomes are well defined for the entire sample. Moreover, selective migration is not a threat to internal validity because as long as we have balance in the availability of identifying information across randomization groups (Table 2) we can track any individual, regardless of the immigration status of the students within the country.

[INSERT TABLE 2 HERE]

We do not find evidence of differential quality in available identifying information across treatment and control groups in the Basic/Savings or Tertiary experiments.[24] We

[23] 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 Basic/Savings) experiment. Similarly, among these individuals in the ECV, 21 percent completed some college, which is exactly the tertiary education (SPADIES) match rate in the Basic/Savings experiment. The rates also align to those reported in Bettinger et al. (2016).

[24] In all the matches we employ the original identification data reported at baseline. The research team put in place strict protocols for data entry, cleaning and coding of the experimental sample dataset. Furthermore, enrollment data is centralized at SED and, to our knowledge, there is no differential treatment in student records across experimental groups or localities. Finally, 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.

have date of birth and first (given) names for 100 percent of applicants in both experiments. We have national ID numbers and complete last (family) names for, respectively, 99.4 and 97.8 percent of them. There are no differences across randomization groups in the availability of national ID or last-name information (Table 2). Moreover, the composition of students for whom we have complete identifying information is balanced across randomization groups in both experiments (Tables A1 and A2).

B. Estimation Strategy

Given student-level random assignment in both the Basic/Savings and Tertiary experiments, 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:

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

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 Basic/Savings experiment includes indicators for the basic and for the savings treatments (with the Basic/Savings control as the omitted group) and in the Tertiary experiment it includes an indicator for the tertiary treatment (with the Tertiary control as the omitted group).

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, 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). We also include school-fixed effects, so that only variation within schools in treatment assignment identifies the parameters of interest.[25] We cluster all standard errors at the school level. There are 144 school clusters in the Basic/Savings experiment and 58 school clusters in the Tertiary experiment.

In some specifications we pool estimates from the Basic/Savings and Tertiary experiments. To do this, and given that the Tertiary 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. The Basic/Savings and Tertiary experiments are independent samples. Hence, in the pooled sample we can only identify the effects of the basic, savings and tertiary treatments relative to each other (and to a pooled control group) conditional on a rich set of socio-demographic controls and school-fixed effects rather than purely from random variation in treatment assignment. Pooling samples from the Basic/Savings and Tertiary experiments for grades nine through 11 is empirically justified given the similarities in baseline characteristics across the two groups of students (see Barrera-Osorio *et al.* 2008 for details).

Given the number of outcomes we analyze, we also conduct joint hypothesis tests of the treatment effect for each treatment in the Basic/Savings and Tertiary experiments, and the difference between the basic and savings treatment in the Basic/Savings experiment. In the medium term, we focus on on-time enrollment in secondary school,

[25] Estimates without baseline controls are identical to those with full set of controls, as shown in the NBER working paper; (Barrera-Osorio et al. 2017).

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. For the joint hypothesis tests we estimate Equation (1) with the various outcomes simultaneously in a Seemingly Unrelated Regressions framework.

III. Results

We present results following the natural academic progression showcased in Figure 1: secondary education outcomes (subsection a), medium-term tertiary education enrollment (subsection b) and long-term tertiary education outcomes (subsection c). In subsection (d) we present various robustness checks that further support the validity of our main results.

A. Secondary education outcomes

We document effects on students' secondary school outcomes in Table 3. We begin with secondary enrollment. In the Basic/Savings experiment, the basic treatment increases students' on-time enrollment relative to the control group by 2.4 percentage points (base rate is 51 percent). This difference is statistically significant at the 10 percent level with full controls (Panel A, column 1, 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 (Panel A, column 1, Table 3). The estimate on the tertiary treatment is 2.2 percentage points and statistically significant at the 10 percent level (from a base of 72 percent; Panel A, column 2, Table 3).

[INSERT TABLE 3 HERE]

To compare estimates across the two experiments, we restrict the sample to students in upper secondary school and pool the samples (Panel A, column 3, Table 3). For students in upper secondary at baseline, the effect of the basic treatment on on-time enrollment 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 in improving on-time secondary enrollment. Among lower secondary students in the Basic/Savings experiment, treatment effects for the basic treatment and savings treatment are about 3.5 statistically significant percentage points (from a base of 42 percent; Panel A, column 4, Table 3).

On-time secondary enrollment effects could be the result of marginal applicants enrolling in secondary school as a result of incentives, progressing through secondary school with fewer repetitions or not dropping out. In Columns 5 to 8, Table 3, we present evidence for these alternative channels. In the Basic/Savings experiment, on-grade enrollment effects of the savings treatment are largely explained by a reduction in the likelihood of dropping out of secondary school among incentivized students, for whom dropout rates fall by 3.2 statistically significant percentage points (from a base of 38 percent; Panel A, column 7, Table 3). Neither the basic or savings treatments affect the probability of being held back in secondary school (Panel A, column 5, Table 3).

In the Tertiary experiment, the tertiary treatment reduces the likelihood of dropping out of secondary school in the three years after randomization by 3.6 statistically significant percentage points (from a base of 23 percent; Panel A, column 8, Table 3). The tertiary treatment does not affect the likelihood of being held back in secondary school in the three years following randomization (Panel A, column 6, Table 3).[26]

For secondary graduation, we find that in the Basic/Savings experiment, the basic treatment increases secondary graduation (as measured by *ICFES* secondary exit exam-taking) by 2.2 percentage points for students in all grades at baseline. The savings treatment does not appear to affect secondary graduation among students in all grades at baseline (Panel B, column 1, Table 3).

In the Tertiary experiment, the effects of the tertiary treatment on secondary graduation are close to zero and statistically insignificant (Panel B, column 2, Table 3). Among control-group students in the Tertiary experiment, the likelihood of graduating from secondary school is high—83 percent. The tertiary treatment does not affect the graduation margin among the remaining 17 percent of students who do not graduate in the absence of incentives.

When we pool the two experiments for the sample of student in grades nine through 11 at baseline, the savings treatment increases secondary graduation by 2.8 percentage points, statistically significant at the 10 percent level (from a base of 80 percent; Panel B, column 3, Table 3). In the pooled sample for upper secondary students at baseline, the tertiary treatment does not appear to improve secondary graduation. However, we cannot reject equality between either the basic or savings and the tertiary treatment effects (Panel B, column 3, Table 3). For lower secondary students in the Basic/Savings experiment, neither treatment appears to increase secondary graduation (Panel B, column 4, Table 3).

In sum, these results suggest that modifying the traditional payment structure of a

[26] In 2002 the national government implemented a policy of "automatic promotion" (Decree 230, 2002), by which 95 percent of the students in any grade should we pass to the next grade. As such, the margin of impact of the program is small – 5 percent –, which may explain the null effect on being held back in secondary school.

CCT program for secondary schooling to delay a portion of payments increases the probability of on-time secondary school enrollment, mostly by reducing the probability of students dropping out of secondary school.

B. Medium-term tertiary education enrollment

We document the treatment effects on students' medium-term tertiary education enrollment using the 2012 tertiary education (*SPADIES*) database. From Figure 1, this dataset captures with higher probability college pathways for individuals who were in upper grades at the beginning of the experiment.

In the Basic/Savings experiment, the savings treatment increases the probability of ever enrolling in a tertiary institution in the medium-term by 1.5 percentage points, statistically significant at the 10 percent level (7 percent from a base of 21 percent; Panel A, column 1, Table 4). The effect of the basic treatment is small and statistically insignificant, but indistinguishable from the savings treatment estimate (Panel A, column 1, Table 4).

In the Tertiary experiment, the tertiary treatment increases tertiary enrollment in the medium-term by 5.7 statistically significant percentage points (Panel A, column 2, Table 4). Relative to control group's base enrollment rate of 35 percent, this estimate represents a 16 percent increase.

[INSERT TABLE 4 HERE]

Pooling experiments, we find that among students who were in grades nine through 11 at registration, the savings treatment increases tertiary enrollment in the medium-term by 3.6 statistically significant percentage points (12 percent from a base tertiary enrollment rate of 31; Panel A, column 3, Table 4). The basic treatment does not increase tertiary enrollment in the medium-term among upper secondary students. The tertiary treatment increases tertiary enrollment by 5.8 percentage points (19 percent from a base of 31 percent; Panel A, column 3, Table 4). In the pooled sample of upper secondary grades the difference between the savings and basic treatment is statistically significant at the 1 percent level. We can also reject the null hypothesis of equality of tertiary enrollment effects between tertiary and basic treatments, but not between savings and tertiary treatments.[27]

Upper secondary students in the savings and tertiary treatments enroll in different types of tertiary education institutions. Among upper secondary students in the Basic/Savings experiment, the savings treatment primarily encourages tertiary enrollment in universities (rather than vocational schools or the low-quality unclassified schools; Panel B, columns 1-3, Table 4). In the Tertiary experiment, the tertiary treatment solely encourages enrollment in low-quality, unclassified tertiary education institutions (Panel B, column 6, Table 4). This may be the result of the tertiary treatment's high-powered incentives for tertiary education that encourage students to enroll more indiscriminately. In sum, among upper secondary students, modifying the payment structure of a traditional CCT program to delay a portion of the payment can improve human capital accumulation

[27] When we interact treatment status with grade at baseline, for the savings treatment the estimate of the interaction term is 1.3 percentage points per grade level (p-value of 0.012) and on the main effect it is -9.1 percentage points (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. 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 for grade nine at baseline (p-value of 0.063). The effects for on-time enrollment and the exit exam do not vary with grade.

in the form of increased tertiary education, which is not incentivized in the savings or tertiary treatments.

In the Basic/Savings experiment, neither basic nor savings treatments increase tertiary enrollment in the medium-term among students in lower secondary grades at baseline (Panel A, column 4, Table 4). Note, however, that in the medium-term, only 16 percent of lower secondary students in the control group of the Basic/Savings experiment enroll in tertiary education. Given the timing, it is possible that the medium-term cut of the tertiary education database is not best suited for tracking collegiate pathways of lower secondary students incentivized to pursue tertiary education as a consequence of program incentives (Figure 1). In the next section we investigate this possibility with the long-term tertiary education data.

C. Long-term tertiary education outcomes

To estimate long-term tertiary education effects, we use the 2016 cut of the tertiary education (*SPADIES*) dataset, which enables us to track tertiary education outcomes between five and 10 years after (on-time) secondary school graduation (Figure 1). To the extent that marginal students are college-bound, this long-term dataset most likely enables us to track collegiate pathways for students from all grades at the time of randomization. We focus on three outcomes: long-term tertiary enrollment, on-time tertiary enrollment and tertiary graduation.[28]

In the Basic/Savings experiment, effects of the basic and savings treatments on long-term tertiary enrollment for students in all grades at baseline are small in magnitude

[28] On-time enrollment and graduation are well defined for the entire sample because they are unconditional on tertiary enrollment or secondary graduation; *e.g.*, a person who did graduate from secondary school or who did not enroll in tertiary has a value of zero for on-time enrollment and for graduation. In subsection (d) we estimate treatment effect bounds conditioning on these outcomes.

and statistically insignificant (Panel A, column 1, Table 5). 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 on long-term tertiary enrollment is 2.8 percentage points, significant at the 10 percent level (7 percent increase from a base of 40 percent; Panel A, column 3, Table 5). A comparison of program impacts on medium- and long-term tertiary enrollment suggests that the savings and tertiary treatments may simply be accelerating tertiary access for upper secondary students rather than increasing access overall. In the long-term, control students are catching-up.

[INSERT TABLE 5 HERE]

We find empirical support for this conjecture when we explore program impacts on long-term on-time tertiary enrollment. In the basic and savings experiment, the basic and savings treatments do not increase on-time tertiary enrollment in the long-term (Panel B, column 1, Table 5). The tertiary treatment, by contrast, increases on-time long-term tertiary enrollment by 3.1 percentage points (13 percent from a base of 24 percent; Panel B, column 2, Table 5).

When we pool experiments for 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 (19 percent and 15 percent, respectively, from a base of 21 percent; Panel B, column 3, Table 5). On-time tertiary enrollment effects are close to zero or negative, and always insignificant for student in lower secondary grades at baseline.

The absence of medium- and long-term 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 in the next subsection.

When we focus on long-term tertiary education graduation, measured between five and 10 years after on-time secondary school graduation, we find that in the Basic/Savings experiment none the treatments affect tertiary graduation among students from all secondary grades at baseline (Panel C, column 1, Table 5). In the Tertiary experiment, the tertiary treatment also does not increase tertiary graduation (Panel C, column 2, Table 5).

When we pool experiments for upper secondary grades at baseline, we find that basic and savings treatments increase tertiary graduation by 1.6 and 1.9 percentage points, respectively (16 percent and 19 percent, respectively, from a base of 10 percent; Panel C, column 3, Table 5). These estimates are statistically significant only at the 10 percent level. In the pooled sample of upper secondary grades, the tertiary treatment does not increase tertiary graduation. We find no effects of the various treatments on tertiary graduation among lower secondary students, which is perhaps unsurprising given no impact of the various treatments on the tertiary enrollment margin among these students (Panel C, Column 4, Table 5).

On the whole, tertiary education results suggest that forcing families to save one-third of the transfer increases long-term human capital accumulation by means of additional tertiary education. At the same time, directly incentivizing on-time tertiary education accelerates tertiary enrollment in low-quality tertiary institutions but has no overall impact on tertiary access or completion.

While we cannot definitively pinpoint to the mechanisms driving the results, some evidence helps to (partially) rule out some possible hypotheses. For example, it is unlikely that the transfers are relaxing liquidity constraints of program participants. If this were the case, we would expect to see stronger effects among the poorest participants, for whom presumably liquidity constraints are most binding. We find no evidence heterogeneity of impacts by household income (Table B1).[29]

The pattern of results for the tertiary treatment in the Tertiary experiment is consistent with imposing conditions for on-time tertiary enrollment. However, the pattern of results for the savings treatment also seems inconsistent with secondary schooling conditions playing a major role for two reasons. First, tertiary education is not incentivized in the savings treatment. Second, a dynamic model of educational decisions (e.g. Dubois, de Janvry & Sadoulet 2012) 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. However, we consistently find stronger effects for students in upper secondary grades.

The strong results for upper secondary students among students in the savings and tertiary treatments is potentially consistent with a "scholarship" model, in which, 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.

[29] To test for heterogeneity by baseline characteristics, we estimate Equation (1) with interactions between the treatment variables and two baseline characteristics (student gender and SISBEN index, as a proxy for income of the household) for the four main outcomes (on-time secondary enrollment, secondary graduation, and enrollment in tertiary institution in the medium-term and in the long-term). We do not find evidence of heterogeneous effects by baseline income (Table B1) or gender (Table B2).

D. Robustness checks

In this subsection we present evidence that our results pass a variety of robustness checks. In short, as we present below, all the results validate our main conclusions.

<u>Statistical corrections for multiply hypothesis testing</u>

We correct for multiple-hypothesis testing by estimating pooled models of key outcomes simultaneously in a Seemingly Unrelated Regressions framework that allows for arbitrary covariance within individuals across outcomes. We estimate three models, one for (only) medium-term outcomes, one for (only) long-term outcomes, and one for both medium- and long-term outcomes.

In the medium term, overall educational effects from all treatments are statistically significant (column 1, Table 6). The p-values on the savings and tertiary treatments are less than 1 percent. The basic treatment also is statistically significant at the 10 percent level with a p-value of 0.078.

[INSERT TABLE 6 HERE]

We reject the hypothesis of equality of medium-term impacts between basic and savings treatment with a p-value of 0.019. Upper secondary students drive this result. 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 (column 1, Table 7).

When testing for joint significance of (only) long-long term outcomes, we are not able to reject the null hypothesis of joint coefficients equal to zero in any of the treatments (Column 2). However, we reject the null of equality in treatment effects between basic and savings treatments, (p-value of 0.038). This result, as before, is driven by differences in treatment effect estimates in long-run educational outcomes among lower secondary students (p-value of 0.033). For upper secondary students the test is marginally significant (p-value of 0.119).

Some long-term educational outcomes depend on students' medium-term tertiary progression. For example, tertiary enrollment is contingent on secondary school graduation. For this reason, it is appropriate to test for the joint significance of medium- and long-term outcomes simultaneously. For all educational outcomes, the savings treatment is the only treatment for which we consistently reject the null hypothesis of zero effects (p-value of 0.001, column 3, Table 6). We also reject equality of effects for all educational outcomes between basic and savings treatments (p-value 0.029).[30] However, we cannot reject equality between the savings and tertiary treatment effects for all educational outcomes among students in upper secondary grades at baseline (p-value of 0.864).

Young (2016) argues that SUR significance tests based on large-sample inference may be biased in favor of rejection of the null hypothesis. As an additional test, we also carry out joint hypothesis tests based on randomization inference in which we compute exact p-values based on 300 randomization permutations (Table C1).[31]

Overall, results in Table C1 based on randomization inference are consistent with those in Table 6 based on large sample inference. Using randomization inference we continue to reject the null joint hypothesis of no treatment effects in medium-term

[30] 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.

[31] We are extremely grateful to Alwyn Young for facilitating his STATA code for randomization inference.

educational outcomes for the basic and savings treatments. We also continue to reject the null joint hypothesis of no treatment effects in both medium- and medium- and long-term educational outcomes ("All Outcomes") for the savings treatment.

There are two main differences between joint significance results based on large sample inference and those based on randomization inference. First, unlike test results based on large sample inference, we cannot reject the null of no tertiary treatment effects for medium-term educational outcomes using randomization inference. Second, joint tests based on randomization inference reject equality between savings and tertiary treatments for medium-term, long- and "all" educational outcomes among students in upper secondary grades at baseline (Column 1, Table C1).

Compositional changes through academic progression

As we document in the previous section, effects on tertiary education outcomes are strongest among students in grades nine to 11 at baseline. One explanation is compositional changes as students progress through secondary school. To test this hypothesis, we re-estimate tertiary enrollment results restricting the sample to students who reach at least grade 9 in secondary school. Table C2 shows that conditioning on reaching grade 9 does not change conclusions, suggesting that changes in student composition in the progression through secondary school do not drive effects.

Compositional changes could also arise in the transition from secondary to tertiary education. In particular, taking the secondary exit exam (ICFES) is a condition for gaining tertiary admission in Colombia, implying that students cannot enroll in tertiary education without taking the exam. So far, we measure tertiary enrollment unconditionally. As a robustness check, we can estimate bounds on tertiary enrollment by trimming the sample by the additional proportion of secondary exit exam takers in each randomization group (Angrist, Bettinger & Kremer 2006, Lee 2009). Results from this bounding approach (Table C3) are consistent with results in Table 4 and reinforce our main result that the basic treatment does not increase tertiary enrollment whereas the savings and tertiary treatments increase tertiary education enrollment. Directly conditioning on taking the secondary exit exam produces similarly consistent results (Table C4).

<u>Interactions with the national CCT program *Familias en Accion*</u>

In section II we explained with aid of institutional evidence why control-group contamination in the form of participation in Colombia's national CCT program *Familias en Accion*, which was rolled out in Bogota starting in 2010, was unlikely. Here we provide additional evidence suggesting no interaction between the Bogota CCT program we study and the rollout in Bogota of *Familias en Accion*. To do so, we exploit the fact that the Bogota CCT program we study targeted students from households in levels I and II of the *SISBEN* national poverty assessment tool, whereas *Familias en Accion* only targeted students from level I of *SISBEN*. Table C5 shows that our results are robust to restricting the sample to only applicants in level II of SISBEN at *baseline*, who are not eligible for the national program. These results align with the institutional evidence and suggest that sample contamination from the national CCT program is unlikely.

<u>Missing student enrollment data</u>

As noted in Section II, information required to match administrative 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.

Effects on our key secondary graduation and tertiary outcomes are unchanged if we only include students for whom information was available for the enrollment match (Table C6).

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.

IV. Conclusion

This paper contributes two new results to the literature on long-term effects of CCT programs on students' educational outcomes. We combine randomization data from three conditional cash transfer programs for secondary schooling introduced in the city of Bogota, Colombia in 2005 and various administrative data sources to track educational in the medium and long-term. A variety of robustness checks give additional credence to our findings.

Our first key result is that forcing families to save one-third of the monetary transfer for at least a year during the incentivized period of secondary schooling increases long-term human capital accumulation by means of additional tertiary education – which is not incentivized. In contrast, a traditional bimonthly payment structure—common to many CCT programs worldwide—does not increase tertiary education enrollment. This suggests that a simple, revenue-neutral modification to the timing of CCT payments can have sizeable economic returns. Mincerian returns to an additional year of tertiary education in Colombia, for instance, exceed 20 percent. In addition, this result casts doubt on the role of conditionalities as a driving mechanism because tertiary education in the "savings" treatment is not directly incentivized, adding to the growing body of evidence regarding the value of imposing conditions on recipients in transfer programs that target the poor (e.g. Benhassine *et al.* 2013; Baird *et al.* 2014).

Our second key result is that directly incentivizing on-time tertiary education through an additional lump sum bonus at the end of secondary schooling only accelerates tertiary enrollment in low-quality tertiary institutions but has no overall impact on tertiary education access or completion in the long-term. While this result is evidence that incentives work, it provides a cautionary tale for the use of high-powered incentives in education because they can often lead to unintended consequences.

It is difficult to pinpoint exactly the mechanisms that drive our key results. They may include the ability of delayed transfers to relax savings constraints, or to incentivize on-time educational progression for college-bound students in what could be characterized as a "scholarship" model. Future research could help shed light on these potential alternative explanations.

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Table 1. Match Rates to Various Administrative Data Sources

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

Notes: 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 2006-2012. 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 by Randomization Status in the Probability of Available Identifying Information for Administrative Data Matches

	Any ID Number (1)	Last Name (2)
A. Basic/Savings Experiment (San Cristobal District)		
Basic Treatment	0.002	-0.004
	(0.001)	(0.004)
Savings Treatment	0.002*	-0.008**
	(0.001)	(0.004)
Control Mean	0.99	0.98
N	10,947	10,947
R^2	< 0.01	< 0.01
p-value Basic=Savings	0.15	0.32
B. Tertiary Experiment (Suba District)		
Tertiary Treatment	< 0.001	0.003
	(< 0.001)	(0.006)
Control Mean	1.00	0.98
N	2,544	2,544
R^2	< 0.01	< 0.01

Notes: 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: Impacts of Various Treatments on Secondary School Outcomes

PANEL A. Outcomes:	On-Time Secondary Enrollment				Held Back		Dropout	
	Basic/ Savings	Tertiary	Pooled (Grades 9-11)	Basic/ Savings (Grades 6-8)	Basic/ Savings	Tertiary	Basic/ Savings	Tertiary
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Basic Treatment	0.024*		0.004	0.035**	-0.009		-0.018	
	(0.013)		(0.017)	(0.015)	(0.008)		(0.012)	
Savings Treatment	0.035***		0.035***	0.034***	-0.007		-0.032***	
	(0.009)		(0.013)	(0.012)	(0.007)		(0.010)	
Tertiary Treatment		0.022*	0.022*			0.005		-0.036***
		(0.013)	(0.012)			(0.009)		(0.014)
Control Mean	0.51	0.72	0.68	0.42	0.13	0.05	0.38	0.23
p-value Basic = Savings	0.33		0.06	0.94	0.82		0.22	
p-value Basic = Tertiary			0.41					
p-value Savings = Tertiary			0.48					
Grades at Registration	All	9-11	9-11	6-8	All	9-11	All	9-11
N	9937	2345	6320	5962	9937	2345	9937	2345
R^2	0.19	0.24	0.22	0.14	0.06	0.11	0.2	0.25

PANEL B. Outcomes:	Secondary Graduation			
	Basic/ Savings	Tertiary	Pooled (Grades 9-11)	Basic/ Savings (Grades 6-8)
	(1)	(2)	(3)	(4)
Basic Treatment	0.022**		0.021	0.02
	(0.010)		(0.016)	(0.014)
Savings Treatment	0.01		0.028*	0.001
	(0.011)		(0.017)	(0.013)
Tertiary Treatment		0.007	0.005	
		(0.015)	(0.014)	
Control Mean	0.68	0.83	0.80	0.61
p-value Basic = Savings	0.12		0.59	0.10
p-value Basic = Tertiary			0.51	
p-value Savings = Tertiary			0.34	
Grades at Registration	All	9-11	9-11	6-8
N	10947	2544	6905	6586
R^2	0.12	0.1	0.09	0.11

Notes: Table presents treatment effect estimates of the various treatments on four secondary school outcomes: (i) on-time enrollment, grades 6-11 (if students have not dropped out and have not been held back); (ii) an indicator for being held back; (iii) an indicator for dropping out of secondary school; and (iv) an indicator for whether or not a student took the ICFES exam (a proxy for high school graduation). All coefficients are estimated using Equation (1), controlling by baseline socio-demographic characteristics and school fixed effects. 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. Impacts of Various Treatments on Medium-Term Tertiary Enrollment

PANEL A. Outcomes:	Tertiary Enrollment, Any Time			
	Basic/ Savings Experiment (Grades 6-11)	Tertiary Experiment (Grades 9-11)	Pooled Experiments (Grades 9-11)	Basic/ Savings (Grades 6-8)
	(1)	(2)	(3)	(4)
Basic Treatment	0.01		0.007	0.012
	(0.009)		(0.016)	(0.013)
Savings Treatment	0.015*		0.036**	0.006
	(0.009)		(0.014)	(0.013)
Tertiary Treatment		0.057***	0.058***	
		(0.021)	(0.021)	
Control Mean	0.21	0.35	0.31	0.16
p-value Basic = Savings	0.46		0.01	0.46
p-value Basic = Tertiary			0.07	
p-value Savings = Tertiary			0.40	
N	10,947	2,544	6,905	6,586
R^2	0.08	0.10	0.09	0.06

PANEL B. Outcome	Tertiary Enrollment by Type of Institution					
	Basic/Savings Experiment Grades 9-11			Tertiary Experiment Grades 9-11		
	University	Vocational	Unclassified	University	Vocational	Unclassified
	(1)	(2)	(3)	(4)	(5)	(6)
Basic Treatment	0.014	-0.004	-0.001			
	(0.013)	(0.009)	(0.006)			
Savings Treatment	0.025*	0.007	0.001			
	(0.014)	(0.008)	(0.008)			
Tertiary Treatment				-0.002	0.019	0.040***
				(0.016)	(0.013)	(0.010)
Control Mean	0.15	0.10	0.03	0.21	0.10	0.04
p-value Basic = Savings	0.36	0.30	0.66			
p-value Basic = Tertiary						
p-value Savings = Tertiary						
N	4361	4361	4361	2544	2544	2544
R^2	0.07	0.04	0.04	0.08	0.08	0.08

Notes: Table presents estimates of the treatment effects on an indicator for whether or not a student enrolled in a tertiary institution and the type of institution (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), controlling by baseline socio-demographic characteristics and schools fixed effect. 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. Impacts of Various Treatments on Long-Term Tertiary Education Outcomes

PANEL A. Outcome:	Tertiary Enrollment Any Time			
	Basic/ Savings	Tertiary (Grades 9-11)	Pooled (Grades 9-11)	Basic/ Savings (Grades 6-8)
	(1)	(2)	(3)	(4)
Basic Treatment	0.006		0.007	0.006
	(0.010)		(0.018)	(0.013)
Savings Treatment	0.002		0.028*	-0.012
	(0.009)		(0.016)	(0.010)
Tertiary Treatment		0.026	0.027	
		(0.022)	(0.022)	
Control Mean	0.32	0.43	0.40	0.29
p-value Basic = Savings	0.67		0.17	0.14
p-value Basic = Tertiary			0.48	
p-value Savings = Tertiary			0.99	
Grades at Registration	All	9-11	9-11	6-8
N	10,947	2,544	6,905	6,586
R^2	0.08	0.10	0.09	0.08

PANEL B. Outcome:	On-time Tertiary Enrollment			
	Basic/ Savings	Tertiary (Grades 9-11)	Pooled (Grades 9-11)	Basic/ Savings (Grades 6-8)
	(1)	(2)	(3)	(4)
Basic Treatment	0.005		0.01	0.001
	(0.009)		(0.015)	(0.011)
Savings Treatment	0.008		0.039***	-0.01
	(0.009)		(0.014)	(0.011)
Tertiary Treatment		0.031*	0.032*	
		(0.018)	(0.018)	
Control Mean	0.18	0.24	0.21	0.18
p-value Basic = Savings	0.67		0.04	0.15
p-value Basic = Tertiary			0.40	
p-value Savings = Tertiary			0.75	
Grades at Registration	All	9-11	9-11	6-8
N	10,947	2,544	6,905	6,586
R^2	0.05	0.09	0.08	0.06

PANEL C. Outcome:	Tertiary Graduation			
	Basic/ Savings	Tertiary (Grades 9-11)	Pooled (Grades 9-11)	Basic/ Savings (Grades 6-8)
	(1)	(2)	(3)	(4)
Basic Treatment	0.006		0.0162*	0.001
	(0.005)		(0.010)	(0.006)
Savings Treatment	0.0104		0.019*	0.006
	(0.007)		(0.011)	(0.007)
Tertiary Treatment		0.011	0.011	
		(0.014)	(0.014)	
Control Mean	0.06	0.11	0.10	0.05
p-value Basic = Savings	0.44		0.77	0.54
p-value Basic = Tertiary			0.77	
p-value Savings = Tertiary			0.64	
Grades at Registration	All	9-11	9-11	6-8
N	10,947	2,544	6,586	6,586
R^2	0.04	0.09	0.06	0.03

Notes: Table presents estimates of the treatment effects on three outcomes: (i) an indicator for whether or not a student enrolled in a tertiary institution; (ii) an indicator for whether or not a student enrolled on time in a tertiary institution, using a 2 year window (after graduation), unconditional on enrollment; (iii) an indicator for whether or not a student graduated from a tertiary institution, unconditional on tertiary enrollment. All coefficients are estimated using Equation (1) controlling by socio-demographic baseline characteristics and school fixed effects. 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. Joint Hypothesis Tests: SUR Estimates

	Medium-Term Educational Outcomes	Long-term Educational Outcomes	All Educational Outcomes
	(1)	(2)	(3)
H_0: Basic = 0			
Chi^2	6.811	1.401	8.253
p-value	0.078	0.705	0.143
H_0: Savings = 0			
Chi^2	21.703	2.795	21.683
p-value	0.000	0.424	0.001
H_0: Tertiary = 0			
Chi^2	15.581	3.315	7.311
p-value	0.001	0.346	0.198
H_0: Basic = Savings			
All Students			
Chi^2	15.185	13.336	20.057
p-value	0.019	0.038	0.029
Upper Secondary (Grades 9-11)			
Chi^2	7.513	5.36	8.147
p-value	0.057	0.147	0.148
Lower Secondary (Grades 6-8)			
Chi^2	3.566	8.728	11.21
p-value	0.312	0.033	0.05
H_0: Savings=Tertiary			
Upper Secondary (Grades 9-11)			
Chi^2	3.600	0.470	1.893
p-value	0.308	0.925	0.864
Outcomes included	On-time Secondary Enrollment, Secondary Graduation, Medium-term Tertiary Enrollment	Long-term Tertiary Enrollment, Long-term On-time Tertiary Enrollment, Long-term Tertiary Graduation	On-time Secondary Enrollment, Secondary Graduation, Long-term Tertiary Enrollment, Long-term On-time Tertiary Enrollment, Long-term Tertiary Graduation

Notes: Table presents joint hypothesis tests results for treatment effects on different outcomes, depending on the temporal horizon. 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.

Figure 1. Timeline

	Grades at baseline incentivized (conditional on school enrollment and attendance)					
	6th	**7th**	**8th**	**9th**	**10th**	**11th**
Basic-Savings Experiment (San Cristobal District)						
Tertiary Experiment (Suba District)						
Years of potential treatment exposure	6	5	4	3	2	1

Year	Datasets	Timeline of Events					
2005	SISBEN Bogota 2004 (Eligibility, baseline covariates) Application lists (Identifying information and randomization roster)	Randomization					
2006	Secondary Enrollment Records (2006) Secondary Exit Exam (ICFES, 2006)	Secondary Enrollment (G7)	Secondary Enrollment (G8)	Secondary Enrollment (G9)	Secondary Enrollment (G10)	Secondary Enrollment (G11)	Secondary Graduation
2007	Secondary Enrollment Records (2007) Secondary Exit Exam (ICFES, 2007)	Secondary Enrollment (G8)	Secondary Enrollment (G9)	Secondary Enrollment (G10)	Secondary Enrollment (G11)	Secondary Graduation	
2008	Secondary Enrollment Records (2008) Secondary Exit Exam (ICFES, 2008)	Secondary Enrollment (G9)	Secondary Enrollment (G10)	Secondary Enrollment (G11)	Secondary Graduation		
2009	Secondary Exit Exam (ICFES, 2009)	Secondary Enrollment (G10)	Secondary Enrollment (G11)	Secondary Graduation			
2010	Secondary Exit Exam (ICFES, 2010)	Secondary Enrollment (G11)	Secondary Graduation				
2011	Secondary Exit Exam (ICFES, 2011)	Secondary Graduation					
2012	Secondary Exit Exam (ICFES, 2012) Tertiary Education Database (SPADIES, 2012)	Secondary Graduation	Tertiary Education Outcomes 2006-2012 (1 to 6 years post secondary graduation, 7 years after randomization)				
2013							
2014							
2015							
2016	Tertiary Education Database (SPADIES, 2016)		Tertiary Education Outcomes 2006-2016 (5 to 10 years post secondary graduation, 11 years after randomization)				

Notes: The figure shows a timeline of the program assuming on-time secondary school progression until graduation for ease of exposition (our secondary enrollment data allows to differentiate between on-time vs. any-time secondary enrollment). The top panel of the figure shows the different grades incentivized in the Basic/Savings and Tertiary Experiments and the potential years of exposure for students who are in different grades at baseline. Students receive no incentives beyond secondary graduation. The bottom panel shows a calendar timeline from randomization (2005) to our last year of data (2016). For each year, the figure shows the outcomes and potential grades that we observe, given available administrative data sources for each year. For 2009 and 2010 we do not have secondary enrollment data, which is why secondary enrollment outcomes for those grades are shown in a light shade. We have no contemporaneous tertiary enrollment data for 2013-2015 but the Tertiary Education Database (SPADIES 2016) retrospectively covers those years.

Appendix A. Composition of randomization groups with complete identifying information

In this appendix we assess whether or not information is differentially available for particular types of students. Tables A1 and A2 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 A1. Comparison of students with valid ID information in application data

	Basic/Savings Experiment				Tertiary Experiment	
	Control Mean (s.d)	Basic-Control (s.e)	Savings-Control (s.e)	Basic-Savings (s.e)	Control Mean (s.d.)	Tertiary-Control (s.e)
	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)
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)
	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)
Male	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)
	C. Household Characteristics					
Single-Headed	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)
Members 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)
	D. Poverty Measures					
Neighborhood 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)

Notes: Table presents a comparison of students in each of the listed research groups for whom a valid national ID number is available in application records for matching to various administrative datasets with outcome measures.. 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 (1 or 2), which is a geographic measure of poverty as well as the SISBEN (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 A2. Comparison of students with valid last (family) name information in application data

	Basic/Savings Experiment				Tertiary Experiment	
	Control Mean (s.d)	Basic-Control (s.e)	Savings-Control (s.e)	Basic-Savings (s.e)	Control Mean (s.d.)	Tertiary-Control (s.e)
	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)
	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)
Male	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)
	C. Household Characteristics					
Single-Headed	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)
Members 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)
	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)

Notes: Table presents a comparison of students in each of the listed research groups for whom a valid last (family) name is available in application records for matching to various administrative datasets with outcome measures. 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 (1 or 2), which is a geographic measure of poverty as well as the SISBEN (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.

Appen Table B1. Impact heterogeneity by household income (measured by SISBEN household score at baseline, lower score=more disadvantaged)

PANEL A. Outcomes:	On-time Secondary Enrollment			Secondary Graduation		
		Experiment			Experiment	
	Basic/	Tertiary	Pooled	Basic/	Tertiary	Pooled
	(1)	(2)	(3)	(4)	(5)	(6)
Basic Treatment	0.036		0.020	0.017		0.007
	(0.035)		(0.044)	(0.020)		(0.052)
Savings Treatment	0.035		-0.001	-0.019		-0.015
	(0.033)		(0.046)	(0.021)		(0.035)
Tertiary Treatment		0.043	0.040		0.015	0.045
		(0.067)	(0.051)		(0.037)	(0.034)
Basic Treatment*SISBEN	-0.001		-0.001	0.000		0.001
	(0.002)		(0.003)	(0.001)		(0.003)
Savings Treatment*SISBEN	0.000		0.003	0.002		0.003
	(0.003)		(0.004)	(0.002)		(0.002)
Tertiary Treatment*SISBEN		-0.002	-0.001		-0.001	-0.003
		(0.005)	(0.004)		(0.002)	(0.002)
SISBEN (from 0 to 22)	0.002	0.004	0.000	0.018***	0.005	0.009***
	(0.003)	(0.005)	(0.003)	(0.003)	(0.004)	(0.003)
H0: Basic*SISBEN = Saving*SISBEN						
p-value	0.65		0.34	0.22		0.52
H0: Basic*SISBEN = Tertiary*SISBEN						
p-value			1.00			0.26
H0: Secondary*SISBEN = Tertiary*SISBEN						
p-value			0.28			0.01
N	9,937	2,345	6,320	10,947	2,544	6,905
R^2	0.194	0.235	0.221	0.118	0.101	0.089
Control Mean	0.51	0.72	0.68	0.68	0.83	0.80
Grades at Registration	All	All	9-11	All	All	9-11

PANEL B. Outcomes:	Medium-term Tertiary Enrollment			Long-term Tertiary Enrollment		
		Experiment			Experiment	
	Basic/	Tertiary	Pooled	Basic/	Tertiary	Pooled
	(1)	(2)	(3)	(4)	(5)	(6)
Basic Treatment	0.040*		0.059	0.029		0.049
	(0.021)		(0.041)	(0.024)		(0.048)
Savings Treatment	0.012		0.022	0.007		0.011
	(0.025)		(0.045)	(0.027)		(0.057)
Tertiary Treatment		0.117	0.121*		-0.012	0.037
		(0.073)	(0.062)		(0.063)	(0.056)
Basic Treatment*SISBEN	-0.003*		-0.004	-0.002		-0.003
	(0.001)		(0.003)	(0.002)		(0.003)
Savings Treatment*SISBEN	0.000		0.001	-0.000		0.001
	(0.002)		(0.004)	(0.002)		(0.004)
Tertiary Treatment*SISBEN		-0.004	-0.005		0.003	-0.001
		(0.005)	(0.004)		(0.004)	(0.004)
SISBEN (from 0 to 22)	0.011***	0.012**	0.013***	0.014***	0.007	0.016***
	(0.003)	(0.006)	(0.004)	(0.003)	(0.006)	(0.004)
H0: Basic*SISBEN = Saving*SISBEN						
p-value	0.11		0.12	0.49		0.25
H0: Basic*SISBEN = Tertiary*SISBEN						
p-value			0.92			0.50
H0: Secondary*SISBEN = Tertiary*SISBEN						
p-value			0.22			0.67
N	10,947	2,544	6,905	10,947	2,544	6,905
R^2	0.081	0.099	0.094	0.079	0.097	0.089
Control Mean	0.21	0.35	0.31	0.32	0.43	0.40
Grades at Registration	All	All	9-11	All	All	9-11

Notes:Table presents impact heterogeneity estimates by baseline SISBEN score on four outcomes: on-time secondary enrollment, secondary school graduation, tertiary enrollment in the medium and in the long term. The model includes the treatment dummy, SISBEN score and the interaction of the two. The score is a proxy for poverty/income, ranges from 0 (extremely poor) to 22 (less poor). All regressions control by baseline socio-demographic characteristics and schools fixed effects. Standard errors are clustered at the school level. Statistical significance at the one-, five- and ten-percent level is indicated by ***, ** and * respectively.

Appen Table B2. Impact heterogeneity by gender

PANEL A. Outcomes:	On-time Secondary Enrollment			Secondary Graduation		
		Experiment			Experiment	
	Basic/	Tertiary	Pooled	Basic/ Savings	Tertiary	Pooled
	(1)	(2)	(3)	(4)	(5)	(6)
Basic Treatment	0.029*		0.0164	0.015		0.0323
	(0.016)		(0.0227)	(0.012)		(0.0198)
Savings Treatment	0.029**		0.0387**	0.007		0.0515***
	(0.013)		(0.0186)	(0.013)		(0.0166)
Tertiary Treatment		0.018	0.0313*		0.017	0.0148
		(0.019)	(0.0176)		(0.020)	(0.0164)
Basic Treatment*Gender	-0.011		-0.0248	0.014		-0.0241
	(0.020)		(0.0290)	(0.020)		(0.0283)
Savings Treatment*Gender	0.013		-0.00744	0.006		-0.0497*
	(0.020)		(0.0305)	(0.018)		(0.0287)
Tertiary Treatment*Gender		0.008	-0.0197		-0.023	-0.0217
		(0.037)	(0.0311)		(0.029)	(0.0240)
Gender (1=male)	-0.075***	-0.064***	-0.0375**	-0.082***	0.000	-4.83e-05
	(0.019)	(0.022)	(0.0185)	(0.014)	(0.025)	(0.0155)
H_0: Basic*Gender = Saving*Gender						
p-value	0.21		0.596	0.63		0.348
H_0: Basic*Gender = Tertiary*Gender						
p-value			0.895			0.941
H_0: Secondary*Gender = Tertiary*Gender						
p-value			0.737			0.373
N	9,937	2,345	6,320	10,947	2,544	6,905
R^2	0.195	0.235	0.221	0.118	0.101	0.089
Control Mean	0.51	0.72	0.681	0.68	0.83	0.805
Grades at Registration	All	All	9-11	All	All	9-11

PANEL B. Outcomes:	Medium-term Tertiary Enrollment			Long-term Tertiary Enrollment		
		Experiment			Experiment	
	Basic/	Tertiary	Pooled	Basic/ Savings	Tertiary	Pooled
	(1)	(2)	(3)	(4)	(5)	(6)
Basic Treatment	0.015		0.010	0.020		0.033
	(0.011)		(0.024)	(0.018)		(0.030)
Savings Treatment	0.016		0.032	0.003		0.023
	(0.010)		(0.020)	(0.011)		(0.020)
Tertiary Treatment		0.070**	0.064**		0.044	0.046*
		(0.027)	(0.027)		(0.030)	(0.026)
Basic Treatment*Gender	-0.010		-0.006	-0.028		-0.054
	(0.018)		(0.031)	(0.025)		(0.037)
Savings Treatment*Gender	-0.001		0.007	-0.002		0.011
	(0.016)		(0.032)	(0.019)		(0.032)
Tertiary Treatment*Gender		-0.029	-0.013		-0.041	-0.042
		(0.030)	(0.030)		(0.037)	(0.029)
Gender (1=male)	0.002	0.026	0.010	-0.003	0.028	0.027*
	(0.012)	(0.022)	(0.015)	(0.012)	(0.024)	(0.014)
H_0: Basic*Gender = Saving*Gender						
p-value	0.57		0.72	0.29		0.14
H_0: Basic*Gender = Tertiary*Gender						
p-value			0.83			0.76
H_0: Secondary*Gender = Tertiary*Gender						
p-value			0.59			0.15
N	10,947	2,544	6,905	10,947	2,544	6,905
R^2	0.081	0.099	0.094	0.079	0.097	0.090
Control Mean	0.21	0.35	0.31	0.32	0.43	0.40
Grades at Registration	All	All	9-11	All	All	9-11

Notes: Table presents impact heterogeneity estimates by student gender on four outcomes: on-time secondary enrollment, secondary school graduation, tertiary enrollment in the medium- and in the long-term. All regressions control for baseline socio-demographic characteristics and schools fixed effects. Standard errors are clustered at the school level. Statistical significance at the one-, five- and ten-percent level is indicated by ***, ** and * respectively.

Appendix C. Additional Robustness Checks

Table C1. Joint Hypothesis Tests: Randomization Inference

Hypothesis	Medium-Term Educational Outcomes (1)	Long-term Educational Outcomes (2)	All Outcomes (3)
H_0: Basic = 0			
min p-value	0.034	0.707	0.090
max p-value	0.034	0.707	0.091
randomized p-value	0.034	0.707	0.091
H_0: Savings = 0			
min p-value	0.009	0.284	0.023
max p-value	0.010	0.284	0.024
randomized p-value	0.010	0.284	0.024
H_0:Tertiary = 0			
min p-value	0.503	0.860	0.957
max p-value	0.503	0. 860	0. 957
randomized p-value	0.503	0. 860	0. 957
H_0: Basic = Savings			
All Students			
min p-value	0.067	0.065	0.048
max p-value	0.068	0.066	0.049
randomized p-value	0.068	0.066	0.049
Upper Secondary (Grades 9-11)			
min p-value	0.00	0.007	0.001
max p-value	0.001	0.008	0.001
randomized p-value	0.001	0.008	0.001
Lower Secondary (Grades 6-8)			
min p-value	0.156	0.045	0.056
max p-value	0.157	0.045	0.057
randomized p-value	0.157	0.045	0.057
H_0: Savings=Tertiary			
Upper Secondary (Grades 9-11)			
min p-value	0.042	0.746	0.361
max p-value	0.042	0.747	0.362
randomized p-value	0.042	0.746	0.361
Educational outcomes included	On-time Secondary Enrollment, Secondary Graduation, Medium-term Tertiary Enrollment	Long-term Tertiary Enrollment, Long-term On-time Tertiary Enrollment, Long-term Tertiary Graduation	On-time Secondary Enrollment, Secondary Graduation, Long-term Tertiary Enrollment, Long-term On-time Tertiary Enrollment, Long-term Tertiary Graduation

Notes: Table presents the results of an omnibus joint significance test computing randomization-c exact p-values based on 2000 randomization permutations (see Table 6 for details on estimating equation and Young 2016 for details on the randomization test procedure).

Table C2. Medium-term tertiary enrollment conditional on reaching grade 9

	Basic-Savings Experiment	
	Medium-term Tertiary Enrollment	Medium-Term Tertiary Enrollment for Students who Reach at Least Grade 9
	(1)	(2)
Basic Treatment	0.010	0.012
	(0.009)	(0.011)
Savings Treatment	0.015*	0.014
	(0.009)	(0.011)
Control Mean	0.21	0.24
N	10,947	7,598
R^2	0.081	0.079
Grades at Registration	All (6-11)	All (6-11)

Notes: Table shows medium-term tertiary enrollment effects in the Basic/Savings experiment. Column 1 reproduces results from column 1, Table 5. Column 2 restricts the sample to individuals who actually reached grade 9. All coefficients are estimated using Equation (1), controlling by baseline socio-demographic characteristics and schools fixed effects. Standard errors are clustered at the school level. Statistical significance at the one-, five- and ten-percent level is indicated by ***, ** and * respectively.

Table C3. Bounds on tertiary enrollment outcomes

Outcome	Medium-term Tertiary Enrollment			Long-Term Tertiary Enrollment		
	Treatments			Treatments		
	Basic	Savings	Tertiary	Basic	Savings	Tertiary
	(1)	(2)	(3)	(4)	(5)	(6)
A. OLS Regression, no controls						
Basic Treatment	0.007			0.005		
	(0.015)			(0.017)		
Savings Treatment		0.033**			0.026*	
		(0.014)			(0.015)	
Tertiary Treatment			0.058***			0.034
			(0.020)			(0.022)
B. Bounds						
Lower	-0.014	0.017	0.056*	-0.016	0.007	0.030
	(0.022)	(0.021)	(0.030)	(0.025)	(0.022)	(0.022)
Upper	0.012	0.045**	0.068***	0.009	0.036*	0.043*
	(0.021)	(0.020)	(0.023)	(0.024)	(0.021)	(0.026)
Grades at registration	9-11	9-11	9-11	9-11	9-11	9-11
N	2,992	2,993	2,544	2,992	2,993	2,544

Notes: Table shows treatment effect estimates on medium and long-term tertiary enrollment for grades 9 to 11 at baseline. Estimates in Panel A are from OLS regressions without controls. Panel B shows lower and upper bound estimates on these outcomes using the Lee (2009) approach, and control for gender and household SISBEN level at baseline. Standard errors for lower and upper bounds are estimated using bootstrap procedure.

Append Table C4. Tertiary enrollment outcomes conditioning on taking the secondary exit exam and enrolling in tertiary education

PANEL A. Outcomes	Conditional on Taking Secondary Exit Exam					
	Tertiary On-time			Tertiary Graduation		
		Treatments			Treatments	
	Basic/	Tertiary	Pooled	Basic/ Savings	Tertiary	Pooled
	(1)	(2)	(3)	(4)	(5)	(6)
Basic Treatment	0.001		0.010	0.003		0.018
	(0.009)		(0.017)	(0.007)		(0.012)
Savings Treatment	0.012		0.050***	0.012		0.025*
	(0.011)		(0.017)	(0.010)		(0.014)
Tertiary Treatment		0.031*	0.032*		0.008	0.009
		(0.018)	(0.018)		(0.016)	(0.016)
H_0: Basics vs. Savings						
p-value		0.24	0.03	0.28		0.56
H_0: Basic vs. Tertiary						
p-value			0.39			0.64
H_0: Savings vs. Tertiary						
p-value			0.47			0.42
N	7,533	2,127	5,595	7,533	2,127	5,595
R^2	0.056	0.104	0.086	0.038	0.094	0.065
Control Mean	0.24	0.28	0.24	0.08	0.14	0.12
Grades at Registration	All	All	9-11	All	All	9-11

PANEL B. Outcomes	Conditional on Tertiary Enrollment					
	Tertiary On-time			Tertiary Graduation		
		Treatments			Treatments	
	Basic/ Savings	Tertiary	Pooled	Basic/ Savings	Tertiary	Pooled
	(1)	(2)	(3)	(4)	(5)	(6)
Basic Treatment	0.005		0.021	0.015		0.043*
	(0.023)		(0.035)	(0.015)		(0.023)
Savings Treatment	0.029		0.078***	0.030		0.039
	(0.020)		(0.025)	(0.020)		(0.028)
Tertiary Treatment		0.035	0.037		0.006	0.006
		(0.029)	(0.027)		(0.027)	(0.026)
H_0: Basics vs. Savings						
p-value	0.32		0.10	0.41		0.88
H_0: Basic vs. Tertiary						
p-value			0.73			0.28
H_0: Savings vs. Tertiary						
p-value			0.26			0.36
N	3,527	1,131	2,796	3,527	1,131	2,796
R^2	0.071	0.116	0.090	0.053	0.138	0.091
Control Mean	0.55	0.57	0.52	0.19	0.27	0.24
Grades at Registration	All	All	9-11	All	All	9-11

Notes: Table presents estimates of the treatment effects on two tertiary outcomes: on-time tertiary enrollment and tertiary graduation, conditioning on taking the secondary exit exam (columns 1-6) and conditioning on enrolling in tertiary (columns 7-12). All coefficients are estimated using Equation (1), controlling by baseline socio-demographic characteristics and schools fixed effects. Standard errors are clustered at the school level. Statistical significance at the one-, five- and ten-percent level is indicated by ***, ** and * respectively.

Append Table C5. Main outcomes, only households in SISBEN level II at baseline

PANEL A. Outcomes:	On-time Secondary Enrollment			Secondary Graduation		
	Experiments			Experiments		
	Basic/ Savings	Tertiary	Pooled	Basic/ Savings	Tertiary	Pooled
	(1)	(2)	(3)	(4)	(5)	(6)
Basic Treatment	0.018		-0.003	0.020*		0.022
	(0.014)		(0.020)	(0.011)		(0.016)
Savings Treatment	0.029**		0.042**	0.016		0.032*
	(0.011)		(0.019)	(0.013)		(0.018)
Tertiary Treatment		0.024	0.024		0.007	0.007
		(0.017)	(0.017)		(0.014)	(0.015)
H_0: Basics vs. Savings						
p-value	0.43		0.03	0.70		0.52
H_0: Basic vs. Tertiary						
p-value			0.33			0.56
H_0: Savings vs. Tertiary						
F-Stat			0.51			0.91
p-value			0.48			0.34
N	6,332	1,850	4,555	6,973	2,006	4,963
R^2	0.191	0.262	0.232	0.103	0.120	0.100
Control Mean	0.54	0.72	0.69	0.71	0.84	0.82
Grades at Registration	All	All	9-11	All	All	9-11

PANEL B. Outcomes:	Medium-term Tertiary Enrollment			Long-Term Tertiary Enrollment		
	Experiments			Experiments		
	Basic/ Savings	Tertiary	Pooled	Basic/ Savings	Tertiary	Pooled
	(1)	(2)	(3)	(4)	(5)	(6)
Basic Treatment	0.002		-0.008	0.002		-0.011
	(0.008)		(0.014)	(0.011)		(0.016)
Savings Treatment	0.013		0.033*	-0.003		0.024
	(0.011)		(0.018)	(0.011)		(0.017)
Tertiary Treatment		0.047**	0.049**		0.037	0.038
		(0.022)	(0.022)		(0.024)	(0.025)
H_0: Basics vs. Savings						
p-value	0.31		0.01	0.71		0.10
H_0: Basic vs. Tertiary						
p-value			0.04			0.09
H_0: Savings vs. Tertiary						
F-Stat			0.32			0.23
p-value			0.58			0.63
N	6,973	2,006	4963	6,973	2,006	4,963
R^2	0.082	0.111	0.102	0.071	0.107	0.095
Control Mean	0.23	0.36	0.33	0.36	0.44	0.42
Grades at Registration	All	All	9-11	All	All	9-11

Notes: Table shows estimates of the various experiments and treatments for students in households who at baseline were in level II of the SISBEN national poverty assessment tool. Outcomes are on-time secondary enrollment, secondary graduation, and tertiary enrollment in the medium- and long-term. All coefficients are estimated using Equation (1) controlling for baseline socio-demographic characteristics and schools fixed effects. Standard errors are clustered at the school level. Statistical significance at the one-, five- and ten-percent level is indicated by ***, ** and * respectively.

Table C6. Secondary graduation and medium-term tertiary enrollment outcomes excluding students with missing administrative secondary enrollment matching information

	Secondary Graduation			Medium-Term Tertiary Enrollment		
	Treatments			Treatments		
	Basic/ Savings	Tertiary	Pooled	Basic/ Savings	Tertiary	Pooled
	(1)	(2)	(3)	(4)	(5)	(6)
Basic Treatment	0.024**		0.025	0.014		0.012
	(0.011)		(0.019)	(0.009)		(0.016)
Savings Treatment	0.013		0.027	0.016*		0.038***
	(0.011)		(0.017)	(0.009)		(0.013)
Tertiary Treatment		0.015	0.013		0.066***	0.065***
		(0.014)	(0.013)		(0.021)	(0.021)
H_0: Basics vs. Savings						
p-value	0.21		0.92	0.78		0.06
H_0: Basic vs. Tertiary						
p-value			0.62			0.06
H_0: Savings vs. Tertiary						
p-value			0.56			0.27
N	9,937	2,345	6,320	9,937	2,345	6,320
R^2	0.19	0.13	0.11	0.09	0.12	0.12
Control Mean	0.68	0.84	0.81	0.21	0.35	0.31
Grades at Registration	All	All	9-11	All	All	9-11

Notes: Table shows estimates of the effects on secondary graduation and medium-term tertiary enrollment excluding students without sufficient information to match to the enrollment data. All coefficients are estimated using Equation (1) controlling for baseline socio-demographic characteristics and schools fixed effects. Standard errors are clustered at the school level. Statistical significance at the one-, five- and ten-percent level is indicated by ***, ** and * respectively.