Compensation vs. Reinforcement: Experimental Identification of Parental Aversion to Inequality in Offspring

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Abstract

The allocation of resources within the household has been extensively theorized, but empirical evidence on this topic has been very scarce. Endogeneity concerns hinder this type of analysis due to the lack of identifying variation within the household. In this paper, we overcome these difficulties by exploiting a unique setting that introduced random variation in resource allocation within households. We evaluate the effects of a program that provided alternative delivery methods of conditional cash transfers in Bogotá, Colombia, and allocated resources at the student level. The individual randomization implied that some households had treated and untreated siblings, allowing us to extend the analysis to estimate spillover effects of the program on beneficiaries and their siblings. Students were randomly allocated to a standard design and a design that prioritized enrollment in tertiary education. We find that standard delivery methods increase educational outcomes for treated children but decrease the same outcomes for untreated siblings of treated students. We rationalize the results by estimating a structural model that uses the standard delivery method for estimation and the alternative delivery method for validation

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1 Introduction

Household theory treats the household as a unit, and different models treat allocation of resources and tasks in different ways. In Becker's canonical model, parents allocate resources among children based on initial endowment of skills (Becker and Lewis, 1973). Several lines of research, including bargaining power strategy, have expanded this theoretical frontier to include decisions within households as determinants of economic behavior (Chiappori, 1988). While this theoretical literature has been extensively developed, empirical contributions have been more limited in their ability to adequately test some of the model's predictions.

Empirical estimations of household decisions are remarkably challenging to perform due to numerous potential sources of bias. Resource allocation among household members is based on unobserved characteristics, which hinder the existence of exogenous variation in resources and incentives. This paper contributes to addressing this problem by analyzing a unique setting that randomly assigned cash transfers within the household.

In 2005, local authorities in Bogotá, Colombia, implemented an evaluation of alternative delivery methods of conditional cash transfer programs aimed at reducing dropout rates in secondary education among low-income students. The intervention was implemented in two out of the 12 localities of Bogotá, under the *Conditional Subsidies for School Attendance Program* ("Subsidios Condicionados a la Asistencia Escolar", in Spanish). Participating students were randomized into two different treatments. The first treatment followed standard delivery methods and provided monthly stipends summing up to a fixed annual rate of 300,000 Colombian pesos (approximately $150 USD) (referred to as the "Basic" treatment hereafter). The second treatment varied the timing of the transfer. It provided two-thirds of the standard amount (i.e., 20,000 COP, monthly), but upon high school graduation, students received an additional amount of 600,000 COP ($300 USD) immediately if they enrolled in a tertiary education program or one year after if they did not enroll (referred to as the "College" treatment hereafter).

The program randomly assigned treatment at the *student* level, making this a very unique setting. In households with multiple participants, all children could be treated, untreated, or some treated. This allocation method differs from previous studies because it introduces within-household variation in treatment status, randomly varying the allocation of resources within the household. This approach allows us to estimate spillover effects of transfers within households and empirically test the implications of the canonical models of household resource allocation.

We exploit this unique setting to estimate the direct and indirect effects of conditional cash transfers on resource allocation within the household. We compare the randomly assigned treated groups to estimate the direct effects of the basic and college treatments. To estimate the indirect effects of the treatments, we compare untreated individuals whose siblings were treated with untreated individuals in households where anyone was treated. We restrict this comparison to households with only two children since larger households have a higher likelihood of having treated siblings.

We combine a comprehensive set of data from various sources to estimate short and long run outcomes. In the short run, we analyze data collected within a year after the intervention to measure school attendance, school enrollment, and working status. In the long run, we link administrative data sets to identify high school graduation, college enrollment, and college graduation.

The basic treatment had short run direct effects that disappear over time. More importantly, we find that this treatment reduced school attendance and enrollment of untreated siblings, leading to a substantial long-term decrease in college enrollment and graduation. These effects are sizable implying a 12 and 30 percent decrease in college enrollment and graduation rates, respectively, compared to the control mean.

In contrast, the college treatment increased attendance and enrollment of treated children but had no spillover effects on untreated siblings. Treated students showed increased school attendance, enrollment, and a reduced likelihood of working during high school. These effects resulted in a notable long-term increase of 12 percent in college enrollment compared to the control mean. Nonetheless, we do not observe any spillover effects of the college treatment among untreated siblings.

Our findings suggest that when resources are randomly allocated, parents reinforce the resources of their offspring by prioritizing short-term educational investments. To explain these results and explore the key mechanisms driving our findings, we develop a dynamic structural model. This model allows households to make decisions about which children attend school, their level of school attendance, and which of them work. The model considers decisions made by households for each child from late childhood through the teenage years and into early adulthood.

Following Galiani, Murphy, and Pantano (2015) and Galiani and Pantano (2022), we estimate the model by exploiting only a portion of the randomized variation available in the experiment. The remaining portion is reserved for model validation, following the approach of Todd and Wolpin (2006). Specifically, we employ a simulation-based estimation approach that seeks to identify the structural parameters that best replicate the effects of the Basic treatment (both short-run and long-run, direct and indirect) as observed in the experiment. We then validate the model's performance out of sample by assessing its ability to reproduce the distinct set of estimated effects found for the College treatment.

The conditional cash transfer experiment at the student level introduces fully exogenous, randomized variation across siblings in the opportunity costs of sending them to school. This variation provides a unique opportunity to convincingly identify the extent of parental aversion to inequality in educational outcomes among their offspring, a parameter introduced by Behrman, Pollak, and Taubman (1982) in the literature on parental investment in children.

Once the model is estimated and validated, we use it to understand the mechanisms underlying the observed negative spillovers on untreated children who have a sibling treated with either the Basic or College treatment. Additionally, we use the model to inform the optimal design of conditional cash transfers, taking into account spillovers across siblings.

The rest of the paper is organized as follow. The next section summarizes the relevant literature and places our contributions in the context of that literature. Section 3 describes the experimental intervention and Section 4 describes our approach to estimate the treatment effects from the experiment. Section 5 provides details on the extensive sets of data sources that we use. Section 6 previews the main reduced form findings from the experiment that we later use to estimate and validate the structural model. Section 7 fleshes out the structural model in detail and Section 8 provides details of our simulation-based, structural estimation strategy.

2 Related Literature and Contribution

This paper builds on work performed by Barrera-Osorio et al. (2011) and Barrera-Osorio et al. (2019), who estimate the short and long run effects, respectively, of the *Conditional Subsidies for School Attendance Program*. These papers mainly focus on estimating the direct effects of the program. We validate their results and extend the analysis to include the estimation of spillover effects on siblings and the structural modeling of the transfers. The inclusion of the model contributes to the understanding of the underlying mechanisms behind the cash transfers and the dynamics of resource reallocation within households.

In addition, our work contributes to four lines of literature. First, our results relate to the broad body of work that examines the impacts of conditional cash transfers on different margins. Since the initial evaluations of *Progresa* in Mexico, multiple studies have assessed the short-run effects of conditional cash transfers on a wide range of outcomes. These studies generally show that conditional cash transfers increase school enrollment across different settings. For example, Behrman, Sengupta, and Todd (2005), Todd and Wolpin (2006), and Attanasio, Meghir, and Santiago (2012) analyze the short-term effects of *Progresa* and consistently find positive effects on school attainment using reduced form and structural methods. Moreover, Attanasio, Fitzsimons, Gomez, Gutiérrez, Meghir, and Mesnard (2010) evaluates a conditional cash transfer program in Colombia (i.e., *familias en acción*) and again finds increases in school enrollment.

By contrast, evidence on the long-run effects of conditional cash transfers is relatively limited in the literature. The available evidence suggests that conditional cash transfers have positive effects on schooling but limited effects on longer-term outcomes such as cognitive skills, learning, socio-emotional skills, employment, and earnings (Behrman, Parker, and Todd, 2011; Barham, Macours, and Maluccio, 2017; Barrera-Osorio, Linden, and Saavedra, 2019; Cahyadi, Hanna, Olken, Prima, Satriawan, and Syamsulhakim, 2020; Molina Millán, Macours, Maluccio, and Tejerina, 2020). Many of these results face methodological challenges and are not fully capable to determine whether the insignificant point estimates reflect a lack of effect or methodological difficulties (Millán, Barham, Macours, Maluccio, and Stampini, 2019).

To the best of our knowledge, our work is the first in the literature on conditional cash transfers to estimate spillover effects within the household. Previous studies typically analyze interventions where transfers are allocated at the household level, which hinders the estimation of within-household spillovers. Our work suggests that conditional cash transfers have the power to alter resources and behavior within the household when they are assigned at the student level.

Second, our work contributes to the literature on spillover effects of policy interventions (see Angelucci and Maro (2016) for a review). In a seminal paper, Miguel and Kremer (2004) measure the spillover effects of deworming on health and school participation. Additionally, some studies specifically focus on spillover effects across treated and untreated households in conditional cash transfer programs. For instance, Angelucci and De Giorgi (2009) and Bobonis and Finan (2009) identify positive spillover effects of cash transfers on ineligible households and neighborhood peers. We contribute to this literature by exploiting random variation to identify within-household spillover effects. These effects analyze different margins that complement those on ineligible households and neighborhood peers.

Third, we contribute to a line of research that emphasizes the value of combining randomized control trials with structural modeling (Todd and Wolpin, 2020). A small but growing literature integrates dynamic structural models and randomized control trials to evaluate conditional cash transfer programs (Todd and Wolpin, 2006; Attanasio, Meghir, and Santiago, 2012; Galiani, Pantano, and Shi, 2022). More generally, we contribute to a stream of this literature, as discussed in Galiani and Pantano (2022), that emphasizes the dual role of multi-arm randomized control trials, which can be used to *both* estimate and validate structural models (e.g., Galiani et al. (2015), and Galiani et al. (2022)).

Fourth, and more substantively, our work contributes to the literature that explores the question of whether parents tend to compensate or reinforce differences among siblings. In a seminal paper, Becker and Tomes (1976) first proposed this question by relaxing the assumption of equal endowments in earlier work by Becker and Lewis (1973) on the quantity-quality trade-off. In a highly influential follow-up to this paper, Behrman, Pollak, and Taubman (1982) introduced parental aversion to inequality in offspring outcomes and used a sample of twins to estimate a static, single-shot structural model, concluding that parents display substantial aversion to inequality in offspring and this leads them to compensate for differences in initial endowments. Rosenzweig and Wolpin (1988) introduced sequential decision-making and uncertainty into this literature, noting that the single-shot model is simple but quite unrealistic, as the parental investment process is dynamic and sequential. More recently, Aizer and Cunha (2012) exploit exogenous variation from the initial launch of Head Start in 1966 and show that parents, through their investments, tend to reinforce initial endowment differences across siblings. In a recent relevant paper, Carneiro, Rasul, and Salvati (2023) study a prenatal intervention that provides information and cash transfers to parents based on a verified pregnancy of a target child. Their results suggest that parents reinforce inputs across siblings in the first 1000 days of life window.

Our work adds to the literature on compensation vs. reinforcement of differences among siblings by exploiting a unique setting that introduced random variation across siblings within the household and by observing long-term effects up to graduation from college. Furthermore, we formulate and estimate a dynamic structural model that rationalizes the decisions a household makes about each of the siblings and the resulting spillover effects. The substance of this model is inspired by the foundational work of Behrman, Pollak, and Taubman (1982), but its implementation mechanics rely on the more modern formulation of estimable dynamic structural models, as in Todd and Wolpin (2006). Moreover, the experimental variation from the conditional cash transfer intervention provides a key source of identification for the parental inequality aversion parameter. Furthermore, a held-out group of households where a lottery-winning sibling was treated with an alternative cash transfer design is used for out-of-sample validation to enhance the credibility of the model. The estimated model allows us to understand the underlying mechanisms that generate spillovers across siblings and helps us think about the optimal conditional cash transfer design in the context of those spillovers.

3 Experimental Intervention

Dropout rates in secondary education were high at the beginning of the 2000s in Latin America (Bassi, Busso, and Muñoz, 2015). Many countries, including Colombia, adopted strong policies targeted at decreasing school dropout rates. Conditional cash transfers had proven to be a successful policy after showing promising results across the region (Behrman, Sengupta, and Todd, 2005; Todd and Wolpin, 2006; Attanasio, Fitzsimons, Gomez, Gutiérrez, Meghir, and Mesnard, 2010; Attanasio, Meghir, and Santiago, 2012).

Therefore, in an effort to address high dropout rates in secondary education, public authorities in Bogotá, Colombia, carried out in 2005 the evaluation of the *conditional Subsidies for School Attendance Program*. The intervention tested alternative delivery methods of conditional cash transfers. It was implemented in two out of the 12 localities of Bogotá, and targeted exclusively low-income households whose children were enrolled in secondary education.

3.1 Timeline of Intervention

The experiment began in 2004, during which baseline data was collected prior to the intervention. The information was gathered using the "System of Identification of Social Program Beneficiaries" (SISBEN), which collects data on low-income households and categorizes them to target social spending.[1]

Two localities in Bogotá with a high prevalence of low-income households, San Cristóbal and Suba, were selected as the sites to conduct the experiment. A robust

[1]This system is originally named "Sistema de Identificación de Potenciales Beneficiarios de Programas Sociales" in Spanish, and it collects information about households that may be classified as low income. This information is used to calculate a poverty index designed to target social programs throughout the country.

advertising campaign was conducted in these two areas during January and early February of 2005. The program was promoted as one that offered incentives for school participation. Subsequently, in late February and March, a 15-day registration period was opened, allowing parents to enroll their children as potential candidates for the program.

To be eligible for the program, several criteria had to be met: 1) at least one parent had to be present during the registration process; 2) students had to have completed fifth grade; 3) students had to be enrolled in secondary education but not have graduated yet; and 4) the family needed to be classified within the two lowest categories of the SISBEN index. A total of 17,225 students (from 12,674 households) registered and met the criteria, with 63 percent from San Cristóbal and 36 percent from Suba.[2] The city allocated sufficient funds to provide subsidies to 56 percent of the students, with 39 percent of them in San Cristóbal and 17 percent in Suba.

Parents were not obligated to register all of their children in the household, resulting in some children being excluded from the experiment, even though their siblings could have been registered. Regarding the distribution of households, 70 percent (of the 12,674 households) registered only one individual, while 25 percent registered two individuals. Additionally, 5 percent of households registered three individuals, and the remaining 1 percent of households registered four or five individuals.

The assignment of treatment was carried out publicly and randomly on April 4, 2005, with strict surveillance, and the lists of beneficiaries were immediately printed. The randomization process was stratified based on site, type of school, grade, and gender.

Importantly, the randomization was performed at the *student* level, which means that some children within the same household were assigned to the treatment group, while others were not. This randomization created variation in treatment status within households, allowing for a comparison between treated and untreated individuals.

Later, between May and July 2005, baseline data on the participants was collected through a student-level survey. The survey was designed as a self-administered questionnaire that students completed during their classes. However, due to budget limitations, the survey and subsequent attendance data collection were carried out only in the 68 schools with the highest number of registered children. This subset of schools accounted for a total potential sample of 9,715 students, which represented 56 percent of the overall registered students.

The transfers under the program commenced in July 2005, which was during the middle of the school year. It is worth noting that the academic year for public schools in Colombia typically starts in January and concludes in November. Data collection on students' attendance took place between October and December 2005, utilizing direct observation methods. In February and March 2006, a follow-up survey was conducted. This survey was administered at the household level to ensure that students who had dropped out of school were accounted for. Remarkably, almost all of the

[2] We drop 84 because they were out of school before the program started.

individuals from the baseline (8,736 out of 8,896, or 98.14 percent) were successfully located and included in the follow-up survey.

3.2 Treatment Arms

The program aimed to evaluate various delivery methods of conditional cash transfers by randomly assigning students to two different treatment arms (Barrera-Osorio et al., 2011, 2019):

1. The "Basic" conditional cash transfer provided participant families with 300,000 Colombian pesos (approximately $150 USD) yearly if the student attended 80 percent of the classes. The money was distributed every two months, with beneficiary families receiving 60,000 pesos (30,000 pesos per month) in each installment.[3]

2. The "College" treatment involved varying the timing of the transfer. Under this treatment, students received two-thirds of the original bi-monthly amount (20,000 COP, monthly), but upon high school graduation, they received a total sum of 600,000 COP ($300 USD). The amount was granted immediately if the student enrolled in a tertiary education program, or after one year if the student did not enroll.

The transfer amount in the college treatment was larger than in the basic treatment, and the delivery method prioritized tertiary education enrollment. It is important to note that participating families were not aware of the existence of multiple treatment options at the time of registration.

Randomization by Sites. The implementation of treatment assignment differed between the two localities. In San Cristóbal, eligible participants ranging from 6th to 11th grades were randomly assigned to the control group or the basic treatment. On the other hand, in Suba, students in 6th to 8th grades were randomized into the control group or the basic treatment, while students in 9th to 11th grades were randomly assigned to either the control group or the college treatment.[4]

4 Experimental Analysis of the Intervention

The treatment was randomly assigned at the student level, so families with eligible children could have been fully treated, partially treated, or had received no treatment at all. We exploit this variation to estimate direct and indirect effects of the college and basic treatments.

[3] Some of the households assigned to this treatment received two-thirds of the amount (20,000 COP or roughly $10 USD) every two months, while the remaining one-third was saved in a bank account and made available at the beginning of the next academic year. Both delivery methods provided approximately the same amount of money, making them income-neutral. We pool together these two delivery methods as they are equivalent for the sake of this project.

[4] In our main specifications we include site and grade dummies to account for this heterogeneity.

4.1 Direct Effects

We estimate the direct effects on treated individuals by comparing outcomes of students that won the lottery and those who lost. These effects are computed at the short and at the long run. The estimations using short-run outcomes correspond to those presented in Barrera-Osorio et al. (2011). The estimations using long term outcomes correspond to those displayed in Barrera-Osorio et al. (2019).

To increase statistical power, we pool both sites into an aggregate estimation, but separate by treatment arms. Formally, we estimate:

$$Y_i = \beta^D_{B,0} + \beta^D_B \texttt{Basic}_i + \beta^D_{B,X} X_i + \varepsilon^D_i, \tag{1}$$

for the basic treatment, and :

$$Y_i = \beta^D_{C,0} + \beta^D_C \texttt{College}_i + \beta^D_{C,X} X_i + \varepsilon^D_i, \tag{2}$$

for the college treatment. Y_i corresponds to a given outcome measured either in the short or the long run. X_i corresponds to a vector that includes a site dummy, a dummy that takes the value of one if the student is in 9th grade or higher, and the interaction between these two. We compute heteroskedasticity-consistent standard errors.[5]

4.2 Indirect Effects

We compute indirect treatment effects on outcomes of siblings of treated students. Siblings are defined as those individuals living in the household that are descendants either of the household head or the spouse (or both). To estimate these effects we compare control individuals in households with treated siblings with control individuals in households where no one was treated. We restrict the estimations to households with only two registered children because this it the most reliable comparison. Including households with more registered students can be worrisome as a larger number of registered individuals increases the likelihood of receiving treatment.

To make things clear, assume three households, X, Y, and Z, each one with two registered students. By random assignment, one of these households, X, gets two treated siblings $X_1 = 1$ and $X_2 = 1$ (X_i corresponds to individual i in household X). The second household randomly gets one treated individual and another one not, $Y_1 = 1$, $Y_2 = 0$, while the third gets not treatment at all, $Z_1 = 0$, $Z_2 = 0$. Our strategy estimates the indirect sibling effects by comparing Y_2 with Z_1 and Z_2. None of these students was treated, but one, Y_2, had a sibling that did. Note that restricting to families with only two siblings, and excluding directly treated students, automatically drops those families with two registered students where both students were treated with in any way.

Formally, we estimate:

$$Y_i = \beta^S_{B,0} + \beta^S_B \texttt{Sibling Basic}_i + \beta^S_{B,X} X_i + \varepsilon^S_i, \tag{3}$$

[5] We implement Huber-White robust standard errors because the treatment varies at the individual level (Abadie et al., 2022). The results are, nonetheless, robust to alternative approaches like clustering standard errors at the school level.

for the basic treatment, and

$$Y_i = \beta^S_{C,0} + \beta^S_C \texttt{Sibling College}_i + \beta^S_{C,X} X_i + \varepsilon^S_i, \tag{4}$$

for the college treatment. Y_i again correspond to short and long run outcomes. $\texttt{Sibling College}_i$ and $\texttt{Sibling Basic}_i$ are dummies that take the value of one if a member in the household was treated either with the college or with the basic treatment, respectively, and zero if none were treated. X_i includes a dummy for the site of the experiment, and we compute heteroskedasticity-consistent standard errors.

5 Data

To track both short-term and long-term outcomes of the students in the experiment, we use a comprehensive set of data sources. Our data includes a universe of households that were surveyed one year after the randomization, which corresponds to nine months after the first transfer took place. Additionally, we compute long-term outcomes by merging the universe of households with administrative data sources that span up to 11 years after the intervention.[6]

5.1 Experimental Baseline Data

Our universe of analysis consists of the 17,225 students who were initially registered in the experiment. We identify these individuals using two primary data sources:

- *Registration Data:* This dataset includes information on the students who registered for the program and resided in the two treated localities in Bogotá. During the registration process, families provided basic information about the students.
- *Lottery Data:* These data provide information on the individual-level lottery conducted in San Cristóbal and Suba, stratified by different grade levels.

By combining the SISBEN data with the registration and lottery datasets, we are able to accurately identify and analyze the individuals who participated in the experiment.

5.2 Short Run Data Sources

After the intervention, data on enrollment were collected through administrative sources. Due to budget constraints, however, follow-up information on attendance and other outcomes was only gathered for students registered in the 68 schools with the highest number of registered students.

Enrollment Data: Information about school enrollment was gathered from administrative records of the city of Bogotá. Enrollment records include information about students enrolled at the beginning of the school year. School calendars in Bogotá start in January, implying that information on school enrollment was collected about six months after the intervention, at the beginning of the next academic year. Students

[6]For more information about the specific details regarding the merging of these data sets, please refer to Appendix E.

in the last grade (i.e., 11th grade) were not included as they were not eligible because they had, very likely, already graduated. Not all students were successfully merged with the registration data because they could have either attended schools that did not report attendance to the local authorities or there was simply not enough information for a successful merge. Nonetheless, enrollment status of more than 90 percent of participating students was matched with the administrative records (Barrera-Osorio, Bertrand, Linden, and Perez-Calle, 2011).

Attendance Data: The collection of attendance data took place during the last quarter of 2005. Only students in the 68 schools with the highest number of registered students were selected to be surveyed. Enumerators visited schools and classrooms randomly over a period of 13 weeks to record attendance information of the selected students. These data provided insights into the students' attendance patterns following the intervention.

Follow-up Survey: In March 2006, a follow-up survey was conducted to gather additional information of the participating students who were enrolled in the selected 68 schools. These students were interviewed in their households to ensure that those who had dropped out of school after the intervention were still observed. The research team located the families of 98 percent of students. This implies that more than half (56 percent) of the individuals who were originally registered in the experiment were interviewed during the follow-up survey in March 2006.[7].

Dropout among the three sub-samples measuring enrollment, attendance, and follow-up information was similar across the three different research groups. We can therefore rule out any selective attrition across treatment arms that could bias our research design. More information about this can be found in Barrera-Osorio et al. (2011).

5.3 Long Run Data Sources

We employ the second wave of the administrative census for low-income households (i.e., SISBEN), which was gathered in 2004, to reconstruct families in the long term. SISBEN serves as a valuable resource for identifying households and household members. The SISBEN data is derived from a comprehensive census of low-income households conducted nationwide. It contains detailed socio-demographic information about members of the household.

The experimental data, combined with the SISBEN data, include individual national identification numbers of program beneficiaries (students) and their family members. These identification numbers enable us to link the sample, as well as their household members, to various long-term administrative data sources. As a result, we can track the students and their family members from the time of the experiment up to 11 years after the intervention.

To compute long run outcomes, we incorporate two additional data sets from the Colombian education system. Firstly, we merge the experimental records with test

[7] For further details regarding the collection of follow-up data, additional information can be found in the study by Barrera-Osorio, Bertrand, Linden, and Perez-Calle (2011)

scores obtained from the high school exit exam called "Saber 11" between 1996 and 2015. This exam evaluates all students who aim to obtain a high school diploma at the time of their graduation. It covers multiple subjects, and the results are utilized for college admission purposes. Taking the exam is a requirement for graduation, making it a very close proxy for high school completion. It is worth noting that over 90 percent of test takers successfully graduate from high school after taking this exam.

Secondly, we assess college attendance by leveraging the administrative records of students who enrolled in any tertiary education program. These records are obtained from the "SPADIES" dataset, which includes data from 1998 to 2016. According to Colombian law, all tertiary education institutions are obligated to report information about their students to the Ministry of Education. The dataset includes details about students' majors, enrollment status, graduation, and dropout rates.

By integrating these data sources, we gained comprehensive insights into the educational trajectories of the students, including their performance in high school and their participation in tertiary education programs.

5.4 Outcomes

The analysis of the effects of the program includes two different sets of outcomes, depending on the timing of measurement. First, we examine the short-term effects of the treatments on school enrollment, attendance, and working status. School enrollment is computed directly from the administrative records, whereas school attendance is calculated using the information on schools visits in the 68 selected schools, and working status is computed using data from the household follow-up surveys. These short-run measures are constructed following the methodology outlined by Barrera-Osorio, Bertrand, Linden, and Perez-Calle (2011).

Second, we investigate the long-term effects by merging the experimental sample with the data from the high school exit exam and college enrollment records, as conducted in Barrera-Osorio, Linden, and Saavedra (2019). These administrative data sets encompass the entire population of individuals who took the high school exit exam or enrolled in tertiary education institutions. Consequently, the absence of a person from the experimental sample in the merged data constitutes an outcome in itself. By linking the experimental sample with these administrative data sets, we are able to construct measures of high school graduation, college enrollment, and college graduation.

We use the household follow-up survey and the second wave of SISBEN to identify siblings. In the short run, we fully identify siblings and their outcomes using the follow-up surveys. These surveys were conducted at the household level, thereby observing the entire household composition of the selected households. In the long run, however, we rely in the SISBEN census of data to identify household members and to increase statistical power. This procedure is not perfect as we can only identify school age children residing within the household, even though they are not necessarily siblings. However, we condition on kids having the same last name to ensure that we are comparing children who either share a mother of a father and reside in the same household.

This comprehensive approach allows us to assess both the short-term and long-term effects of the program on various educational outcomes of treated children and their siblings, such as school attendance, enrollment, high school graduation, and college enrollment and graduation.

5.5 Description of Samples

Our analysis combines four different samples that vary depending on the time of measurement of the outcome and the type of estimated effect:

1. *Direct Effects in the Short Run:* This sample constitutes the entire 17,225 students registered in the experiment.

2. *Indirect Effects in the Short Run:* This sample includes untreated students residing in households with two registered students. It is considerably smaller because it focuses on students who were part of the households included in the follow-up survey.

3. *Direct Effects in the Long Run:* This is the same sample displayed in (1) and corresponds to the universe of registered students.

4. *Indirect Effects in the Long Run:* This sample includes untreated students residing in households with two registered students. In this case we include that entirety of households in the experiment and the families were identified using SISBEN.

Table 1 presents the sample sizes of our universe of analysis, by treatment status and time of measurement of the outcome. The direct effects samples include the totality of the universe of analysis. The sample to estimate the estimates in the long run of the indirect effects limits the sample to untreated students in households with two registered children. The sample to estimate the short run, indirect effects, however, is smaller because it only includes the sub-group of students included in the follow up survey.

Table 1: Sample Sizes in the Short and Long Run

Sample:	Short Run						Long Run					
	Direct Effects			*Indirect Effects*			*Direct Effects*			*Indirect Effects*		
Grades:	6-8	9-11	Total	6-8	9-11	Total	6-8	9-11	Total	6-8	9-11	Total
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A) San Cristóbal												
Control	2,439	1,617	4,056	114	82	196	2,439	1,617	4,056	311	223	534
Basic	4,121	2,730	6,851	253	161	414	4,121	2,730	6,851	548	394	942
College	-	-	-	-	-	-	-	-	-	-	-	-
Total	6,560	4,347	10,907	367	243	610	6,560	4,347	10,907	859	617	1,476
B) Suba												
Control	2,084	1,393	3,477	158	108	266	2,084	1,393	3,477	409	305	714
Basic	1,708	-	1,708	79	56	135	1,708	-	1,708	160	136	296
College	-	1,133	1,133	56	39	95	-	1,133	1,133	147	95	242
Total	3,792	2,526	6,318	293	203	496	3,792	2,526	6,318	716	536	1,252
C) Total												
Control	4,523	3,010	7,533	272	190	462	4,523	3,010	7,533	720	528	1,248
Basic	5,829	2,730	8,559	332	217	549	5,829	2,730	8,559	708	530	1,238
College	-	1,133	1,133	56	39	95	-	1,133	1,133	147	95	242
Total	10,352	6,873	17,225	660	446	1,106	10,352	6,873	17,225	1,575	1,153	2,728

Note: This table displays sample sizes by site and grades in the short and long run. The estimating sample of the direct effects use the entire universe of students in the experiment. The indirect effects sample include untreated individuals in households of two registered children. The indirect effects sample in the short run correspond to households that were surveyed in the follow-up survey (students registered in the 68 schools with the highest number of participants).

6 Reduced Form Results

6.1 Validity

We perform a balance test on multiple pre-treatment outcomes measured in the baseline data of SISBEN. Figure 1 plots the p-values across the multiple specifications and outcomes. As expected, due to the randomized assignment, we do not observe consistent p-values below the pre-established critical values of 0.1 of 0.05. These tests provide strong validity to the randomization and the causal interpretation of our results.

6.2 Treatment Effects of the Basic Arm

We estimate equation 1 to compute the short run effects of the basic treatment, and present these results in Panel A of Table 2. We find that the basic treatment increased the likelihood of attending more than 80 percent of the classes in 0.04 percentage points and the probability of school enrollment in 0.03 percentage points. The positive effects, however, dissipate in the long run. We do not observe any precise point estimate among the long run outcomes, and the magnitudes are very close to zero. These point estimates validate the results in Barrera-Osorio, Bertrand, Linden, and Perez-Calle (2011), for the short run, and Barrera-Osorio, Linden, and Saavedra (2019) for the long run.

Despite the positive direct effects in the short run, the basic treatment had negative short and long run spillover effects on the untreated siblings of treated students. We obtain these results by estimating equation 3 and present them in Panel B of Table 2. The untreated siblings of treated students decreased the likelihood of attending 80 percent of classes in 0.04 percentage points and of self-reporting as enrolled in 0.05 percentage points.

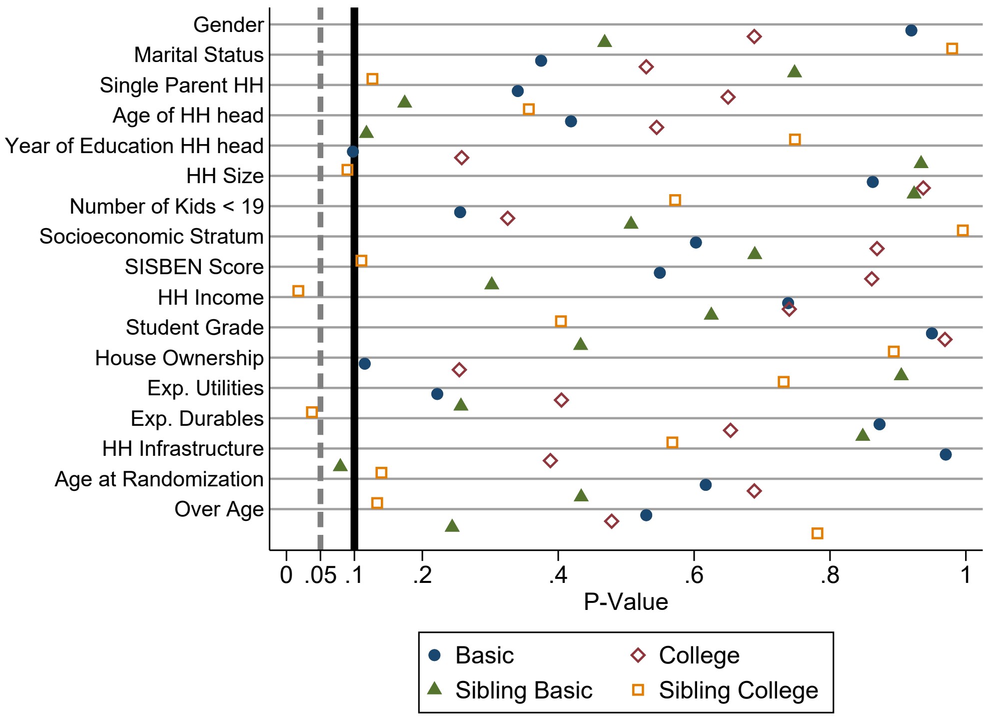
These spillover effects had remarkable long run repercussions in outcomes measured approximately a decade later. Even though we estimate an imprecise negative point estimate on high school graduation, we observe a precise decrease in the probability of college enrollment of 0.05 percentage points and of college graduation in 0.03 percentage points. These are two sizable effects that imply a 12 and 30 percent decrease in college enrolment and graduation rates, respectively.

6.3 Treatment Effects of the College Arm

The college treatment also increased attendance and enrollment among treated children. We estimate equation 2 and present the results in Panel A of Table 3. We observe a precise increase in school attendance and enrollment, and a sharp decrease in the probability of working while in school age. These positive effects translate into a long run increase in college enrolment of 4.5 percentage points that validate what found in Barrera-Osorio et al. (2011) and Barrera-Osorio et al. (2019).

We do not find any sizable spillover effect of the college treatment. We estimate equation 4 and present the results in panel B of Table 3. Even though the magni-

Figure 1: Balance Across Baseline Covariates



Notes: The figures plots p-values of the point estimates across multiple specifications using as outcomes the variables displayed in the y-axis. The direct effect of the basic treatment is estimated using equation 1. The direct effects of the college treatment is estimated using equation 2. The indirect effect of the college treatment is estimated using equation 3. The indirect effects of the college treatment is estimated using equation 4. "HH" stands for households for the names displayed in the y-axis.

Table 2: Effects of Basic Treatment on Short and Long Run Outcomes (Estimation Point Estimates)

	Short Run			Long Run		
	Attends 80%	School Enrollment	Works as Primary Activity	Graduation High-School	College Enrollment	Graduation College
	(1)	(2)	(3)	(4)	(5)	(6)
A) Direct Effects						
Basic	0.039***	0.031***	0.001	0.009	-0.006	0.002
	(0.010)	(0.008)	(0.003)	[0.007]	[0.008]	[0.004]
Observations	9,157	13,393	8,134	16092	16092	16084
Control Mean	0.737	0.734	0.0244	0.761	0.361	0.0858
B) Indirect Effects						
Sibling Basic	-0.043*	-0.054*	0.003	-0.027	-0.047**	-0.031***
	(0.025)	(0.028)	(0.012)	[0.019]	[0.020]	[0.011]
Observations	1,011	865	1,011	2486	2486	2485
Control Mean	0.831	0.846	0.0346	0.744	0.384	0.105

Note: This table presents the results of the estimation of equation 2 on panel A and equation 4 on panel B. The estimations in Panel B include untreated students living in households with two siblings. The indirect effects in the short term are estimated using the sample of students in follow-up surveys. The specification in Panel A controls for a dummy that takes the value of one if the observation is in San Cristóbal, a dummy that takes the valus of one if the individual is in grades 6th-18, and the interaction between these two. The specification in panel B controls for a dummy that takes the value of one if the observation is in San Cristóbal. Panel B is estimated is the subsample of students in the follow-up survey. The outcome in column (1) is a binary variable that takes the value of one if the student attends to 80% of the classes. The outcome in column (2) corresponds to a binary variable that takes the value of one if the student self-reported as enrolled to school in the follow-up surey. The outcome in column (3) corresponds to a binary variable that takes the value of one if the student's main activity is to work, and is collected in the follow-up survey. The outcome in column (4) corresponds to a binary variable that takes the value of one if the student is found in the high shchool exit exam. The outcome in column (5) corresponds to a binary variable that takes the value of one if the student is found in the college records. The outcome in column (6) corresponds to a binary variable that takes the value of one if the student graduates from college. Standard errors correspond to white heteroscedastic-consistent estimates. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Effects of College Treatment on Short and Long Run Outcomes (Validation Point Estimates)

	Short Run			Long Run		
	Attends 80%	School Enrollment	Works as Primary Activity	Graduation High-School	College Enrollment	Graduation College
	(1)	(2)	(3)	(4)	(5)	(6)
A) Direct Effects						
College	0.064***	0.042**	-0.041***	0.011	0.044**	0.016
	(0.025)	(0.021)	(0.013)	[0.013]	[0.020]	[0.015]
Observations	4,694	6,980	4,128	8666	8665	8658
Control Mean	0.737	0.706	0.0244	0.761	0.361	0.0858
B) Indirect Effects						
Sibling College	-0.053	0.043	-0.010	-0.006	0.044	-0.002
	(0.052)	(0.043)	(0.022)	[0.031]	[0.037]	[0.024]
Observations	557	480	557	1490	1490	1490
Control Mean	0.831	0.846	0.0346	0.744	0.384	0.105

Note: This table presents the results of the estimation of equation 2 on panel A and equation 4 on panel B. The estimations in Panel B include untreated students living in households with two siblings. The indirect effects in the short term are estimated using the sample of students in follow-up surveys. The specification in Panel A controls for a dummy that takes the value of one if the observation is in San Cristóbal, a dummy that takes the valus of one if the individual is in grades 6th-18, and the interaction between these two. The specification in panel B controls for a dummy that takes the value of one if the observation is in San Cristóbal. Panel B is estimated is the subsample of students in the follow-up survey. The outcome in column (1) is a binary variable that takes the value of one if the student attends to 80% of the classes. The outcome in column (2) corresponds to a binary variable that takes the value of one if the student self-reported as enrolled to school in the follow-up surey. The outcome in column (3) corresponds to a binary variable that takes the value of one if the student's main activity is to work, and is collected in the follow-up survey. The outcome in column (4) corresponds to a binary variable that takes the value of one if the student is found in the high shchool exit exam. The outcome in column (5) corresponds to a binary variable that takes the value of one if the student is found in the college records. The outcome in column (6) corresponds to a binary variable that takes the value of one if the student graduates from college. Standard errors correspond to white heteroscedastic-consistent estimates. *** p<0.01, ** p<0.05, * p<0.1.

tudes of the point estimates are sizable, they are imprecise and display opposite signs. Nonetheless, the lack of precision can be related with the small number of observations in the estimations.

7 Model

We develop a a dynamic model of household decision-making that can explain the direct and indirect effects observed under the Basic conditional cash transfer treatment. We first set up some notation and discuss the choices available to the household. We then discuss households' preferences, technology and optimal decisions. The model characterizes the behavior of low-income households that have at least one child in grades 6 to 11 in the year 2005, when randomization occurs. Some of the households have more than one child that is eligible for the program.

7.1 Notation and Environment

In $t = 2005$ households differ in the number of children they have and their age structure. We let $N_{h,t}$ denote the number of children household h has in period t and we index children in each household by their birth order $i = 1, 2, ..., N_h$. Each child is endowed with ability $\xi_{i,h}$ which can be low ($\xi = 0$) or high ($\xi = 1$) with the vector $\xi_h = \{\xi_{i,h}\}_{i=1}^{N_h}$ collecting the ability endowments of all siblings. We denote by $a_{i,h,t}$ the age of child i, in household h as of the beginning of year t. Every child completes grade 5 but may drop from school in subsequent grades. $e_{i,h,t}$ captures the completed level of schooling for child i, in household h as of the beginning of year t. We let $\mathcal{K}_{h,t}$ denote the set of children who are eligible for the CCT program in the sense that that they've completed grade 5 and haven't yet completed grade 11: $5 \leq e_{i,h,t} < 11$.

Early in $t = 2005$ parents in household h decide which, if any, of their eligible children $i = 1, 2, ...N_h$ to sign up for the CCT lottery. We let indicator $\ell_{i,h}$ to be equal to one when child i in household h signed up for the CCT lottery, zero otherwise. Randomization occurs among those children who sign up and individual level randomization induces household-level randomization. For example, a household with two children that sign up for the lottery may end up with any of four possible treatments: a) both of them assigned to the control group, b) the first-born treated and the second one control, c) the first-born control and the second born as treated or d) both siblings treated.

Choices. Household h makes a sequence of decisions $d_{h,t} = \{d_{i,h,t}\}$ for each child i from ages $a = 11$ to 25 to maximize the presented discounted value of expected lifetime utility. In particular, the household choices are captured by the following three 0-1 indicators

- $s_{i,h,t}$ whether child i goes to school in year t
- $q_{i,h,t}$ whether child i attends school at least 80% of the days in year t
- w_a whether child i works in year t

Therefore, for each child, the household can decide whether he or she attends school, works or stays at home, and also the level of absenteeism when he or she does attend school. Attending school (with either high or low absenteeism), staying at home and working are mutually exclusive choices.

State Variables. $t(a)$ indexes the calendar year the child is of age a. It affects the availability of UCT/CCT transfers. $y^p_{h,t}$ denotes the (parental) household income, not including the money that the children can get by working. $y^c_{i,h,t}$ is the income child i gets by working in year t and $X_{h,t}$ is a vector of time-varying and time-invariant child- and household-level observable characteristics. ε^u_t is a vector capturing child- and household-level unobserved heterogeneity in preferences for school attendance as well as disutility from work/taste for leisure. ε^u_a is observed by the household before making decisions but not by the econometrician. The vector $\Omega_t = \{t(a), \{a_{i,h,t}\}, \{e_{i,h,t}\}, X_a\}$ collects all the observed (to both household and econometrician) state variables at time t. To simplify the exposition of the model, we let $\varepsilon^u_t = \{\varepsilon^w_t, \varepsilon^s_t, \varepsilon^c_t\}$ collect the ε that affect preferences and let $\varepsilon_t = \{\varepsilon^u_t, \varepsilon^{y^p}_t, \varepsilon^{y^c}_t\}$ collect all the ε, including in addition those that represent shocks to parental income or child income. We let $\chi_t = \{\Omega_t, \varepsilon_t, \xi\}$ collect all state variables relevant for household decision-making, regardless of whether they are observed by econometrician or not. In addition to keep track of the random assignment in the RCT we let Z denote the randomly assigned group:

- `Control`
- `Basic`: Basic Treatment ($300,000 transfer per year)
- `College`: College Treatment ($200,000 per year + $600,000 upon enrollment in college)

We denote by τ^Z the transfer that the household receives.

Decision Horizon. Let $\underline{t}_h$=$t(a_{1,h} = 11)$ be the first calendar year in which the household starts making decisions in our model. This is the year in which their first-born child turns 11. Similarly, let $\bar{t}_h = t(a_{N,h} = 25)$ be the last calendar year in which the household makes decisions in our model. This is the year in which their last-born child turns 25. The time horizon is therefore different for each household depending on the number of children they have and their age spacing. To capture these different horizons, we denote by $T_h = \bar{t}_h - \underline{t}_h + 1$ the number of decision periods the household has ahead of itself when it starts making decisions in the model. In the last decision-making period, when the youngest child reaches $a = 25$, the household collects a terminal value function $V_{\bar{t}}(\Omega_{\bar{t}})$ which depends on the final school levels of all their children. The model applies to households with children born in years $t \leq 1995$ who have at least one child between the ages of $a = 11$ and $a = 22$ in $t = 2005$ when recruitment for RCT takes place. The time unit of the dynamic model is one year.

7.2 Household Preferences

Household level preferences are captured by a period utility function $U(\cdot)$ that depends on:

- $c_{h,t}$: household consumption
- $w_{i,h,t}$ age-varying disutility from the child working
- $s_{i,h,t}$ school attendance of each child

- $s_{i,h,t-1}$ school attendance decisions in the previous period $t-1$ to capture possible school re-entry utility costs.

- $q_{i,h,t}$ the level of absenteeism/presentism in the school attendance of child i.

Household utility in each period is also affected by preference shocks ε^U. Households also value the final completed levels of education according to a terminal value function that, crucially depends on $e_{i,h,\bar{t}_h}$, the final completed education levels of each child and possibly how unequal these are. This terminal value is denoted by $V_{\bar{t}_h}(e_{1,h}, e_{2,h}, ..., e_{N_h,h}; \rho)$. The household takes this value in the year $\bar{t}_h$ in which their youngest child (last-born) reaches age 25. The key structural parameter ρ, introduced in Behrman et al. (1982) enters this terminal value function and controls the degree of parental aversion to inequality in completed education among offspring.

7.3 Household Technology

Household h can produce accumulated levels of schooling $e_{i,h}$ for each of their children i by sending them to school and avoiding them having to work or idling at home. Sending them to school is just a necessary condition. Actual accumulation of completed levels of education will depend on the extent of their child's absenteeism from school. It will also depend on their child's innate ability ξ_i. We model the accumulation of completed levels of education through a grade progression probability π. This probability for is a function of their age a, the number of completed years of education e, their history of absenteeism $\{$, their innate ability endowment ξ and other household characteristics X:

$$\Pr\left(e_{i,h,t+1} = e_{i,h,t} + 1 \mid s_{i,h,t} = 1\right) = \pi_e\left(a_{iht}, e_{iht}, \{q_{iht}\}, \xi_{ih}, X_{ht}\right)$$

7.4 Optimal Household Decisions

The problem the household solves is different before or after they are recruited into the experiment. Before 2005, the household solves a "business-as-usual" problem. In 2005 they are surprised to learn that a CCT lottery will take place and their children are randomized to either the control group or one of the treatment groups. Households in which at least one child won the lottery make these subsequent choices from 2005 onward with an updated budget constraint for

To economize on notation let

- $s_{h,t} = \{s_{iht}\}_i$ collect all school attendance decisions in household h at time t

- $q_{h,t} = (\{q_{iht}\}_i)$ collect household absenteeism decisions for all siblings

- $w_{h,t} = \{w_{iht}\}_i$ collect all child work decisions in household h at time t

Further we omit h subscript at times to simplify notation. Each household h then solves:

$$\max_{\{\{s_{it}, q_{it}, w_{it}\}_{t=\underline{t}}^{t=\bar{t}_h}\}_{i=1}^{i=N_h}} \left\{ E\left[\sum_{t=\underline{t}_h}^{t=\bar{t}_h} \delta^{t-\underline{t}_h} U\left(c_t, s_t, s_{(t-1)}, q_t, w_t; \Omega_t, \varepsilon_t^u\right) + \delta^{T_h} V_{\bar{t}_h}(\Omega_{\bar{t}_h}) \middle| \chi_{\underline{t}_h}\right]\right\} \tag{5}$$

subject to

$$
\begin{aligned}
c_{ht} &= y_t^p + \sum_{i=1}^{i=N_h} \{ w_{it} \times y_{it}^c + s_{it} \times \mathbb{1}\{Z_i \neq \text{Control}\} \times \tau_i(Z_i, e_{i,t}) \} \\
\Pr\left(e_{i,t+1} = e_{i,t} + 1 \mid s_{i,t} = 1\right) &= \pi_e\left(a_{it}, w_{it}, \{q_{it}\}, e_{it}, X_t, \xi_i\right) \\
y_t^p &= y^p(X_t, \varepsilon_t^{y^p}) \\
y_{i,t}^c &= y^c(a_t, e_t, X_t, \varepsilon_{it}^{y^c}) \\
&\text{given } \Pr(X_{t+1} | X_t, d_t)
\end{aligned}
\tag{6}
$$

Notice that $\tau_i(Z_i, e_{i,t})$ is generic enough to capture the two types of CCT incentives given in the "Basic" and " College" treatments:

$$
\tau_i(Z_i, e_{i,t}) = \begin{cases} 300,000 & \text{if } Z_i = \text{Basic and } e_{i,t} < 12 \\ 200,000 & \text{if } Z_i = \text{College and } e_{i,t} < 12 \\ 600,000 & \text{if } Z_i = \text{College and } e_{i,t} = 12 \end{cases}
$$

The sequence representation of the dynamic optimization problem described above can re-cast into its recursive representation using a finite horizon dynamic programming formulation. Appendix D provides computational details about the recursive representation and the algorithm used to obtain the model's solution. After solving the model we obtain the optimal school attendance, work and absenteeism decisions by the household, which are a function of the state variables $(\Omega_t, \varepsilon_t, \xi)$ The policy functions are given by

$$
(s_t^*, w_t^*, q_t^*) = d_t(\Omega_t, \varepsilon_t, \xi)
$$

8 Model Estimation

To estimate the model we follow the estimation strategy in Galiani, Pantano, and Shi (2022). Functional forms associated with model implementation will be detailed in Appendix B. The ability endowment of every child ξ_i is known by the household but unobserved by the econometrician.We specify a discrete distribution of unobserved types $k = 1, ..., K_N$ for each household size N. These types reflect the different configurations of unobserved child ability endowments ξ. For example in a two-children household there are 4 possible types: $(\xi_1, \xi_2) = (0,0), (0,1), (1,0), (1,1)$. p_k denotes the probability that a household is of type k. These probabilities are estimated along with the structural parameters. The joint distribution of the random vector ε is assumed to be multivariate normal $\varepsilon \sim N(0, \Sigma)$. Appendix C provide more details.

Parameters to be estimated are $\theta = \{\alpha, \lambda^e, \phi^{y^p}, \phi^{y^c}, \rho, \Gamma, p_k\}$ where α denotes parameters of the utility function, λ^e those in the grade progression probability function, ϕ^{y^p} and ϕ^{y^c} denote, respectively, parameters in the parental and income equations, ρ captures terminal value function parameters, Γ denotes the Cholesky Decomposition of Σ, the variance-covariance matrix of the ε and p_k denote the unobserved type probabilities for $k = 1, 2, 3, ..., K_N$ where $p_K = 1 - \sum_{k=1}^{K-1} p_k$ We proceed to estimation with a simulation-based estimation approach in the spirit of indirect inference and simulated minimum distance. Starting from an initial guess θ^{guess}. for each vector θ of parameters proposed in the estimation routine we *repeatedly*:

1. solve the model $M(\theta)$ by backwards recursion using parameters θ.

2. Use $M(\theta)$ to forward-simulate a population of households with different number of children and sibling endowed ability configurations and age spacing but consistent with typical observed distributions in the population.

3. Consider the subset of simulated households that had at least one child potentially eligible in 2005.

4. We compute simulated moments ($m(\theta)$) using the simulated data or subsets of it. (some of these moments include, for example, the direct treatment effects and the spillover effects on untreated siblings on various outcomes from the different treatment arms. To get these we run on the simulated data the same specifications that are run in the real empirical data)

5. We compare simulated moments ($m(\theta)$) to the analogous empirical moments from the data (m^{data}).

6. if distance small, stop and obtain $\hat{\theta}$, otherwise go to step 1 and update the guess for θ

$\hat{\theta}$ is the vector of parameters that minimizes the distance between the empirical (m^{data}) and model-simulated ($m(\theta)$) moments:

$$\hat{\theta} =_{\theta} \left\{ \left[m^{\text{data}} - m(\theta) \right]' W \left[m^{\text{data}} - m(\theta) \right] \right\} \tag{7}$$

where W is a weight matrix. Table 2 describes the key set of moments (m^{data}) to be matched in Estimation.[8]

9 Conclusions

We examine how resource allocation within households affects parental aversion to inequality in offspring. Theoretical work in the field highlights the fundamental role of household decisions in understanding economic behavior (Chiappori, 1988). However, empirical tests of these implications have been scarce due to the challenges in finding exogenous variation in resource allocation within the household.

In this paper, we analyze a unique setting that introduced random variation in resource allocation within households and across siblings. We study a randomized intervention conducted in 2005 in the city of Bogotá that tested alternative delivery methods for conditional cash transfers. The participating students were individually assigned to two different treatment arms, which varied the delivery methods of the transfers. One treatment followed a standard method whereas the second prioritized

[8] Appendix F will provide further detail on additional moments used for estimation

college enrollment. Importantly, the treatment assignment was conducted at the individual level, rather than the household level. As a result, this intervention introduced random variation in resource allocation across siblings living in the same household.

Exploiting this unique setting, we estimate both the direct treatment effects on the treated students and the spillover effects on their untreated siblings. Our findings reveal that the conditional cash transfers improve outcomes for the directly treated students. However, the standard delivery method of the transfers has negative spillover effects on the untreated siblings of the treated students. These results suggest that parents reinforce within-household inequality by allocating more resources to the offspring who randomly received the transfers.

To further understand the underlying mechanisms behind these treatment effects, we estimate a dynamic structural model. We utilize the point estimates obtained from the standard delivery method of the conditional cash transfers to estimate the model. Subsequently, we validate the results of the model by comparing the true empirical moments of the delivery method that prioritized college enrollment with the model's predictions.

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A Robustness of the Main Effects

A.1 Different Household Sizes

Table A.1: Effects of Basic Treatment in Households with Different Sizes

	All						No Control Students in Treated HHs					
	Attends 80%	Attends	Attends 80% conditional	School Enrollment	Works as Main Activity	Number of Meals	Attends 80%	Attends	Attends 80% conditional	School Enrollment	Works as Main Activity	Number of Meals
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
One Sibling												
Basic	0.021*	-0.001	0.022*	-0.018	0.004	0.210***	0.021*	-0.001	0.022*	-0.018	0.004	0.210***
	(0.012)	(0.005)	(0.011)	(0.013)	(0.004)	(0.066)	(0.012)	(0.005)	(0.011)	(0.013)	(0.004)	(0.066)
Observations	4,707	4,707	4,600	4,042	4,707	4,494	4,707	4,707	4,600	4,042	4,707	4,494
Control Mean	0.802	0.976	0.822	0.834	0.0218	8.080	0.802	0.976	0.822	0.834	0.0218	8.080
Two Siblings or less												
Basic	0.021**	0.004	0.018**	-0.017	0.001	0.194***	0.017	0.000	0.017*	-0.020*	0.002	0.201***
	(0.009)	(0.004)	(0.009)	(0.010)	(0.003)	(0.053)	(0.010)	(0.004)	(0.010)	(0.011)	(0.003)	(0.057)
Observations	7,239	7,239	7,079	6,276	7,239	6,920	6,595	6,595	6,456	5,723	6,595	6,311
Control Mean	0.807	0.975	0.828	0.832	0.0258	8.112	0.808	0.976	0.827	0.837	0.0241	8.105
Three Siblings or less												
Basic	0.019**	0.004	0.016*	-0.016	0.001	0.193***	0.017	0.002	0.015	-0.018*	0.002	0.189***
	(0.009)	(0.004)	(0.009)	(0.010)	(0.003)	(0.051)	(0.010)	(0.004)	(0.010)	(0.011)	(0.003)	(0.057)
Observations	7,691	7,691	7,520	6,678	7,691	7,349	6,899	6,899	6,750	6,004	6,899	6,602
Control Mean	0.808	0.974	0.829	0.832	0.0256	8.106	0.806	0.975	0.827	0.836	0.0241	8.105
Four Siblings or less												
Basic	0.020**	0.004	0.017*	-0.016	0.001	0.188***	0.017*	0.002	0.016	-0.018*	0.002	0.186***
	(0.009)	(0.004)	(0.009)	(0.010)	(0.003)	(0.051)	(0.010)	(0.004)	(0.010)	(0.011)	(0.003)	(0.057)
Observations	7,749	7,749	7,578	6,718	7,749	7,400	6,935	6,935	6,786	6,031	6,935	6,633
Control Mean	0.808	0.974	0.829	0.833	0.0257	8.108	0.807	0.975	0.827	0.836	0.0241	8.105
Five Siblings or less												
Basic	0.020**	0.004	0.017*	-0.017*	0.001	0.189***	0.017*	0.002	0.015	-0.019*	0.002	0.186***
	(0.009)	(0.004)	(0.009)	(0.010)	(0.003)	(0.051)	(0.010)	(0.004)	(0.010)	(0.011)	(0.003)	(0.057)
Observations	7,754	7,754	7,583	6,723	7,754	7,405	6,939	6,939	6,790	6,035	6,939	6,637
Control Mean	0.807	0.974	0.829	0.833	0.0257	8.108	0.807	0.975	0.827	0.836	0.0241	8.105

Note: *** p<0.01, ** p<0.05, * p<0.1

Table A.2: Effects of College Treatment in Households with Different Sizes

	All						No Control Students in Treated HHs					
	Attends 80%	Attends	Attends 80% conditional	School Enrollment	Works as Main Activity	Number of Meals	Attends 80%	Attends	Attends 80% conditional	School Enrollment	Works as Main Activity	Number of Meals
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
One Sibling												
College	0.050	-0.002	0.052	-0.035	-0.036**	0.289**	0.050	-0.002	0.052	-0.035	-0.036**	0.289**
	(0.034)	(0.012)	(0.033)	(0.035)	(0.017)	(0.147)	(0.034)	(0.012)	(0.033)	(0.035)	(0.017)	(0.147)
Observations	2,401	2,401	2,344	1,918	2,401	2,298	2,401	2,401	2,344	1,918	2,401	2,298
Control Mean	0.802	0.976	0.822	0.834	0.0218	8.080	0.802	0.976	0.822	0.834	0.0218	8.080
Two Siblings or less												
College	0.036	-0.009	0.045*	-0.002	-0.045***	0.173	0.042	-0.007	0.049*	-0.015	-0.041***	0.186
	(0.026)	(0.010)	(0.025)	(0.027)	(0.014)	(0.112)	(0.027)	(0.010)	(0.026)	(0.028)	(0.015)	(0.118)
Observations	3,675	3,675	3,580	3,008	3,675	3,516	3,031	3,031	2,957	2,455	3,031	2,907
Control Mean	0.807	0.975	0.828	0.832	0.0258	8.112	0.808	0.976	0.827	0.837	0.0241	8.105
Three Siblings or less												
College	0.028	-0.009	0.036	-0.007	-0.045***	0.161	0.037	-0.006	0.043	-0.019	-0.042***	0.188
	(0.025)	(0.010)	(0.024)	(0.026)	(0.014)	(0.108)	(0.027)	(0.010)	(0.026)	(0.028)	(0.015)	(0.115)
Observations	3,888	3,888	3,786	3,180	3,888	3,716	3,096	3,096	3,016	2,506	3,096	2,969
Control Mean	0.808	0.974	0.829	0.832	0.0256	8.106	0.806	0.975	0.827	0.836	0.0241	8.105
Four Siblings or less												
College	0.032	-0.009	0.040*	-0.006	-0.044***	0.155	0.036	-0.006	0.042	-0.019	-0.042***	0.187
	(0.025)	(0.010)	(0.024)	(0.026)	(0.014)	(0.107)	(0.027)	(0.010)	(0.026)	(0.027)	(0.015)	(0.114)
Observations	3,916	3,916	3,814	3,198	3,916	3,742	3,102	3,102	3,022	2,511	3,102	2,975
Control Mean	0.808	0.974	0.829	0.833	0.0257	8.108	0.807	0.975	0.827	0.836	0.0241	8.105
Five Siblings or less												
College	0.032	-0.009	0.040*	-0.006	-0.044***	0.155	0.036	-0.006	0.042	-0.019	-0.042***	0.187
	(0.025)	(0.010)	(0.024)	(0.026)	(0.014)	(0.107)	(0.027)	(0.010)	(0.026)	(0.027)	(0.015)	(0.114)
Observations	3,917	3,917	3,815	3,199	3,917	3,743	3,102	3,102	3,022	2,511	3,102	2,975
Control Mean	0.807	0.974	0.829	0.833	0.0257	8.108	0.807	0.975	0.827	0.836	0.0241	8.105

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A.3: Effects of College Treatment in Households with Different Sizes

	All			No Control Students in Treated HHs		
	Graduation High-School	College enrollment	Graduation College	Graduation High-School	College enrollment	College Graduation
	(1)	(2)	(3)	(4)	(5)	(6)
One Sibling						
College	0.018	0.043	0.001	0.018	0.043	0.001
	[0.018]	[0.028]	[0.021]	[0.018]	[0.028]	[0.021]
Observations	4469	4469	4466	4469	4469	4466
Control mean	0.767	0.386	0.0938	0.767	0.386	0.0938
Two Siblings or less						
College	0.016	0.046**	0.011	0.010	0.040*	–0.002
	[0.013]	[0.021]	[0.016]	[0.014]	[0.022]	[0.017]
Observations	7714	7713	7708	6185	6184	6180
Control mean	0.764	0.372	0.0900	0.770	0.383	0.0956
Three Siblings or less						
College	0.008	0.042**	0.015	0.002	0.032	–0.001
	[0.013]	[0.020]	[0.015]	[0.014]	[0.022]	[0.016]
Observations	8511	8510	8504	6410	6409	6405
Control mean	0.763	0.363	0.0866	0.771	0.382	0.0949
Four Siblings or less						
College	0.008	0.043**	0.016	0.001	0.031	0.000
	[0.013]	[0.020]	[0.015]	[0.014]	[0.021]	[0.016]
Observations	8649	8648	8641	6433	6432	6428
Control mean	0.762	0.361	0.0859	0.771	0.383	0.0948
Five Siblings or less						
College	0.011	0.044**	0.016	0.004	0.032	0.001
	[0.013]	[0.020]	[0.015]	[0.014]	[0.021]	[0.016]
Observations	8666	8665	8658	6438	6437	6433
Control mean	0.761	0.361	0.0858	0.771	0.382	0.0947

Note: *** p<0.01, ** p<0.05, * p<0.1

Table A.4: Effects of Basic Treatment in Households with Different Sizes

	All			No Control Students in Treated HHs		
	Graduation High-School	College enrollment	Graduation College	Graduation High-School	College enrollment	College Graduation
	(1)	(2)	(3)	(4)	(5)	(6)
One Sibling						
Basic	0.011	–0.008	0.003	0.011	–0.008	0.003
	[0.010]	[0.011]	[0.006]	[0.010]	[0.011]	[0.006]
Observations	8300	8300	8297	8300	8300	8297
Control mean	0.767	0.386	0.0938	0.767	0.386	0.0938
Two Siblings or less						
Basic	0.013	–0.009	0.001	0.008	–0.019**	–0.003
	[0.008]	[0.008]	[0.005]	[0.009]	[0.009]	[0.005]
Observations	14218	14218	14213	12689	12689	12685
Control mean	0.764	0.372	0.0900	0.770	0.383	0.0956
Three Siblings or less						
Basic	0.008	–0.006	0.002	0.000	–0.024***	–0.004
	[0.007]	[0.008]	[0.004]	[0.008]	[0.009]	[0.005]
Observations	15787	15787	15780	13686	13686	13681
Control mean	0.763	0.363	0.0866	0.771	0.382	0.0949
Four Siblings or less						
Basic	0.009	–0.005	0.002	0.000	–0.026***	–0.005
	[0.007]	[0.008]	[0.004]	[0.008]	[0.009]	[0.005]
Observations	16052	16052	16044	13836	13836	13831
Control mean	0.762	0.361	0.0859	0.771	0.383	0.0948
Five Siblings or less						
Basic	0.009	–0.006	0.002	0.000	–0.027***	–0.005
	[0.007]	[0.008]	[0.004]	[0.008]	[0.009]	[0.005]
Observations	16092	16092	16084	13864	13864	13859
Control mean	0.761	0.361	0.0858	0.771	0.382	0.0947

Note: *** p<0.01, ** p<0.05, * p<0.1

A.2 Other Outcomes

Table A.5: Effects of College Treatment on All Outcomes

	Short Run							Long Run		
	Attends 80%	School Enrollment	Works as Primary Activity	Hours Worked	Number of Meals	Chores as Primary Activity	Hours Worked	Graduation High-School	College Enrollment	Graduation College
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A) Direct Effects										
College	0.064***	0.042**	-0.041***	-1.978***	0.191*	-0.016***	-0.566	-0.007	-0.004	-0.005
	(0.025)	(0.021)	(0.013)	(0.535)	(0.107)	(0.005)	(0.564)	[0.017]	[0.019]	[0.011]
Observations	4,694	6,980	4,128	4,128	3,948	8,666	3,960	8676	8672	8271
Control Mean	0.737	0.706	0.0244	1.518	8.097	0.0109	10.19	0.752	0.365	0.0968
B) Indirect Effects										
Sibling College	-0.053	0.043	-0.010	0.348	-0.131	0.016	0.655	-0.063	-0.022	-0.036*
	(0.052)	(0.043)	(0.022)	(0.916)	(0.234)	(0.023)	(0.956)	(0.039)	(0.038)	(0.022)
Observations	557	480	557	557	536	557	556	1525	1524	1427
Control Mean	0.831	0.846	0.0346	1.387	8.217	0.0195	10.20	0.683	0.332	0.0944

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.6: Effects of Basic Treatment on All Outcomes

	Short Run							Long Run		
	Attends 80%	School Enrollment	Works as Primary Activity	Hours Worked	Number of Meals	Chores as Primary Activity	Hours Worked	Graduation High-School	College Enrollment	Graduation College
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A) Direct Effects										
Basic	0.039***	0.031***	0.001	-0.215	0.175***	0.004**	-0.261	-0.004	-0.011	-0.008
	(0.010)	(0.008)	(0.003)	(0.156)	(0.050)	(0.002)	(0.173)	[0.007]	[0.008]	[0.005]
Observations	9,157	13,393	8,134	8,134	7,769	16,092	8,061	16089	16084	15255
Control Mean	0.737	0.706	0.0244	1.518	8.097	0.0109	10.19	0.752	0.365	0.0968
B) Indirect Effects										
Sibling Basic	-0.043*	-0.054*	0.003	0.812	-0.006	-0.002	-0.709	-0.019	-0.013	0.001
	(0.025)	(0.028)	(0.012)	(0.554)	(0.128)	(0.008)	(0.461)	(0.019)	(0.019)	(0.012)
Observations	1,011	865	1,011	1,011	969	1,011	1,010	2524	2518	2354
Control Mean	0.831	0.846	0.0346	1.387	8.217	0.0195	10.20	0.683	0.332	0.0944

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.7: Effects of Basic Treatment (Estimation Point Estimates). Secondary Outcomes

	Probability of formal employment	Formal employment wage (logs)	Credit 18-25	Crime 18-25
	(1)	(2)	(3)	(4)
A) Direct Effects				
Basic Treatment	–0.009	–0.109	0.002	0.001
	[0.007]	[0.091]	[0.008]	[0.002]
Observations	16047	16045	16092	16092
Control mean	0.295	4.084	0.462	0.0130
B) Indirect Effects				
Sibling with Basic Treatment	–0.006	–0.118	0.037*	0.007
	[0.020]	[0.270]	[0.021]	[0.004]
Observations	2480	2479	2486	2486
Control mean	0.318	4.434	0.454	0.00721

Note: This table presents the results of the estimation of equation 1 on panel A and 3 on panel B. The specification in Panel A controls for a dummy that takes the value of one if the observation is in San Cristóbal, a dummy that takes the valus of one if the individual is in grades 6th-8th, and the interaction between these two. The specification in panel B controls for a dummy that takes the value of one if the observation is in San Cristóbal. SAMPLES? Standard errors correspond to white heteroscedastic-consistent estimates. *** p<0.01, ** p<0.05, * p<0.1.

Table A.8: Effects of College Treatment (Validation Point Estimates). Secondary Outcomes

	Probability of formal employment	Formal employment wage (logs)	Credit 18-25	Crime 18-25
	(1)	(2)	(3)	(4)
A) Direct Effects				
College Treatment	0.003	–0.018	–0.004	–0.003
	[0.020]	[0.271]	[0.020]	[0.004]
Observations	8641	8639	8666	8666
Control mean	0.295	4.084	0.462	0.0130
B) Indirect Effects				
Sibling with College Treatment	–0.014	–0.117	0.042	0.010
	[0.035]	[0.479]	[0.037]	[0.009]
Observations	1487	1487	1490	1490
Control mean	0.318	4.434	0.454	0.00721

Note: This table presents the results of the estimation of equation 1 on panel A and 3 on panel B. The specification in Panel A controls for a dummy that takes the value of one if the observation is in San Cristóbal, a dummy that takes the valus of one if the individual is in grades 6th-8th, and the interaction between these two. The specification in panel B controls for a dummy that takes the value of one if the observation is in San Cristóbal. SAMPLES? The outcome in column (3) corresponds to a binary variable that takes the value of one if the student's main activity is to work. Standard errors correspond to white heteroscedastic-consistent estimates. *** p<0.01, ** p<0.05, * p<0.1.

Table A.9: Effects of College and Basic Treatments in Households with Two Siblings

	Attends 80%	School Enrollment	Works as Primary Activity
	(1)	(2)	(3)
A) Estimation Point Estimates (Siblings SISBEN)			
Basic	0.033**	0.031**	-0.005
	(0.016)	(0.014)	(0.005)
Observations	3,401	4,985	3,061
Control Mean	0.749	0.710	0.0272
B) Validation Point Estimates (Siblings SISBEN)			
College	0.048	0.104***	-0.039**
	(0.037)	(0.031)	(0.019)
Observations	1,795	2,650	1,608
Control Mean	0.749	0.710	0.0272
C) Estimation Point Estimates (Siblings Follow-up)			
Basic	0.021	-0.015	-0.006
	(0.016)	(0.017)	(0.006)
Observations	2,532	2,234	2,532
Control Mean	0.816	0.828	0.0335
D) Validation Point Estimates (Siblings Follow-up)			
College	0.019	0.045	-0.058**
	(0.038)	(0.043)	(0.025)
Observations	1,274	1,090	1,274
Control Mean	0.816	0.828	0.0335

Note: *** p<0.01, ** p<0.05, * p<0.1

Table A.10: Indirect Effects of College and Basic Treatments in Households with Two Siblings

	Attends 80%	School Enrollment	Works as Primary Activity
	(1)	(2)	(3)
A) Estimation Point Estimates (Siblings identified in SISBEN)			
Sibling Basic	-0.031	-0.017	0.009
	(0.024)	(0.021)	(0.010)
Observations	1,428	2,127	1,265
Control Mean	0.756	0.719	0.0253
B) Validation Point Estimates (Siblings identified in SISBEN)			
Sibling College	0.018	0.003	-0.005
	(0.043)	(0.035)	(0.016)
Observations	851	1,277	764
Control Mean	0.756	0.719	0.0253
C) Estimation Point Estimates (Siblings identified in Follow-up)			
Sibling Basic	-0.043*	-0.054*	0.003
	(0.025)	(0.028)	(0.012)
Observations	1,011	865	1,011
Control Mean	0.831	0.846	0.0346
D) Validation Point Estimates (Siblings identified in Follow-up)			
Sibling College	-0.053	0.043	-0.010
	(0.052)	(0.043)	(0.022)
Observations	557	480	557
Control Mean	0.831	0.846	0.0346

Note: *** p<0.01, ** p<0.05, * p<0.1

Table A.11: Effects of College and Basic Treatments in Households with Two Siblings. Long Run.

	Graduation High-School	College Enrollment	Graduation College
	(1)	(2)	(3)
Basic Treatment	0.016	–0.009	–0.003
	[0.012]	[0.013]	[0.007]
Observations	5956	5956	5954
Control mean	0.760	0.354	0.0858
College Treatment	0.012	0.052	0.020
	[0.020]	[0.032]	[0.024]
Observations	3267	3266	3264
Control mean	0.760	0.354	0.0858

Note: This table presents the results of the estimation of equation on panel A and equation on panel B using only households with two registered siblings. Both specifications control for a dummy that takes the value of one if the observation is in San Cristóbal, a dummy that takes the values of one if the individual is in grades 6th-8th, and the interaction between these two. Standard errors correspond to white heteroscedastic-consistent estimates. *** p<0.01, ** p<0.05, * p<0.1.

Table A.12: Heterogeneous Effects by oldest

	Short Run			Long Run		
	Attends 80%	School Enrollment	Works as Primary Activity	Graduation High-School	College Enrollment	Graduation College
	(1)	(2)	(3)	(4)	(5)	(6)
A) Basic Treatment						
Direct Effects						
Basic	0.018	0.031**	-0.003	-0.003	-0.031**	-0.015*
	(0.017)	(0.014)	(0.004)	[0.013]	[0.014]	[0.009]
Oldest*Basic	0.030	-0.000	0.005	0.000	0.029*	0.010
	(0.020)	(0.017)	(0.005)	[0.015]	[0.017]	[0.011]
Observations	9,157	13,393	8,134	16089	16084	15255
Control Mean	0.737	0.706	0.0244	0.752	0.365	0.0968
Indirect Effects						
Sibling Basic	-0.003	-0.054	0.011	-0.041	0.015	0.006
	(0.034)	(0.037)	(0.010)	[0.027]	[0.027]	[0.019]
Oldest*Sibling Basic	-0.079*	0.000	-0.017	0.044	-0.054	-0.009
	(0.048)	(0.052)	(0.023)	[0.037]	[0.037]	[0.025]
Observations	1,011	865	1,011	2524	2518	2354
Control Mean	0.831	0.846	0.0346	0.683	0.332	0.0944
B) College Treatment						
Direct Effects						
College	0.027	0.034	-0.048**	-0.001	0.020	-0.020
	(0.056)	(0.042)	(0.020)	[0.036]	[0.041]	[0.023]
Oldest*College	0.044	0.012	0.007	-0.006	-0.027	0.018
	(0.057)	(0.043)	(0.019)	[0.038]	[0.042]	[0.024]
Observations	4,694	6,980	4,128	8676	8672	8271
Control Mean	0.737	0.706	0.0244	0.752	0.365	0.0968
Indirect Effects						
Sibling College	-0.107*	0.021	-0.002	-0.077*	-0.014	-0.054**
	(0.063)	(0.051)	(0.016)	[0.046]	[0.045]	[0.025]
Oldest*Sibling College	0.261***	0.137**	0.025	0.021	0.007	0.048
	(0.079)	(0.056)	(0.065)	[0.080]	[0.079]	[0.045]
Observations	557	480	557	1525	1524	1427
Control Mean	0.831	0.846	0.0346	0.683	0.332	0.0944

Note: *** p<0.01, ** p<0.05, * p<0.1.

Table A.13: Heterogeneous Effects by female

	Short Run			Long Run		
	Attends 80%	School Enrollment	Works as Primary Activity	Graduation High-School	College Enrollment	Graduation College
	(1)	(2)	(3)	(4)	(5)	(6)
A) Basic Treatment						
Direct Effects						
Basic	0.057***	0.044***	0.001	-0.007	-0.009	-0.004
	(0.013)	(0.011)	(0.005)	[0.010]	[0.011]	[0.007]
Female*Basic	-0.036**	-0.026*	-0.000	0.006	-0.002	-0.008
	(0.018)	(0.016)	(0.006)	[0.014]	[0.015]	[0.009]
Observations	9,157	13,393	8,134	16089	16084	15255
Control Mean	0.737	0.706	0.0244	0.752	0.365	0.0968
Indirect Effects						
Sibling Basic	-0.022	-0.022	-0.004	-0.017	-0.030	-0.021
	(0.036)	(0.040)	(0.018)	[0.027]	[0.027]	[0.017]
Female*Sibling Basic	-0.040	-0.061	0.014	-0.004	0.036	0.044*
	(0.048)	(0.052)	(0.023)	[0.037]	[0.037]	[0.024]
Observations	1,011	865	1,011	2524	2518	2354
Control Mean	0.831	0.846	0.0346	0.683	0.332	0.0944
B) College Treatment						
Direct Effects						
College	0.079**	0.055**	-0.041**	0.010	0.001	-0.007
	(0.033)	(0.027)	(0.016)	[0.023]	[0.025]	[0.015]
Female*College	-0.027	-0.025	0.001	-0.033	-0.008	0.002
	(0.038)	(0.032)	(0.016)	[0.028]	[0.030]	[0.018]
Observations	4,694	6,980	4,128	8676	8672	8271
Control Mean	0.737	0.706	0.0244	0.752	0.365	0.0968
Indirect Effects						
Sibling College	0.022	0.064	-0.002	-0.007	-0.083*	-0.072***
	(0.072)	(0.063)	(0.037)	[0.052]	[0.050]	[0.024]
Female*Sibling College	-0.134	-0.038	-0.014	-0.115	0.128*	0.074*
	(0.096)	(0.080)	(0.042)	[0.074]	[0.072]	[0.040]
Observations	557	480	557	1525	1524	1427
Control Mean	0.831	0.846	0.0346	0.683	0.332	0.0944

Note: *** p<0.01, ** p<0.05, * p<0.1.

Table A.14: Effects of College Treatment (Validation Point Estimates). Long Run. Het. Gender

	Graduation High-School	College enrollment	Graduation College
	(1)	(2)	(3)
Estimation Point Estimates			
Basic Treatment	0.008	–0.009	–0.005
	[0.010]	[0.011]	[0.006]
Basic x Girl	0.003	0.007	0.015*
	[0.013]	[0.015]	[0.008]
Observations	16092	16092	16084
Control mean	0.761	0.361	0.0858
Validation Point Estimates			
Sibling Basic	–0.026	–0.042	–0.026*
	[0.027]	[0.027]	[0.016]
Sibling Basic x Girl	0.000	–0.008	–0.010
	[0.036]	[0.038]	[0.022]
Observations	2486	2486	2485
Control mean	0.744	0.384	0.105

Note: This table presents the results of the estimation. Standard errors correspond to white heteroscedastic-consistent estimates. *** p<0.01, ** p<0.05, * p<0.1.

Table A.15: Effects of College Treatment (Validation Point Estimates). Long Run

	Graduation High-School	College enrollment	Graduation College
	(1)	(2)	(3)
Estimation Point Estimates			
College Treatment	0.035**	0.042	0.006
	[0.018]	[0.027]	[0.019]
College x Girl	–0.044**	0.003	0.019
	[0.021]	[0.032]	[0.023]
Observations	8666	8665	8658
Control mean	0.761	0.361	0.0858
Validation Point Estimates			
Sibling College	0.005	0.017	–0.025
	[0.044]	[0.050]	[0.030]
Sibling College x Girl	–0.020	0.056	0.046
	[0.059]	[0.069]	[0.045]
Observations	1490	1490	1490
Control mean	0.744	0.384	0.105

Note: This table presents the results of the estimation. Standard errors correspond to white heteroscedastic-consistent estimates. *** p<0.01, ** p<0.05, * p<0.1.

B Functional Forms

1. Utility Function Household level preferences are captured by the period utility function $U(\cdot)$ that depends on:
    - $c_{h,t}$:household consumption
    - $w_{i,h,t}$ disutility from school-age child working
    - $s_{i,h,t}$ school attendance of each child ($t-1$ attendance decisions become state variables at t to capture possible school re-entry utility costs.)
    - $q_{i,h,t}$ presentism/absenteeism of child i in household h at time t

2. Terminal Value
    - We specify the terminal value as function of the mean and variance of Completed Schooling as Rosenzweig and Wolpin (1988)[9]

    $$\rho_1 \bar{e}_{\bar{t}_h} + \rho_2 \bar{e}^2_{\bar{t}_h} + \rho_3 \text{Var}(e_{\bar{t}_h}) \tag{8}$$

    where $\bar{e}$ is the average completed education level of all siblings and Var(e) is the variance of their completed education levels.

3. Grade Progression Probability This probability is specified as a simple logit model of where the probability of progression ($e_{t+1} = e_t + 1$ is a function of age a, current level of completed education e, history of absenteeism $\{\}$ and unobserved ability.

4. Income equations

$$\log(y^p_{h,t}) = y^p(X_t, \varepsilon^{y^p}_{h,t}) = \phi^p_0 + \varepsilon^{y^p}_{h,t}$$
$$\log(y^c_{i,h,t}) = y^c(a_t, e_t, X_t, \varepsilon^{y^c}_{it}) = \phi^c_0 + \varepsilon^{y^c}_{i,h,t}$$

[9] Al alternative would be to use a CES as in Behrman et al. (1982)

C Distributional Assumptions

$$\begin{pmatrix} \varepsilon^s \\ \varepsilon^q \\ \varepsilon^w \\ \varepsilon^{y^p} \\ \varepsilon^{y^{c_i}} \end{pmatrix} \sim N \left[\begin{pmatrix} 0 \\ 0 \\ 0 \\ 0 \\ 0 \\ 0 \end{pmatrix}, \begin{pmatrix} \sigma_s^2 & \sigma_{sq} & \sigma_{sw} & \sigma_{sp} & \sigma_{sc} \\ \sigma_{qs} & \sigma_q^2 & \sigma_{qw} & \sigma_{qp} & \sigma_{qc} \\ \sigma_{ws} & \sigma_{wq} & \sigma_w^2 & \sigma_{wp} & \sigma_{wc} \\ \sigma_{ps} & \sigma_{pq} & \sigma_{pw} & \sigma_p^2 & \sigma_{pc} \\ \sigma_{cs} & \sigma_{cq} & \sigma_{cw} & \sigma_{cp} & \sigma_c^2 \end{pmatrix} \right]$$

where $\sigma_{ij} = \rho_{ij} \times \sigma_i \times \sigma_j$

We will normalize some of these parameters. Following Keane and Wolpin (1994) we use $4 + N_h$ standard normal random variables $\eta \sim N(0, I_{4+N_h})$ and use the Cholesky decomposition Γ of Σ where $\Sigma = \Gamma\Gamma'$ to convert the standard normal random variables into a random vector with a joint multivariate normal distribution. We then have

$$\Sigma = \Gamma\eta$$

Note that Γ is lower triangular with generic element γ_{ij} in row i, column j so that we have

$$\begin{aligned} \varepsilon^s &= \gamma_{11}\eta_1 \\ \varepsilon^q &= \gamma_{21}\eta_1 + \gamma_{22}\eta_2 \\ \varepsilon^w &= \gamma_{31}\eta_1 + \gamma_{32}\eta_2 + \gamma_{33}\eta_3 \\ \varepsilon^p &= \gamma_{41}\eta_1 + \gamma_{42}\eta_2 + \gamma_{43}\eta_3 + \gamma_{44}\eta_4 \\ \varepsilon^c &= \gamma_{51}\eta_1 + \gamma_{52}\eta_2 + \gamma_{53}\eta_3 + \gamma_{54}\eta_4 + \gamma_{55}\eta_5 \end{aligned}$$

D Model Solution

We solve the model by backwards recursion, starting from the last year for the youngest child and working our way back to age 11 for the oldest child, the first period we model household decisions.

At $t = \bar{t}_h$ the alternative-specific value functions are given by

$$\begin{aligned} V_d(\Omega_t, \varepsilon_t, \xi) = U(c_t, d, \Omega_a, \xi, \varepsilon_t^u) \\ + V_{\bar{t}_h}(e_{1,h}, e_{2,h}, ..., e_{N_h,h}; \rho) \end{aligned} \tag{9}$$

At any time $t < \bar{t}_h$ during the decision-making time span for the household, the alternative-specific value functions are given by

$$V_d(\Omega_t, \varepsilon_t, \xi) = U(c_t, d, \Omega_t, \xi, \varepsilon_t^u) + \delta E_{\Omega_{t+1}} \left[V(\Omega_{t+1}, \xi) | d, \Omega_t, \xi \right] \tag{10}$$

where $\Omega_t = \{t, \{a_{i,h,t}\}, \{e_{i,h,t}\}, \{d_{i,h,t-1}\}, X_t\}$ The future component multiplying δ is given by

$$E_{\Omega_{t+1}}[V(\Omega_{t+1}, \xi)|d, \Omega_t, \xi] = \sum_{\Omega_{t+1}} \Pr(\Omega_{t+1}|d, \Omega_a, \xi) \times \{V(\Omega_{t+1}, \xi)\} \tag{11}$$

We use simulation-based integration to compute the EMAX terms:

$$V(\Omega_{t+1}, \xi) = E_\varepsilon \left[\max_j \{ V_j(\Omega_{t+1}, \varepsilon_{t+1}, \xi) \} \right] \tag{12}$$

using the following steps:

- We draw S draws from the (5+N)-variate standard normal vector $\eta_{t+1}^{(s)}$ and use the Cholesky decomposition to convert those into a vector of (5+N) correlated normal random draws $\varepsilon_{t+1}^{(s)}$.
- For each draw s, we compute all the alternative-specific value functions $V_j(\Omega_{t+1}, \varepsilon_{t+1}^{(s)}, \xi)$ for $j \in \mathcal{J}$ under that draw
- We take the maximum across the alternative-specific value functions

$$\max_j \{ V_j(\Omega_{t+1}, \varepsilon_{t+1}^{(s)}, \xi) \}$$

- We then average across the S draws

$$V(\Omega_{t+1}, \xi) = E_\varepsilon \left[\max_j \{ V_j(\Omega_{t+1}, \varepsilon_{t+1}, \xi) \} \right] \approx \frac{1}{S} \sum_{s=1}^{S} \left[\max_j \{ V_j(\Omega_{t+1}, \varepsilon_{t+1}^{(s)}, \xi) \} \right] \tag{13}$$

E Data Details

We merge 99.31 percent of the experimental sample with the SISBEN dataset, since the experimental households were required to have a SISBEN score in order to register. This merge provides us with first names, (two) last names, and exact date of birth for all members of the households. Importantly, it also provides us with a permanent unique national identification number (adult id, *cedula de identidad*) for all members that are 18 years or older. This guarantees that we have no attrition among older family members.

This number, however, is not provided for household members under the age of 18, but, instead, we have information on a temporary identification number (minor id, *tarjeta de identidad*). We create, therefore, a cross-walk of minor to adult ids using household location, names, date of births, and pre-existing crosswalks assembled by several Colombian public institution. This crosswalk allows us to recover the adult ids of program beneficiaries and their siblings.

We then use the adult ids (if available) or names and date of births to merge to the secondary education, tertiary education, social security, and credit access data.

F Moments for Estimation and Validation

- “Direct Effect” Design Moments.
- “Sibling Spillover” Design Moments.