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

Felipe Barrera-Osorio, Leonardo Bonilla-Mejía, Matias Busso, Sebastian Galiani, Hyunjae Kang, Juan Muñoz-Morales, Juan Pantano*

March 17, 2025

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.

Keywords: Spillover effects, conditional cash transfers, intra-household, Colombia.

*Barrera-Osorio: Vanderbilt University (felipe.barrera.-.osorio@vanderbilt.edu); Bonilla-Mejía: Banco de la República (lbonilme@banrep.gov.co); Busso: Inter-American Development Bank (mbusso@iadb.org); Galiani: University of Maryland (sgaliani@umd.edu); Kang: Kyoto University (kang.hyunjae.3j@kyoto-u.ac.jp); Muñoz-Morales: IESEG School of Management, Univ. Lille, CNRS, UMR 9221- LEM-Lille Économie Management, F-59000 Lille, France (j.munoz@ieseg.fr); Pantano: University of Arizona (jpanta@arizona.edu). Barrera-Osorio received financial assistance from the Seeding Success Grant Program (No. 004932), Vanderbilt University. We thank Julian Martínez-Correa and Antonella Rivera Bianchi for great research assistance. For helpful discussion and comments, we thank seminar participants at LACEA, AAEP, AASLE and University of Arizona. The opinions expressed in this document are those of the authors and do not necessarily reflect the views of the Inter-American Development Bank or Banco de la República, its Board of Directors, and the countries they represent. Errors are our own.

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. Related to our focus on sibling spillovers, Ferreira, Filmer, and Schady (Ferreira et al.) explore the effects of cash transfers received by scholarship-eligible children on their ineligible siblings in Cambodia.

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 win-

dow.

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.

Cite Guo et al. (2024) and Burlando (2023).

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]

[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.

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 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.

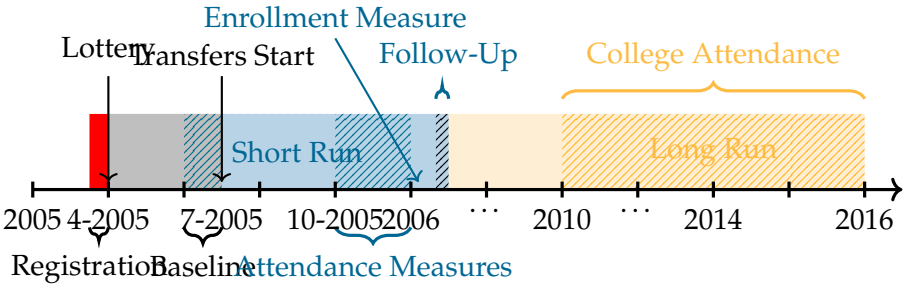
Importantly, the randomization was performed at the *student* level, which means that some children within the same household were assigned to the treatment group, while others were not. This randomization created variation in treatment status within households, allowing for a comparison between treated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

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

Figure 1: Timeline of the Intervention



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 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. The implementation of treatment assignment differed between the two localities. In Suba, students in 6th to 8th grades were randomized into the control

[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.

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 by focusing on households with two children in Suba to estimate four different types of effects from the basic and college treatments. Including households with more registered students can be worrisome as a larger number of registered individuals increases the likelihood of receiving treatment. Also, to increase statistical power, we pool both treatment arms (basic and college) into an aggregate estimation.

4.1 Total Effects

We estimate the total effects on treated individuals by comparing outcomes of students that won the lottery and those who lost, regardless of what happened to their siblings. 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).

Formally, we estimate:

$$Y_i = \beta_0^T + \beta^T \texttt{Basic/College}_i + \beta_X^T X_i + \varepsilon_i^T, \tag{1}$$

$Basic/College_i$ is a treatment indicator that equals 1 if the student was in either the Basic or College treatment arms, and 0 otherwise. 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 dummy that takes the value of one if the individual is in grades 6th-8th, as well as gender, age, sibling's gender, and sibling's age. We compute heteroskedasticity-consistent standard errors.[5]

We can decompose this total effect into the following three types.

4.2 Direct Effects

We compute direct treatment effects on outcomes of treated students with untreated siblings. To estimate these effects we compare treated students in households with untreated siblings with control individuals in households where no one was treated.

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

[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.

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 direct effects by comparing Y_1 with Z_1 and Z_2.

Formally, we estimate:

$$Y_i = \beta_0^D + \beta^D \texttt{Basic/College}_i + \beta_X^D X_i + \varepsilon_i^D, \tag{2}$$

where $\texttt{Basic/College}_i$ is a dummy that take the value of 1 if the student i was treated either with the basic or with the college treatment and their sibling did not, and zero if none were treated.

4.3 Indirect Effects

We compute indirect treatment effects on outcomes of siblings of treated students. To estimate these effects we compare control individuals in households with treated siblings with control individuals in households where no one was treated. Following our previous example, our strategy estimates the indirect effects by comparing Y_2 with Z_1 and Z_2.

Formally, we estimate:

$$Y_i = \beta_0^I + \beta^I \texttt{Sibling Basic/College}_i + \beta_X^I X_i + \varepsilon_i^I, \tag{3}$$

where $\texttt{Sibling Basic/College}_i$ is a dummy variable that equals 1 if the sibling of student i was treated with either the Basic or College treatment while student i was not, and 0 if neither was treated.

4.4 Win-win vs. Lose-lose Effects

Finally, we estimate the treatment effect on outcomes for treated students who also have treated siblings, i.e., households where both siblings won the lottery. To estimate these effects we compare treated students in households with treated siblings with control individuals in households where no one was treated. Following our previous example, our strategy estimates these effects by comparing X_1 and X_2 with Z_1 and Z_2.

Formally, we estimate:

$$Y_i = \beta_0^W + \beta^W \texttt{Basic/College}_i + \beta_X^W X_i + \varepsilon_i^W, \tag{4}$$

where $\texttt{Basic/College}_i$ is a dummy that take the value of 1 if the student i was treated either with the basic or with the college treatment and their sibling was also treated, and zero if none were treated.

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 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

Due to budget constraints, 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.

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].

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

[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)

Dropout among the two sub-samples measuring attendance and follow-up information was similar across the two 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 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 attendance and working status. 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, working status, high school graduation, and college enrollment and graduation.

6 Reduced Form Results

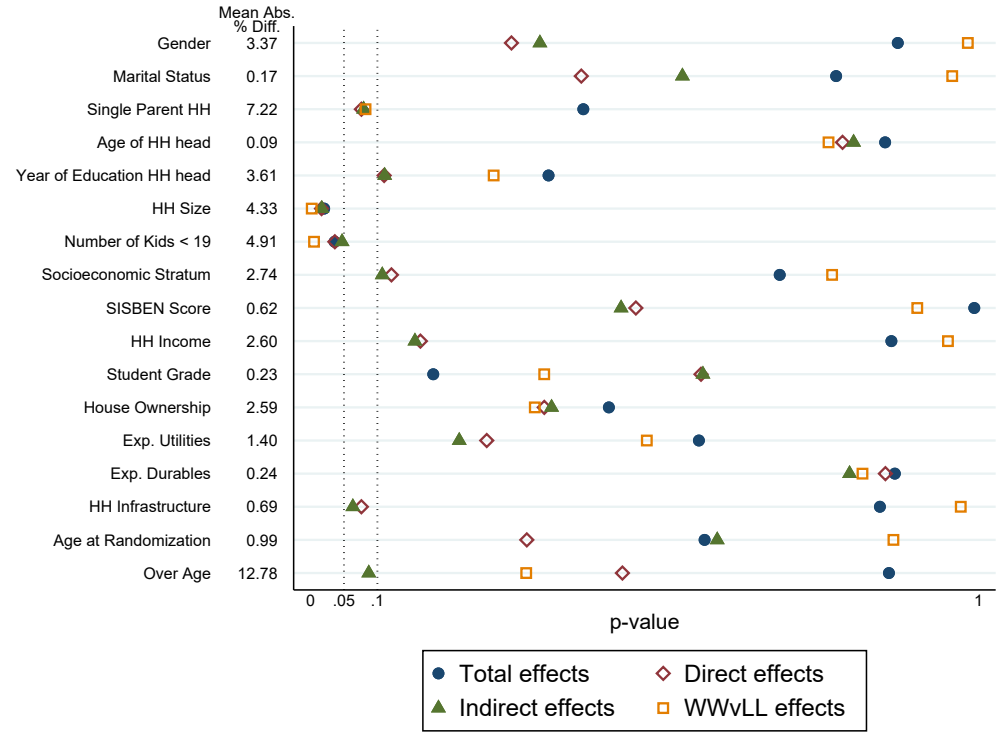
6.1 Validity

We perform a balance test on multiple pre-treatment outcomes measured in the baseline data of SISBEN. Figure 2 plots the p-values across the multiple specifications and outcomes in our Suba sample of families with two registered children, pooling both basic and college treatments. 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 for most variables and groups. We observe significant differences across all four groups for two variables: Household Size and Number of Kids under 19 years old. These differences arise because we have restricted the sample to households with exactly two registered children. As a result, these two variables exhibit limited variability in the sample, and small differences lead to significant statistical differences.

When we examine the means for each type of effect and compare them to the mean of the pure control households, the differences are quite small and not economically meaningful. The second column in the graph shows the average percentage difference

between the means of the treatment and control groups for each of the four categories. For Household Size, the average difference between the four treatment effects and the pure control group is very small at 4.33%. Similarly, for the Number of Kids under 19, the difference is 4.91%. These tests provide strong validity to the randomization and the causal interpretation of our results.

Figure 2: 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 sample is students in households of two registered children in Suba. The total effect of the basic and college treatments together is estimated from a child level regression of Y on a binary variable which equals one for the lottery winners and zero for the lottery losers using all winners and losers, regardless of what happened to their siblings. We estimate the direct effects of the basic and college treatments by regressing Y on a binary variable set to 1 for lottery winners whose sibling lost, and 0 for losers whose sibling also lost. The indirect effects are estimated similarly, using a binary variable set to 1 for losers whose sibling won, and 0 for losers whose sibling lost. Finally, the WWvLL effect is estimated by regressing Y on a binary variable set to 1 for winners whose sibling also won, and 0 for losers whose sibling lost. For each of the four categories, the percentage difference between the means of the treatment and control groups is calculated for each variable, and then its absolute value is taken. The number shown is the average of these four absolute percentage differences. "HH" stands for households for the names displayed in the y-axis.

6.2 Treatment Effects

We estimate equation 1 to compute the total short and long run effects of the basic and college treatments together, and present these results in Panel A of Table 1. We find that, overall, the pooled treatment increased the likelihood of attending classes with high presentism by 11.6 percentage points, reduced the likelihood of attending

classes with low presentism or not studying or working by 8 percentage points, and reduced the likelihood of working as a primary activity by 2.7 percentage points. However, in the long run, we do not observe any precise point estimates for 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.

We obtain similar results in terms of sign when estimating equation 2 to compute the short run effects on treated students of untreated siblings, and present these results. In Panel B of Table 1, we observe that the pooled treatment increased the likelihood of attending classes with high presentism by 7.4 percentage points, reduced the likelihood of attending classes with low presentism or not studying or working by 8 percentage points, and reduced the likelihood of working as a primary activity by 2.7 percentage points. However, in the long run, we do not observe any precise point estimates for the long-run outcomes, and the magnitudes are very close to zero.

Despite the positive direct effects in the short run on attendance, the treatment had negative spillover effects in both the short and long run for the untreated siblings of treated students on attendance, high-school graduation, and college graduation. We obtain these results by estimating equation 3 and present them in Panel C of Table 1. The untreated siblings of treated students decreased the likelihood of attending 80 percent of classes or more in 1.6 percentage points. Even though this last estimate is imprecise, these spillover effects had remarkable long run repercussions in outcomes measured approximately a decade later. We observe a precise decrease in the probability of high school graduation of 3.9 percentage points and of college graduation in 3.7 percentage points.

Finally, when examining the effects on treated students who also have treated siblings, we find a significant short-run positive effect on attending classes with high presentism and significant negative effects on attending classes with low presentism or not studying/working, as well as on the likelihood of working as a primary activity. However, in the long run, the effects remain very close to zero. We obtain these results by estimating equation 4 and present them in Panel D of Table 1.

Table 1: Total, Direct, Spillover and WW vs LL Effects of Winning Lottery on Short and Long Run Outcomes

	Short Run			Long Run		
	Attends School High Presentism	Attends School Low Presentism or No Study or Work	Works as Primary Activity	On-Time High-School Graduation	On-Time College Enrollment	College Graduation
	(1)	(2)	(3)	(4)	(5)	(6)
A) Total Effects						
Basic and College	0.116***	-0.080***	-0.027***	0.009	0.010	0.004
	(0.027)	(0.026)	(0.010)	(0.017)	(0.017)	(0.013)
Observations	1162	1082	1162	2328	2327	2326
Control mean	0.623	0.277	0.0434	0.710	0.210	0.104
B) Direct Effects						
Basic and College	0.074**	-0.055	-0.022	-0.009	0.006	-0.024
	(0.037)	(0.035)	(0.014)	(0.023)	(0.023)	(0.017)
Observations	637	592	637	1283	1283	1283
Control mean	0.633	0.272	0.0417	0.732	0.208	0.121
C) Indirect Effects						
Basic and College	-0.016	0.007	0.001	-0.039*	0.009	-0.037**
	(0.038)	(0.037)	(0.016)	(0.024)	(0.022)	(0.017)
Observations	645	595	645	1284	1284	1284
Control mean	0.633	0.272	0.0417	0.732	0.208	0.121
D) WWvLL Effects						
Basic and College	0.149***	-0.103***	-0.030**	-0.008	0.023	0.002
	(0.036)	(0.034)	(0.013)	(0.024)	(0.024)	(0.019)
Observations	600	563	600	1201	1200	1199
Control mean	0.633	0.272	0.0417	0.732	0.208	0.121

Note: This table presents the results of the estimation of equation 1 on panel A, equation 2 on panel B, equation 3 on panel C, and equation 4 on panel D. The sample in all panels is students in households of two registered children in Suba. This sample does not exclude students who are missing information for certain outcomes or require that their siblings have data for those same outcomes. The specification in all panels control for a dummy that takes the value of one if the individual is in grades 6th-8th, as well as gender, age, sibling's gender, and sibling's age. The outcome in column (1) is a binary variable that takes the value of one if the student attends to more than 80% of the classes. The outcome in column (2) is a binary variable that takes the value of 1 if the student attends 80% or fewer classes or if the student does not study or work. 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. The outcome in column (4) corresponds to a binary variable that takes the value of one if the student is found in the high school exit exam and graduates from high school at the expected age. 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 and is enrolled in college at the expected age. 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.

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. $y_{h,t}^p$ 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 the average of the final completed education levels of each child and possibly how unequal these are. In patricular, the terminal value is denoted by $V_{\bar{t}_h}(e_{1,h}, e_{2,h}, ..., e_{N_h,h}; \rho_1, \rho_2)$. 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_2, 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:

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

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 C 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 A. 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 B 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 $\theta^{\texttt{guess}}$. 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 1 describes the key set of moments (m^{data}) to be matched in Estimation. Along with the regression moments, the set of moments include attendance, stay home, and work choices for different ages, both for observational group and the control group in the experiment[8].

9 Results of Structural Estimation

9.1 Parameter Estimates

Table 2 presents the estimates of the structural parameters. In particular, the ρ_2 parameter captures the household's disutility from the inequalities in siblings education. Figure **??** shows the size of the negative spillover changes on the size of the ρ_3 parameter. The estimated ρ_3 is fairly close to 0, which is consistent with the modest level of aversion of parents on siblings educational inequalities.

The re-entry parameter estimates indicate that it is more costly for students to leave and then re-enter secondary education than it is to leave and then re-enter tertiary education.

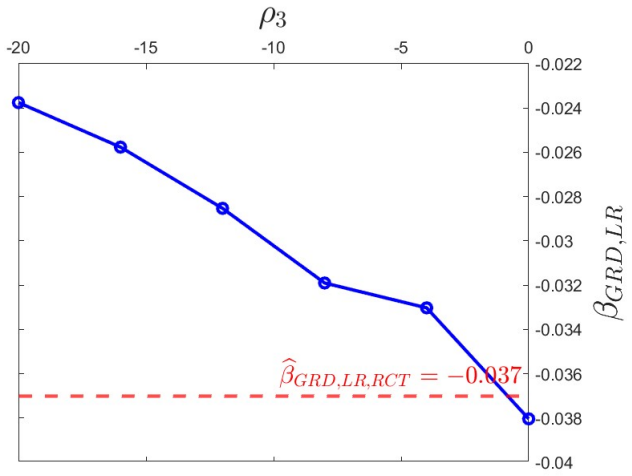
[8] Appendix E will provide further detail on additional moments used for estimation

Table 2: Structural Parameter Estimates

Value Function Parameters		**Passing Rates Parameters**	
ρ_1	0.2694	λ_0^c	–2.1998 (0.10)
ρ_2	-0.4603	λ_0^s	2.9226 (0.95)
α_{low}	0.4512	λ_1^c (Ability Bonus)	4.5412 (0.91)
α_{stay}	0.8893	λ_1^s (Ability Bonus)	0.8312 (0.98)
Reentry Parameters		**Ability Distribution**	
$\alpha_{\text{reentry}}^{\text{second}}$	-6.0697	$\Pr(c_1 = H,\ c_2 = H)$	0.2679
$\alpha_{\text{reentry}}^{\text{college}}$	-2.5743	$\Pr(c_1 = H,\ c_2 = L)$	0.1931
α_e	1.1281	$\Pr(c_1 = L,\ c_2 = H)$	0.1154
α_{both}^{hh}	0.9329	$\Pr(c_1 = L,\ c_2 = L)$	0.4236
α_{both}^{hl}	0.0794		
α_{both}^{lh}	1.5499		
α_{both}^{ll}	1.3100		

Note: The table shows estimated parameters from the value function (block on the left), passing rates (upper-right), reentry parameters (lower-left), and the estimated ability distribution (lower-right).

Figure 3: Identification of indirect effects by ρ_3



Note: This figure presents how the simulated regression coefficient (blueline) changes by the size of ρ_3. The red horizontal line indicates the estimate of the regression in Equation 3.

The passing rate parameters reveal substantial heterogeneity in student ability, especially for college-level progression. Specifically, for a low-ability student, the probability of progressing to the next grade is about 10%, whereas for a high-ability student, it is around 95%.

Depending on the household's ability configuration, the distribution of ability across four types of households is 0.268, 0.193, 0.115, and 0.424, respectively.

9.2 Model Fit

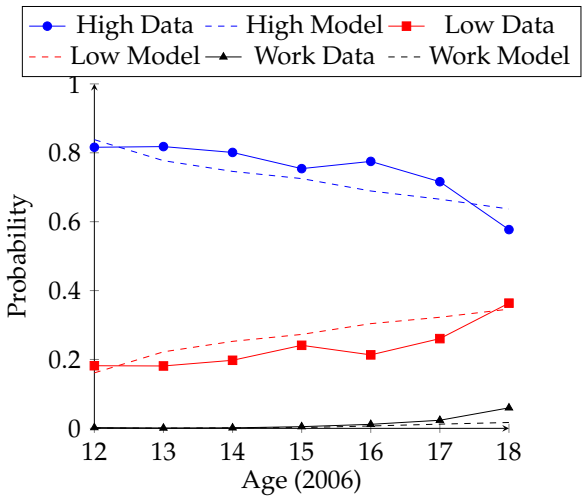
Table 3: Within Sample Fit

	High Attendance		Low or Stay Home		Work		College Graduation	
	Data	Model	Data	Model	Data	Model	Data	Model
Direct	0.074**	0.054	-0.055	-0.052	-0.022	-0.002	-0.024	-0.050
	[0.001,0.147]		[-0.124,0.014]		[-0.049,0.005]		[-0.057,0.009]	
Indirect	-0.016	-0.002	0.007	0.004	0.020	-0.002	-0.037**	-0.033
	[-0.090,0.058]		[-0.066,0.080]		[-0.011,0.051]		[-0.070,-0.004]	
Control Mean	0.633	0.710	0.272	0.285	0.042	0.005	0.121	0.109

Note: This table reports the fit of direct and indirect effects. The "Model" columns show the simulated regression coefficients. The "Data" columns show observed point estimates, along with 95% confidence intervals in brackets. Asterisks indicate significance at the 10% (*), 5% (**), and 1% (***) levels. The final row gives the control means for each outcome.

Table 3 compares the simulated regression coefficients with the RCT estimates. For both the direct and indirect effects, the model's simulated coefficients lie within the 95% confidence intervals of the OLS estimates. Moreover, the model successfully replicates the control means of high attendance, low (or staying at home), work, and college graduation.

Figure 4: All Experimental Outcomes: High, Low, Work

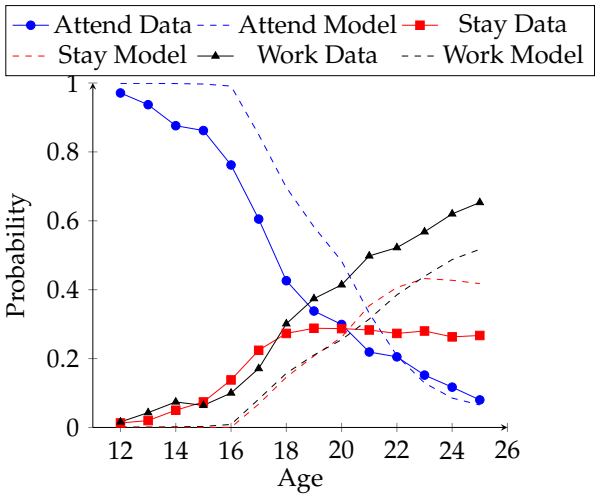


Note: This figure compares Data (solid lines with markers) and Model (dashed lines) for different attendance/work decisions by age for control group in the experiment.

Figure 4 shows how well the model matches the observed choices of high attendance, low attendance or staying home, and working across different ages in 2006 for the experimental sample. Overall, the experimental moments exhibit an excellent fit to the model's predictions.

Figure 5 presents the corresponding fit for non-experimental data. Although the model slightly overestimates attendance at younger ages, it captures well the rise in the share of individuals who start working at age 16—an increase that does not occur among those who were in the experiment.

Figure 5: All Non-experiment (Observed) Outcomes: Attend, Stay, Work



Note: This figure compares Data (solid lines with markers) and Model (dashed lines) for different attendance/stay/work/ decisions by age for observational (Non-experiment) group.

9.3 Out-of-Sample Validation

Table 4 reports the out-of-sample validation results, focusing on households with two winner siblings. The model successfully replicates the strong treatment impact on increasing short-run high attendance and reducing low attendance for these two-winner households. Although the estimated effects on work and college graduation lie within the confidence intervals, they are neither economically nor statistically significant in this subsample.

Table 4: WWLL Effects

Parameter	Data	Model
(1) High Presentism	0.149***	0.082
	[0.078, 0.220]	
(2) Low Presentism	-0.103***	-0.080
	[-0.170, -0.036]	
(3) Work as Primary	-0.030**	-0.001
	[-0.055, -0.005]	
(4) College Grad	0.002	-0.022
	[-0.035, 0.039]	

Note: This table reports the out-of-sample fit of WWLL effects. The "Model" columns show the simulated regression coefficients. The "Data" columns show observed point estimates, along with 95% confidence intervals in brackets. Asterisks indicate significance at the 10% (*), 5% (**), and 1% (***) levels. The final row gives the control means for each outcome.

10 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 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.

References

Abadie, A., S. Athey, G. W. Imbens, and J. M. Wooldridge (2022, 10). When Should You Adjust Standard Errors for Clustering?*. *The Quarterly Journal of Economics 138*(1), 1–35.

Aizer, A. and F. Cunha (2012). The production of human capital: Endowments, investments and fertility. NBER Working Paper 18429.

Angelucci, M. and G. De Giorgi (2009, March). Indirect effects of an aid program: How do cash transfers affect ineligibles' consumption? *American Economic Review 99*(1), 486–508.

Angelucci, M. and V. D. Maro (2016). Programme evaluation and spillover effects. *Journal of Development Effectiveness 8*(1), 22–43.

Attanasio, O., E. Fitzsimons, A. Gomez, M. I. Gutiérrez, C. Meghir, and A. Mesnard (2010). Children's schooling and work in the presence of a conditional cash transfer program in rural colombia. *Economic Development and Cultural Change 58*(2), 181–210.

Attanasio, O., C. Meghir, and A. Santiago (2012). Education choices in mexico: Using a structural model and a randomized experiment to evaluate progresa. *Review of Economic Studies 79*(1), 37–66.

Barham, T., K. Macours, and J. Maluccio (2017, March). Are Conditional Cash Transfers Fulfilling Their Promise? Schooling, Learning, and Earnings After 10 Years. CEPR Discussion Papers 11937, C.E.P.R. Discussion Papers.

Barrera-Osorio, F., M. Bertrand, L. L. Linden, and F. Perez-Calle (2011). Improving the design of conditional transfer programs: Evidence from a randomized education experiment in colombia. *American Economic Journal: Applied Economics 3*(2), 167–195.

Barrera-Osorio, F., L. L. Linden, and J. Saavedra (2019). Medium- and long-term educational consequences of alternative conditional cash transfer designs: Experimental evidence from colombia. *American Economic Journal: Applied Economics*.

Bassi, M., M. Busso, and J. S. Muñoz (2015). Enrollment, graduation, and dropout rates in latin america: Is the glass half empty or half full? *Economía 16*(1), 113–156.

Becker, G. S. and H. G. Lewis (1973). On the interaction between the quantity and quality of children. *Journal of Political Economy 81*(2), S279–S288.

Becker, G. S. and N. Tomes (1976). Child endowments, and the quantity and quality of children. *Journal of Political Economy 84*(4), S143–S162.

Behrman, J., R. Pollak, and P. Taubman (1982). Parental preferences and provision for progeny. *Journal of Political Economy 90*(1).

Behrman, J. R., S. W. Parker, and P. E. Todd (2011). Do conditional cash transfers for schooling generate lasting benefits? a five-year followup of progresa/oportunidades. *The Journal of Human Resources 46*(1), 93–122.

Behrman, J. R., P. Sengupta, and P. Todd (2005). Progressing through progresa: An impact assessment of a school subsidy experiment in rural mexico. *Economic Development and Cultural Change 54*(1), 237–275.

Bobonis, G. J. and F. Finan (2009, 11). Neighborhood Peer Effects in Secondary School Enrollment Decisions. *The Review of Economics and Statistics 91*(4), 695–716.

Burlando, A. (2023). Tuition fees and the intra-household allocation of schooling: Evidence from uganda's free primary education reform. *Economics of Education Review 97*, 102466.

Cahyadi, N., R. Hanna, B. A. Olken, R. A. Prima, E. Satriawan, and E. Syamsulhakim (2020, November). Cumulative impacts of conditional cash transfer programs: Experimental evidence from indonesia. *American Economic Journal: Economic Policy 12*(4), 88–110.

Carneiro, P., I. Rasul, and F. Salvati (2023). Families as Drivers of Inequality: Experimental Evidence from an Early Childhood Intervention. Mimeo.

Chiappori, P.-A. (1988). Rational household labor supply. *Econometrica 56*(1), 63–90.

Ferreira, F. H. G., D. Filmer, and N. Schady. Own and sibling effects of conditional cash transfer programs: Theory and evidence from cambodia. *Research on Economic Inequality*.

Galiani, S., A. Murphy, and J. Pantano (2015). Estimating neighborhood choice models: Lessons from a housing assistance experiment. *American Economic Review 105*(11), 3385–3415.

Galiani, S. and J. Pantano (2022). Structural models. In *Handbook of Labor, Human Resources and Population Economics*. Springer.

Galiani, S., J. Pantano, and Z. Shi (2022). Model-aided identification of policy effects using rcts.

Guo, N., S. Wang, and J. Zhang (2024). The short- and long-run impacts of free education on schooling: Direct effects and intra-household spillovers. *The Economic Journal 134*(663), 2876–2911.

Keane, M. and K. I. Wolpin (1994). The solution and estimation of discrete choice dynamic programming models by simulation and interpolation: Montecarlo evidence. *Review of Economics and Statistics 76*(4), 648–672.

Miguel, E. and M. Kremer (2004). Worms: Identifying impacts on education and health in the presence of treatment externalities. *Econometrica 72*(1), 159–217.

Millán, T. M., T. Barham, K. Macours, J. A. Maluccio, and M. Stampini (2019, 05). Long-Term Impacts of Conditional Cash Transfers: Review of the Evidence. *The World Bank Research Observer 34*(1), 119–159.

Molina Millán, T., K. Macours, J. A. Maluccio, and L. Tejerina (2020). Experimental long-term effects of early-childhood and school-age exposure to a conditional cash transfer program. *Journal of Development Economics 143*, 102385.

Rosenzweig, M. and K. Wolpin (1988). Heterogeneity, intrafamily distribution and child health. *Journal of Human Resources*.

Todd, P. and K. I. Wolpin (2006). Assessing the impact of a school subsidy program in mexico: Using a social experiment to validate a dynamic behavioral model of child schooling and fertility. *American Economic Review 5*(96), 1384–1417.

Todd, P. and K. I. Wolpin (2020). The best of both worlds: Combining rcts with structural modeling. Working Paper.

A 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.)
   - $q_{i,h,t}$ intensity of attendance (high or low) 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]

   $$V_{\bar{t}_h}(\Omega_{\bar{t}_h}) = \rho_1 \sum_i \mathbb{I}(e_{\bar{t}_h,i} \geq 16) \; + \; \rho_2 \, \text{Var}(e_{\bar{t}_h}) \tag{8}$$

   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)

B 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

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

C 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

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

D 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.

E Moments for Estimation and Validation

- "Direct Effect" Design Moments.
- "Sibling Spillover" Design Moments.