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COMPENSATION VS. REINFORCEMENT: EXPERIMENTAL IDENTIFICATION OF PARENTAL AVERSION TO INEQUALITY IN OFFSPRING

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

Parents may invest differently across children by compensating the disadvantaged child or by reinforcing the child with higher expected returns. We study this question using a conditional cash transfer experiment that uniquely randomized transfers at the student level, generating exogenous variation in transfer exposure across siblings within the same household. The transfers increased short-run attendance among treated students but generated negative spillovers on untreated siblings: untreated siblings of treated students were 3.7 percentage points less likely to graduate from college, a decline of about 30 percent relative to the control mean. We interpret these effects using a dynamic model of household schooling decisions that identifies parental aversion to inequality in children's educational outcomes. The estimated model implies limited aversion to inequality in children's educational outcomes and reproduces held-out treatment effects not used in estimation. A decomposition shows that the negative spillover is primarily driven by substitution in educational investments toward the treated child.

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1 Introduction

Parents allocate resources across children in ways that may either compensate for or reinforce differences in children's human capital. In canonical models, parental choices depend on children's endowments, expected returns to investment, and parental preferences over equality in children's outcomes. Identifying these allocation rules is difficult because parental investments respond to unobserved child characteristics, household constraints, and expectations about future returns. Observed differences in investments across siblings therefore rarely reveal whether parents seek to equalize outcomes or concentrate resources where returns are higher. This paper studies this question using a conditional cash transfer (CCT) experiment that randomized transfers at the student level. The resulting within-household variation in transfer exposure allows us to estimate sibling spillovers and provides identifying variation for parental aversion to inequality in children's educational outcomes, a key parameter in our dynamic model of household schooling decisions.

The experiment took place in Bogotá, Colombia, and targeted low-income secondary-school students. It randomly assigned registered students to a control group or to one of two transfer arms. The basic treatment provided attendance-contingent transfers totaling approximately $150 USD per year. The college treatment changed the timing and size of payments: students received $10 USD per month and an additional $300 USD upon high school graduation, paid immediately if they enrolled in tertiary education and one year later otherwise. For the main analysis, we pool the two arms and focus on the contrast between receiving a transfer and receiving no transfer.

The key feature of the experiment is that treatment was randomly assigned at the *student* level. As a result, households with multiple registered children could experience different treatment-exposure configurations: no registered child treated, some registered children treated, or all registered children treated. This variation randomly shifted resources and incentives across children within the same household. It allows us to estimate sibling spillovers and test whether parental responses to child-level transfers are compensatory or reinforcing.

We use this variation to estimate four treatment-effect comparisons. The *total effect* compares students assigned to treatment with those assigned to control, regardless of their siblings' assignment. The *direct effect* compares treated students with untreated siblings to students in pure-control households. The *indirect effect* compares untreated students with treated siblings to students in pure-control households; this is the sibling spillover that is central to the paper. The *win-win versus lose-lose (WWvLL) effect* compares treated students whose siblings were also treated to students in pure-control households. We use the total, direct, and indirect effects to estimate the structural model and reserve the WWvLL effect for out-of-sample validation. The main estimation sample is restricted to households with exactly two registered children.

We combine experimental records, survey data, and administrative data to measure outcomes in both the short and long run. In the short run, we use data collected within one year of the intervention to measure school attendance, whether the student is not engaged in school or working, or whether she works. In the long run, we link students to administrative records to measure college graduation.

The pooled treatment increases high attendance by 11.6 percentage points, reduces school disengagement by 8.0 percentage points, and reduces work as the primary activity by 2.7 percentage points.[1] Direct effects on treated students with untreated siblings are positive for short-run attendance but smaller than the total effect, while long-run direct effects on college graduation are close to zero and imprecisely estimated. The main reduced-form result is the indirect effect: untreated siblings of treated students are 3.7 percentage points less likely to graduate from college, a precisely estimated effect corresponding to a 30 percent reduction relative to the control mean. Among households in which both registered siblings were treated, the WWvLL comparison shows large short-run effects but small long-run effects. Taken together, these results are consistent with households reinforcing educational investments in the treated child, with negative consequences for the untreated sibling's long-run attainment.

To interpret these effects and study the mechanisms behind them, we develop a dynamic model of household schooling decisions. Households choose whether each child attends school, how intensively the child attends, and whether the child works, from late childhood through early adulthood. Following Galiani, Murphy, and Pantano (2015) and Galiani and Pantano (2022), we estimate the model using only part of the experimental variation and reserve the remaining variation for validation, as in Todd and Wolpin (2006). The simulation-based estimator chooses the structural parameters that fit the total, direct, and indirect effects of the pooled treatment. We then validate the model out of sample by testing whether it reproduces the WWvLL effects, which are not used in estimation.

The model identifies parental aversion to inequality in children's educational outcomes, the key preference parameter introduced by Behrman, Pollak, and Taubman (1982). The estimate implies limited aversion to inequality across siblings. This result is consistent with the reduced-form evidence: when one child receives a transfer, households reallocate educational investments toward that child rather than compensating the untreated sibling. The model therefore interprets the negative spillover on untreated siblings as the consequence of reinforcement, not as a failure of the transfer to raise the treated child's schooling incentives.

We then use the model to decompose the negative spillover on the untreated sibling's college graduation. The decomposition separates the substitution effect of changing the treated child's relative schooling incentive from the income effect of increasing household resources. The substitution channel primarily explains the spillover. Holding household utility fixed at the no-treatment level, the compensated simulation generates a 4.24 percentage point decline in the untreated sibling's probability of college graduation, slightly larger than the observed spillover. The residual income effect is positive but small. Thus, the negative spillover is driven mainly by a reallocation of investment toward the treated child.

Finally, we use the estimated model to study how parental inequality aversion shapes educational outcomes and the effectiveness of transfer policy. The counterfactual experiment simulates a transfer that funds both siblings and varies parental inequality aversion—the estimated level, no aversion, and strong aversion—to isolate how preferences shape the household's educational response. With the estimated aversion parameter, transfers increase short-run attendance and college graduation, but also affect the allocation of schooling investments across siblings. When inequality aversion is shut down, households specialize more strongly across children, generating larger education gaps and weaker responses to additional transfers. With strong aversion, parents equalize schooling more strongly across siblings, substantially reducing

[1] We define school disengagement as low school attendance or neither studying nor working.

within-household disparities. The model also suggests that the timing of payments matters: shifting resources from annual stipends toward graduation bonuses can maintain long-run college graduation effects at lower fiscal cost in the counterfactual simulations.

The rest of the paper is organized as follows. The following section discusses the related literature and our contribution. Section 3 describes the experimental intervention. Section 4 presents the data and sample construction. Section 5 defines the reduced-form treatment-effect comparisons. Section 6 reports the reduced-form results. Section 7 presents the dynamic model. Section 8 describes the estimation strategy, model fit, and validation. Section 9 uses the estimated model to conduct counterfactual policy experiments. Section 10 concludes.

2 Related Literature and Contribution

This paper builds on Barrera-Osorio et al. (2011) and Barrera-Osorio et al. (2019), which estimate the short- and long-run effects of the *Conditional Subsidies for School Attendance Program*. Those papers focus on direct treatment effects. We validate their main findings and extend the analysis in two directions. We estimate spillover effects on siblings and develop a structural model to interpret these effects and study how cash transfers reshape resource allocation within the household. In doing so, we contribute to three broad literatures.

Our main contribution is to the literature on parental investment across children. In this literature, parents may allocate resources in a compensatory or reinforcing way across siblings, depending on the distribution of children's endowments.[2] Becker and Tomes (1976) study this question by relaxing the equal-endowments assumption in Becker and Lewis (1973). Behrman, Pollak, and Taubman (1982) introduce parental aversion to inequality in offspring outcomes and estimate a static model using twins, concluding that parents compensate for differences in initial endowments (see also Behrman, 1988). Rosenzweig and Wolpin (1988) extend this framework by introducing sequential decision-making and uncertainty. More recently, Berry, Dizon-Ross, and Jagnani (2025) document preferences for equalizing opportunities across children among both high- and low-income families.

These theoretical models motivated a large empirical literature. One approach exploits natural variation in endowments, such as twin differences, low birth weight, or early-life health shocks. These studies use the timing of the shock and child fixed effects, and find mixed evidence on whether parental responses are compensatory or reinforcing (Rosenzweig and Schultz, 1982; Ayalew, 2005; Datar, Kilburn, and Loughran, 2006; Yi, Heckman, Zhang, and Conti, 2014; Rosales-Rueda, 2014; Restrepo, 2016; Fan and Porter, 2020). A related set of papers exploits exogenous program variation. Aizer and Cunha (2012) use the initial rollout of Head Start and find reinforcing investments across siblings. Carneiro, Rasul, and Salvati (2023) study a prenatal intervention that combines information and cash transfers and also find reinforcing parental behavior in the first 1,000 days of life. Another line of empirical research studies interventions that exogenously vary parents' information about children's skills. A common design measures a proxy for child skill at baseline, then reveals that information to some households and withholds it from others (Barrera-Osorio, Gonzalez, Lagos, and Deming,

[2] Earlier work explored related questions on sibling differences and parental investment. See, for example, Griliches (1979), Sheshinski and Weiss (1979), and Behrman, Rosenzweig, and Taubman (1994).

2020; Dizon-Ross, 2019; Bettinger, Cunha, Lichand, and Madeira, 2021; Bergman, 2021; Berlinski, Busso, Dinkelman, and Martínez, 2025; Rogers and Feller, 2018; Andrabi, Das, and Khwaja, 2017; Kraft and Dougherty, 2013; Zhang, Wei, Song, and Zhang, 2026). These studies generally find positive effects on parental investment and on student outcomes.

Despite this progress, a central empirical challenge remains: parents' allocation of resources and effort across children is typically endogenous. Three recent papers make further progress in identifying how parents reallocate investments across siblings. Dizon-Ross (2019) offers families different goods to allocate across children, although by construction the family must choose one child. Ferreira and Sandholtz (2024) exploit variation in the timing of Free Secondary Education in Tanzania to identify differential investment across children. Giannola (2024) elicits both beliefs and investments to study perceived returns and whether parental responses are compensatory or reinforcing. These papers show that parental responses depend on perceived returns, constraints, and preferences over the distribution of child outcomes. Our paper contributes to this literature by exploiting child-level random assignment within the household, which creates exogenous variation in transfer exposure across siblings. This allows us to estimate within-household spillovers, trace their consequences through college graduation, and quantify parental aversion to inequality in educational outcomes.

We also contribute to the literature on combining experiments and structural models (Todd and Wolpin, 2020). A growing body of work integrates dynamic structural models with randomized control trials to evaluate conditional cash transfer programs (Todd and Wolpin, 2006; Attanasio, Meghir, and Santiago, 2012; Galiani, Pantano, and Shi, 2022). More specifically, we contribute to the strand of this literature that uses multi-arm randomized trials to both estimate and validate structural models (see Galiani, Murphy, and Pantano, 2015; Galiani, Pantano, and Shi, 2022; Galiani and Pantano, 2022). We formulate and estimate a dynamic model of household schooling decisions across siblings and use it to interpret the spillover effects generated by the experiment. The model is grounded in the ideas of Behrman, Pollak, and Taubman (1982), but implemented as an estimable dynamic model in the spirit of Todd and Wolpin (2006). The experimental variation provides a key source of identification for parental aversion to inequality, and households in which both registered children won the lottery provide a held-out sample for out-of-sample validation. Relative to this literature, our contribution is not only to estimate a dynamic model with experimental data, but also to use separate experimental margins for estimation and validation.

Finally, we contribute to the literature on spillover effects of conditional cash transfers and other policy interventions within the household.[3] One strand of this literature studies spillovers from policy interventions more broadly (see Angelucci and Maro, 2016, for a review). Miguel and Kremer (2004) document spillover effects of deworming on health and school participation. In the context of conditional cash transfers, An-

[3] There is a large literature on the impact of conditional cash transfers on human capital accumulation. Several conditional cash transfer programs around the globe have been evaluated, both in the short and long run. The short-run evidence generally points to positive effects on enrollment and other educational outcomes in different contexts (Behrman, Sengupta, and Todd, 2005; Todd and Wolpin, 2006; Attanasio, Meghir, and Santiago, 2012); for reviews, see Fiszbein, Schady, and Ferreira (2009) and García and Saavedra (2023). Long-run effects are more mixed and harder to estimate (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); for a systematic review, see Millán, Barham, Macours, Maluccio, and Stampini (2019).

gelucci and De Giorgi (2009) and Bobonis and Finan (2009) identify positive spillovers on ineligible households and neighborhood peers. A second, closely related strand studies intra-household spillovers from education interventions. Ferreira, Filmer, and Schady (2017) study the effects of CCTs received by scholarship-eligible children on their ineligible siblings in Cambodia, Guo, Wang, and Zhang (2024) study compulsory free education in China, and Burlando (2023) study the abolition of tuition fees in Uganda. Relatedly, Adhvaryu, Gauthier, Jakiela, and Karlan (2026) show that promised future transfers, like immediate cash, increase labor supply and investment, highlighting the role of transfer timing. A central challenge in this literature is that conditional cash transfers are typically allocated at the household level, which limits the identification of within-household spillovers. We contribute by exploiting student-level random assignment of conditional cash transfers, which allows us to distinguish direct effects, sibling spillovers, and the effects of jointly treating both children. This complements existing evidence on spillovers across ineligible households and neighborhood peers, and extends the analysis to long-run educational outcomes.

3 Experimental Intervention

In 2005, public authorities in Bogotá, Colombia, implemented an intervention called *Conditional Subsidies for School Attendance Program*, a conditional cash transfer program designed to reduce dropout in secondary school. The intervention tested alternative transfer schedules for low-income students enrolled in secondary education.[4] It was implemented in two of Bogotá's 12 localities, San Cristóbal and Suba.

The feature of the intervention that matters for this paper is that treatment was assigned randomly at the student level rather than at the household level. As a result, in households with more than one registered child, some siblings could receive the transfer while others did not. This design generates within-household variation in treatment status.

3.1 Timeline of Intervention

The intervention began in 2005 and continued until beneficiaries either dropped out of school or finished high school (and, in one treatment arm, enrolled in tertiary education).[5] Several rounds of data collection also took place after the intervention started.[6] Figure 1 summarizes the intervention and the main stages of data collection.

Target households were identified using a 2004 census (SISBEN), which collected socioeconomic information on households in Colombia with the objective of targeting social spending.[7] The study was conducted in San Cristóbal and Suba, two localities with a large share of low-income households. During January and early February 2005, the city advertised the program. In late February and March, parents had a 15-day period to register eligible children.

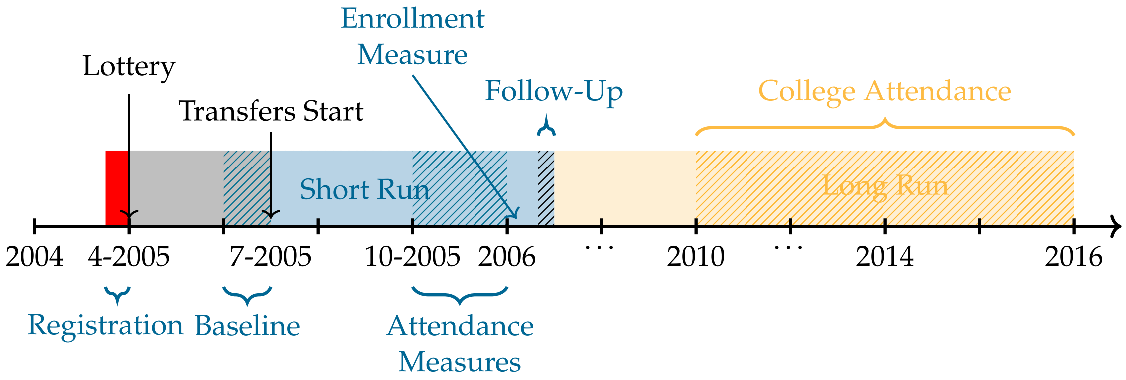
[4] A more detailed description of the intervention can be found in Barrera-Osorio et al. (2011).

[5] The timing of transfers depended on the treatment arm to which students were assigned, as described in Section 3.3.

[6] A detailed description of the data collection is provided in Section 4.

[7] SISBEN, from the original name in Spanish, "Sistema de Identificación de Potenciales Beneficiarios de Programas Sociales."

Figure 1: Timeline of the Intervention



Transfers began in July 2005, at the start of the second half of the school year and continued until the last cohort reached high school graduation or college enrollment–depending on the treatment arm, which we discuss next.

3.2 Eligibility

To qualify, students had to satisfy four conditions: 1) at least one parent had to be present during registration; 2) the student had to have completed fifth grade; 3) the student had to be enrolled in secondary school and not yet have graduated; and 4) the household had to belong to one of the two lowest SISBEN categories. A total of 17,225 students from 12,674 households met these requirements and registered for the program. Of these students, 63 percent were from San Cristóbal and 36 percent from Suba.[8] Available funding covered 56 percent of registered students: 39 percent in San Cristóbal and 17 percent in Suba.

Parents were not required to register all eligible children in the household. Overall, among households that registered at least one child, 77 percent of the age-eligible children were registered for the program.[9] Among the 12,674 households that registered at least one student, 70 percent registered one child, 25 percent registered two, 5 percent registered three, and about 1 percent registered four or five. Selection into program registration does not threaten reduced-form identification because randomization occurred at the student level among registered children. It may, however, affect external validity. Our dynamic model accounts for the selection into the pre-2005 education attainment, allowing us to assess the impact of the selection to the eligibility.

3.3 Treatment Arms

The program randomly assigned students to two treatment arms or to a control group (Barrera-Osorio et al., 2011, 2019):

1. The basic treatment provided participant families with 300,000 Colombian pesos (approximately \$150 dollars) per year if the student attended at least 80 percent

[8] We exclude 84 students because they had already left school before the program began.

[9] We identify the set of potential beneficiaries using census data. Details are provided in Appendix A.

of classes.[10] The transfer was paid every two months, with beneficiary families receiving 60,000 pesos (approximately $30) in each installment.[11]

2. The college treatment changed the timing of the transfer. Students received 20,000 COP per month (approximately $10), and upon high school graduation they received a lump-sum payment of 600,000 COP (approximately $300). This payment was made immediately if the student enrolled in a tertiary education program, or one year later if the student did not enroll.

Relative to the basic treatment, the college treatment provided a larger total transfer and stronger incentives for postsecondary enrollment. Participating families were not informed of the existence of multiple treatment arms at the time of registration. Children randomized to the control group received no transfer.

3.4 Randomization

Treatment assignment took place publicly on April 2005 through a random lottery conducted under close supervision. Lists of selected beneficiaries were printed immediately after the lottery. Randomization was implemented at the *student* level, and stratified by locality, school type, grade, and gender. As a result, some children within the same household were assigned to treatment while others were not. This design created within-household variation in treatment status and allowed comparisons between treated and untreated children in the same family.[12]

Table 1: Distribution of Treatments by Locality and Grade Group

	San Cristóbal		Suba		Total
	6–8	9—11	6—8	9—11	
Control	2,448	1,624	2,101	1404	7,577
Basic	4,138	2,737	1,717	0	8,592
College	0	0	0	1,140	1140
Total	6,586	4,361	3,818	2,544	17,309

Notes: The table reports the number of students assigned to each treatment arm by locality (San Cristóbal vs.Suba) and grade group (6–8 vs. 9–11) at randomization. The 17,309 total is the universe of registered students before any sample restriction; 84 were no longer eligible when transfers began and are dropped, leaving a final sample of 17,225.

Treatment assignment differed across the two localities, Suba and San Cristóbal. Table 1 reports the distribution of treatment arms by locality and grade group. The randomization protocol varied across locality-grade cells. In San Cristóbal, students

[10]As a reference, the Colombian minimum monthly salary in 2005 was 381,500 ($164.4 dollar). The transfer was therefore equivalent to 79 percent of the minimum salary.

[11]Some households assigned to this treatment received two-thirds of the amount (20,000 COP, or about $10) 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 total amount, making them income-neutral. We pool these two delivery methods because they are equivalent for the purposes of this paper.

[12]Appendix Table A.2 reports the joint distribution of households by the number of eligible children and treatment assignment.

in grades 6–8 and 9–11 were randomized between the control group and the basic treatment. In Suba, students in grades 6–8 were randomized between the control group and the basic treatment, whereas students in grades 9–11 were randomized between the control group and the college treatment. Students in Suba enrolled in grades 9–11 were therefore the only ones offered the college treatment.[13]

4 Data

We combine experimental, survey, and administrative data to measure short-run and long-run outcomes for students in the experiment. The data include the full experimental universe, surveys collected shortly after the intervention began, and administrative records that allow us to track outcomes for up to 11 years after the intervention.[14]

Our analysis begins with the 17,309 students who registered for the experiment. We identify these students by combining the registration records, which contain basic information collected at enrollment for students in the two treated localities of Bogotá, with the lottery records, which report student-level treatment assignment.

4.1 Short-Run Outcomes

Between May and July 2005, baseline survey data were collected from participating students. The survey was self-administered during class time. Because of budget constraints, data collection was limited to the 68 schools with the largest number of registered students. These schools accounted for 9,715 students, or 56 percent of all registered students.

Attendance data were collected between October and December 2005. The sample was limited to students in the same 68 schools covered by the baseline survey. Over a 13-week period, enumerators made unannounced visits to schools and classrooms to record student attendance.

In March 2006, a follow-up survey was administered to students in the same 68 schools. Students were interviewed at home so that those who had dropped out after the intervention could still be observed. Follow-up rates were high: 8,736 of the 8,896 students surveyed at baseline, or 98.14 percent, were successfully located and interviewed. This corresponds to 56 percent of all students originally registered in the experiment. Attrition in the attendance and follow-up samples was similar across treatment arms, which reduces concerns about selective attrition bias (Barrera-Osorio et al., 2011).

In the short run, we study attendance and work status. Attendance is measured from school visits and work status is measured from the household follow-up survey. We construct these outcomes following Barrera-Osorio, Bertrand, Linden, and Perez-Calle (2011).

[13]In the main specifications, we include grade dummies to account for this heterogeneity.

[14]Appendix A provides additional details on the merge between the data sets.

4.2 Long-Run Outcomes

We use the 2004 SISBEN census of low-income households to reconstruct households and identify family members over time. Both the experimental data and SISBEN contain national identification numbers for program beneficiaries and their household members, which allow us to link experimental participants and their families to long-run administrative records.

We complement our analysis with the administrative registry of tertiary education students (SPADIES), observed between 1998 and 2016. Because Colombian tertiary institutions are required to report student information to the Ministry of Education, this registry covers the universe of students enrolled in tertiary education and allows us to measure college graduation in the long run. We link the experimental sample to these records following Barrera-Osorio et al. (2019). Because the registry is administrative, failure to match an experimental participant is itself informative.

4.3 Siblings Identification

Sibling links and outcomes are observed directly in the household survey data. For the long-run analysis, we use the 2004 SISBEN census to identify school-age children living in the same household. This approach is not exact, since co-resident school-age children are not necessarily siblings. To reduce misclassification, we restrict the sample to children in the same household who share a last name and therefore are likely to share a parent.

5 Experimental Analysis of the Intervention

5.1 Estimation Sample

Our main analysis imposes two sample restrictions to the experimental sample. First, we restrict the sample to children in one of the two experimental localities (Suba). As Barrera-Osorio et al. (2017) show, the educational effects of the intervention were concentrated in the college treatment, which was offered only in Suba. By contrast, the basic treatment yields imprecise estimates across multiple outcomes. Restricting the sample to Suba therefore focuses the analysis on the treatment variation that generates meaningful effects.[15]

Second, we restrict the sample to households with exactly two registered siblings. Spillover effects can be cleanly identified in this group. In households with three or more registered children, the probability that at least one sibling is treated rises mechanically with family size, which complicates the comparison between untreated siblings in treated households and children in pure-control households. Restricting the sample to two-child households removes this source of variation and allows the treatment effects to be decomposed as in Section 5.2.

[15] Appendix B shows that estimates using a broader sample are qualitatively similar but less precise. Appendix B.1 shows that this restriction isolates the part of the experiment in which the treatment effects are concentrated, while Appendix B.2 shows that broader samples yield qualitatively similar but attenuated estimates.

5.2 Treatment Effect Decomposition

Total Effects. We estimate total effects by comparing the outcomes of students who won the lottery with those of students who lost, regardless of their siblings' treatment status. To increase statistical power, we pool both basic and college treatment and estimate both short-run and long-run effects. The short-run estimates correspond to those in Barrera-Osorio et al. (2011), and the long-run estimates correspond to those in Barrera-Osorio et al. (2019).

Formally, we estimate:

$$Y_i = \beta_0^T + \beta^T D_i + \beta_X^T X_i + \varepsilon_i^T, \tag{1}$$

D_i is a treatment indicator equal to 1 if student i was assigned to a transfer, and 0 otherwise. Y_i denotes an outcome measured either in the short run or in the long run. X_i includes an indicator for grades 6–8, gender, age, sibling's gender, and sibling's age. We report heteroskedasticity-consistent standard errors.[16]

Treatment Decomposition. Consider households with exactly two registered students. Under student-level random assignment, these households fall into three groups: fully treated households, in which both siblings receive treatment; partially treated households, in which one sibling receives treatment and the other does not; and pure-control households, in which neither sibling receives treatment. Denote these household types by X, Y, and Z, respectively, so that $(X_1, X_2) = (1, 1)$, $(Y_1, Y_2) = (1, 0)$, and $(Z_1, Z_2) = (0, 0)$, where the subscript indexes the child within the household.

This classification allows us to separate the analysis into three comparisons, each defined relative to the pure-control group:

1. *Direct Effects.* Direct effects compare treated students in partially treated households with students in pure-control households. In the notation above, this corresponds to comparing Y_1 with Z_1 and Z_2. Formally, we estimate:

$$Y_i = \beta_0^D + \beta^D D_i + \beta_X^D X_i + \varepsilon_i^D, \tag{2}$$

   where D_i equals 1 if student i was assigned to a transfer and the sibling was not treated, and 0 if neither sibling was treated.

2. *Indirect Effects.* Indirect effects compare untreated students in partially treated households with students in pure-control households. In the notation above, this corresponds to comparing Y_2 with Z_1 and Z_2. Formally, we estimate:

$$Y_i = \beta_0^I + \beta^I D_{s(i)} + \beta_X^I X_i + \varepsilon_i^I, \tag{3}$$

   where $D_{s(i)}$ equals 1 if the sibling of student i was assigned to a transfer while student i was not treated, and 0 if neither sibling was treated.

3. *Win-win versus lose-lose Effects.* These effects compare fully treated households with pure-control households. In the notation above, this corresponds to comparing X_1 and X_2 with Z_1 and Z_2. Formally, we estimate:

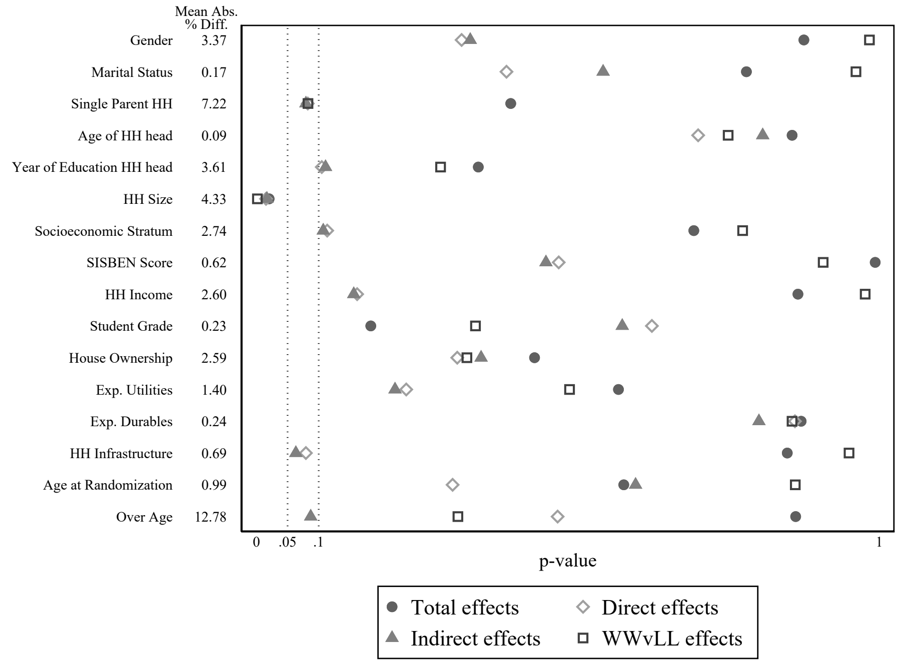
$$Y_i = \beta_0^W + \beta^W D_i + \beta_X^W X_i + \varepsilon_i^W, \tag{4}$$

   where D_i equals 1 if student i was assigned to a transfer and the sibling was also treated, and 0 if neither sibling was treated.

[16] We use Huber-White robust standard errors because treatment varies at the individual level (Abadie et al., 2022). The results are robust to alternatives, including standard errors clustered at the school level.

Together, these comparisons characterize how treatment exposure varies within the household. Direct effects capture the impact of treatment on treated students in partially treated households. Indirect effects capture spillovers on untreated siblings. The win-win versus lose-lose comparison captures the joint effect when both siblings are treated. Under SUTVA at the household level, a child's outcome would depend only on her own treatment status, not on her sibling's assignment. In that case, the total, direct, and win-win versus lose-lose effects would coincide. Differences across these effects therefore provide evidence of within-household spillovers.

Figure 2: Balance Across Baseline Covariates



Notes: The figure plots p-values from balance regressions for the variables listed on the y-axis. The sample consists of students in households with two registered children in Suba. The total effect is estimated from a child-level regression of Y on an indicator equal to 1 for lottery winners and 0 for lottery losers, regardless of siblings' treatment status. The direct effect is estimated by comparing lottery winners whose sibling lost with lottery losers whose sibling also lost. The indirect effect is estimated by comparing lottery losers whose sibling won with lottery losers whose sibling also lost. The win win-lose lose (WWvLL) effect is estimated by comparing lottery winners whose sibling also won with lottery losers whose sibling also lost. For each treatment comparison, the second column reports the absolute percentage difference in means between the treatment and pure-control groups, averaged across the four effect definitions. "HH" denotes household.

6 Reduced Form Results

6.1 Validity

We assess balance across a broad set of pre-treatment variables measured in the 2004 census baseline data. Figure 2 plots the p-values from these balance tests using the estimating sample and pooling the basic and college treatments. As expected under random assignment, we do not observe systematic differences: for most variables and treatment comparisons, p-values are well above conventional significance levels. Household size shows a statistically significant differences across some comparisons. The second column of Figure 2 reports the average percentage difference in means between each treatment group and the pure-control group. For household size, the gap is economically small (4.33%). Overall, the balance tests support the validity of the randomization and the causal interpretation of the estimates.

6.2 Treatment Effects

We estimate equation 1 to obtain the total short-run and long-run effects of the pooled basic and college treatments. Panel A of Table 2 reports the results. The pooled treatment increases the probability of high attendance by 11.6 percentage points, reduces the probability of low attendance or neither studying nor working by 8.0 percentage points, and reduces the probability that work is the student's main activity by 2.7 percentage points. By contrast, the long-run estimates are small and imprecise across all outcomes. These estimates replicate the short-run findings in Barrera-Osorio, Bertrand, Linden, and Perez-Calle (2011) and the long-run findings in Barrera-Osorio, Linden, and Saavedra (2019).[17]

The direct effects on treated students with untreated siblings, estimated from equation 2, are reported in Panel B of Table 2. The pooled treatment increases the probability of high attendance by 7.4 percentage points. The corresponding reductions in low attendance and work are smaller than in the total-effects panel and are not precisely estimated. In the long run, the direct estimates on college graduation are small and statistically indistinguishable from zero.

Despite the positive short-run direct effects on attendance, the intervention generates negative spillovers for untreated siblings. Panel C of Table 2 reports the indirect effects estimated using equation 3. Untreated siblings of treated students are 1.6 percentage points less likely to attend at least 80 percent of classes, although this estimate is imprecise. The long-run spillovers are economically meaningful and statistically significant. They suggest that the probability of college graduation falls by 3.7 percentage points among untreated siblings of treated children.

Finally, Panel D of Table 2 reports the effects estimated using equation 4 for treated students whose siblings were also treated. In the short run, these estimates show a positive effect on high attendance and negative effects on low attendance or neither studying nor working, and on work as the main activity. In the long run, the estimates are small and close to zero.[18]

[17] Appendix B.1 discusses how the estimates in Table 2 relate to the results in Barrera-Osorio, Bertrand, Linden, and Perez-Calle (2011) and Barrera-Osorio, Linden, and Saavedra (2019). A central result in these papers is that the program increased on-time college enrollment. For completeness, we report this outcome in the Appendix. We replicate the positive effect of the College arm on on-time college enrollment in specifications close to Barrera-Osorio, Linden, and Saavedra (2019), but the coefficient attenuates in the two-registered-children Suba sample used for the within-household decomposition.

[18] Appendix B.2 reports alternative estimates for Table 2 using different samples, including a common sample across outcomes, a sample that includes San Cristóbal, and total-effect estimates for the full universe of registered students. Appendix B.3 also reports specification-curve exercises for the total effects across alternative specifications that vary the sample definition and the set of control variables. The short-run and long-run total effects reported in Panel A of Table 2 are qualitatively robust across these alternatives.

Table 2: Total, Direct, Spillover and "Win Win - Lose Lose" Effects of Winning Lottery on Short and Long Run Outcomes

	Short Run			Long Run
	School Attendance (High)	Low School Attendance or Neither Studying nor Working	Works as Primary Activity	College Graduation
	(1)	(2)	(3)	(4)
A) Total Effects				
Treated	0.116***	-0.080***	-0.027***	0.004
	(0.027)	(0.026)	(0.010)	(0.013)
Observations	1162	1082	1162	2326
Control mean	0.623	0.277	0.0434	0.104
B) Direct Effects				
Treated	0.074**	-0.055	-0.022	-0.025
	(0.037)	(0.035)	(0.014)	(0.017)
Observations	637	592	637	1285
Control mean	0.633	0.272	0.0417	0.121
C) Indirect Effects				
Treated	-0.016	0.007	0.001	-0.037**
	(0.038)	(0.037)	(0.016)	(0.017)
Observations	645	595	645	1284
Control mean	0.633	0.272	0.0417	0.121
D) WWvLL Effects				
Treated	0.149***	-0.103***	-0.030**	0.002
	(0.036)	(0.034)	(0.013)	(0.019)
Observations	600	563	600	1199
Control mean	0.633	0.272	0.0417	0.121

Notes: This table reports the four-way decomposition of treatment effect. Panel A reports total effects estimated from equation 1, comparing lottery winners with lottery losers regardless of their siblings' treatment status. Panel B reports direct effects estimated from equation 2, comparing lottery winners whose sibling lost with lottery losers whose sibling also lost. Panel C reports indirect, or sibling spillover, effects estimated from equation 3, comparing lottery losers whose sibling won with lottery losers whose sibling also lost. Panel D reports win-win vs. lose-lose (WWvLL) effects estimated from equation 4, comparing lottery winners whose sibling also won with lottery losers whose sibling also lost. In all panels, the sample consists of students in households with two registered children in Suba. The sample does not exclude students who are missing a given outcome or require their siblings to have non-missing data for that same outcome. All specifications control for an indicator for grades 6–8, gender, age, sibling's gender, and sibling's age. The outcome in column (1) equals 1 if the student attends more than 80% of classes. The outcome in column (2) equals 1 if the student attends 80% or fewer classes, or neither studies nor works. The outcome in column (3) equals 1 if the student's main activity is work. The outcome in column (4) equals 1 if the student graduates from college. Heteroskedasticity-consistent standard errors are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

7 Model

We develop a dynamic model of household decision-making that rationalizes the experimental estimates in Table 2. We begin by introducing the notation and the household choice set. We then describe household preferences, the schooling technology, and optimal decisions. The model applies to low-income households with at least one child in grades 6 to 11 in 2005, the year of randomization. Some households have more than one child who is eligible for the program.

7.1 Notation and Environment

In $t = 2005$, households differ in the number of children they have and in their age structure. Let $N_{h,t} \in 1, 2$ denote the number of children in household h in period t, and index children by birth order, $i = 1, 2$. Each child is endowed with ability $\xi_{i,h}$, which can be low ($\xi = 0$) or high ($\xi = 1$). Let $\xi_h = \{\xi_{i,h}\}_{i=1}^{2}$ collect the ability endowments of the siblings in household h. Let $a_{i,h,t}$ denote the age of child i in household h at the beginning of year t. Every child completes grade 5 but may drop out in subsequent grades. Let $e_{i,h,t}$ denote the completed schooling level of child i in household h at the beginning of year t. Finally, let $\mathcal{K}_{h,t}$ denote the set of children in household h who are eligible for the CCT program. Eligibility requires that, at the beginning of year t, the child has completed grade 5 but not yet 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, $i = 1, 2$, to sign up for the CCT lottery. We let the indicator $\ell_{i,h}$ be equal to one if child i in household h signs up for the CCT lottery, and zero otherwise. We model registration as the outcome of an information shock: with some probability, each eligible child's household learns that the child can be signed up, and we match these probabilities to empirical registration rates by birth cohort and sibling spacing.Children whose households receive this shock register ($\ell_{i,h} = 1$) and the rest do not ($\ell_{i,h} = 0$), so that selection into the lottery reflects differential program awareness rather than unobserved child ability or household preferences.

Randomization occurs among the children who sign up, and individual-level randomization induces household-level variation in treatment status. For example, a household with two children who sign up for the lottery may end up in one of four treatment states: (a) both children are assigned to the control group; (b) the first-born child is treated and the second is assigned to the control group; (c) the first-born child is assigned to the control group and the second is treated; or (d) both siblings are treated.

Choices. Household h makes a sequence of decisions $d_{h,t} = \{d_{i,h,t}\}_{i=1}^{i=2}$ for each child i from ages $a = 11$ to 25 to maximize the present discounted value of expected lifetime utility. In particular, household choices are summarized by the following four 0-1 indicators:

- $d_{i,h,t}^{high}$, indicating whether child i attends school at least 80% of school days in year t;
- $d_{i,h,t}^{low}$, indicating whether child i attends school less than 80% of school days in year t;
- $d_{i,h,t}^{work}$, indicating whether child i works in year t;

- $d_{i,h,t}^{home}$, indicating whether child i stays at home, without working or attending school, in year t.

For each child, the household chooses whether the child attends school, works, or stays at home, and, conditional on school attendance, the level of attendance. High attendance, low attendance, work, and staying at home are mutually exclusive choices.

State Variables. $t(a)$ indexes the calendar year in which a child is age a, and therefore affects transfer availability. Let $y_{h,t}^{p}$ denote parental household income, excluding any income earned by children. Let $y_{i,h,t}^{c}$ denote the labor income earned by child i in year t, and let $X_{h,t}$ denote a vector of observable child- and household-level characteristics, some time-varying and some time-invariant. ε_t^u is a vector of child-specific shocks that captures unobserved heterogeneity in household preferences over school attendance, absenteeism, and work relative to leisure. These shocks are observed by the household before decisions are made, but not by the econometrician. The vector $\Omega_t = \{t(a), \{a_{i,h,t}\}, \{e_{i,h,t}\}, X_t\}$ collects all the observed (to both household and econometrician) state variables at time t. To simplify the exposition, let $\varepsilon_{i,t}^{u} = \{\varepsilon_{i,t}^{high}, \varepsilon_{i,t}^{low}, \varepsilon_{i,t}^{work}, \varepsilon_{i,t}^{home}\}$ collect the ε that affect preferences and let $\varepsilon_t = \{\{\varepsilon_{i,t}^{u}\, \varepsilon_{i,t}^{y^c}\}_{i=1}^{2}, \varepsilon_t^{y^p}\}$ collect all the ε, including those that represent shocks to parental or child income. 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.

To track experimental assignment, let Z denote the child's treatment group. `Basic` denotes the basic treatment, which provides a transfer of \$300,000 per year; `College` denotes the college treatment, which provides \$200,000 per year plus \$600,000 upon enrollment in college; and `Control` denotes no transfer. We denote by τ^Z the transfer associated with assignment Z.

Decision Horizon. Let $\underline{t}_h = t(a_{1,h} = 11)$ be the first calendar year in which household h makes decisions in the model, corresponding to the year in which the first-born child turns 11. Similarly, let $\bar{t}_h = t(a_{2,h} = 25)$ be the last calendar year in which household h makes decisions, corresponding to the year in which the last-born child turns 25. The decision horizon therefore varies across households with the number of children and their age spacing. We denote by $T_h = \bar{t}_h - \underline{t}_h + 1$ the number of decision periods. In the last period, when the youngest child reaches $a = 25$, the household receives a terminal value $V_{\bar{t}}(\Omega\bar{t})$, which depends on the final schooling levels of all children. The time unit of the model is one year. The model applies to households with children born in years $t \leq 1995$ and with at least one child between ages $a = 11$ and $a = 22$ in $t = 2005$, when recruitment for the experiment takes place.

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/attendance in the school attendance of child i;
- $\mathbb{1}(s_1 = 1, q_1 = 1, s_2 = 1, q_2 = 1)$: the extra utility from when both children attend school more than 80% .

Household utility in each period is also affected by preference shocks ε^U.

Households also value children's final completed level of education through a terminal value function that depends on the average schooling level across children and on the dispersion of schooling within the household. In particular, 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 receives this value in year $\bar{t}_h$, when the youngest child reaches age 25. The key structural parameter ρ_2, introduced in Behrman et al. (1982), enters the terminal value function and governs parental aversion to inequality in completed schooling across children.

We use the variance of completed schooling to summarize inequality in educational outcomes within the household. This choice has three advantages in our setting. First, it maps directly into the framework in Behrman et al. (1982) and Behrman (1988), where parents value both the level and the dispersion of children's outcomes. Second, it is symmetric across siblings and therefore does not impose that parents favor a specific child by birth order or baseline attainment. Third, with two children, it provides a parsimonious measure of the schooling gap that is closely connected to the difference between direct and indirect treatment effects on college graduation.[19] Our specification isolates a single inequality-aversion parameter while preserving tractability in the dynamic model. Following Rosenzweig and Wolpin (1988), we specify the functional form of the terminal value as

$$V_{\bar{t}_h}(e_{1,h}, \ldots, e_{N_h,h}; \rho_1, \rho_2) = \rho_1 \sum_i \mathbb{1}(e_{\bar{t}_h,i} \geq 16) \ + \ \rho_2 \operatorname{Var}_i\left(e_{\bar{t}_h,i}\right) \tag{5}$$

where $e_{\bar{t}_h,i}$ is the completed years of education of child i in household h. The first term rewards each child who completes higher education, capturing parents' valuation of children's attainment, while $\operatorname{Var}_i\left(e_{\bar{t}_h,i}\right)$ measures the dispersion of completed schooling across the household's children. The parameter ρ_2 therefore governs how strongly parents dislike unequal educational outcomes among their children: with $\rho_2 < 0$, greater dispersion lowers terminal value.

7.3 Household Technology

Household h produces schooling attainment $e_{i,h}$ for each child i by sending the child to school rather than having the child work or stay at home. School attendance is necessary but not sufficient for grade progression. Progression also depends on the child's attendance intensity and innate ability ξ_i. We model schooling accumulation through a grade progression probability π, which depends on the child's age a, completed years of schooling e, attendance intensity q, their innate ability endowment ξ and other household characteristics X:

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

[19] Alternative aggregators, such as CES or max-min specifications, would impose different curvature on parental preferences over the distribution of children's educational outcomes.

7.4 Optimal Household Decisions

The problem the household solves differs before and after recruitment into the experiment. Before 2005, the household solves a "business-as-usual" problem without CCT transfers. In 2005, households learn that a CCT lottery will take place, and registered children are randomized to either the control group or one of the treatment groups. From 2005 onward, households in which at least one child wins the lottery make decisions under an updated budget constraint that incorporates the transfer schedule associated with each child's assignment.

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 to simplify notation. Each household h then solves:

$$\max_{\{\{s_{it},q_{it},w_{it}\}_{t=\underline{t}}^{t=\bar{t}_h}\}_{i=1}^{i=N_h}} \left\{ E\left[\sum_{t=\underline{t}_h}^{t=\bar{t}_h} \delta^{t-\underline{t}_h} U\left(c_t, s_t, s_{(t-1)}, q_t, w_t; \Omega_t, \varepsilon_t^u\right) + \delta^{T_h} V_{\bar{t}_h}(\Omega_{\bar{t}_h}) \Big| \chi_{\underline{t}_h} \right] \right\} \tag{6}$$

subject to

$$\begin{aligned} c_{ht} &= y_t^p + \sum_{i=1}^{i=N_h} \{w_{it} \times y_{it}^c + s_{it} \times \mathbb{1}\{Z_i \neq \text{Control}\} \times \tau_i(Z_i, e_{i,t})\} \\ \Pr\left(e_{i,t+1} = e_{i,t} + 1 \mid s_{i,t} = 1\right) &= \pi_e\left(a_{it}, w_{it}, q_{it}, e_{it}, X_t, \xi_i\right) \\ y_t^p &= y^p(X_t, \varepsilon_t^{y^p}) \\ y_{i,t}^c &= y^c(a_t, e_t, X_t, \varepsilon_{it}^{y^c}) \\ &\text{given } \Pr(X_{t+1}|X_t, d_t) \end{aligned} \tag{7}$$

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 can be recasted recursively as a finite-horizon dynamic program.[20] After solving the model, we obtain the household's optimal school attendance, work, and attendance-intensity decisions as functions of the state variables $(\Omega_t, \varepsilon_t, \xi)$. The policy functions are given by

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

[20] Appendix C provides computational details on the recursive representation and the algorithm used to solve the model.

8 Model Estimation

8.1 Identification

Although all moments jointly identify the model parameters, each parameter has main sources of variation that provide identification. The inequality-aversion parameter ρ_2 is primarily disciplined by the difference between the direct and indirect treatment effects on college graduation. In particular, the difference in college graduation between untreated students whose siblings are treated and students in pure-control households provides variation that helps identify ρ_2. More negative values of ρ_2 imply smaller differences in completed schooling across siblings, while values closer to zero allow larger within-household gaps. The parameter governing parental preferences for average education, ρ_1, is primarily disciplined by average college graduation rates.

Flow utility parameters are disciplined by age-specific moments for attendance, work, and staying at home, observed in both the experimental control group and the observational data. Variation in these choices across ages helps identify the relative utility of each activity. Re-entry costs are disciplined by delayed progression. In secondary school, re-entry parameters are identified by the share of students who graduate from high school after age 19, since higher re-entry costs imply fewer delayed graduations. In college, re-entry parameters are identified by the age distribution of college graduates, since higher re-entry costs imply more concentrated graduation at younger ages.

The grade progression function parameters are identified by the average progression rates observed in the data. The penalty for low attendance is identified by comparing observed graduation rates between students with consistently high attendance and those who experience any low-attendance spell during secondary school. Finally, the distribution of unobserved household types p_{k_p} of siblings ability configuration is identified by the different college graduation rates by the birth-order of the children.

8.2 Estimation Strategy

To estimate the model, we follow the strategy in Galiani, Pantano, and Shi (2022). Each child's ability endowment ξ_i is known to the household but unobserved by the econometrician. We specify a discrete distribution of unobserved household types $k = 1, ..., 4$. These types capture the possible configurations of child ability endowments. In the two-child households on which we focus, the four possible types are $(\xi_1, \xi_2) = (0, 0), (0, 1), (1, 0), (1, 1)$. Let p_k denote the probability that a household is of type k. These probabilities are estimated jointly with the structural parameters. The random vector ε is assumed to follow a multivariate normal distribution, $\varepsilon \sim N(0, \Sigma)$.[21]

The parameters to be estimated are $\theta = \alpha, \lambda^e, \phi^{y^p}, \phi^{y^c}, \rho, \Gamma, p_k$. Here, α denotes the utility-function parameters; λ^e denotes the parameters of the grade-progression probability function; ϕ^{y^p} and ϕ^{y^c} denote the parameters of the parental and child income equations, respectively; ρ denotes the terminal-value parameters; Γ denotes the Cholesky decomposition of Σ, the variance-covariance matrix of ε; and p_k denotes the unobserved-type probabilities for $k = 1, ..., 3$, with $p_4 = 1 - \sum_{k=1}^{3} p_k$. We estimate the model using a simulation-based approach in the spirit of indirect inference and

[21] Appendix D describes the functional forms used for implementation. Appendix E provides more details on the distributional assumptions.

simulated minimum distance. In addition to regression moments, the moment set includes age-specific attendance, staying-at-home, and work choices from both the observational sample and the experimental control group.[22]

8.3 Estimates, Model Fit, and Validation

The estimates imply limited parental aversion to inequality between siblings. Table 3 reports the preference-parameter estimates. The estimated inequality-aversion parameter is $\widehat{\rho}_2 = -0.085$, which is close to zero within the admissible domain $\rho_2 \in (-\infty, 0]$. This indicates limited aversion to disparities in completed schooling across children. By contrast, the estimated parameter on average education, $\widehat{\rho}_1 = 8.859$, implies that households place significant value on children's total educational attainment.

Table 3: Preference Parameter Estimates

(a) Terminal Value	Estimate	S.E.
Average Education (ρ_1)	8.859	(0.174)
Sibling's Inequality (ρ_2)	-0.085	(0.008)
(b) Flow Utility		
Low Attendance (α_{low})	0.380	(0.042)
Stay (α_{stay})	-0.241	(0.081)
Secondary School Reentry Cost ($\alpha^{\text{second}}_{\text{reentry}}$)	-6.199	(0.060)
College Reentry Cost ($\alpha^{\text{second}}_{\text{reentry}}$)	-0.578	(0.033)
(c) Both Kids Attendance by Types		
High-High ($\alpha^{\text{HH}}_{\text{both}}$)	1.090	(0.028)
High-Low ($\alpha^{\text{HL}}_{\text{both}}$)	0.001	(0.001)
Low-High ($\alpha^{\text{LH}}_{\text{both}}$)	0.018	(0.007)
Low-Low ($\alpha^{\text{LL}}_{\text{both}}$)	2.231	(0.075)
(d) Shock Standard Deviation		
High Attendance (σ_c)	1.167	(0.030)
Low Attendance(σ_{low})	0.639	(0.027)
Work (σ_{work})	1.209	(0.043)

Note: Standard errors in parentheses computed using bootstrap. Panel (a) reports terminal value function parameters where ρ_1 captures parental preference for average education levels and ρ_2 measures aversion to inequality between siblings. Panel (b) reports flow utility parameters. Low attendance and stay utilities are relative to the normalized utility of high attendance. Re-entry costs capture the disutility of returning to school after interruption. Panel (c) reports complementarity parameters for simultaneous attendance decisions, where H denotes high attendance and L denotes low attendance for each child.

Re-entry costs are substantially larger for secondary school, $\alpha^{second}_{reentry}$, than for college, $\alpha^{college}_{reentry}$. This creates stronger path dependence in secondary education. Once students

[22] Appendix F describes the estimation procedure and Appendix G provides details on the moments used for estimation.

exit secondary school, high re-entry costs make return unlikely. By contrast, college allows more flexibility after interrupted enrollment.

Ability differences matter more in college than in secondary school. Table 4 reports additional structural parameters governing grade progression and household heterogeneity. In secondary school, high-ability students have only a modest advantage in progression probabilities, λ_1^s, while low attendance generates a large progression penalty. This implies that attendance is more important than ability for secondary-school progression. In college, the ability differential, λ_1^c, becomes the main determinant of progression. As a result, low-ability students who enroll in college have a lower probability of graduating than their high-ability peers.

Table 4: Other Parameter Estimates

(a) Progression Parameters	Estimate	S.E.
Logit - Baseline Secondary (λ_0^s)	3.742	(0.077)
Logit - High Ability Bonus in Secondary School (λ_1^s)	0.466	(0.029)
Logit - Low Attendance Penalty in Secondary School (λ^{low})	-3.717	(0.052)
Logit - Baseline College (λ_0^c)	-4.993	(0.014)
Logit - High Ability Bonus (College) (λ_1^c)	4.971	(0.059)
(b) Type Distribution		
$\Pr(c_1 = H, c_2 = H)$	0.040	(0.005)
$\Pr(c_1 = H, c_2 = L)$	0.481	(0.009)
$\Pr(c_1 = L, c_2 = H)$	0.159	(0.009)
$\Pr(c_1 = L, c_2 = L)$	0.320	-

Note: Standard errors in parentheses computed using bootstrap. Panel (a) reports logit coefficients for grade progression probabilities. Baseline parameters capture progression probabilities for low-ability students, while high ability bonus parameters capture the additional progression probability for high-ability students. Higher values indicate higher progression probabilities. Panel (b) reports the estimated distribution of unobserved household types, where c_1 and c_2 denote the ability endowments of the first and second child, respectively. H indicates high ability and L indicates low ability. The probability for type (L,L) is computed as a residual to ensure probabilities sum to one, hence no standard error is reported.

The structural model reproduces the key regression moments from the experiment. Table 5 compares simulated regression coefficients with the experimental estimates. For both direct and indirect effects, the simulated coefficients lie within the 95% confidence intervals of the OLS estimates. The model also matches the control means for high attendance, low attendance or staying at home, work, and college graduation.

The model also tracks the age profile of school and work choices in the experimental control group. Figure 3 compares observed and simulated choices for high attendance; low attendance or staying at home; and work across ages in 2006. Overall, the model closely matches these experimental moments.

Furthermore, the model captures key allocation choices, especially labor market dynamics of observational (non-experimental) data. Figure 4 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.

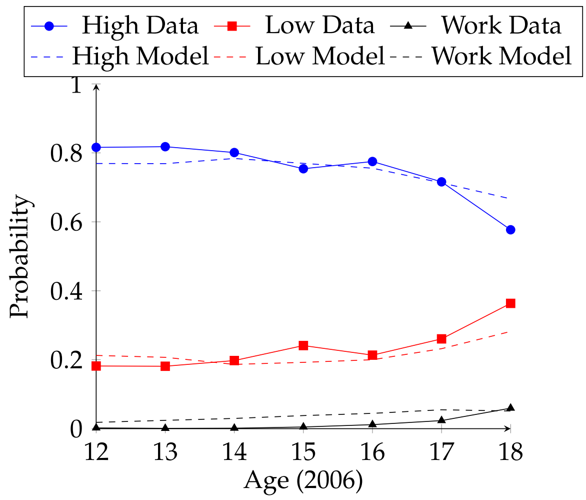
Finally, the model captures the effects observed in the validation group well. Table 5 reports the out-of-sample validation results, focusing on households with two winner

Table 5: Model Fit: Regression Moments

	High Attendance		Low or Stay Home		Work		College Graduation	
	Data	Model	Data	Model	Data	Model	Data	Model
Direct	0.074**	0.076	-0.055	-0.073	-0.022	-0.003	-0.024	-0.016
	[0.001,0.147]		[-0.124,0.014]		[-0.049,0.005]		[-0.057,0.009]	
Indirect	-0.016	0.000	0.007	0.001	0.020	-0.001	-0.037**	-0.037
	[-0.090,0.058]		[-0.066,0.080]		[-0.011,0.051]		[-0.070,-0.004]	
Control Mean	0.633	0.708	0.272	0.250	0.042	0.042	0.121	0.118

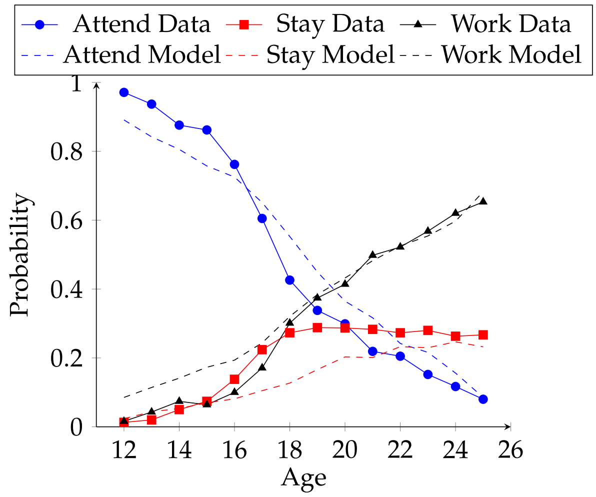
Notes: This table compares experimental estimates with structural model predictions for the direct and indirect effects of conditional cash transfers. Direct effects measure impacts on treated children whose siblings were not treated. Indirect effects (spillovers) measure impacts on untreated children whose siblings were treated. Data columns report experimental estimates with 95% confidence intervals in brackets. Model columns report simulated effects from the estimated structural model. High Attendance indicates attending school more than 80% of days. Low/Stay Home indicates either low attendance (<80%) or neither enrolled nor work. Work indicates the child's primary activity is working. College Graduation indicates completion of tertiary education.

Figure 3: Model Fit: Control Group Decision by Age in 2006



Notes: The figure compares observed data (solid lines with markers) to model predictions (dashed lines) of the control group in the experiment, for three mutually exclusive choices: high attendance, low attendance, and work. High attendance refers to attending school for more than 80% of the academic year, while low attendance refers to attending school but below this threshold. Work includes any form of labor market participation. Sample restricted to control group children aged 12-18 in 2006.

Figure 4: Model Fit: Observational Moments



Notes: The figure compares observed data (solid lines with markers) to model predictions (dashed lines) of students who are not in the experiment (hence "observational" samples), for three mutually exclusive activities: attend school, stay home, and work. Data come from the 2005 Encuesta Continua de Hogares (ECH) household survey for Bogotá. Sample restricted to individuals aged 12-25 from low-income households (below the 75th percentile of household income distribution). Attend includes any form of school enrollment. Stay home captures individuals who are neither in school nor working. Work includes any form of labor market participation.

siblings. The model replicates the strong treatment effect on increasing short-run high attendance and reducing low attendance for these two-winner households. Also, the model replicates the modest treatment effect on decreasing short-run work decision. Although the estimated effect on college graduation lies within the confidence intervals, it is neither economically nor statistically significant in this subsample.

Table 6: Validation Results

Parameter	**Data**	**Model**
(1) High Attendance	0.149***	0.108
	[0.078, 0.220]	
(2) Low Attendance or Stay Home	-0.103***	-0.090
	[-0.170, -0.036]	
(3) Work	-0.030**	-0.017
	[-0.055, -0.005]	
(4) College Grad	0.002	-0.023
	[-0.035, 0.039]	

Notes: This table presents out-of-sample validation results comparing experimental data with model predictions for households where both siblings won the lottery (Winner-Winner vs. Loser-Loser effects). The model was estimated using only the direct and indirect effects from households with one treated child, making these WWLL households a held-out validation sample. Data columns show point estimates with 95% confidence intervals in brackets. Model columns show simulated effects. High attendance indicates attending more than 80% of classes. Low attendance indicates attending less than 80% of classes or not studying/working. Work as Primary indicates the primary activity is working. College Grad indicates graduation from tertiary education.

8.4 Decomposition of the Spillover Effects

Using the estimated structural model, we decompose the negative spillover on the untreated sibling into a substitution component and an income component. The decomposition proceeds as follows. We simulate the behavior of households with one treated and one untreated sibling, but impose an income deduction chosen to keep the household's expected lifetime utility at the same level as in the no-treatment counterfactual. This deduction is computed household by household, ensuring that the value under the altered treatment scenario matches the value in the absence of treatment.

The resulting counterfactual isolates the substitution effect, defined as the change in behavior when utility is held constant at the no-treatment level. The income effect is then identified as the difference between the total observed spillover and this compensated outcome.

Table 7: Hicksian Decomposition of Spillover Effects

	Coefficient
Total Spillover Effect (Observed)	$\mathbf{-0.0371}$
Decomposition:	
Substitution Effect (Compensated)	-0.0424
Income Effect (Residual)	+0.0053

Notes: The table decomposes the estimated spillover on the untreated sibling's college graduation into substitution and income components using the structural model. The **Total Spillover** is the reduced-form spillover estimated under the observed assignment regime. To construct the **Substitution Effect**, we implement a Hicksian compensation scheme at the household level: for each Lose–Win (LW) and Win–Lose (WL) household, we choose a household-specific income penalty so that the household's value in 2005 under its observed (LW/WL) assignment equals the value in 2005 under a Lose–Lose (LL) assignment, evaluated at the same initial state. The value is held constant by withholding a constant component of parental income in each period (i.e., a flow reduction in parental income that is anticipated by the household and therefore enters both current utility and continuation values). The **Income Effect** is defined as the residual difference between the total spillover and the substitution effect.

The substitution channel mainly explains the observed negative spillovers. Specifically, the compensated simulation indicates that the shift in relative incentives alone generates a 4.24 percentage point decline in the untreated sibling's probability of college graduation. This magnitude exceeds the total observed spillover, while income effects are positive but negligible in size (+0.53 pp). Thus, the negative spillover is primarily driven by a substitution of investment that prioritizes the treated child at the expense of the untreated sibling's education.

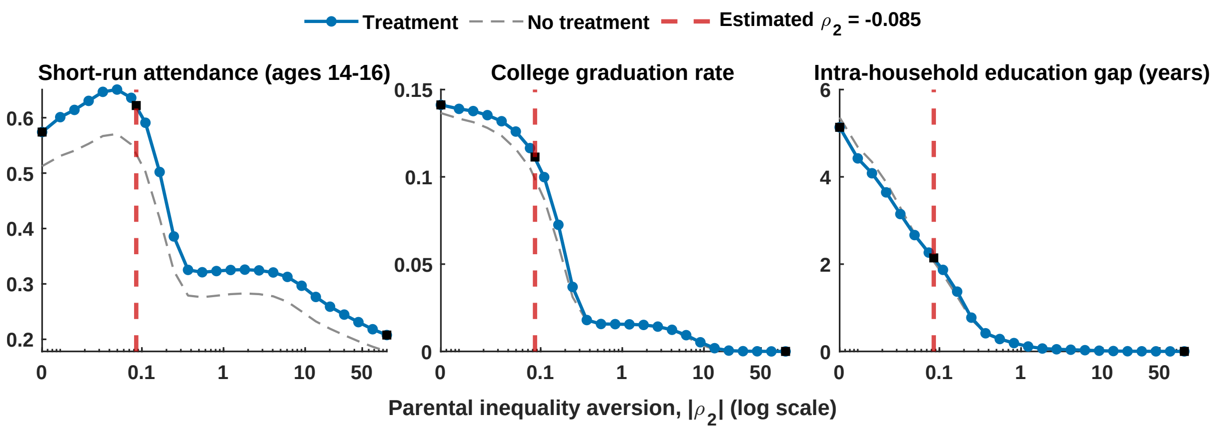
9 Counterfactual Policy Experiment

We use the estimated structural model to conduct counterfactual experiments that vary transfer generosity, payment timing, and parental inequality aversion, isolating how these interact to shape educational outcomes. Importantly, the estimated model allows us to simulate policy effects that correct for endogenous recruitment into the experiment (Galiani et al., 2022). In the original RCT, eligibility required students to already be enrolled at the time of recruitment, meaning that households whose children would have dropped out before recruitment age were excluded. Our simulated policies instead provide transfers to all eligible low-income households with secondary school-aged children throughout the entire duration of secondary school.

Policy Design and Parental Aversion: We study a single transfer design, which we call the *treatment*: both siblings automatically receive an annual scholarship of 300,000 COP conditional on maintaining high attendance ($> 80\%$) during secondary school, plus a 600,000 COP bonus upon graduation. Students lose their recipient status for the remainder of secondary school if they choose low attendance, stay home, or enter the workforce.

To isolate the role of parental preferences, we vary inequality aversion continuously, from no aversion to a strong-aversion limit, and trace how the policy's effects move along that range (Figures 5 and 6). Three cases anchor the discussion. The estimated value ($\rho_2 = -0.085$) represents the degree of inequality aversion needed for the model to match the observed educational disparities across siblings. Shutting aversion down ($\rho_2 = 0$) describes parents who care only about maximizing total achievement, with no concern for equity between siblings. The strong-aversion limit approximates the opposite extreme, in which parents almost fully equalize education across children. Comparing across this range shows how aversion governs both the average educational response to transfers and the resulting intra-household inequality.

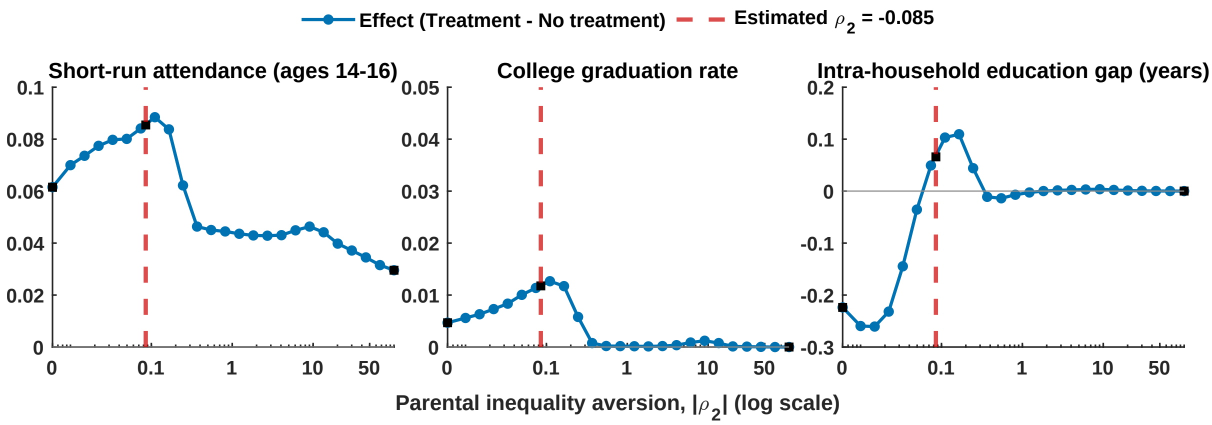
Figure 5: Outcome levels under the treatment by parental inequality aversion



Notes: Short-run attendance (ages 14–16), college graduation, and the intra-household education gap under the treatment (solid) and no treatment (dashed). The horizontal axis is the *absolute value* of the inequality-aversion parameter, $|\rho_2|$, on a log scale, so aversion strengthens from left to right and $\rho_2 = 0$ (no aversion) sits at the left edge. The vertical line marks the estimated value ($\rho_2 = -0.085$); squares mark the no-, estimated-, and strong-aversion illustrative cases.

Policy Effects: The difference between our simulated policy effects and the experimental estimates highlights the importance of accounting for endogenous recruitment. Since the treatment provides transfers to both siblings, the WWLL experimental group, in which both siblings won the lottery, offers the most direct empirical comparison. Our structural model estimates that the policy effect on short-run attendance is 7.0 percentage points, approximately half the 14.9 percentage point effect observed in the

Figure 6: Treatment effects by parental inequality aversion



Notes: Effect of the treatment (Treatment minus No Treatment) on the same three outcomes. The horizontal axis is the *absolute value* of ρ_2 on a log scale ($\rho_2 = 0$, no aversion, at the left edge; larger $|\rho_2|$ means stronger aversion). The vertical line marks the estimated $\rho_2 = -0.085$. The effect on attendance and college graduation is largest near the estimated level of aversion and smaller when parents either barely equalize or fully equalize.

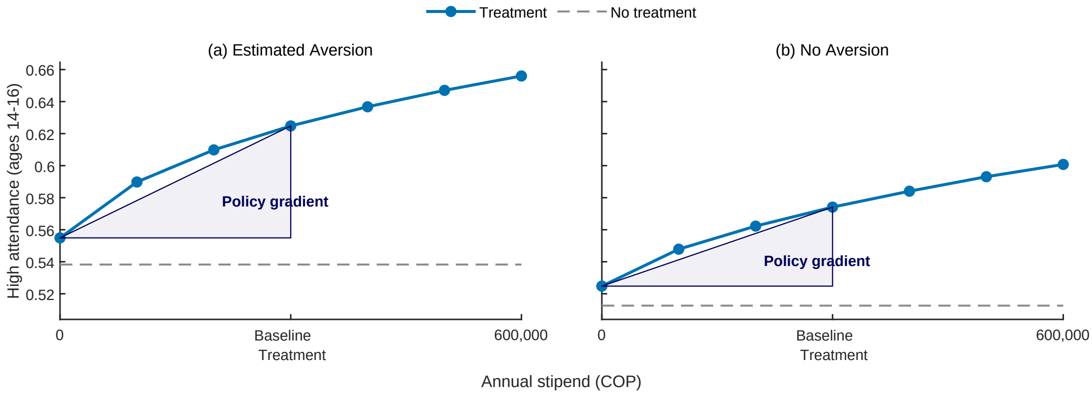
WWLL experimental group. This difference reflects positive selection in the RCT: the WWLL group consists of households both of whose children remained enrolled long enough to be recruited, representing families with stronger preferences for education. Our policy simulation instead covers the full distribution of household types, including those who would have dropped out before recruitment age. While the RCT found a statistically insignificant 0.2 percentage point increase in college graduation, our corrected estimates show a 1 percentage point increase.

The Role of Parental Aversion: Figure 5 shows how parental inequality aversion affects the allocation of investment within the household. Under the treatment, weakening aversion leads parents to specialize: relative to the estimated case, shutting aversion down widens the intra-household education gap from 2.13 to 5.14 years and raises college completion from 0.11 to 0.14. This reflects an efficiency–equity tradeoff of parents: concentrating resources on the higher-return child raises aggregate educational attainment but widens disparities between siblings, whereas inequality-averse parents forgo some total achievement in exchange for a more equal distribution of education across their children.

At the opposite extreme, strong aversion drives the intra-household gap toward zero as parents almost fully equalize schooling across siblings. Aversion also governs how responsive households are to transfers: households that already specialize in one child's education respond little at the margin, because they are already directing resources efficiently toward the higher-return child, so additional transfers do little to change their investment.

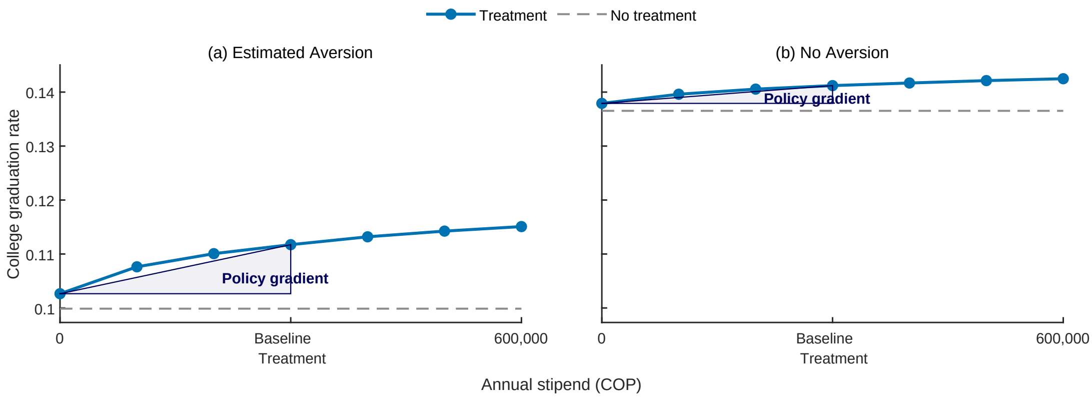
The response to the transfer is non-monotonic in parental inequality aversion (Figure 6). In the simulations, effects are smaller when parents either specialize strongly across children or nearly equalize schooling regardless of incentives. The estimated value of ρ_2 lies in a range where households remain responsive to changes in transfer incentives. Figures 7 and 8 make the estimated-versus-no-aversion contrast explicit: with estimated aversion, raising the annual stipend from zero to 600,000 COP increases short-run attendance by 10 percentage points and college graduation by 1.3 percentage points. With aversion shut down, these gradients fall to 6 percentage points and less than 0.01 percentage points.

Figure 7: Policy Experiment Results: Short-run Attendance



Notes: Each panel plots the average high-attendance rate (ages 14–16) under the treatment—both siblings receive the annual scholarship and a 600,000 COP graduation bonus—as the annual scholarship varies from 0 to 600,000 COP (horizontal axis, in COP; the graduation bonus is held fixed at 600,000 COP). The left panel uses the estimated inequality-aversion parameter ($\rho_2 = -0.085$); the right panel shuts aversion down ($\rho_2 = 0$); the two panels share a common vertical axis. In each panel the solid line is the treatment response and the dashed horizontal line marks the no-treatment level, and the shaded wedge marks the policy gradient over the range from no scholarship to the baseline amount (300,000 COP). High attendance is defined as attending school more than 80% of the academic year.

Figure 8: Policy Experiment Results: College Graduation



Notes: Each panel plots the college graduation rate under the treatment—both siblings receive the annual scholarship and a 600,000 COP graduation bonus—as the annual scholarship varies from 0 to 600,000 COP (horizontal axis, in COP; the graduation bonus is held fixed at 600,000 COP). The left panel uses the estimated inequality-aversion parameter ($\rho_2 = -0.085$); the right panel shuts aversion down ($\rho_2 = 0$); the two panels share a common vertical axis. In each panel the solid line is the treatment response and the dashed horizontal line marks the no-treatment level, and the shaded wedge marks the policy gradient over the range from no scholarship to the baseline amount (300,000 COP). College graduation is measured as completing tertiary education by age 25.

Policy Design for Improvements: We investigate which combination of annual stipend and graduation bonus is most cost-effective (Attanasio, Meghir, and Santiago, 2012). Table 8 presents the outcomes under six policy scenarios with varying combinations of annual stipends (0 to 600,000) and graduation bonuses (0 to 1,000,000). The scenarios include the baseline design, policies that maximize short-run attendance or college graduation, and cost-minimizing alternatives that achieve comparable education gains.

The results reveal that reallocating resources from annual stipends to graduation bonuses improves long-term educational outcomes while reducing program costs. The 'COL, Min Cost' policy reduces annual stipends from 300,000 to 200,000 COP while increasing the graduation bonus from 600,000 to 1,000,000 COP. This reallocation maintains college graduation rates near baseline levels (0.12 vs. 0.11) while reducing total program costs by 35.6%. The maximum impact policy, which combines 340,000 COP stipends with 1,000,000 COP bonuses, increases college graduation by 0.01 but requires 18% higher costs than baseline. These patterns indicate that graduation bonuses generate higher returns per peso invested in terms of college completion, while annual stipends primarily affect short-run attendance.

Table 8: Alternative Policy Analysis

Policy	Stipend	Bonus	COL	SR Attend	IntraHH In.	Rel. Cost
No Treatment	0	0	0.10	0.57	2.07	0.00
Baseline	300000	600000	0.11	0.66	2.14	1.00
Max SA	340000	1000000	0.11	0.67	2.14	1.18
Max COL	340000	1000000	0.11	0.67	2.14	1.18
SR, Min Cost	270000	900000	0.11	0.66	2.13	0.90
COL, Min Cost	200000	1000000	0.11	0.64	2.13	0.64

Notes: This table presents alternative policy configurations under different objectives. "Max SA" and "Max COL" maximize short-run attendance and college graduation respectively, regardless of cost. "SR, Min Cost" achieves baseline short-run attendance at minimum cost. "COL, Min Cost" achieves baseline college graduation at minimum cost. All amounts in Colombian Pesos (COP). SR = Short-run High Attendance (>80% attendance rate); COL = College Graduation rate; IntraHH In. = Intra-Household Education Gap (years); Rel. Cost = Cost relative to baseline policy (Baseline = 1.000).

10 Conclusions

Parents may respond to differences across children by compensating the disadvantaged child or by reinforcing the child with higher expected returns. Distinguishing between these responses is empirically difficult because parental investments across siblings are endogenous to unobserved child characteristics, household constraints, and expectations about future returns. This paper uses a conditional cash transfer experiment in Bogotá, Colombia, that randomized transfers at the student level to generate exogenous variation in resources and incentives across siblings within the same household.

We use this variation to estimate direct effects on treated students, indirect effects on untreated siblings, and the effects of treating both registered siblings. The pooled treatment increases high attendance in the short run and reduces school disengagement and work as the primary activity. The long-run direct effects on treated students are small and imprecisely estimated. The main reduced-form result is the negative spillover on untreated siblings: untreated siblings of treated students are 3.7 percentage points less likely to graduate from college, a decline of about 30 percent relative to the control mean. When both registered siblings are treated, the intervention generates large short-run effects but small long-run effects. Together, these findings indicate that child-level transfers can reallocate educational investments within the household, rather than simply raising investments in the treated child.

To interpret these effects, we develop, estimate, and validate a dynamic model of household schooling decisions across siblings. The model identifies parental aversion to inequality in children's educational outcomes, a key parameter in models of parental investment. The estimate implies limited aversion to inequality across siblings. The model also reproduces the held-out win-win versus lose-lose effects, which are not used in estimation. This validation exercise supports the use of the model to study the mechanisms behind the reduced-form spillover.

The mechanism is substitution in educational investments across children. In the model, changing the treated child's relative schooling incentive induces households to shift investments toward that child. A compensated decomposition shows that this substitution channel accounts for the negative spillover on the untreated sibling's college graduation. The residual income effect is positive but small. The negative spillover reflects a reallocation of investment toward the treated child.

The counterfactual exercises show that the strength of parental inequality aversion affects how a transfer to both siblings affects attendance, college graduation, and within-household education gaps. The simulations also show that the timing of payments matters: shifting resources from annual stipends toward graduation bonuses can maintain long-run college graduation effects at lower fiscal cost in our simulations. These results imply that the effects of child-level transfers depend not only on their direct incentives, but also on how households reallocate investments across children.

The broader implication is that program evaluations that focus only on directly treated children may miss important within-household responses. When interventions change incentives for one child, parents may adjust investments in other children. These spillovers can offset part of the direct gains and change the distribution of outcomes within the family. More broadly, evaluations of child-targeted policies may overstate social gains when within-household reallocations are ignored.

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A Sample Construction

This appendix provides additional detail on the construction of our main estimation sample. Section A.1 describes how we merge the different data sources. Section A.2 documents the sample restrictions that take us from the universe of registered students to the sample used in the main analysis.

A.1 Merge between Experimental Data and Long-Term Outcomes

We combine the experimental data used in Barrera-Osorio et al. (2011), described in Sections 4 and 4.1, with the long-run administrative data described in Section 4.2. We use the 2004 SISBEN census of low-income households to link experimental participants and their household members to the long-run records.

The merge between the experimental data and SISBEN is highly successful: we match 99.31% of the experimental sample. This high match rate reflects that participating households were required to report a SISBEN score at registration. After merging the two data sets, we recover first names, two last names, and exact dates of birth for all household members. We also recover unique national identification numbers for all individuals aged 18 or older, so there is no attrition among adult household members.

National identification numbers are not directly available for household members younger than 18. Instead, we recover temporary identification numbers for minors and use them to construct a crosswalk to adult national identification numbers. The crosswalk relies on household location, names, dates of birth, and pre-existing administrative crosswalks. This procedure allows us to recover adult national identification numbers for most program beneficiaries and their siblings. Specifically, we recover adult national identifiers for 95.5% of individuals living in the household at baseline. Of these, 35.8% were age-eligible for the program (born between 1986 and 1994), and 76.5% of the age-eligible were registered beneficiaries.

We then use national identification numbers, names, and dates of birth to link the experimental data to long-run outcomes from the high school exit exam records (Saber 11) and the college administrative records (SPADIES).

A.2 Sample Selection

The experimental data contain 17,309 students who registered for the lottery. We impose three sequential restrictions to construct the main estimation sample. Appendix Table A.1 reports this selection process. First, we exclude the 84 students who were already out of school at the time of recruitment, along with their households. Second, we restrict the sample to households with exactly two registered children. Third, we restrict the sample to households in Suba. After these restrictions, the main estimation sample consists of 2,328 program participants and siblings from households with exactly two registered children in Suba.

Table A.1: Sample Sizes After Restrictions

	N
Initial sample	17,309
Continued to be enrolled	17,225
Keep households with 2 registered children	6,364
Keep Suba only	2,328

Notes: The table reports the number of students remaining at each step of the sample-restriction process. The first restriction drops 84 students who were not enrolled in school at the time of recruitment and their households. The second restriction keeps only households that registered exactly two children for the lottery. The third restriction keeps only households in the Suba locality.

Table A.2: Number of Households by Number of Registered Students and Treatment Status

	None Treated	Some Treated	All Treated	Total
1 registered student	4023	0	5120	9143
2 registered students	618	1473	970	3061
3 registered students	36	374	110	520
4 registered students	1	57	2	60
5 registered students	1	5	1	7
Total	4679	1910	6203	12792

Notes: This table reports the number of households in our sample according to the number of students that were registered in the program and their treatment status.

B Reduced-Form Robustness

This appendix presents robustness exercises that relate our reduced-form results to previously published estimates from the same experiment. We proceed in three steps. Section B.1 replicates the headline specification from Barrera-Osorio, Linden, and Saavedra (2019) and bridges that specification to ours through a sequence of intermediate steps. Section B.2 reports three alternative samples that relax the Suba-only and two-registered-children restrictions. Section B.3 reports specification curves that summarize the sensitivity of the main estimates to alternative sets of control variables and sample definitions.

B.1 From Prior Estimates to Our Main Specification

We show how our main reduced-form specification relates to the headline specification in Barrera-Osorio, Linden, and Saavedra (2019), which estimates the long-run effects of the same intervention. The two specifications differ along four dimensions: the sample (the universe of registered students in grades 9–11 across both localities versus two-registered-children households in Suba), the treatment arms (Basic, Savings, and College entered separately versus Basic and College pooled into a single indicator), the set of control variables, and the treatment of standard errors. We isolate the contribution of each choice in turn.

B.1.1 Replication of the Prior Specification

Panel A of Table B.1 reproduces the main long-run specification in Barrera-Osorio, Linden, and Saavedra (2019) on our data. We restrict the sample to students in grades 9–11 in both localities, exclude students who were already out of school at recruitment, and estimate

$$Y_{is} = \beta_0 + \beta_1 T^B is + \beta_2 T^S is + \beta_3 T^C is + \beta_4 Xis + \phi_s + \varepsilon_{is},$$

where $T^B is$, $T^S is$, and $T^C is$ are indicators for the basic, savings, and college treatment arms, Xis is the vector of controls used in the original paper, and ϕ_s are school fixed effects. Standard errors are clustered at the school level. As in the original paper, the college arm increases on-time college enrollment by a precisely estimated 4.9 percentage points, while the basic and savings arms have small and not statistically distinguishable from zero. We also recover the short-run pattern documented in Barrera-Osorio, Bertrand, Linden, and Perez-Calle (2011): the college arm increases high attendance and reduces low attendance and work, while the basic and savings arms have smaller short-run effects.

B.1.2 From the Prior Specification to Our Main Results

Panels B–F of Table B.1 introduce, one at a time, the four modifications that take us from the Barrera-Osorio, Linden, and Saavedra (2019) specification to the main specification used in the paper.

Panel B pools the three treatment arms into a single indicator. In the same sample used in Barrera-Osorio et al. (2019), the pooled treatment increases on-time college enrollment by a precisely estimated 2.8 percentage points. This is smaller than the

Table B.1: Reduced Form Results from Prior Estimates to Our Main Specification

	Short Run			Long Run	
	School Attendance (High)	Low School Attendance or Neither Studying nor Working	Works as Primary Activity	On-Time College Enrollment	College Graduation
	(1)	(2)	(3)	(4)	(5)
A) Estimates in Barrera-Osorio et al. (2019)					
Basic	0.022	-0.013	-0.014	0.013	0.008
	(0.020)	(0.019)	(0.012)	(0.013)	(0.011)
Savings	0.021	-0.004	-0.009	0.018	0.015
	(0.020)	(0.020)	(0.012)	(0.013)	(0.012)
College	0.142***	-0.092***	-0.050***	0.049***	0.015
	(0.025)	(0.024)	(0.014)	(0.017)	(0.014)
Observations	3382	3248	3382	6872	6866
Control mean	0.626	0.262	0.0782	0.178	0.127
B) Pooling Treatments					
Treated	0.064***	-0.037***	-0.025***	0.028***	0.013
	(0.014)	(0.014)	(0.008)	(0.009)	(0.008)
Observations	3382	3248	3382	6872	6866
Control mean	0.626	0.262	0.0782	0.178	0.127
C) All Grade Groups					
Treated	0.042***	-0.032***	-0.013***	0.007	0.004
	(0.009)	(0.008)	(0.004)	(0.006)	(0.004)
Observations	8650	8239	8650	17224	17214
Control mean	0.692	0.230	0.0382	0.180	0.0858
D) Suba Only					
Treated	0.072***	-0.055***	-0.021***	0.017*	0.005
	(0.015)	(0.013)	(0.006)	(0.010)	(0.008)
Observations	3040	2834	3040	6317	6313
Control mean	0.633	0.275	0.0419	0.217	0.103
E) Our Set of Controls					
Treated	0.076***	-0.054***	-0.022***	0.018*	0.006
	(0.017)	(0.016)	(0.006)	(0.010)	(0.008)
Observations	3040	2834	3040	6317	6313
Control mean	0.633	0.275	0.0419	0.217	0.103
F) Only Two Students per HH					
Treated	0.116***	-0.080***	-0.027***	0.010	0.004
	(0.027)	(0.026)	(0.010)	(0.017)	(0.013)
Observations	1162	1082	1162	2327	2326
Control mean	0.623	0.277	0.0434	0.210	0.104

Notes: The table reports the coefficient on the treatment indicator across six progressively modified specifications. Panel A reproduces the long-run specification of Barrera-Osorio, Linden, and Saavedra (2019) on our data. Panel B pools the three treatment arms into a single indicator. Panel C drops the restriction to grades 9–11 and uses the full universe of registered students across both localities. Panel D further restricts the sample to Suba. Panel E replaces the original set of control variables with ours. Panel F further restricts the sample to households with exactly two registered children, recovering the main specification reported in the paper. Outcomes are defined as in the main text. Robust standard errors are reported in Panels C–F; standard errors in Panels A and B are clustered at the school level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

college-only estimate of 4.9 percentage points, consistent with the modest long-run effects of the basic and savings arms when entered separately. Panel C expands the sample to all registered students across both localities and grade groups. The short-run effects on attendance, low attendance, and work remain precisely estimated, but the college-enrollment effect attenuates. Panel D then restricts the sample to Suba, yielding a long-run college-enrollment effect of 1.7 percentage points that is only marginally significant.

Panel E replaces the control variables in Barrera-Osorio, Linden, and Saavedra (2019) with the more parsimonious set of controls used in the paper: a grade-group dummy, age, gender, sibling age, and sibling gender. The point estimates are essentially unchanged, indicating that the choice of control variables contributes little to the difference across specifications. Panel F adds the final restriction to households with exactly two registered children, recovering the main estimation sample of 2,328 students. Relative to Panel E, the short-run effects on attendance, low attendance, and work increase substantially in magnitude. However, the long-run point estimates decrease but still yield a positive point estimates of 0.01 on college enrollment. These estimates correspond to the total effect reported in Table 2.

The exercise establishes two facts. First, the headline findings of Barrera-Osorio, Linden, and Saavedra (2019)—a positive short-run effect of CCTs on attendance and a positive long-run effect of the college arm on on-time college enrollment—are robust to pooling, sample changes, and the choice of control variables. Second, the larger short-run estimates reported in the paper are not driven by cherry-picked sample restrictions. They reflect a stronger treatment response among households that registered two children for the lottery, which is precisely the sample for which the direct, indirect, and win-win versus lose-lose decomposition is informative.

B.2 Alternative Estimation Samples

Our main design restricts the sample to households in Suba with exactly two registered children. The rationale for this design becomes clear once we examine where the treatment effects come from. The replication in Section B.1 shows that the college treatment produces a precisely estimated 14.2 percentage-point increase in high attendance, corresponding reductions in low attendance and work in the short run, and a 4.9 percentage-point increase in on-time college enrollment in the long run. By contrast, the basic treatment yields imprecise estimates across both short- and long-run outcomes. Restricting the analysis to Suba therefore concentrates statistical power on the treatment variation that moves outcomes and avoids diluting the estimates with cells in which treatment effects are weak. A second advantage of the Suba restriction is that it removes the savings arm from the analysis, leaving a clean basic-or-college-versus-control comparison.

To assess the robustness of our estimates, we report three alternative versions of the main results that relax the most restrictive features of the analysis sample.

B.2.1 Effects Using a Common Sample Across Outcomes

Because we combine multiple data sources, the estimates in Table 2 are based on outcome-specific samples. The results are not driven by these sample differences. To assess this, we re-estimate Table 2 on a common sample across all outcomes and report the results in Appendix Table B.2. The main pattern is unchanged. The only notable difference is that the indirect effect on long-run college graduation is no longer precisely estimated. Its sign and magnitude, however, remain similar: the estimate is -0.022 in the common sample, compared with -0.037 in Table 2. This loss of precision is expected, since the common sample for that outcome is about one-third the size of the corresponding sample in Table 2.

Table B.2: Reduced Form Estimates using a Common Sample Across Outcomes

	Short Run			Long Run	
	School Attendance (High)	Low School Attendance or Neither Studying nor Working	Works as Primary Activity	On-Time College Enrollment	College Graduation
	(1)	(2)	(3)	(4)	(5)
A) Total Effects					
Treated	0.104***	-0.076**	-0.029**	-0.018	0.004
	(0.031)	(0.030)	(0.013)	(0.029)	(0.022)
Observations	784	784	784	784	784
Control mean	0.682	0.269	0.0491	0.231	0.107
B) Direct Effects					
Treated	0.042	-0.023	-0.020	-0.001	-0.000
	(0.043)	(0.041)	(0.017)	(0.039)	(0.031)
Observations	428	428	428	428	428
Control mean	0.713	0.242	0.0458	0.217	0.117
C) Indirect Effects					
Treated	-0.068	0.061	0.007	0.033	-0.022
	(0.045)	(0.043)	(0.021)	(0.040)	(0.030)
Observations	428	428	428	428	428
Control mean	0.713	0.242	0.0458	0.217	0.117
D) WWvLL Effects					
Treated	0.116***	-0.084**	-0.032*	-0.008	-0.011
	(0.041)	(0.039)	(0.017)	(0.040)	(0.031)
Observations	408	408	408	408	408
Control mean	0.713	0.242	0.0458	0.217	0.117

Notes: This table reports the four-way decomposition of treatment effects using a common sample across all five outcomes. Panel A reports total effects estimated from equation 1, comparing lottery winners with lottery losers regardless of their siblings' treatment status. Panel B reports direct effects estimated from equation 2, comparing lottery winners whose sibling lost with lottery losers whose sibling also lost. Panel C reports indirect, or sibling spillover, effects estimated from equation 3, comparing lottery losers whose sibling won with lottery losers whose sibling also lost. Panel D reports win-win versus lose-lose effects estimated from equation 4, comparing lottery winners whose sibling also won with lottery losers whose sibling also lost. In all panels, the sample consists of students in households with two registered children in Suba who are observed for all five outcomes. All specifications control for an indicator for grades 6–8, gender, age, sibling's gender, and sibling's age. The outcome in column (1) equals 1 if the student attends more than 80% of classes. The outcome in column (2) equals 1 if the student attends 80% or fewer classes, or neither studies nor works. The outcome in column (3) equals 1 if the student's main activity is work. The outcome in column (4) equals 1 if the student appears in the high school exit-exam records and graduates from high school at the expected age. The outcome in column (5) equals 1 if the student appears in the college records and enrolls in college at the expected age. The outcome in column (6) equals 1 if the student graduates from college. Heteroskedasticity-consistent standard errors are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

B.2.2 Total Effects in the Universe of Registered Students

Table B.3 reports the total effect of the pooled basic and college arms in the universe of registered students, across both localities and all family sizes. The point estimates are smaller than the corresponding estimates in Table 2, but the qualitative pattern is the same: a precisely estimated 4.2 percentage-point increase in high attendance, a 3.0 percentage-point reduction in low attendance, and a 1.3 percentage-point reduction in work, with no statistically distinguishable long-run effects on on-time college enrollment, or on college graduation. The attenuation relative to our main sample is consistent with the bridge exercise in Table B.1 and reflects that households with two registered children respond more strongly to treatment, on average, than the full universe of registered households.

Table B.3: Total Effects: Universe of Registered Students, Both Localities, All Family Sizes

	Short Run			Long Run	
	School Attendance (High)	Low School Attendance or Neither Studying nor Working	Works as Primary Activity	On-Time College Enrollment	College Graduation
	(1)	(2)	(3)	(4)	(5)
Treated	0.042***	-0.030***	-0.013***	0.009	0.004
	(0.010)	(0.009)	(0.004)	(0.006)	(0.004)
Observations	8650	8239	8650	17224	17214
Control mean	0.692	0.230	0.0382	0.180	0.0858

Notes: The table reports the total effect on the five main outcomes, using the universe of registered students across both localities (San Cristóbal and Suba) and all family sizes (one, two, three, four, or five registered children per household). The specification controls for a grade-group dummy, age, gender, sibling age, and sibling gender. Heteroskedasticity-consistent standard errors are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

B.2.3 Suba plus San Cristóbal, Grades 9–11

Table B.4 reports the four-way decomposition—total, direct, indirect, and win-win versus lose-lose—for an alternative sample that augments our main Suba sample with students in grades 9–11 from San Cristóbal. This exercise is informative because it relaxes the Suba-only restriction in the most plausible direction. In San Cristóbal grades 9–11, students were randomized between control, basic, and savings; restricting that sample to the basic and control cells yields a clean comparison that can be pooled with the Suba sample under a common "basic or college" indicator. The sample remains restricted to households with exactly two registered children. The qualitative pattern mirrors the one in Table 2: a positive direct effect on short-run high attendance, a negative direct effect on work, and, most importantly, a negative indirect effect on long-run high-school and college graduation. The spillover effect on college graduation is -3.2 percentage points, precisely estimated and economically close to the -3.7 percentage-point spillover reported in the main text.

B.3 Specification Curves

Figure B.1 reports specification curves for the five main outcomes. For each outcome, we estimate the total effect of the pooled basic and college arms across a large set of

Table B.4: Total, Direct, Indirect, and WWvLL Effects: Suba and San Cristóbal 9–11

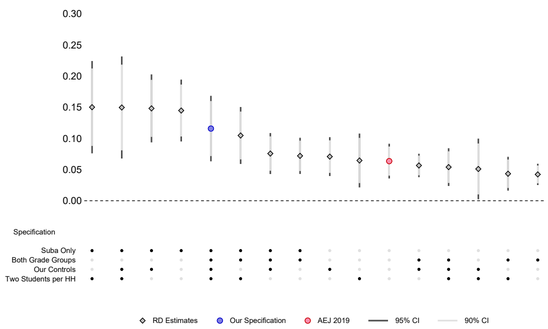
	Short Run			Long Run	
	School Attendance (High)	Low School Attendance or Neither Studying nor Working	Works as Primary Activity	On-Time College Enrollment	College Graduation
	(1)	(2)	(3)	(4)	(5)
A) Total Effects					
Treated	0.062***	-0.032*	-0.027***	0.018	0.006
	(0.020)	(0.019)	(0.009)	(0.012)	(0.010)
Observations	2016	1913	2016	3937	3935
Control mean	0.654	0.248	0.0544	0.184	0.101
B) Direct Effects					
Treated	0.030	-0.009	-0.026**	0.012	-0.022
	(0.029)	(0.027)	(0.013)	(0.018)	(0.014)
Observations	959	904	959	1897	1897
Control mean	0.672	0.234	0.0483	0.193	0.118
C) Indirect Effects					
Treated	-0.045	0.039	0.002	0.002	-0.032**
	(0.030)	(0.029)	(0.014)	(0.018)	(0.014)
Observations	956	900	956	1914	1913
Control mean	0.672	0.234	0.0483	0.193	0.118
D) WWvLL Effects					
Treated	0.056**	-0.022	-0.025**	0.021	0.002
	(0.028)	(0.026)	(0.013)	(0.018)	(0.015)
Observations	1053	1005	1053	2026	2025
Control mean	0.672	0.234	0.0483	0.193	0.118

Notes: The table reports the four-way decomposition of treatment effects for an alternative sample that augments the main Suba sample with students in grades 9–11 from San Cristóbal, restricted to households with exactly two registered children. Panel A reports the total effect of the pooled basic and college arms. Panel B reports direct effects on treated students with untreated siblings. Panel C reports indirect effects on untreated students with treated siblings. Panel D reports win-win versus lose-lose effects on treated students with treated siblings. The specification controls for a grade-group dummy, age, gender, sibling age, and sibling gender. Heteroskedasticity-consistent standard errors are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

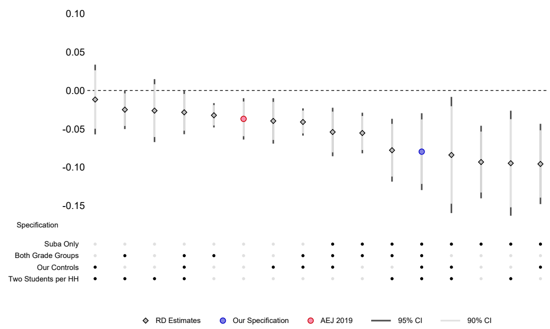
alternative specifications that vary the sample (Suba only versus both localities, two-registered-children households versus all households, and all grades versus grades 9–11 only), the set of control variables (none, our parsimonious set, the AEJ 2019 set, or school fixed effects), and the inclusion of stratum dummies. Each marker corresponds to a single specification, and the curves are sorted by the magnitude of the point estimate. The qualitative pattern is stable across specifications: short-run effects on high attendance are positive and significant in nearly every case, short-run effects on low attendance and work are negative and significant, and long-run effects on graduation, on-time college enrollment, and college graduation are small and rarely statistically distinguishable from zero in the total-effects specification. The dispersion of estimates is also informative. Short-run effects are highly stable across specifications, whereas long-run effects vary somewhat more, reflecting smaller cell sizes and the fact that the long-run sample is constructed from administrative records linked through SISBEN.

Figure B.1: Specification Curves: Total Effect

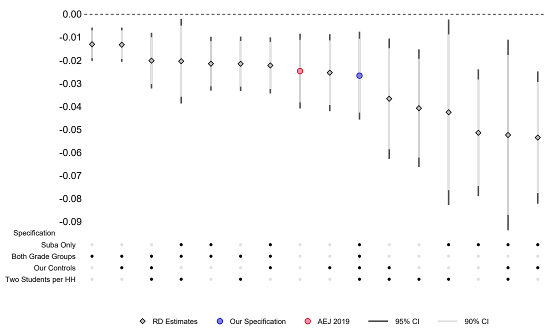
(a) High Attendance



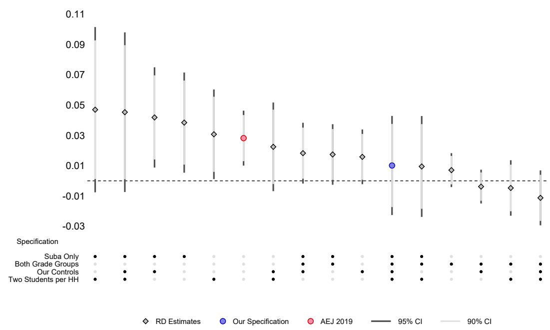
(b) Low Attendance



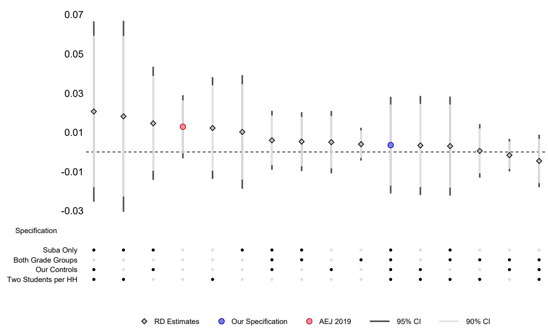
(c) Works as Primary Activity



(d) On-Time College Enrollment



(e) College Graduation



Notes: Each panel reports a specification curve for the total effect on one of the five main outcomes. Each marker corresponds to a single regression specification, with the horizontal axis indexing specifications and the vertical axis reporting the point estimate of the pooled-treatment coefficient. Specifications vary along three dimensions: the analysis sample (Suba only versus both localities; two-registered-children households versus all households; and all grades versus grades 9–11 only), the set of control variables (none, our parsimonious set of demographic controls, the Barrera-Osorio, Linden, and Saavedra (2019) set of controls, or school fixed effects), and the inclusion of stratum dummies. Markers are sorted by the magnitude of the point estimate. Vertical bars report 90 percent confidence intervals based on heteroskedasticity-consistent standard errors, or on standard errors clustered at the school level in specifications that include school fixed effects.

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

$$\begin{aligned} V_d(\Omega_t, \varepsilon_t, \xi) = U(c_t, d, \Omega_a, \xi, \varepsilon_t^u) \\ + V_{\bar{t}_h}(e_{1,h}, e_{2,h}, ..., e_{N_h,h}; \rho) \end{aligned} \tag{8}$$

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

$$V_d(\Omega_t, \varepsilon_t, \xi) = U(c_t, d, \Omega_t, \xi, \varepsilon_t^u) + \delta E_{\Omega_{t+1}} \left[V(\Omega_{t+1}, \xi) | d, \Omega_t, \xi \right] \tag{9}$$

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

$$E_{\Omega_{t+1}}[V(\Omega_{t+1}, \xi)|d, \Omega_t, \xi] = \sum_{\Omega_{t+1}} \Pr(\Omega_{t+1}|d, \Omega_a, \xi) \times \{V(\Omega_{t+1}, \xi)\} \tag{10}$$

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

$$V(\Omega_{t+1}, \xi) = E_\varepsilon \left[\max_j \{V_j(\Omega_{t+1}, \varepsilon_{t+1}, \xi)\} \right] \tag{11}$$

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

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

D 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

   Flow utility of the household is defined as

   $$U = \ln(c_p) + \sum_{k=1}^{2} U_k + \alpha_{both} d_{c_1,high} d_{c_2,high}$$

   where U_i is the flow utility that household h derives from child i

   $$\begin{aligned} U_i = &(\alpha_{high} + \varepsilon_i^{high}) d_{i,high} \\ &+ (\alpha_{low} + \varepsilon_i^{low}) d_{i,low} \\ &+ (\alpha_{work} + \varepsilon_i^{work}) d_{i,work} \\ &+ \alpha_{reentry}(d_{i,work}^{-1} \text{ or } d_{i,stay}^{-1}) \times (d_{i,high} \text{ or } d_{i,low}) \end{aligned}$$

   Following the model definition, it can be also written as

   $$U = \ln(c_p) + \sum_{k=1}^{2} U_k + \alpha_{both}(s_{1,h,t} = 1, q_{1,h,t} = 1, w_{1,h,t} = 0)(s_{2,h,t} = 1, q_{2,h,t} = 1, w_{2,h,t} = 0)$$

   where U_k is the flow utility from child k

   $$\begin{aligned} U_{k^{th}} = &(\alpha_{high} + \varepsilon_{c_k}^{high})(s_{i,h,t} = 1, q_{i,h,t} = 1, w_{i,h,t} = 0) \\ &+ (\alpha_{low} + \varepsilon_{c_k}^{low})(s_{i,h,t} = 1, q_{i,h,t} = 0, w_{i,h,t} = 0) \\ &+ (\alpha_{work} + \varepsilon_{c_k}^{work})(s_{i,h,t} = 0, q_{i,h,t} = 0, w_{i,h,t} = 1) \\ &+ \alpha_{reentry}\Big\{ \underbrace{((s_{i,h,t-1} = 0, q_{i,h,t-1} = 0, w_{i,h,t-1} = 1) \cup (s_{i,h,t-1} = 1, q_{i,h,t-1} = 1, w_{i,h,t-1} = 0))}_{\text{No attendance in the previous period}} \\ &\times \underbrace{((s_{i,h,t} = 1, q_{i,h,t} = 1, w_{i,h,t} = 0) \cup (s_{i,h,t} = 1, q_{i,h,t} = 0, w_{i,h,t} = 0))}_{\text{Attendance in the current period}} \Big\} \end{aligned}$$

   We allow for different re-entry costs for secondary school and college

2. Terminal Value

- We specify the terminal value as function of the mean and variance of Completed Schooling as Rosenzweig and Wolpin (1988)[23]

$$V_{\bar{t}_h}(\Omega_{\bar{t}_h}) = \rho_1 \sum_i \mathbb{I}(e_{\bar{t}_h,i} \geq 16) \; + \; \rho_2 \operatorname{Var}(e_{\bar{t}_h}) \tag{13}$$

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

3. Grade Progression Probability

4. 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 $\{q\}$ and unobserved ability.

5. Income equations

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

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

E Distributional Assumptions

Household shock vector (period *t*). Stack child 1, child 2, and income shocks as

$$\varepsilon_t^H \equiv \begin{pmatrix} \varepsilon_{1,t}^{high} \\ \varepsilon_{1,t}^{low} \\ \varepsilon_{1,t}^{stay} \\ \varepsilon_{1,t}^{work} \\ \varepsilon_{2,t}^{high} \\ \varepsilon_{2,t}^{low} \\ \varepsilon_{2,t}^{stay} \\ \varepsilon_{2,t}^{work} \\ \varepsilon_t^{y^p} \\ \varepsilon_t^{y^{c_1}} \\ \varepsilon_t^{y^{c_2}} \end{pmatrix} \sim \mathcal{N}\left(0,\ \Sigma^H\right), \qquad K = 11.$$

Simulation implementation. Let $\eta_t \sim \mathcal{N}(0, I_K)$ and let Γ be the lower-triangular Cholesky of Σ^H so that $\Sigma^H = \Gamma\Gamma'$. Then

$$\varepsilon_t^H = \Gamma\,\eta_t.$$

F Estimation

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 two-child households from different cohorts, with different 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
7. $\hat{\theta}$ is the vector of parameters that minimizes the distance between the empirical (m^{data}) and model-simulated ($m(\theta)$) moments:

$$\hat{\theta} = \arg\min_{\theta} \left\{ \left[m^{\text{data}} - m(\theta) \right]' W \left[m^{\text{data}} - m(\theta) \right] \right\} \tag{14}$$

where W is a weight matrix.

Table F.1: Externally Estimated Parameters

(a) Income Equation	Estimate	S.E.
Child intercept (ϕ_0^c)	12.23	(0.145)
Child age coefficient (ϕ_a^c)	0.084	(0.008)
Child education coefficient (ϕ_e^c)	0.037	(0.007)
Parental intercept (ϕ_0^p)	15.30	(0.116)
(b) Shock Dispersion		
Child shock s.d. (σ_c)	2.532	–
Parent shock s.d. (σ_p)	2.383	–

Note: Panel (a) reports coefficients from auxiliary income equation regressions using Encuesta Continua de Hogares, household survey in Bogota. Panel (b) reports the standard deviations of idiosyncratic income shocks for children and parents. We use the values of residual standard errors from the income equation regression.

G Moments for Estimation and Validation

Table G.1: Summary of Moments Used in Estimation

Category	Moment description	Source
Regression moments (56)	Direct treatment effects Indirect (spillover) effects	Experiment Experiment
Control-group moments (26)	Control-group mean outcomes Control-group choice moments in 2006 Both children with high attendance in 2006	Experiment Experiment Experiment
Other experimental moments (3)	Graduation probability of the "indirect-group" child Graduation probability by birth order	Experiment Experiment
Observational moments (42)	All household choice moments in 2005	ECH survey for Bogotá
Re-entry and progression (7)	Delayed high-school graduation Age distribution at college graduation Average probability of progression in secondary school	SPADIES SPADIES C-600

Note: This table summarizes the 134 empirical moments used to estimate and validate the model. "Regression moments" are coefficients from randomized-control regressions capturing direct and indirect effects on choice outcomes and graduation. "Control-group moments" describe the distribution of high and low attendance, staying at home, working, and graduation among the randomized control group. The "indirect group" consists of children whose sibling is treated while they themselves do not receive the intervention. "Observational moments" are constructed from the 2005 *Encuesta Continua de Hogares* (ECH) household survey for Bogotá. "Re-entry and progression moments" combine several external sources. Delayed high-school graduation and the age distribution at college completion are computed from the Ministry of Education's *SPADIES* system, an administrative registry that tracks higher-education students over time, including their enrollment cohorts and graduation date. Progression probabilities in secondary school are obtained from the national school census (C-600), an annual census in which school principals report, at the school level, counts of students who pass, fail, drop out, or transfer; these counts are used to construct school-level pass rates.

H Model Fits

Table H.1: Model Fit: Regression Moments

	High Attend		Low or Home		Work as Primary		College Graduation	
	Data	Model	Data	Model	Data	Model	Data	Model
Control Mean	0.633	0.708	0.272	0.250	0.042	0.042	0.121	0.118
Direct Effects	Data	Model	Data	Model	Data	Model	Data	Model
Treated	0.074	0.076	-0.055	-0.073	-0.022	-0.003	-0.024	-0.016
	(0.037)		(0.035)		(0.014)		(0.017)	
1(Grade<9)	-0.144	-0.084	0.112	0.041	0.003	0.043	-0.134	-0.159
	(0.054)		(0.053)		(0.014)		(0.023)	
Age in 2005	-0.068	-0.023	0.040	0.011	0.024	0.012	-0.009	-0.029
	(0.014)		(0.014)		(0.006)		(-0.009)	
1(Female)	0.017	0.055	-0.007	-0.045	-0.015	-0.011	0.015	0.022
	(0.037)		(0.035)		(0.014)		(0.015)	
Sibling's age in 2005	0.001	-0.008	-0.003	0.008	-0.001	-0.000	-0.008	-0.015
	(0.009)		(0.009)		(0.004)		(-0.008)	
1(Sibling is Female)	0.021	-0.014	-0.032	-0.001	0.023	0.015	-0.004	0.005
	(0.037)		(0.035)		(0.014)		(-0.004)	
Constant	1.661	0.871	-0.300	0.161	-0.283	-0.032	0.439	0.378
	(0.257)		(0.256)		(0.090)		(0.439)	
Indirect Effects	Data	Model	Data	Model	Data	Model	Data	Model
Treated	-0.016	0.000	0.007	0.001	0.001	-0.001	-0.037	-0.037
	(0.038)		(0.037)		(0.016)		(0.017)	
1(Grade<9)	-0.169	-0.115	0.146	0.087	0.001	0.028	-0.114	-0.135
	(0.054)		(0.053)		(0.018)		(0.022)	
Age in 2005	-0.077	-0.029	0.048	0.015	0.028	0.014	-0.011	-0.029
	(0.014)		(0.014)		(0.006)		(0.005)	
1(Female)	0.036	0.044	-0.025	-0.040	-0.028	-0.004	0.022	0.004
	(0.038)		(0.037)		(0.016)		(0.017)	
Sibling's age in 2005	0.006	0.003	-0.001	-0.000	-0.005	-0.003	-0.003	-0.014
	(0.009)		(0.009)		(0.004)		(0.004)	
1(Sibling is Female)	0.038	-0.007	-0.069	0.003	0.024	0.003	-0.007	0.016
	(0.037)		(0.037)		(0.015)		(0.017)	
Constant	1.698	0.859	-0.440	0.161	-0.295	-0.020	0.373	0.366
	(0.254)		(0.254)		(0.106)		(0.097)	
WWLL	Data	Model	Data	Model	Data	Model	Data	Model
Treated	0.149	0.108	-0.103	-0.090	-0.030	-0.017	0.002	-0.023
	(0.036)		(0.034)		(0.013)		(0.019)	
1(Grade<9)	-0.190	-0.080	0.170	0.062	-0.004	0.018	-0.171	-0.159
	(0.051)		(0.049)		(0.013)		(0.025)	
Age in 2005	-0.076	-0.020	0.055	0.014	0.019	0.006	-0.015	-0.039
	(0.013)		(0.013)		(0.005)		(0.005)	
1(Female)	-0.002	0.036	0.014	-0.025	-0.014	-0.011	0.024	0.028
	(0.037)		(0.035)		(0.014)		(0.018)	
Sibling's age in 2005	-0.014	0.006	0.015	-0.004	-0.003	-0.002	-0.005	-0.013
	(0.009)		(0.009)		(0.003)		(0.005)	
1(Sibling is Female)	0.002	-0.021	-0.026	0.010	0.018	0.011	-0.011	-0.000
	(0.037)		(0.035)		(0.014)		(0.019)	
Constant	2.038	0.803	-0.837	0.184	-0.192	0.013	0.502	0.413
	(0.248)		(0.241)		(0.075)		(0.107)	

Table H.2: Model Fit: Experiment Moments

	High Attendance		Low or Stay		Work	
	Data	Model	Data	Model	Data	Model
12 years old	0.816	0.769	0.182	0.212	0.002	0.018
13 years old	0.818	0.769	0.181	0.207	0.001	0.024
14 years old	0.801	0.784	0.198	0.186	0.001	0.030
15 years old	0.754	0.770	0.241	0.192	0.005	0.038
16 years old	0.775	0.756	0.213	0.200	0.012	0.044
17 years old	0.716	0.713	0.261	0.232	0.024	0.055
18 years old	0.577	0.667	0.364	0.282	0.059	0.051

Table H.3: Model Fit: Observational Moments

	Attend		Home		Work	
	Data	Model	Data	Model	Data	Model
12 years old	0.971	0.891	0.013	0.024	0.016	0.085
13 years old	0.937	0.842	0.020	0.044	0.043	0.114
14 years old	0.876	0.806	0.050	0.053	0.074	0.141
15 years old	0.862	0.757	0.074	0.070	0.064	0.173
16 years old	0.762	0.725	0.138	0.081	0.100	0.194
17 years old	0.605	0.651	0.224	0.105	0.171	0.244
18 years old	0.426	0.553	0.273	0.127	0.301	0.320
19 years old	0.338	0.450	0.288	0.166	0.374	0.384
20 years old	0.299	0.365	0.287	0.203	0.414	0.432
21 years old	0.219	0.317	0.283	0.201	0.498	0.482
22 years old	0.205	0.242	0.273	0.233	0.522	0.525
23 years old	0.152	0.216	0.280	0.230	0.568	0.554
24 years old	0.117	0.156	0.263	0.247	0.620	0.597
25 years old	0.080	0.085	0.267	0.232	0.653	0.682

Table H.4: Model Fit: Other Moments

	Data	Model		Data	Model
Prop Lagged High School Graduation	0.353	0.289	First Born College Grad Proportion	0.159	0.134
Prop College Graduation at 21	0.150	0.103	Second Born College Grad Proportion	0.082	0.073
Prop College Graduation at 22	0.190	0.251	Both High Attendance in Secondary School	0.531	0.613
Prop College Graduation at 23	0.251	0.381	Low attendance Penalty	0.090	0.122
Prop College Graduation at 24	0.230	0.139	Indirect College Graduation	0.082	0.085
Prop College Graduation at 25	0.179	0.127	Average Progression Rate	0.920	0.888