Beyond Recipients: Sibling Spillovers of College Financial Aid*

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Abstract

This paper examines how expanding access to higher education affects other members of the household. We study sibling spillovers from Ser Pilo Paga, a financial aid program introduced in Colombia in 2014 that offered full scholarships to high-achieving students from low-income families. Using administrative data that link siblings across years and exploiting the program's eligibility thresholds, we estimate how younger siblings respond when an older sibling qualifies for financial aid. Younger siblings of eligible students perform better in school and are more likely to continue to higher education. They score 0.11 standard deviations higher on the national high school exam and are 14 percent more likely to enroll in higher education, with enrollment shifting toward high-quality universities. In the labor market, younger siblings in treated families work less in the short run, consistent with increased time in school, and their employment and earnings later converge to those of similar households without scholarships. Convictions for criminal offenses fall by about 60 percent, concentrated among poorer families. To understand how these effects arise, we combine university records of older siblings with early standardized tests of younger ones. The results show that spillovers appear early and are stronger when the older sibling performs well in college. The evidence points to intangible channels, including motivation, aspirations, and perceived returns to education, rather than to direct financial relief as the main mechanism behind these within-family effects.

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1 Introduction

Families from disadvantaged backgrounds remain severely underrepresented in higher education. In the United States, students from the top income quintile are about 38 percentage points more likely to attend a four-year college than those from the bottom, and in Latin America the gap is around 45 percentage points. Access to higher education is roughly four times more unequal than access to secondary school, and governments have responded by expanding financial aid programs for low-income students. Most studies of these programs focus on direct beneficiaries, but education decisions are rarely made in isolation. They unfold within families, where parents allocate resources across children and siblings shape one another's aspirations, share information, and set expectations about the future. A scholarship that helps one child may lift up others through role modeling and motivation, but it can also redirect time, money, and responsibilities in ways that hold siblings back (Edmonds and Schady, 2012; Soares et al., 2012). If these spillovers are substantial, overlooking them risks misjudging the true returns to public investment in higher education.

Siblings are often the closest peers to an individual, sharing a common upbringing, household, and exposure to similar shocks. As a result, changes in one child's circumstances can spill over to their brothers and sisters (Azmitia and Hesser, 1993; Black et al., 2021). These influences can operate through multiple channels: siblings may motivate one another, share information, or act as role models, but they may also compete for material and non-material resources. Most investments in human capital are made within families (Doepke and Tertilt, 2016), where parents face trade-offs—deciding whether to spread resources evenly across children or concentrate them on the child with the highest expected returns (Berry et al., 2020). Understanding how policies that expand educational opportunities for one child affect others within the same household is therefore key to evaluating their broader welfare implications.

This paper studies the spillover effects of college financial aid on siblings. We examine *Ser Pilo Paga* (SPP, "Being smart pays off"), a large-scale financial aid program introduced in Colombia in 2014 and active through 2017. The program provided loans to high-achieving high school seniors from low-income households who took the national high school exit exam, *Saber 11*. Participation in this exam is very high, about 90 percent of students take it regardless of whether they plan to apply to higher education, and scores are a central input in admissions decisions, with roughly 80 percent of colleges using them as a criterion (Londoño-Vélez et al., 2020). SPP loans were forgivable upon graduation and awarded based on two criteria: (i) household need, measured by the *Sisben* index, Colombia's official means test for targeting social programs, and (ii) academic performance, requiring students to be in roughly the top decile of the national *Saber 11* distribution. Eligible students could only enroll in one of the country's 33 accredited high-quality universities, 12 public and 21 private. Because only about 36 percent of test-takers lived in a municipality with at least one such institution, many recipients had to relocate to study.

To causally identify spillover effects, we exploit the program's eligibility rules as a source of quasi-experimental variation in a difference-in-differences design. Financial aid was granted only to students from need-eligible households who scored above the cutoff on the *Saber 11* exam. Our analysis focuses on households with at least two children, which allows us to observe how shocks to the older sibling's educational opportunity affect the younger sibling's outcomes. We estimate the effects on younger siblings of (i) students who qualify for aid, (ii) students who narrowly miss the cutoff, and (iii) a control group of students scoring in the 70th–80th percentile of the test distribution, low-income but unaffected by the program. Timing of exposure is determined by the cohort in which the older sibling takes the exam, allowing us to compare younger siblings' outcomes before and after the introduction of SPP. The identifying assumption is that, absent the program, the outcomes of younger siblings in all three groups would have followed parallel trends. This design captures both sides of the response: the motivational boost for siblings of students who almost qualify and the effect when an older sibling actually receives the scholarship.

Our analysis proceeds in four steps. We first validate the first stage by examining whether older siblings respond to the program. After 2014, students above the cutoff are 64 percentage points more likely to receive SPP aid, and their probability of immediate post-secondary enrollment increases by 47 percent, shifting toward high-quality universities as required by the program. Employment and earnings initially fall as college attendance rises but later surpass those of the control group, consistent with rising labor-market returns to education.

Second, before turning to the analysis of sibling spillovers, we extend the study of the program's indirect effects to students who were ineligible because they narrowly missed the cutoff. We show that these near-eligible students also experience increases in college enrollment, particularly in high-quality public universities. This pattern is consistent with the displacement of scarce seats: when eligible students shift toward private universities, near-eligible students benefit from the freed capacity in competitive public institutions. Their employment trajectories follow a similar U-shaped pattern, initial declines during college years followed by partial recovery, but do not exhibit long-run gains, consistent with the smaller labor-market premium associated with high-quality public higher education institutions (HEIs) relative to their private counterparts.

Third, we turn to the main analysis of the paper: the effects on younger siblings. When an older sibling qualifies for SPP, younger siblings' high-school exit exam scores increase by about 0.11 standard deviations (26 percent), and their probability of immediate college enrollment rises by 14 percent. Enrollment shifts toward higher-quality institutions, especially private universities, and the probability that younger siblings qualify for SPP themselves increases by 58 percent. In the short run, we observe temporary declines in formal employment and earnings, reflecting delayed labor-market entry as younger siblings remain in education longer. Over time, employment gradually recovers, though the estimates are not statistically significant. These patterns suggest that exposure to a college-going older sibling leads to higher educational investment rather than immediate labor-market gains. Finally, the probability of a criminal conviction falls by roughly 63 percent, with the largest reductions among the poorest households, where baseline risks are higher and constraints more binding. Younger siblings of students who narrowly miss the cutoff also experience positive, albeit smaller, improvements in educational outcomes, consistent with exposure to opportunity and information rather than income effects.

Finally, we explore the mechanisms that drive these intrahousehold spillovers. We contribute to a small but growing literature that seeks to identify how sibling spillovers arise, combining two complementary strategies. We begin by using administrative records from one of Colombia's leading private universities, which enrolled a large share of Ser Pilo Paga (SPP) recipients nationwide, to test whether the strength of the spillover depends on the older sibling's experience in college. We find that eligibility for SPP alone does not affect younger siblings' outcomes. The effects appear only when the older sibling performs well in their first semester, maintaining good academic standing or a high GPA. This suggests that what matters is not only attending college but succeeding once there. The evidence points to an intangible channel, such as motivation, aspiration, or role modeling, rather than to an intrahousehold reallocation of financial resources mechanism. We then exploit the rich age variation among younger siblings in our sample to study spillovers on early standardized test scores in grades 3, 5, and 9, when children are still years away from the college decision stage. The presence of significant gains at these early ages suggests that the effects are not driven by college-specific information or financial factors but by broader changes in effort, aspirations, and engagement with schooling.

Overall, the results show strong positive spillovers on younger siblings. Although we cannot directly observe intrahousehold transfers, the patterns suggest that these effects are unlikely to result from income reallocation toward the scholarship recipient or from a relaxation of the household budget constraint. In a low-income setting, we show that the program changes the extensive margin of college attendance, moving older siblings from not attending college to attending, often in another city. As a result, households lose the labor or caregiving contribution of the firstborn and can take on new costs related to housing, transportation, and study materials. It is unlikely that the improvements among younger siblings come from an easing of financial constraints, since the counterfactual is that the older sibling would not have attended college or drawn on household resources to do so. Our evidence is consistent with intangible mechanisms, such as higher motivation, stronger aspirations, and revised perceptions of the returns to education, rather than with material or income-effects channels.

Related Literature. The primary contribution of this paper is to show that expanding college access for one child reshapes the schooling, labor, and behavioral outcomes of other children in the same household, consistent with mechanisms operating through motivation and aspirations. Prior work has examined how college access influences siblings' educational choices, showing that when an older sibling gains admission, younger siblings are more likely to choose the same college or major (Altmejd et al., 2021) or to enroll and complete college themselves (Barrios-Fernández, 2022). First, we move beyond educational choices to examine a broader set of outcomes, including labor market and behavioral responses, using rich administrative data that link siblings across the education, labor, and criminal justice systems. These data also allow us to study how and when the spillovers occur. We find that they arise only when the older sibling performs well in college, implying that the older sibling's experience and success play a role in shaping the effect. Linking early standardized tests in third, fifth, and ninth grade, we further show that younger siblings' gains emerge years before the college decision stage, consistent with mechanisms related to motivation and aspirations rather than household resource reallocation. Second, unlike prior work, we study a policy that shifts the extensive margin of higher education, moving scholarship recipients from not attending college to attending selective universities. This setting allows us to examine how exposure to educational opportunities affects younger siblings in households facing tight financial and time constraints, capturing how college access alters family choices and intergenerational dynamics. Third, we find that the program's influence is not confined to directly treated families. Even households on the margin of eligibility show signs of improvement, suggesting that the policy's reach, and its spillover potential, extend beyond its intended beneficiaries.

This paper also contributes to the literature on how parents allocate resources among children within the household. Most existing studies focus on early childhood, when parental investments are more flexible and returns are more uncertain. Identifying these reallocations is empirically challenging, as endogeneity often limits within-family comparisons (Barrera-Osorio et al., 2023). A small but growing body of work has begun to address this challenge. An open question in this literature is whether households continue to adjust resources as children grow older, since most evidence to date comes from early-life interventions. Adhvaryu and Nyshadham (2016) exploit variation in exposure to a large-scale iodine supplementation program in Tanzania and find that siblings of treated children are more likely to receive necessary vaccinations. They interpret these findings through a model in which parents are averse to inequality among their children. Carneiro et al. (2023) study sibling spillovers from a prenatal intervention in Nigeria that combined information and cash transfers and show that the program improved health and nutrition outcomes for non-treated siblings, consistent with parents equalizing investments across children. In contrast, Barrera-Osorio et al. (2023) analyze a conditional cash transfer in Bogotá and find negative spillovers on siblings. Yet the intervention itself had limited effects on the directly treated child, making those negative spillovers difficult to interpret as evidence that parents reallocate resources away from other children.

We extend this literature beyond early childhood to adolescence and young adulthood, when children are more autonomous and the opportunity costs of education are higher. We examine how families respond when an older child gains access to higher education and show that younger siblings benefit across multiple ages, around 9, 11, 15, and 17, corresponding to third, fifth, ninth, and eleventh grades. These gains persist into adulthood, with long-run effects such as reductions in criminal behavior. At later ages, parents likely have clearer information about their children's abilities and prospects, which could in principle lead them to reinforce rather than compensate investments. Yet our findings show no evidence of crowding out, the evidence is not consistent with parents reallocating resources away from younger siblings, even when the treated child is older and the intervention occurs later in life.

More broadly, this paper contributes to the literature in the economics of crime that studies the determinants of criminal behavior (Chalfin and McCrary, 2017; Doleac, 2023). Previous research shows that keeping youth in school reduces youth offending (Jacob and Lefgren, 2003; Luallen, 2006; Anderson, 2014; Berthelon and Kruger, 2011), that stronger labor-market conditions lower crime and recidivism (Yang, 2017; Schnepel, 2018; Britto et al., 2022), and that reduced access to mental healthcare increases incarceration (Doleac, 2023). While most of this work emphasizes enforcement or labor-market incentives, this paper argues that criminal involvement is also a function of perceived opportunity and aspirations. We show that expanding access to college for one member of the household can influence the criminal behavior of others who are not direct beneficiaries. When an older sibling gains entry to higher education, younger siblings—who face the same economic environment but a new reference point—become significantly less likely to engage in crime. The results suggest that exposure to opportunity and to a credible role model within the household can reduce criminal behavior by reshaping aspirations and perceived returns to legal activities. In this sense, we highlight a complementary mechanism to deterrence: expanding access to education changes how individuals perceive their prospects, lowering the relative appeal of crime.

Our paper is organized as follows. Section 2 provides background on higher education access in Colombia and describes the design of the Ser Pilo Paga (SPP) program. Section 3 outlines the data sources and sample construction, including the linkage of siblings across administrative records and descriptive statistics for siblings and households. Section 4 presents descriptive evidence on sibling correlations in education, labor, and crime outcomes, as well as motivating evidence on spillovers and local continuity around the SPP cutoff. Section 5 presents the empirical strategy and discusses identification. Section 6 reports the main results, detailing the first stage and the effects of SPP on younger siblings' education, labor, and behavioral outcomes. Section 7 examines heterogeneity across family characteristics and presents robustness checks. Section 8 explores the mechanisms underlying these spillovers. Section 9 concludes.

2 Institutional Background and Program Details

2.1 Context: Higher Education in Colombia

By 2013, only 4 percent, 11 percent, and 34 percent of individuals in poor, vulnerable, and middle-class Colombian households, respectively, had attained tertiary education. Among heads of poor households, only 36 percent had completed secondary education or higher (World Bank Group, 2015). These figures underscore the deep inequality in educational attainment across income groups and highlight the persistent barriers to accessing post-secondary education in the country. Even when access is achieved, the quality and value-added of higher education often remain insufficient (Camacho et al., 2017).

The higher education (HE) system in Colombia is regulated by Law 30 of 1992, which governs the organization and operation of both public and private HE institutions. In the early 2000s, the Ministry of Education introduced new regulations to strengthen quality assurance. Institutions are required to undergo a certification process for each academic program to obtain a *Qualified Registry*, which is a prerequisite for operation and inclusion in the National System of Higher Education Institutions (SNIES). In addition to this mandatory certification, institutions may voluntarily apply for a *High-Quality Accreditation*, which involves an extensive self-evaluation and external peer-review process. This accreditation is regarded as a proxy for excellence in education provision (Camacho et al., 2017). Fewer than 10 percent of institutions held this high-quality accreditation during the period analyzed.

Colombia's HE system includes four main types of institutions that differ by program length and degree level: (i) *Professional Technical Institutions*, which offer one- to three-year technical programs; (ii) *Technological Institutions*, focused on applied technical education; (iii) *University Institutions*, which can offer professional degrees and postgraduate specializations; and (iv) *Universities*, which are the only institutions authorized to confer master's and doctoral degrees.[1] By 2014, there were 290 registered higher education institutions, of which 27 percent were private. Among the 83 institutions classified as universities, only 32 (12 public and 20 private) had received high-quality accreditation as of October 1, 2014 (SNIES data). Admission rates reflected both capacity constraints and high selectivity: while private institutions admitted around 86 percent of applicants, public HEIs admitted only about 36 percent. These figures reveal both a shortage in the supply of public higher education and significant quality segmentation within the system.

Colombia's HE system is tightly linked to standardized testing administered by the Institute for the Assessment of Education (ICFES). Two exams are particularly relevant: the high school exit exam (*Saber 11*) and the higher education graduation exam (*Saber Pro*). The *Saber 11* test has been mandatory since 1980 for graduation from high school and admission into HEIs, serving a role similar to the SAT or ACT in the United States. Its near-universal take-up—over 90 percent of seniors—makes it an excellent proxy for the universe of high school graduates. The *Saber Pro* exam, introduced in 2003 and made mandatory for all higher education graduates by 2011, assesses key cognitive competencies such as quantitative reasoning, critical reading, and written communication (Busso et al., 2023). In 2013, 576,094 students took the *Saber 11* exam. Approximately 17 percent of these students subsequently enrolled in higher education, and among those who did, roughly 30 percent attended a high-quality accredited institution. The exam results play a decisive role in post-secondary admissions, with around four-fifths of HEIs using *Saber 11* scores as

[1] Articles 58–60 of Law 30 (1992) regulate the creation of public institutions, while Act 1478 (1994) governs the establishment of private ones. Subsequent regulations, including Act 2566 (2003) and Law 1188 (2008), set minimum quality requirements for launching new programs. The National Training Service (SENA), created in 1957 and overseen by the Ministry of Labor, provides technical training but is not part of the HE system.

a key admission criterion (Londoño-Vélez et al., 2020). However, for many economically disadvantaged students, the primary barriers to accessing higher education are financial. In some private universities, tuition fees are notably high, and securing student loans or financial aid proves challenging with limited availability (Camacho et al., 2017).

Colombia experienced a remarkable expansion in educational coverage during the 2000s, mirroring regional trends. Secondary school completion increased substantially, and the number of high school graduates rose by about 30 percent between 2001 and 2011. During the same decade, college enrollment expanded by nearly 50 percent, driven largely by the creation of new academic programs. Between 1999 and 2011, the number of higher education programs nearly doubled—from 2,609 to 5,101—with most of the expansion occurring in existing institutions rather than newly established ones (Carranza and Ferreyra, 2019). While this growth broadened access, many of the new programs attracted students with lower high school test scores, suggesting that rapid expansion was accompanied by widening disparities in program quality and student preparedness.

Overall, by the time the *Ser Pilo Paga* program was launched in 2014, Colombia's higher education landscape was characterized by three features: (i) unequal access across socioeconomic groups, (ii) persistent quality segmentation between accredited and non-accredited institutions, and (iii) a growing but uneven supply of higher education opportunities.

2.2 The Ser Pilo Paga Program

On October 1, 2014, Colombia launched *Ser Pilo Paga* (SPP), a large-scale merit- and need-based financial aid program targeting high-achieving students from low-income households. Fully financed with public funds, SPP enabled eligible students to enroll in any high-quality accredited university—public or private—covering full tuition and providing a monthly living stipend between one and four times the Minimum Current Legal Monthly Salary (SMMLV), depending on the municipality where the institution was located. The government planned approximately 10,000 new awards per year from 2015 to 2018, ultimately financing close to 40,000 undergraduate degrees. Owing to its scale and extensive public communication campaign, the program rapidly became one of the most visible and popular social initiatives in the country (Londoño-Vélez et al., 2020).

SPP eligibility combined a national test-score requirement with a poverty-based household classification. Students were required to satisfy four criteria: (i) sit for the national high school exit examination (*Saber 11*) in the fall of 2014, 2015, 2016, or 2017; (ii) score approximately within the top decile nationwide[2]; (iii) gain admission to a high-quality accredited higher education institution (HEI) or one undergoing accreditation renewal; and (iv) appear in the *Sisben III* poverty registry below a cutoff that varied by geographical location, with distinct thresholds for major cities, other urban areas, and rural zones.[3] Given the central role of *Saber 11* in admissions and the scarcity of affordable credit—particularly for private universities—SPP represented a substantial relaxation of financial constraints for academically qualified low-SES students (Camacho et al., 2017).

A first wave of empirical evidence documents large and immediate effects on access and college quality. Exploiting the stringent eligibility threshold for the inaugural cohort, Londoño-Vélez et al. (2020) show that SPP eligibility increased immediate college enrollment by roughly 27–32 percentage points (about 60–85%), effectively closing the socioeconomic enrollment gap among top-decile test-takers. The program also reallocated students across the quality distribution of higher education: beneficiaries shifted from low-quality institutions toward high-quality—predominantly private—universities, dramatically increasing the socioeconomic diversity of historically elite campuses. Londoño-Vélez et al. (2020) further demonstrate that financial aid expanded enrollment among both eligible and ineligible students, generating net social gains. As high-ability, low-income students vacated seats in lower-tier institutions, these were filled by the next-best applicants. Greater visibility and transparent eligibility rules expanded demand, while private high-quality universities responded by modestly increasing cohort sizes.

Longer-run evaluations confirm that these shifts translated into substantial gains in educational attainment, human capital, and labor-market outcomes. Linking national administrative records through 2023, Londoño-Vélez et al. (2025) document large increases in B.A. completion at high-quality institutions (particularly in STEM fields), improved performance on standardized college exit exams, and persistent labor-market advantages: nine years after high school, recipients earn roughly 18 log points more than comparable ineligible peers and are substantially more likely to reach the upper tail of the earnings distribution. The program also narrowed socioeconomic gaps among students with comparable academic ability by equalizing access to high–value-added universities. A welfare analysis of SPP reveals high social returns: the marginal value of public funds (MVPF) ranges between 1.9 and 3.2, indicating that individuals' willingness to pay for the program substantially exceeds

[2]Students taking the test in the fall of 2014 had to score in the 90th percentile, those in 2015 in the 91st, in 2016 in the 95th, and in 2017 in the 96th.

[3]Students' families had to be registered in version III of the System for the Identification of Potential Beneficiaries of Social Programs (*Sisben*) with the following maximum scores: 57.21 (out of 100) for families residing in the 14 main cities, 56.32 for those in other urban areas, and 40.75 for those in rural areas.

its net fiscal cost.

Beyond its direct beneficiaries, SPP also altered incentives upstream in the educational pipeline. Laajaj et al. (2022) show that once the program became publicly known, low-SES students in the 2015 cohort—the first aware of SPP while preparing for *Saber 11*—increased effort and achievement near the merit cutoff, leading to differential enrollment gains among the newly eligible. Related improvements in *Saber 9* scores at the school level point to broader motivational responses earlier in the schooling trajectory, consistent with higher perceived returns to education and expanded aspirational windows among disadvantaged students.

Taken together, the existing literature shows that SPP relaxed binding credit constraints, reallocated talent toward high-quality institutions, enhanced skill acquisition and labor-market outcomes, and generated sizable social returns. Yet, one dimension remains largely unexplored: how these shocks propagate *within families*. If a scholarship propels one child into a selective university, does that experience alter younger siblings' beliefs, effort, information sets, or opportunity costs? This paper examines that margin. Leveraging the eligibility criteria of SPP and rich administrative linkages across siblings, we study whether younger siblings of SPP-eligible students adjust their schooling, higher-education, labor, and behavioral trajectories. This perspective sheds light on the intergenerational and intra-household mechanisms through which large-scale merit- and need-based scholarships may amplify their benefits beyond the direct recipients.

3 Data and Descriptive Statistics

Estimating within-family spillovers requires linking family composition and following individuals within the same household across different stages of life, effectively creating a longitudinal family panel. This paper builds on a large-scale consolidation of multiple administrative datasets to construct a nationwide panel of siblings linked across education, labor, and crime outcomes between 2009 and 2023. The analysis combines administrative records with original data on criminal convictions and institutional data from a large private university that hosted a significant share of *Ser Pilo Paga* (SPP) beneficiaries.

3.1 Data Sources

We begin with the population of test-takers of the national high school exit examination, *Saber 11*, administered by the Instituto Colombiano para la Evaluación de la Educación (*ICFES*, 2009-2023). The exam measures core competencies in mathematics, critical reading, writing, and reasoning. Around 90 percent of high school seniors take the test each year, regardless of whether they plan to continue to higher education, providing an almost complete census of secondary graduates. Two exam rounds are held annually: students in private schools typically sit for the first-semester exam, while most public-school students take it in the second semester. The dataset includes basic demographic characteristics, school identifiers, and self-reported socioeconomic information.

To capture outcomes earlier in the educational trajectory, we incorporate standardized test data for grades 3, 5, and 9—*Saber 3*, *Saber 5*, and *Saber 9*. Individual-level data are available for the 2017 cohort only, covering approximately 760,000, 776,000, and 590,000 students, respectively. This cross-section allows us to estimate sibling spillover effects in earlier stages of schooling, not only at the high school graduation and college decision margin captured by *Saber 11*. In addition to test scores, the accompanying student questionnaires collect information on academic behaviors such as attendance, punctuality, and grade repetition. For example, students report how often they arrived on time, were late, or missed class in the previous month. These measures provide valuable proxies for school engagement and effort in primary and secondary education.

Socioeconomic background and program eligibility are measured using the *Sisben* (*Sistema de Identificación de Potenciales Beneficiarios de Programas Sociales*), Colombia's primary instrument for targeting social programs. Managed by the National Planning Department (DNP), *Sisben* characterizes living conditions among low-income households and assigns each family a continuous index from 0 (poorest) to 100 (least poor), based on housing, utilities, demographics, and education (Camacho and Conover, 2011). Versions I through IV of *Sisben* were implemented in 1999, 2005, 2010, and 2018, jointly covering roughly 12 million households—about 83 percent of all households in the country. The 2010–2018 version (III) defines socioeconomic eligibility for the *Ser Pilo Paga* program.

We next use the longitudinal school census (*SIMAT*, 2005–2023), which provides annual records of enrollment and grade progression for all public and private schools. These data allow us to follow students over time to measure grade advancement, dropout, and completion. Postsecondary outcomes are drawn from the National Higher Education System (*SNIES*, 2009–2023), which records enrollment and degree completion across universities and technical institutions. From *SNIES*, we identify the type of postsecondary program—vocational education and training (VET), two-year technical degrees, or four-year university programs—and the field of study, including whether it belongs to a STEM discipline. We complement these data with results from *Saber Pro*, the mandatory higher-education exit exam taken by graduating students in their final semester. Performance on *Saber Pro* serves as a proxy for college completion and allows us to assess skill acquisition at the end of tertiary education.

Labor-market outcomes are obtained from the Integrated Contribution System, *PILA* (*Planilla Integrada de Liquidación de Aportes, 2009–2021*). It consolidates employer and employee contributions to health, pension, and labor-risk insurance systems, providing monthly information on formal employment and earnings. These data enable the construction of medium-term employment and earnings trajectories after secondary and postsecondary schooling.

To capture behavioral outcomes, we complement these administrative sources with original data on criminal convictions. We web-scrapped approximately 600,000 publicly available conviction records from 17 of Colombia's 33 judicial districts. These districts encompass the country's largest and most urbanized regions, representing roughly 67 percent of the national population, 69 percent of homicides, and 83 percent of property crimes. Each record includes the type of offense, date of ruling, sentence length, and court of jurisdiction.

Finally, we complement the analysis with institutional data from a large private university that enrolled approximately 2,500 *Ser Pilo Paga* beneficiaries during the program's operation. These records include information on admissions, program enrollment, course-taking, GPA, and degree completion. Identifying older siblings within this institution provides a unique opportunity to examine the academic performance of scholarship recipients and to test potential mechanisms for sibling spillovers through exposure, role modeling, and information sharing within families.

3.2 Family Linkage and Sample Construction

We construct a family-level panel that links siblings across education, labor, and behavioral outcomes. The linkage relies on household identifiers in *Sisben*, which allow grouping individuals into family units. According to Colombia's 2018 Population and Housing Census, there are 14.24 million households nationwide, of which 11.87 million appear in *Sisben*, representing about 83 percent of all households. Among these, 9.94 million satisfy the poverty-eligibility criteria of the *Ser Pilo Paga* (SPP) program, and we restrict our analysis to this group.

We further limit the sample to households with at least two members born between 1988 and 2018—the period over which educational trajectories can be observed in our data. This restriction yields 5.05 million poverty-eligible households with at least two school-age individuals. Within these households, we define the *older sibling* as the first-born child in the family and identify them in the national school census (*SIMAT*). We find 2.03 million such first-borns in *SIMAT*, of whom 968,502 took the *Saber 11* exam between 2009 and 2019.

These students constitute our cohort of "older siblings".

We then identify all *younger siblings* of these students within the same households. The 968,502 older siblings are linked to 1.69 million younger siblings, corresponding to an average of 2.75 younger siblings per older sibling. This estimate aligns closely with self-reported family size in the *Saber 11* questionnaire, where students report an average of 2.7 siblings, suggesting that our linkage captures most sibling relationships in the target population. Appendix Figure 5 provides a schematic overview of this process, illustrating how the different data sources are linked to construct the final sibling panel.

After identifying 1.69 million younger siblings, we merge them with administrative records to measure educational, employment, and behavioral outcomes. The final longitudinal panel includes families in all municipalities and follows cohorts born between 1988 and 2018, observed in schooling data from 2005 to 2023 and in higher-education and labor-market records through 2023. Appendix Figure 6 shows that about half of the older siblings in our sample have one younger sibling, with a long right tail reflecting larger families. Appendix Figure 7 displays the distribution of younger siblings' ages when the first-born takes the high school exit exam. The distribution centers around 13 years of age, providing a broad window to study sibling spillover effects both before and after key educational transitions. Together, these sources create a family-level dataset that links standardized test scores, socioeconomic characteristics, educational trajectories, employment, and criminal records. This unique linkage enables the analysis of within-family effects of higher-education access over more than two decades of administrative data.

3.3 Descriptive Statistics of Households and Siblings

Appendix Table 1 provides an overview of the households and individuals in the sibling sample. Panel A describes the socioeconomic background of families, which is shared among siblings, while Panel B reports individual characteristics of older and younger siblings. Together, these patterns indicate that the data represent a population of low-income households consistent with the program's intended target group.

Most households in our sample belong to the lowest socioeconomic strata of Colombia's classification system.[4] Eighty-eight percent of families fall within strata 1 and 2, which

[4]Strata are official socioeconomic categories assigned by municipal governments to residential properties under the *Sistema Nacional de Estratificación Socioeconómica*, as established by Law 142 of 1994 and regulated by the National Planning Department (DNP). The classification (from 1 = lowest to 6 = highest) is based on observable physical and urban characteristics of dwellings and their surroundings. It is used primarily to administer cross-subsidies in public utility tariffs: households in lower strata receive subsidies for water, electricity, and gas, financed through contributions from higher-strata users DANE (2015).

correspond to the poorest residential areas where households receive public-utility subsidies. Families in our sample are larger than the national average. The mean household size is 4.8, compared with 3.2 in the third wave of *Sisben*. When restricting to households in Sisben with at least two children—as in our analysis—the national average rises to 4.95, nearly identical to our estimate. Appendix Figure 8 shows that the distribution of household size in our data closely matches that of the population in the census of the poor, confirming that our linked households are representative of low-income families with children.

Parental education is low: about 45 percent of mothers and 38 percent of fathers have completed some secondary education, and only around 6 percent have any form of higher education. Consistent with this socioeconomic profile, access to durable goods and services is limited—fewer than half of households report owning a computer, 43 percent have internet access, and only 14 percent own a car. These figures align with the targeting criteria of the *Sisben* registry, which identifies the poorest households for social-program eligibility.

Turning to individual characteristics, older siblings were born on average in 1996 and took the *Saber 11* exam at age 17.7, while younger siblings were born roughly six years later, in 2002, and were about 12 years old when the first-born sat for the test. The majority of students attend public schools (91 percent), and roughly 4 percent of older siblings score above the *Ser Pilo Paga* eligibility threshold. Academic achievement is modest but comparable across siblings: both groups score about 0.1 standard deviations below the national mean on *Saber 11*. Immediate postsecondary enrollment rates are low, as expected for this population, and the distribution of fields of study is similar across siblings. Six years after high school, 40–42 percent of individuals hold formal employment, and average monthly earnings correspond to about 40–45 percent of the minimum wage.

4 Descriptive Evidence

4.1 Sibling Correlation

A large body of research in economics and sociology emphasizes the importance of family background in shaping long-term outcomes such as education, earnings, and social mobility. While traditional intergenerational analyses focus on links between parents and children, sibling correlations provide an alternative and complementary perspective. This approach measures the extent to which siblings resemble one another in key life outcomes, capturing shared family influences—both observed and unobserved—including common parental investments, neighborhood environments, and school quality. A high sibling correlation indicates that much of the variation in an outcome reflects differences between families rather than within them, whereas a low correlation suggests that family background plays a smaller role in generating inequality (Corcoran et al., 1976; Solon, 2018). Unlike intergenerational estimates, sibling correlations can be derived using data from a single generation, provided that siblings can be reliably identified and followed over time—a feature uniquely enabled by our linked administrative data.

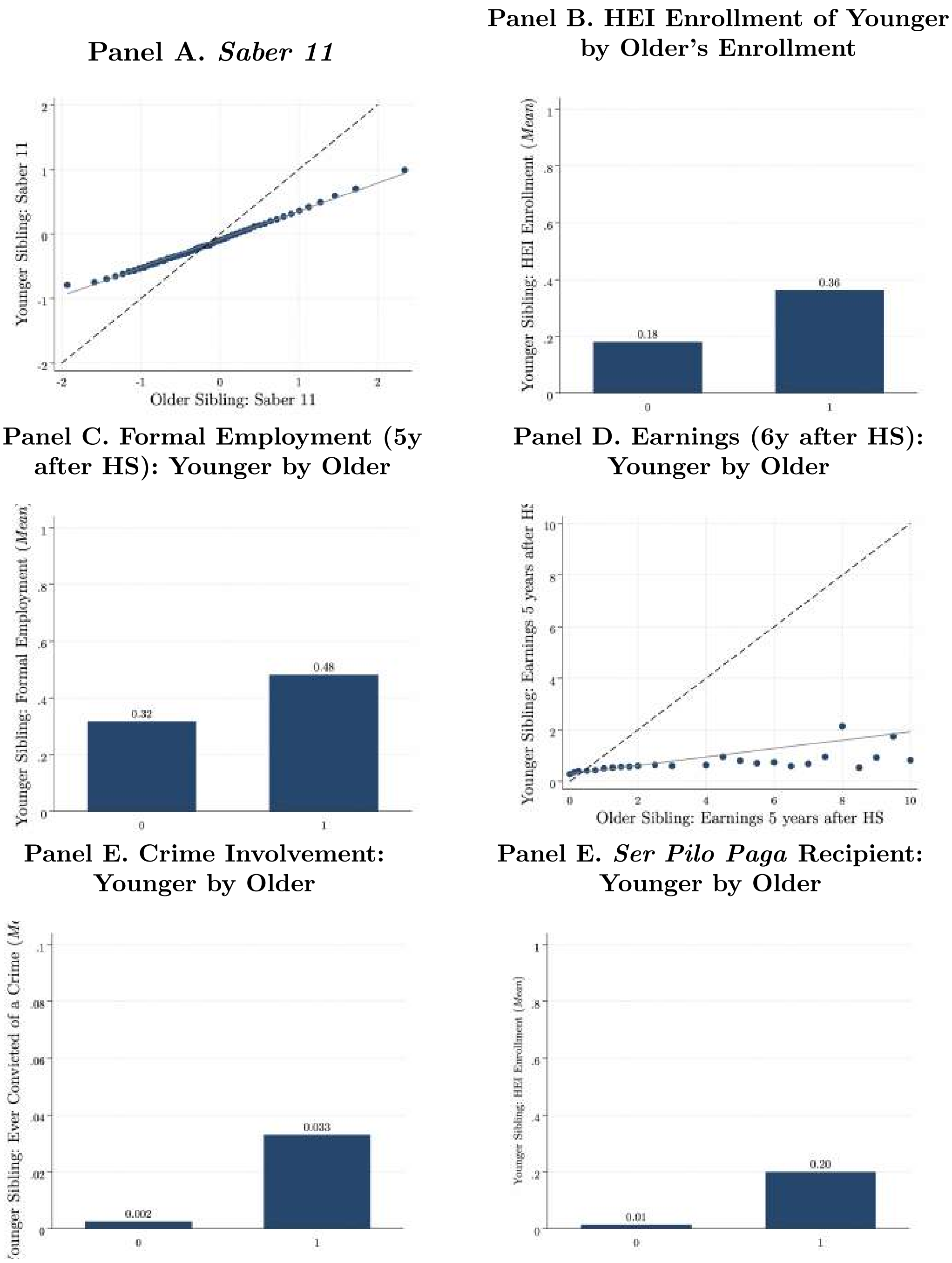
Figure 1 and Appendix Table 2 present descriptive evidence on the correlation between older and younger siblings across educational, labor, and behavioral outcomes. Together, they show a consistent pattern of positive within-family associations: siblings who grow up in the same household tend to perform similarly in school, make comparable educational choices, and experience related early labor-market outcomes.

Panel A of Figure 1 plots younger siblings' scores on the national high school exit exam (*Saber 11*) against those of their older siblings. The fitted line shows a strong positive within-family correlation of 0.44, indicating that siblings' academic achievement is closely related. However, the slope is flatter than the 45-degree line, suggesting that when the older sibling performs very well, the younger sibling tends to perform slightly worse, and conversely, when the older sibling scores poorly, the younger sibling performs somewhat better. This pattern is consistent with partial convergence in performance within families rather than perfect replication of academic outcomes.

Panel B examines correlations in higher-education enrollment. Among younger siblings whose older sibling did not enroll in a higher education institution (HEI), only 18 percent enrolled themselves. In contrast, when the older sibling pursued higher education, 36 percent of younger siblings did so as well—double the rate. Consistent with this pattern, the correlation in immediate college enrollment is 0.18, and correlations across HEI types range between 0.13 and 0.19, with similar magnitudes for public and private institutions and for low- and high-quality HEIs. These correlations suggest that the link between siblings' college attendance may capture both intangible mechanisms—such as aspirations or information sharing—and tangible ones, such as shared financial capacity to fund higher education.

A similar pattern emerges in labor-market outcomes six years after high school. Panel C shows that when the older sibling is formally employed, about 50 percent of younger siblings also hold formal jobs; this share falls to 33 percent when the older sibling is not formally employed. Correspondingly, the correlation in formal employment is 0.16, and in monthly earnings—measured in multiples of the minimum wage—it is 0.17. These values are smaller than the 0.4–0.5 range typically reported for long-term sibling income correlations in the

Figure 1: Sibling Correlations in Education, Labor, and Crime



Notes: Each panel displays within-family correlations between older and younger siblings for key outcomes. Panel A plots older siblings' standardized *Saber 11* scores against those of their younger siblings; both variables are normalized to have mean zero and standard deviation one within each test cohort. Panel B shows the share of younger siblings enrolled in higher education by whether the older sibling enrolled in any higher education institution (HEI). Panel C reports the share of younger siblings formally employed six years after high school, conditional on the older sibling's formal employment status at the same horizon. Panel D presents mean earnings of younger siblings (in multiples of the minimum wage) by the older sibling's earnings six years after graduation. Panel E shows the probability that a younger sibling has any recorded criminal conviction by the older sibling's conviction status.

United States (e.g., Solon, 1999; Mazumder, 2008), where brothers exhibit correlations of 0.47–0.54 in earnings and wages and sisters show values around 0.34–0.45. The lower magnitudes observed in our data likely reflect both life-cycle and contextual factors: individuals in our sample are observed early in their careers—five to six years after completing high school—before earnings trajectories stabilize, and the sample is restricted to low-income, Sisben-eligible households, which exhibit limited between-family variation in resources. In addition, Colombia's labor market is characterized by high informality and mobility, which further attenuates the role of family background in explaining early labor outcomes.

Appendix Table 2 and Panel D documents sibling correlations in criminal behavior. The overall correlation in any criminal conviction is 0.034, with similar magnitudes across specific offense categories. Although these values are modest, they are meaningful given the low baseline prevalence of criminal activity in the data. The probability of ever being convicted of a crime in our sample is around 0.0025 for younger siblings—roughly half the rate observed for adult men in Brazil, where the probability of prosecution for any crime is approximately 0.0052 and even lower for specific offenses such as economic (0.0014) and violent crimes (0.0015) (Britto et al., 2022).

Differences in exposure windows also help explain the relatively low correlations and the low overall probability of conviction in our sample. Because criminal records are observed through 2023, older siblings—who are on average six years older—have had more time to appear in judicial records than their younger siblings. Appendix Figure 9 shows that sibling correlations in criminal convictions increase with the age of the younger sibling's cohort: for instance, the correlation reaches 0.15 when younger siblings were born around 1991. This pattern indicates that the smaller overall correlation is largely driven by the limited exposure of younger cohorts rather than by weaker familial clustering in criminal behavior. Because younger siblings have had fewer years of potential exposure to criminal behavior, our estimates represent lower bounds of the true long-run sibling correlations in crime. As these cohorts age and additional administrative years become available, we expect the measured correlations to rise, consistent with the pattern already visible among older cohorts.

Finally, correlations in *Ser Pilo Paga* (SPP) participation are 0.20, consistent with strong within-family similarities in high academic achievement among low-income students. Because eligibility depends on cohort-specific merit thresholds, this relatively high value suggests that families play an important role in shaping not only educational access but also the likelihood of benefiting from competitive scholarship programs.

Overall, these results reveal consistent within-family correlations across education, employment, and behavioral outcomes, even in early adulthood. Correlations are strongest in cognitive and academic outcomes (≈0.44), moderate in education and labor outcomes (≈0.17–0.19), and smaller but still positive in crime involvement (≈0.03). Relative to the U.S., where sibling correlations in education and long-term earnings often exceed 0.5–0.6, our estimates imply that family background explains a smaller fraction of inequality among poor Colombian households. This difference likely reflects a combination of (i) the early age window observed, (ii) the socioeconomic homogeneity of the Sisben sample, (iii) the volatility of early labor outcomes, and (iv) greater contextual heterogeneity and informality in the Colombian labor market. Together, these patterns provide an important benchmark for interpreting the causal sibling spillover effects.

4.2 Motivating Evidence: Spillovers and Breakdown of Local Continuity at the SPP Cutoff

A natural starting point to study how the *Ser Pilo Paga* (SPP) program affected the siblings of eligible students is to exploit its merit-based eligibility cutoff using a regression discontinuity (RD) design. The program offered student loans that converted into full scholarships upon graduation for high-achieving, low-income students whose *Saber 11* scores exceeded a national threshold. A valid RD design would require that, around this cutoff, potential outcomes for younger siblings evolve smoothly—so that those just below the threshold serve as valid counterfactuals for those just above it. Figure 2 plots younger siblings' educational outcomes against their older siblings' standardized *Saber 11* scores, normalized relative to the SPP eligibility cutoff (values ≥ 0 indicate eligibility). Each panel compares younger siblings' outcomes for pre- and post-program cohorts of older siblings (2010 and 2014) and reports local polynomial RD point estimates with robust bias-corrected confidence intervals following Calonico, Cattaneo, and Titiunik (2014).

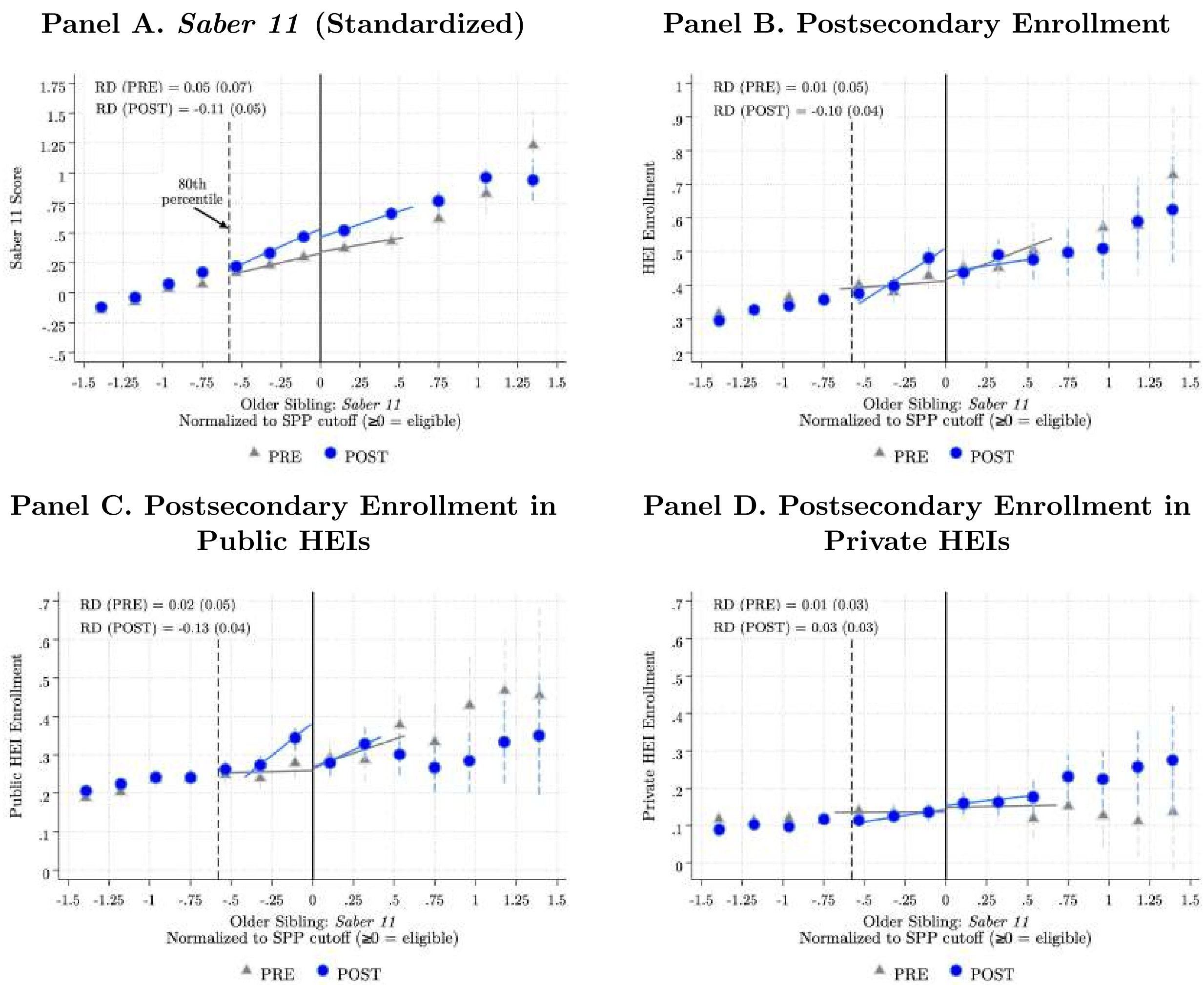
Panel A presents results for younger siblings' *Saber 11* performance. In the pre-program period (2010), outcomes are smooth across the cutoff, and the RD estimate is small and statistically insignificant (0.048, SE = 0.069, $p = 0.48$), confirming continuity in the absence of treatment. After the introduction of SPP (2014), the fitted relationship steepens just below the cutoff, and the RD estimate becomes significantly negative (-0.11, SE = 0.048, $p = 0.022$). This negative discontinuity does not necessarily indicate a deterioration in performance. In fact, Appendix Table 3 shows that, within the RD bandwidth, younger siblings of students taking the test in 2014 (post-period) perform on average 0.10 standard deviations better than their counterparts in 2010 (pre-period), a difference that is statistically significant at the 1% level. Above the cutoff, younger siblings of post-period students also perform better than those of pre-period students, with a mean difference of 0.17 standard deviations, likewise significant at the 1% level. These results suggest that the negative RD estimate may be driven by upward improvements among younger siblings just below the threshold—consistent with indirect benefits for families with an almost-eligible student. When near-eligible households experience positive spillovers, the RD estimate can mechanically understate the true effect of the program and even reverse sign, as observed here.

Panels B–D present analogous patterns for postsecondary enrollment outcomes. For overall higher-education enrollment (Panel B), the post-SPP RD estimate is -0.10 (SE = 0.040, p = 0.012), compared to an insignificant 0.013 (SE = 0.051, p = 0.79) before the program. These results again suggest strong local spillovers below the cutoff that bias the RD toward zero or negative values. Below the threshold, we observe a steeper slope in the probability of enrollment—implying that younger siblings' outcomes became more responsive to the older sibling's performance after SPP was introduced. This pattern is consistent with enhanced within-family transmission of educational aspirations, information, or motivation once the scholarship opportunity became available.

Furthermore, Panels C and D show that these changes are not confined to the immediate neighborhood of the cutoff. In the post-SPP period, shifts for younger siblings of eligible students emerge farther above the threshold: they are less likely to attend public HEIs and more likely to enroll in private ones. This suggests that the program's effects—and the resulting behavioral adjustments within families—extend beyond the local discontinuity in eligibility.

Together, these patterns indicate that the standard RD identifying assumptions do not hold in this setting. The descriptive evidence suggests that younger siblings' outcomes below the cutoff are affected by indirect exposure to treatment, and the treatment effect itself is not strictly local but spreads across a wider range of scores. For this reason, we do not rely on an RD design. Instead, the next section introduces an empirical strategy that exploits variation in older siblings' eligibility across cohorts and households—before and after the introduction of SPP—to estimate both the effect of having an older sibling almost eligible for the program and the spillover effect of having a sibling who actually qualified for SPP.

Figure 2: Younger Siblings' Education Outcomes Around the *Ser Pilo Paga* Eligiblity Cutoff



Note: Each panel compares younger siblings' educational outcomes for pre- and post-program cohorts of older siblings (2010 and 2014). Outcomes are plotted against the older sibling's standardized *Saber 11* score, normalized relative to the *Ser Pilo Paga* (SPP) eligibility cutoff (values ≥ 0 indicate eligibility). Blue lines correspond to the post-program period (2014), and gray lines to the pre-program period (2010). The solid vertical line marks the SPP cutoff, and the dashed line at the 80th percentile highlights where slope changes in younger siblings' outcomes begin to emerge below the threshold. Each plot displays local polynomial Regression Discontinuity (RD) estimates with robust bias-corrected confidence intervals following Calonico et al. (2014). *Post-secondary enrollment* is a binary variable equal to 1 if the student enrolls in a higher education institution (HEI) in the academic year immediately following high school graduation. Pre-program outcomes evolve smoothly across the cutoff, while post-program patterns display steeper slopes below the threshold and noticeable shifts above it, suggesting sibling spillover effects that extend beyond the local discontinuity.

5 Empirical Strategy

We leverage the natural experiment created by the *Ser Pilo Paga* (SPP) college financial aid program using a difference-in-differences design. The analysis compares changes in outcomes for younger siblings across three groups of families: (i) those whose older sibling scored above the program's *Saber 11* eligibility cutoff and qualified for financial aid ($T1$); (ii) those whose older sibling scored just below the cutoff and narrowly missed eligibility ($T2$); and (iii) those whose older sibling scored well below the cutoff and thus serve as a control group. By contrasting these groups before and after the launch of SPP in 2014, we isolate the causal impact of having an older sibling eligible—or almost eligible—for college financial aid on younger siblings' educational, labor-market, and behavioral outcomes.
Specifically, we estimate the following first-stage equation:

$$\begin{aligned} SPP^{OS}_{ismt} = \theta_s + \delta_{mt_{os}} + \alpha_1 T1_i + \alpha_2 T2_i \\ + \underbrace{\sum_{\substack{k=2010 \\ k\neq 2013}}^{2020} \boldsymbol{\beta_{1k}} (T1_i \times \delta_{t_{os}=k})}_{\text{Eligible Older Sibling}} + \underbrace{\sum_{\substack{k=2010 \\ k\neq 2013}}^{2020} \boldsymbol{\beta_{2k}} (T2_i \times \delta_{t_{os}=k})}_{\text{Almost-eligible Older Sibling}} + X_i' \times \delta_t + \epsilon_{ismt} \end{aligned} \tag{1}$$

SPP^{OS}_{ismt} is an indicator equal to 1 if the older sibling of child i, enrolled in school s and municipality m, was a recipient of the *Ser Pilo Paga* (SPP) scholarship in cohort t. As part of the first stage, we also estimate the same specification using the older sibling's probability of enrollment in a higher education institution (HEI). Cohort t is defined by the year in which the older sibling took the *Saber 11* exam. Only students who took the exam in 2014 or later were exposed to the SPP program, while earlier cohorts serve as pre–program comparisons. Within eligible cohorts, scholarship assignment depended on whether the older sibling's *Saber 11* score exceeded the SPP cutoff. Accordingly, $T1_i$ identifies children whose older sibling scored above the cutoff (eligible), and $T2_i$ identifies those whose older sibling scored just below it but remained above the 80th percentile of the score distribution (almost eligible). The control group consists of children whose older sibling scored between the 70th and 80th percentiles of the *Saber 11* distribution, well below the eligibility threshold but comparable in academic ability.

Figure 3 illustrates the empirical design and the construction of these treatment and control groups across older-sibling *Saber 11* cohorts. For each cohort from 2010 to 2017, the figure plots the distribution of exam scores normalized by that year's cutoff, highlighting the regions corresponding to the eligible ($T1$), almost–eligible ($T2$), and control groups. Panels corresponding to 2010–2013 are labeled *Before SPP*, and those for 2014 onward as *After SPP*. Because no cutoff existed prior to the introduction of the program, scores for pre-SPP cohorts are normalized using the 2014 eligibility threshold, which provides a consistent benchmark across cohorts.

Section 7 presents robustness checks showing that the results are stable when redefining the control group using alternative percentile ranges (e.g., 60th–80th or 60th–70th percentiles). The terms $\delta_{t_{os}=k}$ are indicators for the older sibling's exam year relative to 2013, the last cohort before the introduction of the program. The coefficients of interest, β_{1k} and β_{2k}, capture the dynamic effects of having an eligible or almost–eligible older sibling across cohorts, relative to 2013 (the pre–program reference year). All specifications include municipality–by–cohort ($\delta_{mt_{os}}$) and school fixed effects (θ_s), and control for baseline family characteristics interacted with cohort fixed effects $[X_i' \times \delta_t]$ to flexibly account for differential outcome trends across families with different observable attributes. Specifically, we interact the number of siblings in the household, mother's education, and baseline socioeconomic status with cohort fixed effects. This specification allows the evolution of younger siblings' outcomes to vary across families with different demographic and socioeconomic profiles. Standard errors are clustered at the family level.

We then estimate the corresponding reduced-form equation:

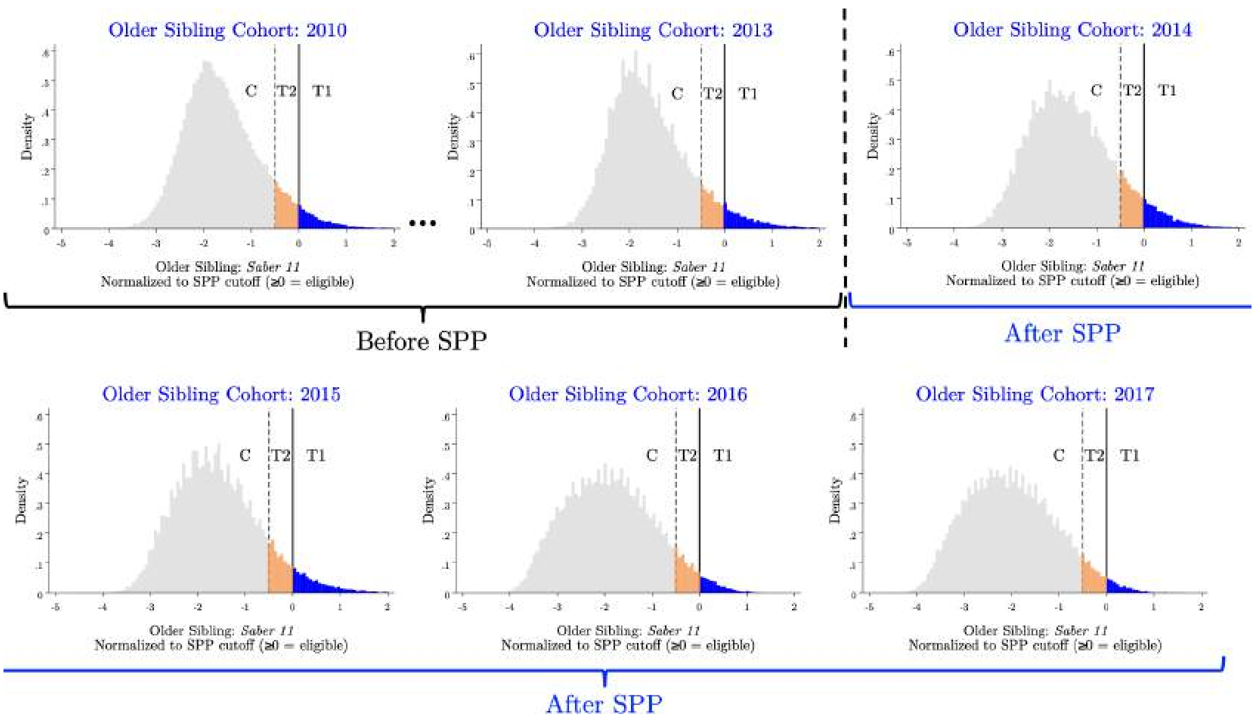
$$\begin{aligned} Y_{ismt} = {} & \theta_s + \delta_{mt_{os}} + \alpha_1 T1_i + \alpha_2 T2_i \\ & + \underbrace{\sum_{\substack{k=2010 \\ k\neq 2013}}^{2020} \boldsymbol{\beta_{1k}} (T1_i \times \delta_{t_{os}=k})}_{\text{Eligible Older Sibling}} + \underbrace{\sum_{\substack{k=2010 \\ k\neq 2013}}^{2020} \boldsymbol{\beta_{2k}} (T2_i \times \delta_{t_{os}=k})}_{\text{Almost-eligible Older Sibling}} + X_i' \times \delta_t + \epsilon_{ismt} \end{aligned} \tag{2}$$

where Y_{ismt} represents the outcome of younger sibling i enrolled in school s and municipality m, whose older sibling took the *Saber 11* exam in cohort t. Y_{ismt} encompasses a wide range of outcomes that capture the younger sibling's educational, labor market, and behavioral trajectories. Educational outcomes include high school graduation, performance on the younger sibling's own *Saber 11* exam, higher education enrollment, the quality of the institution attended, and completion rates. Labor market outcomes include the probability of employment and average earnings from zero to six years after high school graduation. For younger siblings born after 2007, we also study behavioral outcomes, including the likelihood of having a criminal conviction and the type of offense committed (economic, drug-related, or violent crimes).[5] The key identifying assumption does not rely on random assignment

[5] Appendix Section B provides detailed definitions and timing for all outcomes.

of college financial aid among older siblings. Rather, it assumes that, in the absence of the program, the older sibling's higher education enrollment and the younger sibling's outcomes Y_{ismt} would have evolved similarly across older-sibling cohorts for the eligible ($T1_i$), almost–eligible ($T2_i$), and control groups. We begin by estimating Equations (1) and (2) under this assumption and then examine its validity through a series of robustness tests.

Figure 3: Illustration of the Empirical Strategy: Distribution of Older-Sibling *Saber 11* Scores by Cohort.



Note: Each panel shows the distribution of older siblings' *Saber 11* scores, where each score is linked to the younger siblings in the sample whose treatment status is defined by that score. Panels correspond to the cohort in which the older sibling took the *Saber 11* exam, and the distributions are normalized by the relevant year's cutoff. The blue, orange, and gray shaded areas indicate the eligible ($T1$), almost–eligible ($T2$), and control groups, respectively. Panels for 2010–2013 are labeled *Before SPP*, and those for 2014 onward as *After SPP*. Because no cutoff existed prior to 2014, scores for pre-SPP cohorts are normalized using the 2014 cutoff to ensure comparability across cohorts.

6 Results: Sibling Spillover Effects of College Financial Aid

This section presents the estimated effects of the *Ser Pilo Paga* (SPP) program on younger siblings' outcomes. We begin by validating the first stage, showing that eligibility for SPP significantly increases the likelihood that an older sibling in the household enrolls in higher education. We then turn to the reduced-form estimates of the program's impact on younger siblings' educational, labor market, and criminal outcomes.

6.1 First Stage: Changes in Family Exposure to College Enrollment

This section presents the first stage of the empirical design, which estimates how the introduction of the *Ser Pilo Paga* (SPP) program changes the probability that a younger sibling has an older sibling who enrolls in higher education or receives an SPP scholarship. The goal is not to estimate the program's direct effects on the older siblings themselves, but to capture the quasiexperimental variation in family exposure to higher education that underlies the spillover effects analyzed in the following sections. Appendix A reports complementary results for the direct effects on the older siblings.

Table 4 reports post-program effects from a Difference-in-Differences specification that pools all post-2014 cohorts. The interaction terms $T1 \times Post$ and $T2 \times Post$ capture, respectively, the change in the probability that a younger sibling has an older sibling in treatment groups T1 and T2 who enrolled in higher education or received an SPP scholarship, relative to younger siblings in the control group. Figure 10 plots the corresponding event-study coefficients estimated from Equation (1). The figure traces how the probability that a younger sibling has an older sibling enrolled in higher education or receiving an SPP scholarship evolves across cohorts of *Saber 11* test-takers. Consistent with the program timeline, the coefficients remain flat and statistically indistinguishable from zero before 2014 and rise sharply afterward, indicating an increase in younger siblings' exposure to college-going older siblings following the introduction of SPP.

The estimates indicate that program eligibility generated a sharp increase in the likelihood that a household's first-born attended higher education. The probability of having an SPP recipient within the family rose by 20.3 percentage points from a baseline of 0.38, corresponding to an increase of about 53 percent. The expansion in higher-education attendance among eligible households is primarily driven by enrollment in high-quality private universities, consistent with the program's requirement that recipients enroll only in accredited institutions. Students also display a revealed preference for private higher-education institutions (HEIs): the probability of having a first-born enrolled in a high-quality public institution decreased by 6.8 percentage points (about 57 percent relative to baseline). In addition, the probability of enrolling in a four-year program increased by 28.5 percentage points, reflecting a marked shift from shorter two-year programs toward longer academic degrees.

For households whose first-born narrowly missed the eligibility threshold ($T2 \times Post$), the overall increase in higher-education enrollment is smaller—about 2 percentage points, or roughly 5 percent relative to the pre-program mean—but remains statistically significant. At the extensive margin, these families became slightly more likely to send their first-born to higher education. At the intensive margin, however, the composition of institutions attended shifted in a qualitatively different way than among eligible households. The probability that the first-born attended a low-quality institution declined by 2.8 percentage points (a 14 percent decrease from a baseline of 0.20), while the probability of attending a high-quality institution increased by 4.8 percentage points (a 26 percent increase from a baseline of 0.18). This shift is particularly pronounced for high-quality public universities, where the probability increased by 3.4 percentage points—equivalent to a 28 percent rise relative to the pre-program mean of 0.12.

These patterns are consistent with a reallocation of students across the quality distribution of institutions. Once high-performing students gained access to high-quality private universities through SPP, competition for places in top public institutions likely relaxed, allowing slightly lower-performing students to enter. Descriptive evidence in Appendix C.1 supports this interpretation. Panel A of Figure C.1 shows that, after 2014, students with above-average *Saber 11* scores shifted from public and low-quality private universities toward high-quality private institutions. Panel B shows that students with below-average scores became more likely to attend high-quality public universities over the same period. This analysis is descriptive but provides a useful complement to the first-stage results.

The estimates suggest that the introduction of SPP substantially increased the probability that a household's first-born entered higher education, particularly at high-quality private universities. Almost-eligible households also became somewhat more likely to have a college-going first-born, primarily through a reallocation from low- to high-quality institutions. These shifts in family exposure to higher education constitute the first-stage variation exploited in the next section to identify the program's spillover effects on younger siblings' educational, labor-market, and behavioral outcomes.

6.2 Main Results: Reduced-Form Effects on Younger Siblings

Academic Performance. We begin by examining the effects of the *Ser Pilo Paga* (SPP) program on younger siblings' secondary school attainment and academic performance. Table 5 reports Difference-in-Differences estimates comparing younger siblings whose older sibling was eligible ($T1$) or almost eligible ($T2$) for SPP to those in the control group. The probability of completing high school remains effectively unchanged after the introduction of SPP. Although the coefficients are statistically significant due to the large sample size, their magnitudes are very small (–0.3 and –0.2 percentage points for $T1 \times Post$ and $T2 \times Post$, respectively), indicating no meaningful change in high school graduation rates. This suggests that having an older sibling who qualified—or nearly qualified—for college financial aid did not influence the extensive margin of completing secondary education, consistent with already high baseline graduation rates in this population.

In contrast, younger siblings display improvements in academic achievement as measured by their standardized Saber 11 exam scores. Younger siblings of eligible older siblings score 0.079 standard deviations higher on the exam, with similar effects across subject areas: 0.078 s.d. in mathematics and 0.087 s.d. in Spanish. Figure 11 presents the event-study coefficients estimated from Equation (2). In the pre-program period (prior to 2014), the coefficients are flat and not statistically different from zero, suggesting no detectable trend in test performance before the introduction of the program. After the program's implementation in 2014, we observe an increase in test performance. The magnitudes are meaningful. According to Matthew A. Kraft, who compiled over 700 randomized evaluations of education interventions, effect sizes between 0.05 and 0.20 standard deviations represent moderate but meaningful improvements in learning. Given that younger siblings are not direct beneficiaries of the program, these gains are sizable and consistent with strong indirect spillovers within households. Younger siblings of almost-eligible older siblings also perform better, although the effects are smaller in magnitude. Their overall Saber 11 scores increase by 0.048 s.d., with gains of 0.045 s.d. in mathematics and 0.049 s.d. in Spanish. The presence of positive but attenuated effects among this group suggests that information or motivation channels operate even in the absence of direct financial aid to the household. Overall, the introduction of the program did not affect whether younger siblings completed high school, but it substantially improved their academic performance at the end of secondary education. The results indicate that greater exposure to higher-education opportunities within the family—whether through an eligible or almost-eligible older sibling—raised younger siblings' educational aspirations and effort, producing measurable learning gains comparable in size to those observed in direct education interventions.

Postsecondary Enrollment and College Quality. We next examine whether exposure to an older sibling who is eligible or almost-eligible for *Ser Pilo Paga* (SPP) translates into changes in the younger sibling's transition to higher education. Table 7 reports the Difference-in-Differences estimates for postsecondary outcomes, and Figure 12 together with

Table 6 (Panels A–D) present the corresponding event-study coefficients by sector and institutional quality.

For younger siblings in T1 households (older sibling eligible for SPP), we find a roughly 3-percentage-point increase in the probability of postsecondary enrollment relative to a pre-programme mean of 31.5 percent, corresponding to about a 9 percent increase. Within this group, the gains are concentrated in high-quality institutions rather than across all institutions. Columns (3)–(8) of Table 7 distinguish enrollment by low- versus high-quality HEIs and by public versus private institution. Enrollment in high-quality HEIs rises by approximately 3.7 percentage points (around 22 percent above baseline), driven primarily by high-quality private universities, where the increase is about 3.9 percentage points—approximately a 57 percent rise relative to their baseline share of 6.9 percent. By contrast, we find no significant change in enrollment at low-quality institutions. Moreover, Columns (9)–(10) reveal a shift in programme duration: exposure to SPP increases the probability of enrolling in four-year programmes by about four percentage points (roughly a 21 percent increase relative to baseline) and reduces enrollment in two-year programmes by about one percentage point (around a 9 percent decline). These patterns suggest that the programme not only raises overall enrollment probabilities for younger siblings but also tilts their choices toward longer, more ambitious academic trajectories.

In T2 households (older sibling almost-eligible for SPP), the estimated effects on younger siblings' overall enrollment are smaller and often not statistically different from zero. However, we observe a statistically significant increase of about 0.8 percentage points (approximately 7.8 percent of the pre-programme mean) in enrollment at high-quality public HEIs. This pattern—modest changes in overall enrollment, but a focused increase in high-quality public institutions—aligns with the idea that even without direct financial eligibility, younger siblings still benefit via aspiration or informational spillovers through their older siblings' experience.

Field of Study, Sibling Following Behavior, and Graduation We next explore how exposure to an older sibling eligible or almost eligible for *Ser Pilo Paga* (SPP) shapes younger siblings' postsecondary choices and trajectories once they enroll. Table 8 summarizes the estimated effects across three domains: (i) field of study, (ii) following behavior and location choices, and (iii) graduation outcomes. Younger siblings in eligible households (T1) are more likely to choose selective and technical fields of study, particularly in science, technology, engineering, and mathematics (STEM). Columns (1)–(2) show that exposure to an eligible older sibling increases the probability of enrolling in a STEM or STEM-plus program by

1.7 and 2.4 percentage points, respectively—equivalent to 14 and 9 percent of their baseline means. These effects are sizable, especially given that the younger siblings were not directly treated by the program. In contrast, we find no significant changes in the likelihood of pursuing arts or social sciences, suggesting that the program's spillovers reinforced academic orientations toward more demanding and higher-return fields. For younger siblings in almost-eligible households (T2), the coefficients are smaller and not statistically different from zero.

Beyond field of study, we observe meaningful shifts in how younger siblings choose where and what to study. Column (6), and Figure 13, show that younger siblings in T1 households are 2.6 percentage points more likely to enroll in a higher education institution located outside their home municipality—a 32 percent increase relative to baseline. This spatial mobility mirrors the behavior of their older siblings, who often relocated to attend high-quality private universities concentrated in major urban areas. At the same time, younger siblings are 1.7 percentage points less likely to attend the same institution as their older sibling, even though they are 1.7 percentage points more likely to enroll in the same field of study. This pattern reflects the structure of opportunity created by SPP: the program granted older siblings access to Colombia's most selective and expensive universities—institutions that younger siblings, lacking financial aid, could rarely afford to enter. Instead, they appear to emulate their older siblings' academic choices within the constraints of their own resources, pursuing similar fields at more accessible institutions.

Finally, we turn to graduation outcomes, reported in Columns (9)–(11). Younger siblings in T1 households are modestly more likely to complete higher education. The probability of graduating from any postsecondary institution increases by 1.8 percentage points (roughly 3 percent of the mean), with stronger effects for university completion (4.1 p.p., 10 percent increase) and a corresponding decline in graduation from technical or vocational programs (-3.2 p.p., 10 percent decrease). These shifts are consistent with earlier findings showing a move toward longer and higher-quality academic programs. For T2 households, coefficients remain small and statistically insignificant across all graduation outcomes, suggesting that the main spillover effects are concentrated among younger siblings of scholarship recipients.

Taken together, these results indicate that exposure to a college-going, SPP-eligible older sibling not only raises the likelihood that younger siblings pursue higher education but also influences how they navigate it—affecting their choice of field, location, and persistence through completion. The evidence points to within-family transmission of aspirations and information, where exposure to opportunity through one sibling reshapes the educational trajectories of the next generation within the same household. Section X will explore more this mechanism.

Labor Market Outcomes. We next turn to labor-market outcomes to assess whether the educational gains documented above translate into differential employment or earnings trajectories for younger siblings. Table 9 reports Difference-in-Differences estimates for three sets of outcomes: (i) formal employment (Panel A), (ii) total earnings measured in multiples of the minimum wage (Panel B), and (iii) the probability of being in the top quartile of the earnings distribution (Panel C). Each column corresponds to a separate regression tracking outcomes zero to six years after high school graduation. Figure 14 complements these estimates by plotting the corresponding event-study coefficients for each outcome.

For younger siblings in eligible households (T1), we observe short-run declines in formal employment immediately after high school. During the first three to four years, the probability of holding a formal job falls by roughly 1–2 percentage points relative to younger siblings in control households, with the largest decline—about 2.4 p.p.—four years after graduation. These short-run reductions in employment coincide with the period when most younger siblings are enrolled in HEIs, consistent with delayed labor-market entry rather than weaker employability. From year 5 onward, the trend reverses: the coefficients begin to increase and become positive, though they are not statistically significant. This U-shaped pattern—initial declines followed by a gradual recovery—is consistent with a shift in time allocation from work to education during the schooling years and a subsequent reentry into the labor market once those studies conclude. For younger siblings in almost-eligible households (T2), the estimated effects are smaller and less precisely estimated, mirroring the weaker educational responses observed earlier.

Earnings dynamics in Panel B display a similar U-shaped trajectory. Younger siblings in T1 households earn less than their counterparts in the control group in the first few years after graduation, with statistically significant reductions around one and four years after high school. Starting in year 5, the estimates turn positive, suggesting a potential reversal consistent with the completion of tertiary education and entry into higher-paying jobs. However, these late-period gains are not statistically significant, and our data do not yet allow us to observe longer-run outcomes beyond six years. For younger siblings in T2 households, the estimated coefficients are close to zero across all years, indicating no meaningful changes in earnings trajectories.

Panel C focuses on the probability of earning within the top quartile of the earnings distribution. The coefficients again follow a U-shaped profile: negative effects around years three and four (–0.8 to –1.4 p.p.) gradually turn positive after year five, though none reach levels of statistical significance. This pattern reinforces the interpretation that the short-run differences reflect prolonged schooling spells rather than worse labor-market prospects. Taken together, the results suggest that the main spillover mechanism operates through education rather than immediate labor-market outcomes. Exposure to an older sibling who gains access to college through SPP induces younger siblings to postpone entry into formal employment and earnings accumulation while pursuing longer and higher-quality degrees. The resulting short-run declines in employment and earnings are temporary and consistent with higher educational investment. By year five, the coefficients shift upward, suggesting that any labor-market penalties dissipate as cohorts age and begin to enter the workforce. Because the available data only allow us to observe up to six years after high school completion, it remains an open question whether these educational investments ultimately translate into long-term labor-market gains.

Crime Outcomes We conclude by examining whether expanding access to higher education through *Ser Pilo Paga* (SPP) affects the likelihood that younger siblings engage in criminal activity. Education and crime are closely linked in canonical models of the economics of crime (Becker, 1968), which predict that improved educational opportunities reduce criminal behavior by raising the opportunity cost of illegal activity. Empirically, identifying this relationship is challenging because education, income opportunities, and local crime conditions are jointly determined. Our setting provides a rare opportunity to overcome this endogeneity by exploiting quasi-experimental variation in educational access generated by the distribution of SPP scholarships to high-achieving low-income students.

Table 10 reports the Difference-in-Differences estimates for younger siblings' criminal behavior, and Figure 15 shows the corresponding event-study coefficients from Equation (2). The estimates indicate that exposure to an eligible older sibling ($T1$) leads to a statistically significant decline in the probability of committing a crime. The likelihood that a younger sibling is ever convicted of any offense falls by 0.20 percentage points from a baseline mean of 0.45 percent—a 45 percent reduction relative to the pre-program average. In our sample, this corresponds to roughly two fewer convictions per 1,000 younger siblings. This magnitude is substantial when compared with benchmark crime-prevention interventions that directly target at-risk youth, underscoring the broader social returns of expanding educational opportunity within families (cite).

Breaking down by type, the reductions are concentrated in drug-related and violent crimes, which fall by roughly 120 percent and 70 percent relative to their respective baselines. By contrast, effects on economic and other offenses are not statistically different from zero.

This pattern is informative: if the mechanism were driven primarily by changes in income, reporting, or local enforcement, we would expect to see declines in economic crimes as well. Instead, the results point to a reduction in risky or aggressive behavior, consistent with higher opportunity costs of time, improved self-control, and stronger aspirations among exposed cohorts.

In contrast, there are no detectable effects for younger siblings in almost-eligible ($T2$) households. This asymmetry likely reflects differences in exposure intensity. Older siblings in T2 families narrowly missed the scholarship and thus did not experience the financial relief or institutional access that SPP guaranteed to recipients. While some may have enrolled in college, the absence of a visible change in educational trajectories or family circumstances limited the potential for spillovers. By contrast, T1 students entered some of the most selective and costly universities in the country, creating a salient example of upward mobility and academic success that could meaningfully reshape younger siblings' expectations and behavior. The magnitude and composition of these effects indicate that the benefits of educational programs such as SPP extend beyond academic outcomes, generating broader social externalities in crime prevention.

7 Heterogeneity and Robustness

7.1 Heterogeneity in Spillover Effects

We analyze whether the spillover effects vary across different types of families. If the mechanism operates through role-model or motivational channels, we might expect effects to be stronger when the "role model" is more salient or relatable to the younger sibling—for instance, when siblings are close in age, share the same gender, or live together during adolescence.

Importantly, the *Ser Pilo Paga* (SPP) program shifts the extensive margin of higher-education participation for older siblings. For eligible households, the older sibling transitions from a counterfactual in which they would not have enrolled in any tertiary institution to one in which they attend a high-quality university—often outside the home municipality. This shift does not necessarily relax household financial constraints. On the contrary, even though SPP covers tuition and offers a stipend toward living expenses, families commonly face new costs related to housing, transportation, and study materials. Moreover, anecdotal reports and administrative descriptions of the program suggest that the stipend is often insufficient to fully cover rent or relocation costs. In this sense, program-induced college enrollment can tighten rather than relax household budgets, especially among poorer families. It can also trigger intrahousehold reallocation of time and attention toward the college-going child. Prior work documents that in low-SES households, intrahousehold competition over scarce resources can be first order (Carneiro et al., 2023), and that adolescents in low-income environments often contribute to household income or caregiving activities (Edmonds and Schady, 2012; Soares et al., 2012). When an older sibling leaves for college, younger siblings may face higher domestic or labor responsibilities, potentially crowding out their own human-capital investment (Lafortune and Lee, 2014).

This context implies two things. First, we should not interpret SPP as straightforwardly alleviating liquidity constraints for the household. The baseline is not "a student who would have enrolled anyway but now pays less," but rather "a student who would not have enrolled at all, who now enrolls and must be supported." Second, if we observe improvements in younger siblings' schooling and reductions in criminal behavior following an older sibling's enrollment, those gains do not automatically imply that resources were reallocated *toward* the younger sibling. In fact, in the poorest households, the opposite pressure may be present: the younger sibling may bear some of the cost of the older sibling's departure. This motivates testing whether spillovers differ systematically by socioeconomic status (SES), household structure, or parental background.

To assess these possibilities, we estimate Equation (2) separately for a broad set of subgroups and compare the resulting treatment effects. Figures 16–18 plot the estimated coefficients for the eligible group ($T1 \times Post$) in the left panels and the almost-eligible group ($T2 \times Post$) in the right panels. Each coefficient comes from a separate regression run within the indicated subgroup. The horizontal axis reports the estimated effect size, and the vertical axis lists the subgroup definition.

We consider heterogeneity along fourteen dimensions that capture variation in family composition, socioeconomic conditions, and local opportunity structures: (i) all households, (ii) age gap between siblings below four years, (iii) age gap above four years, (iv) same-gender siblings, (v) opposite-gender siblings, (vi) less-poor households, (vii) poorest households, (viii) stay-at-home–mother households, (ix) working-mother households, (x) mother with low versus high education, (xi) father with low versus high education, (xii) households in which neither parent has postsecondary education, (xiii) municipalities with versus without a high-quality local higher-education institution (HEI), and (xiv) households where the older sibling worked during their senior year of high school. These dimensions allow us to ask whether the spillovers are stronger when (a) the sibling is a more plausible role model (same gender, small age gap, co-residence), (b) the household faces tighter resource constraints (poorest

SES, working mother, older sibling working in high school), or (c) local access to higher education is more limited (no accredited HEI nearby, implying older sibling migration).

Across outcomes, the effects are homogeneous.

First, for the probability that the younger sibling takes the national high-school exit exam (*Saber 11*), the estimated coefficients are small and statistically similar across all subgroups. Exposure to an eligible older sibling does not differentially affect the decision to sit for the exam in families with different SES levels, parental education, gender composition, or sibling age gaps. Conditional on taking the exam, the improvements in performance—discussed in Section 6—also appear broadly similar across subgroups.

Second, when we examine younger siblings' *Saber 11* test scores, we again find a high degree of stability. The gains in academic performance are not concentrated among families with smaller age gaps or same-gender siblings, which would be expected if the channel were purely aspirational through a highly "relatable" role model. Nor are they concentrated among households with more educated parents, which one might expect if the channel were primarily informational (e.g., better ability to help interpret college opportunities). Instead, the estimated effects for the eligible group ($T1$) cluster around the overall mean for nearly every subgroup we examine.

Third, we observe a similar pattern for immediate postsecondary enrollment of the younger sibling. Eligibility of the older sibling is associated with higher probabilities of enrolling in higher education across virtually all subgroups. Notably, we do not see larger effects in households classified as less poor. Nor do we see systematically weaker effects in the poorest households, despite the possibility that those households experience the greatest short-run resource pressure when the firstborn leaves for college. The estimated effects also remain stable in municipalities without nearby accredited universities—where the older sibling would have had to migrate to study—suggesting that geographic barriers and the need to finance relocation do not eliminate the spillover.

Finally, we see no clear differential pattern in households where the older sibling worked during high school. If the mechanism were primarily about redistributing financial slack that used to come from the older sibling's earnings, we would expect different behavior in these households. We do not observe that.

Taken together, these results suggest that the spillover effects are broadly shared across households. We find no evidence that they are concentrated only among families with greater baseline resources, under narrower sibling configurations, or in places with easier local access to higher education. The stability of the effects across SES groups is especially informative:

if anything, the poorest households are the ones for whom sending the firstborn to college is most likely to tighten budgets and reassign caregiving or work duties to younger siblings. Yet we continue to observe positive effects on schooling and reductions in crime for those younger siblings.

Overall, the heterogeneity results indicate that the observed spillovers are not driven by a narrow set of advantaged households, nor by a single "high-exposure" sibling profile. Instead, they are consistent with a mechanism that generalizes across diverse family structures and constraint levels—one in which the older sibling's college enrollment changes the younger sibling's beliefs about what is possible, valuable, and expected, rather than one in which the household simply reallocates financial or time resources toward (or away from) the younger child.

7.2 Robustness: Machine-Learning–Based Definition of Comparison Groups

The baseline empirical strategy relies on the institutional rules of the *Ser Pilo Paga* (SPP) program. Conditional on being a member of a household eligible based on *need*, eligibility for financial aid is determined by whether the older sibling's *Saber 11* score exceeds the official threshold in a given cohort. The assignment into treatment is not modeled, predicted, or learned: all students above the cutoff are mechanically classified as *Eligible (T1)*. This categorization is taken as exogenous input to the empirical design.

The key challenge arises in defining appropriate comparison groups among *untreated* students. While program rules determine eligibility, there is no institutional analog to the "almost-eligible" or "control" groups. In the main analysis, these comparison groups are constructed using percentile bands, e.g., siblings whose older sibling scored between the 80th–*eligibility cutoff* percentile, the *Almost-Eligible* (T2) families, and the 70th–80th percentile, the *Control* group. These ranges were selected based on observed changes in the distribution of academic outcomes around the 80th percentile after the implementation of the program in 2014. Specifically, we find that younger siblings of students scoring just below the eligibility cutoff exhibit different levels and trends in key outcomes compared to their pre-SPP counterparts, suggesting a structural break in the data generating process following the introduction of the policy. Nonetheless, these choices are not dictated by the program itself and involve researcher discretion.

To evaluate the sensitivity of the results to the choice of percentile-based cutoff bands, we implement an alternative approach that leverages machine learning tools to define groups among untreated older siblings. The idea is simple: we retain the official eligible group, defined by the program's cutoff, and allow the data to guide the classification of untreated students. Specifically, we use clustering techniques to identify which untreated students most closely resemble the eligible group, thereby defining an "*Almost-Eligible (T2)*" group in a data-driven manner. The algorithm then assigns the remaining students to a broader control group composed of individuals who are not as similar to the eligible population but still represent a reasonable comparison group. This clustering-based procedure serves as a robustness check to ensure that the main findings are not driven by arbitrary definitions of percentile ranges but instead remain consistent under a technically grounded, data-driven classification strategy.

Step 1: Constructing a One-Dimensional Index via Linear Discriminant Analysis (LDA). The first step summarizes pre-treatment characteristics into a single index measuring similarity to the eligible population. To construct this index, we estimate a Linear Discriminant Analysis (LDA) model using observable characteristics of older siblings. Let $\boldsymbol{X}_i$ denote a vector of baseline covariates (test score, age, gender, parental education, socioeconomic status, school characteristics, and municipality fixed effects), and let

$$D_i = \begin{cases} 1 & \text{if older sibling is eligible for SPP} \\ 0 & \text{otherwise.} \end{cases}$$

LDA seeks a linear combination of covariates

$$\text{LDA Score}_i = \boldsymbol{w}'\boldsymbol{X}_i$$

that best separates treated from untreated observations. Formally, LDA chooses $\boldsymbol{w}$ to maximize the ratio of between-group to within-group variance:

$$\max_{\boldsymbol{w}} \quad \frac{\left(\boldsymbol{w}'(\boldsymbol{\mu}_1 - \boldsymbol{\mu}_0)\right)^2}{\boldsymbol{w}'\boldsymbol{\Sigma}\boldsymbol{w}},$$

where $\boldsymbol{\mu}_1$ and $\boldsymbol{\mu}_0$ are the group means and $\boldsymbol{\Sigma}$ is the pooled within-group covariance matrix. The resulting one-dimensional index assigns higher values to untreated students who resemble the eligible population along observable dimensions. Importantly, LDA does *not* modify the treatment indicator. Eligibility is pre-determined by institutional rules, LDA summarizes similarity only among untreated units.

Step 2: Clustering Untreated Units in LDA Space. With each student mapped to an LDA score, we focus exclusively on untreated students ($D_i = 0$). These observations are grouped using K-means clustering applied to their LDA scores:

$$\min_{\{C_k\}} \sum_{k=1}^{K} \sum_{i \in C_k} \|z_i - \mu_k\|^2,$$

where z_i is the LDA score for student i, and μ_k is the centroid of cluster k.

This procedure partitions untreated students into K clusters along the similarity dimension. Since the LDA score is one-dimensional, clustering is transparent and interpretable. To map clusters to empirical categories, we compare each cluster's mean LDA score to that of the eligible group. The cluster with centroid closest to the treated mean is labeled "Almost-Eligible", the next closest is labeled "Control"; remaining clusters are labeled "Other". Treated students retain their mechanically assigned category.

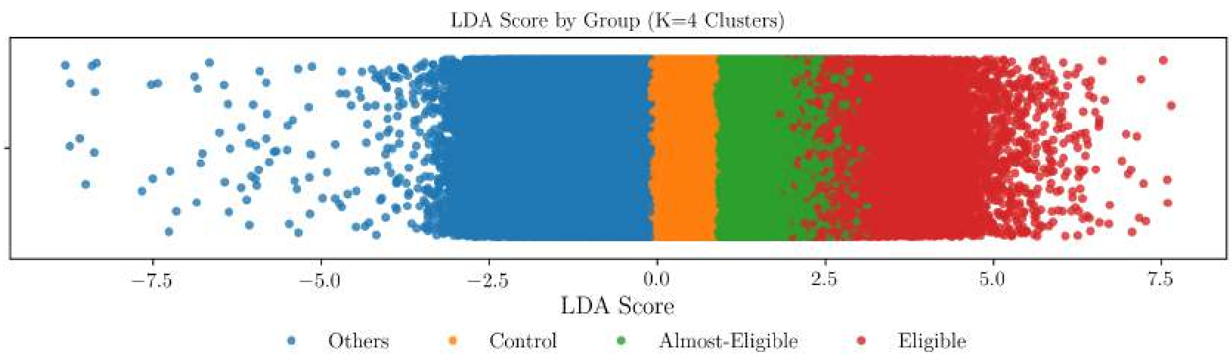
Step 3: Choosing the Number of Clusters. To determine K, we implement the *Elbow Method*: plotting the within-cluster sum of squares (WCSS) against K and looking for the largest decline before marginal improvements diminish. Formally, for $K = 1, 2, \ldots, K_{\max}$,

$$\text{WCSS}(K) = \sum_{k=1}^{K} \sum_{i \in C_k} \|z_i - \mu_k\|^2.$$

As shown in Appendix Figure D.2, the curve exhibits an "elbow" at $K = 4$, which we use in the baseline robustness classification.

Step 4: Visualizing Machine-Learned Group Structure. Figure 4 plots the LDA scores of all observations, colored by their machine-assigned groups. The Eligible group (T1) occupies the upper range of the similarity distribution by construction. Immediately below lies the machine-identified "Almost-Eligible" group (T2), followed by "Control" and a large residual category. The visualization illustrates that the data-driven groups are coherent and monotonic in similarity to treated units, supporting interpretability.

Figure 4: Distribution of LDA Scores by Machine-Learning–Defined Groups (K = 4)



Note: The figure plots each observation's LDA score and colors individuals based on their group assignment using K-means clustering among untreated observations. "Eligible" students are determined mechanically based on SPP program rules (score above yearly cutoff). "Almost-Eligible" and "Control" groups are the clusters whose mean LDA score is closest and second-closest to that of eligible students, respectively; remaining clusters form the "Other" group.

Step 5: Re-estimating Spillover Effects. We re-estimate the main event-study and difference-in-differences models replacing the percentile-based comparison groups with the machine-learned definitions. The estimated spillover effects for younger siblings remain quantitatively similar. Tables D.8 and D.9 provide estimates for representative outcomes. This robustness shows that the primary results are not an artifact of how ineligible groups are defined. This exercise preserves the institutional assignment of treatment while removing researcher discretion in constructing comparison groups among untreated students. The close correspondence between main and machine-learning–based estimates reinforces the validity of the baseline empirical design.

7.3 Robustness Check: Alternative Control Group Definitions

This section tests how sensitive the results are to alternative control groups. Table 14 reports estimates for younger siblings' performance on the Saber 11 exam. The table includes participation, overall scores, and domain-specific results. Panel A shows the baseline results. The control group is younger siblings of students who scored between the 70th and 80th percentiles on the Saber 11 exam. These students are similar in observable characteristics to the treated groups but are far enough from the program cutoff to be unaffected by it. Panel B uses an alternative control group. Here, the control group is siblings of students who scored between the 60th and 70th percentiles. The estimated effects are slightly larger. This suggests that if some spillovers reach families near the cutoff, the baseline estimates represent a lower bound. Panel C repeats the analysis using siblings of students in the 50th to 60th percentile as the control group. Results remain similar to those in the baseline. The effects on test participation and scores are consistent across all control definitions. The results confirm that the main estimates are stable to reasonable variations in the comparison group.

We next repeat this exercise for higher education enrollment outcomes. Table 15 reports the estimated effects for the probability that younger siblings enroll in higher education and the type of institution and program they attend. Panel A presents the baseline results, where the control group consists of younger siblings of students who scored between the 70th and 80th percentiles on the Saber 11 exam. Panels B and C redefine the control group to the 60th–70th, and the 50th–60th percentiles, respectively. The results remain stable across samples. Younger siblings of eligible students are consistently more likely to enroll in higher education and to attend high-quality institutions. When the control group shifts to students in the 60th–70th percentile, the estimated effects increase slightly, suggesting that if some spillovers extend to families near the cutoff, the baseline provides a lower bound of the true effect.

When we use the 50th–60th percentile as the control group, a new pattern emerges. The probability of enrolling in low-quality institutions, particularly public ones, declines, and enrollment in two-year programs also decreases. This difference reflects who the control students are. Students in the 50th–60th percentile are weaker and more likely to select into low-quality public institutions or shorter programs. In contrast, families near the top of the distribution—those in the 70th–80th percentile—are academically stronger and already inclined toward higher-quality and longer programs. Comparing T1 and T2 households to this stronger control group compresses the difference between them, yielding smaller and sometimes statistically indistinguishable gaps. The baseline estimates are thus conservative, reflecting the proximity of the control group to the treatment threshold. Using lower-scoring controls broadens the comparison and makes the reallocation toward high-quality and four-year programs more visible. Overall, the results for higher education enrollment confirm that the estimated spillovers are robust to alternative definitions of the control group and, if anything, are understated in the baseline specification.

8 Mechanisms: Intangible Versus Resource Spillovers

The results so far show that younger siblings of students eligible for *Ser Pilo Paga* (SPP) experience educational gains and a reduction in criminal behavior, even though they are not directly treated by the program. This section explores the mechanisms behind these intrahousehold spillovers.

We conceptualize two broad channels through which older siblings' eligibility for SPP could affect their younger siblings. The first involves *resource-based mechanisms*, typically understood as changes in household liquidity or financial relief when one child receives a full scholarship. However, in the low-income context we study, this interpretation must be revisited. Because SPP primarily affects the *extensive margin* of higher-education participation—enabling students who otherwise would not have gone to college to enroll—the program often removes a productive member from the household. Prior to SPP, many firstborns contributed to family income or caregiving. When they leave for college, often in another city, the household loses both labor and time resources and may even face additional costs associated with relocation and support. Thus, rather than relaxing financial constraints, the program may temporarily *tighten* them.

The second channel encompasses *intangible mechanisms*—changes in information, aspirations, or motivation triggered by observing an older sibling succeed academically and access new opportunities. Because both types of mechanisms may operate simultaneously, we exploit the contrast between the eligible ($T1$) and almost eligible ($T2$) groups to gauge their relative importance. $T1$ households experience both the financial and informational shocks associated with SPP take-up, while $T2$ households experience only the informational component—exposure to the same eligibility threshold and expectations of potential access, but without the scholarship resources themselves. The next subsection examines the role of resource-related mechanisms in this low-income context before turning to the evidence for motivational and informational spillovers.

8.1 Resource Mechanism

If household liquidity or income relief were a key driver of spillovers, we would expect stronger effects among families facing tighter financial constraints. The scholarship could, in principle, alleviate binding credit or liquidity limits, freeing resources that might benefit younger siblings—for instance, by improving living conditions or school inputs. However, in the context of low-income households, the expected sign of this "resource mechanism" is not straightforward. In this setting, the program primarily affects the *extensive margin* of college attendance: it enables students who would otherwise not have enrolled in higher education to do so. Prior to the introduction of *Ser Pilo Paga* (SPP), most eligible firstborns were unlikely to pursue college and often contributed economically or through caregiving within the household. In fact, 16 percent of older siblings report being employed during their senior year of high school.

In developing countries, many adolescents participate in income-generating or caregiving activities that support the family economy (Edmonds and Schady, 2012; Soares et al., 2012). When the firstborn attends college, the younger siblings may need to compensate by increasing domestic or market work, potentially reducing their time available for schooling or study (Lafortune and Lee, 2014). The household may also need to provide financial or logistical support to help the student succeed, such as covering rent, transportation, and relocation costs. Thus, rather than relaxing financial constraints, the program can initially *tighten* them, as families reallocate scarce resources toward the college-going child.

Heterogeneous analyses by baseline socioeconomic status show no evidence of stronger spillover effects among different SES households, as would be expected if liquidity relief were the dominant channel or if resource reallocation happens from one child to another. The educational and behavioral gains for younger siblings are of similar magnitude across the income distribution, suggesting that the program's effects do not arise through household income or liquidity improvements.

8.2 Sibling Academic Success and the Strength of Spillovers

To further explore the mechanisms behind the spillover effects, we examine whether the results depend on the older sibling's experience in higher education. Specifically, we use administrative records from one of Colombia's leading private universities, which enrolled more than one-tenth of all *Ser Pilo Paga* (SPP) recipients nationwide. We link information on older siblings enrolled in this university between 2010 and 2019 to their younger siblings identified in the *Sisben* registry. Because all older siblings in this exercise attend the same institution, families face comparable tuition schemes, financial aid arrangements, and academic environments. This setting therefore provides a useful opportunity to hold constant differences in the *resource channel*—since all students have similar access to institutional and financial resources—and to isolate variation arising from the older sibling's academic performance. The analysis allows us to explore whether younger siblings respond differently when their older sibling performs well in college, consistent with an intangible mechanism.

We estimate Equation (2) on the restricted sample, interacting the treatment term ($T1 \times Post$) with an indicator for whether the older sibling's first-semester grade point average (GPA) was above the 75th percentile of the cohort distribution. This interaction shows whether the spillover effect varies with the academic success of the older sibling, holding constant the college environment. Table 11 reports results for younger siblings' performance in the *Saber 11* examination. The coefficient on $T1 \times Post$ is small and statistically insignificant, suggesting that eligibility alone does not affect younger siblings' academic outcomes within this subset of families. However, when the older sibling performs well in college, the interaction term becomes positive and significant across all subjects. Younger siblings of academically successful older siblings score between 0.25 and 0.32 standard deviations higher in math, language, and overall performance. Following the benchmarks proposed by Kraft (2020), these magnitudes fall within the medium-to-large range for educational interventions. Because this comparison is conducted within a single institution—where financial and academic conditions are similar for all families—the results are consistent with the idea that the spillovers operate through non-monetary channels, such as changes in motivation, aspirations, or perceptions of attainable success, rather than through material resources.

Table 12 turns to postsecondary outcomes for younger siblings. Consistent with the results above, the main eligibility effect is small and not statistically different from zero. In contrast, younger siblings of academically successful older siblings are around 18 percent more likely to enroll in any higher-education institution and similarly more likely to pursue four-year degree programs. The gains are concentrated in high-quality private universities (about 22–23 percent relative to baseline), with no evidence of substitution toward shorter or lower-quality programs. These results reinforce the interpretation that younger siblings respond to the visible success of their older sibling by setting higher academic goals and investing more effort in education.

Overall, these findings provide suggestive evidence that family spillovers materialize primarily when the older sibling's college experience is successful. Eligibility alone is not sufficient to generate meaningful changes in the behavior or educational trajectories of younger siblings. Instead, the results are consistent with a *role-model mechanism*, in which observing a sibling succeed in higher education shapes expectations and aspirations within the household. Because this exercise compares families whose older sibling attends the same university under similar resource conditions, the findings suggest that the effects we document are more likely to operate through motivational or informational channels rather than through direct financial transfers.

8.3 Early Schooling Outcomes and the Timing of Spillovers

A final piece of evidence on the mechanisms driving these effects comes from examining younger siblings' performance much earlier in their schooling careers. To do so, we use data from the national standardized exams *Saber 3*, *Saber 5*, and *Saber 9*, administered annually by ICFES, Colombia's national testing authority. While these exams are conducted every year, individual-level administrative records are only available for the 2017 round, which covers over 600,000 students nationwide. For that year, we can link each child to their older sibling in our main sample and observe whether the older sibling took the high-school exam (*Saber 11*) before or after the introduction of the *Ser Pilo Paga* (SPP) program in 2014. This setup allows us to estimate the same Difference-in-Differences specification as in Equation (2), exploiting variation in exposure across cohorts.

This analysis provides a useful opportunity to assess whether the spillover effects operate only around the time when younger siblings make college decisions, or whether they emerge much earlier—consistent with changes in motivation or effort throughout schooling. Because the students taking these exams are in primary and lower-secondary education, they are several years away from the stage where college-related information or direct financial transfers could plausibly influence their behavior. Any effects observed at these ages therefore speak more directly to intangible mechanisms, such as shifts in aspiration, effort, or engagement with school.

The final sample includes approximately 8,000 younger siblings taking *Saber 3* (average age 8.8), 14,000 taking *Saber 5* (average age 10.9), and 20,000 taking *Saber 9* (average age 15.2). Figure D.1 plots the distribution of younger siblings in our sample taking each of these exams in 2017. The larger number of observations at higher grades reflects the average spacing between siblings in the data—the mean age gap between the older and younger child is about 5.7 years—so a greater share of families have a younger sibling in ninth grade in 2017 than in earlier grades.

Table 13 reports the estimated coefficients for younger siblings' academic performance and effort-related behaviors. We observe consistent positive effects for the eligible group ($T1 \times Post$) across all grades, particularly in Math and Spanish, with magnitudes around 0.18–0.27 standard deviations in *Saber 3* and 0.13–0.21 standard deviations in later grades. These gains are already visible as early as third grade, when children are roughly nine years old, indicating that the influence of older siblings' exposure to SPP extends well before any college-related decision-making horizon. In addition, we find small improvements in punctuality and, to a lesser extent, attendance: the probability of reporting "never late" increases by around seven percentage points in fifth grade, while effects on perfect attendance are smaller and not statistically significant. For the almost-eligible group ($T2 \times Post$), effects are weaker and less precisely estimated, consistent with the broader pattern of smaller spillovers among near-eligible families found in earlier sections.

The timing and nature of these results provide evidence consistent with an intangible mechanism. Because the effects emerge at very early ages—long before exposure to information about university admissions or financial aid—it is unlikely that they reflect higher education information or financial channels. Instead, the evidence points to motivational or aspirational spillovers within the household, where younger siblings internalize the example of their older sibling's educational trajectory and invest more effort in school. The small yet significant improvements in punctuality reinforce this interpretation, as they capture engagement and self-discipline rather than academic ability per se.

Overall, this exercise suggests that the influence of an older sibling's educational opportunity extends well beyond the college decision margin. The positive effects observed among primary and lower-secondary students indicate that family spillovers are rooted in broader shifts in attitudes toward schooling and effort—consistent with an intangible, motivation-based channel rather than resource redistribution or direct information transmission.

9 Conclusion

This paper studies how expanding access to higher education affects other members of the household. Using the eligibility criteria of *Ser Pilo Paga*—a large-scale college financial aid program for high-achieving, low-income students in Colombia—we document that the benefits of financial aid extend beyond direct recipients. When an older sibling becomes eligible for the program, younger siblings experience improvements in educational outcomes, a higher likelihood of college enrollment, suggestive evidence of medium-run improvements in labor market trajectories, along with a notable decline in criminal behavior. These patterns are also visible in educational outcomes, smaller in magnitude, among younger siblings of students who narrowly miss eligibility, suggesting that exposure to opportunity alone can influence family behavior.

The analysis provides insights into how educational investments propagate within families. First, the gains are broadly shared across socioeconomic groups and family structures, indicating that they are not concentrated among households with greater financial capacity. Second, because the program operates on the extensive margin of college participation—moving older siblings from not attending college to attending, often away from home—these effects are unlikely to arise from liquidity relief or relaxed household budgets. Instead, the evidence points toward intangible channels: increased motivation, aspirations, information, and perceived returns to education triggered by observing a sibling succeed academically despite financial constraints. Additional analyses using detailed university and early-schooling data reinforce this interpretation, showing that spillovers emerge early and are stronger when the older sibling performs well in college.

Together, these findings suggest that the impact of higher-education policy can be amplified by social dynamics within families. Financial aid targeted at one student may influence the expectations, effort, and decisions of others who are not directly treated, generating broader social returns than typically captured by program evaluations. At the same time, the results highlight that such spillovers arise in a context where households face persistent resource constraints, suggesting that the motivational and informational dimensions of policy interventions may be as important as their financial components. Recognizing and measuring these indirect effects is therefore essential for understanding the full distributional consequences of higher-education policies and for designing programs that harness family networks as a channel for promoting human capital accumulation.

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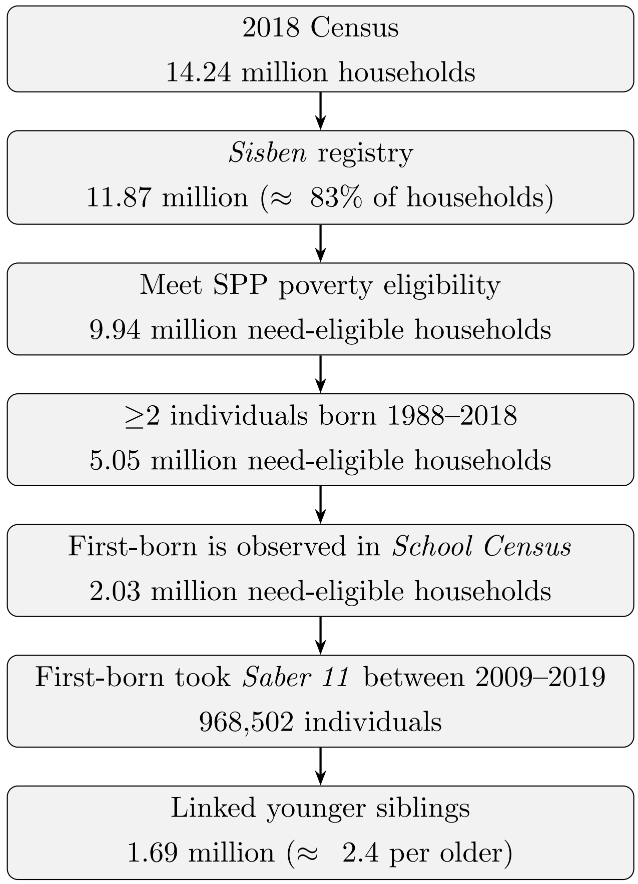
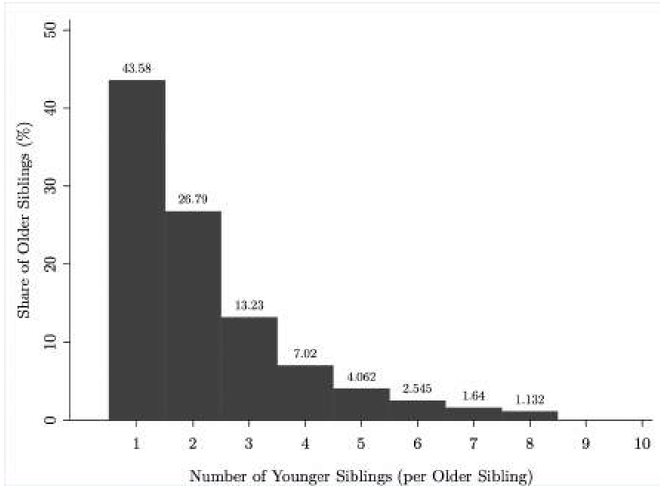
10 Appendix Tables and Figures

Figure 5: Sample construction from census to younger-sibling panel



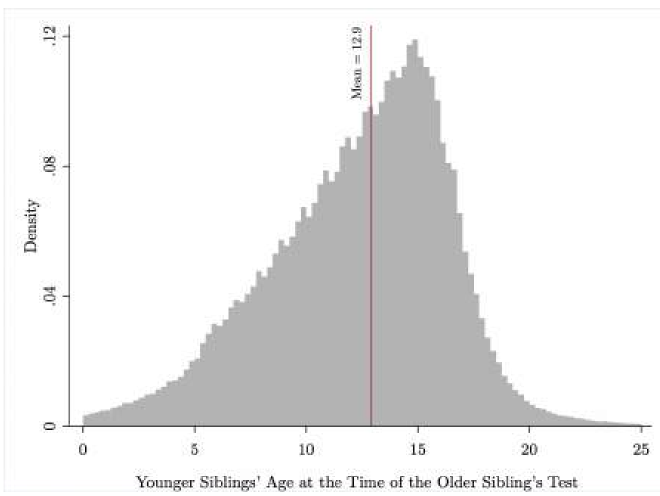
Note: the figure summarizes the sequential steps used to construct the analysis sample. The first box reports the number of households in Colombia from the 2018 Census. The sample construction begins with households listed in *Sisben*, the poverty census, which includes approximately 83 percent of Colombian households. From this registry, we identify household composition and apply the following steps to obtain the final sample of younger siblings: (i) households meeting the SPP poverty-eligibility criteria; (ii) those with at least two members (> 2) born between 1988 and 2018, ensuring that individuals are of schooling age during the years covered by our data; and (iii) those in which the first-born child is observed in the school census (*SIMAT*). The final sample of *older siblings* consists of first-borns who took the *Saber 11* exam between 2009 and 2019 in the eligible households, while *younger siblings* are defined as all other children in the household.

Figure 6: Number of Younger Siblings Linked to Each Older Sibling



Note: The figure shows the distribution of the number of younger siblings per older sibling in the linked household data. Values on the y-axis represent the share of the 968,502 older siblings with the corresponding number of younger siblings observed in the school census (*SIMAT*)

Figure 7: Age of Younger Siblings at the Time the First-Born Takes the Test



Note: The figure shows the distribution of younger siblings' ages at the time their older sibling takes the *Saber 11* exam. The average age of younger siblings at that time is 12.9 years.

Table 1: Descriptive Statistics of the Sibling Sample

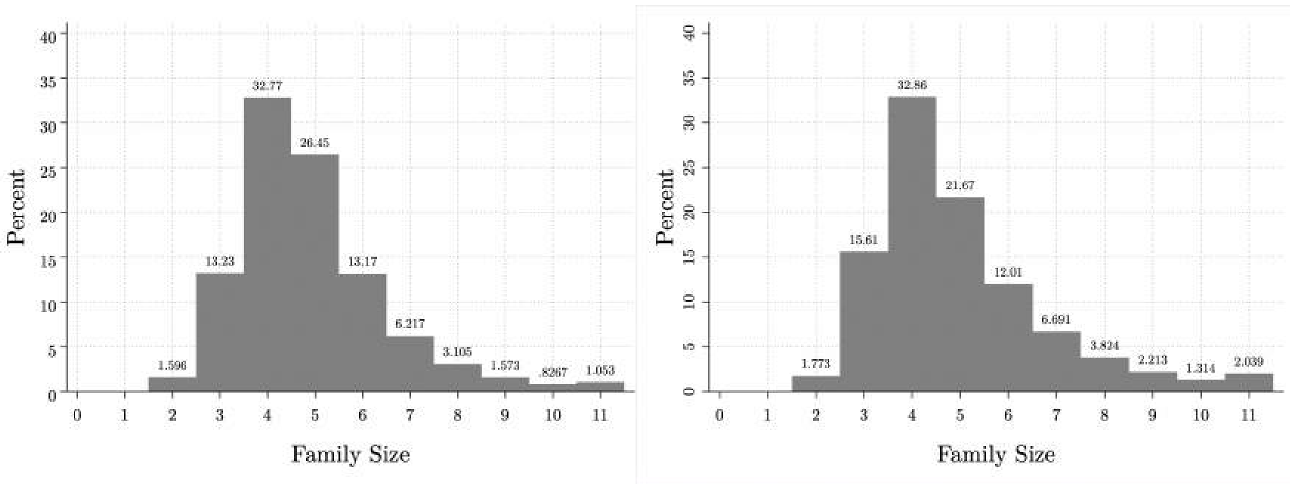
	Older Siblings		Younger Siblings	
Panel A. Family Background (Shared)				
	Mean	N	Mean	N
Family size	4.84	769628	4.84	769628
Children in household	2.67	769628	2.67	769628
Mother Education: primary	0.35	769628	0.35	769628
Mother Education: secondary	0.47	769628	0.47	769628
Mother Education: T&T	0.09	769628	0.09	769628
Mother Education: higher	0.07	769628	0.07	769628
Mother Education: other	0.03	769628	0.03	769628
Father Education: primary	0.39	769628	0.39	769628
Father Education: secondary	0.39	769628	0.39	769628
Father Education: T&T	0.06	769628	0.06	769628
Father Education: higher	0.06	769628	0.06	769628
Father Education: other	0.09	769628	0.09	769628
Family Poverty Index	42.88	769628	42.88	769628
High-quality HEI in mun	0.52	769628	0.52	769628
Household SES: stratum 1 (poorest)	0.50	769628	0.50	769628
Household SES: stratum 2	0.38	769628	0.38	769628
Household SES: stratum 3	0.11	769628	0.11	769628
Household SES: stratum 4	0.01	769628	0.01	769628
Household SES: stratum 5	0.00	769628	0.00	769628
Household SES: stratum 6 (wealthiest)	0.00	769628	0.00	769628
Mother stays home	0.57	769628	0.57	769628
Rooms in household	2.74	769628	2.74	769628
Has washing machine	0.61	769628	0.61	769628
Has computer	0.49	769628	0.49	769628
Has internet	0.40	769628	0.40	769628
Has car	0.14	769628	0.14	769628
Panel B. Individual Characteristics of Older and Younger Siblings				
Year of birth	1996.37	968519	2001.69	1694090
Age when older sibling takes test	.	0	12.23	1694090
Average age gap (years)	.	0	5.73	1694090
Age at Saber 11 test	17.72	968519	17.48	989167
SPP-eligible (=1)	0.04	968519	0.03	989167
Public school (=1)	0.91	967166	0.92	1693065
Saber 11 Score (SD)	-0.12	968153	-0.14	986577
Immediate post-secondary enrollment	0.22	966011	0.22	910298
HEI enrollment away from home	0.06	966011	0.06	910298
Two-year HEI degree	0.10	966011	0.10	910298
Four-year HEI degree	0.11	966011	0.11	910298
HEI Field of study: STEM	0.08	966011	0.08	910298
Earnings 6 years after HS	0.44	560245	0.42	296867
Formal employment 6 years after HS	0.42	560245	0.40	296867

Notes: This table reports summary statistics for the linked sibling sample. Columns (1) and (2) display means for older and younger siblings, respectively. Family background variables in Panel A are shared within households, while Panel B reports individual-level characteristics. The stratum system is a government classification of residential areas into six socioeconomic levels, from 1 (poorest) to 6 (wealthiest). The designation is based on the physical characteristics of a neighborhood, such as the quality of housing and surrounding infrastructure, rather than the residents' individual income. Source: Authors' calculations based on *Sisben*, *SIMAT*, *Saber 11*, and *PILA*

Figure 8: Distribution of Household Size in the Sample and the Census of the Poor

Panel A. Linked Sibling Sample (N = 968,519)

Panel B. Census of the Poor (*Sisben*)



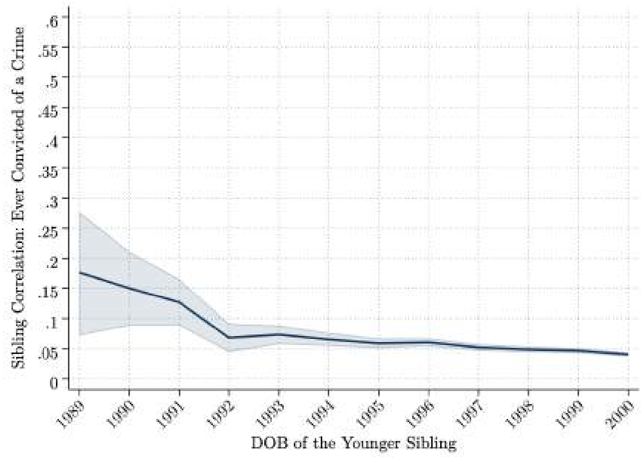
Note: The figure compares the household-size distribution in the linked sample with that in the 2018 *Sisben* census of the poor. The close overlap between the two distributions indicates that the sibling sample is representative of low-income families with children nationwide.

Table 2: **Sibling Correlations: Education, College, and Labor Outcomes**

Outcome Variable	Sibling Correlation	N
Saber 11: Performance in High School Exit Exam	0.435	986,154
College Enrollment	0.179	908,473
Enrollment in Public HEI	0.138	908,473
Enrollment in Private HEI	0.194	908,473
Enrollment in Low-Quality (Non-Accredited) HEI	0.130	908,473
Enrollment in Public, Low-Quality HEI	0.127	908,473
Enrollment in Private, Low-Quality HEI	0.128	908,473
Enrollment in High-Quality (Accredited) HEI	0.193	908,473
Enrollment in Public, High-Quality HEI	0.149	908,473
Enrollment in Private, High-Quality HEI	0.185	908,473
Enrollment in College Outside Home Municipality	0.176	908,473
Formal Employment (5 Years After High School)	0.166	316,131
Wage (in Monthly MW, 5 Years After High School)	0.172	316,131
Ser Pilo Paga Recipient	0.201	98,636
Any Criminal Conviction	0.034	546,342
Economic or Property Crime	0.018	546,342
Drug-Related Crime	0.028	546,342
Violent Crime	0.031	546,342
Other Crime	0.030	546,342

Notes: The table shows the value of the correlation between outcomes of older and younger siblings.

Figure 9: Sibling Correlation in Crime by Birth-Cohort of the Younger Sibling



Note: Each point represents the correlation coefficient between the crime outcomes of older and younger siblings, computed for all pairs in which the younger sibling was born before the indicated cutoff year. The x-axis therefore shows cumulative cohorts: for example, the 1988 point includes all sibling pairs where the younger sibling was born before December 31, 1988. The shaded area corresponds to 95% confidence intervals. The upward pattern reflects differences in exposure time: older cohorts have had more years to be observed in crime records, while younger cohorts—still at earlier ages—have lower probabilities of conviction and less variation in outcomes, mechanically leading to lower observed sibling correlations.

Table 3: Younger Siblings' Outcomes Below and Above the *Ser Pilo Paga* Cutoff, Pre- and Post-Program

Younger Siblings' Outcomes	Mean (Pre)	Mean (Post)	Diff	SE	p-value
Below Cutoff					
Saber 11 Score	0.237	0.334	0.097	0.018	0.0000
HEI Enrollment	0.399	0.416	0.017	0.013	0.2051
Public HEI Enrollment	0.255	0.303	0.048	0.014	0.0006
Private HEI Enrollment	0.136	0.123	-0.013	0.009	0.1519
Above Cutoff					
Saber 11 Score	0.392	0.568	0.177	0.030	0.0000
HEI Enrollment	0.466	0.456	-0.010	0.023	0.6620
Public HEI Enrollment	0.302	0.299	-0.003	0.022	0.8912
Private HEI Enrollment	0.151	0.164	0.014	0.017	0.4049

Note: This table reports mean outcomes for younger siblings before (2010) and after (2014) the introduction of the *Ser Pilo Paga* program, separately for observations below and above the eligibility cutoff of the older sibling's standardized *Saber 11* score. Means are computed within the optimal bandwidth on each side of the threshold, as determined by the robust local polynomial estimation of Calonico et al. (2014). Columns show the pre- and post-program means, their difference, standard error, and corresponding p-value. Differences capture descriptive changes across periods and do not represent causal effects.

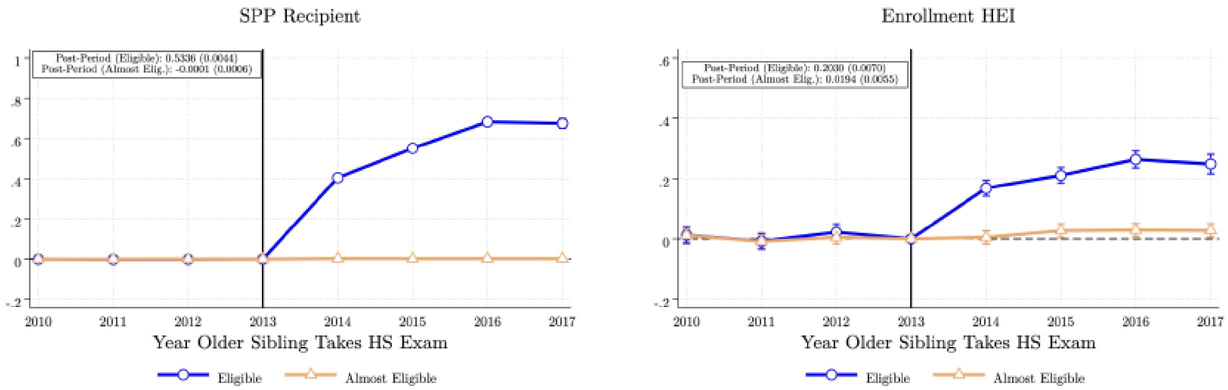
Table 4: First Stage: Effect of SPP Eligibility on the Probability That the Older Sibling Enrolls in Postsecondary Education, by Institution Type, Quality, and Program Duration

			Low-quality			High-quality			Program Duration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	SPP	Any	Any	Public	Private	Any	Public	Private	Two-years	Four-years
Differences-in-Differences Model:										
T1 $\times$ Post	0.534***	0.203***	-0.115***	-0.069***	-0.046***	0.318***	-0.068***	0.386***	-0.082***	0.285***
	(0.004)	(0.007)	(0.005)	(0.004)	(0.003)	(0.006)	(0.005)	(0.005)	(0.005)	(0.006)
T2 $\times$ Post	-0.000	0.019***	-0.028***	-0.016***	-0.012***	0.048***	0.034***	0.014***	-0.020***	0.039***
	(0.001)	(0.006)	(0.005)	(0.004)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)
Observations	283209	283209	283209	283209	283209	283209	283209	283209	283209	283209
Mean	0.0000	0.3840	0.2027	0.1410	0.0617	0.1814	0.1203	0.0611	0.1683	0.2151
Effect rel. to mean (T1)	.%	53%	-57%	-49%	-74%	176%	-57%	633%	-49%	133%
Effect rel. to mean (T2)	n.s.	5%	-14%	-12%	-19%	26%	28%	23%	-12%	18%

Note: The dependent variables are defined as follows. Column (1) reports the probability that the older sibling in the household receives a *Ser Pilo Paga* (SPP) scholarship. Column (2) shows the probability of having an older sibling enrolled in a higher education institution (HEI). Columns (3)–(5) and (6)–(8) distinguish between low- and high-quality HEIs by sector type. Columns (9) and (10) report the probability of having an older sibling enrolled in a two-year and four-year HEI program, respectively.

Figure 10: First Stage: Event-Study Estimates

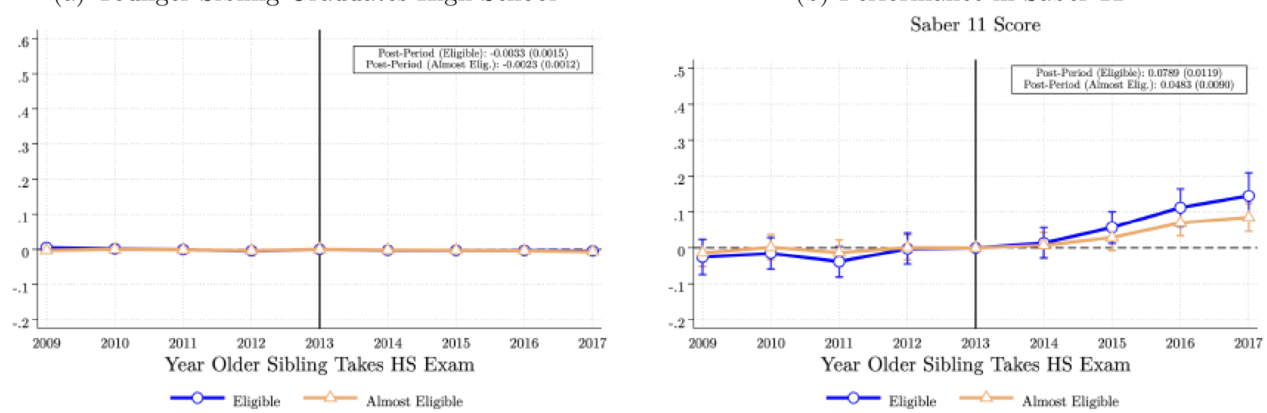
Panel A. Older Sibling Receives a Ser Pilo Paga (SPP) Scholarship | **Panel B. Older Sibling Enrolls in a Higher Education Institution (HEI)**



Note: Each panel plots event-study coefficients estimated from Equation (1), showing the dynamic effect of *Ser Pilo Paga* (SPP) eligibility on the probability that the household's first-born (A) becomes an SPP scholarship recipient or (B) enrolls in a higher-education institution (HEI). Coefficients represent differences relative to the 2013 cohort, the last cohort before the introduction of SPP. Vertical bars denote 95% confidence intervals, with standard errors clustered at the family level. The boxes in each panel display the corresponding difference-in-differences coefficients, summarizing the post-program effects by pooling all cohorts after 2014. The sample is restricted to households with at least one younger sibling observed in the school census. A positive and statistically significant effect for post-2014 cohorts indicates that program eligibility sharply increased the likelihood that the first-born pursued higher education.

Figure 11: Event Study Plots: Intensive and Extensive Margin

(a) Younger Sibling Graduates High School | (b) Performance in Saber 11



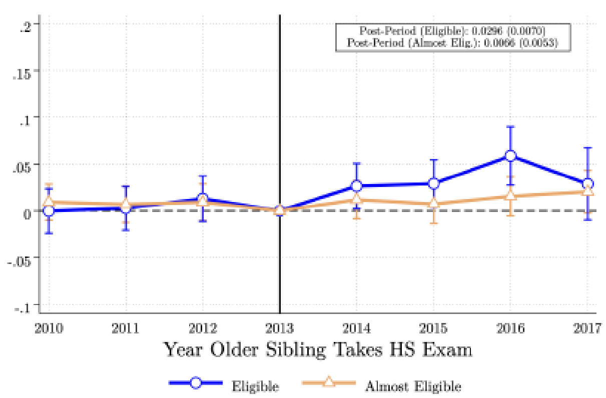
Note: Each panel plots event-study coefficients estimated from Equation (2) for younger siblings' educational outcomes. Panel A shows the probability of high-school graduation; Panel B shows standardized scores on the overall *Saber 11* exam. Coefficients are relative to 2013, the last cohort before the introduction of the *Ser Pilo Paga* (SPP) program. Vertical bars represent 95% confidence intervals, with standard errors clustered at the family level. Appendix D, Table D.1, displays the complete set of event-study coefficients corresponding to these outcomes.

Table 5: Younger Sibling: Performance in National Standardized Test

	Extensive Margin	Intensive Margin		
	(1)	(2)	(3)	(4)
	HS Graduation	Overall	Math	Spanish
Differences-in-Differences Model:				
T1 × Post	-0.003**	0.079***	0.078***	0.087***
	(0.002)	(0.012)	(0.012)	(0.012)
T2 × Post	-0.002*	0.048***	0.045***	0.048***
	(0.001)	(0.009)	(0.009)	(0.009)
Observations	276802	212764	212764	212764
Mean	0.7799	0.3979	0.3804	0.3564

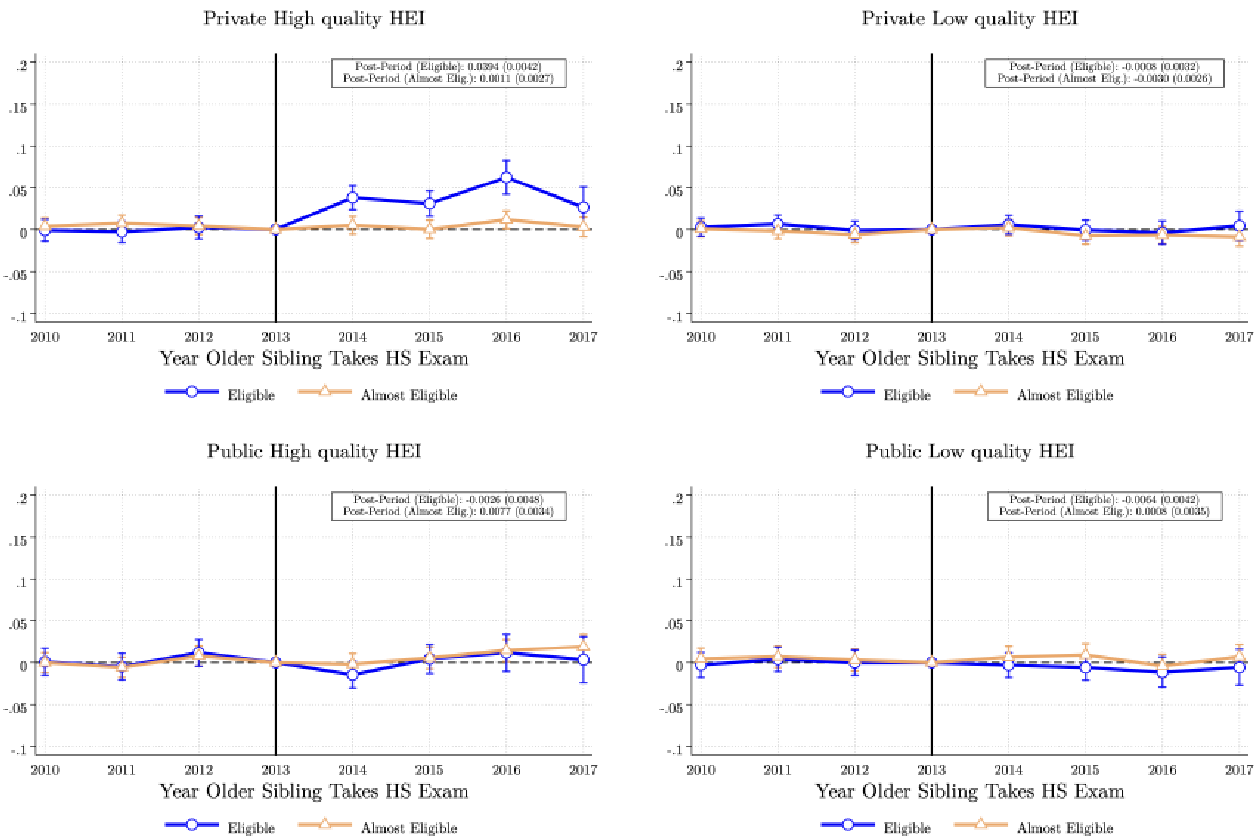
Note: The table reports Difference-in-Differences estimates from Equation (2) for younger siblings' educational outcomes. Each column corresponds to a separate regression. The coefficients on $T1 \times Post$ and $T2 \times Post$ capture the change in outcomes for younger siblings of eligible and almost-eligible older siblings, respectively, relative to the control group of families whose older sibling scored well below the SPP cutoff. High-school graduation is measured as an indicator equal to one if the younger sibling takes the Saber 11 exam. *Saber 11* outcomes are standardized by year to have mean zero and standard deviation one. All regressions include school and municipality-by-cohort fixed effects and control for baseline family characteristics interacted with cohort dummies. Standard errors are clustered at the family level.

Figure 12: Immediate Enrollment in Postsecondary Education



Note: The figure plots event-study coefficients estimated from Equation (2) for the probability of younger siblings enrolling in any higher-education institution immediately after high school. Coefficients are relative to 2013, the last cohort before the introduction of the *Ser Pilo Paga* (SPP) program. Vertical bars represent 95% confidence intervals, with standard errors clustered at the household level.

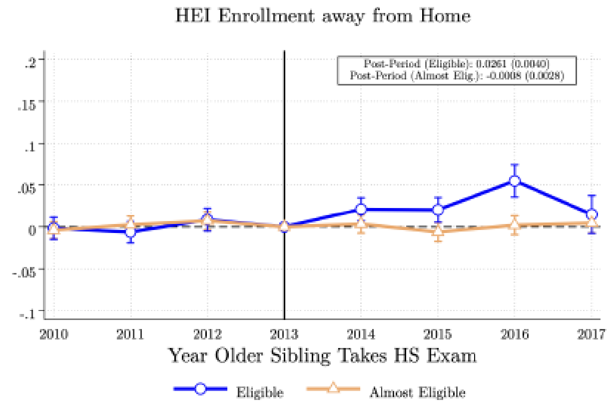
Table 6: Immediate Enrollment in Postsecondary Education, by Institution Type, Quality, and Program Duration



Note: Each panel plots event-study coefficients estimated from Equation (2) for younger siblings' probability of enrolling in any higher-education institution immediately after high school. Panel A shows the probability of enrollment in high-quality private institutions; Panel B shows enrollment in low-quality private institutions; Panel C shows enrollment in high-quality public institutions; Panel D shows enrollment in low-quality public institutions. Coefficients are relative to 2013, the last cohort before the introduction of the *Ser Pilo Paga* (SPP) programme. Vertical bars represent 95% confidence intervals, with standard errors clustered at the family level. Appendix D, Table D.2 displays the complete set of event-study coefficients corresponding to these outcomes.

Figure 13: Event Study Plots: Enrollment in HEI Outside Home Municipality

(a) Graduates HS



Note: Note: The figure plots event-study coefficients estimated from Equation (2) for younger siblings' probability of enrolling in a higher-education institution (HEI) immediately after high school in a municipality different from their home municipality. Coefficients are shown for each year relative to 2013, the last cohort before the introduction of the *Ser Pilo Paga* (SPP) program. Vertical bars represent 95% confidence intervals, and standard errors are clustered at the household level.

Table 7: Sibling Spillover Effects on Postsecondary Enrollment by Institution Type, Quality and Program Duration

			Low-quality			High-quality			Program Duration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	SPP	Any	Any	Public	Private	Any	Public	Private	Two-years	Four-years
Differences-in-Differences Model:										
T1 × Post	0.013***	0.030***	-0.007	-0.006	-0.001	0.037***	-0.003	0.039***	-0.010**	0.040***
	(0.003)	(0.007)	(0.005)	(0.004)	(0.003)	(0.006)	(0.005)	(0.004)	(0.005)	(0.006)
T2 × Post	-0.002	0.007	-0.002	0.001	-0.003	0.009**	0.008**	0.001	-0.004	0.011**
	(0.002)	(0.005)	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)
Observations	151741	178819	178819	178819	178819	178819	178819	178819	178819	178819
Mean	0.0262	0.3145	0.1462	0.0973	0.0489	0.1683	0.0992	0.0691	0.1199	0.1936
Effect rel. to mean (T1)	51.2%	9.4%	n.s.	n.s.	n.s.	21.9%	n.s.	57.0%	-8.6%	20.7%
Effect rel. to mean (T2)	n.s.	n.s.	n.s.	n.s.	n.s.	5.2%	7.8%	n.s.	n.s.	5.8%

Note: The dependent variables are defined as follows. Column (1) reports the probability of being a *Ser Pilo Paga* (SPP) scholarship recipient. Column (2) shows the enrolling in a higher education institution (HEI). Columns (3)–(5) and (6)–(8) distinguish between low- and high-quality HEIs by sector type. Columns (9) and (10) report the probability of enrolling in a two-year and four-year HEI program, respectively.

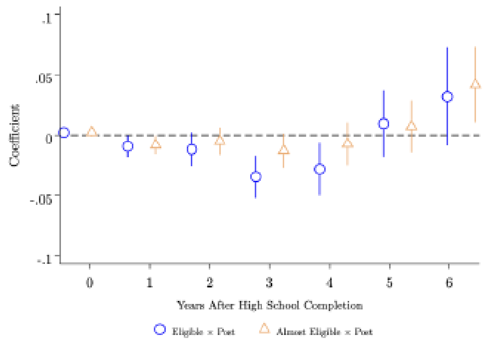
Table 8: Sibling Spillover Effects on Post-Secondary Outcomes: Field of Study, Sibling Following Behavior, and Graduation

	Field of Study						Following Older Sibling's Path		Graduation Outcomes		
	STEM	STEM Plus	Arts	S.S.H.	N.A.	HEI Away from Home	Same Institution	Same Field	Any HEI	University	TVET
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Differences-in-Differences:											
T1 × Post	0.017***	0.024***	0.002	0.002	0.001	0.026***	-0.017***	0.017***	0.018*	0.041***	-0.032***
	(0.005)	(0.007)	(0.002)	(0.003)	(0.001)	(0.004)	(0.004)	(0.005)	(0.010)	(0.011)	(0.011)
T2 × Post	0.005	0.006	-0.000	-0.002	0.002***	-0.001	0.003	0.005	0.003	0.007	-0.006
	(0.004)	(0.005)	(0.001)	(0.002)	(0.001)	(0.003)	(0.003)	(0.003)	(0.009)	(0.009)	(0.009)
Observations	178819	178819	178819	178819	178819	178819	178819	178819	90766	91870	75620
Mean	0.1253	0.2634	0.0127	0.0340	0.0039	0.0821	0.0939	0.0960	0.6289	0.4250	0.3081
Effect rel. to mean (T1)	13.7%	9.2%	n.s.	n.s.	n.s.	31.8%	-18.5%	17.8%	2.9%	9.7%	-10.2%
Effect rel. to mean (T2)	n.s.	n.s.	n.s.	n.s.	58.3%	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.

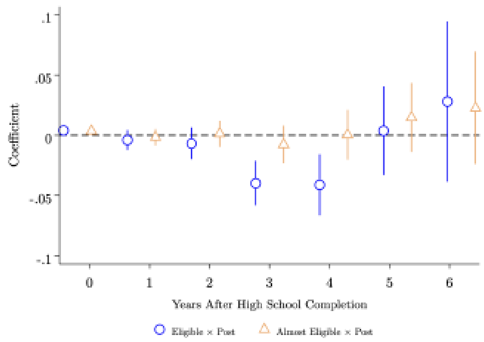
Note: This table reports event-study coefficients estimated from Equation (2) for younger siblings' post-secondary outcomes. Each column corresponds to a separate regression for the outcome listed. Columns (1)–(5) refer to the field of study (STEM, STEM Plus, Arts, Social Sciences & Humanities, and N.A.); columns (6)–(8) capture sibling following behavior and location choices (enrollment away from home municipality, in the same institution, or in the same field as the older sibling); and columns (9)–(11) present graduation outcomes (any HEI, university, and TVET graduation). The specification includes school and cohort-by-municipality fixed effects, as well as baseline family characteristics interacted with cohort dummies. Standard errors are clustered at the household level.

Figure 14: Event Study Plots: Formal Employment and Earnings

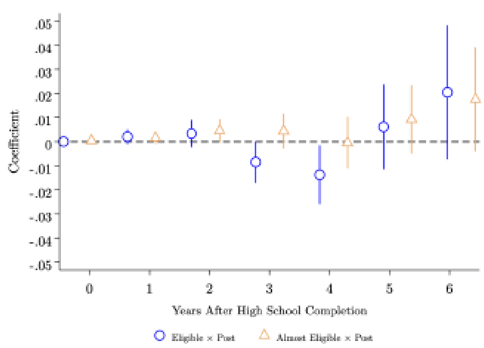
(a) Formal Employment



(b) Earnings



(b) Earnings: top 25%



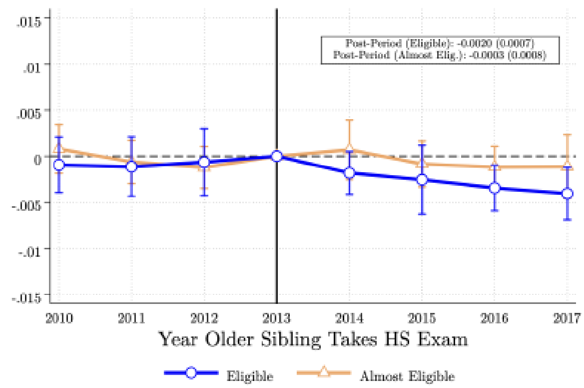
Note: Each panel plots event-study coefficients from Equation (2), tracing the evolution of younger siblings' labor-market outcomes relative to the 2013 cohort, the last group unexposed to *Ser Pilo Paga* (SPP). Panel A displays effects on formal employment, Panel B on total earnings (in multiples of the monthly minimum wage), and Panel C on the probability of being in the top quartile of the earnings distribution. The figure shows a U-shaped trajectory: coefficients are negative during the first four years after high school—when most younger siblings are enrolled in higher education—and gradually turn positive beginning in year five, although later effects are not statistically significant. Ninety-five percent confidence intervals are clustered at the household level. The corresponding tables with estimated coefficients are reported in Appendix D, Tables D.3, D.4, and D.5.

Table 9: Younger Sibling: Labor Market Outcomes

	Formal Employment Ψ years after High School						
	Zero	One	Two	Three	Four	Five	Six
Differences-in-Differences Model:							
Eligible × Post	-0.004*	-0.013**	-0.005	-0.017*	-0.024**	0.008	0.008
	(0.002)	(0.005)	(0.008)	(0.009)	(0.012)	(0.015)	(0.022)
Almost Eligible × Post	-0.003	-0.013***	-0.001	0.003	-0.002	0.004	0.004
	(0.002)	(0.004)	(0.006)	(0.008)	(0.009)	(0.012)	(0.018)
Observations	162754	145203	127153	107268	87550	68360	49763
Mean	0.0267	0.1023	0.2534	0.3349	0.3788	0.4359	0.4667
	Earnings Ψ years after High School						
	Zero	One	Two	Three	Four	Five	Six
Differences-in-Differences Model:							
Eligible × Post	0.004**	-0.004	-0.007	-0.040***	-0.041***	0.004	0.028
	(0.002)	(0.004)	(0.007)	(0.009)	(0.013)	(0.019)	(0.034)
Almost Eligible × Post	0.003**	-0.002	0.001	-0.008	0.000	0.015	0.023
	(0.001)	(0.003)	(0.006)	(0.008)	(0.011)	(0.015)	(0.024)
Observations	188898	164691	142016	118281	95770	74980	54766
Mean	0.01	0.08	0.19	0.27	0.32	0.37	0.46
	Top 25% of the earnings distribution Ψ years after High School						
	Zero	One	Two	Three	Four	Five	Six
Differences-in-Differences Model:							
Eligible × Post	0.000	0.002	0.003	-0.009*	-0.014**	0.006	0.020
	(0.001)	(0.002)	(0.003)	(0.004)	(0.006)	(0.009)	(0.014)
Almost Eligible × Post	0.000	0.002	0.004*	0.004	-0.000	0.009	0.018
	(0.001)	(0.001)	(0.002)	(0.004)	(0.005)	(0.007)	(0.011)
Observations	188898	164691	142016	118281	95770	74980	54766
Mean	0.00	0.01	0.03	0.06	0.08	0.10	0.13

Note: The table reports Difference-in-Differences estimates of Equation (2) for younger siblings' labor-market outcomes. Panel A presents the probability of formal employment zero to six years after high school graduation. Panel B reports effects on total earnings (expressed in multiples of the monthly minimum wage), and Panel C reports the probability of being in the top quartile of the earnings distribution. Each column corresponds to a separate regression by years since high school completion. All specifications include school and municipality-by-cohort fixed effects, as well as baseline family characteristics interacted with cohort dummies. Standard errors are clustered at the household level. Negative short-run effects coincide with the college-going period documented in Section 5.3, followed by a gradual recovery in later years.

Figure 15: Crime



Note: The figure plots event-study coefficients estimated from Equation (2), showing the effect of exposure to the *Ser Pilo Paga* (SPP) program through an older sibling on the probability that a younger sibling is convicted of any crime. Coefficients are relative to the 2013 cohort, the last group unexposed to SPP. Ninety-five-percent confidence intervals are displayed, with standard errors clustered at the household level. The corresponding tables reporting event-study coefficients by type of offense are presented in Appendix D, Table D.6.

Table 10: Younger Sibling: Crime

	(1) Any crime	(2) Economic	(3) Drugs	(4) Violent	(5) Others
Differences-in-Differences Model:					
Eligible × Post	-0.0020***	-0.0008	-0.0011***	-0.0004**	0.0003
	(0.0007)	(0.0005)	(0.0003)	(0.0002)	(0.0004)
Almost Eligible × Post	-0.0003	-0.0000	-0.0002	-0.0001	-0.0001
	(0.0008)	(0.0005)	(0.0004)	(0.0002)	(0.0004)
Observations	88624	88624	88624	88624	88624
Mean	0.0045	0.0020	0.0009	0.0006	0.0009
Effect relative to the mean (Elig.)	-45%	n.s.	-120%	-71%	n.s.
Effect relative to the mean (Almost Elig.)	n.s.	n.s.	n.s.	n.s.	n.s.

Note: Each column reports coefficients from a Difference-in-Differences specification estimated on younger siblings' criminal outcomes. The dependent variables indicate whether the younger sibling was ever convicted of (1) any crime, (2) an economic offense, (3) a drug-related offense, (4) a violent offense, or (5) another type of crime. The mean row reports the pre-program probability of conviction for each offense category. The baseline probability of committing any crime is 0.45 percent (4.5 per 1,000 individuals). Standard errors are clustered at the household level.

Table 11: Younger Sibling: *Saber 11* Outcomes by Older Sibling Success in a Top Private University

	Takes *Saber 11*	Overall Score	Math Score	Spanish Score
T1 × Post	–0.041 (0.133)	–0.244 (0.223)	–0.229 (0.192)	0.262 (0.348)
T1 × Post × Sibling Success	0.020 (0.030)	0.294*** (0.076)	0.321*** (0.066)	0.255*** (0.090)
Observations	1,234	994	994	994
Mean	0.84	1.16	1.07	0.96

Note: Each column reports coefficients from a Difference-in-Differences specification estimated on the subsample of families where the older sibling enrolled in the same private university. The interaction term $T1 \times Post \times Sibling\ Success$ captures whether spillovers are stronger when the older sibling's GPA in the first semester is above the 75th percentile of the cohort. Standard errors are clustered at the household level.

Table 12: Younger Sibling: Immediate Postsecondary Enrollment by Older Sibling Success in a Top Private University

		Low-Quality			High-Quality			Duration	
	(1) Any	(2) Any	(3) Public	(4) Private	(5) Any	(6) Public	(7) Private	(8) Two-Year	(9) Four-Year
T1 × Post	0.026 (0.178)	0.019 (0.159)	–0.001 (0.108)	0.020 (0.078)	0.007 (0.076)	–0.092* (0.050)	0.099* (0.053)	0.069 (0.132)	–0.038 (0.100)
T1 × Post × Sibling Success	0.099** (0.042)	–0.004 (0.034)	0.003 (0.027)	–0.008 (0.019)	0.104* (0.053)	0.040 (0.043)	0.064** (0.029)	0.016 (0.029)	0.082** (0.038)
Observations	915	915	915	915	915	915	915	915	915
Mean	0.45	0.09	0.06	0.04	0.36	0.12	0.23	0.07	0.38

Note: Each column reports Difference-in-Differences coefficients for younger siblings' postsecondary enrollment outcomes, by institution type, quality, and degree length. The interaction $T1 \times Post \times Sibling\ Success$ indicates whether spillovers are amplified when the older sibling's GPA in the first semester exceeds the 75th percentile. Standard errors are clustered at the household level.

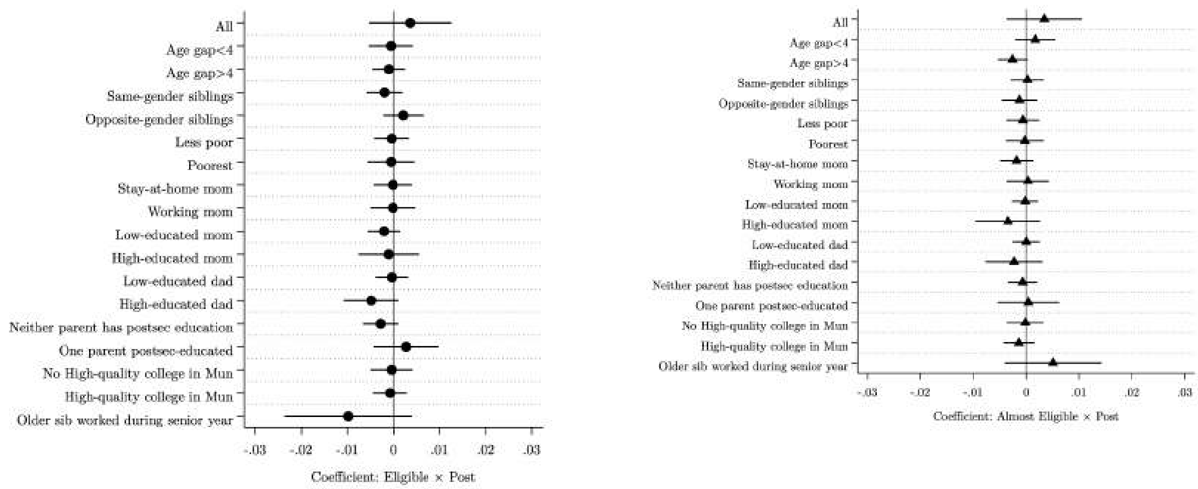
Table 13: Younger Siblings: *Saber 3*, *Saber 5*, and *Saber 9* Outcomes (2017 ICFES Data)

	Saber 3		Saber 5				Saber 9			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Math	Spanish	Math	Spanish	Perfect Attendance	Never late to class	Math Math	Spanish Spanish	Perfect Attendance	Never late to class
Differences-in-Differences Model:										
Eligible × Post	0.27***	0.18*	0.18***	0.20***	0.04	0.07**	0.13***	0.02	-0.02	0.01
	(0.10)	(0.10)	(0.07)	(0.07)	(0.04)	(0.03)	(0.05)	(0.05)	(0.03)	(0.03)
Almost Eligible × Post	-0.09	-0.02	0.03	0.09*	-0.01	0.04	0.05	-0.05	0.01	0.01
	(0.07)	(0.08)	(0.05)	(0.05)	(0.03)	(0.03)	(0.04)	(0.03)	(0.02)	(0.02)
Observations	8103	8103	14185	14185	14185	14185	20131	20131	20131	20131
Mean	0.18	0.25	0.30	0.31	0.60	0.64	0.25	0.25	0.50	0.48
Effect r.t. mean (Elig.)					n.s.	10%			n.s.	n.s.
Effect r.t. the mean (Almost Elig.)					n.s.	n.s.			n.s.	n.s.
Average age	8.8		10.9				15.2			

Note: Each column reports coefficients from a Difference-in-Differences specification estimated using the 2017 *Saber 3*, *Saber 5*, and *Saber 9* test scores and self-reported behavioral indicators. *Perfect Attendance* equals 1 if the student reports not missing any school days in the previous month; *Never Late* equals 1 if the student reports never arriving late. All models include school and cohort-by-municipality fixed effects and baseline family characteristics interacted with cohort dummies. Standard errors are clustered at the household level.

Figure 16: Heterogeneity Analysis: Probability of Taking the *Saber 11* Exam

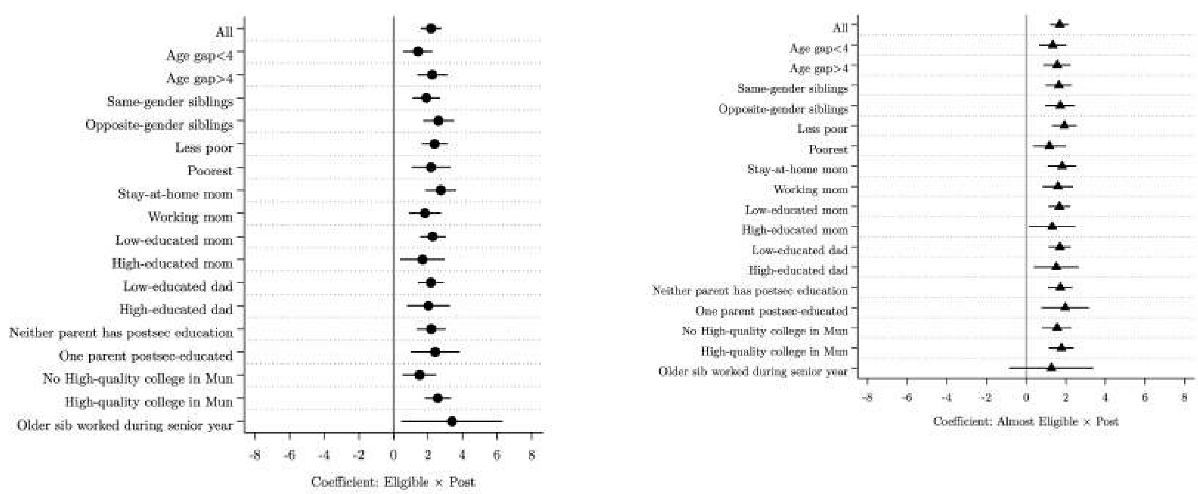
Panel A. Eligible × Post **Panel B. Almost Eligible × Post**



Note: Each panel displays subgroup-specific estimates of the post-treatment interaction term ($T1 \times Post$ or $T2 \times Post$) from Equation (2). The dependent variable equals one if the younger sibling takes the national high-school exit exam (*Saber 11*). Vertical lines denote 95% confidence intervals. Estimates are homogeneous across subgroups, indicating that exposure to an eligible older sibling does not differentially affect test participation by family composition or socioeconomic characteristics.

Figure 17: Heterogeneity Analysis: *Saber 11* Test Scores

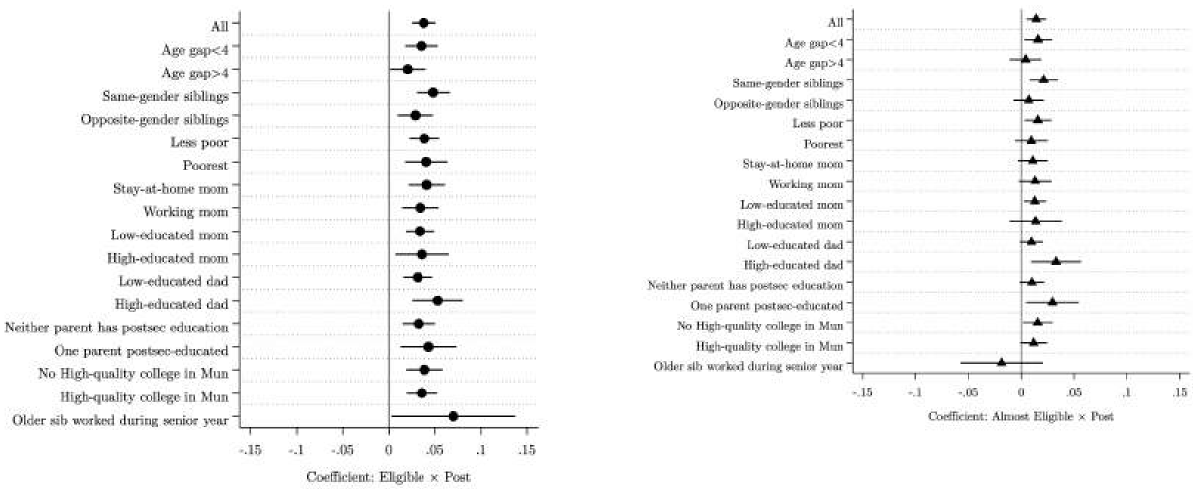
Panel A. Eligible × Post **Panel B. Almost Eligible × Post**



Note: The figures plot subgroup-specific estimates of the treatment interaction terms ($T1 \times Post$ and $T2 \times Post$) from Equation (2), where the outcome is the standardized *Saber 11* test score. Subgroups include sibling characteristics (age gap, gender), household poverty, parental education, and maternal employment. Vertical bars denote 95% confidence intervals. The coefficients are stable across subgroups, showing no systematic heterogeneity by socioeconomic status or family structure.

Figure 18: Heterogeneity Analysis: Immediate Postsecondary Enrollment

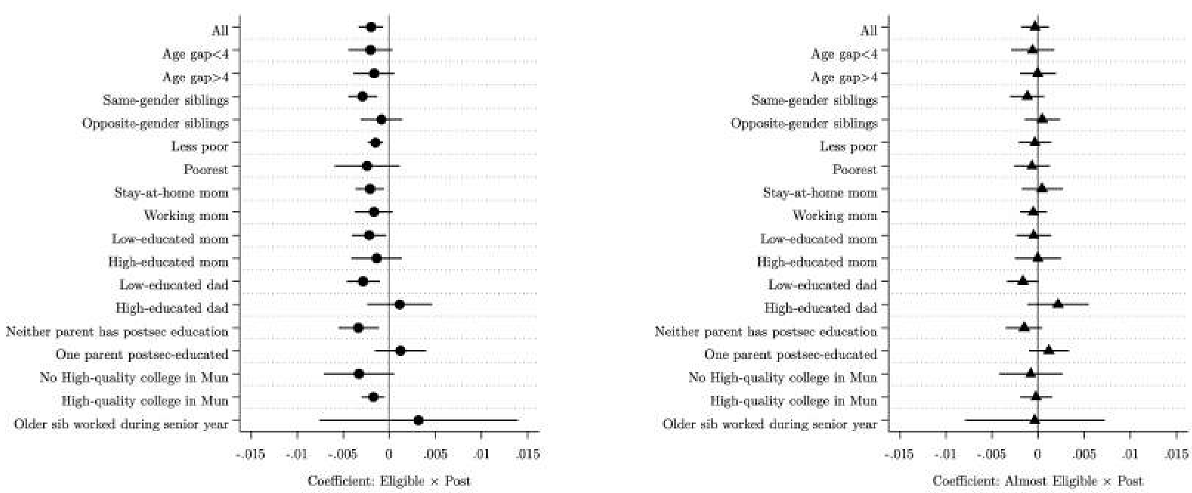
Panel A. Eligible × Post **Panel B. Almost Eligible × Post**



Note: Each panel shows subgroup-specific estimates of the interaction term ($T1 \times Post$ or $T2 \times Post$) for the probability that the younger sibling enrolls in any higher-education institution immediately after high school. Subgroups are defined by household poverty, parental schooling, and the presence of a high-quality HEI in the municipality. Vertical bars represent 95% confidence intervals. Estimates are similar across groups, suggesting that spillovers operate through aspirational rather than geographic or financial channels.

Figure 19: Heterogeneity Analysis: Ever Being Convicted of a Crime

Panel A. Eligible × Post **Panel B. Almost Eligible × Post**



Note: Each panel shows subgroup-specific estimates of the interaction term ($T1 \times Post$ or $T2 \times Post$) for the probability that the younger sibling enrolls in any higher-education institution immediately after high school. Subgroups are defined by household poverty, parental schooling, and the presence of a high-quality HEI in the municipality. Vertical bars represent 95% confidence intervals. Estimates are similar across groups, suggesting that spillovers operate through aspirational rather than geographic or financial channels.

Table 14: Robustness Check – Younger Sibling: Performance in National Standardized Test

	Extensive Margin	Intensive Margin		
	(1)	(2)	(3)	(4)
	HS Graduation	Overall	Math	Spanish
Differences-in-Differences Model:				
Panel A: Baseline				
T1 × Post	-0.003**	0.079***	0.078***	0.087***
	(0.002)	(0.012)	(0.012)	(0.012)
T2 × Post	-0.002*	0.048***	0.045***	0.048***
	(0.001)	(0.009)	(0.009)	(0.009)
Observations	276802	212764	212764	212764
Mean	0.7799	0.3979	0.3804	0.3564
Panel B: Control: 60th – 70th percentile				
T1 × Post	-0.001	0.093***	0.097***	0.099***
	(0.002)	(0.012)	(0.012)	(0.012)
T2 × Post	-0.000	0.060***	0.061***	0.056***
	(0.001)	(0.009)	(0.009)	(0.009)
Observations	287505	217932	217932	217932
Mean	0.7706	0.3330	0.3217	0.2985
Panel C: Control: 50th – 60th percentile				
T1 × Post	-0.002	0.099***	0.110***	0.103***
	(0.002)	(0.012)	(0.012)	(0.012)
T2 × Post	-0.000	0.066***	0.071***	0.062***
	(0.001)	(0.009)	(0.009)	(0.009)
Observations	296217	221698	221698	221698
Mean	0.7583	0.2822	0.2765	0.2524

Note: The table reports Difference-in-Differences estimates from Equation (2) for younger siblings' educational outcomes. Each column corresponds to a separate regression. The coefficients on $T1 \times Post$ and $T2 \times Post$ capture the change in outcomes for younger siblings of eligible and almost-eligible older siblings, respectively, relative to the control group of families whose older sibling scored well below the SPP cutoff. High-school graduation is measured as an indicator equal to one if the younger sibling takes the Saber 11 exam. *Saber 11* outcomes are standardized by year to have mean zero and standard deviation one. All regressions include school and municipality-by-cohort fixed effects and control for baseline family characteristics interacted with cohort dummies. Standard errors are clustered at the family level.

Table 15: Sibling Spillover Effects on Postsecondary Enrollment by Institution Type, Quality and Program Duration

Differences-in-Differences Model:			Low-quality			High-quality			Program Duration	
	(1) SPP	(2) Any	(3) Any	(4) Public	(5) Private	(6) Any	(7) Public	(8) Private	(9) Two-years	(10) Four-years
Panel A: Baseline										
T1 × Post	0.013***	0.030***	-0.007	-0.006	-0.001	0.037***	-0.003	0.039***	-0.010**	0.040***
	(0.003)	(0.007)	(0.005)	(0.004)	(0.003)	(0.006)	(0.005)	(0.004)	(0.005)	(0.006)
T2 × Post	-0.002	0.007	-0.002	0.001	-0.003	0.009**	0.008**	0.001	-0.004	0.011**
	(0.002)	(0.005)	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)
Observations	151741	178819	178819	178819	178819	178819	178819	178819	178819	178819
Mean	0.0262	0.3145	0.1462	0.0973	0.0489	0.1683	0.0992	0.0691	0.1199	0.1936
Effect rel. to mean (T1)	51.2%	9.4%	n.s.	n.s.	n.s.	21.9%	n.s.	57.0%	-8.6%	20.7%
Effect rel. to mean (T2)	n.s.	n.s.	n.s.	n.s.	n.s.	5.2%	7.8%	n.s.	n.s.	5.8%
Panel B: Control: 60th – 70th percentile										
T1 × Post	0.013***	0.038***	-0.007	-0.012***	0.004	0.046***	0.001	0.044***	-0.015***	0.053***
	(0.003)	(0.007)	(0.005)	(0.004)	(0.003)	(0.006)	(0.005)	(0.004)	(0.005)	(0.006)
T2 × Post	-0.002	0.014***	-0.000	-0.004	0.003	0.014***	0.011***	0.004	-0.008**	0.022***
	(0.002)	(0.005)	(0.004)	(0.003)	(0.002)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)
Observations	155196	182994	182994	182994	182994	182994	182994	182994	182994	182994
Mean	0.0241	0.3014	0.1447	0.0967	0.0480	0.1567	0.0932	0.0636	0.1188	0.1818
Effect rel. to mean (T1)	53%	13%	n.s.	-12%	n.s.	29%	n.s.	70%	-12%	29%
Effect rel. to mean (T2)	n.s.	5%	n.s.	n.s.	n.s.	9%	12%	n.s.	-7%	12%
Panel C: Control: 50th – 60th percentile										
T1 × Post	0.012***	0.031***	-0.015***	-0.016***	0.001	0.045***	0.004	0.042***	-0.017***	0.048***
	(0.003)	(0.007)	(0.005)	(0.004)	(0.003)	(0.006)	(0.005)	(0.004)	(0.005)	(0.006)
T2 × Post	-0.003**	0.004	-0.010**	-0.009***	-0.000	0.014***	0.013***	0.001	-0.013***	0.017***
	(0.002)	(0.005)	(0.004)	(0.003)	(0.002)	(0.004)	(0.003)	(0.002)	(0.004)	(0.004)
Observations	157125	186057	186057	186057	186057	186057	186057	186057	186057	186057
Mean	0.0237	0.2945	0.1428	0.0962	0.0466	0.1517	0.0905	0.0612	0.1165	0.1771
Effect rel. to mean (T1)	50%	10%	-10%	-17%	n.s.	30%	n.s.	68%	-14%	27%
Effect rel. to mean (T2)	-14%	n.s.	-7%	-10%	n.s.	9%	14%	n.s.	-11%	10%

Note: Each column reports separate estimates of Equation (2) for different higher education enrollment outcomes. Column (1) reports the probability that the younger sibling receives a *Ser Pilo Paga* (SPP) scholarship. Column (2) reports the probability of enrolling in any higher education institution (HEI). Columns (3)–(5) and (6)–(8) separate low- and high-quality HEIs by sector type (public or private). Columns (9) and (10) report enrollment in two-year and four-year programs. Panel A uses as a control group younger siblings of students scoring in the 70th–80th percentile of the Saber 11 exam, Panel B uses the 60th–70th percentile, and Panel C uses the 50th–60th percentile. All specifications include the same controls and fixed effects as in the baseline model, and standard errors are clustered at the household level.

Table 16: Younger Sibling: Dropouts and Absenteeism

	Grade of the Younger Sibling										
	1st	2nd	3rd	4th	5th	6th	7th	8th	9th	10th	11th
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Differences-in-Differences Model:											
	Younger Sibling Drops out										
Eligible × Post	-0.001	0.000	0.002*	-0.000	0.001	-0.001	0.001	0.000	-0.001	-0.001	-0.002*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Almost Eligible × Post	-0.000	0.001	0.000	0.001	0.001	0.001	0.001	0.000	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Observations	247119	271296	290262	300848	304404	306228	287153	261734	232018	215521	202319
Mean	0.0112	0.0097	0.0096	0.0081	0.0134	0.0331	0.0312	0.0340	0.0352	0.0315	0.0045
Effect relative to the mean (Elig.)	n.s.	n.s.	17%	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.	-35%
Effect relative to the mean (Almost Elig.)	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.
	Younger Sibling is absent for a year										
Eligible × Post	-0.001	0.003	0.000	-0.003	-0.002	-0.004	-0.002	0.002	-0.002	-0.002	-0.001
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.001)
Almost Eligible × Post	0.000	0.001	0.001	-0.000	0.002	0.002	-0.001	0.000	-0.002	-0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.001)
Observations	247961	272688	292738	304506	310379	314757	298660	276801	249233	217544	184559
Mean	0.0532	0.0428	0.0428	0.0368	0.0441	0.0631	0.0626	0.0693	0.0787	0.0796	0.0060
Effect relative to the mean (Elig.)	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.
Effect relative to the mean (Almost Elig.)	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.

A Appendix: Older Sibling

Table A.1: Immediate Enrollment in Postsecondary Education, by Type of Institution

		Low-quality			High-quality		
	(1) Any	(2) Any	(3) Public	(4) Private	(5) Any	(6) Public	(7) Private
Differences-in-Differences Model:							
Eligible × Post	0.187***	-0.101***	-0.061***	-0.040***	0.288***	-0.027***	0.316***
	(0.007)	(0.007)	(0.005)	(0.004)	(0.008)	(0.008)	(0.010)
Almost Eligible × Post	0.029***	-0.025***	-0.013***	-0.012***	0.054***	0.043***	0.011***
	(0.005)	(0.004)	(0.003)	(0.002)	(0.004)	(0.004)	(0.002)
Event Study Model:							
Eligible: Year -4	0.010	0.049***	0.035***	0.014*	-0.039**	-0.023*	-0.017*
	(0.017)	(0.011)	(0.010)	(0.006)	(0.013)	(0.011)	(0.008)
Eligible: Year -3	-0.011	0.047***	0.043***	0.005	-0.059***	-0.052***	-0.006
	(0.016)	(0.011)	(0.009)	(0.006)	(0.011)	(0.011)	(0.007)
Eligible: Year -2	0.024	0.035**	0.032***	0.003	-0.012	-0.002	-0.010
	(0.017)	(0.012)	(0.009)	(0.006)	(0.013)	(0.012)	(0.008)
Eligible: Year 0	0.163***	-0.058***	-0.030**	-0.028***	0.221***	-0.083***	0.304***
	(0.017)	(0.016)	(0.011)	(0.008)	(0.016)	(0.012)	(0.017)
Eligible: Year 1	0.196***	-0.075***	-0.035***	-0.040***	0.271***	-0.110***	0.381***
	(0.016)	(0.014)	(0.010)	(0.008)	(0.014)	(0.011)	(0.012)
Eligible: Year 2	0.254***	-0.097***	-0.052***	-0.045***	0.351***	-0.082***	0.433***
	(0.018)	(0.015)	(0.012)	(0.007)	(0.014)	(0.017)	(0.012)
Eligible: Year 3	0.236***	-0.073***	-0.035***	-0.037***	0.309***	-0.049*	0.358***
	(0.018)	(0.013)	(0.010)	(0.007)	(0.016)	(0.020)	(0.018)
Eligible: Year 4	0.160***	-0.059***	-0.023*	-0.036***	0.218***	0.116***	0.102***
	(0.019)	(0.014)	(0.011)	(0.007)	(0.017)	(0.017)	(0.011)
Eligible: Year 5	0.170***	-0.068***	-0.034**	-0.034***	0.238***	0.022	0.216***
	(0.020)	(0.014)	(0.011)	(0.007)	(0.017)	(0.023)	(0.021)
Eligible: Year 6	0.137***	-0.033*	-0.014	-0.018**	0.170***	0.001	0.169***
	(0.024)	(0.014)	(0.011)	(0.007)	(0.022)	(0.021)	(0.022)
Almost Eligible: Year -4	0.006	0.017*	0.012	0.006	-0.012	-0.006	-0.006
	(0.011)	(0.007)	(0.008)	(0.004)	(0.008)	(0.007)	(0.005)
Almost Eligible: Year -3	-0.004	0.012	0.015*	-0.002	-0.017*	-0.013*	-0.003
	(0.011)	(0.007)	(0.008)	(0.004)	(0.008)	(0.007)	(0.004)
Almost Eligible: Year -2	-0.001	0.009	0.010	-0.000	-0.010	-0.008	-0.002
	(0.011)	(0.008)	(0.008)	(0.004)	(0.008)	(0.008)	(0.005)
Almost Eligible: Year 0	0.010	0.004	0.010	-0.006	0.006	0.002	0.005
	(0.011)	(0.007)	(0.008)	(0.004)	(0.008)	(0.007)	(0.005)
Almost Eligible: Year 1	0.015	-0.012	-0.001	-0.011*	0.026**	0.012	0.014**
	(0.011)	(0.008)	(0.008)	(0.005)	(0.009)	(0.008)	(0.005)
Almost Eligible: Year 2	0.028*	-0.027***	-0.015	-0.012*	0.054***	0.045***	0.009
	(0.012)	(0.008)	(0.008)	(0.005)	(0.009)	(0.009)	(0.006)
Almost Eligible: Year 3	0.039***	-0.016*	-0.005	-0.011*	0.054***	0.050***	0.004
	(0.011)	(0.008)	(0.008)	(0.004)	(0.010)	(0.009)	(0.005)
Almost Eligible: Year 4	0.053***	-0.019*	-0.006	-0.013**	0.072***	0.064***	0.008
	(0.014)	(0.008)	(0.008)	(0.005)	(0.011)	(0.011)	(0.005)
Almost Eligible: Year 5	0.041**	-0.021*	-0.009	-0.012*	0.062***	0.055***	0.007
	(0.015)	(0.008)	(0.008)	(0.005)	(0.012)	(0.011)	(0.005)
Almost Eligible: Year 6	0.011	-0.015*	-0.004	-0.011**	0.026*	0.025**	0.001
	(0.013)	(0.008)	(0.008)	(0.004)	(0.011)	(0.010)	(0.005)
Observations	286965	286965	286965	286965	286965	286965	286965
Mean	0.40	0.20	0.14	0.06	0.19	0.13	0.07

Note: The dependent variables are the likelihood of enrolling in a higher education institution, with distinctions in columns (2) to (4) and (5) to (7) between high-quality and low-quality higher education institutions by sector type. The sample are older siblings whose families meet the Sisben eligibility criteria, i.e., those in need.

Figure A.1: Event Study: Immediate Post-Secondary Enrollment in a Higher Education Institution

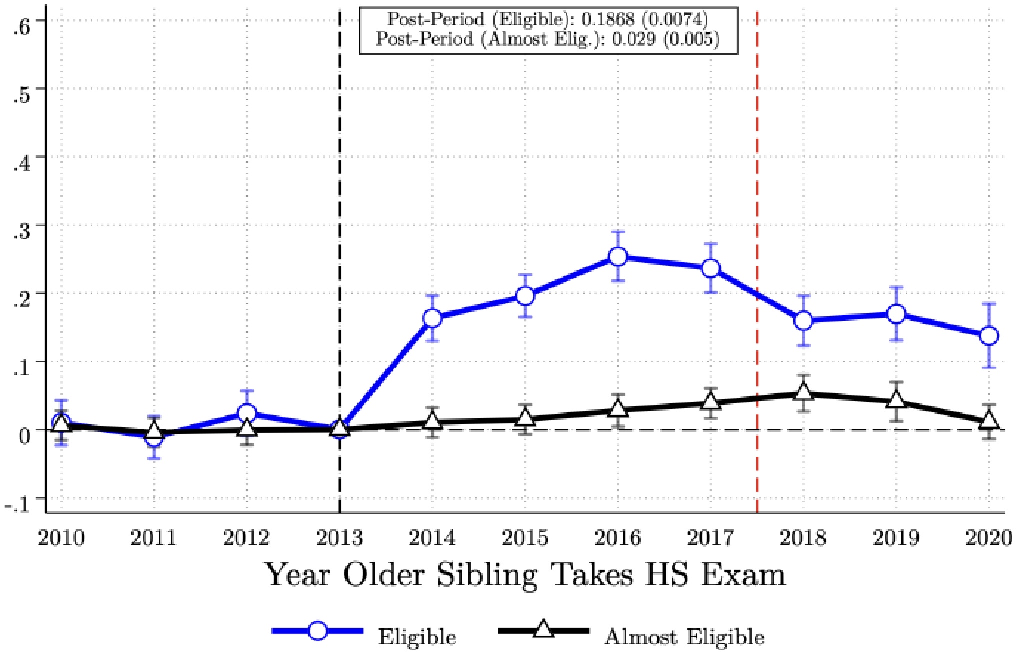
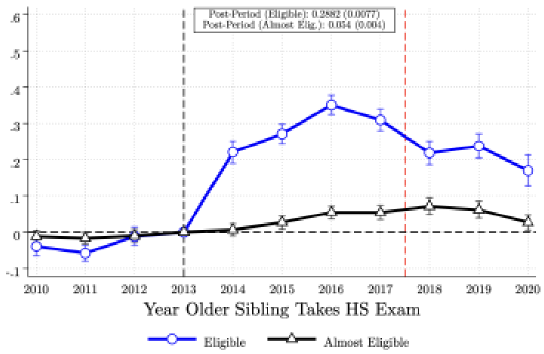
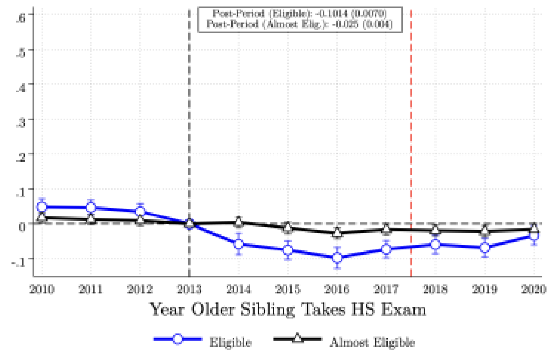


Table A.2: Event Study Plots by Institution Type and Quality

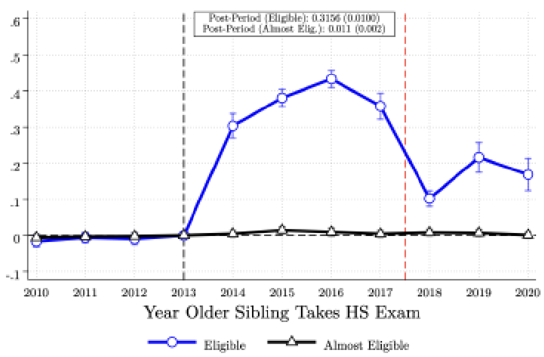
(a) High-quality



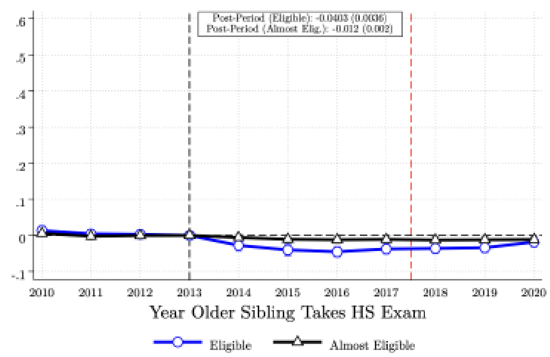
(b) Low-quality



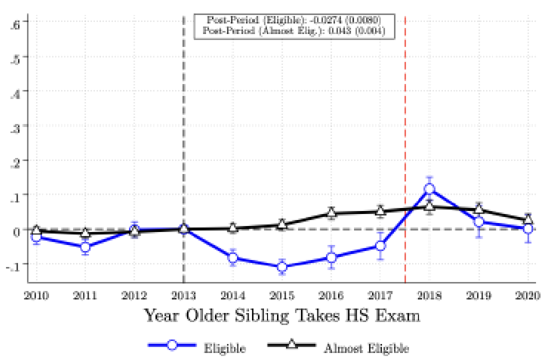
(c) High-quality Private



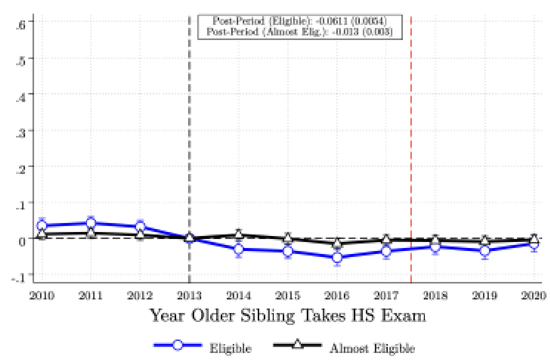
(d) Low-quality Private



(e) High-quality Public



(f) Low-quality Public



B Appendix: Replication

B.1 Differences in Immediate Enrollment: Comparison with SPP Estimates by Londoño-Vélez et al. (2020)

We replicate the effect of the Ser Pilo Paga program estimated by Londoño-Vélez et al. (2020) for the first cohort, launched in 2014. Our results are presented in Figure B.1. We find that the probability of immediate enrollment in a higher education institution increases by 26.6 percentage points. This estimate is lower than the 32.8 percentage point increase reported by the authors. The difference can be explained by the data sources used in each analysis. Our estimates are based on SNIES data, which includes enrollment in vocational training institutions. In contrast, Londoño-Vélez et al. (2020) uses SPADIES data, which does not capture this type of enrollment. This distinction is particularly relevant for students just below the eligibility cutoff, who are more likely to attend institutions such as SENA.

Figure B.1: Immediate Enrollment in Postsecondary Education: Older Siblings and All the test-takers in the Fall 2014

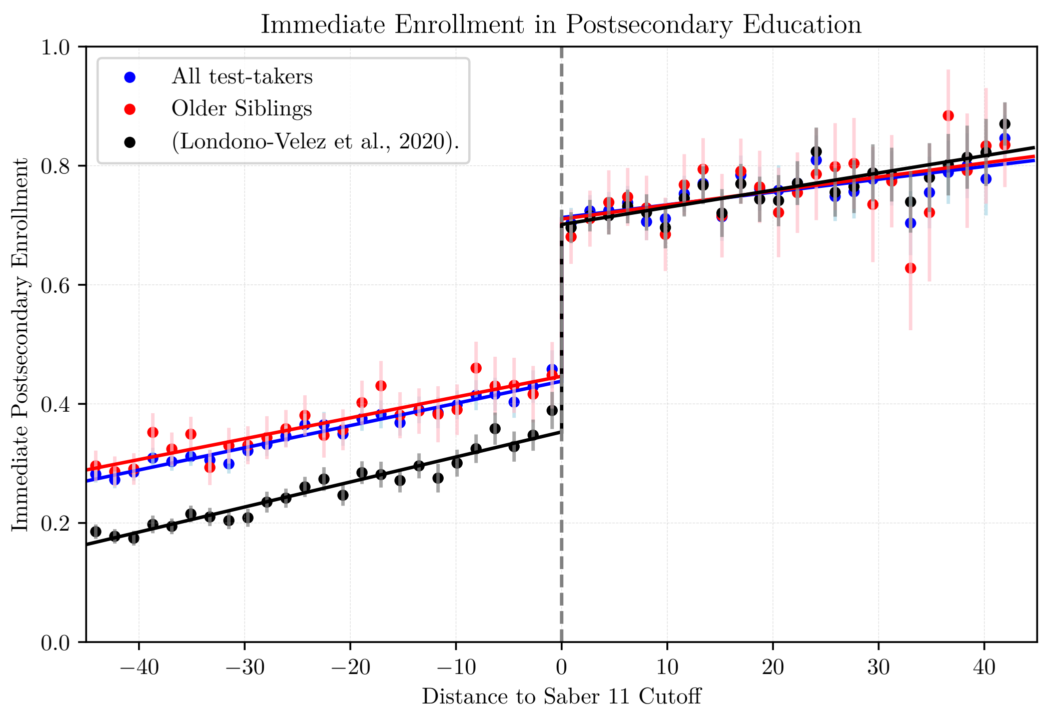


Table B.1: **Immediate Enrollment in Postsecondary Education, by Type of Institution**

		High quality			Low quality		
	Any	Any	Private	Public	Any	Private	Public
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Overall Effects							
RF	0.266	0.361	0.433	-0.075	-0.093	-0.037	-0.056
	(0.026)	(0.024)	(0.013)	(0.011)	(0.009)	(0.005)	(0.007)
Mean Control	0.439	0.276	0.076	0.203	0.161	0.06	0.099
Observations	266402	266402	266402	266402	266402	266402	266402
BW loc. poly.	32.346	28.979	30.87	22.493	23.328	33.577	23.502
Effect obs. control	34253	28953	31834	19894	20864	36279	21358
Effect obs. treat	11331	10758	11104	9332	9551	11524	9646
Panel B. Older Siblings							
RF	0.246	0.365	0.428	-0.062	-0.116	-0.044	-0.071
	(0.027)	(0.025)	(0.022)	(0.017)	(0.016)	(0.01)	(0.013)
Mean Control	0.44	0.257	0.074	0.184	0.183	0.07	0.112
Observations	80955	80955	80955	80955	80955	80955	80955
BW loc. poly.	20.451	22.658	22.411	30.686	27.034	29.155	27.449
Effect obs. control	5708	6504	6492	10015	8503	9341	8667
Effect obs. treat	2960	3117	3117	3635	3432	3541	3451
Panel C. Overall Effects (Londoño-Vélez et al., 2020)							
RF	0.328	0.484	0.484	-0.002	-0.16	-0.069	-0.088
	(0.013)	(0.012)	(0.011)	(0.007)	(0.012)	(0.007)	(0.009)
Mean Control	0.372	0.104	0.03	0.075	0.27	0.109	0.159
Observations	266402	266402	266402	266402	266402	266402	266402
BW loc. poly.	26.48	28.297	25.787	27.09	22.016	25.776	24.551
Effect obs. control	25050	27856	24542	26174	19439	24542	22425
Effect obs. treat	10230	10614	10159	10393	9231	10071	9806

Note: The dependent variables are the likelihood of enrolling in a higher education institution, with distinctions in columns (2) to (4) and (5) to (7) between high-quality and low-quality higher education institutions by sector type. The sample are older siblings whose families meet the Sisben eligibility criteria, i.e., those in need.

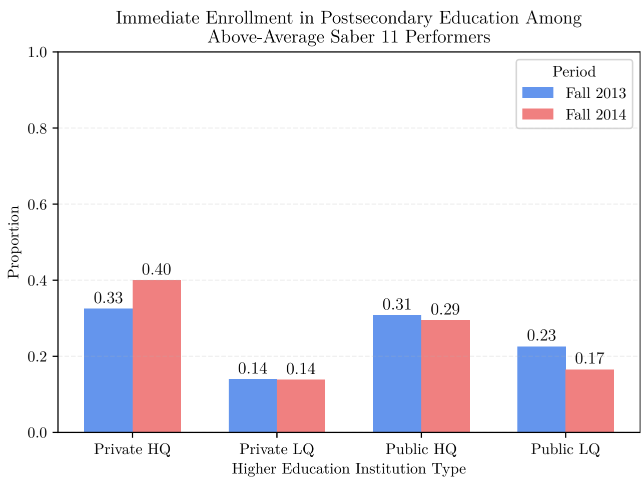
C Appendix: HEI Supply Analysis

C.1 Do SPP-Eligible Students Free Up Seats in Public Universities?

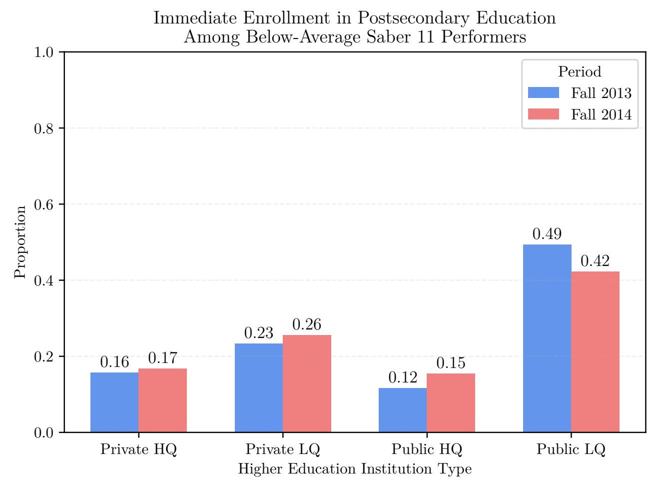
In the results, we show that the increase in enrollment rates is driven by eligible students gaining greater access to private, high-quality higher education institutions. We also observe that almost-eligible students experience increases in enrollment, driven primarily by higher attendance at high-quality public institutions, which tend to be more competitive.

One hypothesis is that students eligible for SPP enroll in high-quality private universities, thereby freeing up seats in competitive public institutions for students just below the eligibility threshold. Figure C.1 illustrates this pattern: on average, students performing above the cutoff in the standardized high school exit exam between 2013 and 2014 increase their attendance at private high-quality institutions and reduce their attendance at public institutions, particularly those of lower quality. In contrast, Panel B of Figure C.1 shows that students below the cutoff decrease their attendance at low-quality public institutions and increase enrollment in private low-quality and public high-quality universities.

Figure C.1: Immediate Enrollment in Postsecondary Education, by Type of Institution



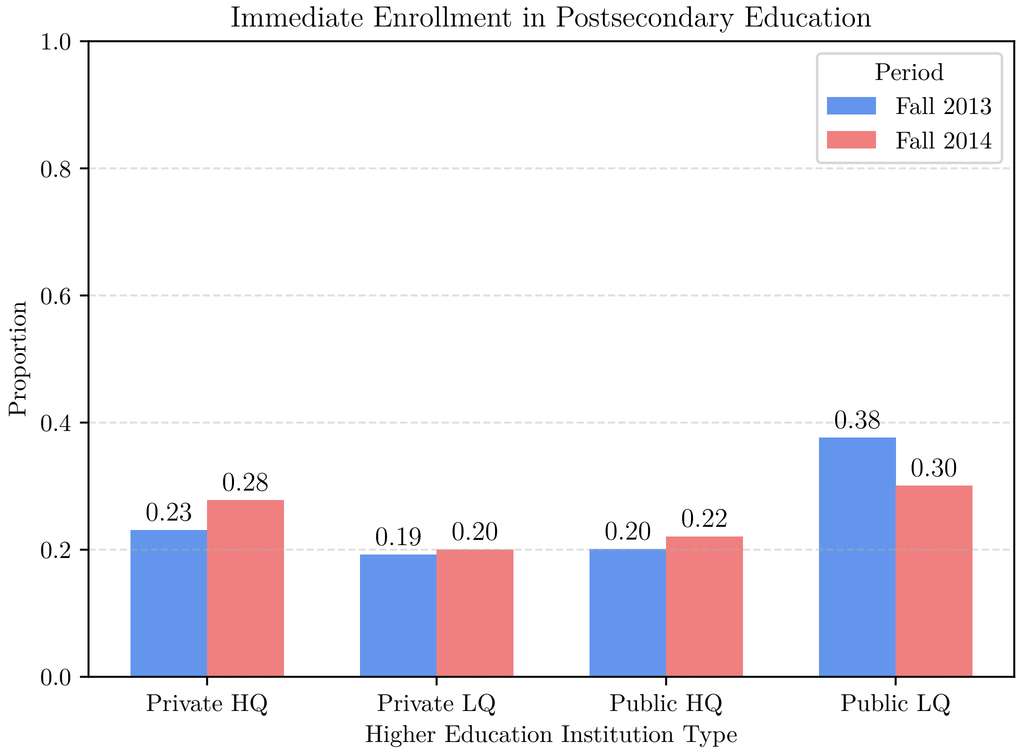
(b) Above-Average Saber 11



(d) Below-Average Saber 11

Note: Authors' calculations.

Figure C.2: Immediate Enrollment in Postsecondary Education: Older Siblings and All the test-takers in the Fall 2014



D Appendix: Additional Tables and Figures

Table D.1: Younger Sibling: Performance in National Standardized Test

	Extensive Margin	Intensive Margin		
	(1) HS Graduation	(2) Overall	(3) Math	(4) Spanish
Differences-in-Differences Model:				
Eligible × Post	-0.003**	0.078***	0.078***	0.087***
	(0.002)	(0.012)	(0.012)	(0.012)
Almost Eligible × Post	-0.002*	0.048***	0.045***	0.048***
	(0.001)	(0.009)	(0.009)	(0.009)
Event Study Model:				
Eligible: Year 2010	0.001	-0.017	-0.019	-0.036
	(0.003)	(0.022)	(0.023)	(0.023)
Eligible: Year 2011	-0.001	-0.040*	-0.054**	-0.032
	(0.003)	(0.022)	(0.022)	(0.022)
Eligible: Year 2012	-0.005	-0.004	-0.008	-0.002
	(0.003)	(0.022)	(0.023)	(0.022)
Eligible: Year 2014	-0.003	0.012	0.021	0.017
	(0.003)	(0.022)	(0.022)	(0.022)
Eligible: Year 2015	-0.003	0.055**	0.049**	0.067***
	(0.003)	(0.022)	(0.023)	(0.023)
Eligible: Year 2016	-0.003	0.111***	0.088***	0.099***
	(0.003)	(0.027)	(0.027)	(0.027)
Eligible: Year 2017	-0.005	0.142***	0.102***	0.147***
	(0.004)	(0.033)	(0.034)	(0.033)
Almost Eligible: Year 2010	-0.000	0.003	0.009	0.006
	(0.003)	(0.018)	(0.018)	(0.019)
Almost Eligible: Year 2011	-0.001	-0.013	-0.011	-0.016
	(0.003)	(0.018)	(0.018)	(0.019)
Almost Eligible: Year 2012	-0.004	0.002	-0.001	0.018
	(0.003)	(0.018)	(0.018)	(0.018)
Almost Eligible: Year 2014	-0.002	0.007	0.022	0.010
	(0.003)	(0.018)	(0.019)	(0.019)
Almost Eligible: Year 2015	-0.003	0.029	0.025	0.034*
	(0.002)	(0.018)	(0.019)	(0.019)
Almost Eligible: Year 2016	-0.004	0.070***	0.066***	0.070***
	(0.002)	(0.018)	(0.019)	(0.019)
Almost Eligible: Year 2017	-0.007***	0.085***	0.068***	0.093***
	(0.003)	(0.020)	(0.020)	(0.020)
Observations	276802	213220	212764	212764

Note: This table reports the event-study coefficients estimated from Equation (2) for younger siblings' educational outcomes. Each column corresponds to a separate regression for the indicated outcome. Coefficients measure the difference in outcomes between younger siblings of eligible ($T1$) or almost-eligible ($T2$) older siblings and those in the control group, relative to the 2013 cohort (the last pre-program year). All outcomes are standardized by cohort to have mean zero and unit variance; coefficients are therefore expressed in standard deviation units. The specification includes school and municipality-by-cohort fixed effects and controls for baseline family characteristics (mother's education, number of siblings, socioeconomic status) interacted with cohort dummies. Standard errors are clustered at the family level.

Table D.2: Younger Sibling: Immediate Enrollment in Postsecondary Education, by Institution Type and Quality

			Low-quality			High-quality			Program Duration	
	(1) SPP	(2) Any	(3) Any	(4) Public	(5) Private	(6) Any	(7) Public	(8) Private	(9) Two-years	(10) Four-years
Differences-in-Differences Model:										
T1 × Post	0.013***	0.030***	-0.007	-0.006	-0.001	0.037***	-0.003	0.039***	-0.010**	0.040***
	(0.003)	(0.007)	(0.005)	(0.004)	(0.003)	(0.006)	(0.005)	(0.004)	(0.005)	(0.006)
T2 × Post	-0.002	0.007	-0.002	0.001	-0.003	0.009**	0.008**	0.001	-0.004	0.011**
	(0.002)	(0.005)	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)
Event Study Model:										
Eligible: Year 2010	-0.008*	-0.000	-0.000	-0.003	0.003	0.000	0.001	-0.001	0.003	-0.003
	(0.005)	(0.012)	(0.009)	(0.008)	(0.005)	(0.010)	(0.008)	(0.007)	(0.008)	(0.010)
Eligible: Year 2011	-0.007	0.003	0.010	0.004	0.007	-0.008	-0.005	-0.003	0.012	-0.008
	(0.004)	(0.012)	(0.009)	(0.007)	(0.005)	(0.010)	(0.008)	(0.006)	(0.008)	(0.010)
Eligible: Year 2012	-0.001	0.013	-0.001	-0.000	-0.001	0.014	0.012	0.002	0.003	0.010
	(0.005)	(0.012)	(0.009)	(0.008)	(0.006)	(0.010)	(0.008)	(0.007)	(0.008)	(0.011)
Eligible: Year 2014	0.011**	0.026**	0.003	-0.003	0.006	0.024**	-0.014*	0.038***	0.007	0.020*
	(0.005)	(0.012)	(0.009)	(0.008)	(0.006)	(0.010)	(0.008)	(0.007)	(0.008)	(0.011)
Eligible: Year 2015	0.009*	0.029**	-0.006	-0.006	-0.001	0.036***	0.004	0.031***	-0.014*	0.043***
	(0.005)	(0.013)	(0.009)	(0.008)	(0.006)	(0.011)	(0.009)	(0.008)	(0.008)	(0.011)
Eligible: Year 2016		0.059***	-0.015	-0.012	-0.004	0.074***	0.012	0.062***	-0.015	0.074***
		(0.016)	(0.011)	(0.009)	(0.007)	(0.014)	(0.011)	(0.010)	(0.010)	(0.014)
Eligible: Year 2017		0.029	-0.001	-0.006	0.004	0.030*	0.003	0.027**	-0.011	0.041**
		(0.020)	(0.014)	(0.011)	(0.009)	(0.017)	(0.014)	(0.012)	(0.012)	(0.018)
Almost Eligible: Year 2010	-0.002	0.009	0.006	0.004	0.001	0.004	-0.000	0.004	0.007	0.001
	(0.003)	(0.010)	(0.008)	(0.006)	(0.005)	(0.007)	(0.006)	(0.005)	(0.007)	(0.008)
Almost Eligible: Year 2011	-0.001	0.007	0.005	0.007	-0.002	0.002	-0.006	0.007	0.006	0.001
	(0.003)	(0.010)	(0.008)	(0.007)	(0.005)	(0.008)	(0.006)	(0.005)	(0.007)	(0.008)
Almost Eligible: Year 2012	0.001	0.009	-0.003	0.003	-0.006	0.012	0.008	0.004	0.000	0.008
	(0.003)	(0.010)	(0.008)	(0.007)	(0.005)	(0.008)	(0.006)	(0.005)	(0.007)	(0.008)
Almost Eligible: Year 2014	-0.002	0.012	0.009	0.006	0.002	0.003	-0.002	0.005	0.006	0.006
	(0.003)	(0.010)	(0.008)	(0.007)	(0.005)	(0.008)	(0.006)	(0.005)	(0.007)	(0.009)
Almost Eligible: Year 2015	-0.004	0.007	0.002	0.009	-0.007	0.006	0.006	0.000	-0.005	0.012
	(0.003)	(0.011)	(0.008)	(0.007)	(0.005)	(0.008)	(0.007)	(0.006)	(0.007)	(0.009)
Almost Eligible: Year 2016		0.016	-0.011	-0.004	-0.007	0.026***	0.015**	0.012**	-0.007	0.022**
		(0.011)	(0.008)	(0.007)	(0.005)	(0.008)	(0.007)	(0.005)	(0.008)	(0.009)
Almost Eligible: Year 2017		0.020*	-0.002	0.007	-0.009	0.022**	0.019**	0.003	0.001	0.019**
		(0.012)	(0.009)	(0.008)	(0.006)	(0.009)	(0.007)	(0.006)	(0.008)	(0.010)
Observations	151741	178819	178819	178819	178819	178819	178819	178819	178819	178819

Note: This table reports event-study coefficients estimated from Equation (2) for younger siblings' enrollment outcomes immediately following high school. The dependent variables are defined as follows. Column (1) reports the probability of being a *Ser Pilo Paga* (SPP) scholarship recipient. Column (2) shows the enrolling in a higher education institution (HEI). Columns (3)–(5) and (6)–(8) distinguish between low- and high-quality HEIs by sector type. Columns (9) and (10) report the probability of enrolling in a two-year and four-year HEI program, respectively. Standard errors are clustered at the household level.

Table D.3: **Younger Sibling: Formal Employment X years after High School**

	Formal Employment X years after High School						
	Zero	One	Two	Three	Four	Five	Six
Differences-in-Differences Model:							
Eligible × Post	0.002	-0.009**	-0.012*	-0.035***	-0.028**	0.009	0.032
	(0.002)	(0.005)	(0.007)	(0.009)	(0.011)	(0.014)	(0.021)
Almost Eligible × Post	0.002	-0.008**	-0.005	-0.013*	-0.007	0.007	0.042***
	(0.002)	(0.004)	(0.006)	(0.007)	(0.009)	(0.011)	(0.016)
Event Study Model:							
Eligible: Year -4	-0.003	0.021***	0.014	0.004	0.002	-0.020	-0.026
	(0.004)	(0.008)	(0.012)	(0.013)	(0.016)	(0.017)	(0.021)
Eligible: Year -3	-0.004	0.012	0.006	0.015	0.007	-0.007	0.007
	(0.004)	(0.008)	(0.011)	(0.013)	(0.015)	(0.017)	(0.021)
Eligible: Year -2	-0.004	0.011	-0.016	-0.004	0.002	-0.024	-0.015
	(0.004)	(0.008)	(0.012)	(0.014)	(0.016)	(0.019)	(0.023)
Eligible: Year 0	-0.001	0.011	0.004	-0.013	-0.003	0.000	0.008
	(0.004)	(0.008)	(0.012)	(0.015)	(0.018)	(0.021)	(0.031)
Eligible: Year 1	-0.003	-0.006	-0.022*	-0.049***	-0.055***	-0.027	0.048
	(0.004)	(0.008)	(0.013)	(0.016)	(0.021)	(0.027)	(0.037)
Eligible: Year 2	-0.002	-0.001	-0.013	-0.038*	-0.017	0.086*	0.058
	(0.005)	(0.011)	(0.018)	(0.023)	(0.035)	(0.052)	(0.113)
Eligible: Year 3	0.011*	0.007	-0.032	-0.056	-0.081	-0.018	-0.006
	(0.006)	(0.013)	(0.023)	(0.037)	(0.057)	(0.110)	(0.126)
Eligible: Year 4	-0.002	-0.010	-0.009	-0.054	-0.079	-0.075	-0.059
	(0.007)	(0.016)	(0.036)	(0.060)	(0.099)	(0.113)	(0.128)
Eligible: Year 5	0.011	-0.000	-0.088	-0.082	-0.014	-0.139	0.185
	(0.011)	(0.028)	(0.054)	(0.094)	(0.124)	(0.131)	(0.192)
Almost Eligible: Year -4	-0.000	0.008	0.004	0.024**	0.007	-0.012	-0.011
	(0.003)	(0.007)	(0.010)	(0.012)	(0.013)	(0.015)	(0.018)
Almost Eligible: Year -3	-0.001	0.008	0.010	0.024**	0.023*	0.013	-0.001
	(0.004)	(0.007)	(0.010)	(0.012)	(0.013)	(0.015)	(0.018)
Almost Eligible: Year -2	-0.000	0.007	0.003	0.008	-0.004	-0.020	-0.008
	(0.004)	(0.007)	(0.010)	(0.012)	(0.014)	(0.016)	(0.019)
Almost Eligible: Year 0	0.003	0.004	0.004	0.007	0.012	0.005	0.045*
	(0.004)	(0.008)	(0.011)	(0.013)	(0.015)	(0.018)	(0.024)
Almost Eligible: Year 1	0.001	-0.004	0.001	-0.002	-0.004	-0.007	0.023
	(0.004)	(0.008)	(0.011)	(0.015)	(0.018)	(0.023)	(0.032)
Almost Eligible: Year 2	0.004	-0.009	-0.003	-0.005	-0.022	0.027	0.066
	(0.004)	(0.008)	(0.012)	(0.016)	(0.021)	(0.029)	(0.066)
Almost Eligible: Year 3	0.000	0.001	-0.015	0.002	0.021	0.013	-0.015
	(0.004)	(0.009)	(0.015)	(0.022)	(0.032)	(0.058)	(0.078)
Almost Eligible: Year 4	-0.003	-0.001	0.003	0.019	-0.021	-0.026	-0.037
	(0.005)	(0.011)	(0.020)	(0.031)	(0.054)	(0.073)	(0.087)
Almost Eligible: Year 5	0.008	-0.011	0.009	0.039	-0.126*	-0.133	0.041
	(0.006)	(0.015)	(0.029)	(0.055)	(0.070)	(0.088)	(0.105)
Observations	188898	164691	142016	118281	95770	74980	54766
Mean	0.03	0.11	0.25	0.33	0.37	0.42	0.45

Note: The dependent variables are , with distinctions in columns (2) to (4) and (5) to (7) between. The sample are younger siblings whose families meet the Sisben eligibility criteria, i.e., those in need.

Table D.4: Younger Sibling: Earnings X years after High School

	Earnings X years after High School						
	Zero	One	Two	Three	Four	Five	Six
Differences-in-Differences Model:							
Eligible × Post	0.005**	-0.006	-0.009	-0.049***	-0.050***	0.010	0.043
	(0.002)	(0.005)	(0.008)	(0.011)	(0.016)	(0.023)	(0.040)
Almost Eligible × Post	0.004**	-0.003	0.001	-0.010	-0.001	0.021	0.033
	(0.002)	(0.004)	(0.007)	(0.010)	(0.013)	(0.018)	(0.028)
Event Study Model:							
Eligible: Year -4	-0.003	0.015*	0.011	0.021	0.005	-0.001	-0.025
	(0.003)	(0.008)	(0.012)	(0.015)	(0.019)	(0.023)	(0.032)
Eligible: Year -3	-0.006*	0.000	-0.005	0.027*	0.004	0.005	0.000
	(0.003)	(0.007)	(0.011)	(0.014)	(0.018)	(0.023)	(0.032)
Eligible: Year -2	-0.004	0.002	-0.013	-0.011	0.006	-0.012	0.006
	(0.003)	(0.007)	(0.011)	(0.014)	(0.019)	(0.025)	(0.035)
Eligible: Year 0	0.002	0.006	-0.000	-0.018	-0.023	-0.000	-0.010
	(0.003)	(0.007)	(0.011)	(0.016)	(0.020)	(0.028)	(0.048)
Eligible: Year 1	0.001	-0.002	-0.016	-0.052***	-0.074***	-0.032	0.075
	(0.003)	(0.007)	(0.012)	(0.016)	(0.023)	(0.035)	(0.062)
Eligible: Year 2	-0.002	-0.011	-0.000	-0.008	0.020	0.167**	0.138
	(0.003)	(0.009)	(0.016)	(0.025)	(0.040)	(0.074)	(0.142)
Eligible: Year 3	0.005	0.009	-0.029	-0.004	-0.003	0.061	-0.035
	(0.003)	(0.011)	(0.021)	(0.038)	(0.065)	(0.112)	(0.129)
Eligible: Year 4	0.000	-0.004	-0.027	-0.092*	-0.173	0.058	0.122
	(0.004)	(0.013)	(0.028)	(0.051)	(0.108)	(0.271)	(0.599)
Eligible: Year 5	0.001	-0.003	-0.079	-0.105	0.150	-0.014	0.107
	(0.005)	(0.024)	(0.049)	(0.111)	(0.157)	(0.177)	(0.240)
Almost Eligible: Year -4	-0.001	0.001	0.005	0.011	0.003	-0.012	-0.022
	(0.003)	(0.007)	(0.010)	(0.012)	(0.016)	(0.019)	(0.028)
Almost Eligible: Year -3	-0.002	0.003	0.002	0.010	0.007	0.002	-0.025
	(0.003)	(0.007)	(0.010)	(0.012)	(0.015)	(0.020)	(0.028)
Almost Eligible: Year -2	-0.005	0.004	-0.001	-0.007	-0.005	-0.022	-0.027
	(0.003)	(0.007)	(0.010)	(0.013)	(0.017)	(0.022)	(0.030)
Almost Eligible: Year 0	0.005*	0.002	0.004	-0.002	0.015	0.019	0.032
	(0.003)	(0.007)	(0.010)	(0.015)	(0.018)	(0.024)	(0.038)
Almost Eligible: Year 1	0.001	0.001	0.005	-0.005	-0.015	-0.019	-0.047
	(0.003)	(0.007)	(0.011)	(0.015)	(0.020)	(0.030)	(0.048)
Almost Eligible: Year 2	0.001	-0.003	0.006	-0.014	-0.017	0.005	0.024
	(0.003)	(0.007)	(0.011)	(0.017)	(0.025)	(0.038)	(0.088)
Almost Eligible: Year 3	0.000	0.005	-0.013	-0.007	0.042	0.051	-0.147
	(0.003)	(0.008)	(0.015)	(0.022)	(0.037)	(0.068)	(0.095)
Almost Eligible: Year 4	-0.002	0.001	0.019	0.054	0.002	0.056	-0.046
	(0.003)	(0.009)	(0.019)	(0.034)	(0.066)	(0.088)	(0.116)
Almost Eligible: Year 5	0.001	-0.012	-0.006	0.004	-0.013	-0.078	0.120
	(0.004)	(0.012)	(0.028)	(0.090)	(0.079)	(0.127)	(0.141)
Observations	188898	164691	142016	118281	95770	74980	54766
Mean	0.01	0.08	0.19	0.27	0.32	0.37	0.46

Note: The dependent variables are , with distinctions in columns (2) to (4) and (5) to (7) between. The sample are younger siblings whose families meet the Sisben eligibility criteria, i.e., those in need.

Table D.5: Younger Sibling: Earnings Top 25% X years after High School

	Earnings X years after High School						
	Zero	One	Two	Three	Four	Five	Six
Differences-in-Differences Model:							
Eligible × Post	0.000	0.002	0.003	-0.009*	-0.014**	0.006	0.020
	(0.001)	(0.002)	(0.003)	(0.004)	(0.006)	(0.009)	(0.014)
Almost Eligible × Post	0.000	0.002	0.004*	0.004	-0.000	0.009	0.018
	(0.001)	(0.001)	(0.002)	(0.004)	(0.005)	(0.007)	(0.011)
Event Study Model:							
Eligible: Year -4	-0.001	0.002	0.004	0.006	0.006	0.003	-0.008
	(0.001)	(0.003)	(0.005)	(0.008)	(0.010)	(0.011)	(0.015)
Eligible: Year -3	-0.003**	-0.000	-0.001	0.006	0.005	0.004	-0.005
	(0.001)	(0.003)	(0.005)	(0.007)	(0.009)	(0.011)	(0.015)
Eligible: Year -2	-0.002	-0.001	-0.001	-0.002	-0.006	-0.000	0.017
	(0.001)	(0.003)	(0.005)	(0.007)	(0.010)	(0.012)	(0.016)
Eligible: Year 0	-0.001	0.004	0.004	-0.002	-0.009	-0.002	0.007
	(0.001)	(0.003)	(0.005)	(0.008)	(0.010)	(0.014)	(0.021)
Eligible: Year 1	-0.001	0.001	0.000	-0.012	-0.019*	0.008	0.055**
	(0.001)	(0.003)	(0.005)	(0.008)	(0.011)	(0.017)	(0.027)
Eligible: Year 2	-0.003**	0.001	0.017**	0.003	-0.013	0.092***	-0.036
	(0.001)	(0.003)	(0.007)	(0.011)	(0.017)	(0.034)	(0.065)
Eligible: Year 3	-0.001	0.002	-0.000	0.005	0.023	0.065	0.021
	(0.001)	(0.005)	(0.009)	(0.018)	(0.032)	(0.057)	(0.062)
Eligible: Year 4	-0.001	0.008	-0.007	-0.043**	-0.107	-0.007	-0.133
	(0.002)	(0.005)	(0.010)	(0.017)	(0.066)	(0.100)	(0.122)
Eligible: Year 5	-0.003*	0.010	-0.016	-0.023	0.078	-0.008	0.085
	(0.002)	(0.009)	(0.012)	(0.060)	(0.080)	(0.098)	(0.145)
Almost Eligible: Year -4	-0.002	0.000	-0.001	0.001	-0.004	-0.005	-0.006
	(0.001)	(0.003)	(0.004)	(0.006)	(0.008)	(0.010)	(0.013)
Almost Eligible: Year -3	-0.001	-0.001	-0.002	0.000	0.005	-0.006	-0.012
	(0.001)	(0.002)	(0.004)	(0.006)	(0.008)	(0.010)	(0.013)
Almost Eligible: Year -2	-0.001	-0.001	-0.003	-0.005	-0.011	-0.014	-0.010
	(0.001)	(0.003)	(0.004)	(0.006)	(0.008)	(0.011)	(0.014)
Almost Eligible: Year 0	0.000	0.003	0.000	0.004	0.001	0.006	0.009
	(0.001)	(0.003)	(0.005)	(0.007)	(0.009)	(0.012)	(0.018)
Almost Eligible: Year 1	-0.001	0.001	0.001	0.007	-0.010	-0.003	0.005
	(0.001)	(0.003)	(0.005)	(0.007)	(0.010)	(0.014)	(0.021)
Almost Eligible: Year 2	-0.001	-0.001	0.008	0.003	-0.007	0.017	0.028
	(0.001)	(0.003)	(0.005)	(0.008)	(0.012)	(0.018)	(0.048)
Almost Eligible: Year 3	-0.000	0.000	0.006	-0.011	0.016	-0.010	-0.031
	(0.001)	(0.003)	(0.006)	(0.011)	(0.020)	(0.029)	(0.046)
Almost Eligible: Year 4	-0.002	0.004	0.008	0.014	-0.024	-0.027	0.036
	(0.001)	(0.003)	(0.008)	(0.014)	(0.033)	(0.043)	(0.068)
Almost Eligible: Year 5	-0.001	-0.001	0.003	-0.034	0.008	-0.042	0.095
	(0.002)	(0.004)	(0.011)	(0.029)	(0.040)	(0.062)	(0.071)
Observations	188898	164691	142016	118281	95770	74980	54766
Mean	0.00	0.01	0.03	0.06	0.08	0.10	0.13

Note: The dependent variables are , with distinctions in columns (2) to (4) and (5) to (7) between. The sample are younger siblings whose families meet the Sisben eligibility criteria, i.e., those in need.

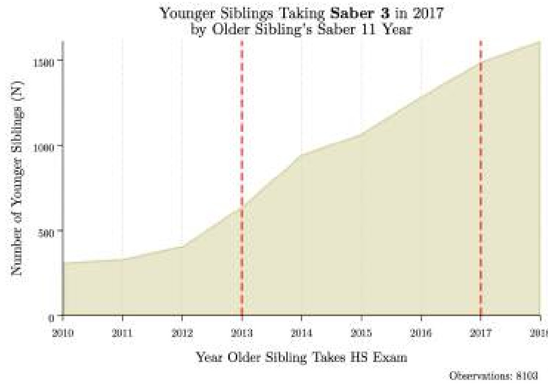
Table D.6: Younger Sibling: Crime

	(1) Any crime	(2) Economic	(3) Drugs	(4) Violent	(5) Others
Differences-in-Differences Model:					
Eligible × Post	-0.002***	-0.001	-0.001***	-0.000**	0.000
	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)
Almost Eligible × Post	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Event Study Model:					
Eligible: Year 2010	-0.001	-0.001	0.001	-0.000	-0.000
	(0.002)	(0.001)	(0.001)	(0.000)	(0.001)
Eligible: Year 2011	-0.001	-0.001	0.000	0.001	-0.001
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Eligible: Year 2012	-0.001	0.002*	-0.001	-0.001**	-0.001
	(0.002)	(0.001)	(0.001)	(0.000)	(0.001)
Eligible: Year 2014	-0.002	-0.000	-0.001**	-0.001	0.000
	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
Eligible: Year 2015	-0.003	-0.001*	-0.001	-0.001	-0.000
	(0.002)	(0.001)	(0.001)	(0.000)	(0.001)
Eligible: Year 2016	-0.003***	-0.002**	-0.001	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)
Eligible: Year 2017	-0.004***	-0.001	-0.001*	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Almost Eligible: Year 2010	0.001	-0.000	0.001	-0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)
Almost Eligible: Year 2011	-0.001	-0.000	-0.000	-0.000	0.000
	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)
Almost Eligible: Year 2012	-0.001	0.001	-0.001	-0.000	-0.001
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)
Almost Eligible: Year 2014	0.001	0.000	0.001	-0.001	0.000
	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)
Almost Eligible: Year 2015	-0.001	-0.000	0.000	-0.001	-0.000
	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)
Almost Eligible: Year 2016	-0.001	-0.001	-0.001	-0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)
Almost Eligible: Year 2017	-0.001	0.001	-0.001	-0.000	-0.001
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	88624	88624	88624	88624	88624
Mean	0.0045	0.0020	0.0009	0.0006	0.0009

Note: The dependent variables are , with distinctions in columns (2) to (4) and (5) to (7) between. The sample are younger siblings whose families meet the Sisben eligibility criteria, i.e., those in need.

Figure D.1: Number of Younger Siblings Taking Saber 3,5, 9 in 2017

(a) Saber 3



(b) Saber 5

(c) Saber 9

Note: The figure plots the distribution of younger siblings in our sample who took the *Saber 3*, *Saber 5*, and *Saber 9* exams in 2017, by the year in which their older sibling sat for the high-school exam (*Saber 11*). The x-axis represents the cohort of the older sibling (2010–2019), and the y-axis shows the corresponding number of younger siblings observed in each cohort. Panel (a) corresponds to younger siblings taking *Saber 3*, Panel (b) to those taking *Saber 5*, and Panel (c) to those taking *Saber 9*.

Table D.7: Heterogeneity Analysis – Younger Sibling: Ever Being Convited of a Crime

Differences-in-Differences Model:

	(1) All	(2) Age gap $\leq$ 4 years	(3) Age gap $>$ 4 years	(4) Same-Gender Siblings	(5) Opposite-Gender Siblings	(6) SES: Less Poor
T1 × Post	-0.002***	-0.002*	-0.002	-0.003***	-0.001	-0.002***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)
T2 × Post	-0.000	-0.001	-0.000	-0.001	0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	88624	39901	48422	46303	42051	58893
Mean	0.0019	0.0024	0.0015	0.0017	0.0021	0.0014
Effect rel. to mean (T1)	-105%	-85%	n.s.	-166%	n.s.	-107%
Effect rel. to mean (T2)	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.
	(7) SES: Poorest	(8) Stay-at-home Mom	(9) Working Mom	(10) Low-educated Mom	(11) High-educated Mom	(12) Low-educated Dad
T1 × Post	-0.002	-0.002***	-0.002	-0.002**	-0.001	-0.003***
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
T2 × Post	-0.001	0.000	-0.001	-0.000	-0.000	-0.002*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	25876	38305	43766	61999	19328	59843
Mean	0.0029	0.0017	0.0019	0.0021	0.0011	0.0020
Effect rel. to mean (T1)	n.s.	-122%	n.s.	-103%	n.s.	-145%
Effect rel. to mean (T2)	n.s.	n.s.	n.s.	n.s.	n.s.	-85%
	(13) High-educated Dad	(14) Neither parent has postsecondary education	(15) One parent has postsecondary education	(16) No High-Quality College in Mun	(17) High-Quality College in Mun in Mun	(18) Older sibling worked in Senior Year
T1 × Post	0.001	-0.003***	0.001	-0.003*	-0.002***	0.003
	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.005)
T2 × Post	0.002	-0.002	0.001	-0.001	-0.000	-0.000
	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.004)
Observations	16698	51154	19315	17081	71533	3764
Mean	0.0014	0.0021	0.0017	0.0020	0.0019	0.0029
Effect rel. to mean (T1)	n.s.	-158%	n.s.	-165%	-93%	n.s.
Effect rel. to mean (T2)	n.s.	n.s.	n.s.	n.s.	n.s.	n.s.

Notes: Each column reports separate estimates of Equation (2) for the indicated subsample. Column (1) includes all younger siblings. Columns (2) and (3) restrict the sample to families where the age gap with the older sibling is less than or equal to four years, and greater than four years, respectively. Columns (4) and (5) include same-gender and opposite-gender sibling pairs. Columns (6) and (7) split the sample by socioeconomic status (SES): “Less poor” households correspond to strata 2 or higher, and “Poorest” households to stratum 1. Columns (8) and (9) distinguish households with a stay-at-home versus working mother. Columns (10)–(13) split the sample by parental education: mother or father with low (no secondary) versus high (completed secondary or higher) education, and whether neither or at least one parent has postsecondary education. Columns (14) and (15) separate municipalities without and with at least one accredited high-quality higher-education institution. Column (16) restricts the sample to households where the older sibling was employed during the last year of high school. Standard errors are clustered at the household level. All specifications include the same set of controls and fixed effects as in the baseline model.

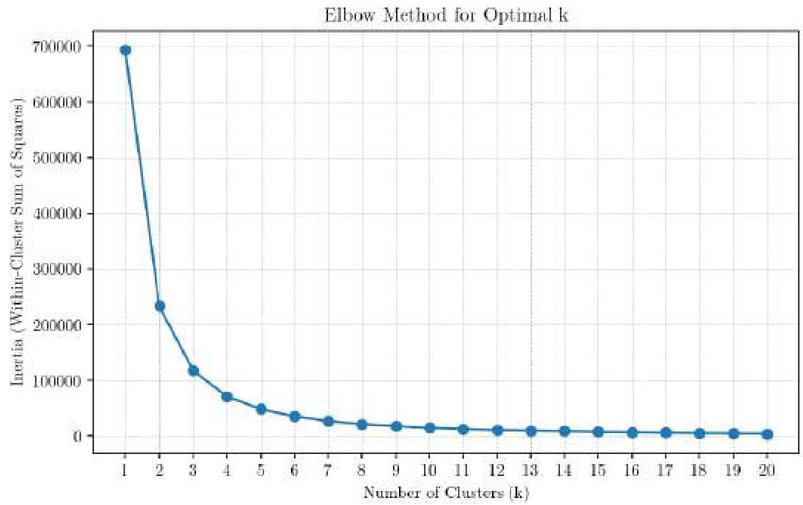


Figure D.2: Elbow Method for Determining Optimal Number of Clusters

Table D.8: **Younger Sibling: Performance in National Standardized Test**

	Extensive Margin	Intensive Margin		
	(1)	(2)	(3)	(4)
	HS Graduation	Overall	Math	Spanish
Differences-in-Differences Model:				
Panel A. Baseline				
T1 × Post	-0.003**	0.079***	0.078***	0.087***
	(0.002)	(0.012)	(0.012)	(0.012)
T2 × Post	-0.002*	0.048***	0.045***	0.048***
	(0.001)	(0.009)	(0.009)	(0.009)
Observations	276802	212764	212764	212764
Mean	0.7799	0.3979	0.3804	0.3564
Panel B. Machine-Learning–Defined Groups				
T1 × Post	-0.001	0.069***	0.078***	0.072***
	(0.001)	(0.011)	(0.011)	(0.011)
T2 × Post	0.000	0.050***	0.055***	0.042***
	(0.001)	(0.006)	(0.007)	(0.007)
Observations	510208	373570	373570	373570
Mean	0.7441	0.1794	0.1836	0.1696

Note: The table reports Difference-in-Differences estimates from Equation (2) for younger siblings' educational outcomes. Each column corresponds to a separate regression. The coefficients on $T1 \times Post$ and $T2 \times Post$ capture the change in outcomes for younger siblings of eligible and almost-eligible older siblings, respectively, relative to the control group of families whose older sibling scored well below the SPP cutoff. High-school graduation is measured as an indicator equal to one if the younger sibling takes the Saber 11 exam. *Saber 11* outcomes are standardized by year to have mean zero and standard deviation one. All regressions include school and municipality-by-cohort fixed effects and control for baseline family characteristics interacted with cohort dummies. Standard errors are clustered at the family level.

Table D.9: Sibling Spillover Effects on Postsecondary Enrollment by Institution Type, Quality and Program Duration

			Low-quality			High-quality			Program Duration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	SPP	Any	Any	Public	Private	Any	Public	Private	Two-years	Four-years
Differences-in-Differences Model:										
Panel A. Baseline										
T1 × Post	0.013***	0.030***	-0.007	-0.006	-0.001	0.037***	-0.003	0.039***	-0.010**	0.040***
	(0.003)	(0.007)	(0.005)	(0.004)	(0.003)	(0.006)	(0.005)	(0.004)	(0.005)	(0.006)
T2 × Post	-0.002	0.007	-0.002	0.001	-0.003	0.009**	0.008**	0.001	-0.004	0.011**
	(0.002)	(0.005)	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)
Observations	151741	178819	178819	178819	178819	178819	178819	178819	178819	178819
Mean	0.0262	0.3145	0.1462	0.0973	0.0489	0.1683	0.0992	0.0691	0.1199	0.1936
Effect rel. to mean (T1)	51.2%	9.4%	n.s.	n.s.	n.s.	21.9%	n.s.	57.0%	-8.6%	20.7%
Effect rel. to mean (T2)	n.s.	n.s.	n.s.	n.s.	n.s.	5.2%	7.8%	n.s.	n.s.	5.8%
Panel B. Machine-Learning–Defined Groups										
T1 × Post	0.013***	0.031***	-0.013***	-0.014***	0.002	0.044***	0.001	0.042***	-0.015***	0.046***
	(0.003)	(0.006)	(0.004)	(0.004)	(0.003)	(0.005)	(0.004)	(0.004)	(0.004)	(0.005)
T2 × Post	-0.001	0.008**	-0.003	-0.004	0.001	0.011***	0.009***	0.002	-0.005**	0.014***
	(0.001)	(0.004)	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)
Observations	297618	349338	349338	349338	349338	349338	349338	349338	349338	349338
Mean	0.0177	0.2782	0.1431	0.0964	0.0467	0.1351	0.0795	0.0556	0.1164	0.1609
Effect rel. to mean (T1)	76%	11%	-9%	-15%	n.s.	32%	n.s.	76%	-13%	29%
Effect rel. to mean (T2)	n.s.	3%	n.s.	n.s.	n.s.	9%	11%	n.s.	-5%	9%

Note: The dependent variables are defined as follows. Column (1) reports the probability of being a *Ser Pilo Paga* (SPP) scholarship recipient. Column (2) shows the enrolling in a higher education institution (HEI). Columns (3)–(5) and (6)–(8) distinguish between low- and high-quality HEIs by sector type. Columns (9) and (10) report the probability of enrolling in a two-year and four-year HEI program, respectively.

Figure D.3: Age of Children in the Family in 2021