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Upstream and Downstream Impacts of College Merit-Based Financial Aid for Low-Income Students: Ser Pilo Paga in Colombia†

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How does financial aid affect postsecondary enrollment, college choice, and student composition? We present new evidence based on a large-scale program available to high-achieving, low-income students for attending high-quality colleges in Colombia. RD estimates show financial aid eligibility raised immediate enrollment by 56.5 to 86.5 percent, depending on the complier population. This rise, driven by matriculation at private, high-quality colleges, closed the SES enrollment gap among high achievers. Moreover, a DID approach suggests enrollment of aid-ineligible students also improved because college supply expanded in response to heightened demand. With ability stratification largely replacing SES stratification, diversity increased 46 percent at private, high-quality colleges. (*JEL* I22, I23, I24, I26, J24, O15)

The high skill wage premium observed in many countries suggests college is important to financial well-being (Goldin and Katz 2008, OECD 2018). However, there is clear evidence that students from disadvantaged backgrounds are severely underrepresented in the postsecondary pipeline worldwide, particularly at selective higher education institutions, or HEIs (Chetty et al. 2017, Ferreyra et al. 2017, Hoxby and Avery 2013). This often persists despite extensive financial aid available to high-achieving, low-income students offered by governments and HEIs

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in developed countries.[1] In developing countries, credit markets are generally less well developed; financial aid and student loan programs are more recent developments. Even so, research on the efficacy of such programs is more nascent (Solis 2017). Nonetheless, it is precisely in these contexts that financial aid policies may have the largest enrollment potential.

We study the impact of transitioning from a setting with very little financial aid to one with full scholarship loans for high-achieving, low-income students. We exploit a large-scale program introduced in Colombia between 2014 and 2018 called Ser Pilo Paga (SPP). SPP has four key characteristics. First, beneficiaries are extremely high achieving, scoring in the top decile of the standardized high school exit exam (taken by all seniors, regardless of their postsecondary intentions). Beneficiaries also belong to the poorest 50 percent of households, as measured by the main proxy-means testing system for social assistance. Second, the program is highly visible and has simple, easy-to-understand eligibility rules and application procedures. Students are readily able to ascertain their own aid eligibility from their test score and household wealth index. Third, SPP is generous and large in scale. The program covers the full tuition cost of attending *any* four-year or five-year undergraduate program in *any* government-certified "high-quality" university in Colombia. SPP annually benefits roughly one-tenth of all first-year postsecondary enrollees and one-third of those entering "high-quality" universities immediately after graduating high school. Finally, program eligibility strongly predicts overall access to financial aid because students in Colombia, as in many other developing countries (Solis 2017), have little alternative sources of aid.

We exploit SPP's sharp merit and need requirements to identify the program's causal impacts on enrollment and college choice in a regression discontinuity (RD) design. We focus mainly on the first cohort, who cannot manipulate nor influence their scores around the eligibility cutoffs. SPP was announced after they took the national standardized exam (merit) and there was no time to request a reevaluation of their household wealth index (need). We find very large immediate enrollment impacts of financial aid. For need-eligible students, having a test score just above the cutoff raises immediate enrollment by 32 percentage points, or 86.5 percent. For merit-eligible students, crossing the household wealth index cutoff raises immediate enrollment by 27.4 percentage points, or 56.5 percent. These massive enrollment impacts virtually eliminated the socioeconomic status (SES) enrollment gradient among top decile test-takers.

Moreover, the program substantially altered students' college choices, shifting students from low-quality to high-quality HEIs. For instance, among need-eligible students, crossing the test score cutoff lowers enrollment at low-quality HEIs by 15.4 percentage points (57.7 percent) and raises enrollment at high-quality HEIs by

[1] See Angrist et al. (2014); Bettinger et al. (2019); Dynarski (2003); Fack and Grenet (2015); Kane (2003); Melguizo, Sanchez, and Velasco (2016); Solis (2017). In fact, the enrollment gap between high-income and low-income students has persisted despite a plethora of alternative initiatives, such as class-based affirmative action (Alon and Malamud 2014, Kahlenberg 2014), information provision (Hastings, Neilson, and Zimmerman 2015; Hoxby and Avery 2013; Hoxby and Turner 2013), intensive college counseling (Castleman and Goodman 2017), personal assistance in completing FAFSA filing (Bettinger et al. 2012), reduction in application costs (Avery et al. 2006, Pallais 2015), and making college entry exams mandatory (Goodman 2016).

46.5 percentage points (426.6 percent). This implies aid substantially improved the quality of HEIs students are exposed to (e.g., better peers, more resources, higher graduation rates). Importantly, students disproportionately chose to enroll in private over public high-quality HEIs. This student sorting into private education is not explained by differential quality accreditation status. Instead, and consistent with previous literature (Riehl, Saavedra, and Urquiola 2016), students appear to perceive private schools as more prestigious and as producing greater value added.

The large expansion of financial aid may have no impact on overall postsecondary enrollment if seats are fixed and aid recipients simply displaced other prospective students from colleges. Alternatively, aid may induce both demand and supply side effects that alter and perhaps improve outcomes for nonrecipients too (i.e., a net social gain). On the demand side, financial aid could increase the option value of applying to colleges, with students sending their applications before determining whether they are eligible for aid. The advertising push associated with SPP could have increased the perceived benefits of attending college. Peers of eligible students may also feel encouraged to attend college. On the supply side, colleges (especially private, high-quality HEIs) may enlarge their cohorts in response to any rightward shift in demand. In addition, low-quality HEIs might fill the empty seats left by financial aid recipients with the next best applicants. Such demand-driven and/or supply-driven changes may affect the enrollment of aid-ineligible students and impact the postsecondary education system as a whole.

In the second part of the paper, we focus on these broader effects of financial aid on students who are eligible and ineligible for aid. We exploit rich administrative data linking high school test-takers to all postsecondary attendees—as well as college admission records—in the years before and after the program was introduced. Using a difference-in-differences approach that compares cohorts more or less exposed to financial aid expansion across time, we find evidence consistent with both demand-side and supply-side mechanisms being at play. The demand for private, high-quality undergraduate education significantly expanded following the expansion of financial aid, and these institutions responded by admitting and enrolling more students. Moreover, as aid recipients sorted out of low-quality HEIs, the empty seats left were filled by low-income, lower-performing applicants (test score deciles 9 and under). This suggests financial aid raised college attendance among both students who were eligible and ineligible for aid, thus producing not only equity gains but also net social gains.

As financial aid pushed high-achieving, low-income students into private, high-quality HEIs, the student body composition at these colleges changed dramatically. Specifically, both student quality and socioeconomic diversity significantly increased at private, high-quality HEIs, with the share of low-income entering students increasing by a staggering 46 percent at these institutions.[2] That is, by relaxing their credit constraints, financial aid enabled low-income high-achievers to access expensive HEIs historically reserved for those who could afford them. In

[2] Using survey experiments and administrative data from a private, high-quality university, Londoño-Vélez (2020) analyzes how the socioeconomic diversity brought about by SPP in private, high-quality institutions affected high-income students' perception of inequality and preferences for redistribution.

contrast, average student quality dropped at low-quality HEIs. Ability stratification thus largely replaced SES stratification in postsecondary schooling as a result of financial aid.

We end with two suggestive findings on the effect of financial aid on the behavior of low-quality HEIs and younger high school students. The exodus of high-ability aid beneficiaries from low-quality HEIs put pressure on them to become more efficient and obtain High Quality Accreditation to attract high-achieving students. Indeed, the number of institutional requests for High Quality Accreditation increased discontinuously after SPP was announced. However, the number of HEIs awarded this accreditation has only gradually increased over the last three years, and whether or not this reflects an actual quality improvement remains to be seen. Finally, relative pre-collegiate achievement improved among very low-income high school students following the expansion of aid. Comparing relative test performance among 2.7 million test-takers between 2012 and 2016 by socioeconomic background, very low-income students are 32 percent more likely to score in the top decile and 175 percent more likely to score in the top percentile two generations after policy rollout. These findings complement recent work by Laajaj, Moya, and Sánchez (2018), whose RD design causally shows that 2015 test-takers whose household wealth index renders them barely eligible for SPP performed better than those just barely ineligible by the wealth cutoff.[3] We interpret these findings as evidence against the notion that the overall pool of high-achieving, low-income students is inelastic (Angrist and Lavy 2009; Angrist et al. 2002; Barrera-Osorio and Filmer 2016; Hoxby and Avery 2013; Kremer, Miguel, and Thornton 2009; Pallais 2009). Given that test scores are positively correlated with college degree completion and labor market outcomes in Colombia—even after controlling for baseline individual and college characteristics (MacLeod et al. 2017)—our results suggest SPP may bring future gains, promoting social mobility and reducing intergenerational inequality.

Our work contributes to the burgeoning literature that evaluates the impact of need-based and merit-based financial aid on college attendance in developed and developing countries.[4] Our work is most closely related to Bettinger et al. (2019), Cohodes and Goodman (2014), Scott-Clayton and Zafar (2016), and Solis (2017), but builds on their work in three key aspects. First, we shed new light on the full effect of aid for low-income high-achievers, expanding the analysis to the impact of financial aid on both eligible and ineligible students. Indeed, a large expansion of financial aid may induce demand and supply responses that indirectly alter outcomes for nonrecipients too. However, this has been difficult to evaluate in the United States, as financial aid has been gradually phased in (Dynarski and Scott-Clayton 2013). Instead, we document the effects of drastically expanding financial

[3] Our descriptive result, from comparing performance by socioeconomic strata among *all* test-takers three years before and two years after SPP rollout, is consistent with the well-identified LATE estimate from Laajaj, Moya, and Sánchez (2018). While our approach is less well identified, we qualitatively reproduce their main findings while comparing the performance of low-income and high-income students across time and two years after SPP was rolled out. We find that low-income students crowded out high-income students from the top of the test score distribution. The magnitude of these results increases over time, as students, parents, and teachers have more time to reoptimize in response to the policy change.

[4] See Angrist et al. (2014, 2016); Bettinger et al. (2019); Castleman and Long (2013); Dynarski (2003); Fack and Grenet (2015); Marx and Turner (2015); Melguizo, Sanchez, and Velasco (2016).

aid available for high-achieving, low-income students. In our setting with ex ante little existing financial aid, we find that such a policy has extensive impacts on the population of eligible and ineligible students.

Second, our potential eligible population ranks higher on the relative ability distribution, since the financial aid programs in previous studies affect students with above-median test scores (Solis 2017), above-median high school GPA (Bettinger et al. 2019, Scott-Clayton and Zafar 2016), or top-quartile high school test scores in each school district (Cohodes and Goodman 2014). In contrast, financial aid recipients in Colombia score in the ninety-first percentile and may enroll at top-ranked universities. Studying college choice as an outcome is particularly relevant in our context, since one of the main reasons preventing eligible students from enrolling in these top institutions is access to financial aid.[5]

Third, we estimate the effect of targeted financial aid on the entire population of high school seniors (i.e., students that ex ante may or may not be interested in attending postsecondary education). Our population thus differs from the self-selected subsample of high school students that express interest in postsecondary attendance, for instance, by filing a student loan application form (Angrist et al. 2014, Bettinger et al. 2019) or taking a college admission test (Solis 2017). Because financial aid may induce high schoolers to be interested in postsecondary education, estimating the enrollment effects of aid for the entire population of high school students—including students who would be inframarginal in other studies—is of direct interest to policymakers concerned with questions of access.

Lastly, our work is related to the literature on school vouchers in secondary schooling. In particular, it contributes to research analyzing the effect voucher programs have on participants, student sorting across schools, as well as the impact that nonrandom migration of students from public to private schools may have on aggregate educational performance (Epple, Romano, and Urquiola 2017; Urquiola 2016). We contribute to this literature by offering a case of a voucher for postsecondary education that increases access to quality schooling for high-achieving students at the lower end of the socioeconomic ladder. As such models predict, we find that school choice generates equity gains and reduces the stratified education provision wherein quality rises with socioeconomic status. But as high-achieving students sort out of a no-college education and low-quality schools and enter private elite colleges, stratification by ability increases. Finally, we observe that financial aid restricting college choice to high-quality schools pressures low-quality schools to become more efficient at attracting high-ability students.

While the magnitude of our enrollment results is similar to what previous studies have found in Latin America (Solis 2017), the response to financial aid observed in Colombia is much stronger than that documented in more developed countries. There are four main factors driving this difference. First, baseline levels of postsecondary attainment—even among high-achievers—are lower in Colombia relative to OECD countries (OECD and World Bank 2012) and the SES enrollment gradient is

[5] Further, the fact that pre-collegiate achievement overlaps between financial aid recipients and nonrecipients at high-quality colleges partly explains why we do not find evidence of mismatch (Dillon and Smith 2017; Goodman, Hurwitz, and Smith 2017; Hoxby and Avery 2013), as detailed in online Appendix C.

relatively steep.[6] Second, the program is visible, transparent, and simple; three key determinants of financial aid take-up and postsecondary enrollment (Bettinger et al. 2012; Dynarski and Scott-Clayton 2013; Hoxby and Turner 2013). Third, in addition to their high ability, eligible students are able to access top-ranked universities because admissions are mostly based on standardized test scores and rarely require qualitative inputs like letters of recommendation or essays. Lastly, and most importantly, Colombian students are ex ante severely constrained in their ability to finance their collegiate studies through credit markets. Few private HEIs offer resources to low-income high achievers, and only 11 percent of first-year undergraduate students had access to student loans before SPP. With binding credit constraints, low-income students were often squeezed out of collegiate opportunities. Together, these four factors account for the large enrollment impacts we document relative to previous studies of financial aid.

We thus interpret our findings as the result of relaxing credit constraints in a context of wide SES enrollment gaps with costly top-ranked private institutions (relative to per capita income) and a shortage of access to credit for low-income individuals. Our results suggest that, in the long run, SPP is likely to have important impacts on the education and labor markets in Colombia far beyond the ones analyzed here. Studying these impacts will undoubtedly come in time as the required data becomes available.

The remainder of this paper is organized as follows. Section I provides some institutional background. Section II describes the data. Section III presents the impacts of financial aid on immediate postsecondary enrollment and college choice. Section IV describes the upstream and downstream effects of financial aid on overall enrollment, student body composition, and pre-collegiate test performance. Section V concludes.

I. Background

A. *Higher Education in Colombia*

The postsecondary admissions process in Colombia starts with SABER 11, the national standardized high school exit exam. SABER 11 is generally analogous to the SAT in the United States, but differs in two important ways. First, SABER 11 is taken by more than 90 percent of high school seniors regardless of whether they intend on applying to a HEI. High school seniors take the exam in either the spring or fall semester according to their graduation date, as there are two graduating cohorts per year. Most private high school seniors take the exam in the spring semester, while most public high school students take SABER 11 in the fall semester. In all, 15 percent and 85 percent of students take SABER 11 in the spring and fall semesters, respectively.

[6] Comparisons by SEDLAC (CEDLAS and The World Bank) using household survey data show that the ratio in years of education between the top and bottom income quintiles was higher in Colombia than in any other Latin American country in 2015.

Second, SABER 11 plays a larger role in admissions in Colombia than the SAT does in the United States. College applications and admissions are decentralized. While colleges decide and apply their own admission criteria and processes, nearly four-fifths of them use SABER 11 as an admission criterion (OECD and World Bank 2012). In fact, many schools award admission offers solely based on performance in this exam. There is no common date by which all applications must be submitted, or by which colleges send acceptance letters. Students may make multiple applications and prospective students apply to a college-major pair. Because there are two graduating cohorts, colleges admit new students every semester and have separate admission processes and distinct SABER 11 cutoffs for each cohort.

All HEIs are required to obtain the Ministry of Education's Qualified Registry of minimum quality standards to provide their education services. Institutions can also voluntarily apply for a certificate of High Quality Accreditation, awarded by the National Accreditation Council (CNA, a Spanish-derived acronym). The CNA, composed of members representing the academic and scientific community, sets the quality criteria and carries out the peer review evaluation process. This process is designed to encourage continuous self-evaluation, self-regulation, and institutional/program improvement (OECD 2016).[7] Importantly, High Quality Accreditation proxies quality of education provision, as measured by the college exit tests, and graduates' wage profiles (Camacho, Messina, and Uribe 2016). One in ten HEIs had received High Quality Accreditation by October 1, 2014, the day SPP was announced.[8] We henceforth refer to these institutions as "high-quality" HEIs. Among these 33 high-quality HEIs, 12 were public and 21 were private. In terms of the proportion of students, roughly one-third of students enrolled in higher education attend a high-quality HEI.

Private universities in Colombia are expensive even by international standards (OECD and World Bank 2012). Very few private HEIs offer resources to high-achieving, low-income students. Private HEIs are very expensive relative to public HEIs; their tuition fees are more than eightfold those of public HEIs, with the latter being able to charge low and means-tested tuition fees thanks to heavy government subsidization. The low fee of attending public, high-quality HEIs for low-income students helps explain why these HEIs have historically been oversubscribed, making their admission processes highly competitive.

Despite progress in the past decades, educational credit markets and financial aid mechanisms remain substantially less developed in Colombia than in the United States and other developed countries. Before SPP, only 11 percent of first-year undergraduate students had a loan from ICETEX, the public institution providing student loans in Colombia (Ferreyra et al. 2017). In contrast, 70 percent of American undergraduate students receive some form of financial aid, and 30 percent borrowed federal loans in 2017 (CollegeBoard 2017). High direct college costs, coupled with

[7] The High Quality Accreditation can be program-specific (e.g., the economics undergraduate program at University of Los Andes) and/or institutional (e.g., University of Los Andes); the latter encompassing the former. Institutional accreditation is closely related to the official university ranking made by Colombia's Ministry of Education: 19 of the top-ranked 20 HEIs have High Quality Accreditation.

[8] This number would increase from 33 to nearly 50 two years after SPP was announced (see Section IVC).

limited mechanisms for financing the full cost of college, squeeze many low-income Colombian students out of collegiate opportunities.[9]

As access to private, high-quality HEIs largely depends on students' own financial resources, sorting into private colleges in Colombia is strongly defined by the tuition rates they charge (Riehl, Saavedra, and Urquiola 2016). Conditional on enrolling, low-income students sort into low-quality (public and private) HEIs, and only a minority of extremely high-performing students access the highly competitive high-quality public HEIs. In contrast, high-income students are most likely to attend private HEIs—mostly high-quality, but sometimes also low-quality HEIs—regardless of their ability (Ferreyra et al. 2017). As students sort across HEI types, postsecondary education in Colombia becomes de facto severely segregated.

B. Ser Pilo Paga Financial Aid Program

In light of this situation, the Santos administration created Ser Pilo Paga (roughly, "hard work pays off" in Spanish), a merit-based financial aid program for low-income students. Announced October 1, 2014 and extended for a quadrennium, SPP is a publicly funded program that covers the full tuition cost of attending a four- or five-year, degree-awarding undergraduate program at any university with High Quality Accreditation in Colombia. The loans are forgivable upon graduation and can be used to study any major. The academic cost of each SPP beneficiary is transferred by the central government to the university he or she attends. In addition, SPP recipients directly receive a biannual stipend of one to four times the minimum wage, depending on whether they migrated to a different metropolitan area to attend college.[10] Between 2014 and 2018, SPP benefited some 40,000 students, or roughly 10,000 individuals per year. Its large scale and an immense government advertisement campaign contributed to making SPP one of the most popular social programs in the country.

To become eligible, applicants must satisfy three conditions. First, they must score above a cutoff in SABER 11 in the fall term of the year they graduate high school. For the first cohort of SPP recipients, this meant scoring at least 310 out of 500 (i.e., among the top 9 percent of test scores in 2014) (Figure 1, panel A). Due to rising program take-up and binding budget constraints, the government subsequently increased this test score cutoff to 318 in 2015 (i.e., top 8 percent), 342 in 2016 (i.e., top 4 percent), and 348 in 2017 (i.e., top 3 percent). Second, students

[9] For this reason, the Gini coefficient of access to postsecondary education is higher in Colombia than in many other Latin American countries, including Argentina and Chile (Ferreyra et al. 2017). Further, while gross enrollment rates have increased from 31.6 percent in 2007 to 49.4 percent in 2015, most of this increase has been due to the expansion of low-quality programs in low-quality HEIs, with questionable impacts on academic performance and future job prospects (Camacho, Messina, and Uribe 2016).

[10] As in the rest of Latin America and the Caribbean, students tend to attend HEIs close to home. Almost 75 percent of students in Colombia attend a HEI located in their same department (or state) of residence at the time of finishing high school (Ferreyra et al. 2017). Students also tend to live at home while enrolled in postsecondary education. In addition to the SPP stipend, SPP recipients may receive a biannual subsidy of COP$800,000 pesos (US$(2015)1 = COP$3,174) awarded by the National Planning Department upon completion of the academic semester, and an additional COP$200,000 per semester if their college GPA is 3.5/5.0 or above. Furthermore, SPP recipients often benefited from additional in-kind subsidies offered by receiving institutions (e.g., free photocopies, reduced lunch fees).

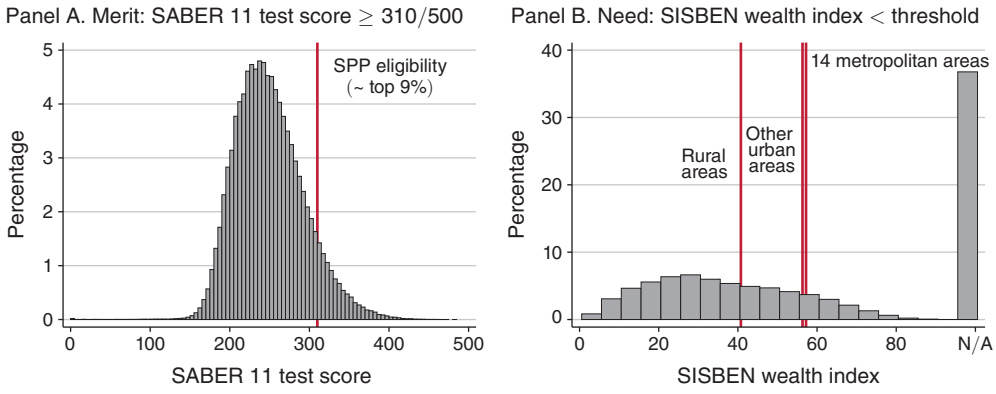

FIGURE 1. SPP ELIGIBILITY CONDITIONS

Notes: To be eligible for financial aid SPP, students must score above a cutoff in the national standardized high school exit exam, SABER 11. Their household wealth index, SISBEN, must also be below a cutoff. These figures show the distribution of SABER 11 test scores (panel A) and SISBEN poverty index (panel B) for fall 2014 test-takers. The red vertical lines represent the SPP eligibility cutoffs. The figures suggest both variables are distributed smoothly around the eligibility cutoffs. Panel A plots the distribution of test-takers in fall 2014 by SISBEN eligibility status. In panel B, the SISBEN eligibility cutoff varies by applicant geographic location. Test-takers not included in SISBEN (e.g., individuals that do not receive welfare) do not have a SISBEN score and appear in panel B as "N/A."

Source: Authors' calculations based on ICFES, DNP, and MEN.

must come from a disadvantaged household, as measured by the main proxy-means testing instrument used by the government to target social welfare program recipients, SISBEN. The student's SISBEN wealth index must be below a cutoff that varies with geographic location: 57.21 in the 14 main metropolitan areas; 56.32 in other urban areas; and 40.75 in rural areas (Figure 1, panel B).[11] Third, applicants must have been admitted by a university with High Quality Accreditation.

Three program characteristics are worth noting. First, SPP was introduced by surprise almost two months *after* some 575,000 individuals had taken SABER 11 and only a couple of weeks before most colleges' application deadlines. Students could not manipulate their test scores to become eligible for SPP, lending credence to our identifying assumption of test scores being quasi-randomly assigned close to the SPP eligibility cutoff (validated in Section IIIA).[12] This surprise introduction partly explains why program take-up among eligible students was incomplete (59.4 percent) and why the first cohort of beneficiaries was less prepared for college relative to later cohorts (DNP, CNC, and UniAndes 2016). Second, given the requirement that test-takers score above a cutoff in the fall term of the year they graduate

[11] SISBEN uses data from a proxy-means survey to assign households a single and continuous score from 0 to 100 (poorest to richest) based on housing quality, possession of durables, public utility services, and human capital endowments, among others. SPP's SISBEN cutoffs coincide with eligibility cutoffs of other social programs, such as the conditional cash transfer program "Familias en Acción" and humanitarian aid for victims of Colombia's armed conflict. Online Appendix Figure A.1 plots the distribution of test-takers in fall 2014 by SISBEN eligibility status. The figure shows that while the subsample of test-takers coming from disadvantaged households have a lower performance relative to the overall population of test-takers, a significant fraction of them score above the SABER 11 eligibility cutoff.

[12] While in theory students could ask for a SISBEN reclassification, in practice the short application deadline explains why a reclassification was made for only one beneficiary in the first cohort of SPP.

high school, ineligible students could not reapply for SPP by retaking the SABER 11 exam in subsequent years. Third, the program was remarkably large in scale. A year prior to policy rollout, 102,000 test-takers enrolled in a HEI immediately after taking SABER 11 in fall 2013. Of these, roughly 30,000 enrolled in a high-quality HEI (of which 16,600 did so in a private HEI and 13,300 in a public HEI). Thus, by providing scholarship loans to roughly 10,000 students, the program aimed to benefit one in every three students attending a high-quality HEI in Colombia.

Finally, while SPP loans are forgivable upon graduation, students who drop out must repay the loan. A student is considered to have dropped out if he or she does not attend a high-quality HEI for three or more consecutive semesters. Data from ICETEX—the institution in charge of repayment schemes for SPP—indicate that, as of June 2018, 743 beneficiaries (1.9 percent of all beneficiaries) from the first three cohorts had dropped out from the program. This is less than one-tenth the average dropout rate among comparable college students (see online Appendix C). The dropout rate is the highest among the first cohort (467 of 8,971 recipients enrolling in spring of 2015, or 5.2 percent), partly because they began postsecondary schooling a longer time ago and partly because this cohort was the least prepared for college. On average, monthly loan repayments were around US$80, for a total of US$2 million (COP$5.9 billion) owed, or US$2,730 per dropout; 75 percent of this total amount was owed by the first cohort of SPP beneficiaries. By June 2018, 80 of the 743 dropouts had already paid back the loan, and the rest were in the process of loan repayment.

II. Data

We use administrative data from six main sources as well as survey data specially collected for the impact evaluation of SPP. First, we use data from the Instituto Colombiano para el Fomento de la Educación Superior (ICFES), the institution in charge of standardized testing in Colombia. They contain test scores and sociodemographic information (e.g., socioeconomic stratum, parental education, municipality of residence) for all SABER 11 test-takers in the spring and fall semesters of 2011 through 2016.[13] ICFES data are then merged with data from the Department of National Planning (DNP), which contain SISBEN scores. Together, these two sources allow the identification of the eligible population (i.e., students with a test score above the cutoff and wealth index below the geographic thresholds).

Third, we use the Ministry of Education's Sistema para la Prevención de la Deserción en la Educación Superior (SPADIES), which tracks students along the postsecondary education system. We use SPADIES data from fall of 2011 to spring of 2016, which provide a wealth of individual-by-semester level information on student observable characteristics, including enrollment status, HEI, major of study, share of courses passed, and graduation or dropout status. These data cover roughly

[13] *Estratos* ("strata") from the Colombian socioeconomic stratification system classify housing according to its physical characteristics and environment. Dwellings are classified into one of six strata, with strata 1 being the poorest and strata 6 being the wealthiest.

90 percent of all postsecondary enrollees; information from a handful of institutions is omitted due to poor or inconsistent reporting.

Fourth, we use data from ICETEX, the institution that manages national and international scholarships and grants for postbaccalaureate programs—including SPP—on behalf of public and private organizations. These data allow us to identify SPP beneficiaries and also, in case of dropout from SPP, to observe their loan repayment behavior.

The above sources of data represent census information for the population of interest—SABER 11 test-takers, SISBEN-eligible households, postsecondary students, and SPP beneficiaries, respectively. Our main RD analysis focuses on fall 2014 test-takers—the first cohort of SPP recipients—for reasons detailed below. We thus use information from almost 575,000 students, out of which 9,166 are SPP beneficiaries (online Appendix D extends the analysis to include the second cohort of SPP). Moreover, to understand the drivers of institutional choice, we use survey data collected from a representative sample of 1,479 low-income, high-achieving high school seniors who took SABER 11 in fall of 2015. The data were collected considering a RD design using the second cohort of SPP recipients, surveying SISBEN-eligible individuals who scored above and below the SABER 11 eligibility threshold.

The analysis related to the upstream and downstream consequences of SPP uses the aforementioned census information of all high school exit test-takers and postsecondary enrollees in the years 2011–2016. This comprises almost 4 million individuals. Moreover, to explore the impact of SPP on college application and admissions processes, we use administrative data on applications and admissions directly collected by us from a sample of High Quality Accredited universities in Colombia.

Fifth, we use earnings records for formal sector workers during 2008–2013 from the Ministry of Education's Observatorio Laboral para la Educación (OLE). OLE uses data from the Ministry of Social Protections PILA database on contributions to pension and health insurance funds, and includes data on monthly wages, employment status, and four-digit economic activity codes for nearly all college graduates between 2001–2013 in Colombia. Lastly, we use information from balance sheets and financial accounts provided by HEIs to the Ministry of Education. Those data include annual information regarding, for instance, spending per student and research spending per faculty member. We also use data on requests made by HEIs to receive High Quality Accreditation from the Ministry of Education, as well as accreditations awarded by the Ministry of Education across time.

III. Direct Impacts of Financial Aid

A. *RD Design and Validity*

To estimate the causal impact of SPP on postsecondary enrollment, we exploit the SABER 11 and SISBEN cutoffs using a RD design. Let $Z_i = \mathbf{1}(R_i > k)$ be an indicator for SPP eligibility, where k is the point of a discontinuous assignment rule (e.g., SABER 11 score, SISBEN). Note that being SISBEN-eligible and

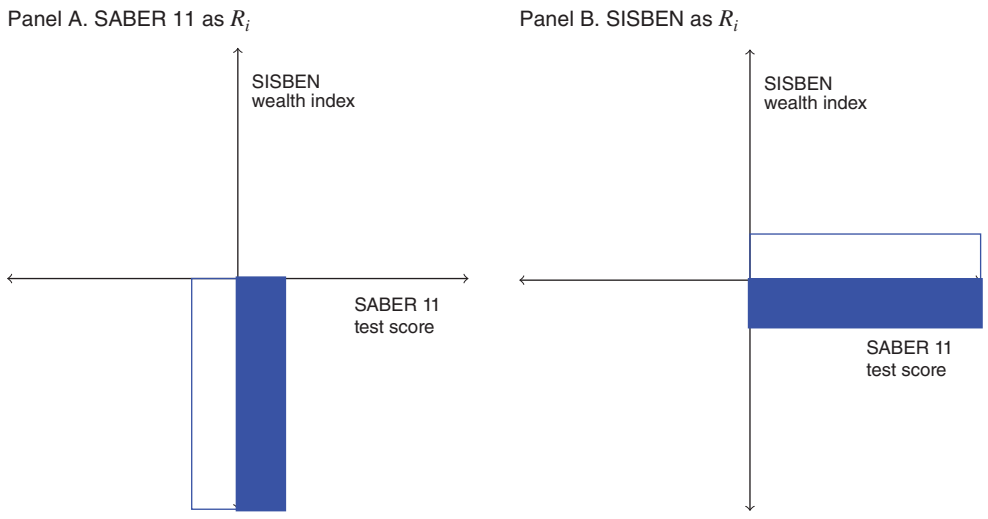

FIGURE 2. ILLUSTRATION OF THE TWO TYPES OF COMPLIERS

Notes: This figure compares the two types of compliers of need-based and merit-based financial aid program SPP. Panel A uses the SABER 11 test score as the running variable and compares students around the test cutoff who are SISBEN-eligible. Panel B uses the SISBEN wealth index as the running variable and compares students around the wealth cutoff who are SABER 11-eligible.

SABER 11-eligible are necessary but not sufficient conditions to receive SPP financial aid. The third eligibility requirement—admission at a high-quality HEI, which we do not directly observe—requires that the student apply and be granted admission by such an institution. Students must provide the government proof that all three conditions have been satisfied to receive SPP.

Denote D_i as an indicator for whether an individual is a beneficiary of SPP. Since receiving SPP depends on both the SISBEN wealth index and the SABER 11 test score, this multidimensional RD setting can separately identify two types of compliers: (i) need-eligible students around the test score cutoff, and (ii) merit-eligible students around the need cutoff (see Figure 2 for an illustration). There are many strategies for dealing with multidimensional regression discontinuities, as discussed by Wong, Steiner, and Cook (2013). In the economics of education, recent examples include Cohodes and Goodman (2014) and Bettinger et al. (2019). We follow previous studies and report estimates separately, collapsing the discontinuity into a single dimension for each student by defining the distance of SABER 11 (SISBEN) scores from the eligibility cutoff, given SISBEN (SABER 11) eligibility status.[14] We argue that the resulting "frontier-specific" effects from this univariate approach are the preferred causal estimand over the frontier average treatment effect (i.e., a weighted average of the two univariate RD effects). This is because the running variables

[14] We use data-driven (that is, fully automatic) local-polynomial-based robust inference procedures through "rdrobust." This command implements the bias-corrected inference procedure proposed by Cattaneo, Calonico, and Titiunik (2014), which is robust to "large" bandwidth choices. It also offers robust bias-corrected confidence intervals for average treatment effects at the cutoff (Calonico, Cattaneo, and Titiunik 2014; Imbens and Kalyanaraman 2012).

are neither in the same metric nor in the same content area. More importantly, the discontinuities represent different populations, and the heterogeneity in estimated impacts across these frontiers is informative.

We focus our analysis on the first cohort of SPP; that is, students who graduated from high school in late 2014 and began college early 2015. The first cohort guarantees the highest internal validity: students were informed about the financial aid program *after* they had taken the SABER 11 exam. This eliminates concerns regarding nonrandom sorting across the eligibility cutoff. In contrast, later cohorts may react to the program by, for instance, exerting more effort in the high school exit examination or requesting an evaluation from local authorities to be included in SISBEN. Indeed, anecdotal evidence suggests there has been a significant rise in the number of households requesting SISBEN evaluations since 2015. We further avoid pooling different SPP cohorts because public knowledge of the program has increased over time. As a result, the program's take-up rate—the share of eligible students who become SPP beneficiaries—is higher in the most recent cohorts (see DNP, CNC, and UniAndes 2016). This raises concern about endogeneity due to time-varying unobservables and complicates pooling different cohorts.

The three key assumptions for the validity of the RD design are the following: (i) there is no evidence of manipulation in assignment to treatment near the discontinuity; (ii) any observed differences in the neighborhood of the discontinuity occur only as a result of the differences in the running variables; and (iii) the predicted discontinuity creates a large change in assignment to treatment as a function of the running variable. We address each of these assumptions in turn.

First, an assumption often employed in RD is that there is no selective sorting across the treatment threshold. The skeptical econometrician might fear students control their test scores and/or wealth index and behave strategically so as to ensure that they are just above or below the eligibility thresholds, thereby violating this assumption. We argue against this concern in the case of SPP, as there is little scope for manipulation of these variables. As previously mentioned, SPP was announced on October 1, 2014, almost two months *after* students took SABER 11. Thus, students did not know the eligibility cutoff—nor, in fact, the mere existence of this financial aid program—at the moment they sat for the exam. Once the program and eligibility conditions were announced, students could not go back in time and retake SABER 11 to become eligible. Similar arguments can be made for the SISBEN score: household SISBEN scores were assigned well before the program was announced. Even though SISBEN reclassifications are possible, neither students nor their families had the time to ask for reassessments before the SPP application deadline in November 2014. Moreover, SABER 11 exams are centrally scored and raw scores transformed into scaled scores via an algorithm unknown to students, their families, or teachers. Finally, ICFES reformed SABER 11 in fall of 2014 such that students taking the exam that semester were not familiar with the scoring mechanism ex ante. We thus conclude that our identifying assumption that the running variables are quasi-randomly assigned close to the SPP eligibility cutoffs is a plausible one.

To formally test for manipulation of the running variable, we use the recent local polynomial density estimator proposed by Cattaneo, Jansson, and Ma (2018,

2019).[15] The resulting robust-corrected p-values are 0.218 with SABER 11 as R_i, and 0.436 with SISBEN as R_i (see Table A.1 in the online Appendix). This confirms there is no statistical evidence of systematic manipulation of the running variable.

Second, we expect the behavior of individuals to be correlated with Z_i only because of its correlation with D_i (i.e., the exclusion restriction necessary for Z_i to be a valid instrument for D_i). We test this possibility by examining whether observable covariates are different on either side of the discontinuity point. The results in Table A.2 in the online Appendix suggest there is balance in most of the 25 covariates. Specifically, using SABER 11 (SISBEN) as the running variable, there is balance in 23 (19) of the 25 baseline characteristics. In the few cases where an imbalance is detected, these differences are small in magnitude. Overall, we cannot reject the joint null hypothesis of balance in covariates around the discontinuity. As a robustness check, we perform a dimension reduction exercise where we predict core outcomes using all 25 observable characteristics, and then run the RD on the predicted outcomes. The result of this exercise, displayed in Table A.3 in the online Appendix, confirms that imbalances in baseline covariates do not explain the large impacts we document on immediate postsecondary enrollment.

Figure 3 presents the take-up rate of SPP, that is, $E\left[D_i \mid R_i\right]$ against the running variable R_i, the SABER 11 score for those eligible by SISBEN (panel A), and the SISBEN score for those eligible by SABER 11 (panel B). The figure shows the sharp eligibility rules; since no student below the cutoff received SPP, $\Pr\left(D_i = 1 \mid Z_i = 0\right) = 0$ (i.e., there are no always-takers). The eligibility cutoffs increase SPP receipt by 55.4 percentage points when using SABER 11 as the running variable and by 62.3 percentage points when using SISBEN as the running variable. Thus, although there is a discontinuity in the probability of receiving SPP financial aid, the eligibility cutoffs do not deterministically predict SPP receipt because there is incomplete take-up. This one-sided noncompliance was due in large part to the short timespan between the announcement of SPP and the high-quality university application deadline (in many cases, a couple of weeks), as suggested by qualitative field evidence from DNP, CNC, and UniAndes (2016).

Having validated our RD design, we now document how the population directly affected by this policy compares to the typical high school exit test-taker in Colombia. Table 1 characterizes the sample population as well as the subpopulations of compliers—individuals who respond to financial aid eligibility by receiving SPP—and never-takers for each running variable (Abadie 2002, Imbens and Rubin 1997). The mean baseline characteristics are presented for all 574,269 test-takers in fall of 2014 (column 1). Consistent with need-eligibility, relative to the average test-taker, SISBEN-eligible students come from larger, less educated, and poorer families (column 2). They are also less likely to attend high school full time or

[15] In addition, we perform McCrary tests on the sample of SABER 11-eligible test-takers in fall of 2013 (placebo) and fall of 2014 (non-placebo), using SISBEN as R_i, and perform a t-test on the differenced outcomes. The resulting t-statistic is 0.224, which suggests there is no manipulation of SISBEN as result of SPP. However, when we attempt a similar comparison among SISBEN-eligible students using SABER 11 as the running variable, the resulting t-statistic is -7.365, which would point to manipulation of test scores among the treated cohort. The histograms in panel A of online Appendix Figure A.2 show that this result is likely due to changes in score rounding rules in the most recent version of SABER 11—which incidentally began in fall of 2014. The histograms thus give us confidence in concluding there was no manipulation of SABER 11 in 2014.

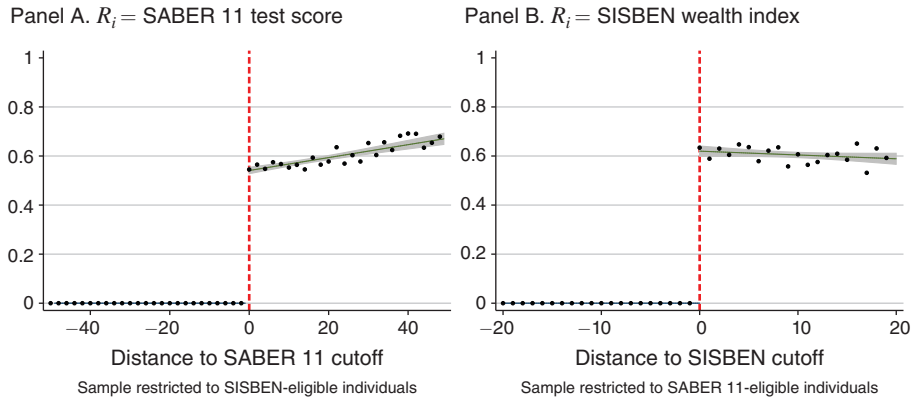

FIGURE 3. DISCONTINUITY IN THE PROBABILITY OF RECEIVING SPP FINANCIAL AID

Notes: The figures plot the take-up rate, that is, the probability of receiving SPP financial aid program as a function of the distance to the SABER 11 (panel A) and SISBEN (panel B) eligibility cutoffs, restricting the sample to SISBEN- and SABER 11-eligible students, respectively. The probability of being a SPP recipient increases from 0 percent to 55.4 percent using SABER 11 as the running variable (panel A) and to 62.3 percent using SISBEN as the running variable (panel B). Sample average within bin.

Source: Authors' calculations based on ICFES, DNP, and MEN.

TABLE 1—CHARACTERIZATION OF COMPLIERS AND NEVER TAKERS

	All (1)	SISBEN eligible (2)	Bandwidth (3)	Compliers (4)	Never takers (5)
Panel A. SABER 11 as the running variable					
Female	0.547	0.575	0.485	0.463	0.448
Age	17.914	17.968	16.684	16.388	16.925
Ethnic minority	0.132	0.139	0.089	0.075	0.081
Employed	0.136	0.136	0.095	0.053	0.109
Family size	4.688	4.871	4.617	4.620	4.485
Mother education: primary	0.369	0.449	0.263	0.211	0.274
Mother education: secondary	0.431	0.436	0.489	0.482	0.509
Mother education: T&T	0.090	0.067	0.133	0.155	0.106
Mother education: higher	0.110	0.048	0.114	0.150	0.110
Father education: primary	0.434	0.525	0.353	0.324	0.374
Father education: secondary	0.385	0.375	0.430	0.428	0.419
Father education: T&T	0.072	0.051	0.101	0.116	0.089
Father education: higher	0.110	0.049	0.118	0.133	0.120
Household SES: stratum 1	0.424	0.559	0.365	0.336	0.337
Household SES: stratum 2	0.349	0.340	0.441	0.443	0.452
Household SES: stratum 3	0.177	0.093	0.180	0.206	0.191
Household SES: stratum 4	0.035	0.007	0.014	0.016	0.017
Household SES: stratum 5	0.010	0.001	0.003	0.003	0.005
Household SES: stratum 6	0.004	0.000	0.000	0.000	0.002
School hours: full day	0.196	0.139	0.194	0.206	0.182
School hours: morning	0.513	0.548	0.606	0.607	0.621
School hours: evening	0.070	0.076	0.015	0.007	0.017
School hours: afternoon	0.156	0.164	0.173	0.173	0.168
School hours: weekends	0.064	0.074	0.013	0.008	0.013
Private school	0.254	0.156	0.165	0.170	0.173
Observations	574,269	299,475			

(*continued*)

Table 1—Characterization of Compliers and Never Takers (*continued*)

	All (1)	SABER 11 eligible (6)	Bandwidth (7)	Compliers (8)	Never takers (9)
Panel B. SISBEN as the running variable					
Female	0.547	0.448	0.434	0.459	0.432
Age	17.914	16.751	16.579	16.282	16.981
Ethnic minority	0.132	0.060	0.061	0.064	0.047
Employed	0.136	0.082	0.075	0.044	0.119
Family size	4.688	4.238	4.319	4.266	4.156
Mother education: primary	0.369	0.106	0.157	0.111	0.184
Mother education: secondary	0.431	0.336	0.445	0.414	0.398
Mother education: T&T	0.090	0.172	0.177	0.202	0.160
Mother education: higher	0.110	0.386	0.220	0.274	0.259
Father education: primary	0.434	0.148	0.218	0.203	0.197
Father education: secondary	0.385	0.321	0.414	0.381	0.398
Father education: T&T	0.072	0.140	0.151	0.186	0.143
Father education: higher	0.110	0.391	0.216	0.231	0.258
Household SES: stratum 1	0.424	0.122	0.150	0.103	0.129
Household SES: stratum 2	0.349	0.306	0.503	0.522	0.503
Household SES: stratum 3	0.177	0.349	0.306	0.344	0.330
Household SES: stratum 4	0.035	0.148	0.025	0.029	0.038
Household SES: stratum 5	0.010	0.052	0.005	0.005	−0.001
Household SES: stratum 6	0.004	0.023	0.000	0.000	0.001
School hours: full day	0.196	0.454	0.318	0.326	0.323
School hours: morning	0.513	0.428	0.510	0.482	0.526
School hours: evening	0.070	0.006	0.007	0.006	0.010
School hours: afternoon	0.156	0.108	0.159	0.177	0.135
School hours: weekends	0.064	0.004	0.005	0.001	−0.002
Private school	0.254	0.525	0.330	0.383	0.360
Observations	574,269	53,632			

Notes: This table characterizes compliers from a regression discontinuity design. Column 1 presents mean baseline covariates from the universe of fall 2014 SABER 11 test-takers. Columns 2–5 compare characteristics using SABER 11 test score as the running variable, while columns 6–9 do so using SISBEN poverty index as the running variable. Bias-corrected RD results estimated with package rdrobust (Calonico, Cattaneo, and Titiunik 2014). The table does not present statistics for always takers because there was perfect left-hand side compliance; that is, there are no always takers.

Source: Authors' calculations based on ICFES, DNP, MEN, and SPADIES.

attend private high schools than the average test-taker. In contrast, SISBEN-eligible students scoring close to the SABER 11 cutoff (i.e., high performers) are more likely to be younger, nonethnic minority males from smaller, more educated, and wealthier families, and are more likely to attend a private high school full time (column 3). Compliers are very similar in observable characteristics to other students within the bandwidth, but have somewhat more educated parents (column 4). Relative to compliers, never-takers are more likely to be male and twice as likely to be employed at the time of taking SABER 11, which is presumably why they did not take up financial aid (column 5).

A comparison of columns 4 and 8 in Table 1 illustrates the differences in observable baseline covariates between the two types of compliers. Compliers using SABER 11 as the running variable are significantly poorer and have less educated parents than compliers using SISBEN as the running variable. They are also more likely to attend a public high school part time. These stark observable differences underline why studying effects separately for the two populations of compliers is

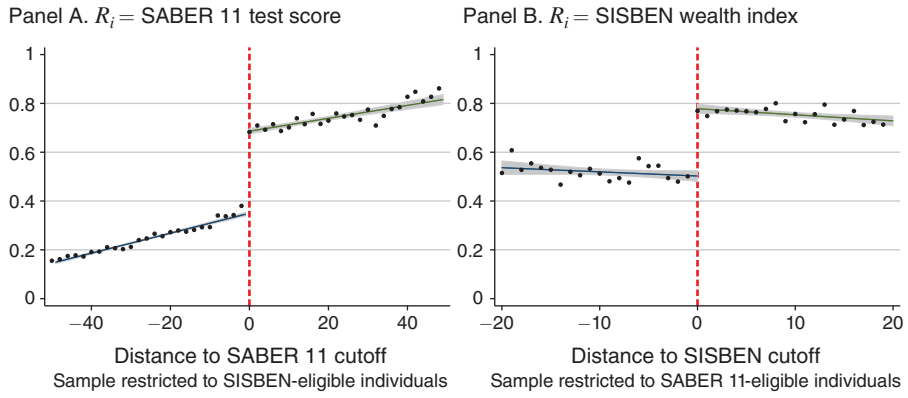

FIGURE 4. IMMEDIATE POSTSECONDARY ENROLLMENT

Notes: The figures plot the probability of immediate enrollment in any postsecondary institution as a function of the distance to SABER 11 (panel A) and SISBEN (panel B) SPP eligibility cutoffs. The likelihood of immediately accessing any postsecondary institution increases by 32 percentage points (86.5 percent) using SABER 11 as the running variable (panel A) and by 27.4 percentage points (56.5 percent) using SISBEN as the running variable (panel B). See reduced-form estimates in Table 2.

Source: Authors' calculations based on ICFES, DNP, and MEN.

important. Due to their more disadvantaged background, credit constraints are likely more severely binding for compliers using SABER 11 as the running variable. We thus expect any impact on postsecondary enrollment to be larger in magnitude when using SABER 11 rather than SISBEN as the running variable.

B. *RD Results: Immediate Enrollment, School Quality, and Choice*

Having validated our RD design, we now estimate the effect of financial aid on immediate postsecondary enrollment. Figure 4 plots the probability of immediate enrollment in any HEI using SABER 11 (panel A) and SISBEN (panel B) as the running variables. The dots are cell means, and the lines are fitted values from a regression of immediate postsecondary enrollment on the running variable estimated separately on either side of the eligibility cutoff. The figures suggest there is a discontinuous jump in the likelihood of immediate postsecondary enrollment at the eligibility cutoffs. Financial aid encouraged postsecondary attendance by subsidizing students who would have not otherwise gone to college immediately after high school.

These reduced-form estimates are large and precisely estimated, as suggested by column 1 in Table 2. Using SABER 11 as the running variable (panel A), financial aid eligibility raises enrollment by 32 percentage points; on a base of 37, this implies an 86.5 percent increase in immediate enrollment.[16] As expected, financial aid has a smaller impact at the household wealth cutoff for sufficiently high performers than at

[16] Online Appendix Table A.4 presents equivalent results, including all 25 baseline characteristics, and compares estimates with and without these controls. The inclusion of baseline covariates does not significantly affect the magnitude nor the significance of the RD results.

Table 2—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. SABER 11 as the running variable							
RF	0.32	0.465	0.466	0	−0.154	−0.063	-0.087
	(0.012)	(0.012)	(0.011)	(0.007)	(0.011)	(0.007)	(0.009)
Mean control	0.37	0.109	0.033	0.075	0.267	0.105	0.159
Observations	299,475	299,475	299,475	299,475	299,475	299,475	299,475
BW loc. poly.	29.679	24.804	23.861	32.065	23.562	30.554	25.059
Effect obs. control	31,170	23,600	22,473	36,290	22,473	33,042	25,256
Effect obs. treat	11,711	10,641	10,442	12,264	10,442	11,953	10,927
Panel B. SISBEN as the running variable							
RF	0.274	0.396	0.477	−0.079	−0.12	−0.052	−0.076
	(0.027)	(0.024)	(0.02)	(0.018)	(0.022)	(0.015)	(0.016)
Mean control	0.485	0.261	0.067	0.194	0.225	0.097	0.134
Observations	23,132	23,132	23,132	23,132	23,132	23,132	23,132
BW loc. poly.	9.028	10.954	12.224	11.604	8.05	9.176	10.329
Effect obs. control	3,868	4,604	5,043	4,854	3,488	3,964	4,392
Effect obs. treat	3,902	4,703	5,221	4,967	3,450	3,976	4,454

Notes: This table presents the effect of financial aid eligibility on immediate postsecondary enrollment using a regression discontinuity design. The dependent variable is immediate enrollment by type of postsecondary institution (e.g., high-quality, low-quality, private, public). Panel A uses SABER 11 test score as the running variable, restricting the sample to SISBEN-eligible students. Panel B uses SISBEN wealth index as the running variable, restricting the sample to SABER 11-eligible students. The reduced-form coefficient in column 1 of panel A suggests that, for individuals below a certain level of poverty, financial aid eligibility raises immediate postsecondary enrollment by 32.0 percentage points. On a basis of 37.0 percent, this implies an 86.5 percent increase in immediate enrollment. Bias-corrected RD results estimated with package rdrobust (Calonico, Cattaneo, and Titiunik 2014). Robust standard errors in parentheses. These regressions exclude all baseline covariates. Online Appendix Table A.4 reproduces these reduced-form estimates and compares how the exclusion or inclusion of baseline covariates affects the reduced-form coefficient.

Source: Authors' calculations based on ICFES, DNP, MEN, and SPADIES.

the test score cutoff for sufficiently poor students; the corresponding reduced-form estimate is 27.4 percentage points (56.5 percent). As explained above, SISBEN-eligible individuals around the test score cutoff are significantly poorer and ex ante less likely to attend college than SABER 11-eligible individuals around the household wealth index: the control means are 37 percent versus 48.5 percent, respectively. As a result, financial aid has the largest enrollment impacts among low-income students whose test scores render them barely eligible for financial aid.[17]

The estimates in Table 2 could be confounded by an effect of passing the SABER 11 or SISBEN threshold that is not purely due to access to financial aid. This would be the case, for instance, if students were more likely to be offered college admission if they scored above the SABER 11 threshold, or if the SISBEN cutoff were correlated with other factors affecting college entry (e.g., other transfer programs). A way to detect this kind of bias is to perform a placebo test by running the same regression in the pre-period sample. Intuitively, in order for the RD

[17] We focus on the reduced-form estimates, which capture the effect of financial aid eligibility on the outcomes of interest. Insofar as need-based and merit-based eligibility represent financial aid offers (contingent on aid being used to study at a high-quality HEI), this answers the relevant policy question.

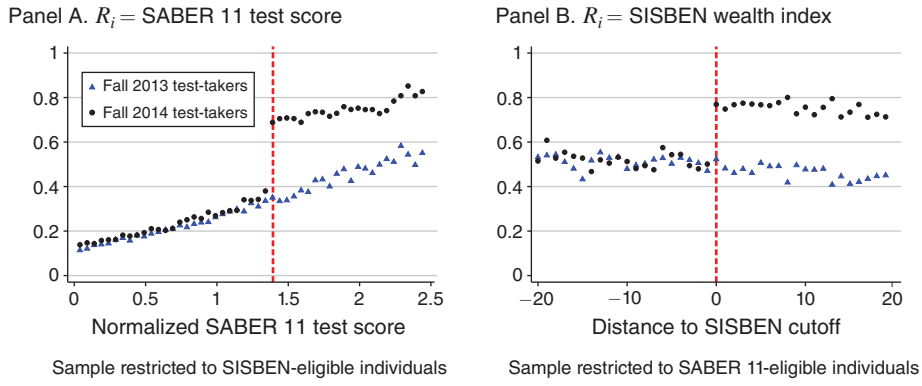

FIGURE 5. PLACEBO TEST USING PRE-TREATMENT PERIOD

Notes: These figures plot the probability of immediately accessing any postsecondary institution for test-takers in fall 2013 (before SPP) and fall 2014 (after SPP) as a function of the normalized SABER 11 score (panel A) and the SISBEN (panel B) eligibility cutoffs. The SABER 11 scoring mechanism changed in fall 2014; for this reason, panel A uses normalized SABER 11 score as the running variable. For fall 2013 test-takers (placebo), the regression coefficients—estimated using package rdrobust (Calonico, Cattaneo, and Titiunik 2014)—are −0.009 (robust p-value is 0.327) using SABER 11 as the running variable and 0.007 (robust p-value is 0.667) using SISBEN as the running variable. These results suggest there is no discontinuous change at the cutoffs in the likelihood of immediately attending postsecondary education in the year before SPP is implemented. Moreover, the differences in enrollment probabilities before and after policy rollout become statistically significant only above the cutoffs.

Source: Authors' calculations based on ICFES, DNP, and MEN.

design to identify the causal impact of financial aid on the outcomes of interest, then the running variables R_i cannot affect the outcome of interest in the absence of SPP. We test for a discontinuity in immediate postsecondary enrollment around the equivalent SABER 11 and SISBEN eligibility thresholds among students that took SABER 11 in fall of 2013, a year before SPP was created (a placebo test).

Figure 5 overlays these probabilities. Using SABER 11 as the running variable, the regression coefficient is −0.009 (p-value is 0.327) for fall of 2013 test-takers; using SISBEN it is 0.004 (p-value is 0.667). Moreover, the difference in immediate postsecondary enrollment among barely-ineligible students before and after policy rollout is not statistically significant below the cutoffs. This, coupled with the absence of a statistically significant discontinuity at either SABER 11 or SISBEN cutoffs in fall of 2013, lends credence to the identifying assumption that the jump in immediate postsecondary enrollment is *caused* by the financial aid program.[18] Remarkably, SPP raised immediate enrollment by 25 percentage points even among extremely high-performing, low-income test-takers (e.g., those scoring 2.5 standard deviations above the mean, or the top 1.5 percent). This suggests that outstanding

[18] Further, Figure 5 informs about the pre-treatment selection. Panel A suggests that, among sufficiently poor students, the likelihood of immediate college enrollment is a strictly increasing function of test scores. In fact, test-takers scoring one standard deviation above the mean in fall of 2013 are more than twice as likely to immediately enroll in any HEI than those scoring exactly at the mean. In contrast, this enrollment gradient does not exist for sufficiently high performers around the SISBEN wealth cutoff; if anything, immediate enrollment *falls* for extremely low-income students.

students were not attending college due to binding credit constraints and that financial aid relaxed these constraints.

A crucial characteristic of the SPP program is that it restricts institutional choice to universities awarded High Quality Accreditation. Figure 6 plots enrollment in high-quality and low-quality HEIs by SABER 11 test score (panels A and C, respectively) and SISBEN wealth score (panels B and D, respectively). The corresponding RD estimates are presented in columns 2 and 5 in Table 2. The results suggest that financial aid eligibility raised enrollment in high-quality HEIs by 46.5 percentage points at the test score cutoff. On a base of 10.9 percent, this implies an increase of 426.6 percent. The estimated coefficient is similarly large and significant at the SISBEN cutoff, even though the baseline is more than twice as large. Contrastingly, enrollment in low-quality HEIs dropped in similar magnitudes for both types of compliers: the reduced-form estimate is −15.4 to −12 percentage points (−57.7 to −53.3 percent). Therefore, financial aid pushed students out of no-college education and low-quality education and into high-quality education. This is one of the most important features of SPP.

The shift to colleges with High Quality Accreditation also gears students toward larger returns to schooling investment and is concomitant with access to more selective institutions, higher-quality peers, and more generous student resources. Online Appendix B analyzes how our results compare when using six different metrics of college quality. To focus on the impact financial aid has on the quality of the institution a student attends, we restrict the sample to test-takers who enrolled in a university immediately after high school. Aid has significant improvements along this intensive margin, raising peer quality (mean high school test scores), university quality (college exit test scores, graduation rate, share of faculty with a doctorate), and resources students are exposed to (spending per student, research spending per faculty member). We conclude that since college quality causally affects earnings (Hoekstra 2009, Saavedra 2009, Zimmerman 2014), financial aid has the potential to significantly promote intergenerational mobility.

As a result of the gains in postsecondary attendance among low-income, high-achieving students, the SES enrollment gradient shrank following a financial aid expansion. Figure 7 plots immediate enrollment probabilities for SABER 11-eligible students by socioeconomic stratum (where 1 represents the poorest households and 6 represents the wealthiest households) immediately before and after policy rollout. Financial aid yielded a large equity gain: enrollment for bottom-strata students increased by 46.4 percent at any HEI (panel A) and by 182.1 percent at high-quality HEIs (panel B). In doing so, SPP leveled access to postsecondary schooling and high-quality schools among high-performing students.

Even though SPP required students attend a high-quality university, there was no restriction on whether it be a public or private HEI. Table 2 further decomposes low-quality and high-quality enrollment by public versus private HEIs. We again analyze the results separately for each type of complier. For sufficiently low-income students around the test score cutoff, Figure 8 shows that the aforementioned high-quality enrollment effect operates completely through enrollment at *private,* high-quality HEIs. In fact, on a base of just 3.3 percent, aid eligibility raised private, high-quality enrollment more than fifteenfold. Critically, for this population of

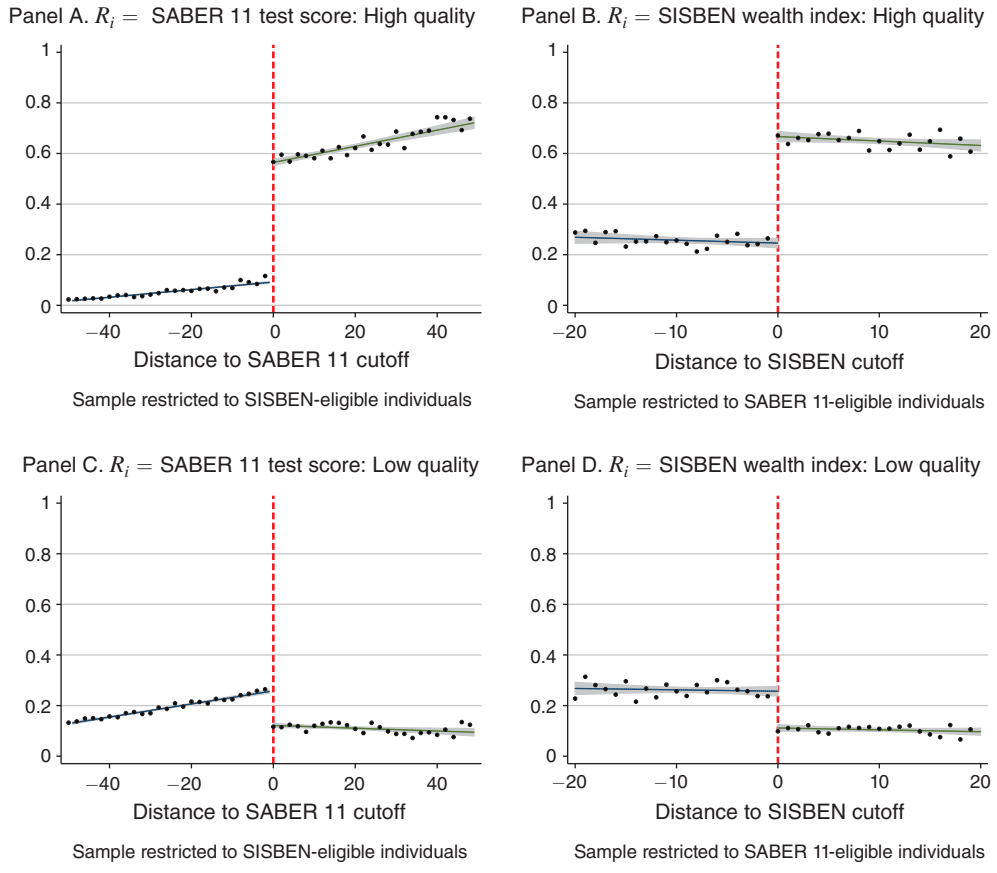

FIGURE 6. IMMEDIATE POSTSECONDARY ENROLLMENT: HIGH- VERSUS LOW-QUALITY INSTITUTIONS

Notes: The figures plot immediate enrollment probabilities by HEI quality for each running variable, SABER 11 test score and SISBEN poverty index. Panels A and B plot enrollment in high-quality HEIs as a function of the distance to the eligibility cutoffs. Panels C and D do the same for low-quality HEIs. The figures show that the likelihood of attending a high-quality HEI immediately after high school rose between 39.6 and 46.5 percentage points (152–427 percent), while the probability of attending a low-quality HEI decreased between 12 and 15.4 percentage points (53–58 percent). See reduced-form estimates in Table 2.

Source: Authors' calculations based on ICFES, DNP, and MEN.

compliers, enrollment in *public* high-quality HEIs remains virtually unaffected by financial aid (see Table 2, panel A, column 4). For high performers barely above the wealth cutoff—who are more than twice as likely to attend public, high-quality HEIs than the controls from the former comparison—enrollment in public, high-quality HEIs *decreased* by 7.9 percentage points or 40.7 percent (see online Appendix Figure A.3). These results suggest SPP induced students to sort across institutions, particularly from low-quality to high-quality HEIs, and often from public to private HEIs.

To explore what drives students to choose private over public high-quality HEIs in Colombia, we turn to survey evidence from SISBEN-eligible students who took SABER 11 in fall of 2015 and scored slightly above or below the SABER 11 eligibility cutoff. Those attending a HEI by spring of 2016 (68 percent of surveyed

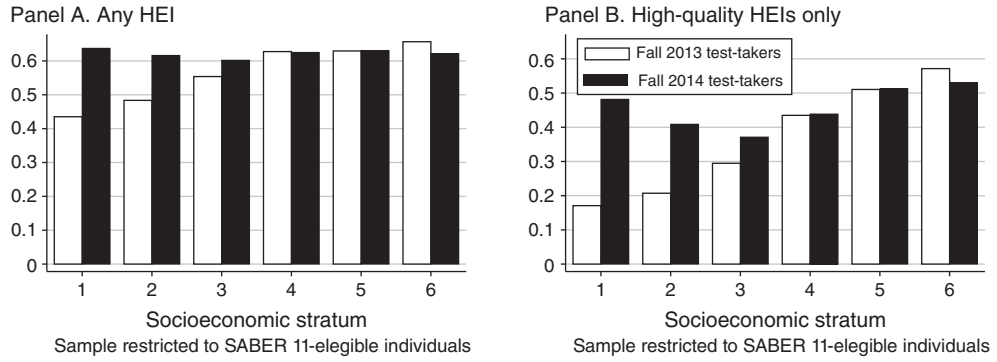

Figure 7. The Enrollment Gap Disappeared among Top Students

Notes: The figures present the probability of immediately enrolling in any postsecondary institution (panel A) and a high-quality institution (panel B) among SABER 11-eligible test-takers in fall 2013 and 2014 aged 14–23. The likelihood of immediately enrolling in postsecondary education increased from 43.5 percent to 63.7 percent for students in stratum 1, i.e., an increase of 46.4 percent (panel A). The probability of attending a high-quality institution increased by 182.1 for students from stratum 1 (panel B).

Source: Authors' calculations based on ICFES, DNP, MEN, and SPADIES.

students) reported the main factors driving their institutional choice among a list of alternatives. Online Appendix Table A.5 displays these summary statistics. The most important factor is prestige, second only to availability of preferred major.[19] Importantly, prestige, academic quality, and better job prospects are more prevalent among students attending private versus public HEIs. Indeed, graduates from top private schools enjoy a wage premium over top public schools, even when controlling for individual-level characteristics (e.g., SABER 11 score, household SES) and college-level characteristics (see Riehl, Saavedra, and Urquiola 2016 and online Appendix H). In contrast, affordability is one of the most attractive features of public HEIs, confirming tuition fees are a key determinant of student sorting across schools. In sum, survey evidence suggests that the higher demand for private postsecondary education is a response to the perception that private HEIs are more reputable and produce greater value added—broadly defined—for students.

In addition to the answers provided by survey respondents, there are other reasons why students may prefer to attend private over public universities. First, for a student undecided between public or private university, she may select the school that offers the highest subsidy in price value (i.e., the private school). Second, public HEIs often require applicants to sit for their own competitive entrance exam. In 2017, UNAL, Colombia's flagship public university, charged applicants COP$98,000 (US$34) for this exam. For applicants on the margin of attending a public versus private HEI, the investment required to prepare for this entrance exam may be deemed too costly. Third, test performance among SPP beneficiaries may be high enough to be

[19] This is consistent with models in which applicants have endogenous tastes for colleges with good reputation (i.e., those with high-ability peers) because firms set their wages by inferring skill levels from the reputation of the college attended (see MacLeod and Urquiola 2015). Indeed, evidence from Colombia suggests college reputation determines initial wages as well as subsequent earnings growth (see online Appendix Figure H.1 and MacLeod et al. 2017).

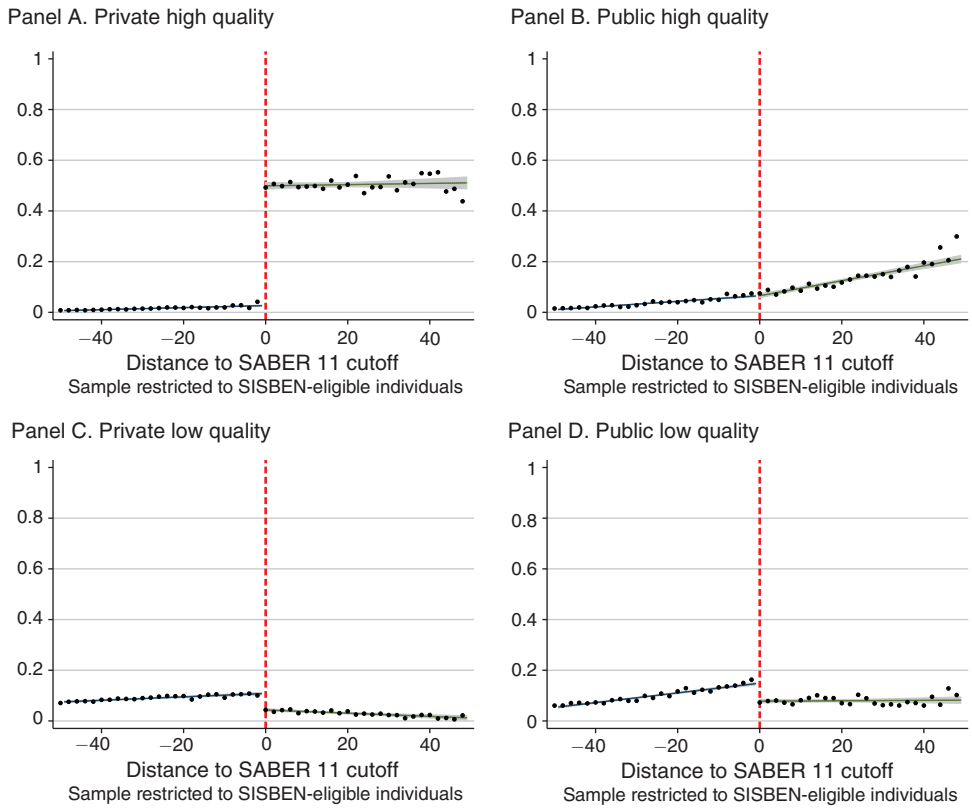

FIGURE 8. IMMEDIATE POSTSECONDARY ENROLLMENT: HIGH- VERSUS LOW-QUALITY, PRIVATE VERSUS PUBLIC INSTITUTIONS (R_i = SABER 11 TEST SCORE)

Notes: The figures plot the probability of immediate enrollment in a private or public, high- or low-quality postsecondary institution as a function of the distance to the SABER 11 test eligibility cutoff. The sample is restricted to SISBEN-eligible students. The likelihood of immediately attending a private, high-quality institution rose 46.6 percentage points (1,412 percent), while the probability of attending a public, high-quality institution did not change. The likelihood of attending a private or public low-quality institution decreased by 6.3 and 8.7 percentage points (59 and 55 percent), respectively. See reduced-form estimates in Table 2. The equivalent figures using SISBEN as the running variable are displayed in online Appendix Figure A.3.

Source: Authors' calculations based on ICFES, DNP, and MEN.

admitted at private but not public HEIs. However, the bottom 5 percent of UNAL's fall 2013 entering cohort (i.e., pre-SPP) had SABER 11 scores *below* the equivalent SPP cutoff. Ceteris paribus, if admission at public colleges were based solely on SABER 11, SPP beneficiaries would have been admitted at UNAL—and arguably any other top-ranked public school—had they chosen to apply.[20]

[20] Another explanation is the application deadline of some public HEIs—such as UNAL—expired before SPP was announced. While this deadline may have been binding for some SPP applicants, it is by no means the driver of the general gap between public and private institutions. Had potential SPP applicants wanted to attend UNAL, the number of UNAL applicants would have increased in spring of 2016 relative to spring of 2015, shrinking the gap between public and private HEIs in the second cohort of SPP. In practice, we observe the exact opposite: the number of UNAL applicants stayed constant between 2014 and 2016, and the public-private gap widened for the second cohort of SPP students (see online Appendix D).

Before we turn to the broader effects of financial aid on secondary and postsecondary education, we briefly summarize our findings on medium-term postsecondary enrollment and persistence 1.5 years after taking the high school exit exam, analyzed in online Appendix C.[21] First, financial aid significantly improved any enrollment within 1.5 years. While this outcome remains the same for aid-ineligible students before and after SPP, it jumps dramatically for students above the aid eligibility cutoffs. However, as the probability of any enrollment of aid-ineligible students increases over time (although not more so than they would have in the absence of SPP), the magnitude of the enrollment gains diminishes relative to the immediate enrollment results. For instance, for need-eligible students, aid eligibility increases any enrollment within 1.5 years by 19.1 percentage points. On a base of 60.9 percent, this implies a 31 percent increase in any enrollment within 1.5 years, or roughly a third of the effect on immediate enrollment.

Second, changing the dependent variable in the RD design for being enrolled in any HEI in the spring of 2016 term—as well as the type of HEI a student attends that term—leads to a similar conclusion: the impact of financial aid is positive and significant, although diminishes relative to immediate enrollment because control students somewhat catch up over time. However, these RD results conflate persistence with the positive extensive and intensive margin results we documented above (e.g., compliers attend better-quality HEIs with lower dropout rates). We therefore complement this analysis with OLS and IV-2SLS specifications, where we restrict the sample to immediate enrollees and control for relevant individual and institutional characteristics. Our preferred specification suggests SPP increased medium-term persistence by 15.8 percent. Partly by shifting students towards high-quality colleges with better graduation rates and more resources, and partly by requiring beneficiaries pay back the loan if dropping out, financial aid improved college persistence for these high-achieving, low-income students.

IV. Upstream and Downstream Impacts of Financial Aid

A. *Financial Aid Raised Overall Immediate Enrollment*

The significant enrollment gains from financial aid might have little impact on overall college enrollment if aid recipients simply displaced nonrecipients from high-quality HEIs (i.e., a zero-sum admission game). Alternatively, postsecondary attendance might also increase among students *ineligible* for aid if college supply and demand are responding to the large aid expansion, thus producing net social gains. To study enrollment changes across time among *all* high school students—both eligible and ineligible for aid—we move away from the cross-sectional RD design and instead use a difference-in-differences design that compares outcomes across all high school exit test-takers between 2011 and 2015 (roughly 3 million students).

[21] With the first SPP beneficiaries scheduled to graduate starting in 2019, longer-term outcomes such as overall dropout rate and completion will only become observable in our data around 2021–2022.

Recall from Section I that high school seniors take SABER 11 in either the spring or the fall semester according to their graduation date, as there are two graduating cohorts per year. Immediate postsecondary enrollment decisions for each graduating cohort occur at different moments in a year and relatively independently from one another. Colleges admit new students every semester and have separate admission processes and distinct SABER 11 cutoffs for each cohort. This, coupled with the fact that SPP eligibility was based on fall—not spring—test performance, indicates spring test-takers may serve as an adequate control group in a difference-in-differences model.

Our baseline empirical strategy compares enrollment outcomes between spring (control) and fall (treatment) test-takers across time:

$$(1) \qquad y_{itm} = \alpha_0 + \alpha_1 \mathbf{1}\big(\textit{fall test-taker}\big)_i + \delta_t + \sum_{k \neq 2013} \beta_k \mathbf{1}\big(\textit{fall test-taker}\big)_i \times \delta_t + \gamma_m + \epsilon_{itm},$$

where y_{itm} is outcome y for test-taker i in year t and municipality m, $\mathbf{1}\big(\textit{fall test-taker}\big)_i$ is an indicator for taking the SABER 11 exam in the fall semester, δ_t are calendar year fixed effects, γ_m are municipality fixed effects, and ϵ_{itm} is the individual-specific error term. We normalize the results with respect to 2013, the year immediately before financial aid expanded.[22] We plot the event study coefficients β_k in the figures and the standard difference-in-differences coefficients in online Appendix Table A.6, which summarize the difference in outcomes between the treatment group relative to the control group, before and after SPP.

Naturally, spring test-takers will be different in observable and unobservable characteristics from fall test-takers. For instance, spring test-takers are ex ante more likely to graduate from private high schools, have wealthier families, more educated parents, and attend at least *some* college. However, the identifying assumption underlying our design is that the *trends* in the two groups evolve in parallel before policy rollout. Because we observe three periods before SPP (2011–2013), we can assess the validity of this assumption by plotting the difference in outcomes between treatment and control groups across time.

If spring test-takers somehow were affected by financial aid, this could potentially threaten our identification strategy. For instance, if high school students can decide their graduation semester and/or when to take the SABER 11 exam in response to SPP, then control students might self-select into treatment. Yet, we do not find evidence consistent with strategic changes in test-taking behavior driving our main results. First, SPP was announced *after* high school seniors took SABER 11 in 2014, thus ruling out the hypothesis that the large impacts we document taking place that year are driven by changes in the composition of test-takers; if these exist, they would affect 2015 test-takers only. Second, graduation semester is fixed within school. Spring 2015 students could transfer to a fall graduation school or, what is more plausible, retake the SABER 11 exam in fall of 2015 and apply with those new

[22] We restrict our estimation sample to test-takers aged 14 to 23, as they are more likely to be high school seniors at the time of taking SABER 11.

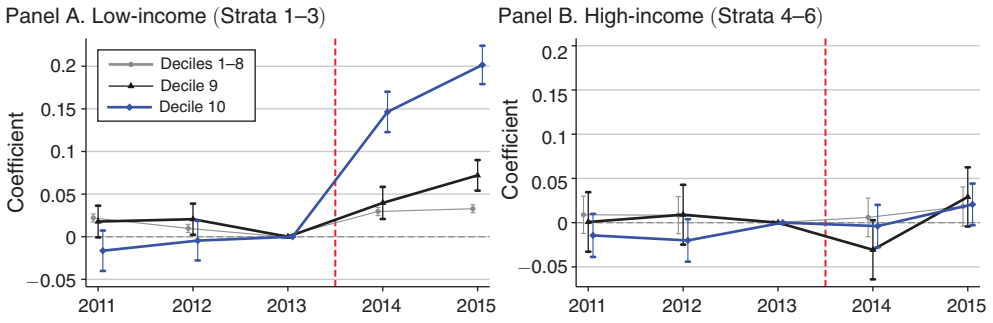

Figure 9. Immediate Enrollment for Low- and High-Income Students by SABER 11 Decile

Notes: These figures plot immediate enrollment probabilities separately by socioeconomic stratum and test score performance among fall test-takers (treatment) relative to spring test-takers (control) before and after SPP financial aid is introduced (red vertical line) using specification (1). Panel A suggests that financial aid raised immediate enrollment for low-income students. This effect is strongest for top-performing students—i.e., decile 10, as these students are most likely to receive financial aid—but it is also positive and significant for lower-performing students. In contrast, panel B shows that financial aid had little enrollment impact among high-income students, except a temporary displacement effect for decile 9. Online Appendix Figure A.4 confirms high-income students were temporarily displaced from private, high-quality HEIs.

Source: Authors' calculations based on ICFES, DNP, and MEN.

scores. However, we find that the number of fall test-takers remained stable before and after 2015.[23]

In what follows, we present results separately for low-income and high-income test-takers. Because we do not have SISBEN scores for all cohorts, we use socioeconomic stratum—a variable reported by virtually all test-takers—as a proxy for SES. We henceforth refer to "low-income" students as those from strata 1–3 and to "high-income" students as those from strata 4–6. Figure 9 plots immediate enrollment probabilities separately for low-income and high-income students by their relative test performance using specification (1). Panel A confirms the findings from the RD design, namely that, thanks to SPP, low-income students scoring in the top decile are significantly more likely to enroll in high-quality HEIs (blue curve). However, enrollment appears to have also increased among lower-performing, low-income students who, by virtue of scoring below the top decile, are *ineligible* for SPP (gray and black curves). While these results are an order of magnitude smaller than those documented for the top decile, they are positive and statistically significant. In contrast, panel B shows that enrollment remained stable across time for high-income students.[24]

[23] A separate concern is that HEIs may reallocate spaces from spring to fall following scholarship rollout, thus leading to an upward bias because treatment is negatively affecting the spring graduation cohorts (we thank an anonymous referee for pointing out this potential concern). However, as detailed in online Appendix F, we do not find evidence of this; the number of spring test-takers accessing HEIs immediately after high school remained stable after SPP.

[24] In fact, the black curve in Figure 9 suggests some high-income students may have been temporarily displaced from HEIs the year financial aid was introduced. Online Appendix Figure A.4 confirms this displacement took place at private, high-quality HEIs. Online Appendix E delves into this temporary displacement effect using admission records from one of the country's top-ranked private HEIs. We identify and characterize displaced applicants and

This positive spillover effect of financial aid on low-income, aid-*ineligible* students could be a result of demand-side and/or supply-side responses to aid. On the demand side, the advertising push associated with SPP could spill over into aid ineligible populations. For instance, it could increase the option value of applying to colleges, with students sending their applications before determining whether they are eligible for aid. It could also have increased the perceived benefits of attending college. Peers of eligible students may also feel encouraged to attend college. On the supply side, colleges (especially high-quality, private HEIs) may enlarge their cohorts in response to any higher demand. Furthermore, low-quality HEIs might fill the empty seats left by aid-eligible students with the next best applicants.

To explore demand-side and supply-side responses to financial aid, we complement our postsecondary enrollment data with college admission records. Our results, summarized in online Appendix F, suggest that, first, the demand for high-quality education significantly increased following the expansion of financial aid. Specifically, the number of undergraduate applications received by *private,* high-quality HEIs skyrocketed, even doubling at some of these institutions two years after policy rollout. In contrast, the demand for low-quality HEIs and *public,* high-quality HEIs appears to be largely unaffected by financial aid. Second, in response to the heightened demand, private, high-quality HEIs expanded their supply: cohort size increased at these institutions and, as a result, so did overall enrollment. The effect is smaller and not statistically significant for comparable public HEIs.

Figure 10 further decomposes the positive enrollment effects among low-income students from Figure 9 by type of HEI. Panel A shows that enrollment at low-quality HEIs decreased for top-performing students and increased for lower-performing students (deciles 1–9). Instead, the coefficients for high-income students are close to zero and not statistically significant. This suggests low-quality HEIs—both public and private—fill the empty seats left by financial aid recipients with lower-performing, low-income students. Panel B in Figure 10 presents the equivalent results for high-quality HEIs. Interestingly, enrollment also increased for top-decile students at public, high-quality HEIs (blue line). This suggests public, high-quality HEIs remain highly oversubscribed despite the outflow of SPP beneficiaries and, therefore, fill their empty seats with aid-ineligible but equally high-achieving applicants.[25] These results are consistent with both supply-side and demand-side responses to financial aid expansion generating positive overall enrollment gains among low-income, aid-*ineligible* students.

track them to the colleges where they end up enrolled around the country. We find that displaced high-income applicants enrolled in lower-ranked, high-quality, private HEIs.

[25] In fact, panel B in online Appendix Table A.6 and Figure A.4 present the equivalent results for students from strata 4–6, and suggest public, high-quality HEIs filled their empty seats with *high-income,* high-achieving students too. There is no statistically significant difference in low-quality enrollment before and after financial aid expanded for high-income students.

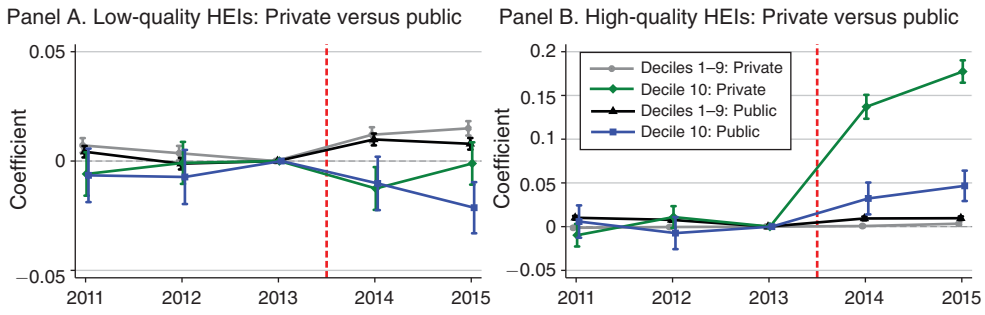

FIGURE 10. LOW-INCOME STUDENTS ONLY: ENROLLMENT BY SABER 11 DECILE AND HEI TYPE

Notes: These figures plot, for low-income students (strata 1–3), the difference in immediate enrollment probabilities separately by test score decile and HEI type between fall (treatment) and spring (control) test-takers before and after SPP financial aid is introduced (red vertical line) using specification (1). Panel A suggests that while enrollment somewhat fell at low-quality HEIs for top-performers, it increased for lower-performing students (deciles 1–9). This is presumably because low-quality HEIs are filling the empty seats left by SPP beneficiaries with their next best, lower-performing applicants. Panel B confirms the RD results and shows that enrollment of low-income top-achievers at private, high-quality HEIs significantly increased two years after the expansion of financial aid. Interestingly, although SPP beneficiaries sorted out of public, high-quality HEIs, overall enrollment of top-decile test-takers increased at these HEIs, albeit by a significantly smaller amount. Online Appendix Figure A.4 plots the equivalent figures for high-income students.

Source: Authors' calculations based on ICFES, DNP, and MEN.

B. *College Student Body Composition: Student Quality and SES*

As financial aid encouraged postsecondary attendance among high-achieving, low-income students, the college student body composition—specifically, student quality and socioeconomic status—changed drastically. To be consistent with previous analyses, we measure "student quality" as the share of new enrollees scoring in the top decile of the test score distribution and "class diversity" as the share of entering students from strata 1–3.[26] To test the effect of aid expansion on student quality and the share of low-income students, we compare these two outcomes within colleges and between treatment and control groups across time:

$$(2) \qquad y_{it} = \phi_0 + \phi_1 \mathbf{1}\big(\textit{spring entering class}\big)_i + \delta_t + \sum_{k \neq 2014} \beta_k \mathbf{1}\big(\textit{spring entering class}\big)_i \times \delta_t + \gamma_{j(i)} + e_{it},$$

where y_{it} is the outcome of interest of student i attending HEI j in year t, $\mathbf{1}\big(\textit{spring entering class}\big) = 1$ for enrollment in the spring term (i.e., fall test-takers), δ_t and $\gamma_{j(i)}$ are calendar year and HEI fixed effects, respectively, and e is the error term.

As Section IVA suggested, demand for private, high-quality education expanded after SPP and, as a result, so did its supply. Yet because cohort size increased less than one-to-one with respect to applications, the admission rate of these institutions

[26] Some researchers use the distribution of skill among graduates or the average quality of admitted students as a measure of college "reputation" (see MacLeod and Urquiola 2015, MacLeod et al. 2017).

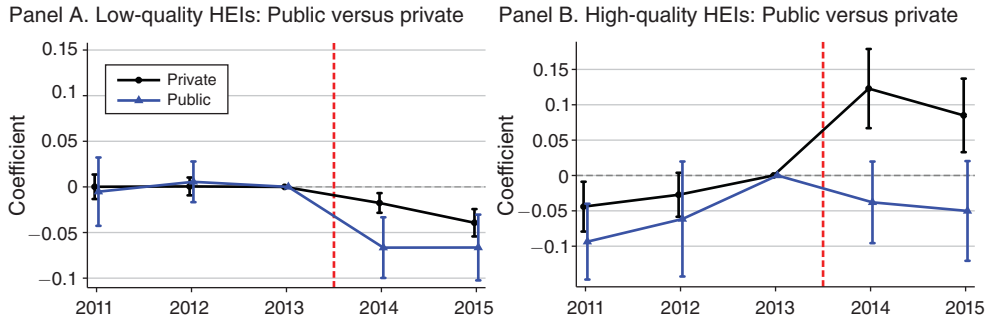

Figure 11. Student Quality: Share of Entering Students Scoring in Top Decile by HEI Type

Notes: These figures plot the share of new enrollees scoring in the top decile of SABER 11 test scores among spring (treatment) relative to fall (control) entering classes before and after SPP financial aid is introduced (red vertical line) using specification (2). Panel A suggests student quality dropped at low-quality HEIs, presumably because the empty seats left by financial aid recipients are being filled with lower-performing students. Panel B shows student quality was not significantly affected at public, high-quality HEIs, but increased at private, high-quality HEIs thanks to the influx of SPP beneficiaries. These results are qualitatively similar using average SABER 11 percentile as an alternative measure of student quality.

Source: Authors' calculations based on ICFES, DNP, and MEN.

dropped, making them significantly more selective. For instance, the admission rate at the University of Los Andes, Colombia's flagship private university, dropped by one-half just two years after policy rollout (see online Appendix F). Figure 11 plots student quality by HEI type, using specification (2). Panel A shows student quality dropped at low-quality HEIs after SPP. This is consistent with low-quality HEIs filling the empty seats left by financial aid recipients with the next best, lower-performing applicants. Contrarily, student quality was virtually unaffected at public and high-quality HEIs, as these (generally oversubscribed) HEIs fill any empty seats with aid-ineligible students of similar ability (see online Appendix Table A.6). Finally, student quality increased at private, high-quality HEIs thanks to the influx of SPP beneficiaries.[27]

To test the effect of financial aid on class diversity, Figure 12 plots the β_k coefficients from specification (2) using the students' socioeconomic stratum (i.e., strata 1–3) as the outcome variable by type of HEI. The share of low-income enrollees increased by 13.7 percentage points two years after financial aid expanded at private, high-quality HEIs. On a base of 29.9 percent in the control group prior to SPP, this represents a 46 percent increase in SES diversity at these elite institutions. The equivalent increase at private, low-quality HEIs is an order of magnitude smaller and not statistically significant. Moreover, SES diversity remained completely unaffected at public HEIs, whose student population historically is made up

[27] Online Appendix Figure A.5 complements these results by plotting, for each high-quality HEI, the difference in mean SABER 11 percentile among first-semester students immediately before and after SPP. The figure suggests that the magnitude of the positive ability impact is inversely proportional to the institutional ranking, as measured by the average quality of admitted students before SPP (panel A).

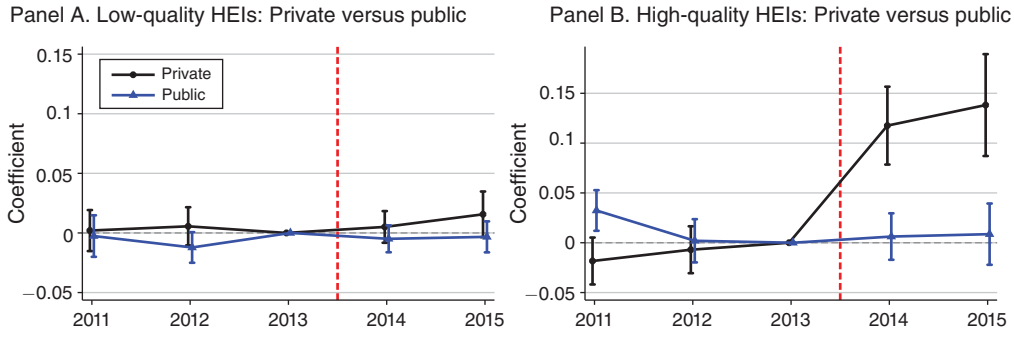

Figure 12. Class Diversity: Share of Entering Students from Strata 1–3 by HEI Type

Notes: These figures plot the share of entering students in the spring (treatment) relative to fall (control) entering classes who are low-income (i.e., strata 1–3, as opposed to strata 4–6) before and after SPP financial aid is introduced (red vertical line) using specification (2). While the share of low-income first-year enrollees did not change either in low-quality HEIs (panel A) or in public, high-quality HEIs (panel B, blue curve), this share increased by 13.7 percentage points at private, high-quality HEIs two years after financial aid expanded (panel B, black curve). On a base of 29.9 percent, this represent a 46 percent increase in SES diversity at private, high-quality HEIs.

Source: Authors' calculations based on ICFES, DNP, and MEN.

of students from low-income and middle-income backgrounds, as suggested by the control means from panel C in online Appendix Table A.6.[28]

Together, these results provide suggestive evidence of important compositional effects due to nonrandom sorting of students from no postsecondary education and across college types. As private, high-quality HEIs "cream skim" the most able students away from no-college and low-quality schools, increased sorting by ability raises stratification by ability and widens the quality gap in equilibrium. It also substantially promotes socioeconomic diversity at these elite HEIs (see Londoño-Vélez 2020).

C. *Demand for High Quality Accreditation by Low-Quality HEIs*

As with other voucher programs, institutions that are left out of the program are pressured to become more efficient (Epple, Romano, and Urquiola 2017). In the case of SPP, the requirement that students enroll at high-quality HEIs puts pressure on low-quality HEIs to receive High Quality Accreditation (but whether or not this reflects an actual improvement in the quality of education provided remains to be seen). Indeed, HEIs immediately responded to SPP's announcement by requesting High Quality Accreditation (although receiving this accreditation status did not systematically follow, as shown in online Appendix Figure A.7). This suggests that

[28] To further illustrate this point, online Appendix Figure A.6 compares the SES distribution of entering cohorts before and after financial aid expansion in two flagship private and public HEIs. The figure shows that the share of students from the bottom two strata *decreased* by 3 percentage points at a top-ranked public HEI (panel C), while it increased by only 6.3 percentage points at another flagship public HEI (panel D).

SPP, by restricting college choice to high-quality HEIs, pressured low-quality HEIs to become more efficient to attract high-ability students.[29]

D. *Improvements in Relative Pre-Collegiate Test Performance*

The newfound possibility of a tuition-free postsecondary education brought about by SPP raised low-income high school students' incentives to perform well in SABER 11 (Laajaj, Moya, and Sánchez 2018). In this section, we compare relative performance in standardized testing across time by socioeconomic strata and document improvements among very low-income students after SPP was announced.

Figure 13 plots the percentage change in the share of students that score in the top SABER 11 percentiles by socioeconomic stratum between fall 2012 and 2016. Panel A suggests that the share of very low-income students (strata 1 and 2) scoring in the top decile increased by 32 and 14 percent, respectively, between fall 2012 and 2016. Moreover, low-income students crowded out their higher-income peers from the top of the distribution: the shares of top decile performers from strata 3 through 6 *decreased* by 1–2 percent.[30] Panel B, which plots the percentage change between fall 2012 and 2016 in the share of test-takers in strata 1 and 6 in the top SABER 11 percentile, shows that the improvement in test performance occurs at the very top of the test score distribution: the share of students in strata 1 and 2 scoring in the top percentile increased during this period by 175 and 28 percent, respectively (see also online Appendix Figure A.8).[31] Moreover, the magnitude of these results increases over time, as students, parents, and teachers arguably have more time to reoptimize in response to the policy change.

We interpret these findings as suggestive evidence that student effort responds to the higher incentives for performance in standardized testing provided by merit-based financial aid. This is consistent with Laajaj, Moya, and Sánchez (2018), who show that test score improvements in fall 2015 are concentrated precisely among SISBEN-eligible high school seniors. It is also consistent with previous studies documenting the extent to which scholarships and vouchers improve individual study effort in other contexts (Angrist and Lavy 2009; Angrist et al. 2002; Barrera-Osorio and Filmer 2016; Kremer, Miguel, and Thornton 2009). Even though we remain agnostic as to which mechanisms are behind such test score gains (e.g., improved learning, teacher effort, parental investment, test cramming), these gains may have positive medium-run and long-run effects, insofar as higher SABER 11

[29] The expansion of financial aid could also induce other supply responses such as colleges raising tuition fees, which we explore in online Appendix G. Even though the demand for high-quality private education expanded, we do not detect an increase in tuition fees for this type of education. We argue tuition fees are regulated in Colombia, even among private HEIs. Moreover, tuition hikes are not implemented in practice because HEIs are restricted in the amount they can raise tuition fees in real terms from year to year.

[30] Between fall 2012 and 2016, the share of stratum 1 test-takers scoring in the top decile increased by 1 percentage point. Given that only 3.1 percent of stratum 1 test-takers scored in the top decile ex ante, this represents a 32 percent increase in performance for the lowest-income test-takers. In contrast, the share of top decile test-takers decreased by 1.4 percentage points among stratum 6. Given that 57.4 percent of stratum 6 test-takers scored in the top decile ex ante, this represents a 2.4 percent decrease in performance for the highest-income test-takers.

[31] The absence of bunching around the top decile cutoff could suggest students are not simply increasing their scores to barely make themselves eligible for SPP; inter alia, this would not be a dominant strategy because SPP's SABER 11 eligibility cutoff has increased over time (see Section I).

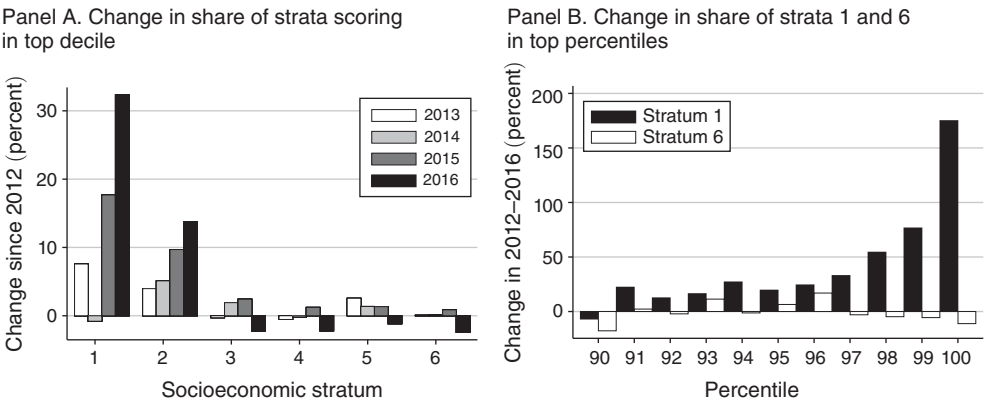

Figure 13. Gains in Test Performance for Low-Income Students

Notes: These figures show improvements in SABER 11 performance among low-income students (strata 1 and 2) since the expansion of financial aid in Colombia. Panel A plots the percentage change in the share of students in each socioeconomic stratum that score in the top SABER 11 decile in fall 2013 through 2016, using 2012 as baseline. The figure suggests the share of test-takers from the bottom stratum scoring in the top decile increased by 32 percent between 2012 and 2016. Panel B plots the percentage change between fall 2012 and 2016 in the share of test takers in strata 1 and 6 scoring in the top SABER 11 decile, by percentile of the test score distribution. The figure shows there was an increase in the share of students in stratum 1 scoring in the top decile, and particularly in the top percentile, where the increase was of 175 percent. The sample in all figures is restricted test-takers aged 14–23.

Source: Authors' calculations based on ICFES.

scores have been shown to be associated with improved performance in college and better labor-market outcomes—even after controlling for baseline individual and college characteristics (MacLeod et al. 2017).

V. Conclusion

The Colombian higher education system was characterized by a severe segregation due to costly tuition fees and a dearth of financial support for low-income students. In this context, a large-scale, need-based and merit-based financial aid program significantly improved postsecondary enrollment among high-achieving, low-income students. In fact, the program virtually eliminated the SES enrollment gradient among top decile test-takers. Moreover, providing beneficiaries choice over college types induces a shift from low-quality to high-quality HEIs and, to a lesser extent, from public to private HEIs.

We also observe significant upstream and downstream effects of financial aid on secondary and postsecondary education, which also affect students who are ineligible for aid. The program drastically changed the student body composition of colleges. It promoted class diversity at private, high-quality HEIs—institutions historically reserved for the lucky few able to afford them. As the demand for private, high-quality education expanded, financial aid raised entry competition and improved student quality at these HEIs. However, program beneficiaries did not fully crowd out high-SES students because the supply of private, high-quality postsecondary schooling also expanded. Moreover, as low-quality HEIs filled their empty seats

with the next best applicants, immediate enrollment also rose for aid-ineligible students, and particularly low-income students just below the test score cutoff. Finally, the announcement of financial aid was followed by an increase in high school exit test performance by students from relatively poor backgrounds.

We posit that financial aid programs like SPP have the potential to shrink the SES enrollment gaps at selective colleges and positively impact secondary and postsecondary education systems. Ultimately, though, we care about how students' long-run outcomes are affected (e.g., college exit test scores, graduation rates, and earnings) and whether financial aid truly promotes social mobility. This also enables assessing whether such a program, which geared students toward elite colleges that are more costly to both the taxpayer and the student (if she were to drop out), is cost-beneficial. A key factor in answering these questions relates to the labor market returns for SPP beneficiaries and nonbeneficiaries, a critical question if elite colleges produce high-end labor market outcomes only for students from wealthy households (Zimmerman 2019). Although the evidence from Colombia is more nuanced (Saavedra 2009), given the numerous upstream and downstream impacts we documented and the potential for peer effects varying across HEI types, it remains too early to draw strong conclusions on this matter. With these cohorts starting to graduate college only circa 2019, it is imperative that future research will explore these issues once data on longer-term outcomes become available.[32]

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[32] Even though a first approximation of a cost-benefit analysis is presented in online Appendix H for the interested reader, the factors highlighted here, as well as others detailed in the online Appendix, preclude us from a convincing assessment.

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