Elite Colleges as Engines of Upward Mobility: Evidence from Colombia's *Ser Pilo Paga**

Juliana Londoño-Vélez
Catherine Rodríguez
Fabio Sánchez
Luis Esteban Álvarez

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Abstract

We study the role of elite colleges as engines of upward mobility. A groundbreaking financial aid reform in Colombia overhauled access to elite universities by rendering them financially accessible for academically successful low-SES students. We leverage stringent program eligibility criteria and nationwide administrative microdata on long-term educational and labor market outcomes to estimate effects on economic mobility, equity, and efficiency. Using a regression discontinuity design, we find that enhancing college quality substantially improved individuals' long-term educational attainment and earnings and successfully narrowed socioeconomic gaps. Using difference-in-differences, we find that it did not disadvantage nonrecipients, promoting both equity and efficiency. **JEL:** H52, I22, I23, I24, I26

*A previous version of this paper circulated as "*Financial Aid and Social Mobility: Evidence from Colombia's* Ser Pilo Paga." Corresponding author: Juliana Londoño-Vélez, Assistant Professor of Economics, Department of Economics, UCLA, Bunche Hall 8283, 350 Portola Plaza, Los Angeles, CA 90095, USA, (e-mail: j.londonovelez@econ.ucla.edu, phone: 510-984-8668). Catherine Rodríguez is a Researcher at Universidad de Los Andes. Fabio Sánchez is a Professor of Economics at Universidad de Los Andes. Luis Esteban Álvarez is a Consultant at Department of National Planning (DNP). We are grateful to Patricia Moreno, Alejandro Corrales, and Yamit López from DNP. Joleen Chiu provided outstanding research assistance. We have benefited from comments and suggestions from Felipe Barrera-Osorio, Martha Bailey, Sandra Black, Zack Bleemer, Natalie Bau, Guillermo Cruces, Susan Dynarski, John Friedman, Caroline Hoxby, Larry Katz, Adriana Lleras-Muney, Yotam Shem-Tov, Miguel Urquiola, Seth Zimmerman, and participants at UCLA, HKS, UChicago Harris, USC, UCR, U. of Los Andes, NBER SI (Labor Studies and Education), LACEA-LAMES, SoCCAM, and Zurich Conference on Public Finance in Developing Countries. Juliana, Fabio, and Catherine gratefully acknowledge financial support from the Spencer Foundation Large Grant #10040436.

1 Introduction

How can governments enhance upward mobility? While many policymakers and scholars view higher education as a pathway out of poverty (Goldin and Katz, 2008), there is a dearth of evidence demonstrating that colleges, particularly those in the top tier, serve as engines of upward mobility. Previous research indicates that students from a low socioeconomic status (SES) encounter difficulties accessing top-tier colleges (Chetty et al., 2023, 2020; Hoxby and Avery, 2013) and do not consistently reap the anticipated benefits (Zimmerman, 2019). Recognizing that these challenges often arise from financial constraints, governments worldwide invest heavily in financial aid for college students. However, evidence on whether such aid effectively improves low-SES students' post-college earnings is limited and mixed (Dynarski et al., 2022).

This paper investigates the role of elite colleges as engines of upward mobility. Our laboratory is Colombia, whose quasi-experimental variation and data are ideal to causally estimate the long-run effects of access to elite colleges for low-SES students. We leverage a groundbreaking financial aid reform that overhauled access to the country's leading universities by rendering these institutions financially accessible for academically successful low-SES students. Prior research indicates a remarkable 46% increase in the enrollment of low-SES students on average (Londoño-Vélez et al., 2020). At the nation's top university, the share of entering students with low SES nearly *quintupled* from 7% to 33% (Londoño-Vélez, 2022). This reform represents the largest experiment ever conducted to diversify elite colleges.[1] Its stringent eligibility criteria provide powerful exogenous sources of variation in elite college enrollment. Moreover, Colombia's comprehensive administrative microdata links all high school test-takers, postsecondary attendees, and formal workers nationwide. Our dataset spans from 2012 (two years before the reform) through 2022 and includes college outcomes (enrollment, quality, completion, and learning) and labor market outcomes (employment and earnings), providing a comprehensive view of the educational and professional trajectories of all high school graduates.

[1] To provide perspective, the impact of the Colombian policy on socioeconomic diversity at elite colleges dwarfs that of hypothetically changing U.S. Ivy League colleges' admissions practices. Chetty et al. (2023) predict that removing legacy preferences, non-academic advantages, and athlete recruitment from high-income families and replacing them with students of similar SAT scores would raise the proportion of students from the bottom 95% of the parental income distribution by only 8.8 percentage points (p.p.). Similarly, adopting a "need affirmative" admissions policy or eliminating race-based affirmative action would also yield modest effects compared to the Colombian program.

In 2014, the national government introduced the "Ser Pilo Paga" (SPP) program, a generous student loan initiative designed to enhance upward mobility by expanding college opportunities for high-achieving low-SES students. The program provided full tuition coverage to individuals who enrolled in one of the 33 government-certified "high-quality" universities (henceforth, HQ colleges). These institutions, comprising both private and public entities, had higher test scores, graduation rates, and per-student spending and "added" more value in imparting skills and providing high-paying job prospects than other institutions. To qualify, individuals had to score in the top 10% on the national standardized high school exit exam and belong to the lower half of the wealth distribution (henceforth, 'low SES'). Notably, those completing their bachelor's degree had their loans forgiven.

Previous research cautions that policies diversifying elite universities are not necessarily welfare-enhancing. While social interactions with high-SES college peers predict future income (Chetty et al., 2022a; Michelman et al., 2021; Zimmerman, 2019), simply exposing low-SES students to high-SES peers does not guarantee cross-class social interactions. Students often display high levels of friendship 'homophily' (Chetty et al., 2022b). The resulting social "mismatch" can lead low-SES students to drop out (Andrews et al., 2020; Arcidiacono et al., 2011), and even if they graduate, securing high-paying positions may prove challenging without access to the same networks as high-SES peers. Low-SES individuals may also encounter class discrimination in the job market (Kraus et al., 2019). Moreover, there is a risk of unintended consequences for nonrecipients, such as potential crowding out from the same institutions or compromised instructional quality. Finally, questions arise about whether the potential benefits, if any, outweigh the substantial program costs associated with funding elite education.

We tackle these concerns by estimating causal impacts on program recipients and nonrecipients, and analyze effects on economic mobility, equity, and efficiency using four separate identification strategies. To identify effects on program recipients, we employ two complementary regression discontinuity (RD) designs. In one, we compare outcomes for students who meet the poverty requirement but have test scores close to the minimum eligibility threshold. In the other, we compare students who meet the test score requirement but are near the poverty cutoff for eligibility. These discontinuities relate to distinct groups: the first group comprises high school graduates in the country's poorest tertile scoring near the 90th percentile on the exam, while the second group consists of students near the 53rd wealth percentile scoring within the top 5%. Interestingly, despite the above-mentioned concerns, the benefits are large for both groups, implying substantial gains from reducing financial barriers hindering access to elite colleges across the distribution of student SES and ability.

Specifically, we find that the policy substantially improves college outcomes. Low-SES students just above the test score cutoff experience a ten p.p. increase in ever attending any college, a 12% improvement over those just below the test score cutoff (the control group). Notably, while most students in the control group typically enroll in low-quality (LQ) institutions, program eligibility significantly boosts the chance of attending HQ colleges by 44 p.p., a 240% increase, with the effects concentrated in elite private institutions. Low-SES high-achievers thrive in HQ colleges, with their likelihood of graduating from these institutions increasing by over 32 p.p., or 330%. Additionally, we present, for the first time, evidence that elite colleges contribute to enhancing students' skills. Analyzing performance in Colombia's mandatory standardized college graduation exam, widely recognized by students, colleges, and employers (see MacLeod et al., 2017), we find that recipients just above the test score threshold perform at least 17% better.

Recipients also experience substantial improvements in the labor market. Eight years after high school completion, their monthly earnings are 36% higher at the test score cutoff and 26% higher at the poverty cutoff. They are also more than twice as likely to belong to the top 1% of the earnings distribution (relative to others of the same cohort), indicating improved upward mobility *and* upper-tail mobility. Importantly, these earnings gains not only offset the temporary earnings loss experienced by students during their college years but also continue to grow over time, consistent with the returns to college quality rising with labor market experience (MacLeod et al., 2017; Zimmerman, 2014). Notably, targeted students achieve earnings gains that surpass the average returns of the institutions they attended, as measured by "value-added," consistent with some research highlighting the benefits of high-quality education for disadvantaged students (Black et al., 2023a; Bleemer, 2021a,b; Dale and Krueger, 2014).

To examine impacts on equity and efficiency, we analyze the entire population of high school seniors graduating before and after the policy implementation. Specifically, to examine equity impacts, we compare outcomes for high-SES and low-SES students with similar test scores before and after the policy change and determine whether the reform increased or decreased SES gaps. We find that the policy significantly narrows the socioeconomic gap in college attainment, skill development, and earnings among academically comparable students.

To assess efficiency, we compare high-SES and low-SES students across the distribution of test scores before and after the aid expansion using difference-in-differences (DD). We determine if any particular group experienced adverse consequences from the reform or if the reform constituted a Pareto improvement. Despite concerns about potential crowd-out effects from such a large-scale program, our analysis reveals no discernible adverse impacts on students ineligible for aid. In response to increased demand, elite institutions expanded their incoming cohorts by around 50%, marking an unprecedented expansion in the current literature.[2] While there might be apprehensions about this expansion compromising instructional quality, diminishing learning experiences, intensifying job competition, and devaluing degrees (Blair and Smetters, 2021; MacLeod and Urquiola, 2015; Urquiola, 2020), our findings contradict these concerns. We confidently rule out negative impacts on degree completion, learning outcomes, and earnings for aid-ineligible students. If anything, our results suggest *positive* spillover effects, possibly attributable to improved peer quality. Consequently, the policy improved both equity *and* efficiency.

To assess whether the policy's benefits justify the high cost of elite education, we conduct a welfare analysis using the marginal value of public funds (MVPF). Even ignoring the positive spillovers, the estimated MVPF, defined as the ratio of recipients' willingness to pay for the policy to the net cost to the government, is 5.6 based on the test score threshold and 4.8 based on the poverty threshold. This indicates that the policy yields over $4.8 of benefits per dollar of net government spending, positioning the Colombian program well above most estimated MVPFs for other grant aid programs (Angrist et al., 2021; Hendren and Sprung-Keyser, 2020).

Our findings contribute to three separate literatures. First, we contribute to a literature investigating whether government policies can foster upward mobility. While existing research offers descriptive mobility analyses (e.g., Chetty et al., 2014), there is scant *causal* evidence showing whether public policies can affect economic mobility. Education is often lauded as a solution to enhance economic mobility, but existing research has mainly focused on policies reinforcing early childhood education (Bailey et al., 2021; Chetty et al., 2011; Fredriksson et al., 2012; Jackson et al., 2015).

[2] Colombian private colleges are constrained in raising prices (Law 30/1992; Decree 110/1994) and rely primarily on student tuition (rather than endowments or government funding). Coupled with limited housing constraints as students typically reside off-campus, this contributes to their supply flexibility.

However, the quality of education in later stages may exert an even greater influence on long-term outcomes (Hoxby, 2021), with colleges playing a pivotal role (Chetty et al., 2023, 2020). This raises the question of what policy levers policymakers can use to enhance educational quality during the "age of opportunity." Some researchers have examined hypothetical changes in admission policies at highly selective U.S. private colleges, but their findings indicate limited mobility gains (Chetty et al., 2023). Moreover, changing private institutions' admissions practices is generally not a policy lever available for policymakers. In contrast, our study showcases substantial mobility effects from a large-scale government policy to facilitate low-SES high-achievers' ability to afford high-quality education.

Second, we contribute to the literature on the returns to elite colleges on later-life outcomes, which has yielded conflicting findings, especially concerning low-SES students (Anelli, 2020; Chetty et al., 2023, 2020; Dale and Krueger, 2014, 2002; Ge et al., 2022; Jia and Li, 2021; Michelman et al., 2021; Saavedra, 2009; Zimmerman, 2019). Our study focuses on what, to our knowledge, is the largest experiment in the diversification of elite universities. Despite concerns that low-SES students marginally induced to attend these institutions may not succeed, our findings indicate substantial and lasting improvements in both educational and labor market outcomes, leading to improved economic mobility and upper-tail mobility.

Lastly, our findings contribute to the literature on the long-term impact of financial aid on educational attainment and earnings. While prior studies generally find that financial aid enhances college completion rates (Nguyen et al., 2019), evidence of its effect on post-college earnings is limited and mixed (Dynarski et al., 2022). Some studies indicate positive effects (Bettinger et al., 2019; Black et al., 2023b; Denning et al., 2019), while many others yield imprecise or null results (Barr et al., 2021; Bucarey et al., 2020; Chu and Cuffe, 2021; Eng and Matsudaira, 2021; Gurantz, 2022; Scott-Clayton and Zafar, 2019). This ambiguity arises partly because financial aid is often used to attend lower-quality institutions, with no program consistently improving college quality.[3] We study the first financial aid initiative explicitly designed to propel low-SES students into higher-quality universities. We find substantial earnings gains, the

[3] For instance, U.S. federal aid policies lack conditions related to institutional quality and often support students attending community/technical colleges and for-profit institutions. Moreover, state-level programs often encourage enrollment in in-state public institutions, which are not consistently better than the counterfactual colleges (e.g., Bettinger et al., 2019; Cohodes and Goodman, 2014). In developing countries, financial aid has yet to result in quality upgrading and improved earnings, with marginal institutions offering limited labor market returns (Bucarey et al., 2020).

largest reported in the existing literature.[4] Additionally, we highlight impacts on both equity and efficiency, aspects often overlooked in existing literature. These aspects are crucial as large-scale programs boosting college enrollment for one group may inadvertently limit access for others. Our research demonstrates non-negative impacts on the entire population of high school graduates, with the program successfully enhancing opportunities for all. This model of reform could potentially serve as a guide for other countries.

Why are the gains from the Colombian program so large? First, recipients were induced to attend universities with superior career prospects. Second, over two-thirds of the increase in bachelor's attainment occurs in STEM-related fields, known for their substantial wage returns (Hastings et al., 2015; Kirkeboen et al., 2016; Riehl et al., 2018). Lastly, whether policies that diversify elite universities ultimately diversify top earners hinges on academic match and social integration (Arcidiacono and Lovenheim, 2016; Michelman et al., 2021). In our setting, the use of exam-based admissions by colleges ensures an academic match and promotes cross-class social integration, as students naturally build friendships with peers of similar ability (Carrell et al., 2013). Furthermore, the scale of the Colombian policy, which significantly increases the representation of low-SES students within elite college populations, has the potential to magnify observed cross-class social interactions (Londoño-Vélez, 2022; Álvarez Rivadulla et al., 2022; Velasco, 2023), benefiting low-SES students and mitigating inequality.

2 Higher Education in Colombia

Colombia has around 300 higher education institutions, encompassing professional technical institutions, technological institutions, technological schools, university institutions, and universities. For simplicity, we collectively refer to all these institutions as "colleges." In Colombia, colleges provide "short-cycle" programs lasting two or three years, categorized as "technical and technological," and "professional"

[4] For instance, consider the study by Bettinger et al. (2019), which examined students near the eligibility cutoffs for California's Cal Grant based on GPA and family income. While they found positive effects for students near the GPA cutoff and null effects for those near the income cutoff, we demonstrate consistently positive, significant, and large impacts for both margins. Our reduced-form RD estimate on log earnings at the poverty cutoff is 24%, starkly contrasting with Bettinger et al. (2019)'s -2%. Even at the test score cutoff, where our effect size is smaller, it remains about 1.5 to 2 times larger than the effect sizes found in Bettinger et al. (2019), Black et al. (2023a), and Denning et al. (2019).

programs, which span four or five years, akin to associate and bachelor's degree programs in the United States.

Programs and colleges in Colombia vary in terms of selectivity, quality, and prices. Unlike in the U.S., undergraduate admissions in Colombia primarily rely on students' performance in the national standardized high school exit exam, known as SABER 11. This exam assesses knowledge in subjects like mathematics, physics, chemistry, biology, language, philosophy, social science, and English. Almost 90% of high school seniors take this exam, regardless of their intention to apply to college. When applying to colleges, students indicate their preferred college-major combination, and admissions are decentralized and occur twice a year due to the different academic calendars followed by high schools. Around 85% of students begin college in the spring term, while the remaining 15%, primarily from elite private high schools, start in the fall.

Since 2010, a distinctive aspect of the Colombian higher education system is the mandatory standardized exam for undergraduate students upon graduation (Law 1324/2009). This exam provides insights into the educational value-added by individual colleges, a practice not widely adopted in other countries (OECD, 2016). For bachelor's degree seekers who have completed at least 75% of their academic credits, the exam is SABER PRO. Prior to 2016, students pursuing associate degrees took SABER PRO, while after 2016, they took a separate exam called SABER T&T. These exams evaluate generic competencies such as writing, critical reading, quantitative reasoning, English, and citizenship, as well as program-specific skills. Students achieving the highest scores in the program-specific component receive academic distinctions (Busso et al., 2022).

To recruit students, all programs and colleges in Colombia must meet the Ministry of Education's "Qualified Registry" standards and renew this status every seven years. Additionally, colleges have the option to undergo a peer review proceess to obtain "High-Quality Accreditation" (HQA). HQA aims to foster continuous self-evaluation, self-regulation, and improvement of institutions and programs (OECD, 2016).[5] Programs offered by colleges with HQA automatically receive HQA, while colleges without HQA can still have individual programs that achieve HQA. However, by 2014, shows that only 9% of programs and 12% of colleges achieved HQA (OECD, 2016). Among the 43 colleges with HQA, 33 were universities, while the remaining

[5] HQA is granted by the National Accreditation Council, composed of members from the academic and scientific community, and lasts for three to ten years, requiring re-accreditation thereafter.

10 were non-university institutions. For convenience, we refer to these 33 universities with HQA as HQ colleges, while all other colleges are referred to as LQ colleges.

Table A.1 reports key descriptive statistics for different college types. Among the 33 HQ colleges, 13 are public, and 20 are private. Together, they account for about one-fifth of total college enrollment. Students enrolled in HQ colleges exhibit meaningfully higher entry and exit test scores in comparison to those attending LQ colleges. Furthermore, HQ colleges demonstrate superior graduation rates and a higher percentage of faculty members with a Ph.D. However, this comes at a higher expense: HQ colleges are about twice as costly as LQ institutions, even though public colleges offer discounted fees due to substantial government subsidies.

Colombia's student loan and grant programs have lagged behind those of other OECD countries (OECD, 2016), and private colleges offer limited financial aid. This creates significant challenges for low-income students pursuing higher education, as financial resources play a crucial role in accessing higher education in Colombia (Riehl et al., 2018). High-achieving students with financial means can afford HQ private colleges, while entrance to competitive and affordable HQ public colleges is limited to a small exceptional minority. Consequently, most low-income students either attend LQ colleges or encounter barriers accessing higher education altogether (Ferreyra et al., 2017). This sorting based on financial capacity results in misallocation of talent and socioeconomic segregation within higher education, where educational opportunities and future prospects are heavily influenced by financial circumstances. This perpetuates inequality and hampers economic mobility.

The SPP Financial Aid Program

On October 1, 2014, the Colombian government announced "Ser Pilo Paga," a merit-based financial aid program aimed at low-SES students. SPP was a publicly funded student loan program that fully covered the tuition fees for four- or five-year bachelor's dgree programs at any of Colombia's 33 HQ colleges. The government directly paid the tuition fees for each SPP beneficiary to their chosen university. Additionally, beneficiaries received a modest stipend every six months, equivalent to one monthly minimum wage or about US$40 per month. If the student relocated to a different metropolitan area to attend college, the stipend increased to four minimum wages. Crucially, SPP included an incentive component where the loan was automatically forgiven upon graduation.

SPP incorporates both merit- and need-based criteria for eligibility, requiring applicants to meet three conditions. Firstly, they must achieve a minimum score of 310 out of 500 in the August 2014 SABER 11 exam, placing them in the top 9.5% of test scores (see Figure I, Panel A). Secondly, applicants must come from economically disadvantaged households, assessed through the government's SISBEN proxy-means testing instrument. SISBEN assigns households a wealth score from 0 to 100 based on factors like housing quality and possession of durable goods. The applicant's SISBEN index must fall below a cutoff varying by geographic location: 57.21 in the 14 main metropolitan areas; 56.32 in other urban areas; and 40.75 in rural areas (Figure I, Panel B).[6] Around 52.8% of test takers qualify based on their SISBEN score, indicating a greater emphasis on merit over need in SPP. Henceforth, we interchangeably use the terms "SISBEN-eligible students" and "low-SES students" to describe those meeting SPP eligibility based on their SISBEN score, and "SISBEN-ineligible students" or "high-SES students" for all others. Thirdly, applicants must secure admission to an HQ university; SPP does not alter the college admissions process for aid beneficiaries.

High-SES students tend to achieve higher SABER 11 scores than low-SES students, as shown by Figure A.1. Among merit-eligible students, 71.4% are high SES while 28.6% are low SES. Conversely, among low-SES students, approximately 15,000 students, which translates to about one in twenty, also meet the test score requirement, compared to one in seven for high-SES students. Despite the performance gap by socioeconomic background, there are nearly twice as many high-achieving students from low-SES backgrounds in Colombia than Chetty et al. (2020) found in the U.S.

Importantly, the announcement of SPP came as a surprise nearly two months after students had taken the SABER 11 exam. Eligibility for SPP was determined based on test scores received before the application deadlines of most colleges. This prevented students from manipulating their scores or wealth index to become eligible for SPP, supporting our assumption of quasi-random assignment near the eligibility cutoffs, which we validate in Section 4.1. The program benefited approximately 40,000 students between 2014 and 2018. Additionally, a widespread government advertising campaign contributed to SPP becoming one of Colombia's most popular social programs.

[6] SPP's SISBEN cutoffs align with the eligibility criteria of other social programs, such as the "Familias en Acción" cash transfers and humanitarian aid for victims of Colombia's armed conflict.

3 Data

We use administrative data from six main sources:

1. The population of SABER 11 test takers from the *Instituto Colombiano para el Fomento de la Educación Superior* (ICFES), the institution in charge of standardized testing in Colombia. These data contain test scores and sociodemographic information (e.g., socioeconomic status, parental education, sex) and cover the fall semesters of 2012, 2013, and 2014, capturing both pre- and post-expansion of financial aid.

2. The universe of households from DNP's *Sistema de Identificación de Potenciales Beneficiarios de Programas Sociales* (SISBEN) from 2012 to 2014.

3. The population of program beneficiaries from SPP from ICETEX, the institution that manages all student loans and grant aid for postbaccalaureate programs. These data allow us to identify program beneficiaries, quantify the program's cost, and observe dropouts' loan repayment behavior.

Together, these three sources allow us to identify the eligible population and program recipients. The following three sources enable us to measure key outcomes of interest:

4. The Ministry of Education's *Sistema Nacional de Información de la Educación Superior* (SNIES) tracks students in the postsecondary education system and provides student-by-semester level information on enrollment status, institution and type of program attended (e.g., associate, bachelor's, graduate), field of study, academic performance (credits and courses passed), persistence, and degree completion. We have SNIES microdata from 2013 to 2020. We complement this dataset with information from institutional financial accounts and balance sheets reported by colleges to Colombia's Ministry of Education, providing institution-by-year reports on educational expenditures per full-time student.

5. The population of college graduation test takers from ICFES. It includes information from SABER PRO from 2013 to 2021 and SABER T&T from 2016 to 2021. Since 2016, SABER PRO is offered annually while SABER T&T is offered each semester, specifically for students in associate degree programs. Both exams consist of five generic competency tests (writing, critical reading,

quantitative reasoning, English, and citizenship competencies) and field-specific components related to the students' majors (e.g., economics, biology). The scores obtained in the five generic modules were summed and standardized to have a mean of zero and standard deviation of one for students taking the test in 2016. The test scores are comparable between 2013 and 2021.

6. Social security records from Colombia's Ministry of Health and Social Protection's *Planilla Integrada de Liquidación de Aportes* (PILA). It provides a comprehensive record of individual-by-month contributions to healthcare, pension funds, and workers' compensations. It includes detailed information on payroll, earnings, days worked, and employer characteristics (e.g., firm size, sector, location) for all formal workers in Colombia. However, it does not capture earnings for informal workers; the implications for our analysis are discussed in Section 4.5 The dataset covers April, August, and December from 2013 to 2022.

Out of the 574,259 individuals who took the SABER 11 exam in August 2014, we exclude approximately 11,000 individuals (2% of test takers) who had previous college experience before retaking the exam. Our main analysis focuses on the remaining sample of 563,027 individuals. Among these individuals, 297,279 (52.8%) qualify for SPP based on their SISBEN score, while 53,636 (9.5%) qualify for SPP based on their SABER 11 score.

4 Impacts on Recipients

4.1 RD Design and Validity

To estimate the causal effects on program recipients, we utilize an RD design by leveraging the SABER 11 and SISBEN cutoffs. While applicants must meet both need- and merit-based criteria and obtain admission to an HQ university to receive SPP, we focus solely on eligibility determined by test scores and households' poverty index to avoid potential biases caused by students expecting financial aid and adjusting their college application decisions.

This multidimensional RD setting allows us to identify two types of compliers: (1) need-eligible students near the test score cutoff and (2) merit-eligible students near the wealth cutoff (Figure A.2). We report separate estimates by collapsing the discontinuity into a single dimension for each student. This is achieved by measuring the distance of SABER 11 (SISBEN) scores from the eligibility cutoff, based on their SISBEN- (SABER 11-) eligibility status. We adopt this univariate approach instead of calculating a weighted average of the two RD effects because the two discontinuities pertain to different student populations who, as we will demonstrate, respond differently to the policy.

Indeed, Table A.2 shows that the RD design, employing the *test score* as the running variable, compares students who score around the 90th percentile of the test score distribution. These students generally have a lower SES, with control group students at the 31st percentile of Colombia's wealth distribution. In contrast, the RD design using the *wealth index* as the running variable compares students around the 53rd percentile of Colombia's wealth distribution. These students demonstrate higher SES, with smaller families, more educated parents, attendance at private full-day high schools, and urban residences. Moreover, this group performs exceptionally well on the exam, with the control group scoring above the 95th percentile. Considering the higher SES and academic performance of the latter population, they are likely to encounter fewer financial constraints and have a higher chance of attending an HQ college without financial aid. Consequently, we can anticipate that the impact of financial aid on college attendance and quality may be more pronounced for the former population than for the latter.

Our primary RD analysis focuses on students who took the high school exit exam in the fall semester of 2014. This cohort provides the highest internal validity as they were informed about the financial aid program and the strict eligibility cutoffs *after* completing the SABER 11 exam in 2014, mitigating concerns about non-random sorting based on test scores. In contrast, students in subsequent years reacted to the prospect of receiving aid by putting in more effort during standardized exams, as shown by Laajaj et al. (2022) and Bernal and Penney (2019). Additionally, younger cohorts have more time to request an evaluation from local authorities for inclusion in SISBEN, which could introduce non-random sorting based on the need criterion.

We use a data-driven approach to select the optimal bandwidth using the 'rdrobust' package by Cattaneo et al. (2014). Notwithstanding, Appendix B shows that the estimated RD coefficient and 95% confidence intervals are stable across smaller and larger bandwidth choices for all of our main outcomes of interest. Moreover, our analysis provides support for our identifying assumption of no manipulation of SABER 11 or SISBEN for fall 2014 test takers. The histograms in Figure I indicate no apparent manipulation of these variables. Furthermore, we conduct a formal test for manipulation using the local polynomial density estimator proposed by Cattaneo et al. (2018, 2020). The resulting robust-corrected p-values are 0.823 when using SABER 11 as the running variable, R_i, and 0.413 when using SISBEN as the running variable (Figure A.3). These results confirm that there is no statistical evidence of systematic manipulation of the running variable. Additionally, Table A.2 shows that we cannot reject the joint null hypothesis of balance in covariates around the two discontinuities.[7]

Figure A.4 presents the likelihood of receiving SPP based on the SABER 11 score (for SISBEN-eligible individuals) in Panel A, and the SISBEN score (for SABER 11-eligible individuals) in Panel B. The eligibility criteria were stringent, resulting in only a few individuals below the cutoffs receiving SPP. Moreover, the program had a high take-up rate: 58.3% at the test score cutoff and 64.5% at the wealth cutoff. The higher take-up rate at the wealth cutoff is consistent with the complier population, which has higher SES and test scores, being more likely to apply and receive admission from an HQ university. Nevertheless, there is incomplete take-up due to reasons such as students not applying or being admitted to an HQ university that semester, or not applying to the SPP program.

4.2 Impacts on College Attendance and Quality

We begin by examining the impact of the SPP policy on having any college attendance in the six years after high school. Figure II compares low-SES students above and below the test score cutoff. Test scores predict college attendance. For instance, Panel A shows that a student scoring 40 points above the cutoff (98th percentile) has over 50 p.p. higher enrollment compared to a student scoring 40 points below (71st percentile). Financial aid eligibility increases immediate postsecondary enrollment by 28.7 p.p., from 41.4% among control students to 70.1%, a 69.5% increase.[8] Panel

[7] When using SABER 11 as the running variable, we cannot reject the null hypothesis of no statistical difference for all but three of the 40 baseline characteristics. Similarly, when using SISBEN as the running variable, we cannot reject the null hypothesis for 28 of the 40 baseline characteristics. There is less balance at the SISBEN cutoff since, as mentioned in footnote 6, it coincides with those used by other social programs.

[8] In Londoño-Vélez et al. (2020), which focused on the immediate enrollment effects of the SPP program, we utilized data from SPADIES to track throughout their postsecondary education journey and calculated a 32 p.p. impact. In contrast, the current study, based on SNIES data, estimates a 28.7 p.p. impact. The discrepancy between these two estimates can be attributed to the fact that control group students are more likely to enroll in SENA, Colombia's largest vocational training institution, which is included in SNIES but not in SPADIES.

B shows that the proportion of control students ever attending any college has risen from 41.4% immediately after high school to 77.3% six years later. As a result, the overall enrollment effect is 9.6 p.p. (or 12.4%; see Table I).

Importantly, the program substantially improves the quality of the institutions that students attend. Figure III displays the distribution of college attendance between HQ and LQ colleges. Approximately three-quarters of control group students choose LQ institutions. However, the policy redirects students away from these colleges and towards HQ colleges. Consequently, six years after high school, marginally-eligible students are 43.6 p.p. (241%) more likely to have attended an HQ college. Thus, there is a lasting improvement in college quality as students shift from no college or low-quality education to high-quality educational opportunities. Subsequent sections will provide further evidence of the "value-added" of HQ colleges in terms of skill development and job placement, leading to improved labor market outcomes.

Two key pieces of evidence support the claim that credit market imperfections hindered investments in human capital. Firstly, Figure A.5 shows that the policy has a stronger impact on HQ enrollment for students from the poorest socioeconomic stratum compared to their less economically disadvantaged counterparts. Secondly, there is a significant difference in the effect on college attendance and quality between students close to the test score cutoff and those near the wealth cutoff: the policy increases college enrollment by only 4.9 p.p. (5.8%) for the latter group and HQ enrollment by 35.7 p.p. or 99% (Figures A.6 and A.7 and Table I).

Program recipients strongly favor *private* HQ colleges, with eligibility raising attendance at these institutions by 47 p.p. for both cutoffs (Table I and Figure A.8). However, the effect on college choice varies between the two groups of compliers. Near the test score cutoff, there is a larger shift away from no college and LQ colleges. Conversely, near the wealth cutoff, where students have higher test scores and SES, there is a greater shift away from the highly selective HQ *public* colleges, where they were twice as likely to attend compared to students near the test score cutoff.

The policy also shifts students from associate's degree programs toward bachelor's degree programs. At the test score cutoff, attendance at bachelor's degree programs increases by 21.2 p.p. (37.6%) and attendance at associate's degree prorams decreases by 12.1 p.p. (56.3%). For students at the wealth cutoff, who have higher test scores and SES, the likelihood of attending bachelor's degree programs is already higher. However, the policy aid further increases attendance in this type of program by 14.5 p.p. (Figures A.9 and A.10 and Table I).

4.3 Impacts on College Attainment

We now examine impacts on degree attainment, addressing concerns that broadened HQ attendance may not lead to higher graduation rates if low-SES high-achievers face challenges that lead them to drop out of college.

Figure IV examines the probability of obtaining a bachelor's degree within seven years after high school, using the SABER PRO college graduation exam as a proxy. Panel A illustrates the strong association between test scores and degree attainment. Low-SES students scoring 40 points above the cutoff (98th percentile) are 28 p.p. more likely to earn a bachelor's degree compared to those just below the cutoff (90th percentile), and nearly 44 p.p. more likely compared to those scoring 40 points below the cutoff (71st percentile). Moreover, program eligibility increases the likelihood of attaining a bachelor's degree by 15.6 p.p., representing a 38.8% increase relative to the control group (Table II).[9] The instrumental variable (IV) estimate, obtained by scaling the reduced-form coefficient by the take-up rate (58.3%), indicates an 26.8 p.p. improvement in bachelor's degree attainment or 66.5% relative to the control group (Table III).

Panel B of Figure IV offers a placebo test by comparing low-SES students who took the high school exit exam in the fall semesters of 2012 and 2013, before the policy implementation (shown in black). These students have an equal likelihood of earning a bachelor's degree compared to those from 2014 (shown in red) if they score below the test score cutoff. Additionally, their probability of earning a bachelor's degree remains constant at the threshold. The RD coefficient is almost zero and statistically insignificant with the p-value is 0.841, providing further evidence that the policy *caused* improved degree attainment.

In addition, the figure sheds light on equity by comparing the series against high-SES students, who do not qualify for SPP because either they lack a SISBEN score or their score exceeds SPP's cutoff. Reflecting their higher SES, these students are approximately ten p.p. more likely to earn a bachelor's degree compared to low-SES students prior to the policy rollout (shown in gray and black, respectively). This disparity persists across all ranges of test scores. The policy does not affect B.A. attainment for high-SES students (shown in blue), while significantly enhancing outcomes for low-SES students (shown in red). As a result, the policy eliminates the SES gap in B.A. attainment among equally-achieving students.

[9] The effect remains consistent across different bandwidth choices (Figure B.7).

Interestingly, despite financial aid having a greater impact on college *access* for students near the test score cutoff (with lower performance and lower SES) compared to students near the wealth cutoff (with higher performance and higher SES), the effect on college *attainment* is remarkably similar. This reflects the fact that students with higher test scores and SES are less likely to drop out from college (Figure A.11). For this group, the reduced-form RD estimate indicates a 14.5 p.p. increase in B.A. attainment and the IV estimate is 22.5 p.p. (Tables II and III, and Figure A.12).

More than half of the increase in B.A. attainment is concentrated in STEM fields (science, technology, engineering, mathematics, and medicine). This outcome is not ex-ante obvious since the program covered tuition for all majors, allowing students the freedom to choose their field of study. At the test score cutoff, the probability of earning a STEM degree increased by 8.8 p.p. (64.3%). Including STEM-related majors like Architecture, Business, Economics, and Psychology (referred to as "STEM-Plus" in Table II), the effect increases to 12.4 p.p. (41.2%), or over four-fifths of the attainment gains. Additionally, there is a 3.1 p.p. (60.8%) increase in the likelihood of earning a degree in social sciences and humanities, and a 1.5 p.p. (327.5%) increase in the likelihood of earning an art degree.[10] Near the wealth cutoff, where students perform better academically and come from higher socioeconomic backgrounds, the likelihood of earning a STEM degree is nearly double and 70% of the increase in bachelor's degree attainment is observed in the STEM-Plus fields. Crucially, STEM and STEM-related fields have the highest labor market returns in our data.

The program significantly boosts degree attainment from HQ colleges, with the reduced-form and IV coefficients at 32.2 p.p. (330%) and 55.2 p.p. (566%), respectively. This effect holds across all test score ranges, including top 2% scores (Figure A.13). The increase is mainly driven by HQ *private* colleges, where the probability of earning a degree rises by 34.3 p.p. (1094%) at the test score cutoff and 39.0 p.p. (534%) at the wealth cutoff, with a slight opting out from HQ *public* colleges (Table II and Figure A.14). Conversely, the program decreases graduation from LQ colleges by 16.1 p.p. (-53.0%) and from short-cycle programs by 10.1 p.p. (-54.8%) as recipients shift away from these institutions and programs. However, there is an overall increase in the likelihood of earning *any* degree, with a 6.2 p.p. (10.6%) increase at the test score cutoff and a 7.7 p.p. (11.6%) increase at the wealth cutoff (Table II).

[10] Some LQ colleges do not report students' field of study, leading to a reduction in the likelihood of earning a bachelor's degree with a missing field of study (Column 14 of Table II).

Table IV shows impacts on additional educational outcomes. For example, the likelihood of pursuing graduate studies increased. While graduate education is rare in our data (only 0.8% of control students at the test score cutoff attend graduate studies within six years of completing high school), program eligibility increases this likelihood by 0.5 p.p. (61.6%). For merit-eligible students at the wealth cutoff, who have higher test scores and higher SES, this increase is three times larger at 1.6 p.p. (106%).[11]

4.4 Impacts on Skill Development

Having shown the program's impact on college attendance and quality, we now examine its effect on students' skill development as measured by Colombia's mandatory college graduation exam. We analyze the five generic competency tests taken by all students in all majors, which are comparable to the SABER 11 exam. These scores are commonly used by the Colombian government and researchers to assess college learning outcomes.[12] We specifically focus on bachelor's program students and their SABER PRO scores, as the standardized testing institution, ICFES, advises against comparing scores from SABER PRO and SABER T&T.

We begin by analyzing impacts on exam scores taken within five years of high school completion, which aligns with the timing for the majority of SPP recipients and the average student in Colombia. Figure V plots college test scores as a function of the distance to the SABER 11 cutoff for low-SES students. Notably, *high school* scores strongly correlate with *college* scores. Program eligibility further improves learning performance, with a reduced-form RD coefficient of 9.6% of a standard deviation (22.7% compared to the control group) and an IV estimate of 11.9% of a standard deviation (28.2% compared to the control group).[13]

Panel B highlights impacts on equity. Before the implementation of SPP, there

[11] Furthermore, by expanding college attendance, persistence, and program duration, the total number of years students attended an undergraduate program increased by 0.51 to 0.76 years, or 13.2% to 22.8%, depending on the complier population. Despite this, financial aid reduces the time to graduation by 0.13 to 0.19 years (2.4% to 3.6%), partially due to students choosing *private* HQ colleges, which Table A.1 showed offer shorter bachelor's degree programs.

[12] Our focus on the generic component means that teaching to the test is not a concern because students compete for an academic distinction based on the program-specific component.

[13] Control students take longer to access college, leading to a delayed completion of the college exit exam. Figure A.15 includes all exams taken within seven years of high school completion. The reduced form estimate shows a significant and economically meaningful 12.4% improvement, while the IV estimate indicates a 17% improvement. These effects hold consistently across different RD bandwidth choices (Figures B.12 and B.13).

is an SES gap in skill development in college. Despite equal performance in high school, high-SES students outperform low-SES students (shown in gray and black, respectively), with a gap of at least 5% of a standard deviation just below the test score cutoff. Moreover, this gap persists across the entire test score distribution and widens for the top 5% of high school test scores. Section 4.7 will demonstrate that this disparity is attributed to the SES gap in college "value-added." However, barely-eligible low-SES students display higher test scores after the policy implementation, eliminating the learning gap between low- and high-SES students (shown in red and blue, respectively).

Merit-eligible students near the wealth cutoff, who have higher test scores and higher SES, achieve significantly higher scores on the college exit exam. The control group performs almost twice as well as the control group below the test score cutoff (Table III). Despite their already high performance, they still display improved learning. The IV coefficient is 5.9% of a standard deviation for exams taken within five years. Similarly, Panel B of Figures A.16 and A.17 shows that aid-eligible students outperform comparable students before the expansion of financial aid, indicating greater learning during college. However, the estimates at this particular margin have lower precision due to the smaller sample size, and we cannot reject the null hypothesis of no effect.

The college graduation exam is typically taken by graduating students, which means we lack test scores for those who dropped out before completing their degree. However, the program boosted degree completion among potentially at-risk students. As a result, the observable characteristics of students who take the exam and are located above SPP's cutoff differ from those below the cutoff.[14] Specifically, low-SES test takers above the test score cutoff are more likely to have attended a public, rural high school. If these at-risk students induced to graduate tend to perform worse on the college graduation exam, which is highly plausible, the RD approach underestimates student learning. We revisit this issue in Section 4.7.[15]

[14] Joint significance tests around the eligibility cutoffs using all baseline characteristics reject the null, supporting this conclusion (Table A.3).

[15] However, the direction of the bias is less clear at the wealth cutoff due to some existing covariate imbalance, as shown in Table A.2 and discussed in footnote 7.

4.5 Impacts on Labor Market Outcomes

Having shown improved long-term educational outcomes, we now analyze impacts on early-career labor market outcomes. We start by examining effects on formal monthly earnings, measured in multiples of Colombia's monthly minimum wage for full-time workers. Individuals who are not formally employed receive zero formal earnings. Panel A of Figure VI displays these earnings eight years after high school, plotted against the distance to the test score cutoff for low-SES students. The graph illustrates a strong correlation between test scores and formal earnings: a low-SES student scoring in the top 2% of test scores earns twice as much as a student scoring in the 71st percentile. Program eligibility increases earnings by 20.6% of a monthly minimum wage, or US$46.62 more per month, corresponding to a 20.9% rise compared to the control group mean. The IV estimate indicates an increase of US$79.90 more per month, or 35.7%. Notably, this effect does not solely stem from increased formal employment, which we describe below, as the IV coefficient on log earnings is also positive, substantial, and statistically significant (Table III).

In addition, Table V sheds light on the impacts on students' earnings relative to their peers of the same age. To explore this aspect, we rank individuals who took the SABER 11 exam in the fall of 2014 based on their formal monthly earnings. Notably, barely-eligible low-SES students rank about 4.5 percentiles higher in earnings. They are also 6.2 p.p. (18%) more likely to belong to the top quartile of the earnings distribution and twice as likely to belong to the top 1%. This signifies a substantial boost in upper-tail mobility.

Panel B of Figure VI examines the dynamics of earnings effect. Earnings generally decline while students are more likely to be in college in years one to four after high school but marginally increase six years after high school (the reduced-form coefficient is 3.9% of the minimum wage with a p-value of 0.058). The earnings gap between aid-eligible and aid-ineligible students widens seven years after high school, with the former group experiencing significantly higher earnings. This gain surpasses the temporary earnings reduction observed earlier and continues to grow in the eighth year, aligning with findings from MacLeod et al. (2017) and Zimmerman (2014) documenting that the return to attending more selective and prestigious colleges increases with experience. Section 4.7 will demonstrate how the program boosts earnings by encouraging students to attend colleges and programs that offer a substantial earnings premium.

Figure VII assesses equity impacts by comparing low-SES and high-SES students before and after the reform. Pre-policy, high-SES students (in gray) consistently earned more across the test score distribution, with approximately double the likelihood of reaching the (within-cohort) top 1% of earnings compared to low-SES students (in black). Additionally, test scores were more predictive of future earnings for high-SES. The policy narrowed the earnings gap between low- and high-SES students, along with the disparity in the probability of reaching the top 1% (in red and blue, respectively), indicating improved equity.[16,17] Moreover, for low-SES students, the gradient between test scores and earnings more closely resembles that of high-SES students post-policy. In Section 4.7, we will show that this can be attributed to low-SES students facing financial barriers preventing them from attending elite colleges with higher "value-added," resulting in lower earnings. Financial aid plays a crucial role in leveling opportunities for accessing quality education and mitigating these socioeconomic gaps.

Moreover, our findings from Section 4.4 suggest that the earnings gains partly result from enhanced skills, not solely from signaling effects. Based on a simple cross-sectional analysis using pre-reform data, a one standard deviation increase in the college test score is associated with an earnings boost of 45.7% of the monthly minimum wage eight years after high school for students who took the exam within five years from high school. Consequently, an improvement of 11.9% of a standard deviation in the college graduation exam would imply recipients earning 5.4% more of the monthly minimum wage. Comparing this to the actual IV estimate of 35.4%, 15.4% of the observed impact on earnings would be associated with better skills. However, this estimate should be taken with a grain of salt; it is not causally identified.

Recipients display improved rates of formal employment in a country where one in five individuals aged 15 to 24 are unemployed, according to SEDLAC (CEDLAS and The World Bank). Initially, in the first four years after high school, aid eligibility reduces formal employment due to higher college attendance (Figure A.19). However, as time progresses, recipients complete their degrees and enter the labor force. The RD coefficient becomes non-significant six years after high school, consistent with

[16] When focusing on earnings, we restrict the comparison group to the 2013 cohort because the COVID-19 pandemic impacts the earnings of the 2012 cohort eight years after high school in 2020. However, Figure A.18 shows similar results using both the 2012 and 2013 cohorts as the comparison group.

[17] Few low-SES students reach the top 1% earnings before the policy, causing noisy estimates. While there would appear to be a jump at the cutoff (depicted in black), it is not statistically significant: the p-value is 0.155.

equal college attendance likelihood for aid-eligible and aid-ineligible students (Figure A.20). Furthermore, seven and eight years after high school, barely-eligible low-SES students have *higher* employment rates than barely-ineligible students. The IV coefficient in Table III indicates a 6.9 p.p. (11.6%) improvement in formal employment eight years after high school.

Compared to need-eligible students near the test score cutoff, merit-eligible students near the wealth cutoff, who have higher test scores and higher SES, earn higher salaries. However, beneficiaries still experience an increase in formal earnings of 34.2% of a minimum wage eight years after high school, statistically indistinguishable from the effect for the former group of compliers (Table III).[18] Interestingly, the earnings gain for this population is driven by improvements in daily wages, with no discernible effect on employment. The coefficient on log earnings is approximately twice as large compared to those near the test score cutoff.[19]

Our focus on formal labor market outcomes means that individuals engaged in informal work are recorded as having zero formal earnings, potentially leading us to overestimate the effect if the control group is more likely to work informally. However, several factors mitigate this concern. Firstly, informality is less common in our study population, as both treated and control groups have achieved some level of college education. The informality rate for workers with more than 13 years of education is only 7.9% (CEDLAS and The World Bank), and a high percentage of control students at the test score and wealth cutoffs have at least 13 years of education: 77% and 85%, respectively (Table I). Secondly, our study sample is highly-achieving students, and students with high test scores are more likely to work formally. Thirdly, even among formally-employed individuals, the policy still had a significant and substantial increase in earnings.

We expect the earnings effect to further increase in the coming years for several reasons. Firstly, many graduate students have zero formal earnings during their studies, but often earn high salaries upon graduation. As financial aid has increased the likelihood of pursuing graduate studies, we anticipate that the earnings effect will

[18] Similarly, merit-eligible students near the wealth cutoff also experience a temporary employment decrease in the four years after high school, but this effect diminishes and becomes statistically insignificant thereafter (Figure A.21). Again, the earnings gains increase over time (Figure A.22).

[19] Additionally, program eligibility reduces the time it takes to secure the first formal job after college graduation by 11.6% to 13.3% (Table V). It also has a minor positive influence on the number of days worked and the probability of employment in Colombia's 13 largest cities, where most HQ colleges are located. However, there is no significant impact on firm size or sector, except for a higher likelihood of working in information and communication jobs (Tables A.4 and A.5).

rise once program recipients complete their graduate degrees, given the high returns associated with graduate education. Secondly, graduates with STEM majors typically earn higher salaries and experience steeper earnings growth in Colombia (Bayona and Sanchez, 2023). Thirdly, as we mentioned earlier, the return to attending more selective and prestigious colleges tends to increase with experience (MacLeod et al., 2017; Zimmerman, 2014).

4.6 Outcome Heterogeneity by Baseline Characteristics

In Appendix C, we investigate the heterogeneous treatment effects on students' educational and labor market outcomes. The policy consistently leads to positive and significant gains across all baseline characteristics. Notably, the largest impacts are observed among students from disadvantaged high schools with low average test scores and limited transitions to HQ colleges. Furthermore, financial aid disproportionately benefits female students in terms of college graduation from HQ colleges. However, females often choose fields with lower returns, such as social sciences and humanities, leading to similar earnings gains compared to males. Lastly, first-generation college students benefit from financial aid to the same extent as students with college-educated parents.

4.7 Mechanisms

In this section, our objective is to assess the extent to which recipients' educational and labor market gains can be attributed to the colleges and programs they attended. Following a similar approach as Melguizo et al. (2017) and Riehl et al. (2018), we examine various dimensions of colleges' and programs' "value-added" by considering graduation rates, skill development, and labor market outcomes. We analyze these dimensions separately, as different college-program combinations may excel in specific outcomes. For example, some college-program combinations may be more effective in graduating students from their programs, while others may teach more skills, and yet others may add most to students' earnings.

We exploit the fact that Colombian students apply to specific college-program combinations when seeking higher education. These combinations have varying levels of selectivity and primarily consider students' SABER 11 test scores. This variation allows us to estimate the contributions of individual college-program pairs to students' outcomes. Appendix D provides more detailed information on our empirical approach; here, we summarize the main steps. We utilize data from students who took the SABER 11 exam in the fall semesters of 2012 and 2013, before the implementation of the SPP policy, to estimate the "value-added" of college-program combinations. We regress each outcome on the college-program fixed effects and control for baseline ability, sociodemographic characteristics, student selection across programs, and peer cohort qualities using the following specification:

$$y_{i,t} = \alpha + \delta_{j(i,t)p(i,t)} + \mathbf{X_i}'\Gamma + \epsilon_{i,t} \tag{1}$$

where $y_{i,t}$ is the outcome y for individual i taking the SABER 11 exam in semester t, $\delta_{j(i,t)p(i,t)}$ are the college-program fixed effects based on the first college and program attended, $\mathbf{X}$ is a vector of baseline covariates, and $\epsilon_{i,t}$ is a student-specific error term.[20] The vector $\mathbf{X}$ includes relevant student sociodemographic information related to these outcomes of interest and capturing students' selection across colleges and programs. In particular, we first control for a student's SABER 11 score using a third-degree polynomial. Following Melguizo et al. (2017), we also include the leave-one-out mean SABER 11 score in the entering college and program cohort. Because colleges select students based mainly on their SABER 11 scores, these two measures enable controlling for a big part of the selection into colleges and programs. Additionally, we use a rich vector of baseline sociodemographic covariates correlated with the outcomes of interest and influencing students' selection across programs.[21] By controlling for these factors, we aim to isolate the contributions of colleges and programs independent of student and peer characteristics.

Appendix D provides detailed information on the estimated patterns and robustness checks; here, we briefly summarize the key findings. $\hat{\delta}_{jp}$ varies widely across different college types, and their ranking changes depending on the outcome of interest and the inclusion of baseline covariates, particularly students' SABER 11

[20] Our focus is on measuring the following five student outcomes $y_{i,t}$ observed seven or eight years after high school completion: (1) attainment of any degree, approximated using SABER PRO or SABER T&T exams, (2) attainment of a bachelor's degree, approximated using the SABER PRO exam, (3) SABER PRO test score, (4) formal employment status, and (5) formal monthly earnings, expressed as multiples of the monthly minimum wage.

[21] Specifically, we include student demographics (sex, ethnic minority, third-degree polynomials of age, and an indicator for the exam year), household characteristics (size, socioeconomic stratum, parental educational attainment, SISBEN score, and third-degree polynomials of distance to the college), and time-invariant high school characteristics (private indicator, calendar dummies, urban indicator). Additionally, we include leave-one-out mean socioeconomic stratum, parental education, and SABER 11 test scores at the high school-cohort level, as well as leave-one-out mean socioeconomic stratum, parental education, and SISBEN score at the college-program-cohort level.

test score. Without controlling for $\mathbf{X}$, HQ *private* colleges would appear to have the highest graduation "value-added," surpassing all other institutions (Table D.1). However, these colleges attract students with exceptionally high ability and privileged socioeconomic backgrounds. When we account for these observable differences, LQ colleges exhibit the highest graduation "value-added," while HQ *public* colleges display the lowest; HQ *private* colleges fall in between. In terms of teaching skills, HQ colleges excel, with HQ *private* institutions displaying the highest "value-added" in this aspect. Additionally, HQ *private* colleges outperform other types in providing higher-paying job opportunities, while HQ *public* colleges have the lowest earnings "value-added" compared to all other college types.[22]

Using the estimated college-program fixed effects, we assess the program's impact on the "value-added" of attended colleges and programs, treating the $\hat{\delta}_{jp}$s as outcome variables in the RD design. The results are presented in Table VI and Figure VIII. Column (1) of Table VI examines the impact on the likelihood of obtaining *any* college degree for enrolled students. Once we condition on enrollment, the increase in degree attainment may stem from students attending college-program combinations more likely to graduate their students or from the loan forgiveness policy encouraging graduation.[23] Column (2), reporting the RD coefficient on the graduation "value-added" (Figure D.3), reveals small effects, suggesting that changes in the type of colleges and programs pursued by students cannot explain the improved college completion. In fact, SPP directs students to programs in HQ colleges, which, as Column (2) shows, are more demanding than the counterfactual programs they would have pursued. Similarly, Columns (3) and (4), which analyze bachelor's degree attainment (Figure D.4), show that the observed increase in degree attainment surpasses the impact predicted by changes in colleges and programs. Hence, we posit that the SPP program's substantial incentives for degree completion are a key driving force behind the increase in graduation. (Notably, the impact on "value-added" plays a larger role for students near the wealth cutoff because they tend to shift from HQ *public* to *private* colleges, which have a higher graduation "value-added.")

Columns (5) and (6) assess the learning performance of students who took the SABER PRO exam within seven years of high school completion. Panel A of Figure

[22] Riehl et al. (2018) also find that Colombia's top public institutions are better in teaching skills than in job placement, while top private schools do relatively better on earning. They posit that students at top private colleges may benefit more from peer and alumni networks in the labor market.

[23] SPP may have also expanded graduation because the stipend reduces the opportunity cost of attending college, but this stipend is tiny compared to the sizable debt they must repay if they drop out of college.

VIII shows that, before the policy, students with higher test scores enrolled in colleges and programs that offer greater skill acquisition, but a large socioeconomic gap existed between low-SES and high-SES students. The SPP program successfully redirected students toward colleges and programs with higher learning "value-added," eliminating this gap. Table VI shows that this effect is especially notable for those near the test score cutoff, who were more inclined to attend the less productive LQ colleges. Notably, the observed learning effect is smaller than the predicted effect size due to the aforementioned selection of graduating students, since the policy motivated some at-risk students to complete their degree.

Columns (7) through (10) focus on impacts on employment and earnings "value-added." We observe these outcomes for all students, including those who never attended college or did attend but did not graduate, and express "value-added" relative to the average outcomes of students without college experience. Columns (7) and (8) reveal that the observed employment effects closely align with the predictions based on employment "value-added" (Figure D.6), with overlapping confidence intervals. This suggests that the policy effectively steers students towards colleges that enhance their employment prospects.

Crucially, Panel B of Figure VIII illustrates that, before the reform, students' SES strongly correlated with the earnings "value-added" of their chosen college-program combination. Remarkably, test scores improved earnings "value-added" for high-SES students *but not for low-SES students*. This disparity likely arises from the unaffordability of elite private colleges, known for better earning outcomes (as shown by Table D.1), for low-SES students. As indicated by Riehl et al. (2018), the earnings "value-added" of colleges strongly correlates with their tuition levels. However, the reform enables low-SES students to afford these colleges, thereby leveling the opportunity playing field. Interestingly, Table VI shows that the effect on realized earnings is nearly twice as large as that predicted by "value-added," indicating that targeted students experience treatment effects surpassing the average returns to those universities. Recipients seem to derive greater benefits from high-quality education.

5 Impacts on Nonrecipients

This section evaluates the overall effects by considering the program's impact on all high school test takers, regardless of their socioeconomic status or academic performance.

College admissions in Colombia heavily rely on test scores, and the SPP program specifically targets low-SES students in the top decile. This targeting may result in the displacement of high-SES students and those scoring below the top decile, who are considered *ineligible* for SPP. To address this, we analyze the outcomes of approximately 1.7 million students who took the high school graduation exam in the fall of 2012, 2013, and 2014, spanning the period before and after the policy. We employ a DD approach, comparing outcomes across the distribution of SABER 11 scores separately for low- and high-SES students using the following ordinary least squares (OLS) regression:

$$y_{idt} = \alpha + \gamma_t + \delta_d + \sum_{k=6, k \neq \{1,5\}}^{10} \beta_k \cdot 1(d = k) \times 1(t = 2014) + \epsilon_{idt} \tag{2}$$

where y_{idt} is the outcome for individual i taking SABER 11 in year t and scoring in decile d, γ_t are the year fixed effects, δ_d are the SABER 11 decile fixed effects (with deciles 1 through 5, unlikely admitted by HQ colleges, being the omitted category), and ϵ_{idt} is the individual-specific error term. The β_ks are the coefficients of interest and represent the difference in outcomes before and after the policy for aid-eligible and aid-ineligible students. Since students scoring in the top decile of test scores are eligible, β_{10} for low-SES students captures the direct effect of the policy. By contrast, β_{10} for high-SES students, and β_6 through β_9, capture the "spillover" effects for different groups. The identifying assumption of this DD specification is that the trends between the two groups would be similar in the absence of the policy. The absence of pre-trends using the 2013 as the placebo group supports this assumption (Figure A.23).

Figure IX plots the β_k coefficients and 95% confidence intervals from Specification (2) for college attendance outcomes. The markers in red and blue distinguish between low- and high-SES students, respectively. Panels A to D focus on immediate college enrollment right after high school, presenting effects separately for various college types. Panel E examines overall college enrollment within six years after high school completion. The DD results are consistent with the RD analysis: for low-SES students in the top test score decile, the policy expanded attendance at HQ *private* colleges (Panel B), slightly reduced it at HQ *public* colleges (Panel C), and significantly decreased it at LQ colleges (Panel D). Overall, the policy increased their likelihood of ever attending any college (Panel E).[24]

Importantly, the DD analysis reveals that the policy did not displace nonrecipients from college opportunities or reduce their college quality. As aid recipients chose HQ colleges over LQ ones, LQ colleges filled their vacancies with less qualified applicants, expanding enrollment for students below the top test score decile. Moreover, high-SES students were not displaced from HQ institutions.[25] This outcome can be attributed to the oversubscription of HQ *public* colleges, which could admit other high-achieving applicants into the vacant seats. Additionally, HQ *private* colleges, driven by tuition, increased available seats by about 50% while maintaining fees (Figures A.26 and A.27). Consequently, the educational quality, as indicated by college-program "value-added," also remained unaffected for students ineligible for SPP (Figure A.28).

Critics of expanding elite colleges often raise concerns about potential downsides, such as compromised instructional quality and diminished degree value. To address these concerns, we assess impacts on nonrecipients' bachelor's degree attainment, skill development, and labor market outcomes. Figures X and XI indicate no adverse effects on instructional quality. Nonrecipients' bachelor's degree attainment from HQ private colleges and skill acquisition were not negatively affected. In fact, high-SES students with top scores display enhanced learning. Similarly, Figure XII rejects negative impacts on labor market outcomes and indicates an improvement in earnings.[26] These findings indicate that the expansion of elite colleges, driven by an influx of low-SES students, did not adversely affect high-SES, high-achieving students who typically attend these institutions.

What causes the positive spillovers on nonrecipients? We argue that it is attributed to changes in peer compositions resulting from the policy, which raised the demand for elite education among high-achieving students. Low-SES high-achievers, who would not have applied for HQ private colleges due to affordability issues, now applied for admission. As their test scores met the admission criteria, these institutions enrolled more high-achieving students. Notably, we observe a

[24] Additionally, the DD results reveal that the earnings gains for low-SES students are particularly pronounced for the top 5% of test scores, who experienced larger gains in college-program "value-added," compared to the subsequent 5% (Figures A.24 and A.25).

[25] Figure IX suggests that high-SES students scoring in the top decile also reassessed their choices and were less likely to attend LQ colleges after the policy announcement, indicating potential gaps in their baseline knowledge about college quality. Dynarski et al. (2021) found that information frictions also exist in the U.S.

[26] In Figure XII, the comparison group is based only on the 2013 cohort because the COVID-19 pandemic in 2020 affected the 2012 cohort's earnings eight years after high school.

nearly 5% increase in the average quality of entering students at HQ private colleges after the policy (Figure A.26). This exposure of high-SES students to more capable peers potentially contributed to their own academic improvements. Additionally, the benefits of studying alongside a more socioeconomically diverse student group could also play a role in these positive outcomes (Londoño-Vélez, 2022).[27]

Lastly, Figures X, XI, and XII illustrate that learning and earnings for low-SES students in the ninth decile of SABER 11 remained relatively stable or slightly improved, even without changes in college quality or "value-added." The average student quality did not decline at LQ colleges, primarily due to the relatively small number of SPP recipients compared to the overall students body at these institutions (Figure A.26). Instead, it appears that the students who took the place of SPP recipients at LQ colleges displayed enhanced skills. This, coupled with their improved college access, may have ultimately resulting in higher earnings.

To summarize, there are no discernible negative impacts from the policy; on the contrary, it seems to have yielded positive spillover effects, improving outcomes for all students in the cohort. Overall, both equity and efficiency improved across the board.

6 Cost-Benefit Analysis

To evaluate the potential cost-effectiveness of the SPP program, we conduct a prospective cost-benefit analysis using the concept of the MVPF. This analysis compares the program's impact on projected lifetime earnings to the overall program costs, specifically focusing on the ratio of program benefits among beneficiaries to the net costs incurred by the government (Hendren and Sprung-Keyser, 2020).

Projecting Lifetime Earnings. To project the earnings impacts of the SPP program throughout the lifecycle, we make several assumptions. Firstly, we assume an average age of 18 one year after graduating high school, based on the average age of taking the SABER 11 exam of 17 (Table A.2). Secondly, we assume a retirement age of 60, consistent Colombia's current retirement age (57 for women and 62 for men). Thirdly, we estimate the lifecycle earnings profile of those affected by the policy using population average trajectory based on the 2019 *Gran Encuesta Integrada de*

[27] Other possible reasons for the positive spillover effects on nonrecipients include the potential capital and labor responses of HQ private colleges, such as increasing per-student expenditures or the number of teaching instructors, which could have provided added support to nonrecipients. Nonetheless, we do not find significant changes in per-student spending or instructor numbers (Figures A.29 and A.30).

Hogares (GEIH), Colombia's main employment and earnings survey.[28] Fourthly, we use the RD-IV estimates one to eight years after high school from Table VII, treating censored observations as zeros and combining temporary earnings losses with estimated earnings gains starting in year six.[29] As in Hendren and Sprung-Keyser (2020), we project the eight-year percentage earnings gain forward throughout the lifecycle to obtain the earnings impacts for the rest of individuals' lives. This projection assumes a constant percentage earnings impact over time, starting from eight years after high school. This is a conservative assumption for reasons discussed in Section 4.5. Fifthly, we convert monthly earnings to annual earnings by multiplying by 12 and adjust for inflation using the consumer price index. Finally, we discount all earnings gains by 3% back to the time of initial expenditure. Overall, Column (2) of Table VII shows that financial aid is expected to increase discounted lifetime earnings by 110,283,598 pesos (US$27,222) for each treated student at the test score cutoff and 82,790,624 pesos (US$20,436) at the wealth cutoff.

Estimating Direct and Indirect Costs. We follow the methodology used by Angrist et al. (2021) to calculate direct and indirect cost measures. We calculate total government expenditure on the SPP program over eight years using ICETEX data. This expenditure measure, denoted as D_i, includes average educational expenses per full-time student transferred to the institution and the stipend provided to the recipient. In the case of private colleges, the average educational expenses per SPP recipient correspond to the tuition fee for full-time students, which is directly transferred by the government to institutions. However, public colleges' average educational expenses per full-time student exceed the discounted tuition fees (Table A.1). Hence, we incorporate the actual average educational expenses per SPP recipient in public institutions based on ICETEX data. In the IV model, we employ D_i on the left-hand side to represent direct government spending on the SPP program. As nonrecipients of the SPP program do not receive any aid, the program's effect on D_i in Column (3) of Table VII reflects the average government expenditure on treated students.

However, it is essential to consider that SPP nonrecipients also generate costs

[28] We drop individuals who are inactive in the labor force, outside municipal cores (*cabeceras municipales*), aged younger than 18, aged older than 60, or without a high school diploma.

[29] The estimates earnings gains and losses using SABER 11 as the running variable are statistically significant. However, when using SISBEN as the running variable, we take a conservative approach by assuming no negative earnings impact in years 1 and 2 after high school, and no positive earnings impact in years 5 and 6 as these estimates are not statistically significant at the 10% level (Table VII).

for the government when they attend public colleges. To incorporate this cost, we calculate the total cost of attendance (COA_i) for all students who enroll in any undergraduate program within six years after completing high school, utilizing data from SNIES. Unlike D_i, COA_i takes into account the government's spending on nonrecipients who pursue associate and bachelor's degree programs at public institutions.[30,31] (For nonrecipients who attend private institutions, we assume that their tuition fees cover the entire educational expenses, resulting in no additional cost to the government.) As a result, Column (4) of Table VII showcases the additional educational expenditure incurred by the government due to the SPP program. The program increased per-student educational expenditure due to (i) its direct costs, (ii) by influencing the duration of college enrollment, moving students from short-cycle programs to four- or five-year degree programs, and (ii) by shifting recipients to more expensive institutions, as we demonstrated earlier. We discount D_i and COA_i back to year one at an annual rate of 3%.

The statistics in Table VII show the difference between D_i and COA_i. D_i is 78,335,884 pesos (US$19,336) at the test score cutoff and 86,192,140 pesos (US$21,275) at the wealth cutoff since the latter students are more likely to attend college and choose more expensive institutions. When considering the total costs of attendance (COA_i) for both recipients and nonrecipients, the marginal increase in educational spending decreases to 51,083,113 pesos (US$12,609) at the test score cutoff and 47,786,944 pesos (US$11,796) at the wealth cutoff. Once again, the difference between COA_i and D_i reflects that nonrecipients opt for public colleges, incurring additional costs for the government captured in COA_i.

The MVPF. In our analysis, the observed earnings gains after taxes and transfers represent the willingness to pay for individuals who change their behavior due to the SPP program. The discounted lifetime earnings gains are US$27,222 at the test score cutoff and US$20,436 at the wealth cutoff. Assuming a tax and transfer rate of 19%, similar to previous studies (Angrist et al., 2021; Hendren and Sprung-Keyser, 2020),

[30] We use data on educational expenditures per full-time student from ICETEX for HQ public colleges; for LQ public colleges, we use per-full time student expenditure data from financial accounts and balance sheets reported by the colleges to Colombia's Ministry of Education. For institutions that do not disclose information on average educational expenses, such as SENA, we rely on the average educational expenses of full-time students enrolled in the same degree type (associate or bachelor's), institution type (private or public), and institutional quality (high or low).

[31] This calculation of the cost of attendance does not include expenses for books, supplies, housing, transportation, or variations in the marginal cost of educating different types of students with varying levels of academic support.

the total willingness to pay is calculated by summing the post-tax and post-transfer earnings gains with the value of the transfer (D_i) for individuals who do not change their behavior. Table VII shows a willingness to pay of 167,665,598 pesos (US$41,386) at the test score cutoff and 153,252,546 pesos (US$37,828) at the wealth cutoff.

Assuming a 19% tax rate on incremental earnings reduces the government's program costs by the same amount as the reduction in total willingness to pay. Based on Table VII, the direct costs of financial aid (D_i) are 57,382,000 pesos (US$14,164) at the test score cutoff and 70,461,922 pesos (US$17,393) at the need cutofff. This implies an MVPF of 2.92 (SABER 11) and 2.17 (SISBEN), indicating that each dollar of public spending on the SPP program generates $2.92 and $2.17 of private benefits, respectively. When considering the impact on marginal educational spending (COA_i), the MVPF roughly doubles to 5.56 (SABER 11) and 4.78 (SISBEN). The SPP program exhibits higher MVPFs compared to other cost-effective financial aid programs targeting college-bound high school students discussed in Hendren and Sprung-Keyser (2020) and Angrist et al. (2021).[32]

And yet, these cost-benefit comparisons may underestimate the impact on social welfare due to several reasons. Firstly, they ignore the positive spillover effects on nonrecipients documented earlier. Secondly, they do not account for future economic returns from the program's impact on graduate education, increasing returns to STEM-related degrees, and increasing returns to degrees from elite universities. Lastly, they overlook potential non-pecuniary benefits of education, like improved health and lower crime, and reduced public spending on healthcare and criminal justice.

7 Conclusions

This paper studied the role of elite colleges as catalysts for upward mobility, leveraging a groundbreaking financial aid reform in Colombia that pushed academically successful low-SES students into elite universities. We exploited stringent program eligibility criteria and nationwide administrative microdata on long-term educational and labor market outcomes. Using multiple complementary identification strategies,

[32] For instance, SPP surpasses the MVPF of Nebraska's STBF program (1.75), Michigan's HAIL Scholarship (1.30), the Wisconsin Scholars Grant program (1.43), Georgia's HOPE Scholarship (4.0), Ohio's Pell Grants (2.49), Pell Grants to adults (2.18), Tennessee's HOPE Scholarship (1.86), and Kalamazoo's Promise program (1.93).

we found improved economic mobility, enhanced equity, and increased efficiency.

There are two caveats worth considering. First, quality upgrading in our setting is facilitated by the observability of institutional quality to policymakers and students, admissions based on academic credentials, and low-SES students' test scores meeting admission criteria for high-quality colleges. Conversely, achieving these results may be more challenging when assessing institutional quality is difficult, when elite colleges' admissions practices disadvantage low-SES students, or when few low-SES students have sufficiently high test scores (Chetty et al., 2023, 2020). Second, although we find that expanding elite colleges does not harm aid-ineligible students, caution should be taken when extrapolating these findings to policies that aim to expand access for low-SES students through changes in admissions practices, especially if such policies lower the quality of the admitted class.

Future research could explore several additional dimensions. Firstly, delving into how elite colleges influence students' social and professional networks, job search behavior, and aspirations could provide valuable insights. Secondly, investigating the broader socioeconomic effects of high-quality education beyond the labor market, such as life satisfaction, health, civic engagement, marriage, fertility, and spousal quality, would contribute to a more comprehensive understanding. Thirdly, the observed heterogeneous effects across the two eligibility margins of the financial aid policy suggest the potential existence of an equity-efficiency trade-off in its design. Lastly, comparing the MVPF of a demand subsidy with a policy subsidizing the supply of public universities, particularly in light of the policy changes in 2022, would provide essential information for evaluating potential government policies and assessing where public spending could have the biggest "bang for the buck."

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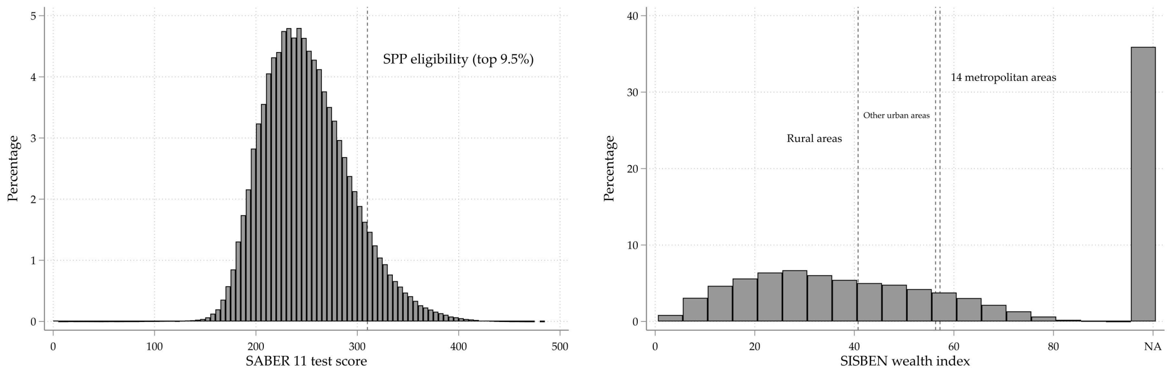
Figures and Tables

Figure I: Eligibility Conditions for the SPP Financial Aid Program

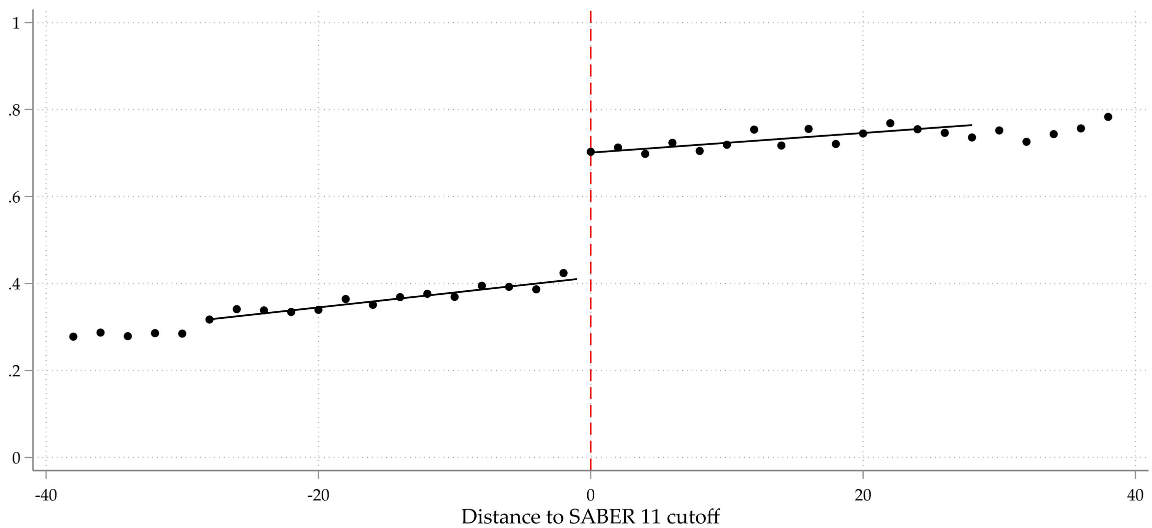
(a) Merit: SABER 11 test score $\geq 310/500$ (b) Need: SISBEN wealth index $<$ threshold



Notes: The SPP financial aid program has specific eligibility criteria based on performance in the national standardized high school exit exam, SABER 11, and the household SISBEN wealth index. Panel A shows the distribution of test scores two months before the policy announcement in 2014, with the vertical line marking the highest 9.5% of test scores eligible for SPP. Panel B illustrates the wealth distribution for test takers, with vertical lines indicating SPP's location-specific eligibility thresholds. (One-third of test takers are not included in SISBEN and denoted as "N/A.") Among test takers, 52.8% meet the eligibility criteria based on their SISBEN score. The subsequent figures use "SISBEN-eligible" or "low-SES" interchangeably to refer to individuals meeting SPP eligibility based on their SISBEN score, while "SISBEN-ineligible" or "high-SES" describe those who do not. Figure A.1 displays the distribution of test scores, categorized by SES, revealing that low-SES students tend to exhibit lower performance.
Sources: Authors' calculations based on SABER 11 (ICFES) and SISBEN (DNP).

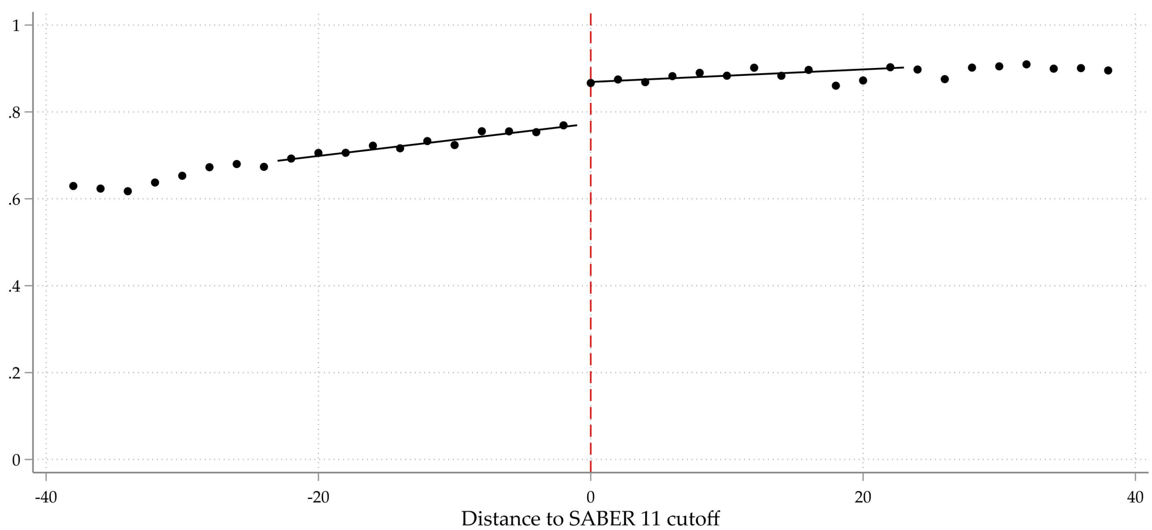
Figure II: A Sizable Expansion of College Attendance

(a) Probability of Attending College Immediately After High School



Sample restricted to SISBEN-eligible individuals.

(b) Probability of Ever Attending College

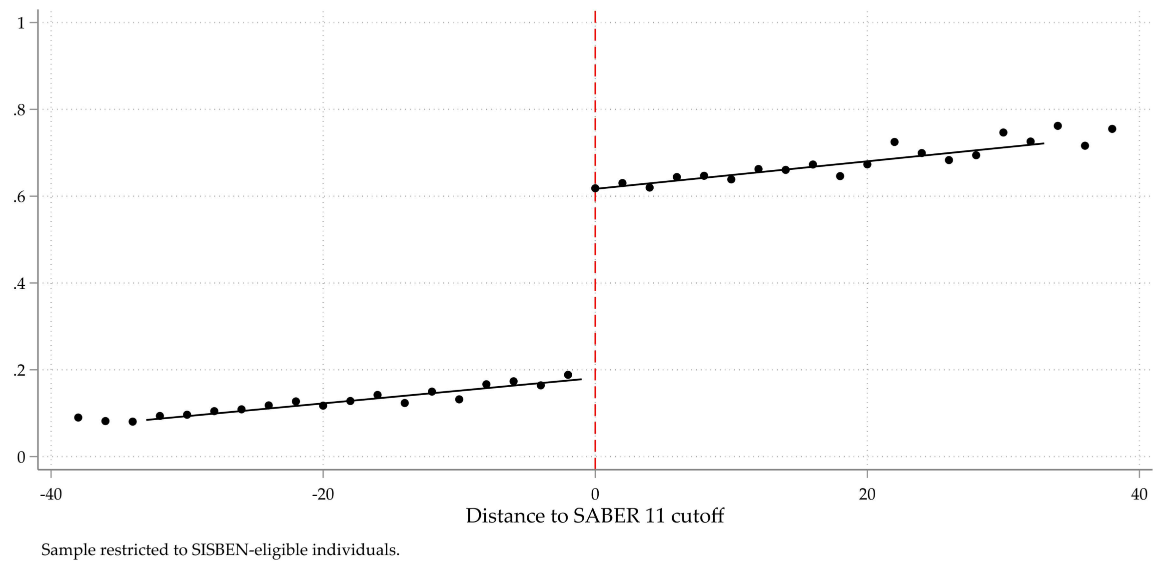


Sample restricted to SISBEN-eligible individuals.

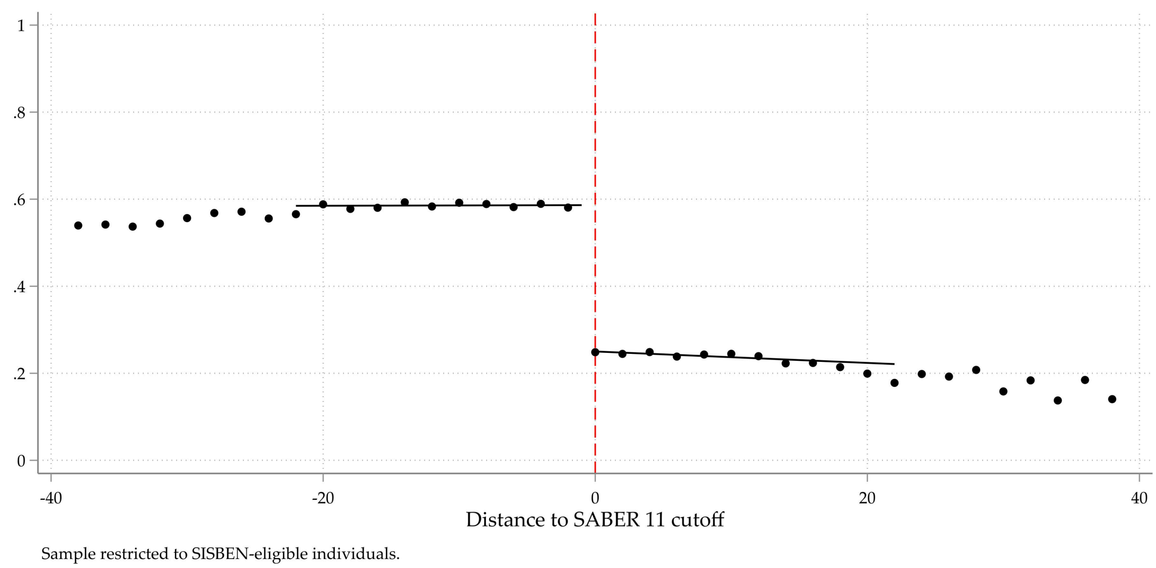
Notes: Panels A and B display low-SES students' probability of ever attending college within zero and six years after completing high school, respectively, based on the distance to the test score cutoff. Table I reports the reduced-form RD estimates. Figure A.6 shows similar results using SISBEN as the running variable.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SNIES (MEN).

Figure III: A Substantial Improvement in College Quality

(a) Probability of Ever Accessing an HQ College



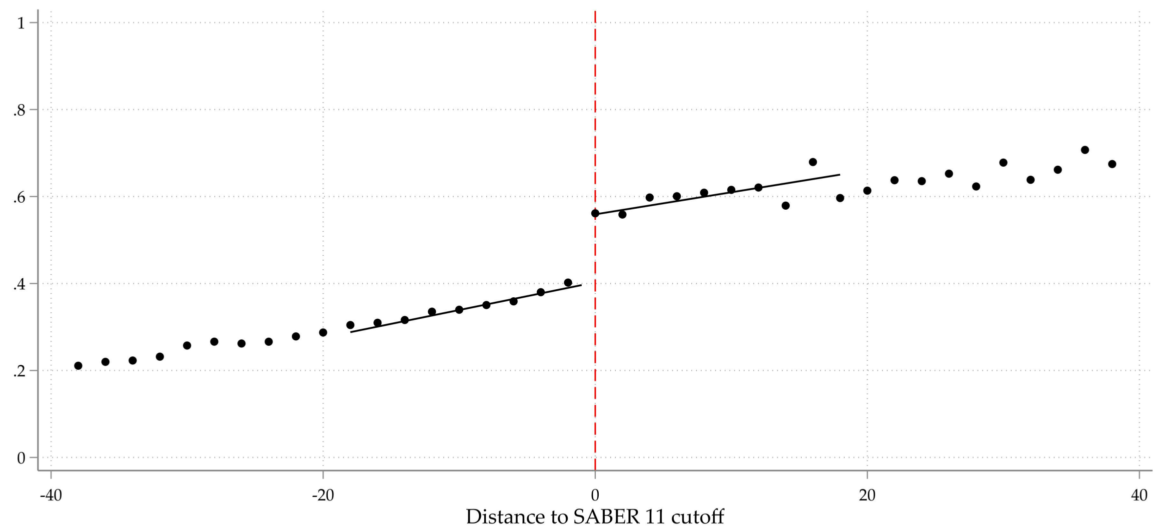
(b) Probability of Ever Accessing an LQ College



Notes: The figure breaks down the long-term enrollment effect by college quality. An HQ college refers to one of the 33 institutions with high-quality status by October 2014; all other colleges are LQ colleges. Figure A.9 plots the RD coefficient over time and further breaks down the results by program duration. Figures A.7 and A.10 show similar patterns utilizing SISBEN as the running variable.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SNIES (MEN).

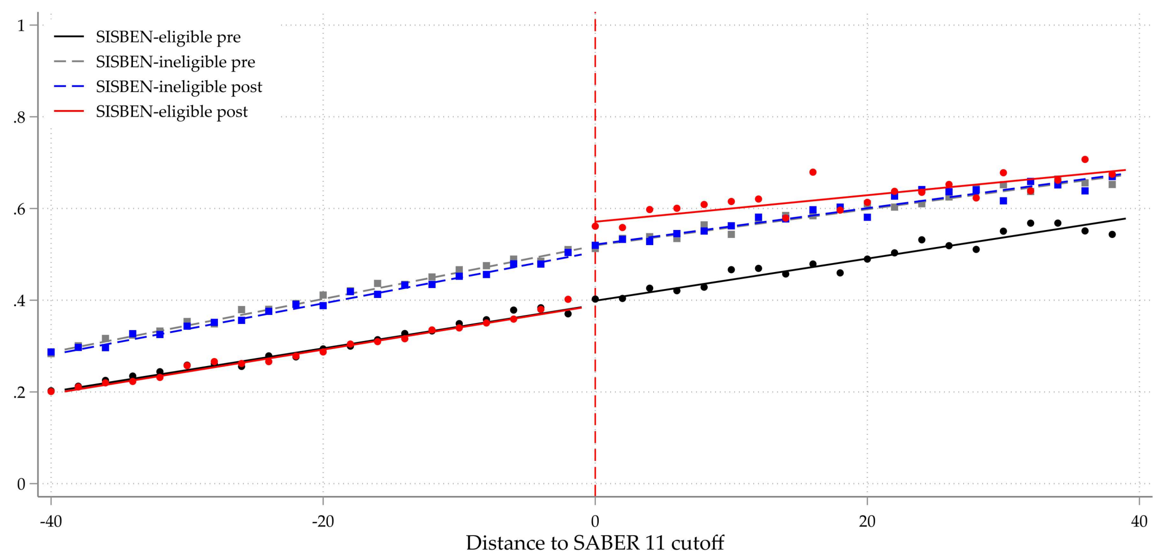
Figure IV: An Improvement in Bachelor's Degree Attainment

(a) Probability of Earning a Bachelor's Degree



Sample restricted to SISBEN-eligible individuals.

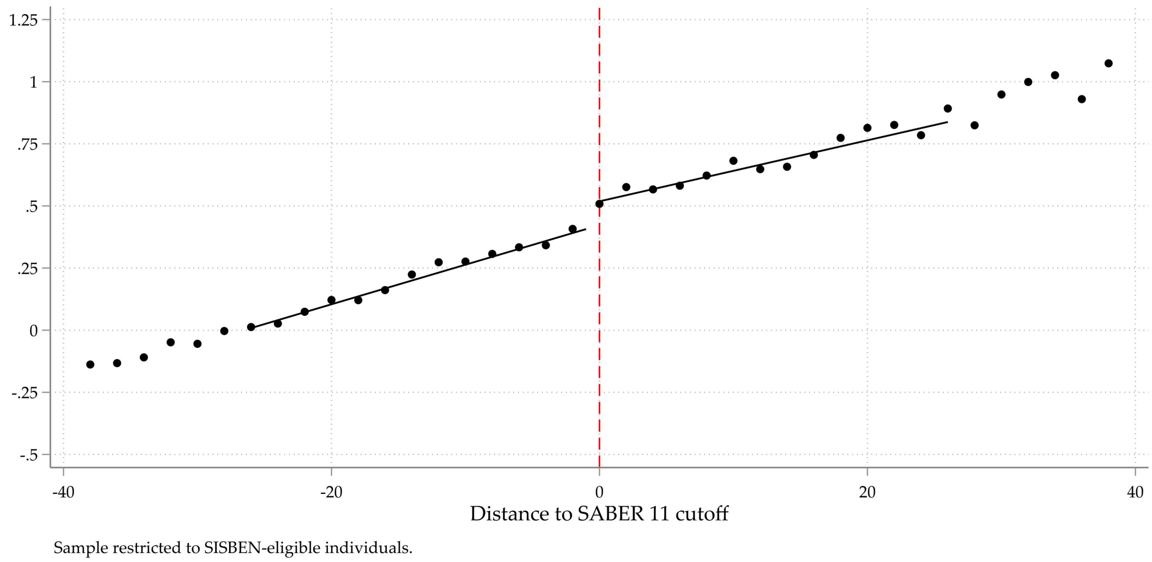
(b) The Program Eliminated the SES Gap in Bachelor's Degree Attainment



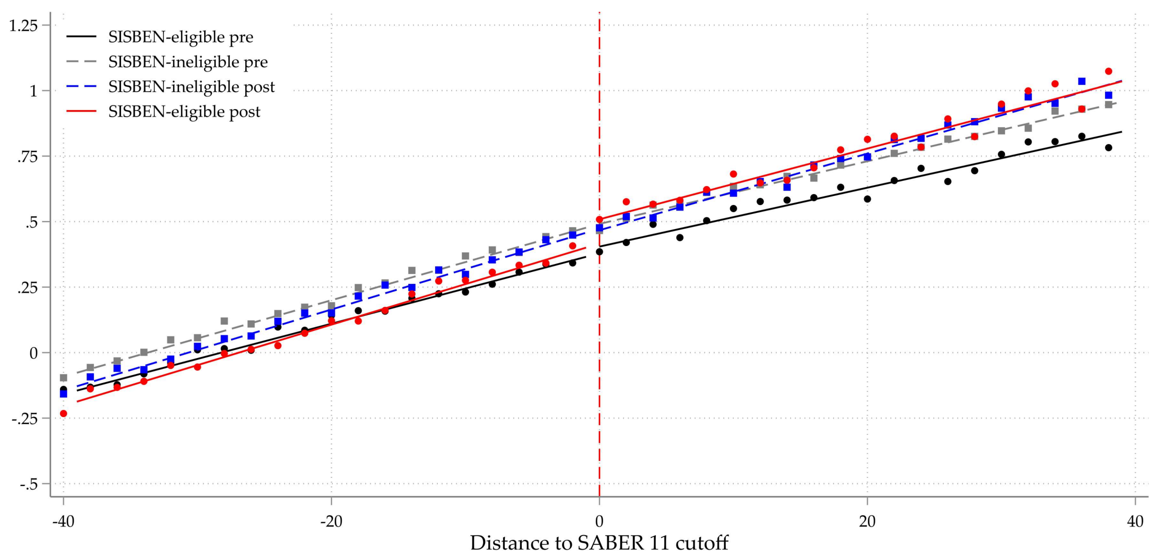
Notes: Panel A depicts low-SES students' likelihood of obtaining a bachelor's degree, proxied by taking the SABER PRO exam within seven years of high school completion, as a function of the distance to the test score cutoff. Table II provides the reduced-form RD estimate. Panel B demonstrates the impact on equity by comparing this outcome before and after the policy for high-SES and low-SES students. Before the policy, a 10 p.p. gap in bachelor's degree attainment existed between low-SES and high-SES students with similar test scores (depicted in black and gray, respectively). The policy had no discernible effect on bachelor's degree attainment for high-SES students (depicted in blue). However, it significantly increased attainment for low-SES students (depicted in red), effectively eliminating the socioeconomic gap.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SABER PRO (ICFES).

Figure V: An Improvement in Skill Development

(a) Performance in the College Graduation Exam



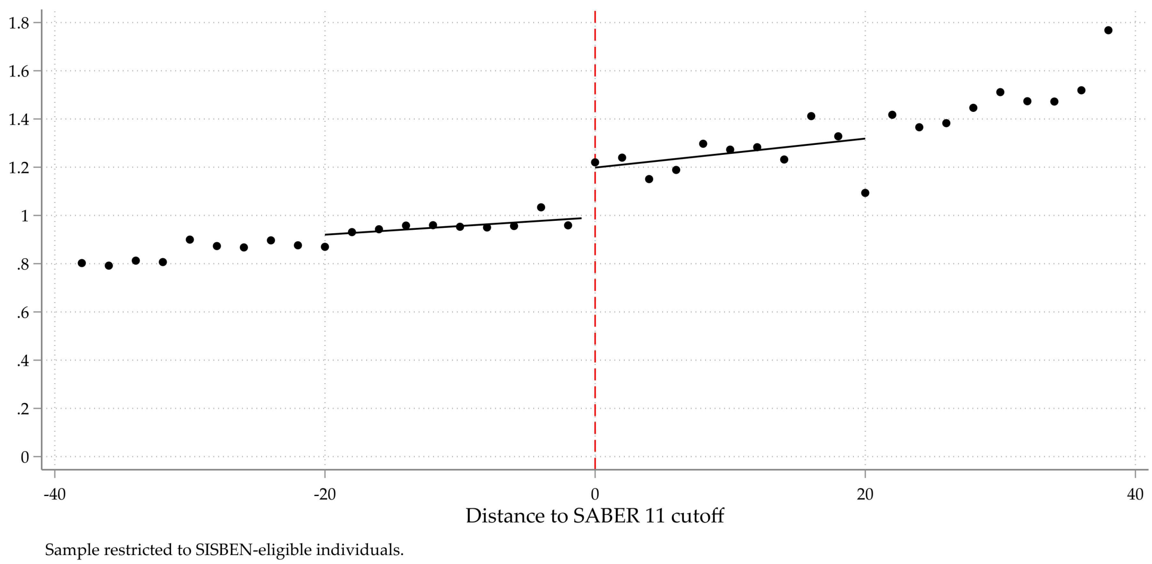
(b) The Program Eliminated the SES Gap in Skill Development



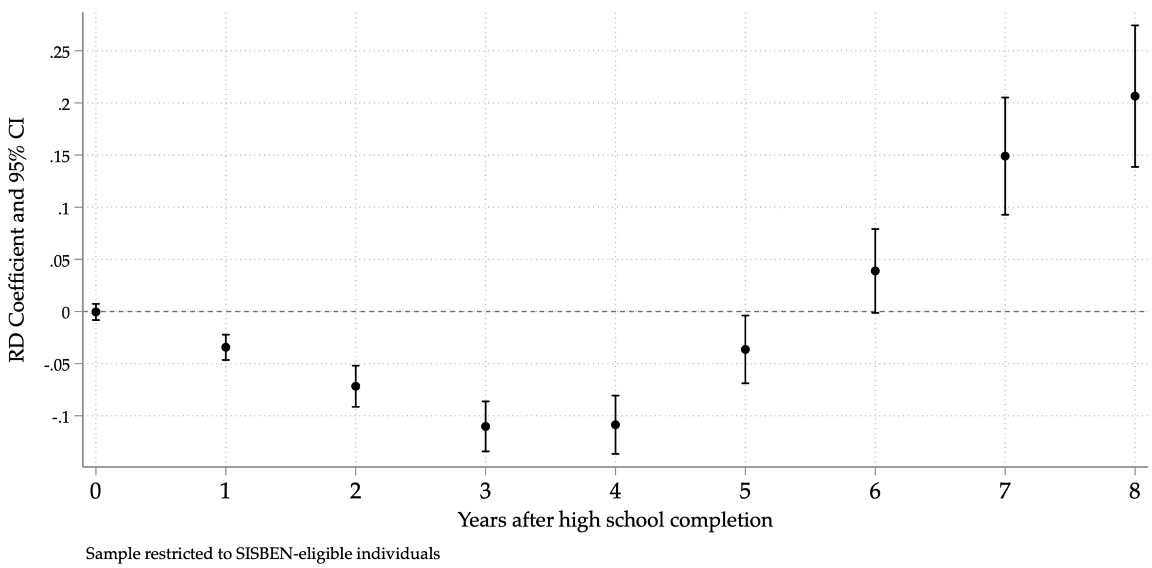
Notes: The figures plot students' performance in Colombia's college graduation exam, SABER PRO, within five years of completing high school. Table IV provides the reduced-form RD estimates. Panel B demonstrates the impact on equity by comparing this outcome before and after the policy for high-SES and low-SES students. Before the policy, there was a gap in college learning between low-SES and high-SES students who achieved similar test scores in high school (depicted in black and gray, respectively). This gap was attributed to differences in college learning "value-added" based on SES (Figure D.5). The program had no discernible effect on learning performance for high-SES students (depicted in blue). However, it significantly improved learning for low-SES students (depicted in red), effectively eliminating the SES gap.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SABER PRO (ICFES).

Figure VI: Substantial Gains in Formal Earnings

(a) Monthly Earnings Eight Years After High School Completion



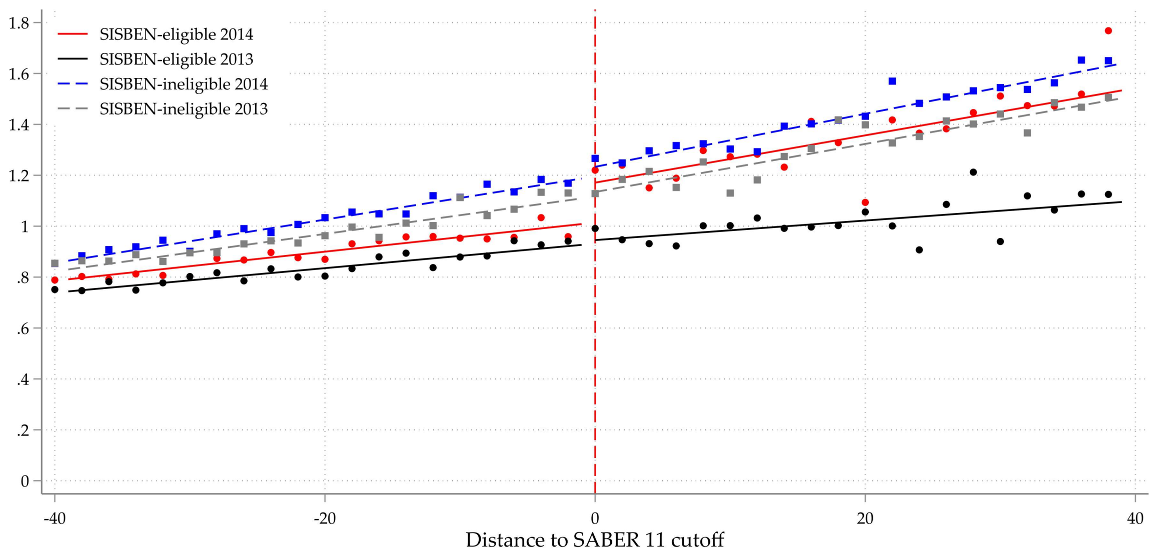
(b) The Dynamics of the Earnings Effect



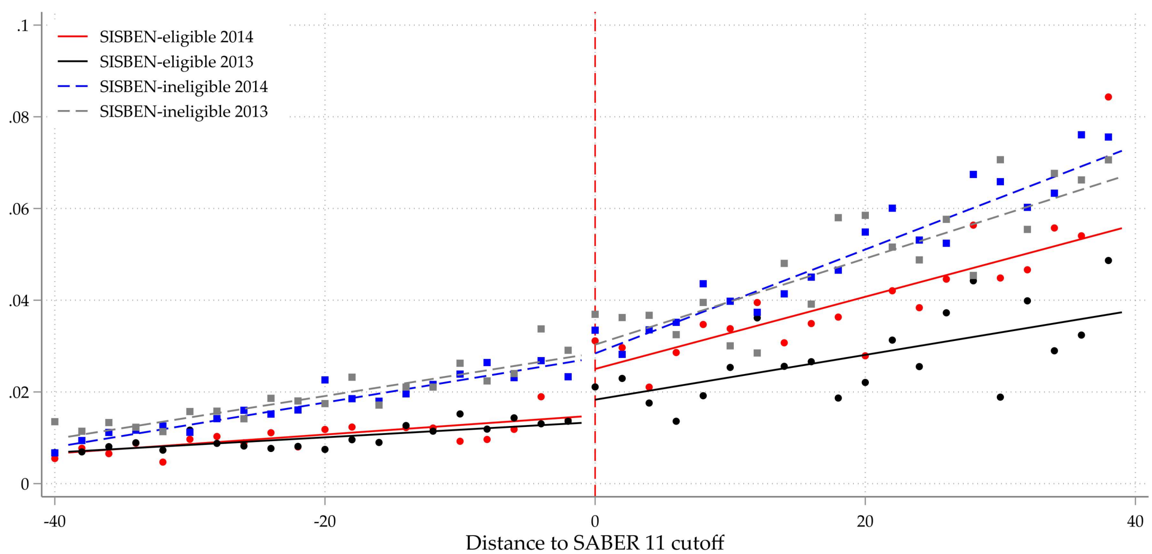
Notes: The figures depict the impact on formal monthly earnings, expressed as multiples of the monthly minimim wage, for low-SES students. Individuals without formal employment are assigned zeros earnings. Panel A compares earnings eight years after high school completion based on individuals' proximity to the test score cutoff. Table V reports the reduced-form RD estimates. Panel B plots the RD coefficient and 95% confidence intervals over time. Figure A.22 shows similar effects using SISBEN as the running variable.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and PILA (MinSalud).

Figure VII: The Program Narrowed the SES Gap in Earnings

(a) Monthly Earnings



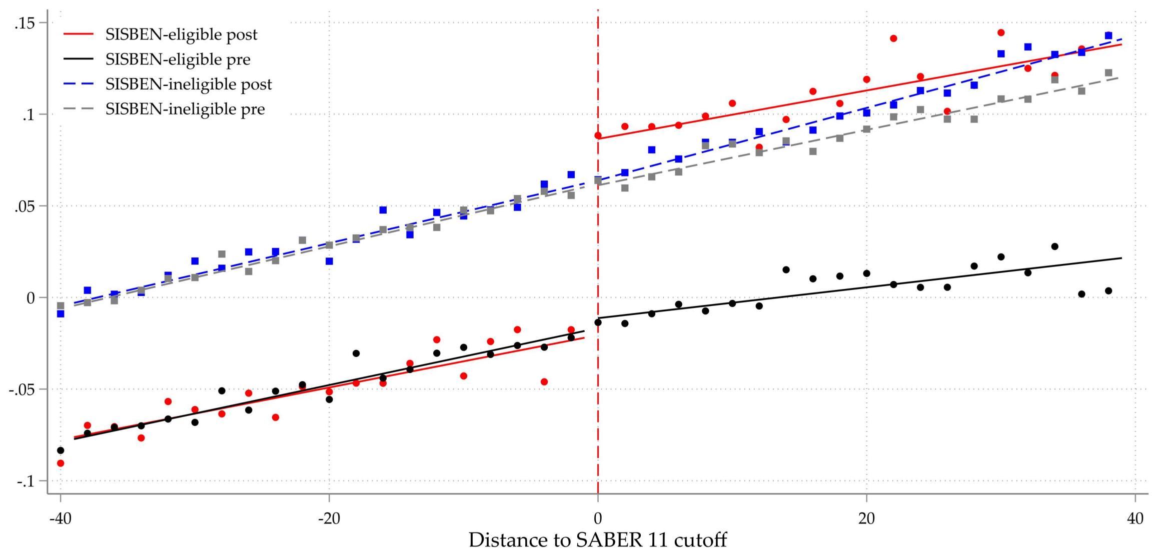
(b) Top 1% of Monthly Earnings



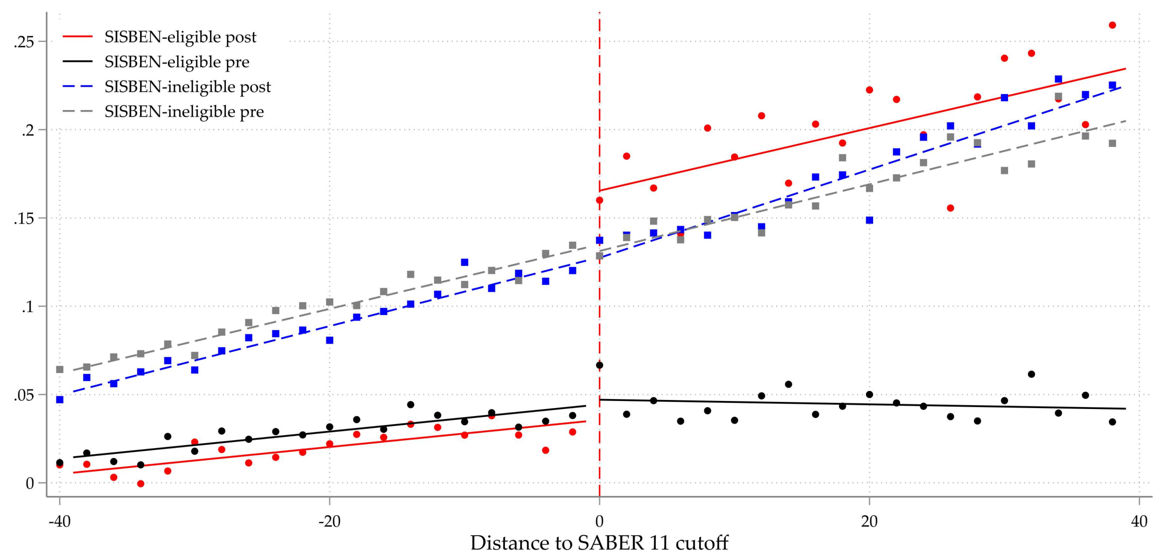
Notes: This figure illustrates that the policy narrowed the socioeconomic disparity in post-college earnings. In Panel A, the outcome is formal earnings eight years after high school completion, represented in multiples of the monthly minimum wage. In Panel B, the outcome is the likelihood of earning in the top 1% of the earnings distribution compared to others from the same high school cohort. Before the policy, there existed an earnings gap between low-SES and high-SES students who achieved similar test scores in high school (shown in black and gray, respectively). While high-SES students' earnings were unaffected (depicted in blue), low-SES students experienced substantial earnings gains (depicted in red), effectively reducing the socioeconomic gap.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and PILA (MinSalud).

Figure VIII: The Program Closed the SES Gap in College "Value-Added"

(a) Learning "Value-Added"



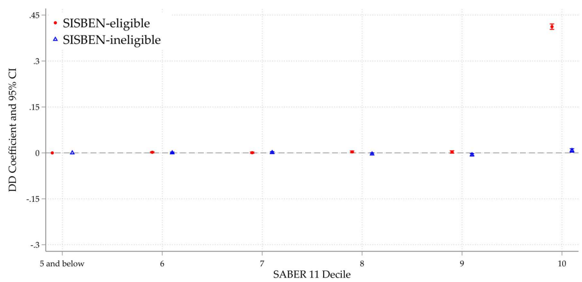
(b) Earnings "Value-Added"



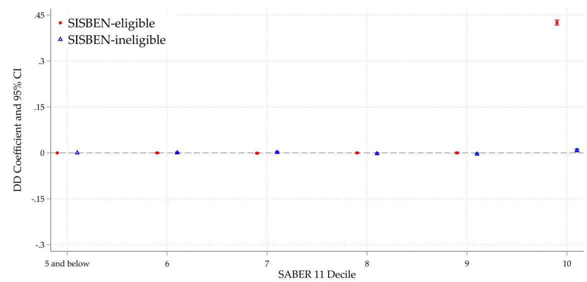
Notes: This figure illustrates that the policy improves equity by reducing the socioeconomic gap in college "value-added." Panel A derives this measure from college graduation test scores using pre-policy data, while Panel B derives it from formal monthly earnings eight years after high school, also using pre-policy data. Pre-policy, a college "value-added" gap existed between low-SES and high-SES students with similar test scores in high school (in black and gray, respectively). Higher test scores correlated with higher *learning* "value-added" for both low-SES and high-SES students. However, this association did not extend to *earnings* "value-added" for low-SES students, due to their inability to afford private HQ colleges known for excelling in *earnings* "value-added." The policy not affect college "value-added" for high-SES students (in blue). Instead, it facilitated access for low-SES students to colleges with superior learning and earnings prospects (in red), eliminating the SES gap.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), SNIES (MEN), SABER PRO (ICFES), and PILA (MinSalud).

Figure IX: No Crowd-Out Effects

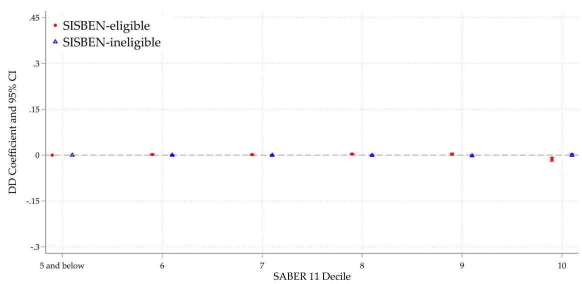
(a) HQ Colleges



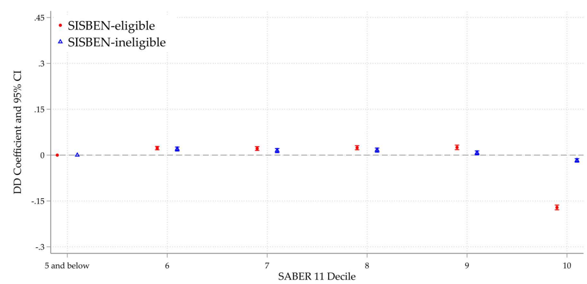
(b) HQ Private Colleges



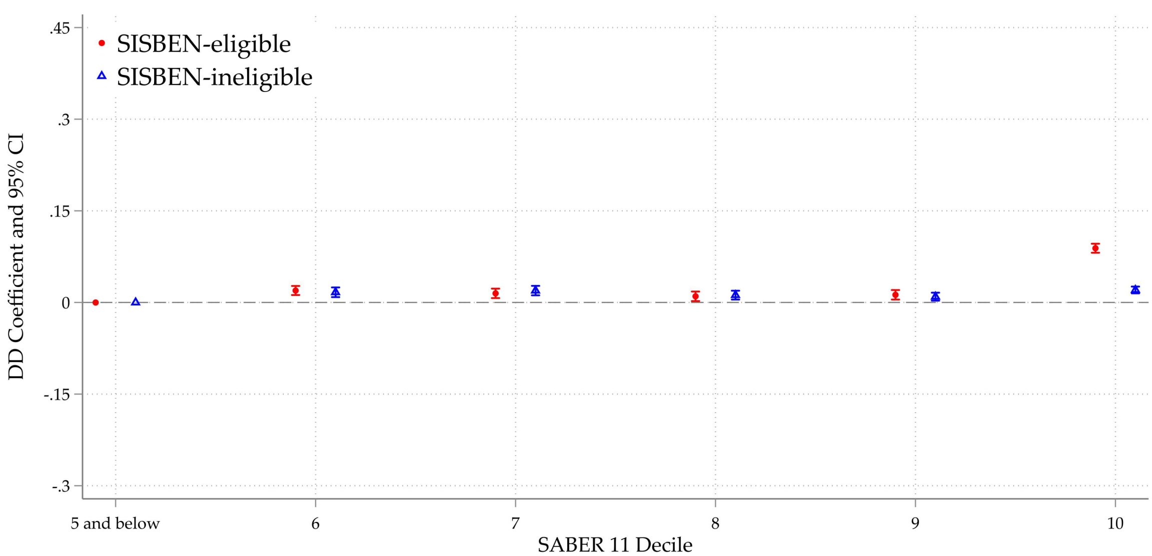
(c) HQ Public Colleges



(d) LQ Colleges



(e) Any College Within Six Years from High School

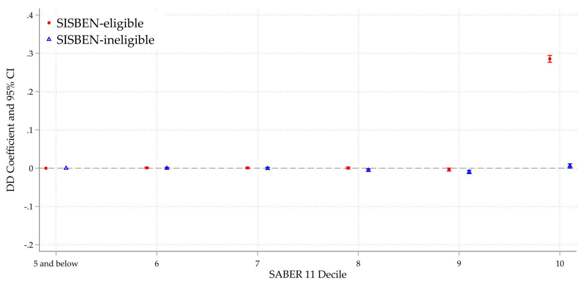


Notes: This figure displays the DD coefficients and their 95% confidence intervals from Specification (2), indicating the overall effect of the policy on college access and quality. The red and blue markers represent the low-SES and high-SES students, respectively. Each panel corresponds to a different outcome: immediate enrollment in HQ colleges (A), HQ *private* colleges (B), HQ *public* colleges (C), LQ colleges (D), and *any* college within six years after high school (E). Financial aid did not displace nonrecipients from HQ colleges (this is also supported by Figure A.28, which indicates that their college "value-added" remained unchanged). HQ private colleges expanded capacity and LQ colleges admitted lower-performing applicants to fill the vacant seats, improving college enrollment for the entire cohort. Figure A.23 provides support for the parallel trends assumption, using the 2013 cohort as a placebo.

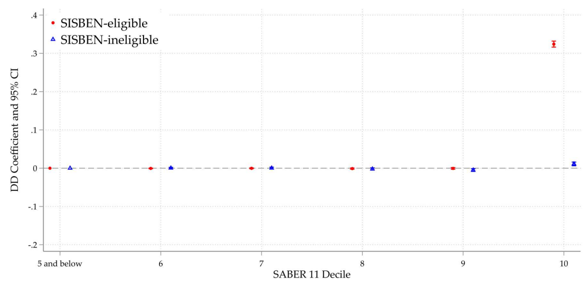
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SNIES (MEN).

Figure X: Nonrecipients' are not Less Likely to Earn a Bachelor's Degree

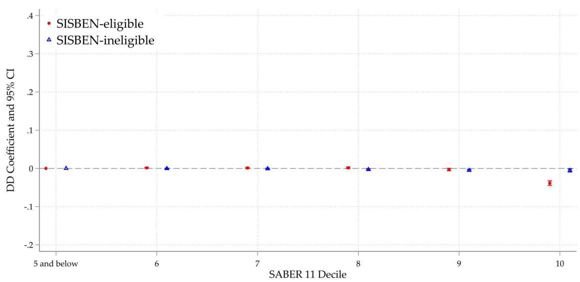
(a) HQ Colleges



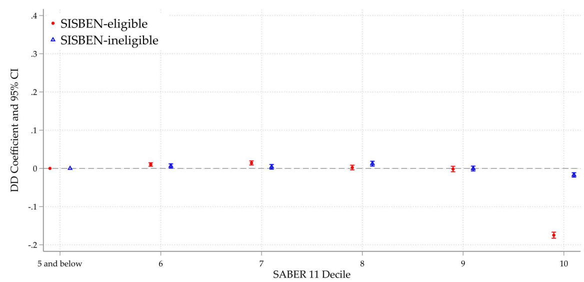
(b) HQ Private Colleges



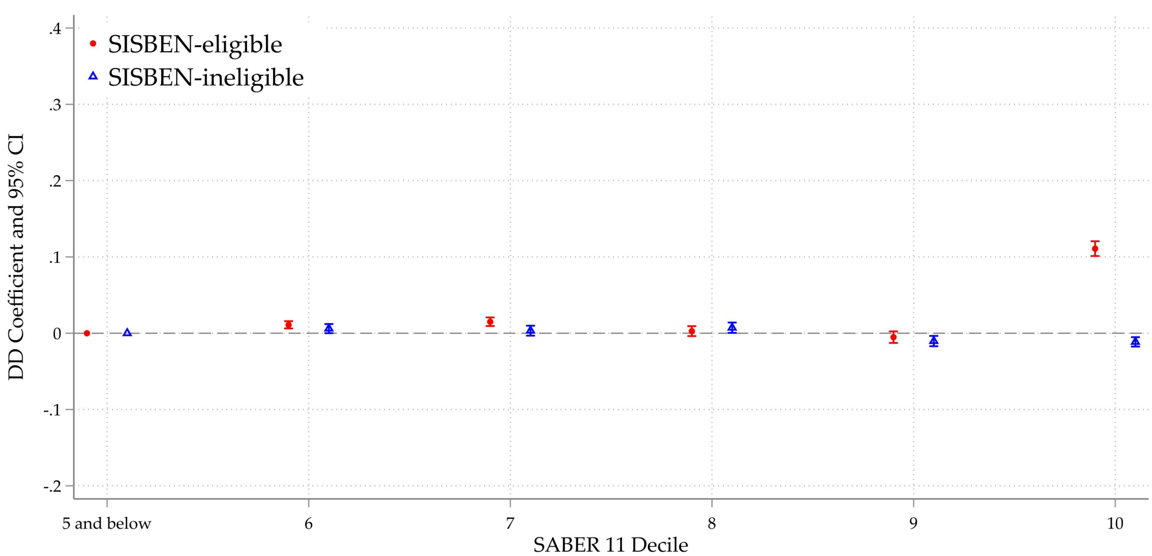
(c) HQ Public Colleges



(d) LQ Colleges

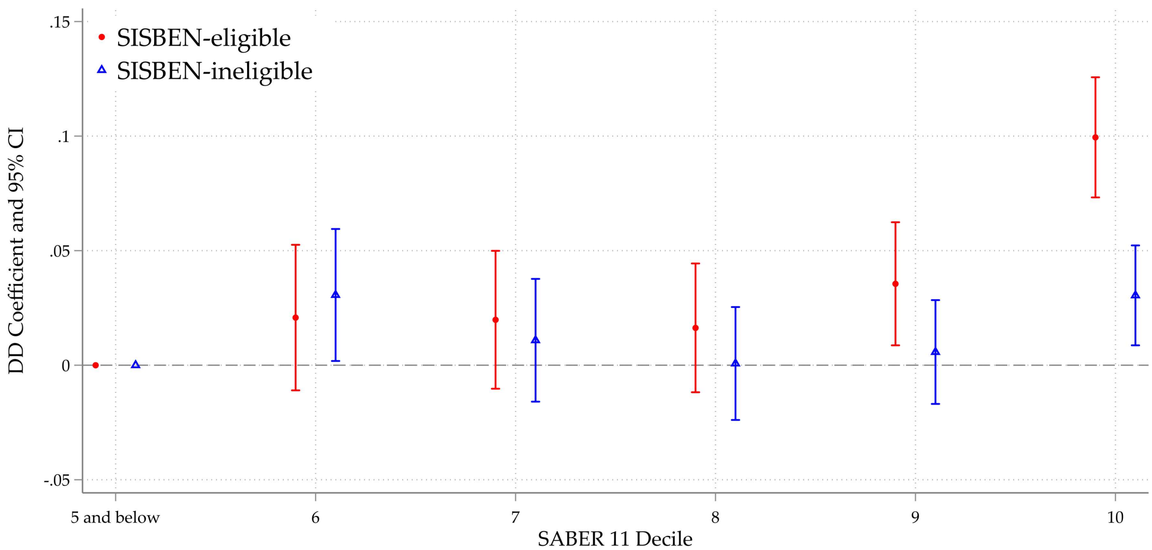


(e) Any College



Notes: This figure plots the DD coefficients and their 95% confidence intervals from Specification (2), indicating the overall impact of the policy on bachelor's degree attainment, as measured by completing SABER PRO within seven years after high school. Each panel represents bachelor's attainment from different college types: HQ colleges (A), HQ *private* colleges (B), HQ *public* colleges (C), LQ colleges (D), and any college (E). The policy did not affect nonrecipients' likelihood of earning a bachelor's degree from an HQ college. Figure A.23 provides support for the parallel trends assumption, using the 2013 cohort as a placebo.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SABER PRO (ICFES).

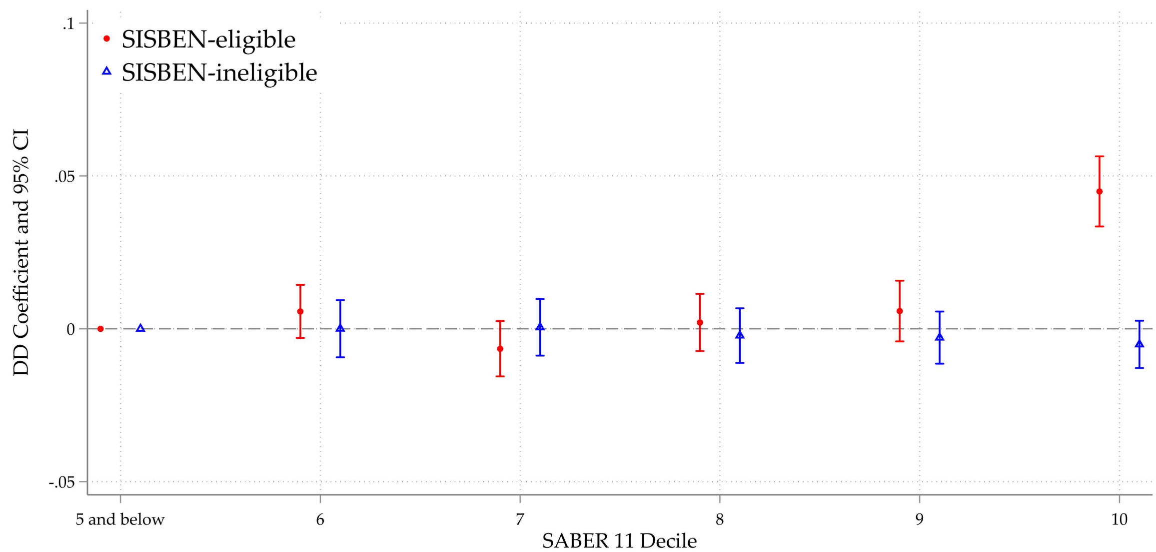
Figure XI: Nonrecipients Display Improved Learning



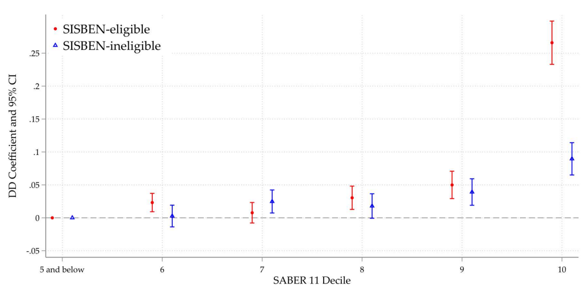
Notes: This figure plots the DD coefficients and their 95% confidence intervals from Specification (2), which represent the overall effect of the reform on performance in Colombia's college graduation exam, SABER PRO. The exam is usually taken within five years after high school completion. The reform did not hinder nonrecipients' learning performance. Interestingly, there was an improvement in test scores for high-SES students in the top test score decile (blue markers). This improvement cannot be attributed to changes in college learning "value-added" (Figure A.28). Instead, these positive effects may come from attending college with higher-achieving peers (Figure A.26). Furthermore, low-SES students in the ninth decile (red markers) also experienced an enhancement in test scores, suggesting an increase in overall efficiency. Figure A.23 sprovides support for the parallel trends assumption, using the 2013 cohort as a placebo.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SABER PRO (ICFES).

Figure XII: Nonrecipients Exhibit Higher Earnings

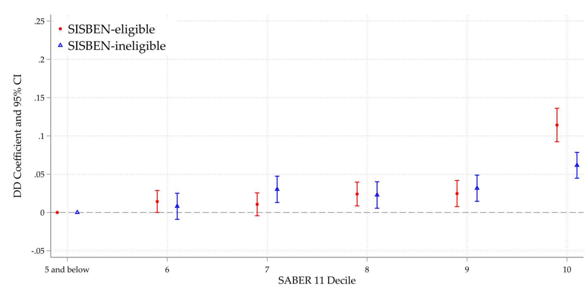
(a) Employment



(b) Earnings



(c) Log Earnings



Notes: This figure plots the DD coefficients and their 95% confidence intervals from Specification (2), representing the impact on nonrecipients' formal labor market outcomes eight years after high school completion. The outcome is employment in Panel A, earnings (measured in multiples of the monthly minimum wage and including zeros) in Panel B, and log earnings in Panel C. The comparison group is based on the 2013 cohort since the COVID-19 pandemic in 2020 affected the 2012 cohort's earnings eight years after high school. The reform did not devalue degrees from HQ colleges. Interestingly, there was an improvement in earnings for high-SES students in the top decile (blue markers) and a slight earnings gain for low-SES students in the ninth decile (red markers). Again, these improvements cannot be attributed to changes in college "value-added" (Figure A.28). Instead, the earnings gain is consistent with nonrecipients having greater college access (Figure IX) and acquiring more knowledge (Figure XI).
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and PILA (MinSalud).

Table I: Reduced-Form Estimates on Enrollment Over Time by Type of College and Program

	Enrollment within zero years from high school completion									Enrollment within six years from high school completion								
	Any	High-quality college			Low-quality college			Program duration		Any	High-quality college			Low-quality college			Program duration	
	college	Any	Private	Public	Any	Private	Public	Two years	Four years	college	Any	Private	Public	Any	Private	Public	Two years	Four years
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
***Panel A**: SABER 11 is the running variable*																		
Reduced form	0.287	0.469	0.470	-0.003	-0.180	-0.065	-0.117	-0.063	0.345	0.096	0.436	0.472	-0.038	-0.336	-0.120	-0.217	-0.121	0.212
	(0.011)	(0.009)	(0.009)	(0.006)	(0.009)	(0.006)	(0.007)	(0.006)	(0.012)	(0.010)	(0.009)	(0.010)	(0.008)	(0.012)	(0.008)	(0.010)	(0.009)	(0.012)
Mean control	0.414	0.096	0.028	0.071	0.317	0.106	0.213	0.112	0.307	0.773	0.181	0.044	0.140	0.586	0.200	0.390	0.215	0.562
Observations										297,279								
BW loc. poly.	28.48	30.60	26.86	25.30	27.12	30.22	32.74	25.92	21.08	23.33	33.15	23.71	26.19	22.18	26.74	26.99	22.92	20.29
Effect obs. control	29,368	32,363	25,871	24,714	27,607	32,363	35,547	24,714	18,948	21,963	37,647	21,963	25,871	20,459	25,871	25,871	20,459	17,966
Effect obs. Treat	11,214	11,576	10,754	10,576	11,002	11,576	11,880	10,576	9,489	10,107	12,061	10,107	10,754	9,815	8,796	10,754	9,815	9,317
***Panel B:** SISBEN is the running variable*																		
Reduced form	0.226	0.418	0.473	-0.053	-0.185	-0.077	-0.109	-0.060	0.286	0.049	0.357	0.468	-0.114	-0.302	-0.125	-0.177	-0.096	0.145
	(0.021)	(0.019)	(0.015)	(0.015)	(0.018)	(0.011)	(0.016)	(0.010)	(0.021)	(0.016)	(0.020)	(0.018)	(0.017)	(0.021)	(0.015)	(0.017)	(0.012)	(0.019)
Mean control	0.535	0.241	0.074	0.167	0.292	0.113	0.181	0.094	0.442	0.851	0.360	0.097	0.264	0.490	0.198	0.291	0.150	0.701
Observations										22,552								
BW loc. poly.	11.24	12.84	14.83	12.56	10.31	11.81	9.60	12.18	11.78	9.33	12.02	10.82	11.85	9.80	10.34	11.04	13.13	9.90
Effect obs. control	4,674	5,164	5,712	5,096	4,351	4,894	4,085	4,990	4,885	4,005	4,961	4,523	4,904	4,166	4,360	4,602	5,257	4,198
Effect obs. Treat	4,797	5,368	6,072	5,272	4,412	5,029	4,124	5,173	5,019	4,012	5,118	4,627	5,047	4,199	5,234	4,721	5,484	4,238

Notes: This table presents the reduced-form effect of program eligibility on postsecondary enrollment within zero (Columns 1–9) and six years (Columns 10–18) from high school completion using an RD design. The dependent variable is enrollment by college type (e.g., HQ, LQ) and program duration (two or three years versus four or five years). Panel A uses the SABER 11 test score as the running variable, restricting the sample to low-SES students. Panel B uses the SISBEN wealth index as the running variable, restricting the sample to merit-eligible students. The reduced-form coefficient in Column (1) of Panel A suggests that, for low-SES individuals, program eligibility raises immediate postsecondary enrollment by 28.7 p.p. or 69.5% relative to a control mean of 41.4%. Conventional local linear RD estimates and standard errors in parentheses are estimated with package rdrobust (Cattaneo et al., 2014).
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SNIES (MEN).

Table II: Reduced-Form Estimates on Degree Attainment by Type of College and Program

	Any Degree	Two Year Degree	Four Year Degree											
			Any college	High-quality college			Low-quality college			Field of study				
				Any	Private	Public	Any	Private	Public	STEM	STEM Plus	Arts	S.S.H.	N.A.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
***Panel A:** SABER 11 is the running variable*														
Reduced form	0.062	-0.101	0.156	0.322	0.343	-0.016	-0.161	-0.066	-0.080	0.088	0.124	0.015	0.031	-0.017
	(0.012)	(0.009)	(0.014)	(0.011)	(0.009)	(0.006)	(0.009)	(0.006)	(0.007)	(0.009)	(0.014)	(0.003)	(0.006)	(0.005)
Mean control	0.584	0.184	0.403	0.098	0.031	0.063	0.304	0.109	0.147	0.137	0.301	0.005	0.051	0.048
Observations								297,279						
BW loc. poly.	22.71	22.97	18.78	19.46	26.35	20.86	30.67	30.89	25.41	24.64	17.53	27.34	27.55	24.60
Effect obs. control	20,459	20,459	15,683	16,562	25,871	17,966	32,363	32,363	24,714	23,070	14,367	27,607	27,607	23,070
Effect obs. Treat	9,815	9,815	8,796	8,987	10,754	9,317	11,576	11,576	10,576	10,299	8,464	11,002	11,002	10,299
***Panel B:** SISBEN is the running variable*														
Reduced form	0.077	-0.060	0.145	0.327	0.390	-0.065	-0.179	-0.081	-0.081	0.063	0.101	0.017	0.051	-0.015
	(0.023)	(0.015)	(0.020)	(0.019)	(0.017)	(0.014)	(0.017)	(0.012)	(0.013)	(0.019)	(0.023)	(0.007)	(0.015)	(0.009)
Mean control	0.661	0.111	0.546	0.238	0.073	0.167	0.306	0.124	0.136	0.237	0.409	0.015	0.071	0.045
Observations								22,552						
BW loc. poly.	8.72	7.42	12.43	13.74	12.65	12.20	12.00	11.33	11.79	12.37	10.72	11.41	8.43	10.52
Effect obs. control	3,738	3,199	5,053	5,417	5,118	4,995	4,953	4,698	4,888	5,036	4,480	4,723	3,615	4,424
Effect obs. Treat	3,761	3,162	5,234	5,689	5,303	5,179	5,108	4,830	5,025	5,220	4,582	4,852	3,603	4,496

Notes: This table presents the reduced-form effect on the likelihood of earning a degree (proxied by college exit exam test-taking) within seven years from high school completion using an RD design. Following U.S. Department of Homeland Security, STEM fields include Engineering, Biological and Biomedical Sciences, Mathematics and Statistics, Physical Sciences, and Medicine. STEM-Plus adds Agriculture and Related Sciences; Natural Resources Conservation; Architecture; Education; Military Science; Psychology; Accounting, Business, and Economics; and Health Professions and Related Programs. Arts includes Plastic and Visual Arts; Music; Advertising; Design. Social Sciences and Humanities include Anthropology; Geography and History; Sociology and SocialWork; Philosophy and Theology; Literature; Library Science; Social Communication and Journalism; Sports and Physical Education; Law; Political Science and International Relations. S.S.H. refers to social sciences and humanities. N.A. refers to missing field of study (all of which come from LQ colleges). See the notes under Table I for other details.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), SABER PRO (ICFES), and SABER T&T (ICFES).

Table III: Instrumental Variables Estimates for Educational and Labor Market Outcomes

	Enrollment within six years from high school					Degree attainment					College exit test score if exam taken within...		Formal	Formal earnings (includes zeros)		
	Any	High-quality college		Program duration		Any	Two-	Four-year degree						in	in	in
	college	Any	Private	Two Years	Four Years	degree	year degree	Any college	High-quality college Any	High-quality college Private	Five years	Seven years	work	constant pesos	monthly min. wages	natural logarithm
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Panel A: *SABER 11 is the running variable*																
IV	0.164	0.752	0.810	-0.208	0.363	0.106	-0.173	0.268	0.552	0.590	0.119	0.076	0.069	323,762.40	0.354	0.122
	(0.016)	(0.012)	(0.011)	(0.015)	(0.020)	(0.021)	(0.015)	(0.023)	(0.016)	(0.012)	(0.026)	(0.025)	(0.020)	(53,815.60)	(0.059)	(0.033)
First stage	0.583	0.580	0.583	0.583	0.584	0.583	0.583	0.584	0.584	0.582	0.804	0.732	0.582	0.583	0.583	0.623
	(0.009)	(0.008)	(0.009)	(0.009)	(0.010)	(0.009)	(0.009)	(0.010)	(0.010)	(0.009)	(0.011)	(0.011)	(0.009)	(0.009)	(0.009)	(0.011)
Mean control	0.773	0.181	0.044	0.215	0.562	0.584	0.184	0.403	0.098	0.031	0.423	0.448	0.598	905,908.90	0.992	14.056
Observations						297,279					23,059	41,429		297,279		146,252
BW loc. poly.	23.335	33.150	23.706	22.922	20.293	22.714	22.966	18.781	19.459	26.351	26.531	24.865	24.880	20.919	20.910	26.903
Effect obs. control	21,963	37,647	21,963	20,459	17,966	20,459	20,459	15,683	16,562	25,871	4,491	7,350	23,070	17,966	17,966	15,351
Effect obs. Treat	10,107	12,061	10,107	9,815	9,317	9,815	9,815	8,796	8,987	10,754	4,576	6,186	10,299	9,317	9,317	6,943
Panel B: *SISBEN is the running variable*																
IV	0.081	0.553	0.731	-0.146	0.227	0.124	-0.100	0.225	0.500	0.604	0.059	0.044	0.005	311,081.00	0.342	0.209
	(0.026)	(0.032)	(0.026)	(0.024)	(0.031)	(0.038)	(0.022)	(0.038)	(0.033)	(0.029)	(0.046)	(0.047)	(0.038)	(116,697.40)	(0.128)	(0.059)
First stage	0.634	0.634	0.634	0.633	0.635	0.634	0.634	0.635	0.635	0.633	0.801	0.740	0.634	0.635	0.635	0.671
	(0.018)	(0.018)	(0.018)	(0.019)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.019)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
Mean control	0.849	0.359	0.096	0.147	0.702	0.659	0.114	0.546	0.239	0.069	0.809	0.842	0.669	1,208,255.00	1.324	14.187
Observations						22,552					9,047	13,694		22,552		14,975
BW loc. poly.	8.305	8.428	8.278	7.792	8.604	8.135	8.075	8.663	8.589	7.361	12.116	10.209	8.278	8.483	8.480	11.142
Effect obs. control	3,560	3,613	3,552	3,337	3,686	3,481	3,466	3,721	3,682	3,180	1,572	2,386	3,552	3,640	3,640	3,113
Effect obs. Treat	3,544	3,598	3,527	3,331	3,688	3,475	3,450	3,720	3,681	3,137	2,376	2,851	3,527	3,630	3,630	3,257

Notes: This table presents the IV estimates on educational and labor market outcomes realized up to eight years after high school completion using an RD design. The outcomes in Columns (6)–(10) are measured within seven years from high school completion, while the outcomes in Columns (13)–(16) are measured exactly eight years after high school completion. See the notes under Table I for other details.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), SNIES (MEN), SABER PRO (ICFES), SABER T&T (ICFES), and PILA (MinSalud).

Table IV: Reduced-Form Estimates on Other Educational Outcomes

	Years of undergrad. study	Time to bachelor's degree attainment					Any graduate study	College exit test score if exam taken within…	
		Any college	High quality college			Low quality college		Five years	Seven years
			Any	Private	Public				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: *SABER 11 is the running variable*									
Reduced form	0.758	-0.125	-0.220	0.115	-0.154	0.070	0.005	0.096	0.056
	(0.063)	(0.038)	(0.074)	(0.123)	(0.104)	(0.073)	(0.002)	(0.021)	(0.019)
Mean control	3.319	5.213	5.277	4.920	5.467	5.193	0.008	0.423	0.448
Observations	297,279	22,476	8,800	6,484	2,316	13,676	297,279	23,059	41,429
BW loc. poly.	18.96	25.41	24.19	18.40	26.70	23.02	29.91	26.53	24.86
Effect obs. control	15,683	3,986	824	227	573	2,830	30,526	4,491	7,350
Effect obs. Treat	8,796	5,342	4,425	3,305	572	759	11,339	4,576	6,186
Panel B: *SISBEN is the running variable*									
Reduced form	0.507	-0.190	-0.234	0.024	-0.080	-0.001	0.016	0.057	0.033
	(0.113)	(0.062)	(0.081)	(0.114)	(0.126)	(0.116)	(0.007)	(0.040)	(0.035)
Mean control	3.836	5.234	5.253	4.934	5.406	5.226	0.016	0.804	0.843
Observations	22,552	10,691	8,253	6,309	1,944	2,438	22,552	9,047	13,694
BW loc. poly.	7.94	9.17	9.04	8.75	9.61	14.05	9.37	9.89	10.03
Effect obs. control	3,421	1,365	641	254	408	991	4,011	1,320	2,359
Effect obs. Treat	3,385	2,227	1,959	1,605	318	408	4,024	1,969	2,804

Notes: This table presents the reduced-form estimates on educational outcomes using an RD design. Column (1) reports the effects on the total years in undergraduate studies and assigns zeros for people who do not attend any undergraduate program within six years from high school. Columns (2)–(6) report effects on the number of years to obtain a bachelor's degree (proxied by taking the SABER PRO exam within seven years from high school), restricting the sample to students who attend college immediately after high school. Column (7) reports the effects on the likelihood of attending any graduate program within six years from high school. Finally, Columns (8) and (9) report effects on the SABER PRO test score for exams taken within five and seven years from high school completion, respectively. See the notes under Table I for other details.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), SNIES (MEN), and SABER PRO (ICFES).

Table V: Reduced-Form Estimates on Early-Career Labor Market Outcomes

	Formal	Formal earnings (includes zeros)						Time
	work	in constant pesos	in monthly min. wages	in natural logarithm	Percentile rank	Top 25%	Top 1%	to first formal job
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: *SABER 11 is the running variable*								
Reduced form	0.040	188,897.30	0.206	0.076	4.498	0.062	0.014	-0.496
	(0.012)	(31,591.41)	(0.035)	(0.021)	(1.030)	(0.013)	(0.004)	(0.119)
Mean control	0.598	905,908.90	0.992	14.056	47.109	0.348	0.014	3.744
Observations	297,279	297,279	146,252	146,252	297,279	297,279	297,279	31,463
BW loc. poly.	24.88	20.92	20.91	26.90	22.53	20.51	22.00	34.44
Effect obs. control	23,070	17,966	17,966	15,351	20,459	17,966	20,459	6,744
Effect obs. Treat	10,299	9,317	9,317	6,943	9,815	9,317	9,815	3,165
Panel B: *SISBEN is the running variable*								
Reduced form	0.008	194,399.70	0.213	0.235	3.191	0.056	0.005	-0.382
	(0.023)	(72,732.85)	(0.080)	(0.055)	(1.814)	(0.024)	(0.010)	(0.233)
Mean control	0.668	1,214,661.00	1.331	14.109	54.386	0.445	0.043	3.296
Observations	22,552	22,552	22,552	14,975	22,552	22,552	22,552	5,483
BW loc. poly.	9.20	8.93	8.93	5.58	10.99	9.82	8.59	9.62
Effect obs. control	3,954	3,811	3,811	1,639	4,579	4,171	3,682	738
Effect obs. Treat	3,973	3,841	3,841	1,639	4,690	4,212	3,681	1,191

Notes: This table presents the reduced-form estimates on early-career labor market outcomes using an RD design. The outcomes in Columns (1)–(7) are measured eight years after high school completion. Earnings are reported in December 2021 pesos. Converting COP to USD at the market exchange rate on December 31, 2021, the reduced form coefficient in Column (2) of Panel A is US$46.63 and the control mean is US$223.61 including zeros and US$373.79 excluding zeros. Column (8) reports the effects on the time to first formal job, measured in periods of four months since graduation according to SNIES. See the notes under Table I for other details.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), SNIES (MEN), and PILA (MinSalud).

Table VI: Impacts on Educational and Labor Market Outcomes and College-Program "Value Added"

	College attainment				College exit test score		Formal labor market outcomes			
	Any degree		Four-year degree				Employment		Earnings	
	Outcome	VA	Outcome	VA	Outcome	VA	Outcome	VA	Outcome	VA
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: *SABER 11 is the running variable*										
Reduced form	0.032	-0.010	0.062	0.009	0.054	0.108	0.044	0.025	0.223	0.137
	(0.013)	(0.004)	(0.016)	(0.004)	(0.019)	(0.007)	(0.013)	(0.003)	(0.036)	(0.011)
Observations	130,376	130,376	68,416	68,416	35,493	35,374	284,782	284,782	284,782	284,782
Panel B: *SISBEN is the running variable*										
Reduced form	0.079	0.002	0.082	0.021	0.021	0.064	-0.004	0.017	0.226	0.146
	(0.026)	(0.007)	(0.023)	(0.006)	(0.033)	(0.011)	(0.025)	(0.006)	(0.083)	(0.024)
Observations	19,471	19,471	17,605	17,605	12,488	12,466	21,219	21,219	21,219	21,219

Notes: This table compares the reduced-form estimates on educational and early-career labor market outcomes and those predicted by college-program "value-added" using an RD design. The outcomes are measured within seven years from high school completion in Columns (1) through (6) and eight years after high school completion in Columns (7) through (10). The dependent variable is the outcome of interest in odd columns and the associated college-program "value-added" in even columns. Formal earnings in Columns (9) and (10) are measured in multiples of the monthly minimum wage and have zeros for individuals not formally employed. See Appendix D and the notes under Table I for other details.

Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), SNIES (MEN), SABER PRO (ICFES), SABER T&T (ICFES), and PILA (MinSalud).

Table VII: Discounted Lifetime Benefits and Costs of the SPP Program and the MVPF

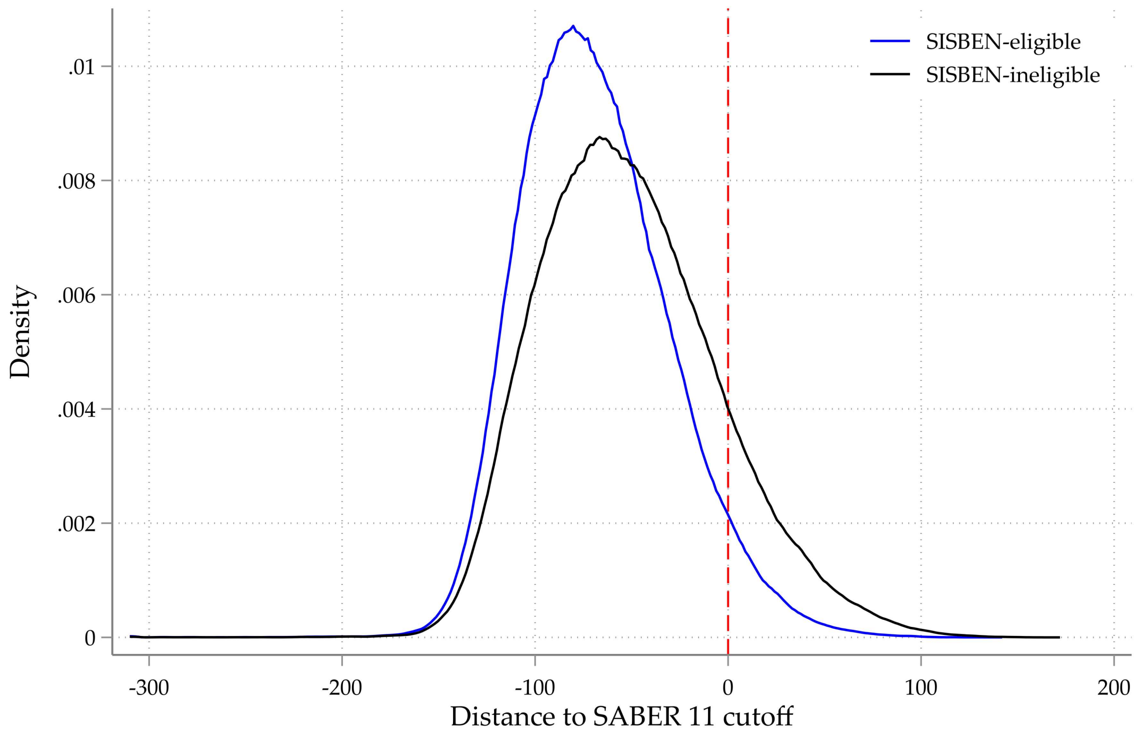
Years	Annual earnings		D		COA	
after high school completion	Mean control (1)	RD-IV estimate (2)	Mean control (3)	RD-IV estimate (4)	Mean control (5)	RD-IV estimate (6)
Panel A: *SABER 11 is the running variable*						
1	824,191	-586,135	0	18,245,360	5,244,523	12,645,545
2	1,954,522	-1,225,576	0	16,759,246	4,915,485	11,606,497
3	2,860,744	-1,935,274	0	17,021,608	5,121,074	11,538,549
4	3,560,159	-1,956,422	0	16,564,866	4,896,173	11,312,535
5	4,625,874	-673,952	0	12,050,541	4,301,969	7,494,951
6	5,753,196	743,385	0	2,124,008	3,159,448	-833,290
7	8,113,900	2,820,266	0	91,675		
8	10,870,907	3,885,149	0	0		
Lifetime		110,283,598		78,335,884		51,083,113
WTP / Costs		167,665,598		57,382,000		30,129,230
MVPF				2.92		5.56
Panel B: *SISBEN is the running variable*						
1	573,210	-275,993†	101,793.70	19,762,078	7,181,810	11,847,911
2	1,495,811	-424,379†	123,586.60	18,557,160	6,576,327	11,134,620
3	2,380,560	-1,120,974	89,005.48	18,829,610	6,619,477	11,433,690
4	2,929,795	-1,187,153	127,393.90	18,137,244	6,734,639	10,689,724
5	4,388,284	665,341†	104,272.60	13,313,946	5,808,413	6,936,964
6	6,640,765	1,141,438†	43,930.00	2,128,963	4,117,933	-1,848,626
7	10,683,776	3,074,324	14,223.06	406,543		
8	14,499,060	3,732,972	-	74,207		
Lifetime		82,790,624		86,192,140		47,786,944
WTP / Costs		153,252,546		70,461,922		32,056,726
MVPF				2.17		4.78

Notes: This table presents the IV estimates on annual earnings, D_i, and COA_i, as described in the main text, one to eight years following high school graduation using an RD design. Lifetime earnings and costs are discounted back to year one at a rate of 3%. Willingness to pay and costs assume that incremental earnings are subject to a 19% tax rate. Columns (1), (3), and (5) present the average outcomes for the control group. In Column (3) of Panel A, D_i is zero because no student received SPP without meeting the SABER 11 requirement. Conversely, in Panel B, it is positive but relatively small because a few students received SPP without meeting the SISBEN condition. † denotes not statistically significant at the 10% level. See the main text and the notes under Table I for other details.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), SNIES (MEN), SPP (ICETEX), institutional financial accounts and balance sheets (MEN), and PILA (MinSalud).

Online Appendix

Appendix A Additional Figures and Tables

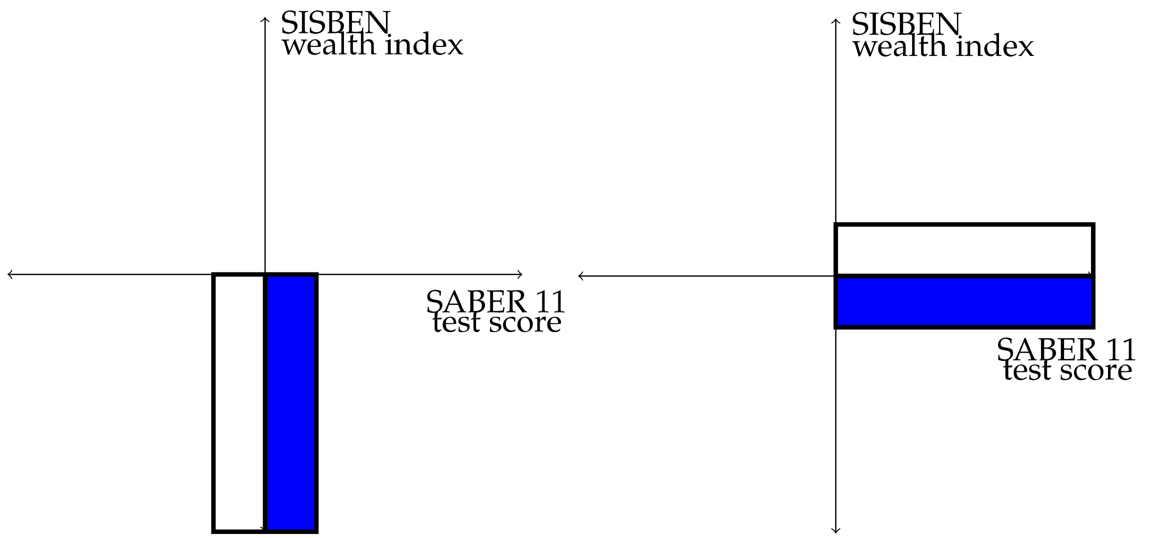
Figure A.1: The Distribution of SABER 11 Test Scores for SISBEN-Eligible and SISBEN-Ineligible Students



Notes: The figure plots the distribution of SABER 11 test scores separately for students who are need-eligible and need-ineligible for the SPP financial aid program based on their SISBEN score. The red dashed line marks SPP's SABER 11 test score cutoff.
Sources: Authors' calculations based on SABER 11 (ICFES) and SISBEN (DNP).

Figure A.2: Illustration of the Two Types of Compliers

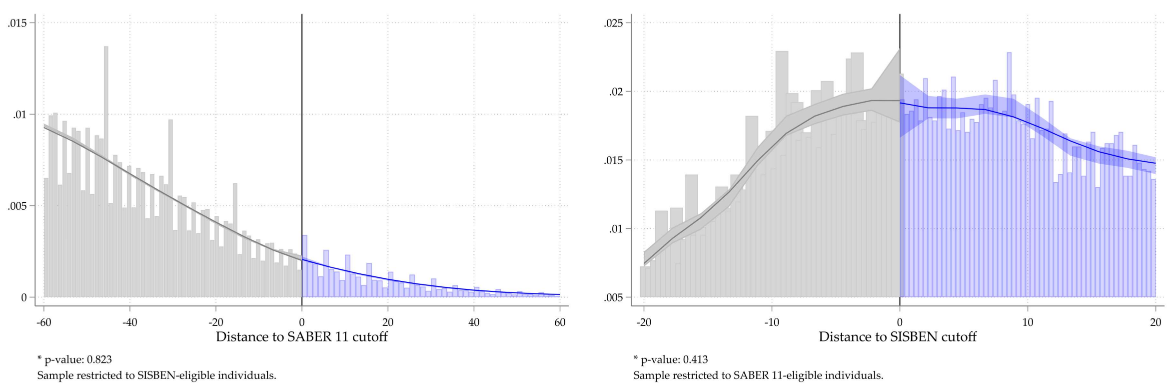
(a) SABER 11 as the running variable (b) SISBEN as the running variable



Notes: This figure depicts the two types of compliers in the SPP financial aid program. Panel A compares need-eligible students who are barely merit-eligible (blue) and merit-ineligible (white). Panel B compares merit-eligible students who are barely need-eligible (blue) and need-ineligible (white).

Figure A.3: Manipulation Testing based on Density Discontinuity

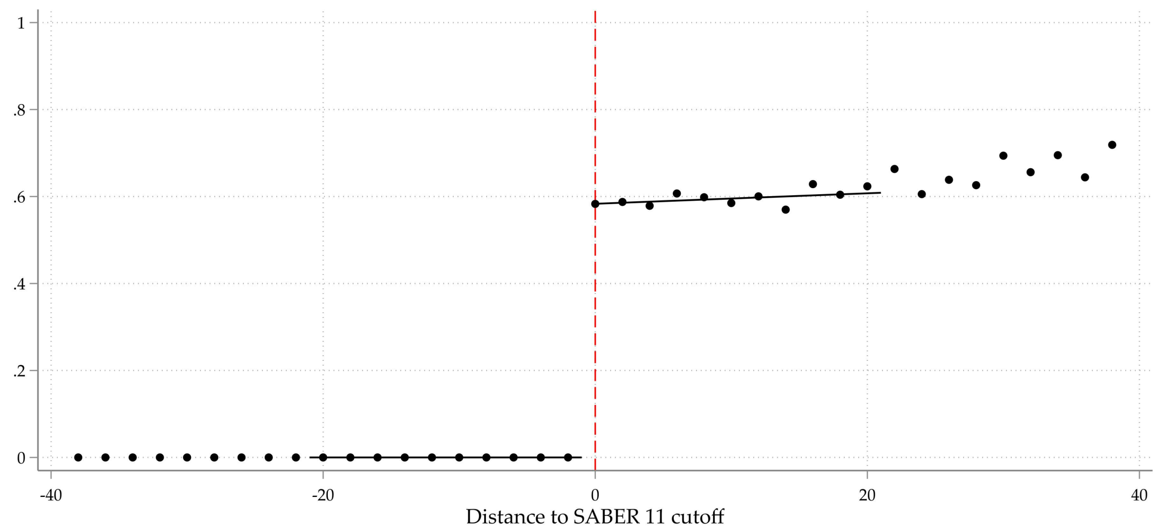
(a) SABER 11 as the running variable (b) SISBEN as the running variable



Notes: This figure tests for manipulation of the running variable based on density discontinuity. All results are estimated with package rddensity (Cattaneo et al., 2018) using an unrestricted model and a triangular kernel function, and employ the jackknife standard errors estimator. Panel A restricts the sample to SISBEN-eligible individuals. Panel B restricts the sample to SABER 11-eligible individuals. The p-values suggest we cannot statistically detect manipulation in either variable.
Sources: Authors' calculations based on SABER 11 (ICFES) and SISBEN (DNP).

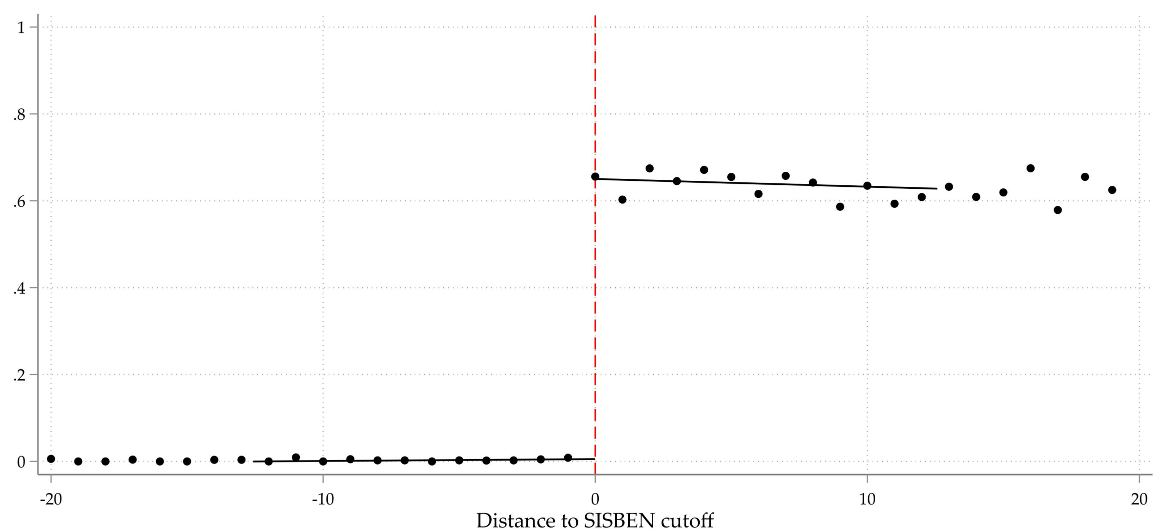
Figure A.4: Discontinuity in the Probability of Receiving SPP Financial Aid

(a) Merit-Based Eligibility



Sample restricted to SISBEN-eligible individuals.

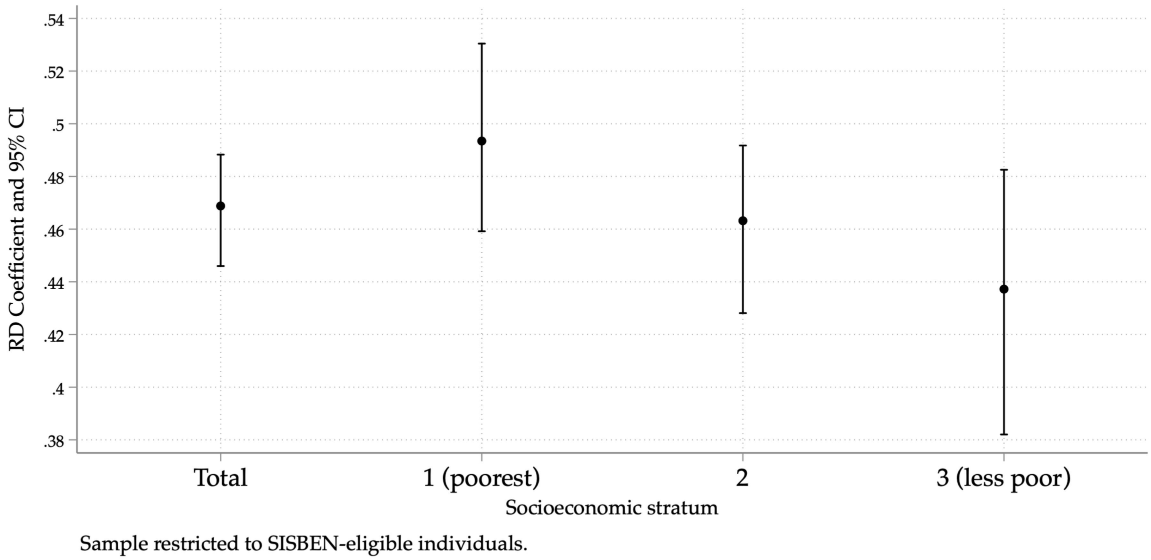
(b) Need-Based Eligibility



Sample restricted to SABER 11-eligible individuals.

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 need- and merit-eligible students, respectively. The probability of being a SPP recipient increases from 0% to 58.3% using SABER 11 as the running variable (Panel A) and from 0% to 64.5% using SISBEN as the running variable (Panel B). Sample average within bin. The line is plotted for the optimal bandwidth (Cattaneo et al., 2014).
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and ICETEX.

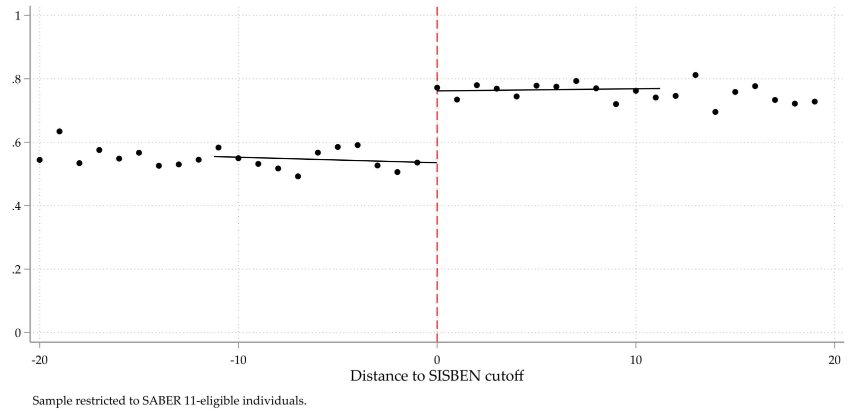
Figure A.5: The Effect of Financial Aid on HQ College Access by SES (Test Score Cutoff)



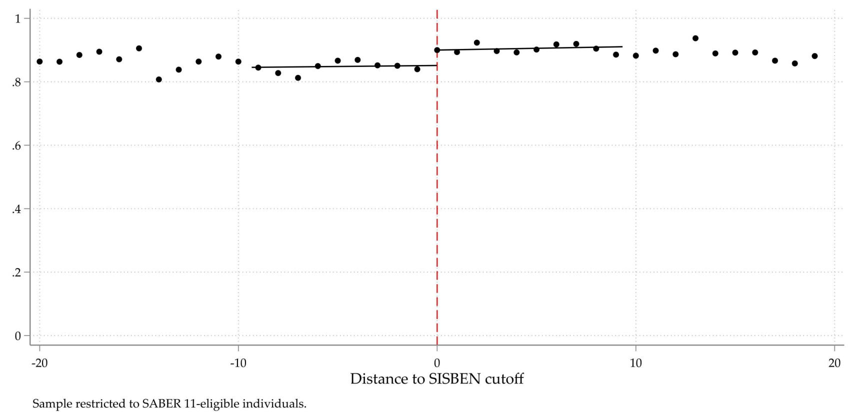
Notes: This figure compares the reduced-form RD coefficient and 95% confidence intervals by socioeconomic status. The running variable is the SABER 11 test score, and the outcome is the likelihood of accessing an HQ college immediately after high school. The sample is restricted to SISBEN-eligible individuals. The term "socioeconomic stratum" refers to Colombia's socioeconomic stratification system (*estratos*), which categorizes households based on their affluence using neighborhood and dwelling characteristics. Stratum 1 corresponds to the poorest households. More than 99% of SISBEN-eligible individuals belong to strata 1, 2, and 3.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SNIES (MEN).

Figure A.6: The Effect of Financial Aid on College Access (Wealth Cutoff)

(a) Probability of Attending College Immediately After High School



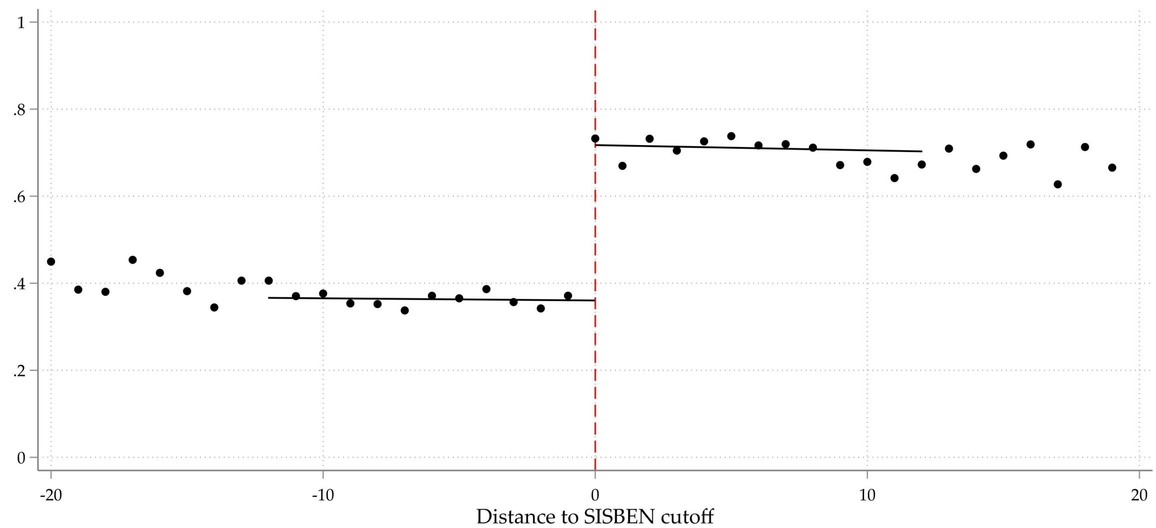
(b) Probability of Ever Attending College



Notes: Panels A and B plot the probability of ever attending college within zero and six years after high school completion, respectively, as a function of the distance to the wealth cutoff (for merit-eligible students). Table I reports the reduced-form RD estimates.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SNIES (MEN).

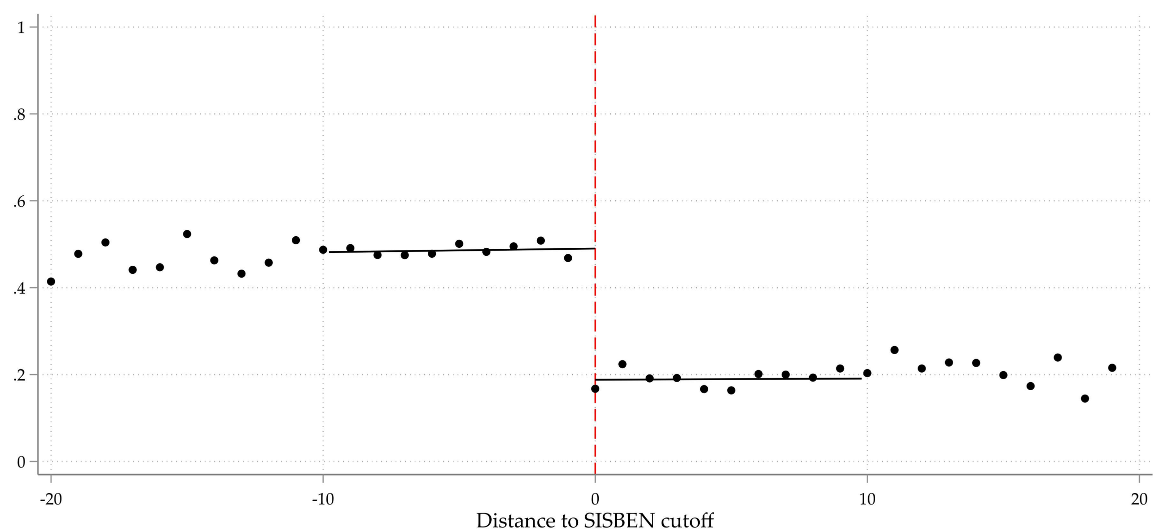
Figure A.7: The Effect of Financial Aid on College Quality (Wealth Cutoff)

(a) High-Quality College



Sample restricted to SABER 11-eligible individuals.

(b) Low-Quality College

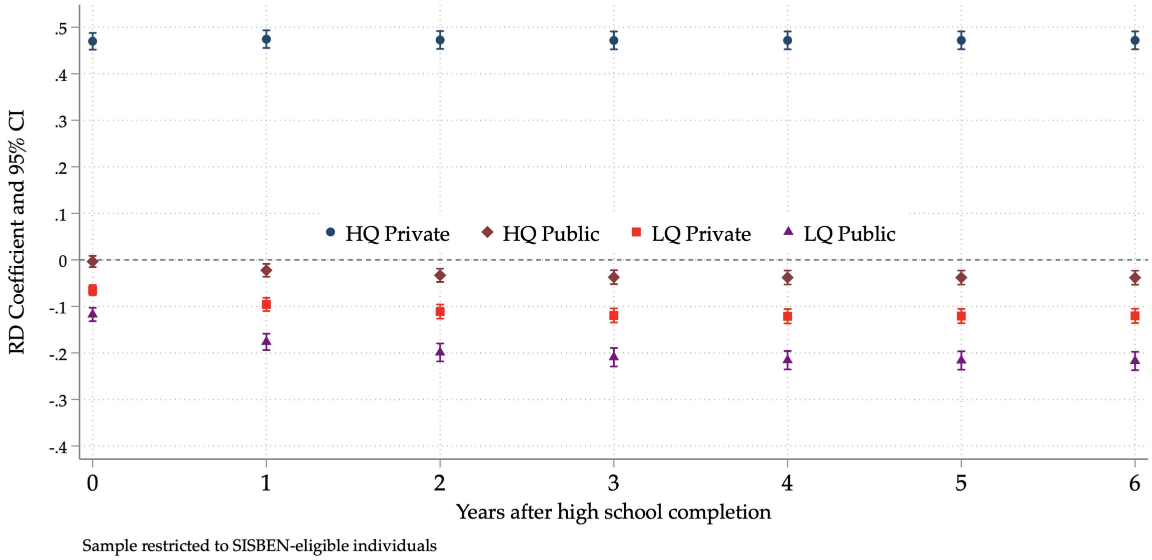


Sample restricted to SABER 11-eligible individuals.

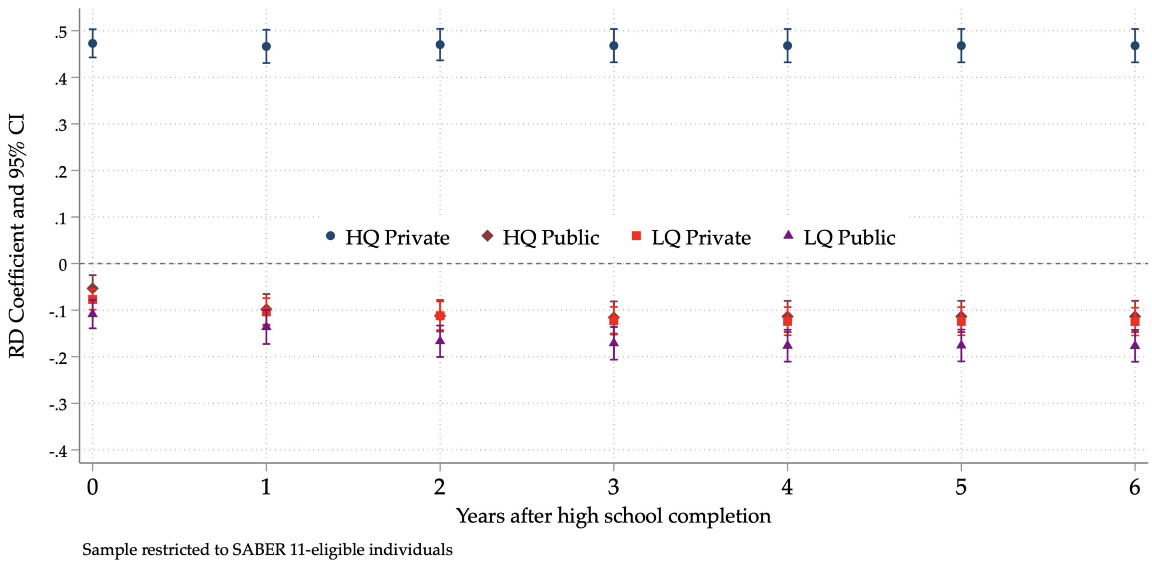
Notes: The figures decompose the enrollment effect six years after high school by college quality based on the need discontinuity (for merit-eligible students). Panels A and B plot the probability of ever attending a high- and low-quality college, respectively. Figure A.10 plots the RD coefficient over time. Table I reports the reduced-form estimates.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SNIES (MEN).

Figure A.8: Enrollment by College Type: High- vs. Low-Quality and Private vs. Public

(a) Test Score Cutoff



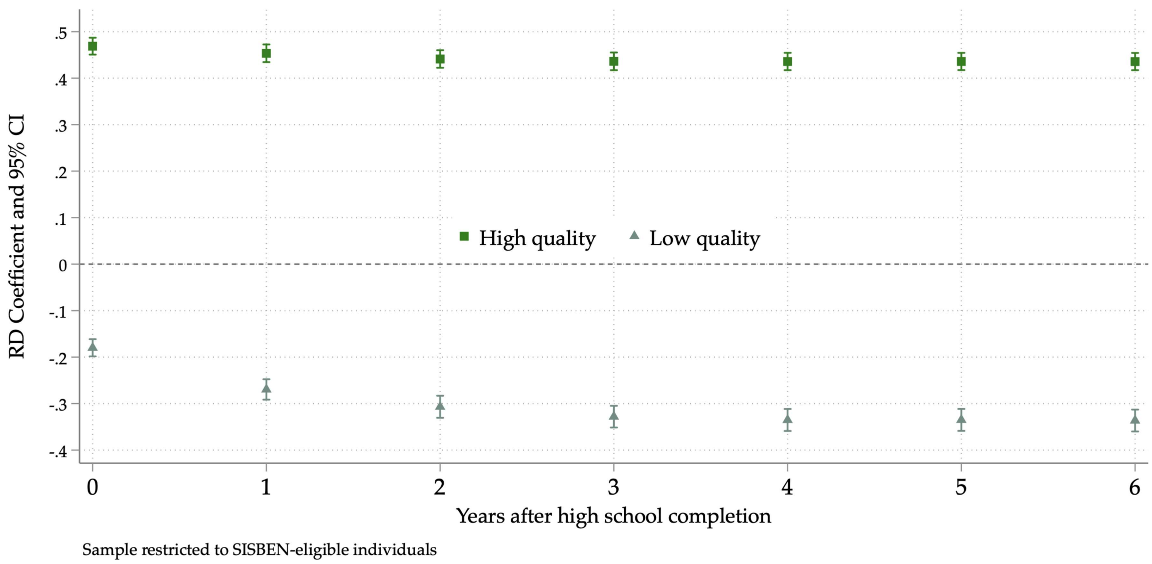
(b) Wealth Cutoff



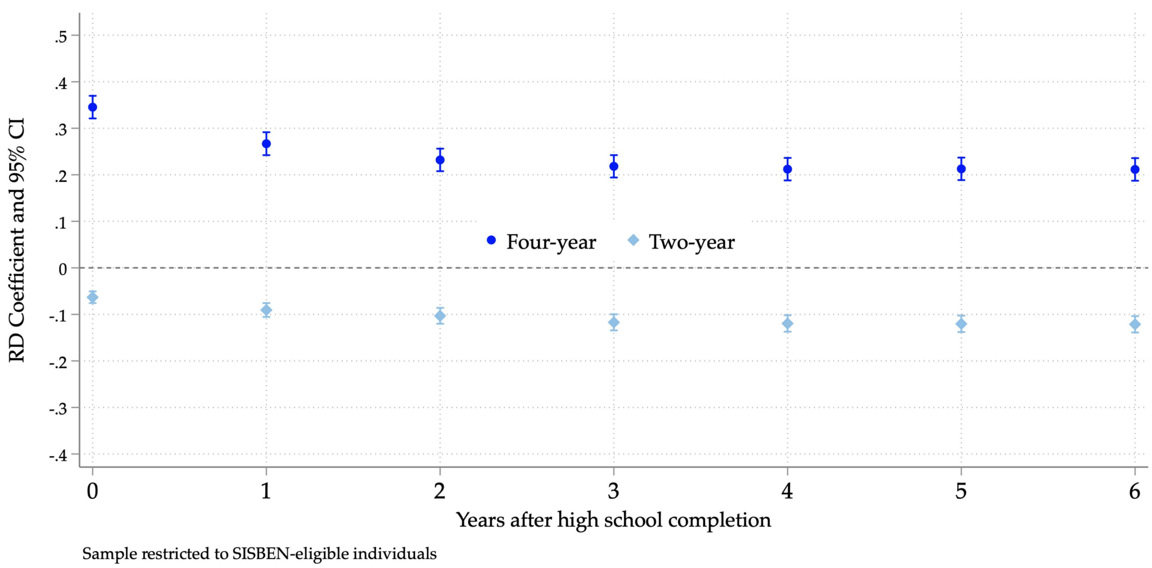
Notes: The figures decompose the enrollment effects over time by college quality and whether the institution is public or private. Panel A plots the RD coefficient based on the test score discontinuity (for need-eligible students), while Panel B plots the RD coefficient based on the need discontinuity (for merit-eligible students). Table I reports the reduced-form estimates.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SNIES (MEN).

Figure A.9: Enrollment by College Quality and Program Duration (Test Score Cutoff)

(a) High- versus Low-Quality College



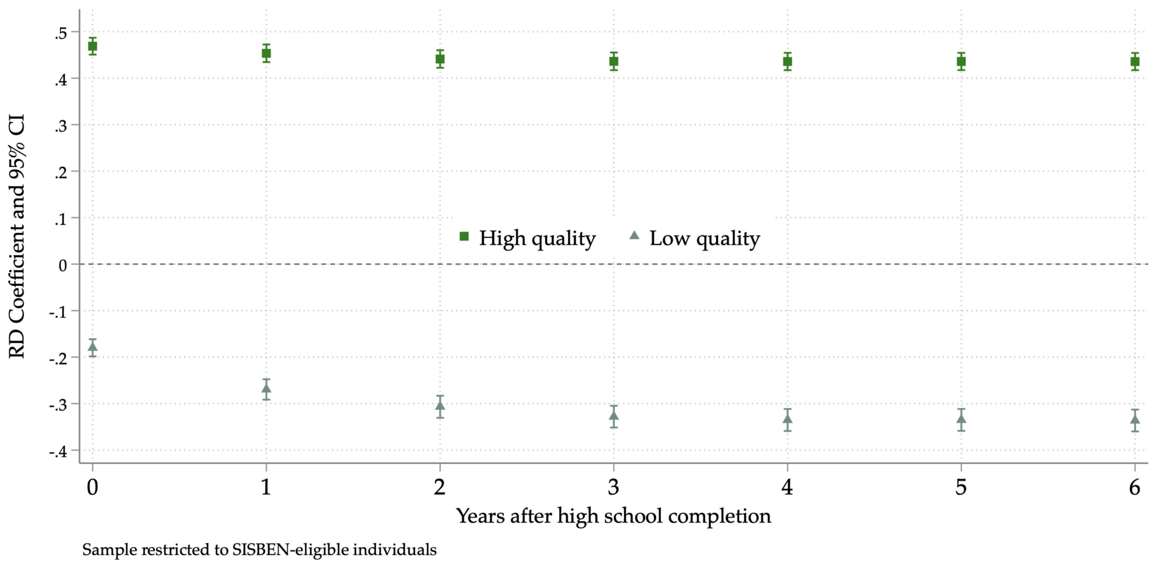
(b) Four- (or five-)year program versus two- (or three-)year program



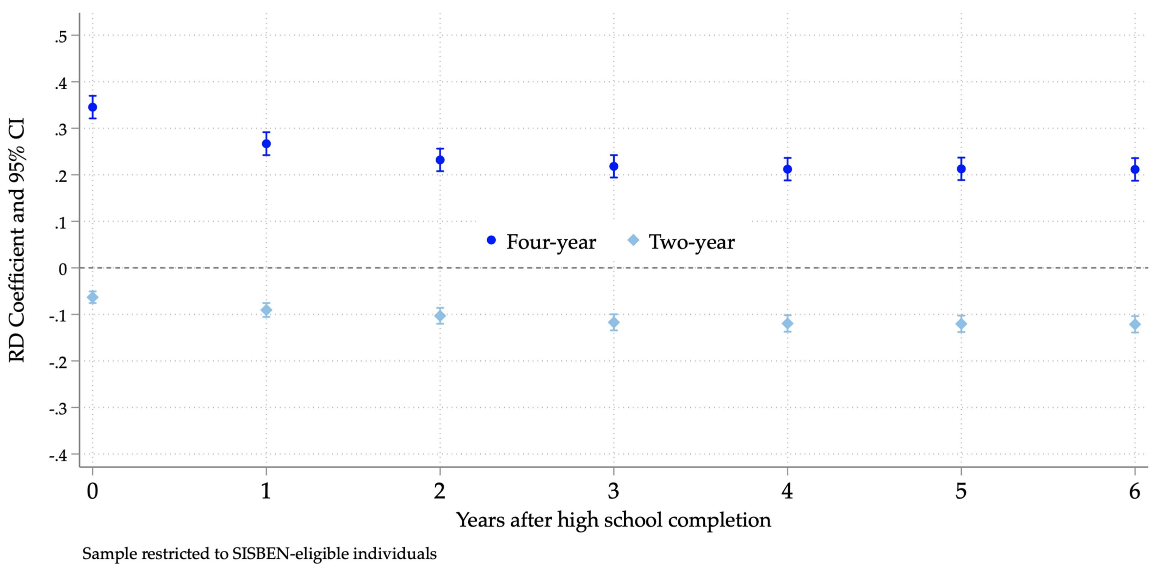
Notes: The figures decompose the enrollment effects over time from Figure II by college quality and program duration based on the test score discontinuity for low-SES students. Panel A plots the RD coefficient on the probability of ever attending a high- or low-quality college. Panel B plots the RD coefficients on the probability of ever attend a four- (or five-)year program or a two- (or three-)year program. Figure A.10 shows similar effects using SISBEN as the running variable. Table I reports the reduced-form estimates.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SNIES (MEN).

Figure A.10: Enrollment by College Quality and Program Duration (Wealth Cutoff)

(a) High- versus Low-Quality College



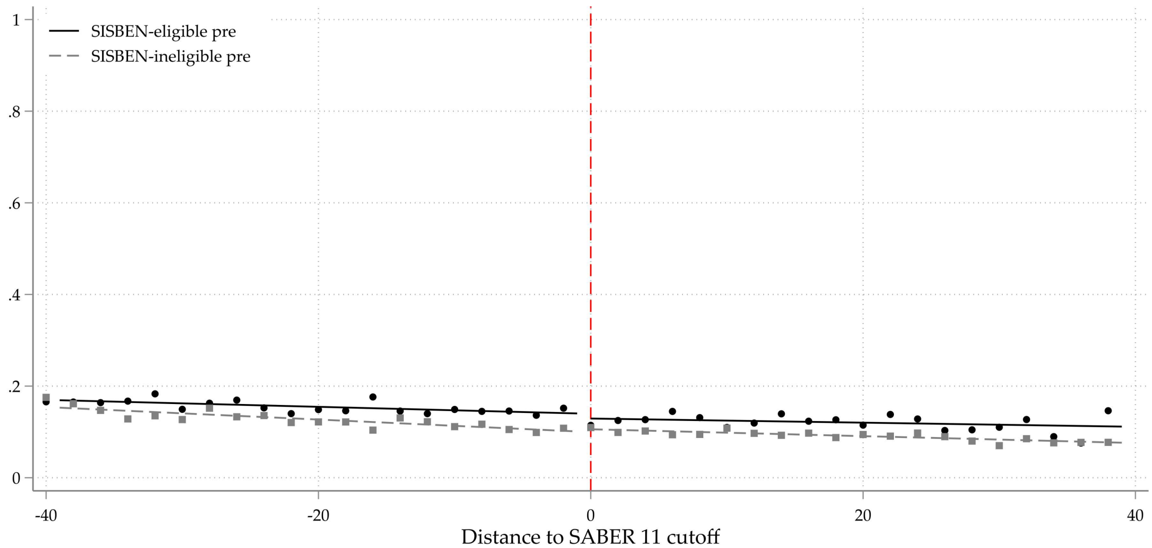
(b) Four- (or five-)year program versus two- (or three-)year program



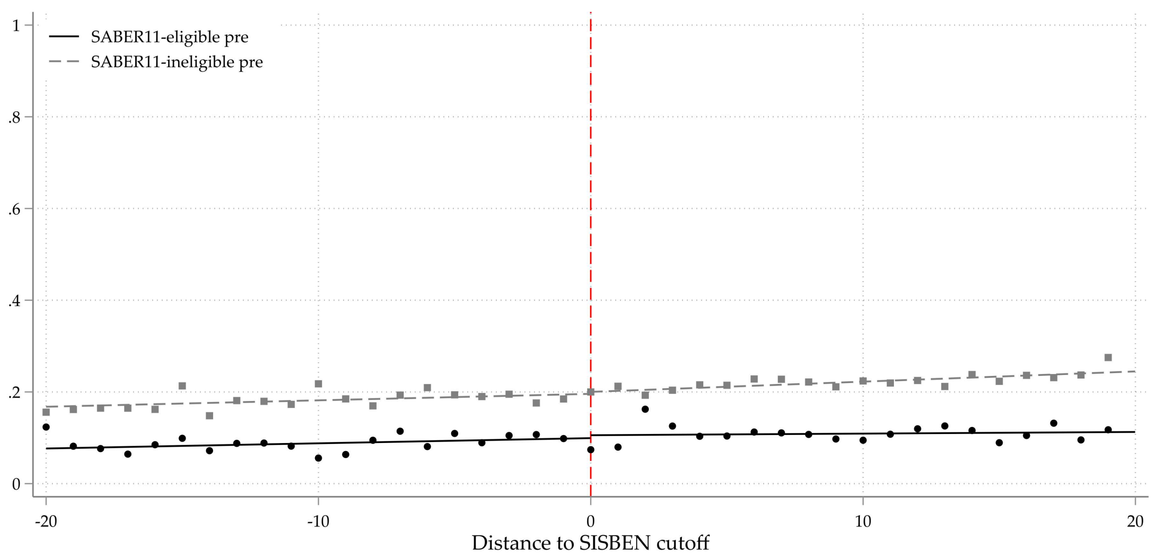
Notes: The figures decompose the enrollment effects over time from Figure A.6 by college quality and program duration based on the need discontinuity (for merit-eligible students). Panel A plots the RD coefficient on the probability of ever attending a high- or low-quality college. Panel B plots the RD coefficients on the probability of ever attend a four- (or five-)year program or a two- (or three-)year program. Figure A.9 shows similar effects using SABER 11 as the running variable. Table I reports the reduced-form estimates.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SNIES (MEN).

Figure A.11: The Likelihood of Dropping Out from a Bachelor's Program

(a) Test Score Cutoff



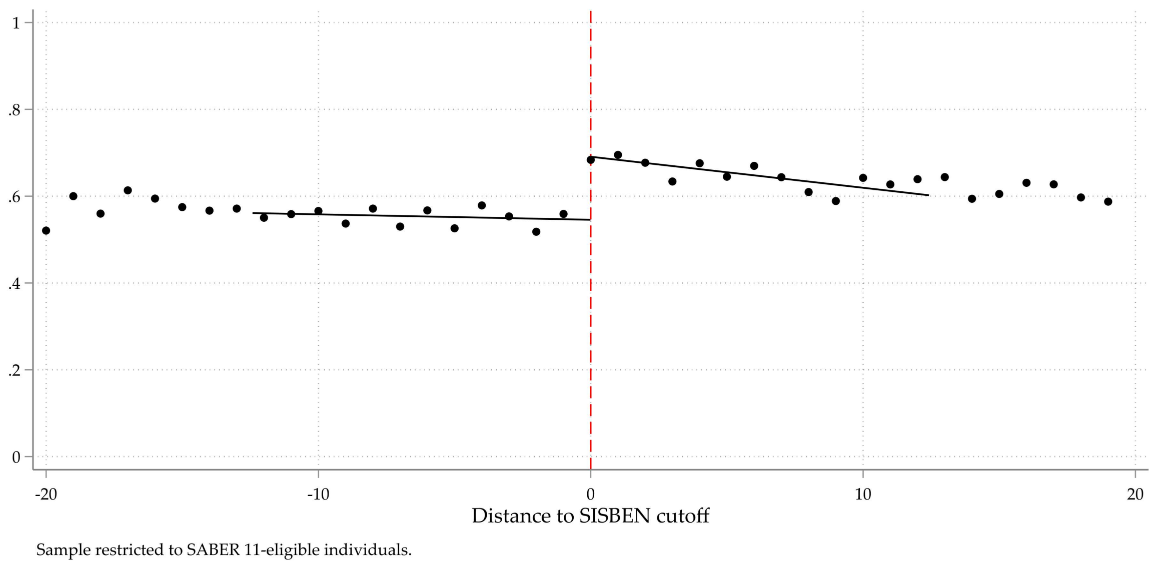
(b) Need Cutofff



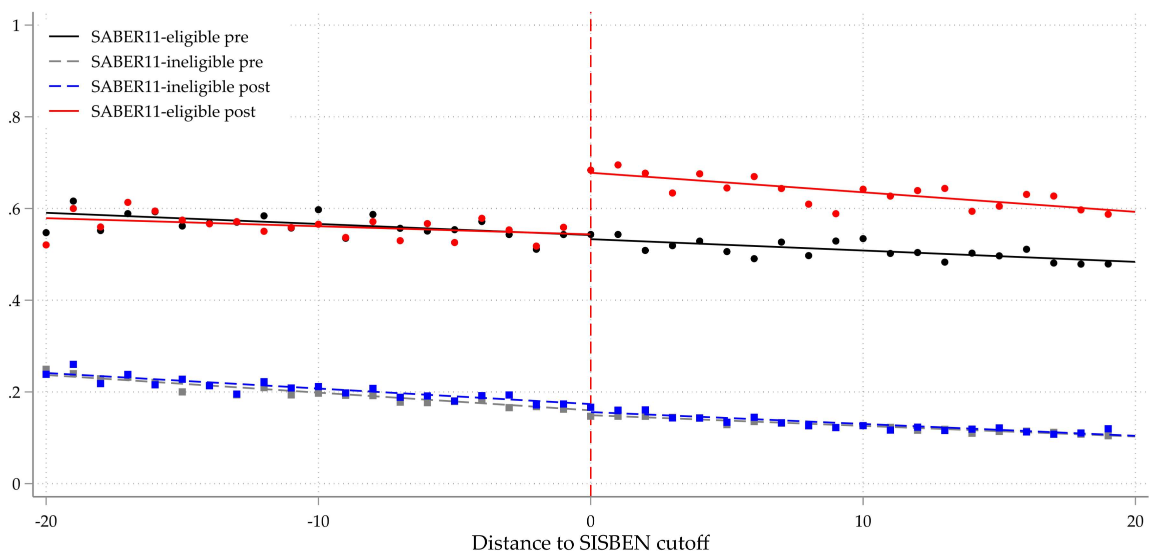
Notes: The figures plot the likelihood of dropping out of a bachelor's program as a function of the distance to the test score and need cutoffs in Panels A and B, respectively. The sample is restricted to individuals who took SABER 11 in the fall of 2012 or 2013 (i.e., before the expansion of financial aid) and accessed a bachelor's program immediately after high school. The figures compare the outcome for aid-eligible and -ineligible students in black and gray, respectively.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SABER PRO (ICFES).

Figure A.12: Bachelor's Degree Earned Within Seven Years from High School

(a) Wealth Cutoff



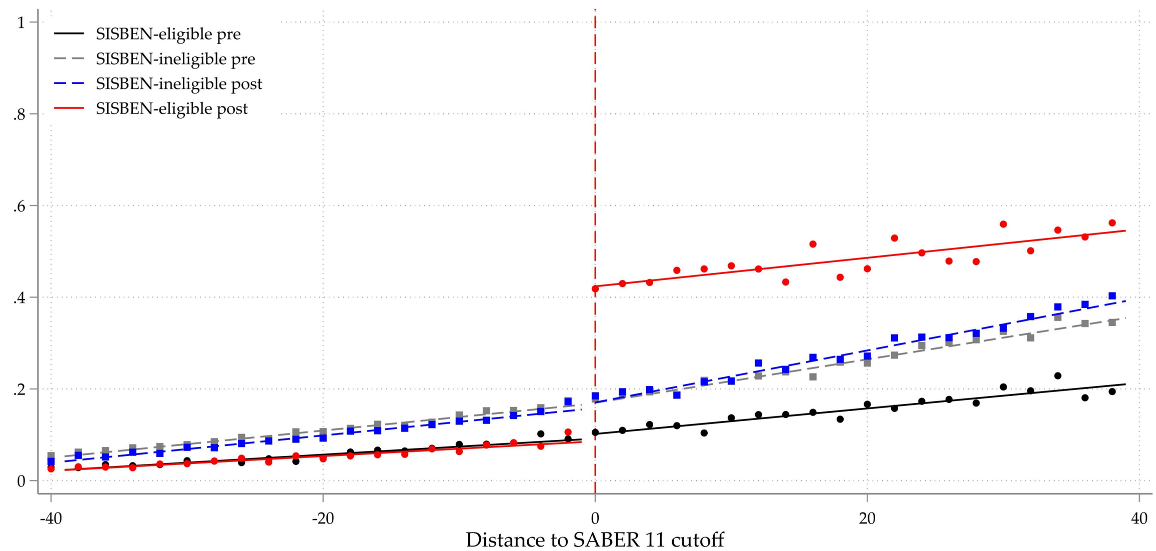
(b) Placebo



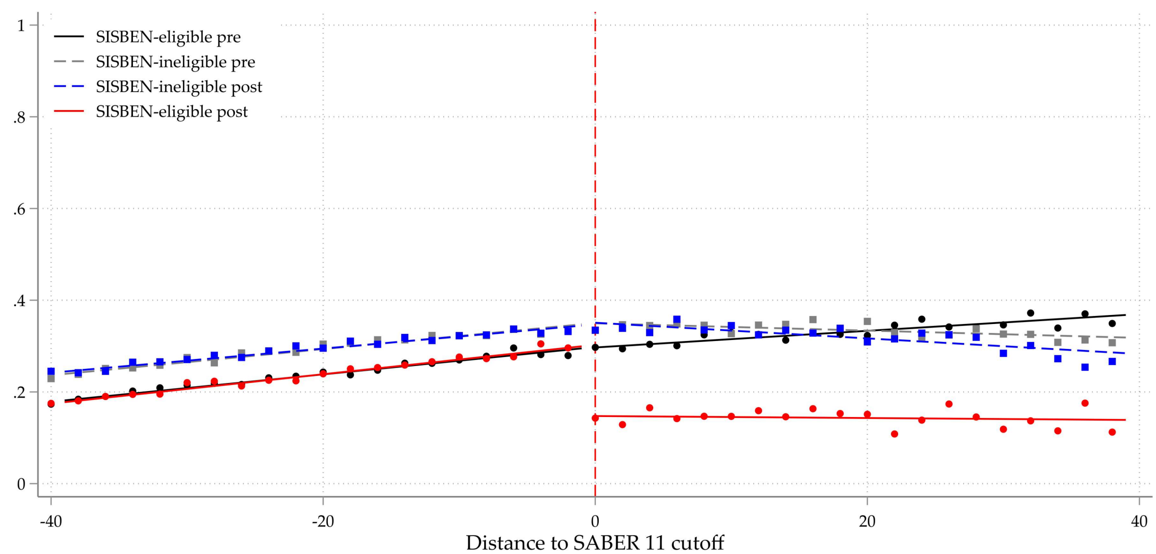
Notes: The figures plot the likelihood of earning a bachelor's degree (proxied by taking the SABER PRO exam) within seven years from high school completion as a function of the distance to the wealth cutoff. Panel A restricts the sample to merit-eligible students (Table II reports the reduced-form estimate). Panel B compares that series (in red) with a placebo series of SABER 11-eligible students from 2012 and 2013 (in black), and SABER 11-ineligible students before and after the program (in gray and blue, respectively).
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SABER PRO (ICFES).

Figure A.13: Bachelor's Degree Attainment by College Quality (Test Score Cutoff)

(a) High Quality



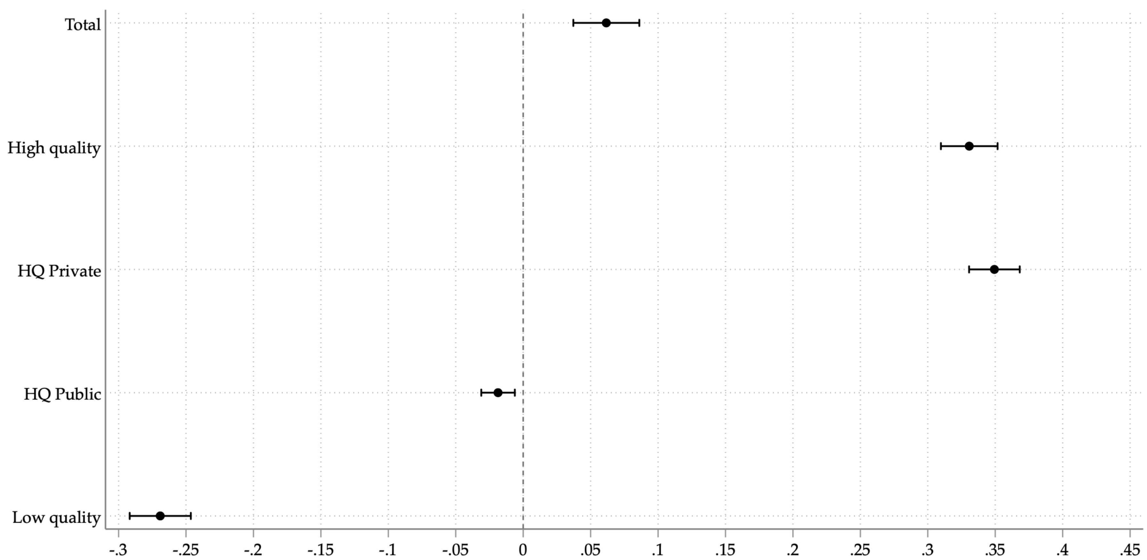
(b) Low Quality



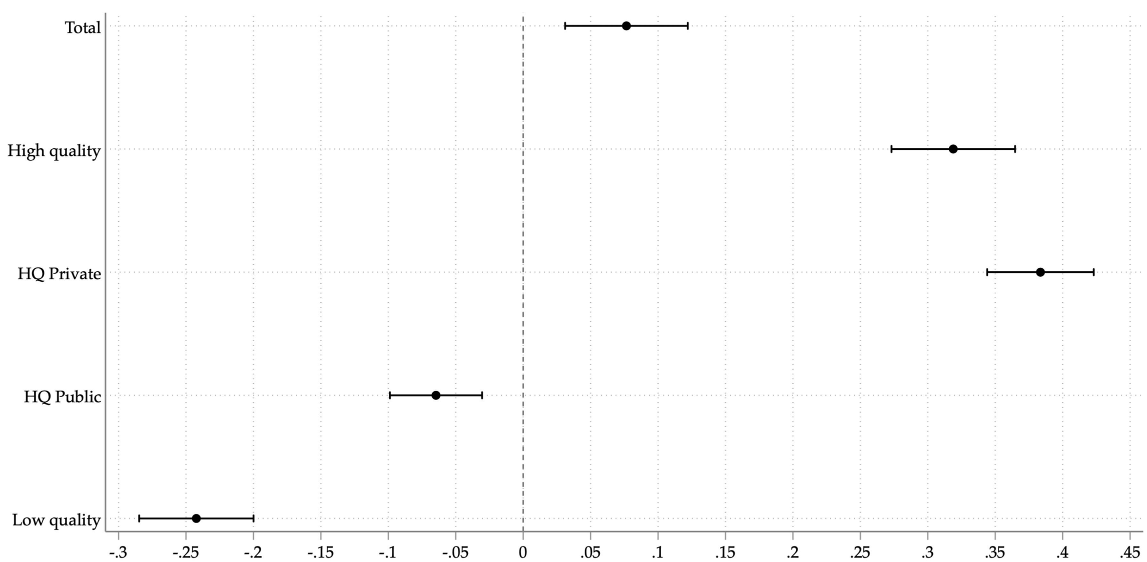
Notes: The figures decompose bachelor's degree attainment (proxied by taking the SABER PRO exam) by high- and low-quality colleges in Panels A and B, respectively. The figures show the equity implications of expanding financial aid by comparing low-SES students from 2014 (in red) and three placebo series: SISBEN-eligible and SISBEN-ineligible students from 2012 and 2013, before the SPP program (in black and gray, respectively) and SISBEN-ineligible students in 2014 (in blue). SISBEN-ineligible students are those whose SISBEN score is above SPP's eligibility cutoff and those without a SISBEN score. Table II reports the reduced-form estimates.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SABER PRO (ICFES).

Figure A.14: The Increase in Any Degree Attainment is Driven by HQ Private Colleges

(a) Test Score Cutoff



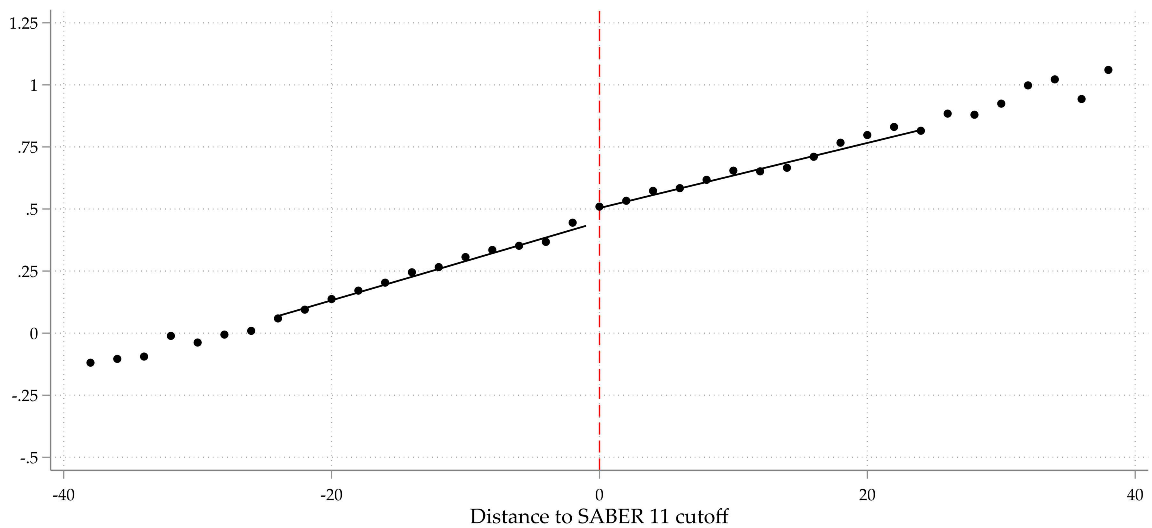
(b) Wealth Cutoff



Notes: The figures plot the RD coefficients and 95% confidence intervals, decomposing any degree attainment (proxied by taking the SABER PRO or SABER T&T exams) by HQ, LQ, private, and public colleges. Panel A (B) uses SABER 11 (SISBEN) as the running variable and restricts the sample to need- (merit-) eligible students. The bandwidth selected by Cattaneo et al. (2014) for "Total" is 22.71 (8.72) in Panel A (B), and we use this bandwidth for all subcategories, so they add up to the "Total" coefficient. Table II reports the reduced-form estimates when the bandwidth is not fixed.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), SABER PRO (ICFES), and SABER T&T (ICFES).

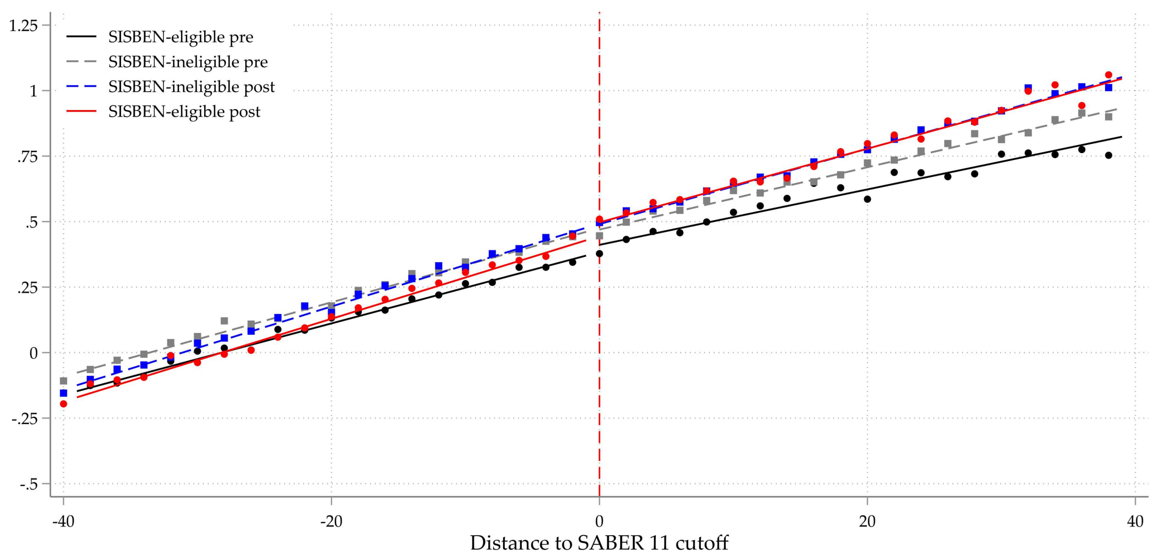
Figure A.15: Standardized College Exit Test Score Within Seven Years

(a) Test Score Cutoff



Sample restricted to SISBEN-eligible individuals.

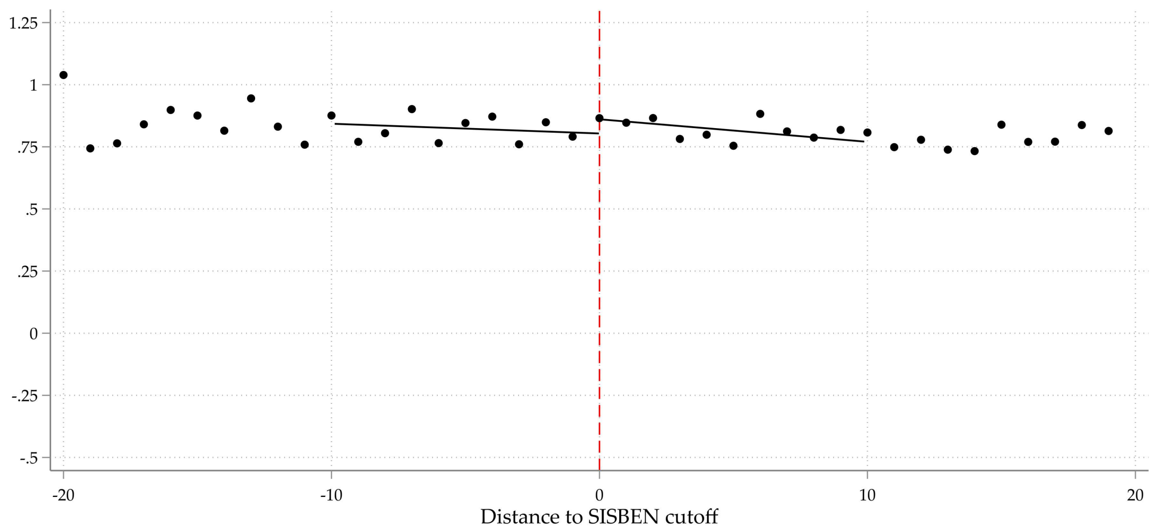
(b) Impact on Equity and Placebo



Notes: The figures plot students' performance in Colombia's mandatory standardized college exit exam, SABER PRO, within seven years from high school completion as a function of the distance to the test score cutoff. Panel A restricts the sample to low-SES students (Table IV reports the reduced-form estimate). Panel B compares that series (in red) with several placebo series: SISBEN-eligible and SISBEN-ineligible students in 2012 and 2013 (in black and gray, respectively), and SISBEN-ineligible students in 2014 (in blue).
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SABER PRO (ICFES).

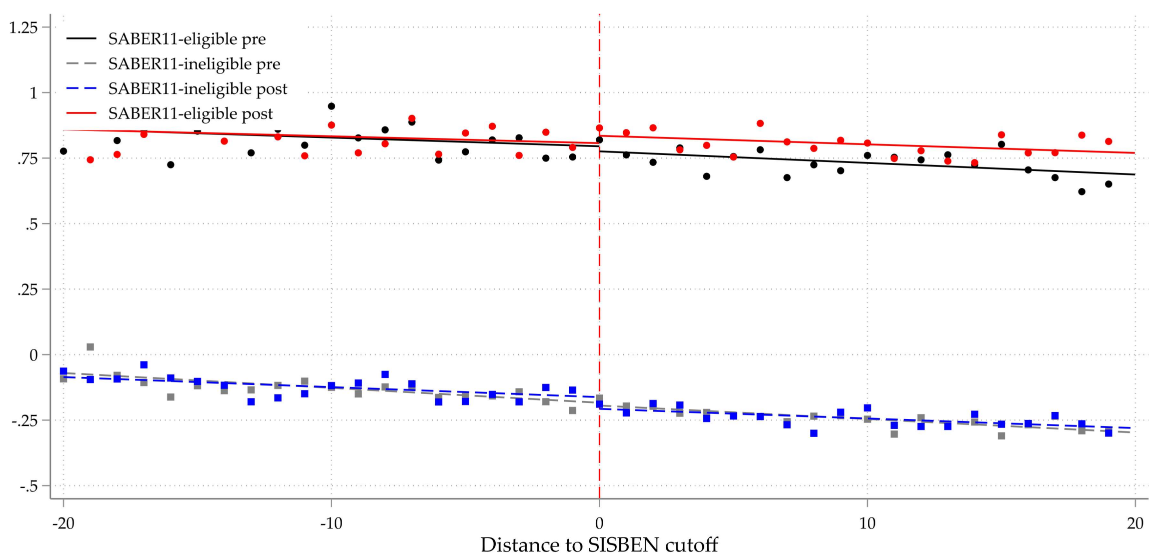
Figure A.16: Standardized College Exit Test Score Within Five Years

(a) Wealth Cutoff



Sample restricted to SABER 11-eligible individuals.

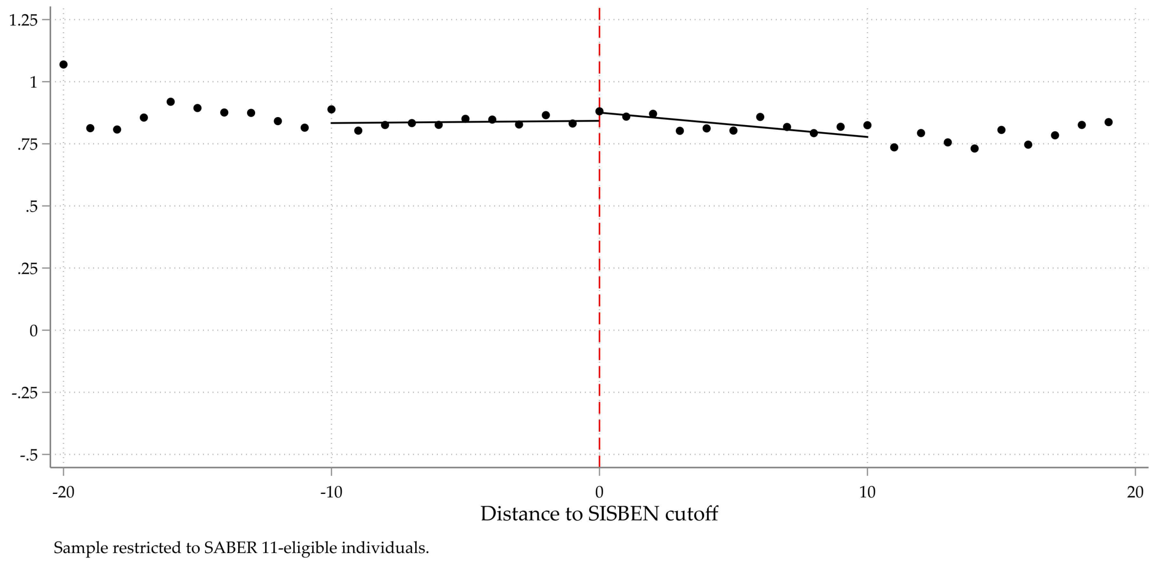
(b) Impact on Equity and Placebo



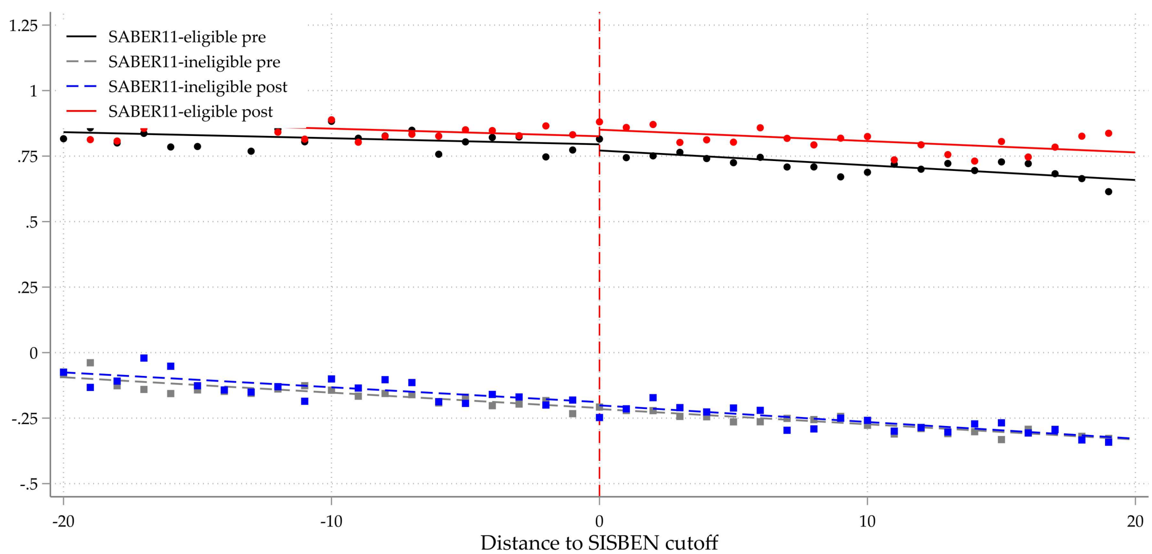
Notes: The figures plot students' performance in Colombia's mandatory standardized college exit exam, SABER PRO, within five years from high school completion as a function of the distance to the wealth cutoff. Panel A restricts the sample to merit-eligible students (Table IV reports the reduced-form estimate). Panel B compares that series (in red) with several placebo series: SABER 11-eligible and SABER 11-ineligible students in 2012 and 2013 (in black and gray, respectively), and SABER 11-ineligible students in 2014 (in blue).
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SABER PRO (ICFES).

Figure A.17: Standardized College Exit Test Score Within Seven Years

(a) Wealth Cutoff



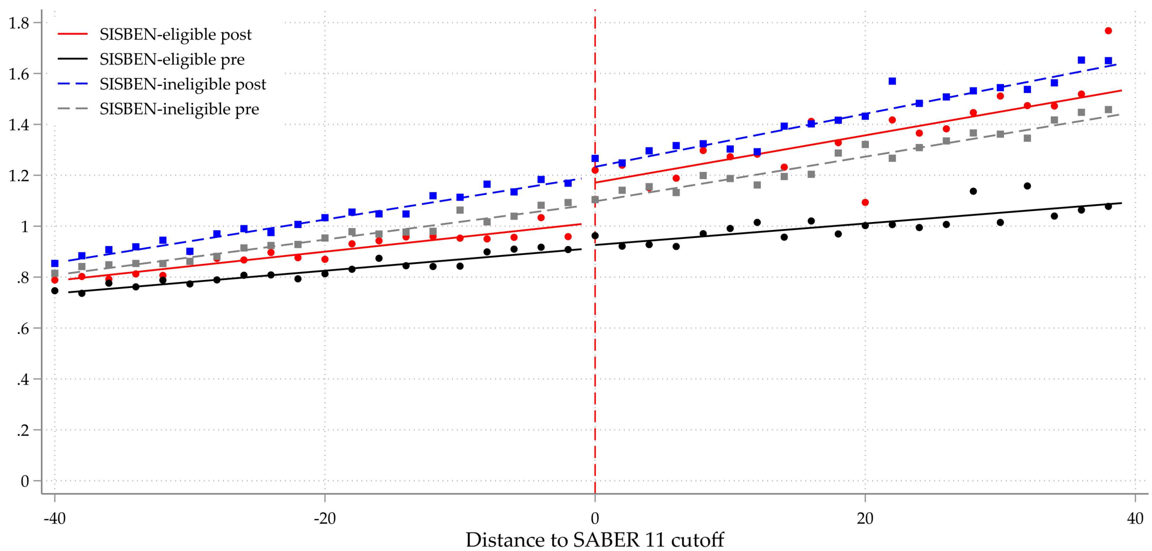
(b) Impact on Equity and Placebo



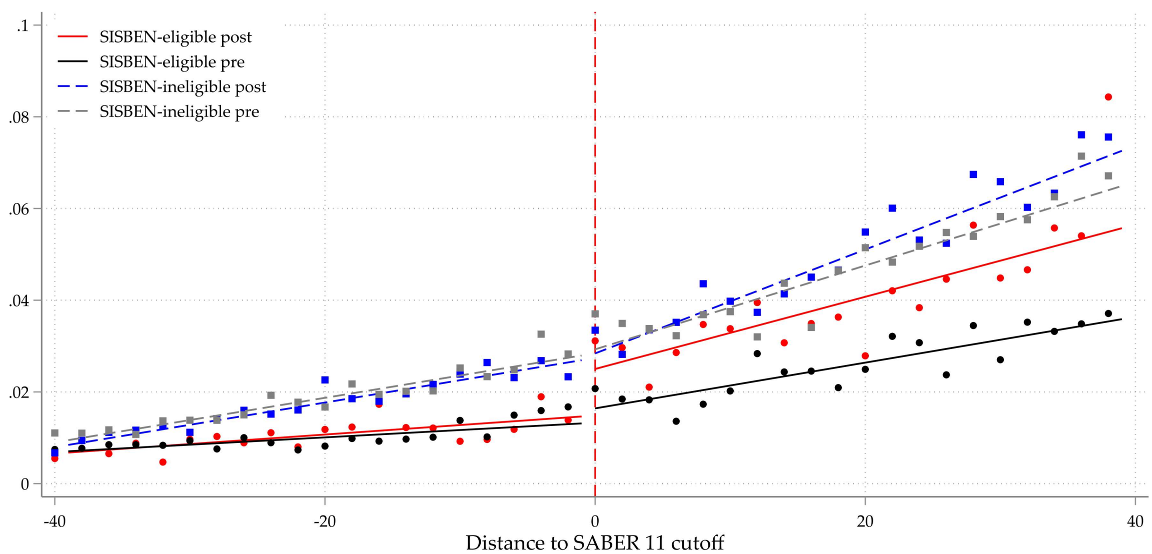
Notes: The figures plot students' performance in Colombia's mandatory standardized college exit exam, SABER PRO, within seven years from high school completion as a function of the distance to the wealth cutoff. Panel A restricts the sample to merit-eligible students (Table IV reports the reduced-form estimate). Panel B compares that series (in red) with several placebo series: SABER 11-eligible and SABER 11-ineligible students in 2012 and 2013 (in black and gray, respectively), and SABER 11-ineligible students in 2014 (in blue).
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SABER PRO (ICFES).

Figure A.18: Impacts on Earnings Equity Including 2012 in the Comparison Group

(a) Monthly Earnings



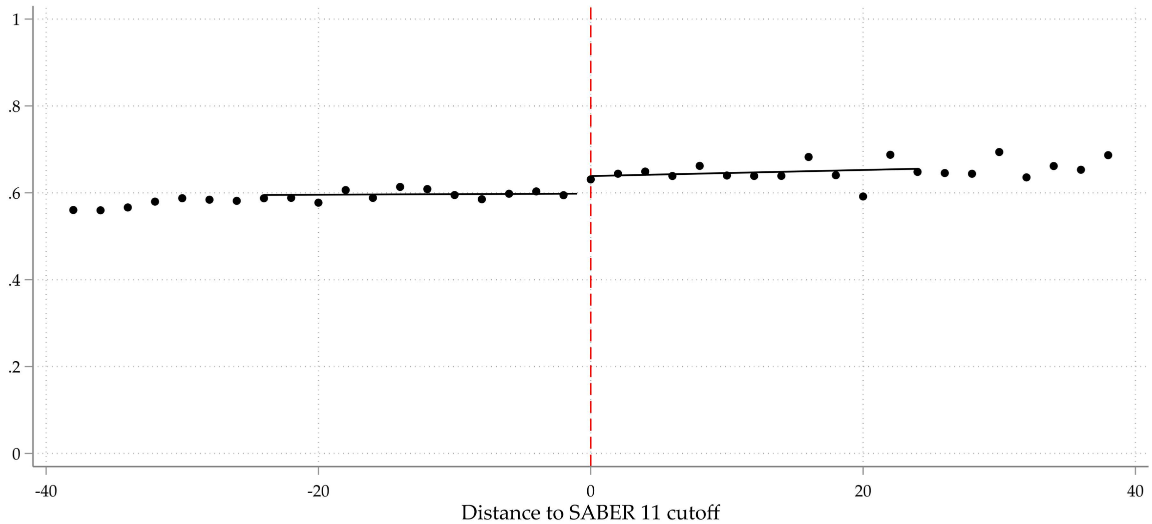
(b) Top 1% of Earnings



Notes: This figure reproduces VII including the 2012 cohort in the comparison group.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and PILA (MinSalud).

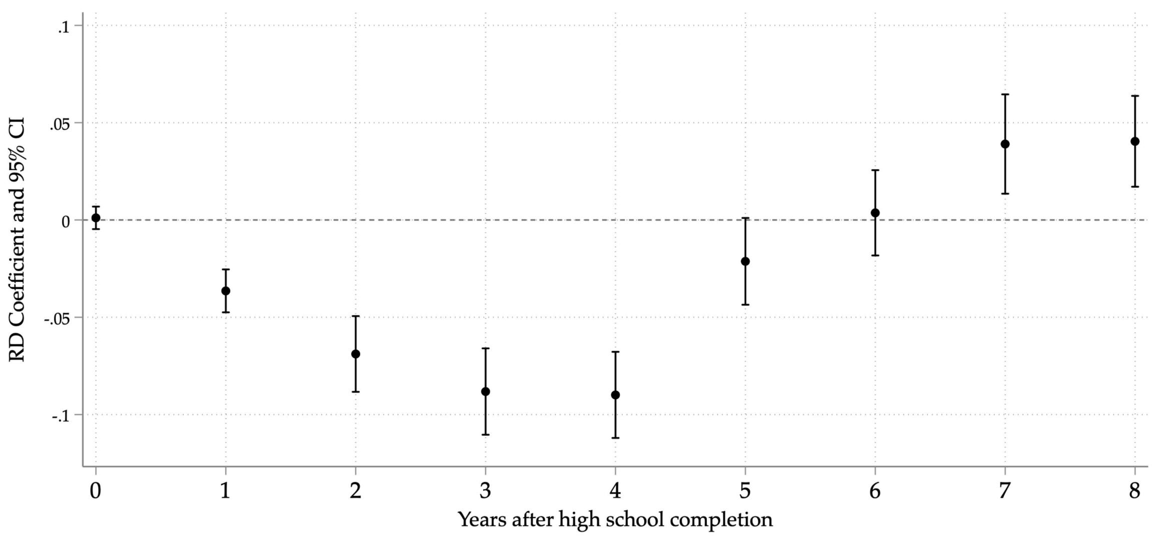
Figure A.19: Formal Employment (Test Score Cutoff)

(a) Eight Years After High School Completion



Sample restricted to SISBEN-eligible individuals.

(b) The Dynamics of the Employment Effect

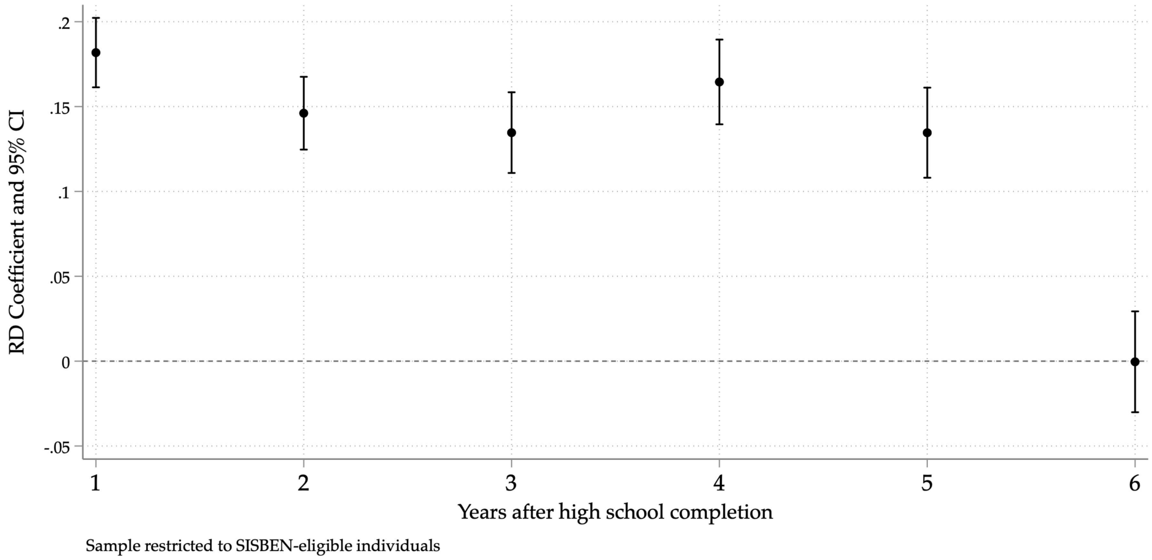


Sample restricted to SISBEN-eligible individuals

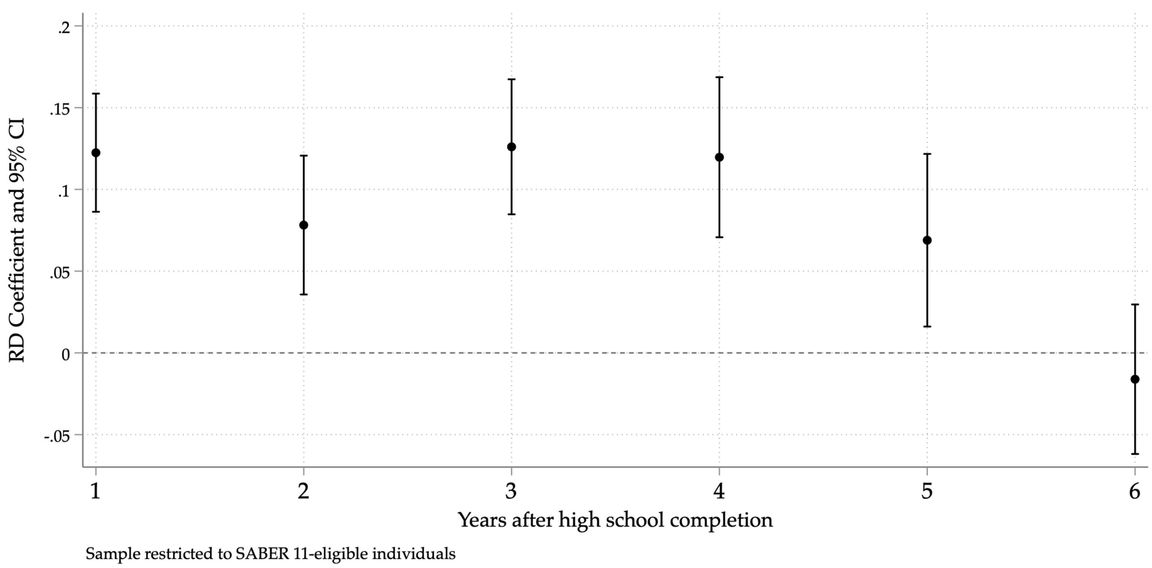
Notes: Panel A plots low-SES students' probability of formal employment eight years after high school completion as a function of the distance to the test score cutoff. Table V reports the reduced-form estimate. Panel B plots the RD coefficient over time. Figure A.21 shows similar effects using SISBEN as the running variable.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and PILA (MinSalud).

Figure A.20: Persistence: Likelihood of Being Enrolled in College Over Time

(a) Test Score Cutoff



(b) Wealth Cutoff

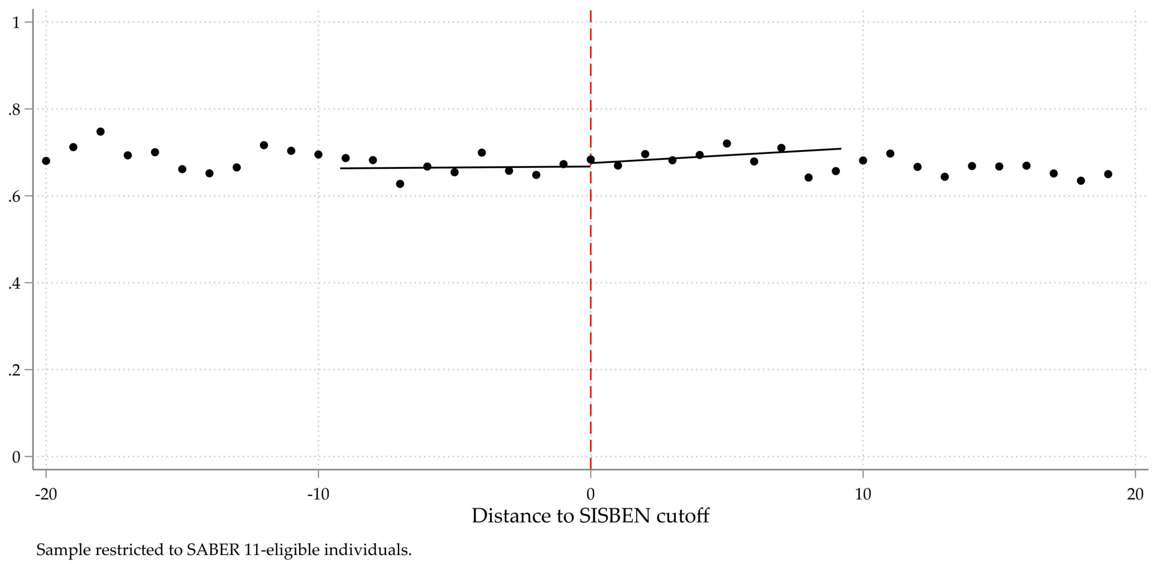


Notes: The figures plot the RD coefficient and 95% confidence intervals on the likelihood of being enrolled in college in a given year one to six years after high school completion. Panel A (B) uses SABER 11 (SISBEN) as the running variable and restricts the sample to need- (merit-) eligible students.

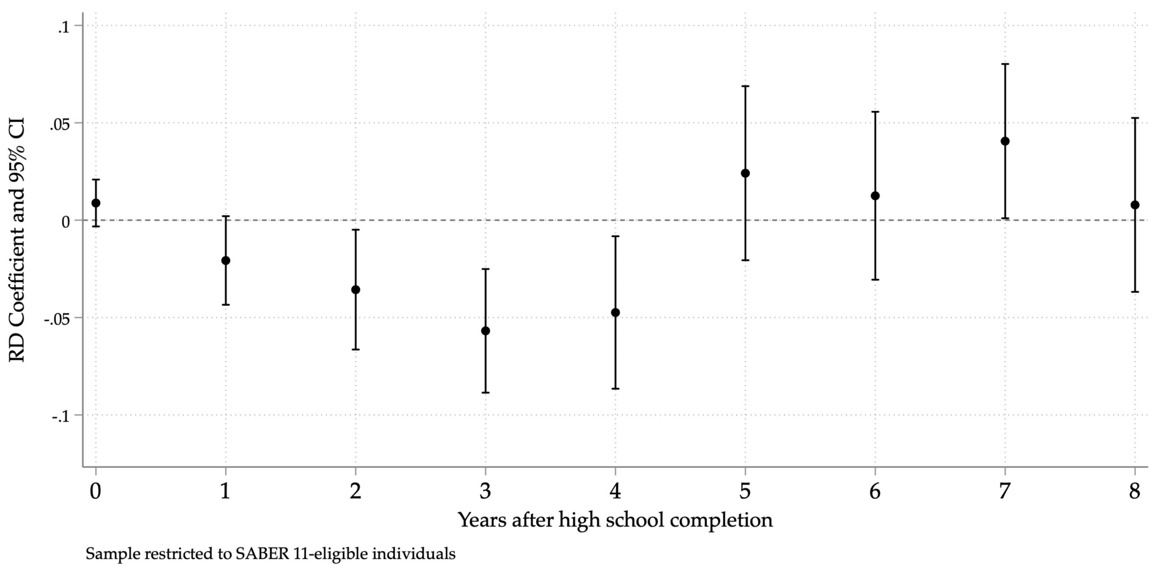
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SNIES (MEN).

Figure A.21: Formal Employment (Wealth Cutoff)

(a) Eight Years after High School Completion



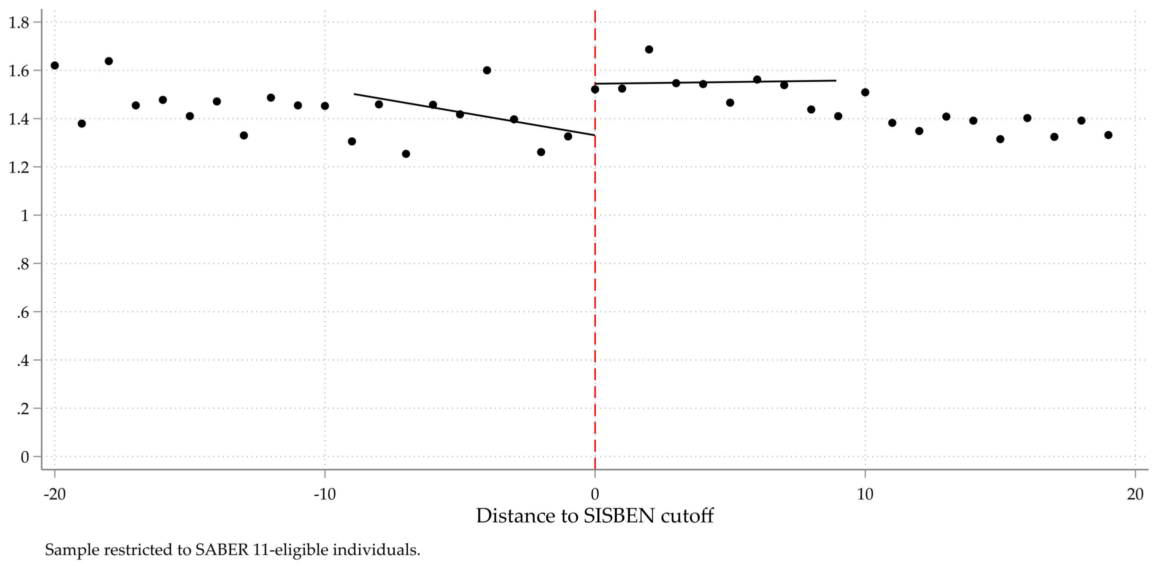
(b) The Dynamics of the Employment Effect



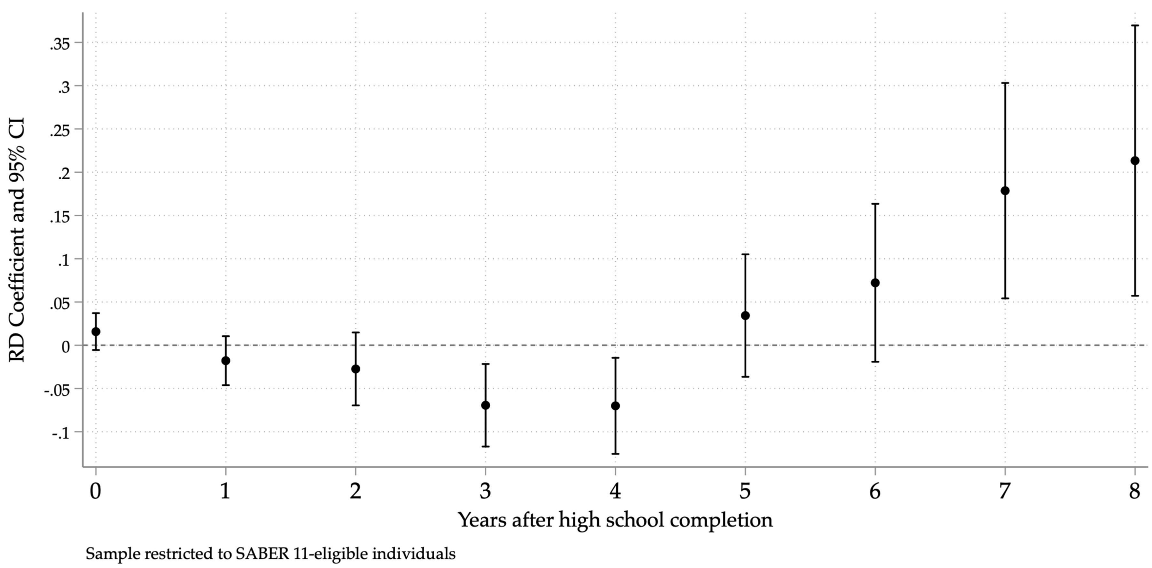
Notes: Panel A plots the probability of formal employment eight years after high school completion as a function of the distance to the wealth cutoff (for merit-eligible students). Panel B plots the RD coefficient over time. Table V reports the reduced-form estimates.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and PILA (MinSalud).

Figure A.22: Formal Earnings (Wealth Cutoff)

(a) Eight Years after High School Completion



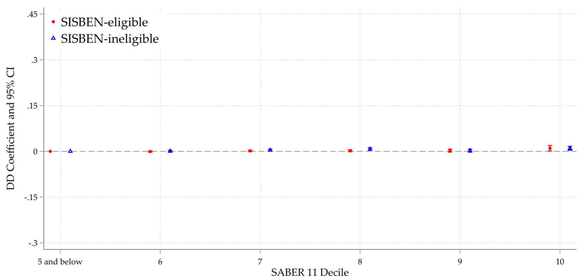
(b) The Dynamics of the Earnings Effect



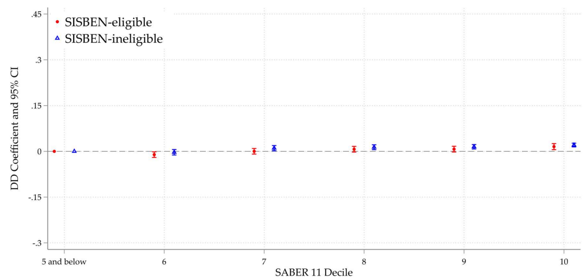
Notes: Panel A plots individuals' formal earnings (expressed as multiples of the monthly minimim wage) eight years after high school completion as a function of the distance to the wealth cutoff (for merit-eligible students). Individuals without formal employment are assigned zero earnings. Panel B plots the RD coefficient over time. Table V reports the reduced-form estimates.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and PILA (MinSalud).

Figure A.23: DD Placebo Using the Fall 2013 Cohort

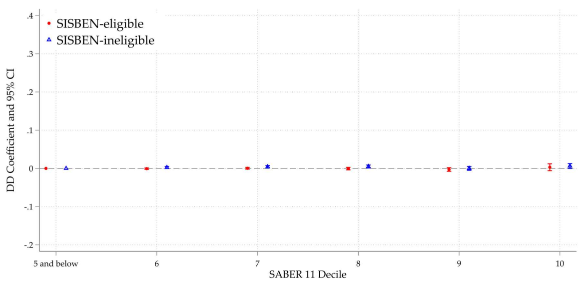
(a) Immediate Access to HQ Colleges



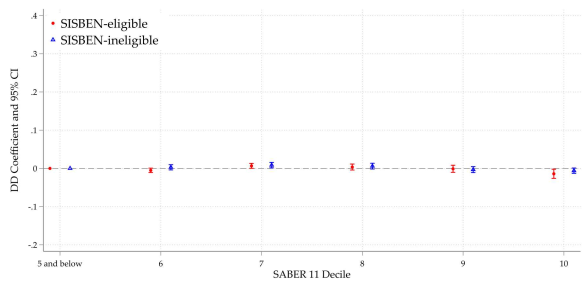
(b) Any College Within Six Years



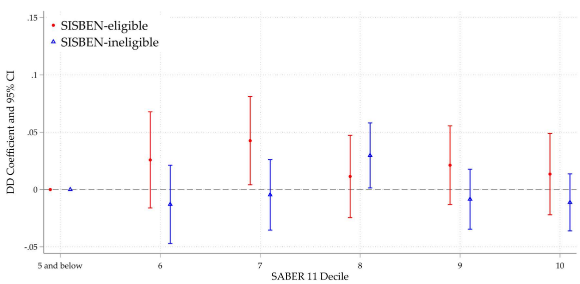
(c) Bachelor's Attainment in HQ Colleges



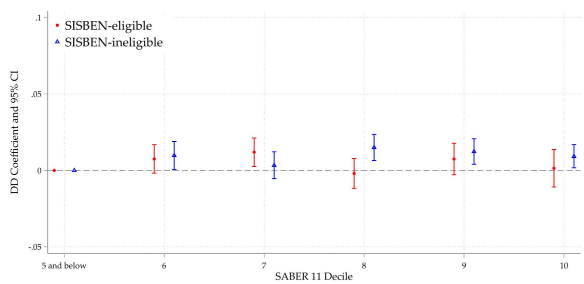
(d) Any Bachelor's Degree Attainment



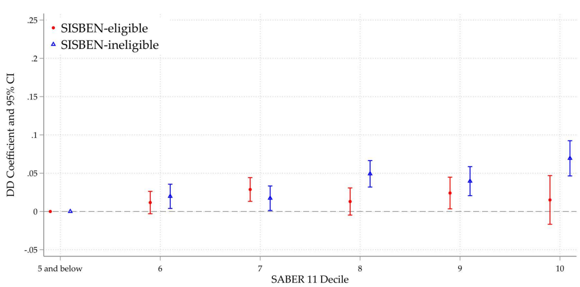
(e) Learning



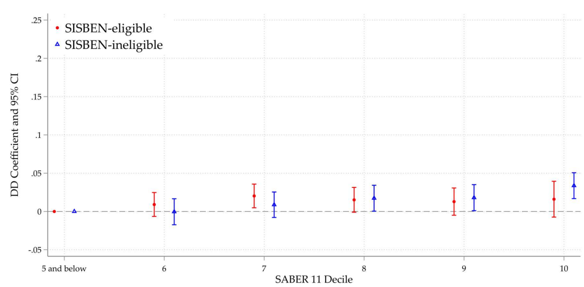
(f) Formal employment



(g) Earnings



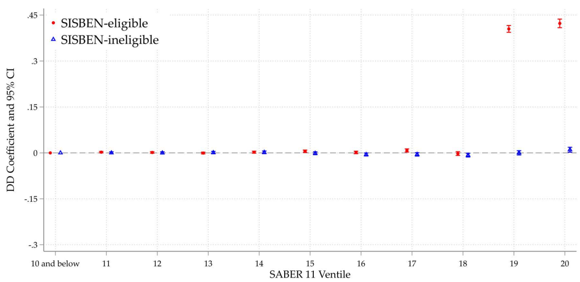
(h) Log earnings



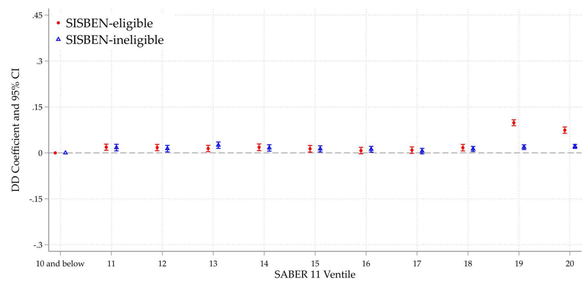
Notes: This figure reports the placebo results from comparing outcomes for the 2013 and 2012 cohorts using Specification (2). The outcome is immediate access to an HQ college in Panel A, access to any college within six years in Panel B, taking SABER PRO in an HQ college within seven years in Panel C, taking SABER PRO from any college in Panel D, the SABER PRO scores within five years in Panel E, formal employment eight years later in Panel F, formal earnings (in multiples of the minimum wage, including zeros) in Panel G, and log formal earnings in Panel H.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), SNIES (MEN), SABER PRO (ICFES), and PILA (MinSalud).

Figure A.24: DD Using Ventiles

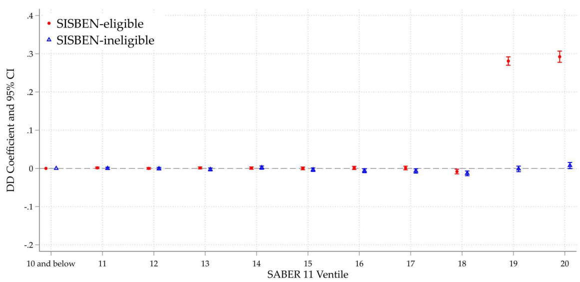
(a) Immediate Access to HQ Colleges



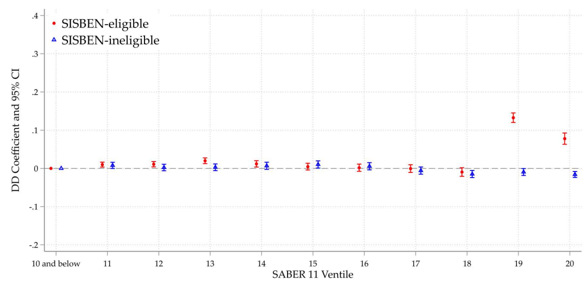
(b) Any College Within Six Years



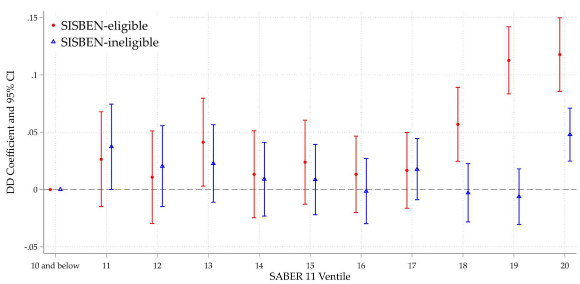
(c) Bachelor's Attainment in HQ Colleges



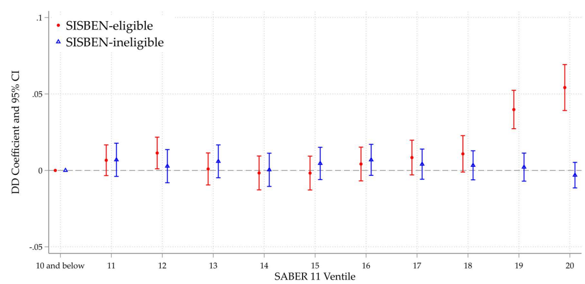
(d) Any Bachelor's Degree Attainment



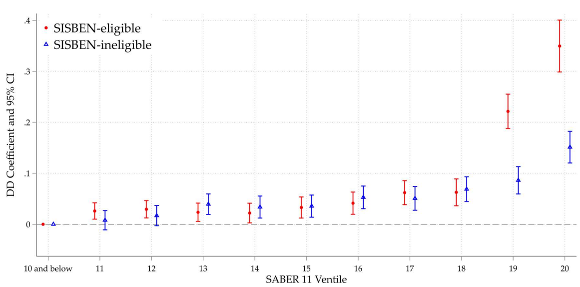
(e) Learning



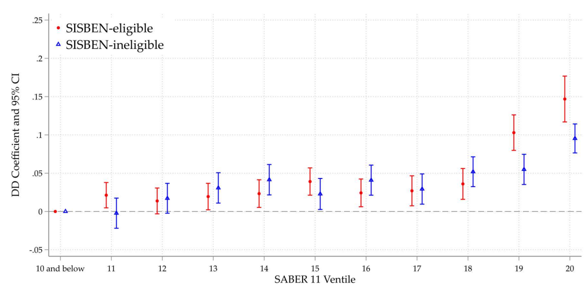
(f) Formal employment



(g) Earnings



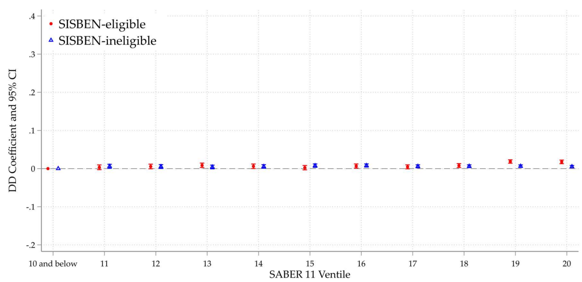
(h) Log earnings



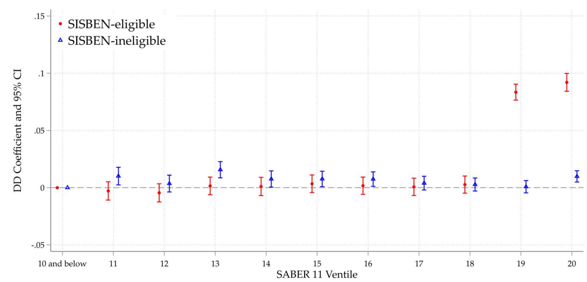
Notes: This figure plots the β_k coefficients and 95% confidence intervals from a modified version of Specification (2) using test score ventiles instead of deciles. For all outcomes, the comparison group is based on the 2012 and 2013 cohorts.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), SNIES (MEN), SABER PRO (ICFES), and PILA (MinSalud).

Figure A.25: The Impact on the College-Program "Value Added" Using Ventiles

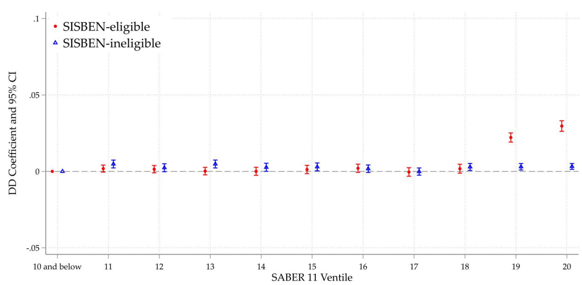
(a) Bachelor's Graduation Fixed Effect



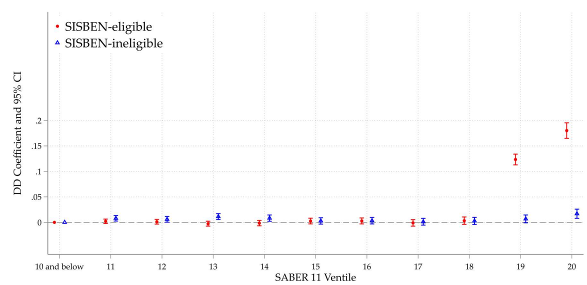
(b) Learning Fixed Effect



(c) Employment Fixed Effect



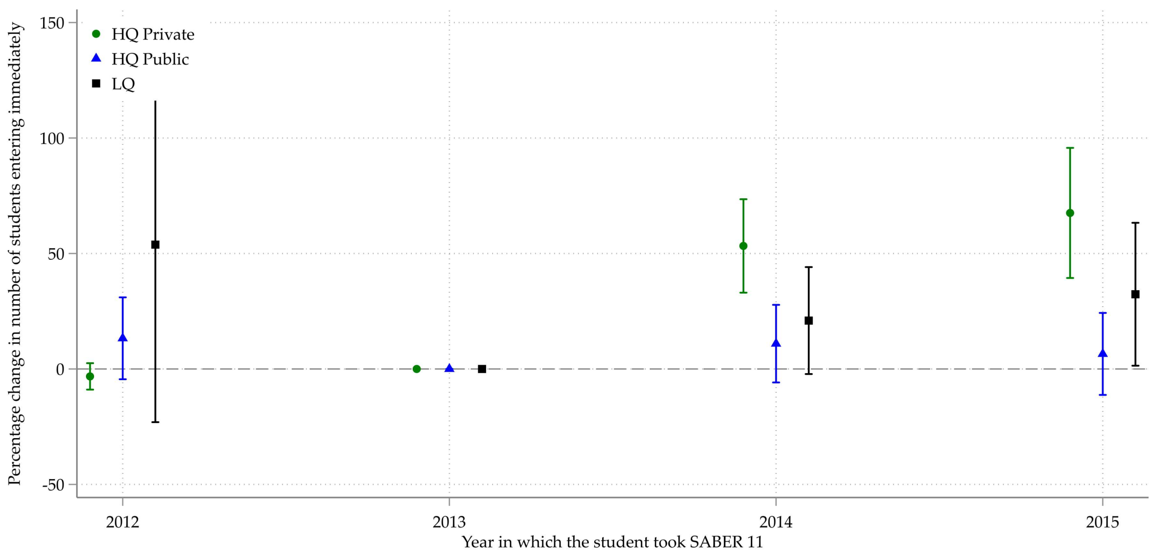
(d) Earnings Fixed Effect



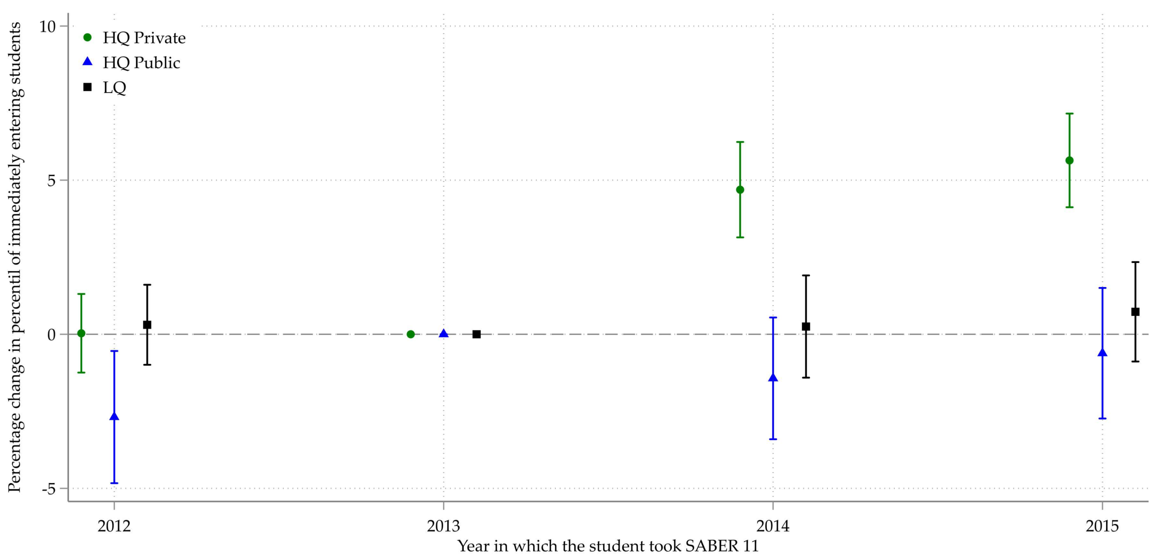
Notes: This figure plots the β_k coefficients and 95% confidence intervals using a modified version of Specification (2) using test score ventiles instead of deciles. The outcome is the college-program fixed effect, as described in Appendix D; specifically, the bachelor's graduation fixed effect (proxied by taking SABER PRO within seven years after high school) in Panel A, the learning fixed effect (using SABER PRO scores within five years after high school) in Panel B, the formal employment fixed effect (measured eight years after high school completion) in Panel C, and the formal earnings fixed effects (realized eight years after high school) in Panel D.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), SNIES (MEN), SABER PRO (ICFES), and PILA (MinSalud).

Figure A.26: Changes in Cohort Size and Student Ability Before and After the Expansion of Financial Aid by College Type

(a) Cohort Size

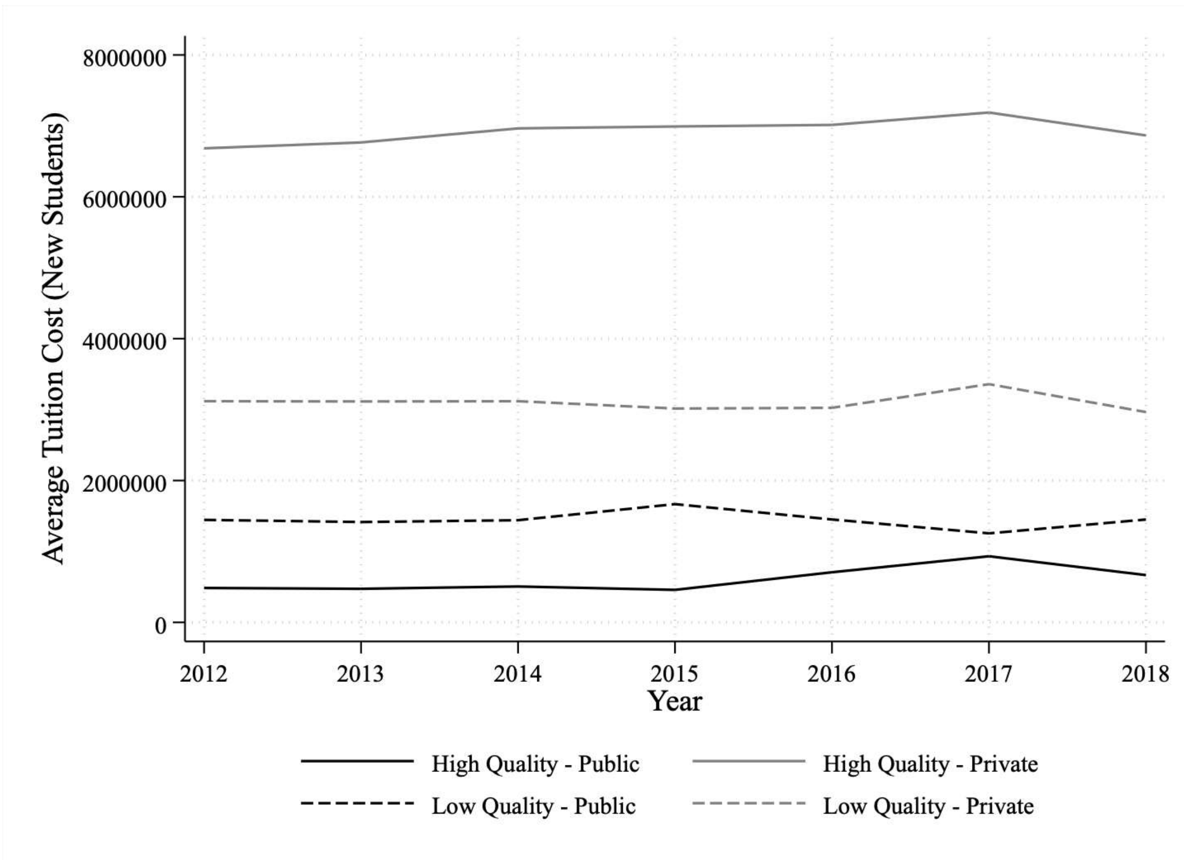


(b) Entering Students' SABER 11 Score



Notes: This figure compares outcomes across SABER 11 cohorts from the fall semesters of 2012 to 2015. The coefficients are relative to the college-specific mean for the 2013 cohort and averaged by college type. In Panel A, the outcome is the number of students who immediately accessed a given college after high school. Following the 2014 financial aid expansion, the cohort size increased by approximately 50% for HQ *private* colleges, but not for HQ *public* college or LQ colleges. In Panel B, the outcome is the average SABER 11 percentile of entering students. The average percentile increased by 5% at HQ *private* colleges, while there was no change for other college types.
Sources: Authors' calculations based on SABER 11 (ICFES) and SNIES (MEN).

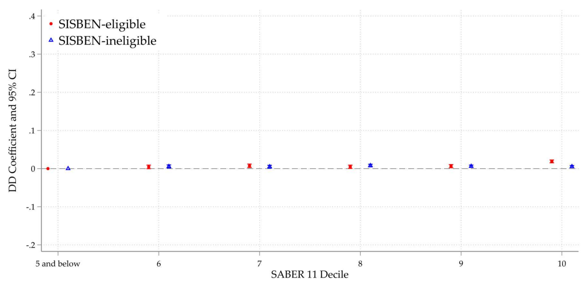
Figure A.27: Tuition Fees by College Type



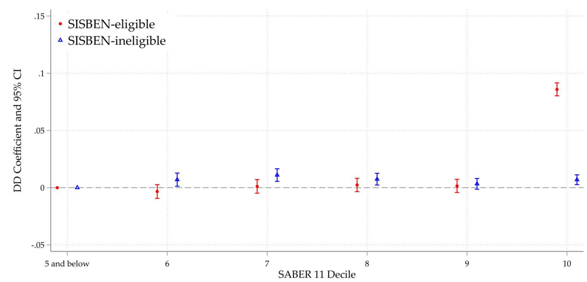
Notes: This figure plots the average annual tuition fee (in constant pesos) for new undergraduate students between 2012 and 2018 by college type. The sample is restricted to colleges reporting tuition fees for at least five years.
Sources: Authors' calculations based on SNIES (MEN).

Figure A.28: The Impact of Financial Aid on the College-Program "Value Added"

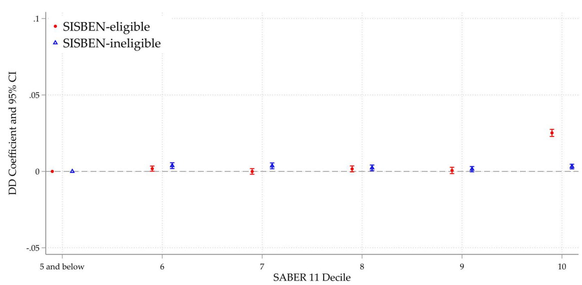
(a) Bachelor's Graduation Fixed Effect



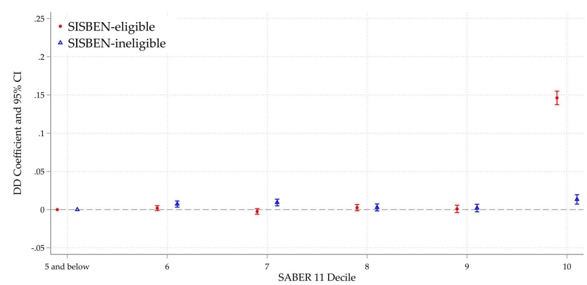
(b) Learning Fixed Effect



(c) Employment Fixed Effect

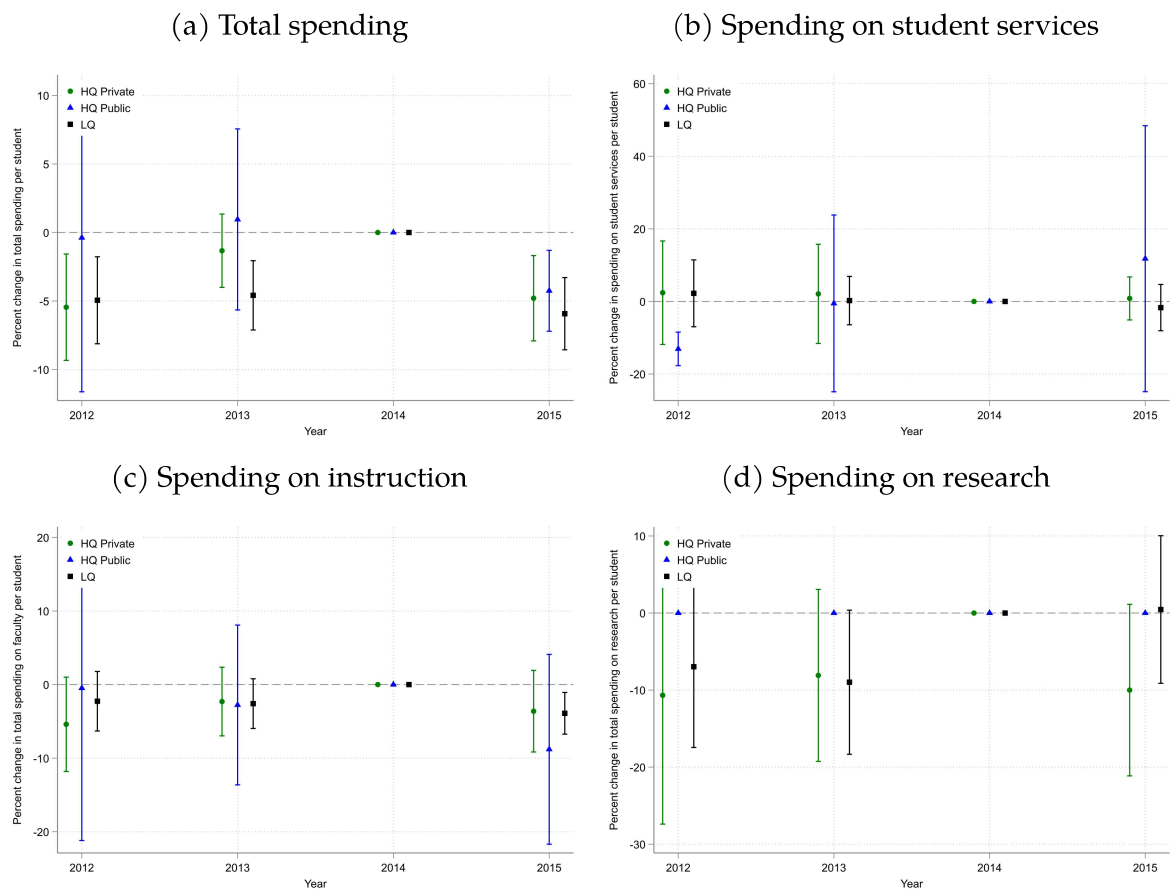


(d) Earnings Fixed Effect



Notes: This figure plots the β_k coefficients and 95% confidence intervals from Specification (2) when the outcome is the college-program fixed effect, as described in Appendix D; specifically, the bachelor's graduation fixed effect (proxied by taking SABER PRO within seven years after high school) in Panel A, the learning fixed effect (using SABER PRO scores within five years after high school) in Panel B, the formal employment fixed effect (measured eight years after high school completion) in Panel C, and the formal earnings fixed effects (realized eight years after high school) in Panel D.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), SNIES (MEN), SABER PRO (ICFES), and PILA (MinSalud).

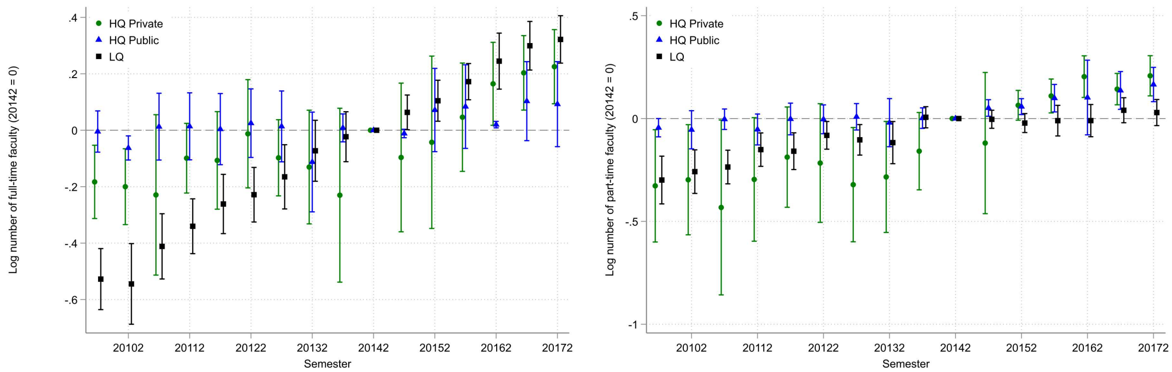
Figure A.29: Per Student Spending Before and After the Expansion of Financial Aid



Notes: This figure compares expenses across college types. Panel A reports total expenses (in constant pesos), while Panels B, C, and D show the type of expense. Annual information on institutional expenses is available only for a subsample of colleges. The sample is balanced from 2012 to 2015. For total expenses, the sample includes 16 HQ private colleges, 8 HQ public colleges, and 147 LQ colleges, but fewer institutions report expense type. The coefficients are relative to the college-specific mean for 2014 and averaged by college type.
Sources: Authors' calculations based on institutional financial accounts and balance sheets (MEN).

Figure A.30: Number of Instructors Before and After the Expansion of Financial Aid

(a) All full-time instructors (b) All part-time instructors



Notes: This figure compares the log number of full-time and part-time instructors. The coefficients are relative to the college-specific mean for the second semester of 2014 and averaged by college type. *Sources:* Authors' calculations based on SNIES (MEN).

Table A.1: Baseline Characteristics by College Type

	High quality		Low quality	
	Private	Public	Private	Public
	(1)	(2)	(3)	(4)
SABER 11 score	303.1	293.3	259.0	261.9
(Standardized) SABER 11 score	1.77	1.44	0.30	0.40
(Standardized) SABER PRO score	0.50	0.27	-0.18	-0.15
Graduation rate (%)	75.1	66.6	59.3	60.2
Faculty with PhD (%)	11.9	15.2	1.9	2.4
Program length (in semesters)	9.2	9.6	9.2	9.4
Sticker price of tuition (in min wages)	17.7	6.4	8.1	3.7
Public spending per student (in min wages)		14.5		8.5
N students	50,642	60,704	214,203	267,636
N college campuses	21	16	207	79
N colleges	20	13	181	66

Notes: This table reports descriptive statistics by college type. The information is based on high school exit test takers from the fall 2012 and 2013 cohorts who ever accessed college within six years from high school completion. *Sources:* Authors' calculations based on SABER 11 (ICFES), SABER PRO (ICFES), SNIES (MEN), and institutional financial accounts and balance sheets (MEN).

Table A.2: Baseline Covariate Balance Test around SPP Eligibility Threshold

	Running variable					
	SABER 11			SISBEN		
	Mean (1)	RD Coeff. (2)	p-value (3)	Mean (4)	RD Coeff. (5)	p-value (6)
SABER 11 percentile				95.287	0.143	0.345
Wealth percentile (including missing SISBEN)	31.765	-0.494	0.209			
Took the Saber 11 test as a student	0.970	0.006	0.109	0.984	-0.007	0.467
Female	0.469	-0.011	0.316	0.443	0.002	0.877
Age	16.608	-0.018	0.628	16.355	0.073	0.582
Ethnic minority	0.037	0.001	0.835	0.024	0.016	0.060
Employed	0.044	0.002	0.739	0.045	-0.008	0.349
Family size	4.599	-0.039	0.386	4.385	-0.137	0.041
Mother's education: primary	0.252	-0.012	0.213	0.130	0.003	0.909
Mother's education: secondary	0.502	-0.011	0.446	0.476	-0.056	0.048
Mother's education: T&T	0.135	0.002	0.837	0.185	-0.005	0.815
Mother's education: professional	0.111	0.021	0.006	0.209	0.055	0.008
Father's education: primary	0.342	-0.005	0.620	0.181	0.015	0.729
Father's education: secondary	0.429	-0.001	0.754	0.450	-0.062	0.020
Father's education: T&T	0.104	0.002	0.649	0.174	-0.008	0.516
Father's education: professional	0.122	0.007	0.407	0.196	0.056	0.013
Household SES: Stratum 1	0.341	0.000	0.823	0.128	-0.013	0.303
Household SES: Stratum 2	0.461	-0.017	0.205	0.506	0.005	0.873
Household SES: Stratum 3	0.183	0.011	0.285	0.333	0.005	0.823
Household SES: Stratum 4	0.009	0.006	0.017	0.020	0.009	0.305
Household SES: Stratum 5	0.003	0.001	0.632	0.007	-0.003	0.476
Household SES: Stratum 6	0.001	-0.001	0.224	0.001	0.000	0.746
School hours: Full day	0.197	-0.004	0.702	0.291	0.025	0.207
School hours: Morning	0.614	0.000	0.955	0.541	-0.033	0.180
School hours: Evening	0.008	0.002	0.596	0.006	0.001	0.815
School hours: Afternoon	0.173	0.000	0.925	0.156	0.016	0.342
School hours: Weekends	0.008	0.003	0.444	0.008	-0.007	0.041
Private school	0.170	0.001	0.934	0.304	0.058	0.012
School schedule: A	0.999	0.000	0.798	0.997	0.000	0.864
School schedule: B	0.001	0.000	0.982	0.000	0.001	0.455
School schedule: Other	0.001	0.000	0.515	0.002	-0.002	0.464
Floor: cement/ gravel/ brick	0.433	-0.014	0.161	0.263	0.005	0.706
Floor: wood, board, wooden plank	0.039	0.002	0.613	0.039	0.014	0.167
Floor:polished wood, tile, marble, carpet	0.500	0.010	0.261	0.688	-0.009	0.659
Floor: land, sand	0.027	0.001	0.773	0.009	0.000	0.857
Family has internet	0.589	0.019	0.136	0.782	0.003	0.771
Family has a laptop	0.732	0.002	0.865	0.878	0.030	0.039
Family has a car	0.172	0.013	0.235	0.260	0.060	0.014
Family has a cellphone	0.943	0.010	0.074	0.944	0.024	0.034
Student resides: Urban	0.862	-0.008	0.355	0.936	-0.005	0.739
School location: Urban	0.917	-0.006	0.540	0.965	-0.005	0.554
Joint F-Stat (p-value, LB on bandwidth)		0.470			0.168	
Joint F-Stat (p-value, UB on bandwidth)		0.703			0.176	

Notes: This table plots the reduced-form coefficient from an RD specification where the outcome is a baseline characteristic and the running variable is either SABER 11 test scores in Columns (1)–(3) or SISBEN poverty index in Columns (4)–(6). The sample is restricted to SISBEN-eligible individuals in Columns (1)–(3) and SABER 11-eligible individuals in Columns (4)–(6). Columns (1) and (4) present control means, Columns (2) and (5) present conventional coefficients, and Columns (3) and (6) present p-values based on conventional standard errors. The last two rows report the p-value from a joint significance test using all baseline characteristics and small or large bandwidths: $\pm$ 20 or 40 test score units in Column (2) and $\pm$ 7 or 15 household wealth units in Column (5). All results are estimated with package rdrobust (Cattaneo et al., 2014). *Sources:* Authors' calculations based on SABER 11 (ICFES) and SISBEN (DNP).

Table A.3: Baseline Covariate Balance Test around SPP Eligibility Threshold Conditional on Taking SABER PRO Within Seven Years

	Running variable					
	SABER 11			SISBEN		
	Mean (1)	RD Coeff. (2)	p-value (3)	Mean (4)	RD Coeff. (5)	p-value (6)
SABER 11 percentile				95.703	0.010	0.937
Wealth percentile (including missing SISBEN)	31.622	0.534	0.301			
Took the Saber 11 test as a student	0.974	0.013	0.023	0.986	0.003	0.602
Female	0.561	-0.019	0.262	0.492	0.009	0.842
Age	16.352	-0.055	0.171	16.162	0.002	0.866
Ethnic minority	0.035	0.000	0.962	0.033	0.005	0.520
Employed	0.030	0.008	0.282	0.032	0.002	0.958
Family size	4.628	-0.095	0.076	4.362	-0.054	0.400
Mother's education: primary	0.208	-0.002	0.686	0.113	-0.004	0.718
Mother's education: secondary	0.492	-0.013	0.361	0.463	-0.055	0.129
Mother's education: T&T	0.141	0.009	0.319	0.191	0.001	0.900
Mother's education: professional	0.158	0.007	0.449	0.233	0.058	0.043
Father's education: primary	0.289	0.012	0.572	0.157	0.019	0.744
Father's education: secondary	0.422	0.011	0.858	0.421	-0.048	0.148
Father's education: T&T	0.133	-0.016	0.345	0.185	-0.004	0.859
Father's education: professional	0.147	0.006	0.516	0.222	0.051	0.092
Household SES: Stratum 1	0.333	-0.018	0.264	0.116	-0.010	0.501
Household SES: Stratum 2	0.441	0.018	0.385	0.495	0.011	0.781
Household SES: Stratum 3	0.205	0.001	0.873	0.353	-0.013	0.691
Household SES: Stratum 4	0.012	0.004	0.310	0.025	0.013	0.265
Household SES: Stratum 5	0.003	0.002	0.438	0.006	-0.001	0.978
Household SES: Stratum 6	0.001	-0.001	0.565	0.002	-0.001	0.749
School hours: Full day	0.220	-0.003	0.827	0.318	0.024	0.314
School hours: Morning	0.623	-0.014	0.478	0.561	-0.083	0.018
School hours: Evening	0.002	0.002	0.362	0.000	0.002	0.322
School hours: Afternoon	0.152	0.011	0.374	0.121	0.059	0.021
School hours: Weekends	0.003	0.003	0.284	0.006	-0.004	0.262
Private school	0.204	-0.024	0.074	0.323	0.049	0.094
School schedule: A	1.001	-0.002	0.033	0.998	0.001	0.439
School schedule: B	-0.001	0.001	0.053	0.000	0.000	0.318
School schedule: Other	0.000	0.001	0.124	0.001	-0.001	0.350
Floor: cement/ gravel/ brick	0.416	-0.016	0.270	0.254	0.009	0.713
Floor: wood, board, wooden plank	0.031	0.007	0.334	0.043	0.004	0.848
Floor:polished wood, tile, marble, carpet	0.527	0.007	0.493	0.698	-0.016	0.549
Floor: land, sand	0.026	-0.001	0.816	0.007	0.003	0.626
Family has internet	0.645	-0.002	0.957	0.819	-0.009	0.967
Family has a laptop	0.783	-0.012	0.362	0.902	0.017	0.287
Family has a car	0.200	0.005	0.603	0.295	0.039	0.161
Family has a cellphone	0.953	0.005	0.454	0.937	0.037	0.016
Student resides: Urban	0.890	-0.028	0.013	0.934	0.012	0.364
School location: Urban	0.932	-0.016	0.096	0.976	-0.016	0.204
Joint F-Stat (p-value, LB on bandwidth)		0.006			0.000	
Joint F-Stat (p-value, UB on bandwidth)		0.004			0.000	

Notes: This table plots the reduced-form coefficient from an RD specification where the outcome is a baseline characteristic and the running variable is either SABER 11 test scores in Columns (1)–(3) or SISBEN poverty index in Columns (4)–(6). Unlike in Table A.2, the sample is restricted to individuals who took the SABER PRO exam within seven years from high school completion. *Sources:* Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SABER PRO (ICFES).

Table A.4: Reduced-Form Estimates on Other Labor Market Outcomes

	Type of employment		Days	Conditional on working						
				Days	In 13	Firm size				
	Employee	Independent	worked	worked	largest cities	No. of employees	Micro	Small	Medium	Large
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: *SABER 11 is the running variable*										
Reduced form	0.033	0.011	1.199	0.059	0.033	299.98	0.005	0.003	0.001	-0.009
	(0.013)	(0.004)	(0.360)	(0.162)	(0.012)	(216.18)	(0.013)	(0.010)	(0.010)	(0.015)
Mean control	0.513	0.019	17.092	28.56	0.701	2011.25	0.230	0.176	0.147	0.446
Observations	297,279	297,279	297,279	146,252	149,762	146,087	149,595	149,595	149,595	149,595
BW loc. poly.	21.69	24.57	24.77	31.80	33.60	23.10	25.99	35.05	27.42	25.11
Effect obs. control	18,948	23,070	23,070	19,879	22,893	13,042	15,160	24,932	16,858	15,160
Effect obs. Treat	9,489	10,299	10,299	7,579	8,042	6,507	7,028	8,203	7,307	7,028
Panel B: *SISBEN is the running variable*										
Reduced form	0.019	0.016	0.935	1.377	0.038	240.085	-0.034	0.025	0.042	-0.018
	(0.022)	(0.009)	(0.646)	(0.446)	(0.024)	(400.45)	(0.022)	(0.022)	(0.023)	(0.027)
Mean control	0.554	0.029	18.923	28.05	0.775	2393.50	0.259	0.153	0.112	0.468
Observations	22,552	22,552	22,552	14,975	15,469	14,886	15,378	15,378	15,378	15,378
BW loc. poly.	10.88	9.86	10.82	5.26	9.00	11.77	12.00	8.99	6.66	11.06
Effect obs. control	4,536	4,188	4,525	1,543	2,635	3,249	3,426	2,609	1,983	3,163
Effect obs. Treat	4,648	4,229	4,632	1,548	2,720	3,408	3,568	2,699	1,981	3,284

Notes: This table presents the reduced-form estimates of the effect of financial aid on labor market outcomes eight years after high school completion using an RD design. Columns (1) and (2) indicate whether the individual is employed as a wage earner or an independent contractor. Column (3) reports the effects on the number of days formally employed and assigns zeros for people with no formal employment. Columns (4)–(10) restrict the sample to individuals who are formally employed eight years after high school. See the notes under Table I for other details.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and PILA (MinSalud).

Table A.5: Reduced-Form Estimates on Employment Sector

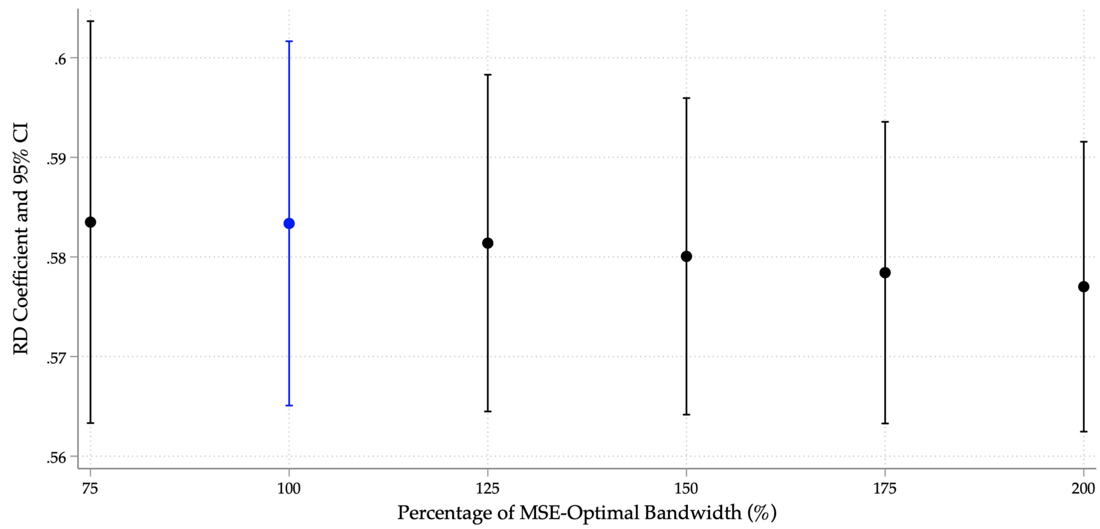
	Conditional on working												
	Agriculture (1)	Mining and quarrying (2)	Manufacturing (3)	Electricity, gas, water supply (4)	Construction (5)	Trade, transport, accomodation, food (6)	Information and communication (7)	Finance and insurance (8)	Real estate (9)	Professional, scientific, technical, admin (10)	Public admin, eduction, health, social work (11)	Arts, entertainment, other service (12)	Extra-territorial orgs (13)
***Panel A:** SABER 11 is the running variable*													
Reduced form	-0.006	-0.003	0.002	0.002	-0.008	-0.013	0.011	0.002	0.002	0.012	0.004	-0.003	0.000
	(0.004)	(0.002)	(0.007)	(0.002)	(0.007)	(0.010)	(0.007)	(0.005)	(0.002)	(0.016)	(0.011)	(0.007)	(0.001)
Mean control	0.02	0.01	0.06	0.00	0.06	0.16	0.06	0.03	0.00	0.38	0.13	0.07	0.00
Observations							146,252						
BW loc. poly.	32.43	27.63	27.14	22.99	26.99	32.78	26.04	29.75	20.79	23.48	23.71	29.68	21.55
Effect obs. control	21,000	16,354	16,354	12,188	15,351	21,000	15,351	18,089	10,721	13,072	13,072	18,089	11,312
Effect obs. Treat	7,686	7,099	7,099	6,325	6,943	7,686	6,943	7,316	5,991	6,520	6,520	7,316	6,095
***Panel B:** SISBEN is the running variable*													
Reduced form	-0.011	-0.002	-0.004	-0.009	0.000	-0.006	0.034	-0.008	-0.004	0.032	-0.034	0.003	0.000
	(0.008)	(0.004)	(0.013)	(0.005)	(0.011)	(0.018)	(0.015)	(0.010)	(0.004)	(0.026)	(0.023)	(0.016)	(0.000)
Mean control	0.03	0.005	0.069	0.015	0.048	0.131	0.055	0.043	0.006	0.385	0.138	0.073	0.000
Observations							14,975						
BW loc. poly.	9.45	10.16	11.76	10.97	12.34	10.58	9.41	12.47	7.68	12.79	6.96	9.01	3.79
Effect obs. control	2,710	2,888	3,283	3,073	3,400	2,979	2,702	3,425	2,186	3,480	1,999	2,558	1,124
Effect obs. Treat	2,791	2,978	3,421	3,199	3,567	3,094	2,775	3,593	2,262	3,653	2,044	2,668	1,118

Notes: This table presents the reduced-form estimates of the effect of financial aid on employment sector for individuals who are formally employed eight years after high school completion using an RD design. See the notes under Table I for other details.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and PILA (MinSalud).

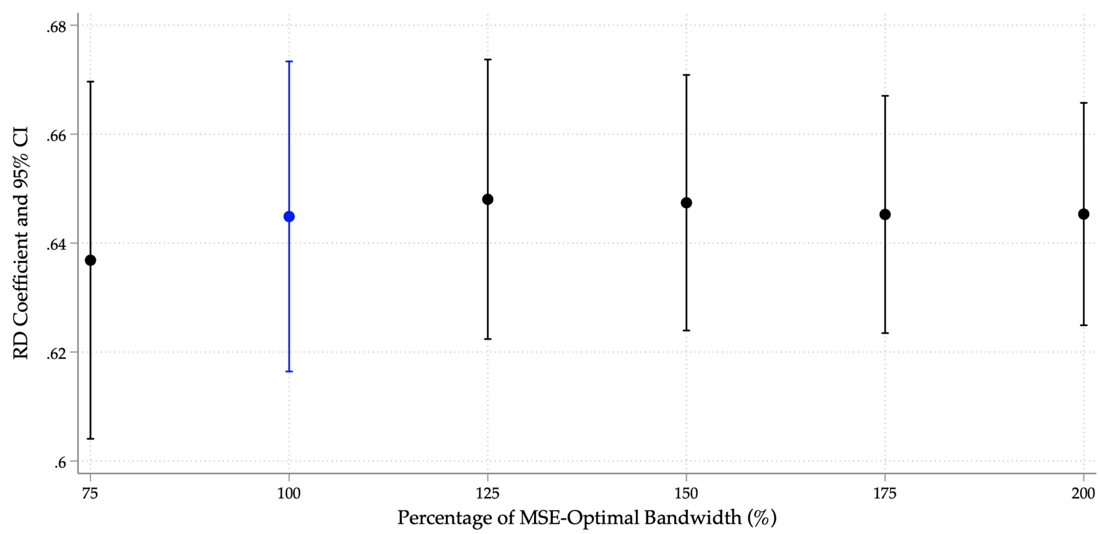
Appendix B Robustness to RD Bandwidth Selection

Figure B.1: Probability of Receiving SPP Financial Aid

(a) Test Score Cutoff



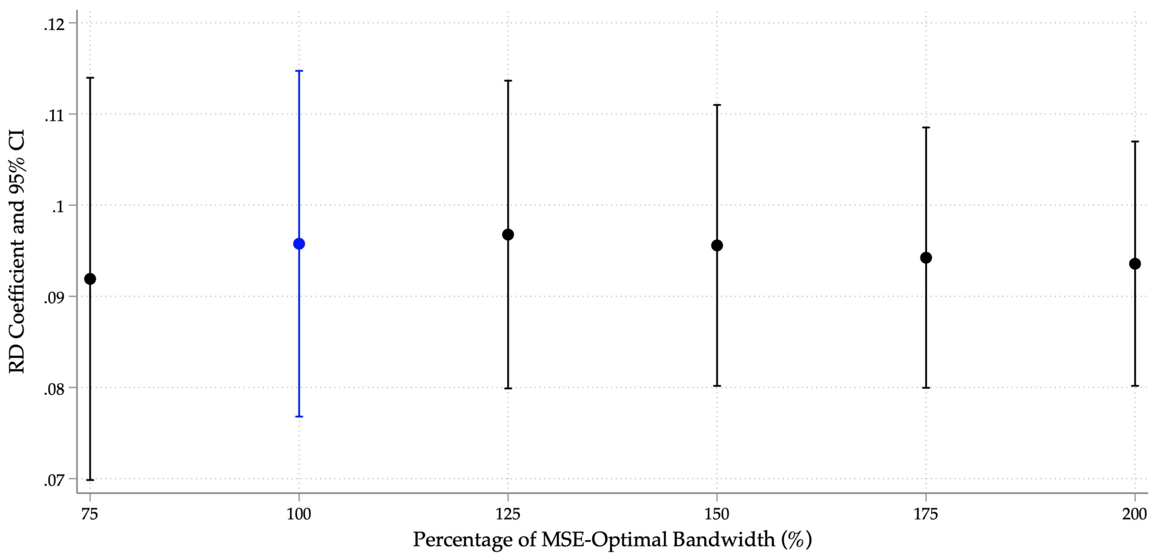
(b) Wealth Cutoff



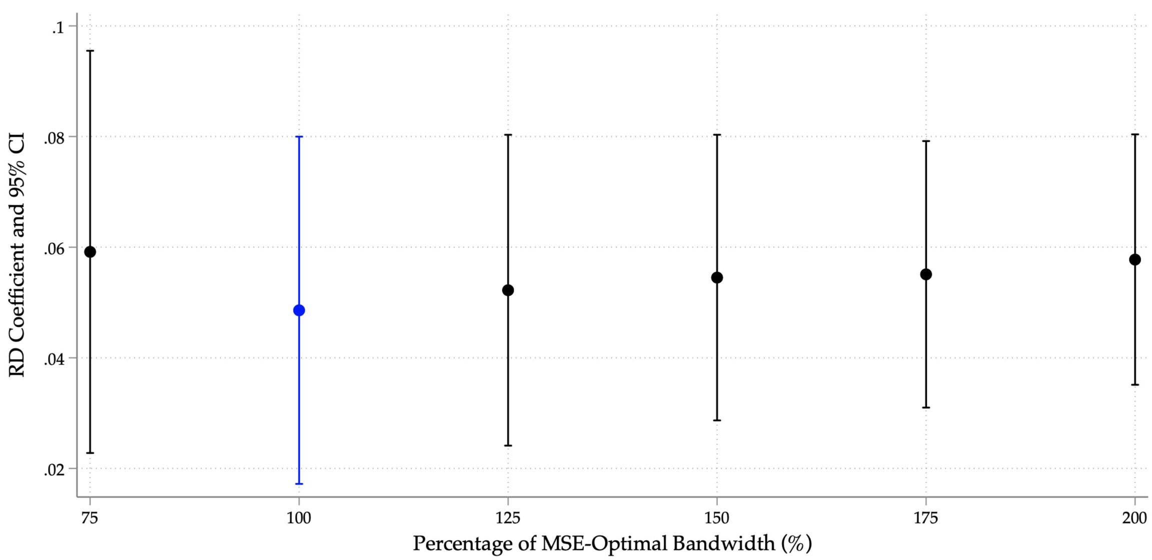
Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test score (SISBEN wealth score) as the running variable, restricting the sample to need- (merit-) eligible students. The dependent variable is likelihood of receiving SPP financial aid.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and PILA (MinSalud).

Figure B.2: Access to Any College Within Six Years from High School Completion

(a) Test Score Cutoff



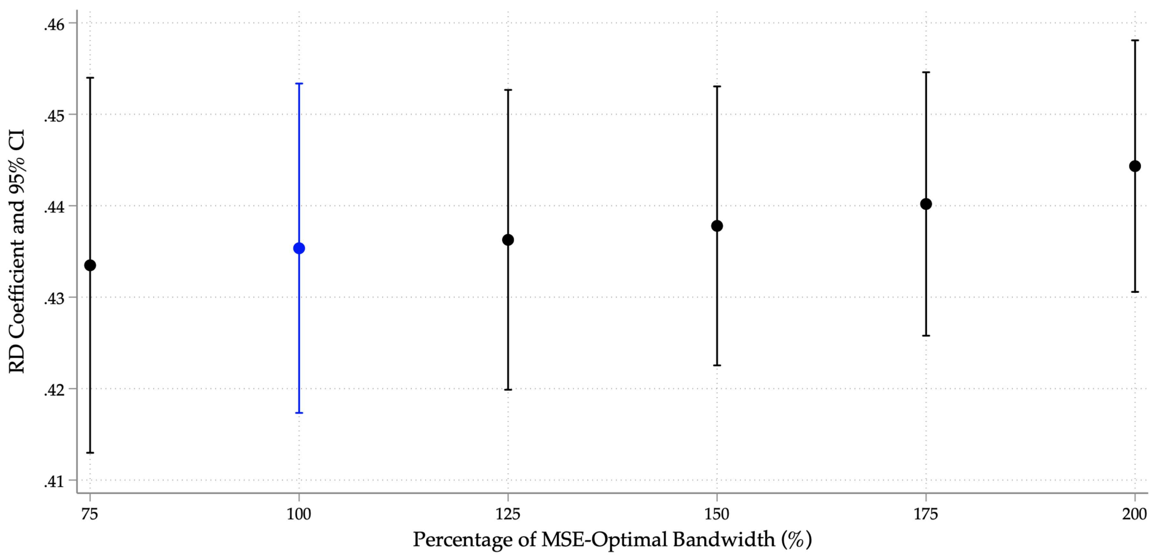
(b) Wealth Cutoff



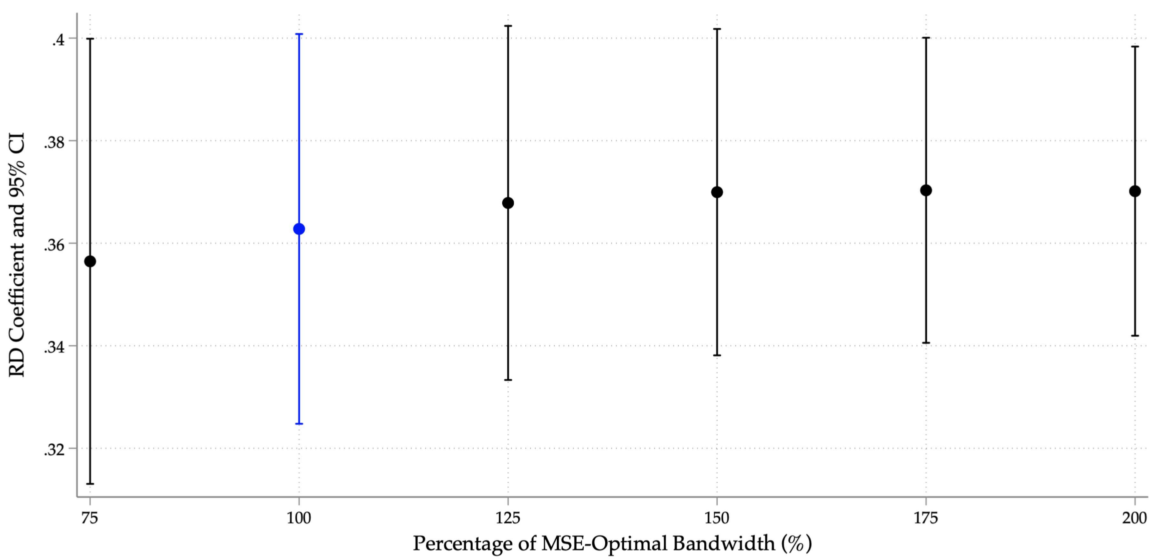
Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test score (SISBEN wealth score) as the running variable, restricting the sample to need- (merit-) eligible students. The dependent variable is the likelihood of accessing any college within six years from high school completion.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SNIES (MEN).

Figure B.3: Access to a High-Quality College Within Six Years from High School

(a) Test Score Cutoff



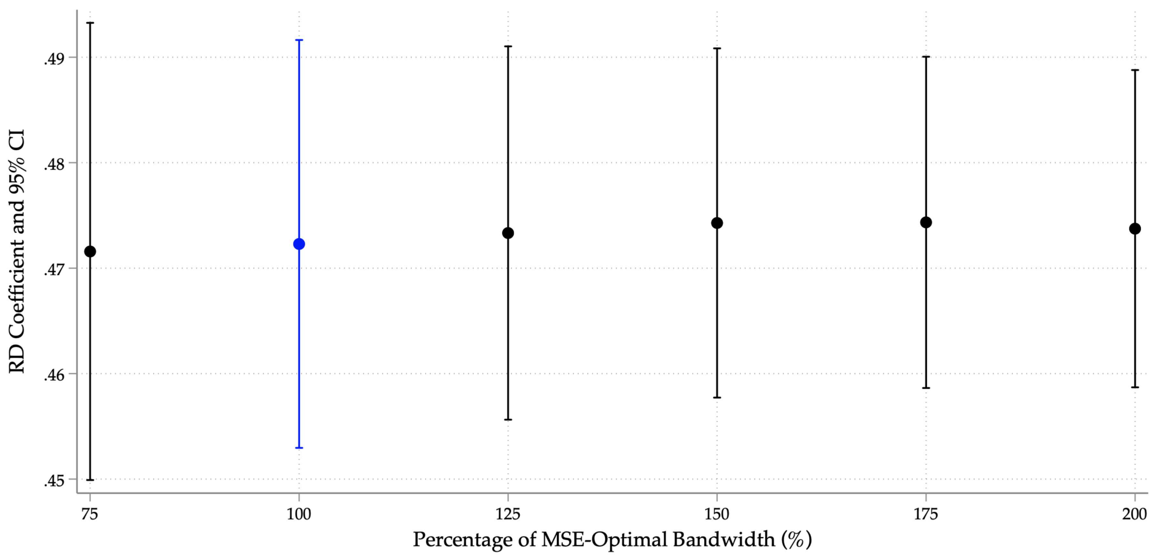
(b) Wealth Cutoff



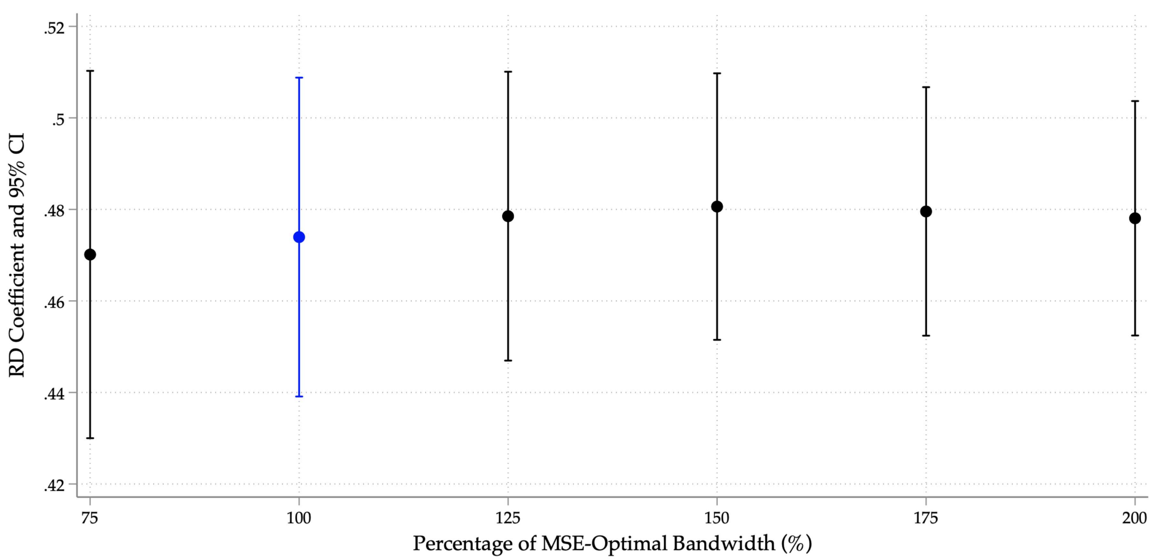
Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test score (SISBEN wealth score) as the running variable, restricting the sample to need- (merit-) eligible students. The dependent variable is the likelihood of accessing an HQ college within six years from high school completion.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SNIES (MEN).

Figure B.4: Access to a High-Quality Private College Within Six Years from High School

(a) Test Score Cutoff



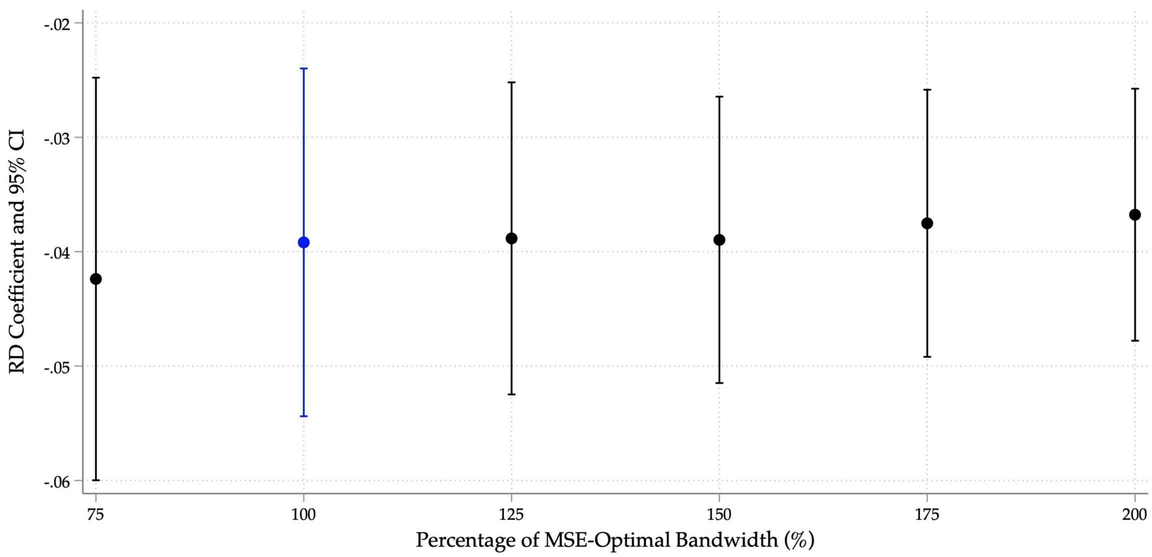
(b) Wealth Cutoff



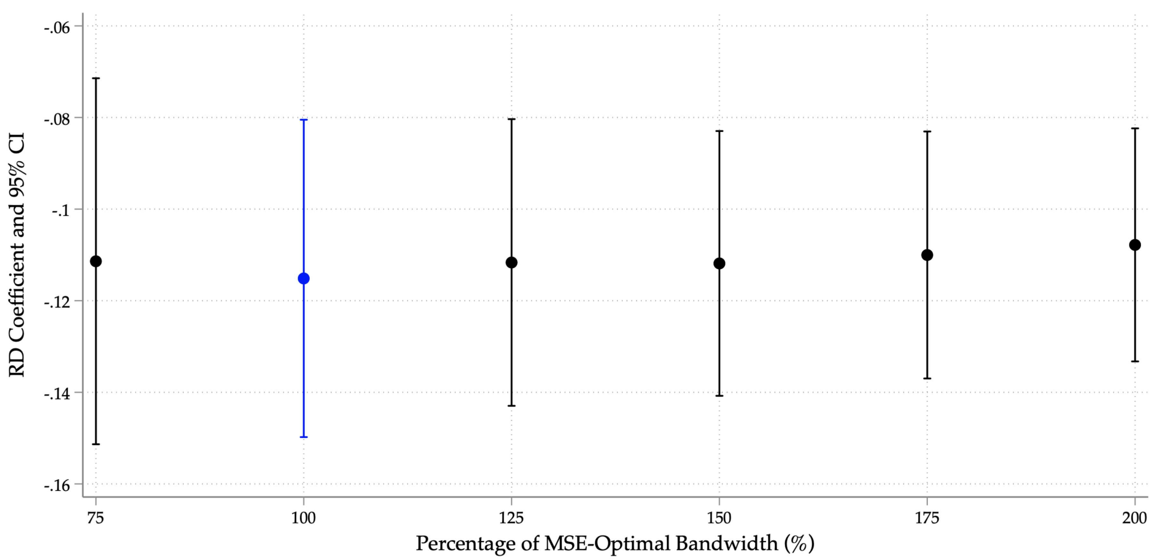
Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test score (SISBEN wealth score) as the running variable, restricting the sample to need- (merit-) eligible students. The dependent variable is the likelihood of accessing an HQ private college within six years from high school completion.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SNIES (MEN).

Figure B.5: Access to a High-Quality Public College Within Six Years from High School

(a) Test Score Cutoff



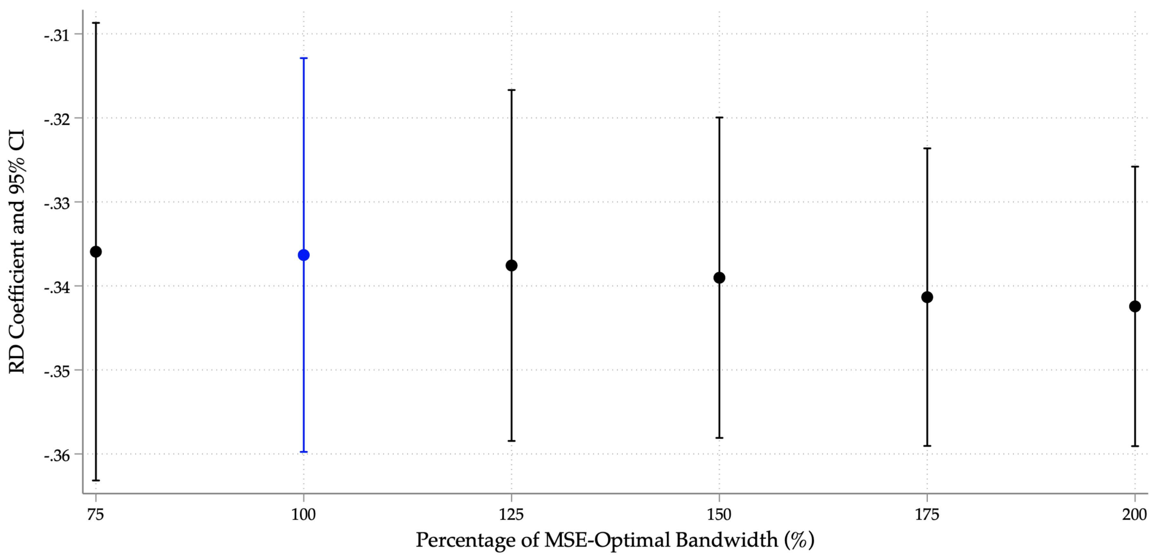
(b) Wealth Cutoff



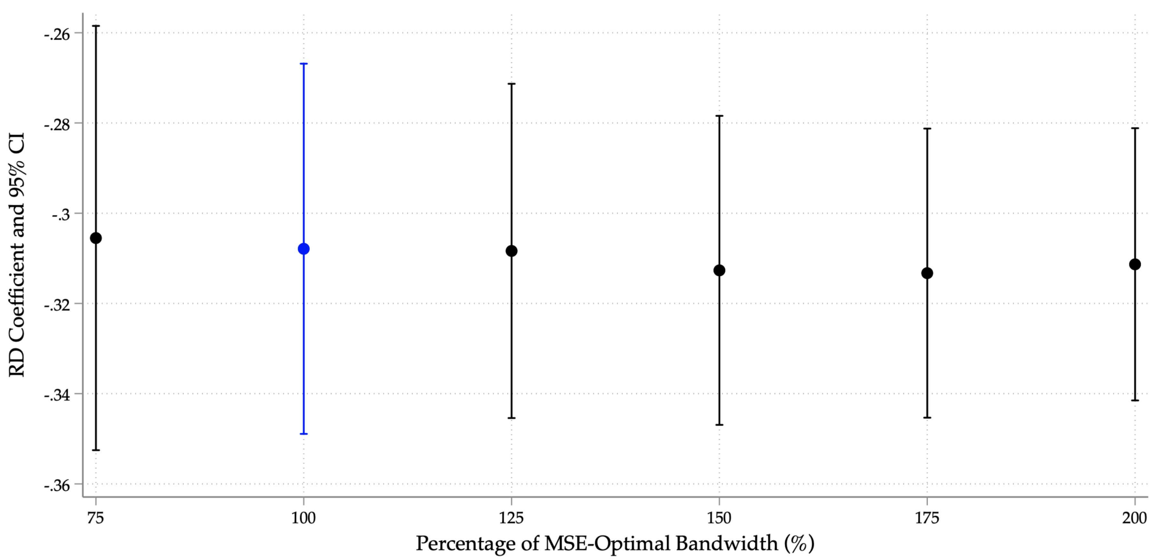
Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test score (SISBEN wealth score) as the running variable, restricting the sample to need- (merit-) eligible students. The dependent variable is the likelihood of accessing an HQ public college within six years from high school completion.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SNIES (MEN).

Figure B.6: Access to a Low-Quality College Within Six Years from High School

(a) Test Score Cutoff



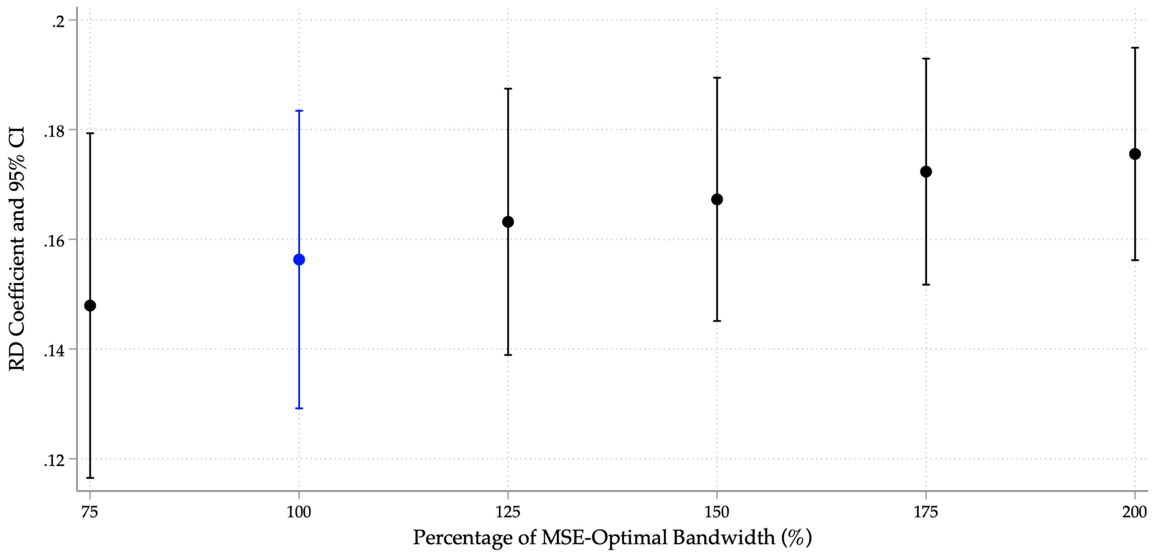
(b) Wealth Cutoff



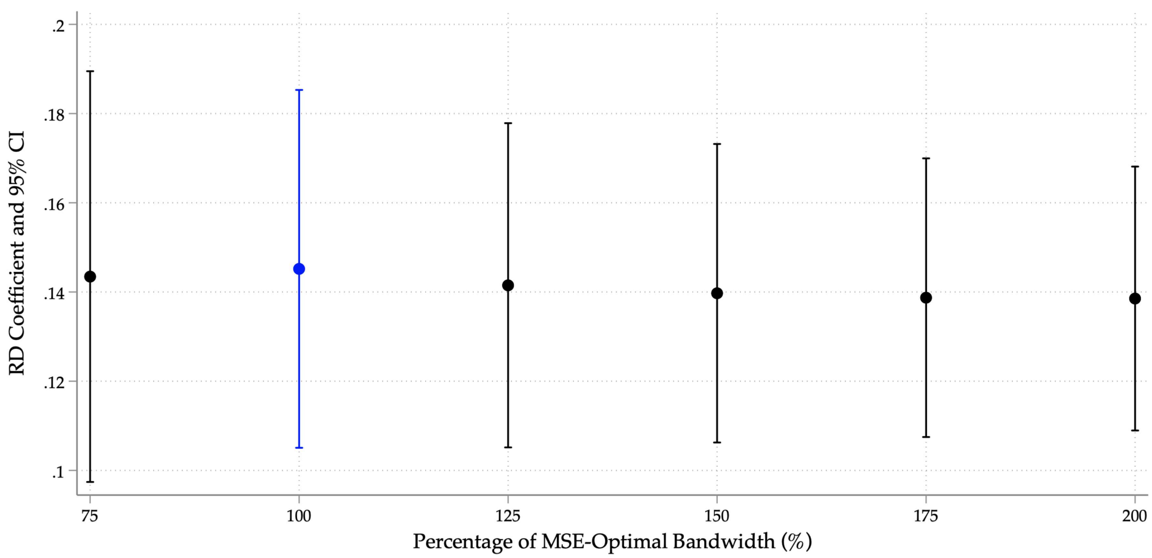
Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test score (SISBEN wealth score) as the running variable, restricting the sample to need- (merit-) eligible students. The dependent variable is the likelihood of accessing an LQ college within six years from high school completion.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SNIES (MEN).

Figure B.7: Bachelor's Degree Earned Within Seven Years from High School

(a) Test Score Cutoff



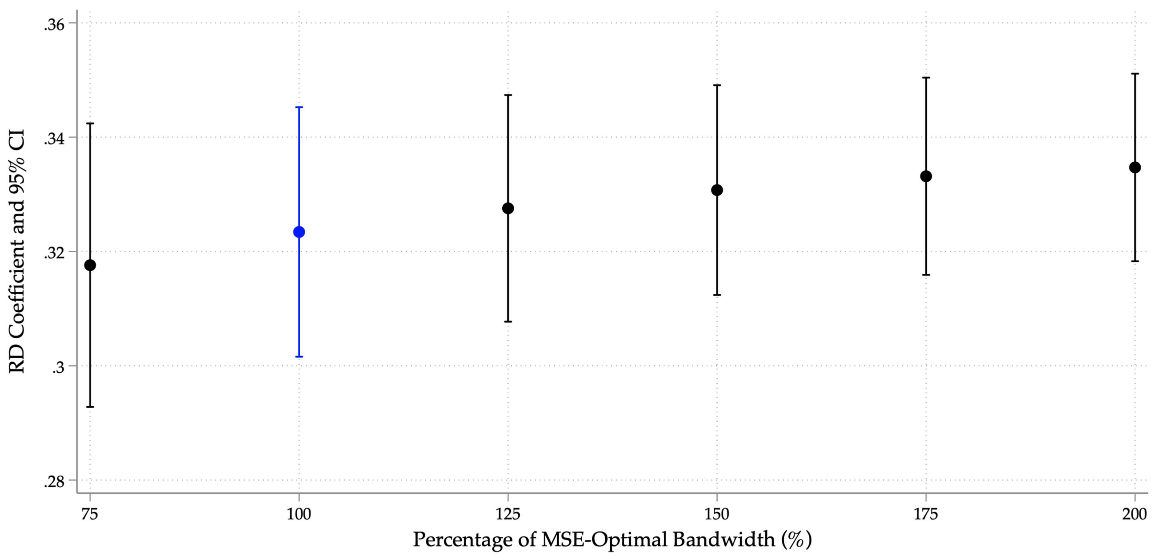
(b) Wealth Cutoff



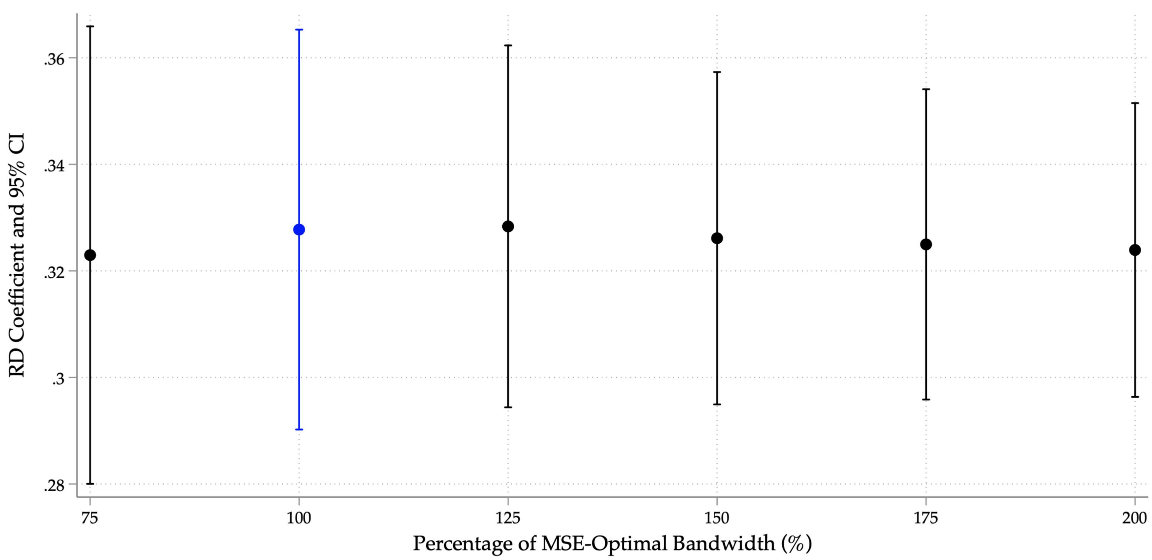
Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test score (SISBEN wealth score) as the running variable, restricting the sample to need- (merit-) eligible students. The dependent variable is the likelihood of earning a bachelor's degree, proxied by taking SABER PRO, within seven years from high school completion.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SABER PRO (ICFES).

Figure B.8: Bachelor's Degree Earned from a High-Quality College Within Seven Years from High School

(a) Test Score Cutoff



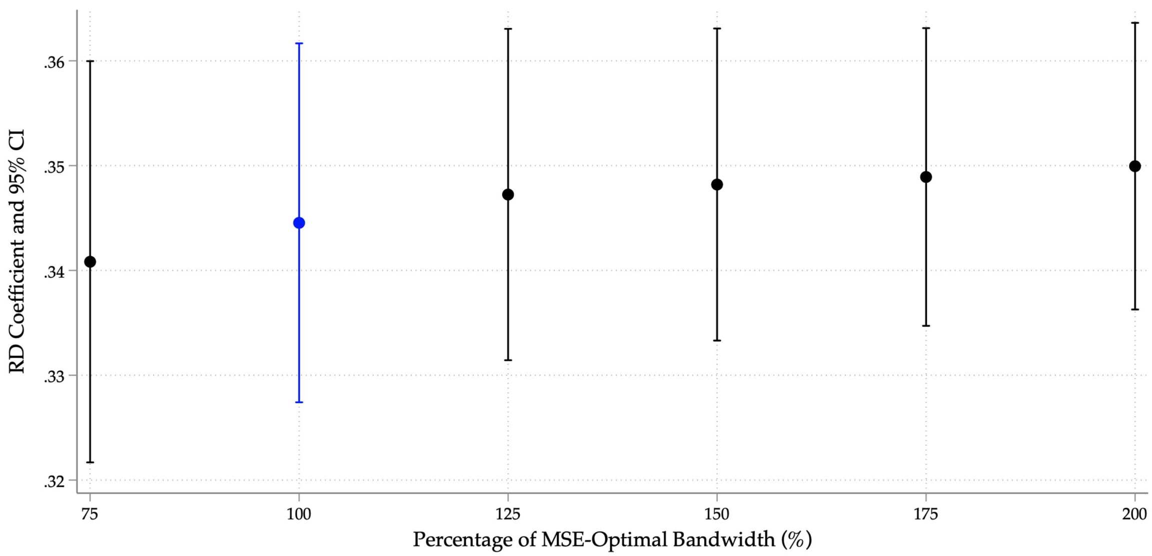
(b) Wealth Cutoff



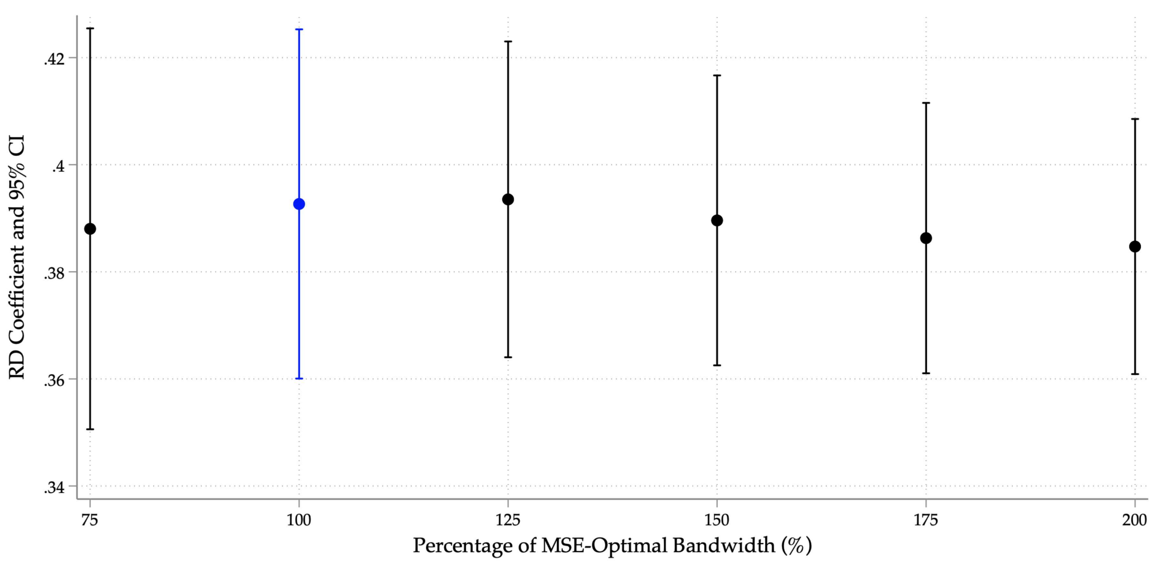
Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test score (SISBEN wealth score) as the running variable, restricting the sample to need- (merit-) eligible students. The dependent variable is the likelihood of earning a bachelor's degree, proxied by taking SABER PRO, from an HQ college within seven years from high school completion.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SABER PRO (ICFES).

Figure B.9: Bachelor's Degree Earned from a High-Quality Private College Within Seven Years from High School

(a) Test Score Cutoff



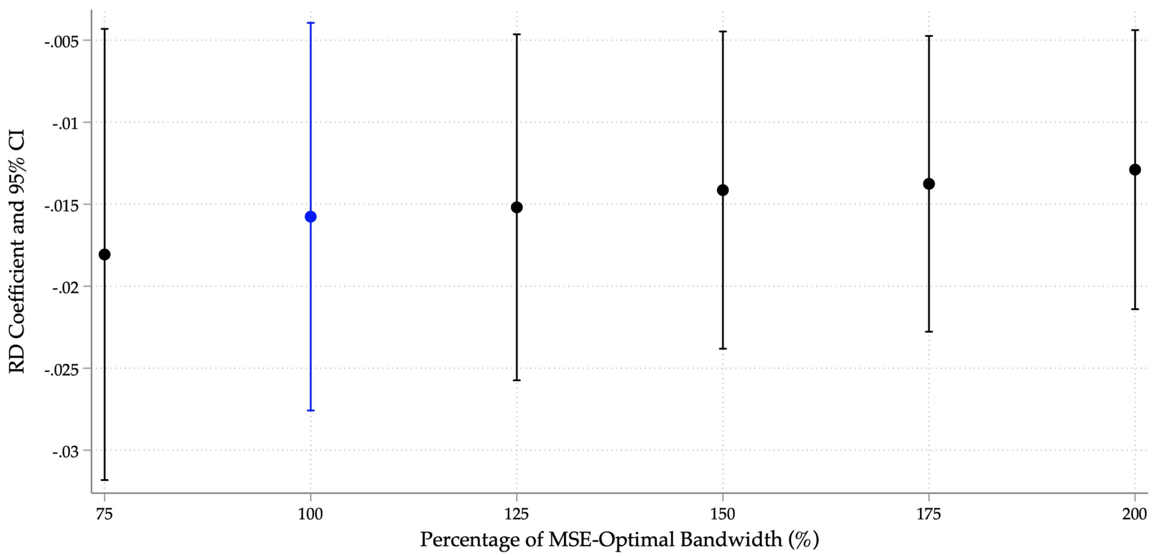
(b) Wealth Cutoff



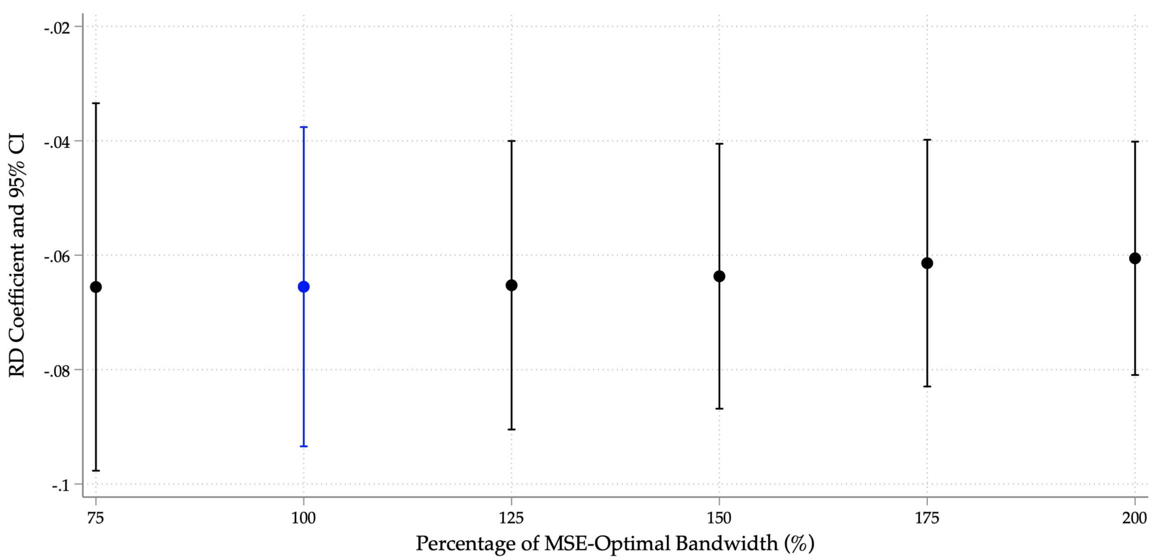
Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test score (SISBEN wealth score) as the running variable, restricting the sample to need- (merit-) eligible students. The dependent variable is the likelihood of earning a bachelor's degree, proxied by taking SABER PRO, from an HQ private college within seven years from high school completion.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SABER PRO (ICFES).

Figure B.10: Bachelor's Degree Earned from a High-Quality Public College Within Seven Years from High School

(a) Test Score Cutoff



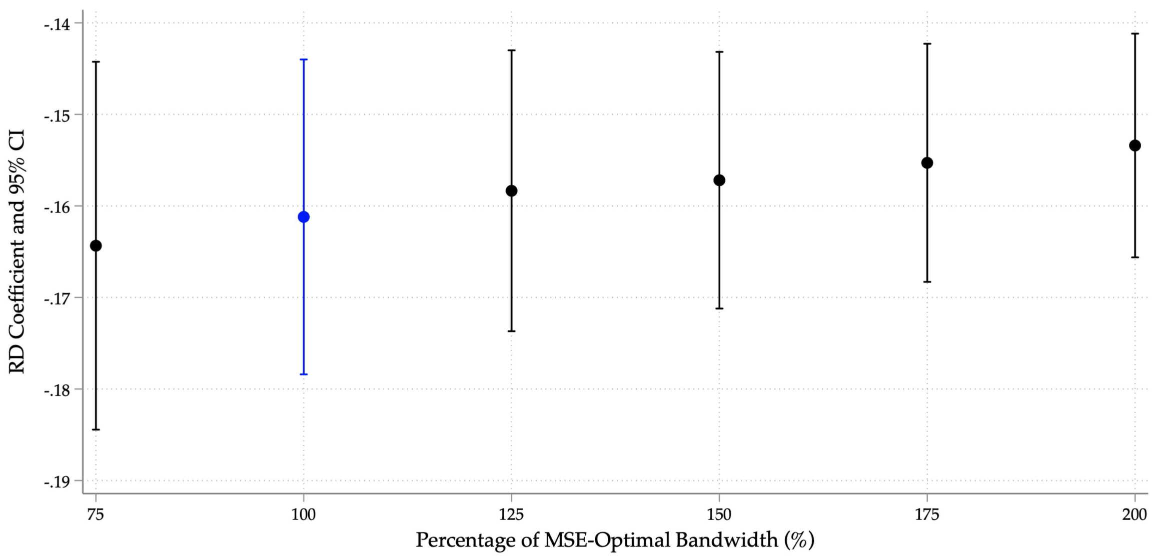
(b) Wealth Cutoff



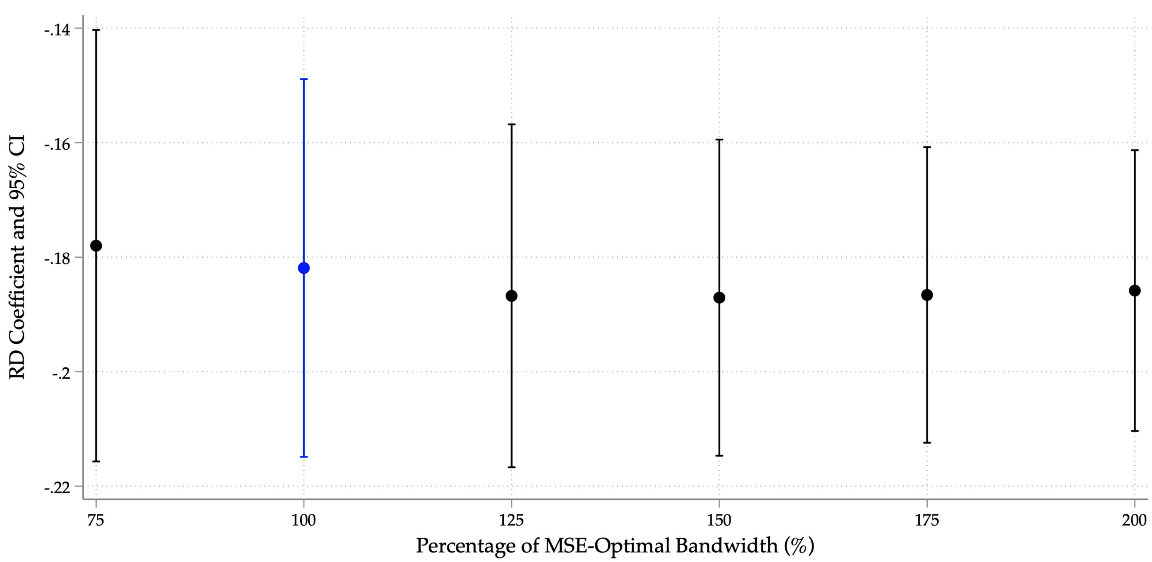
Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test score (SISBEN wealth score) as the running variable, restricting the sample to need- (merit-) eligible students. The dependent variable is the likelihood of earning a bachelor's degree, proxied by taking SABER PRO, from an HQ public college within seven years from high school completion.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SABER PRO (ICFES).

Figure B.11: Bachelor's Degree Earned from a Low-Quality College Within Seven Years from High School

(a) Test Score Cutoff



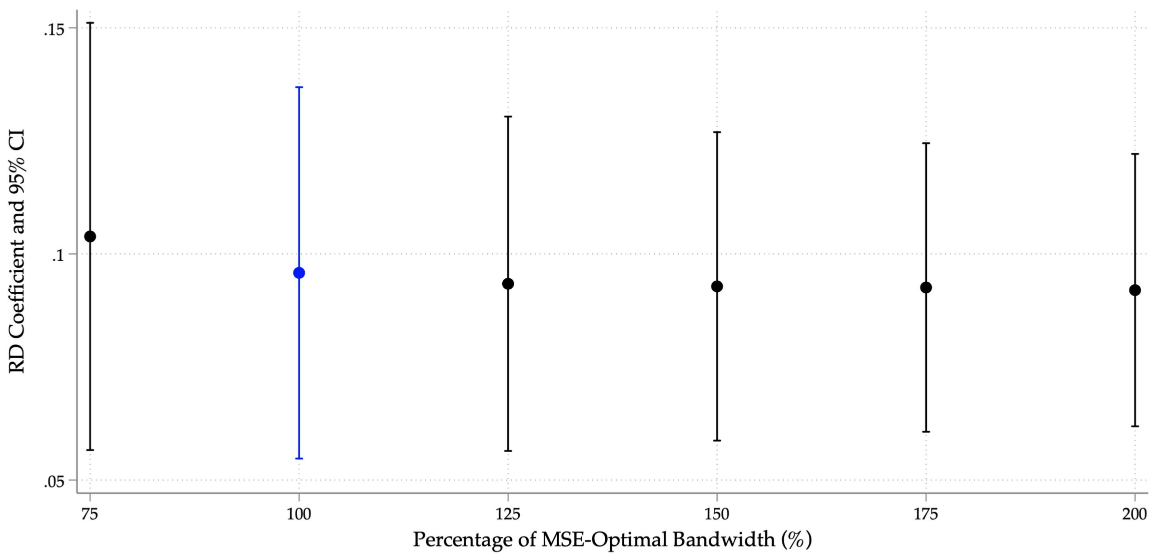
(b) Wealth Cutoff



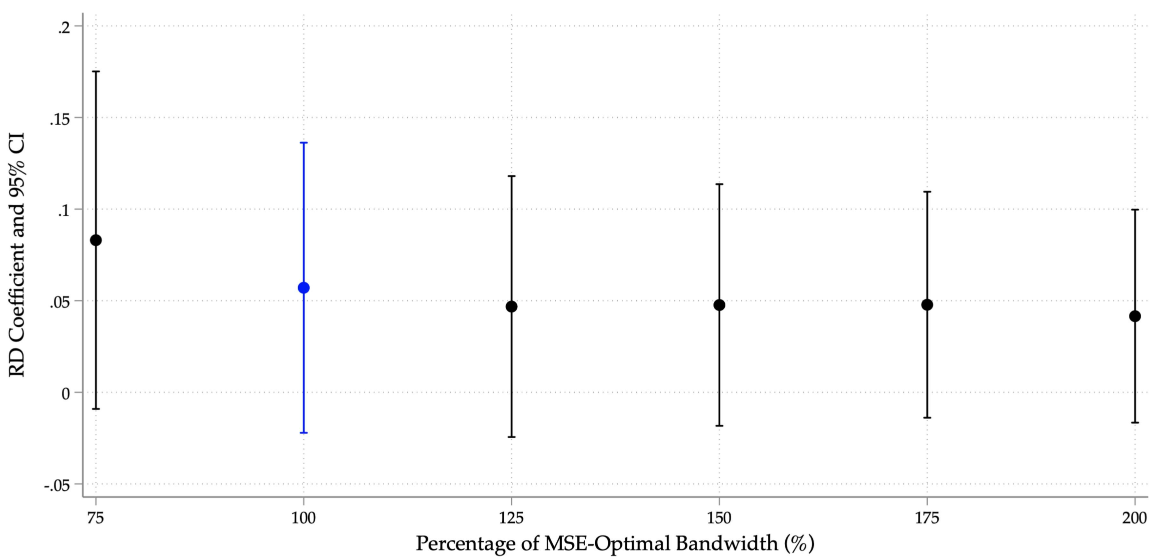
Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test score (SISBEN wealth score) as the running variable, restricting the sample to need- (merit-) eligible students. The dependent variable is the likelihood of earning a bachelor's degree, proxied by taking SABER PRO, from an LQ college within seven years from high school completion.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SABER PRO (ICFES).

Figure B.12: Standardized College Exit Test Score Within Five Years from High School

(a) Test Score Cutoff



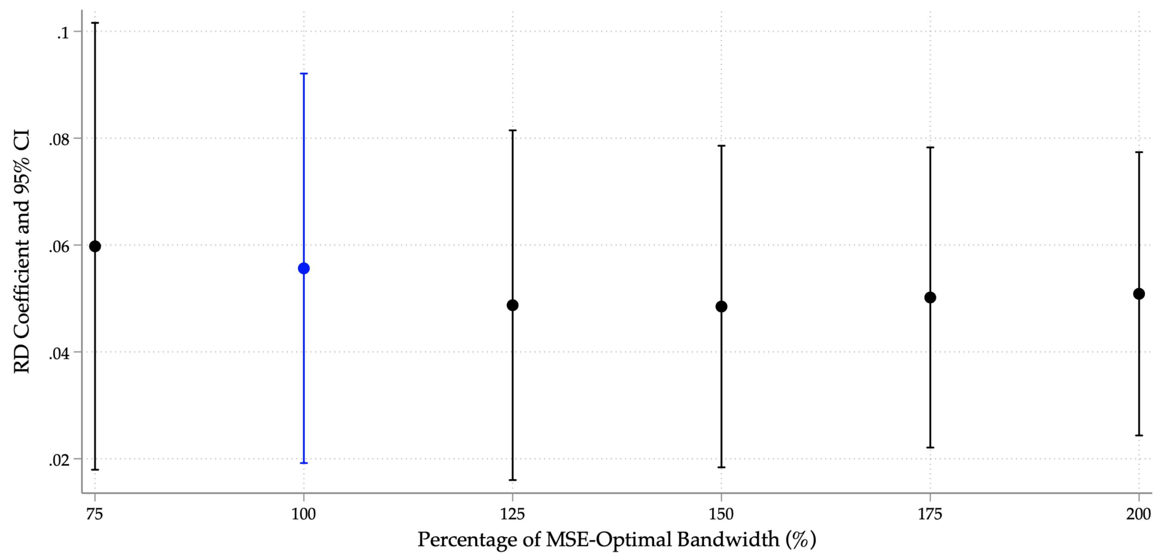
(b) Wealth Cutoff



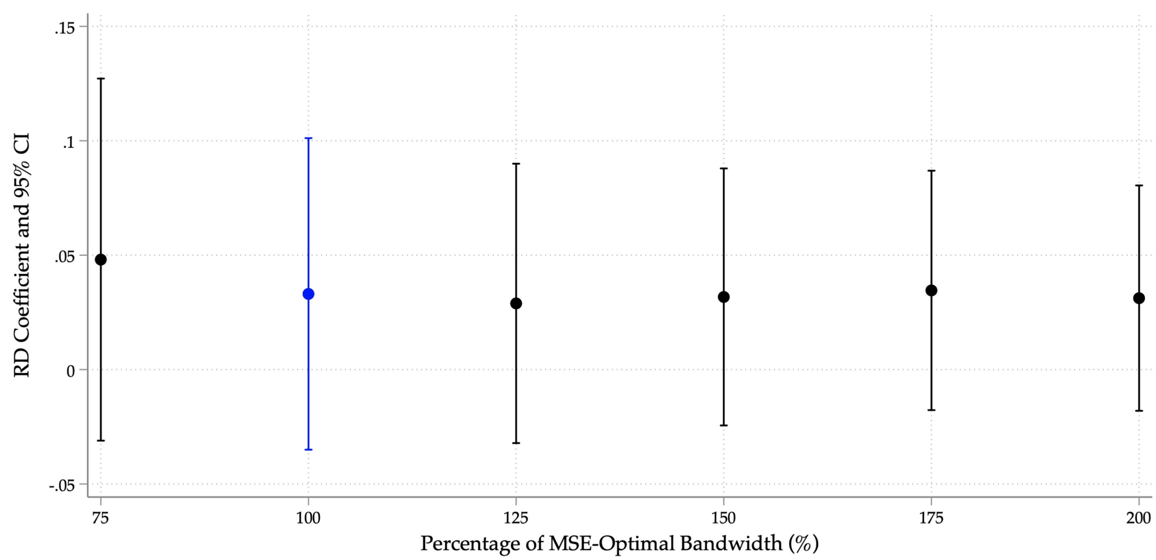
Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test score (SISBEN wealth score) as the running variable, restricting the sample to need- (merit-) eligible students. The dependent variable is the student's performance in SABER PRO for exams taken within five years from high school completion.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SABER PRO (ICFES).

Figure B.13: Standardized College Exit Test Score Within Seven Years from High School

(a) Test Score Cutoff



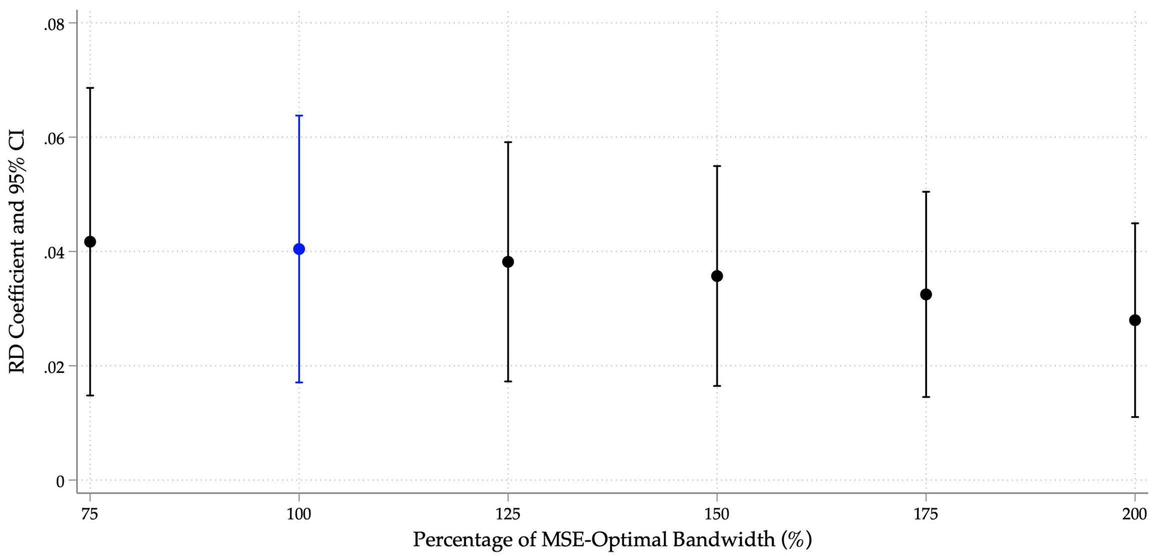
(b) Wealth Cutoff



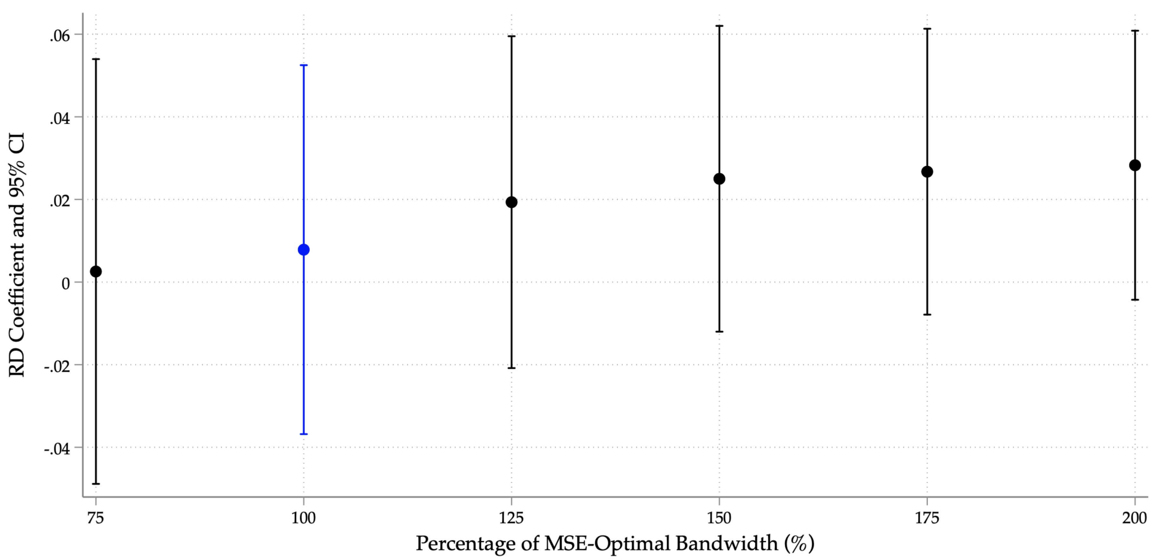
Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test score (SISBEN wealth score) as the running variable, restricting the sample to need- (merit-) eligible students. The dependent variable is the student's performance in SABER PRO for exams taken within seven years from high school completion.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SABER PRO (ICFES).

Figure B.14: Formal Employment Eight Years after High School

(a) Test Score Cutoff



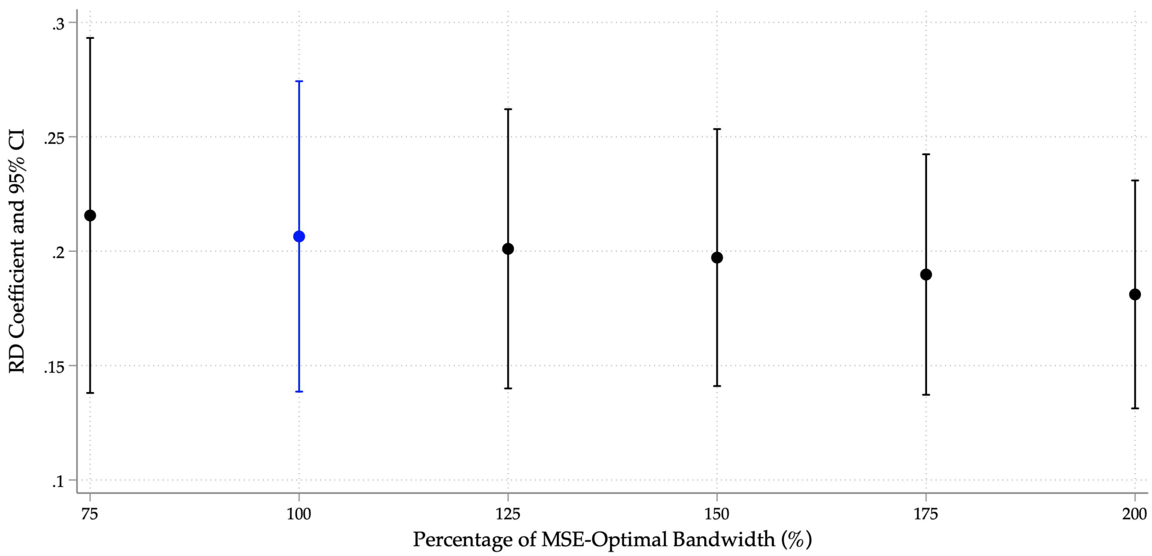
(b) Wealth Cutoff



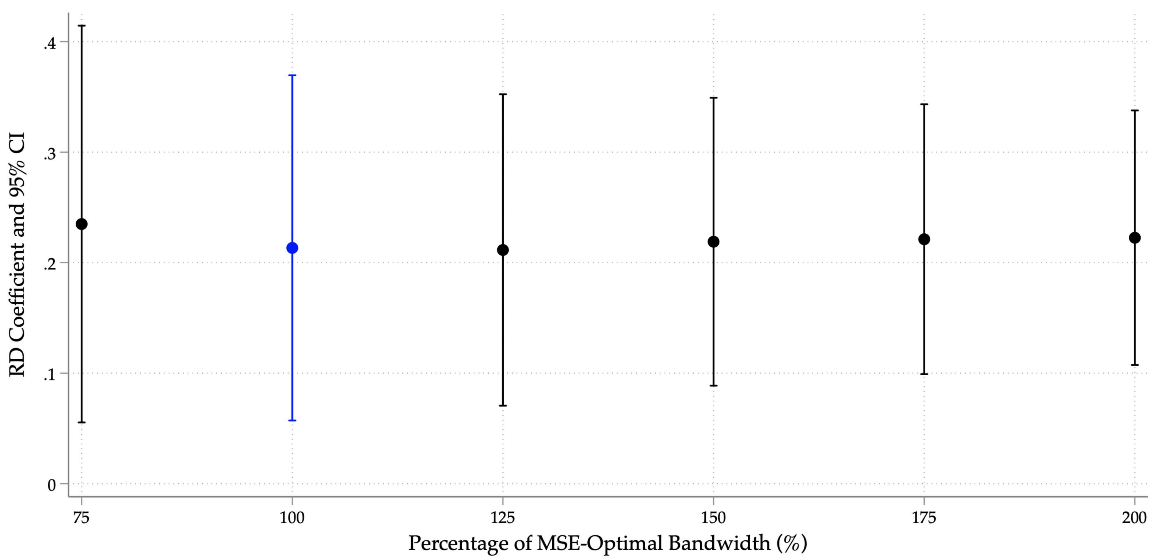
Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test score (SISBEN wealth score) as the running variable, restricting the sample to need- (merit-) eligible students. The dependent variable is the likelihood of being formally employed eight years after high school.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and PILA (MinSalud).

Figure B.15: Formal Earnings (in Min Wages) Eight Years after High School

(a) Test Score Cutoff



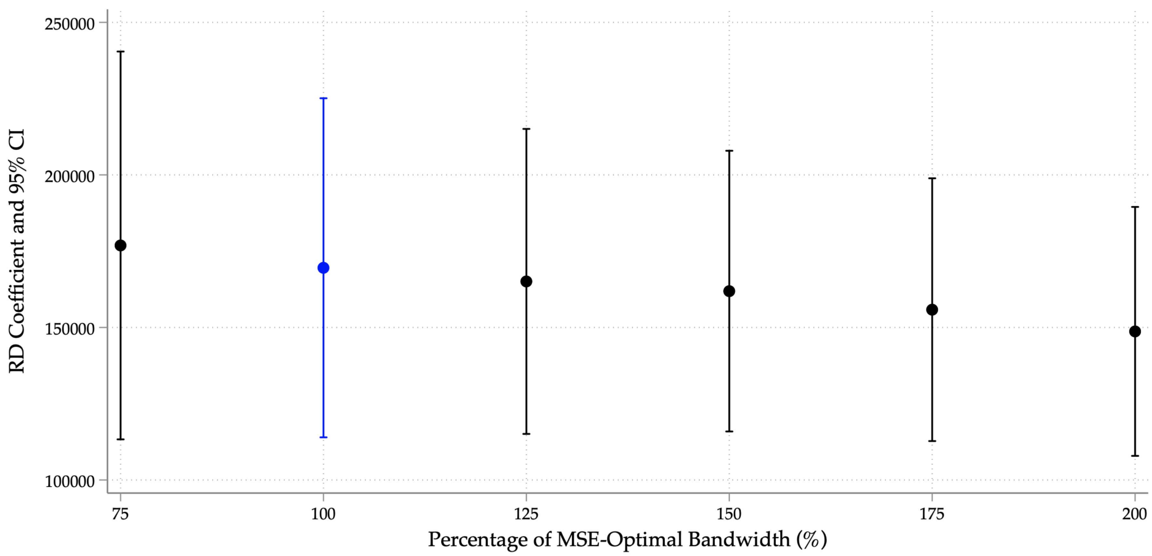
(b) Wealth Cutoff



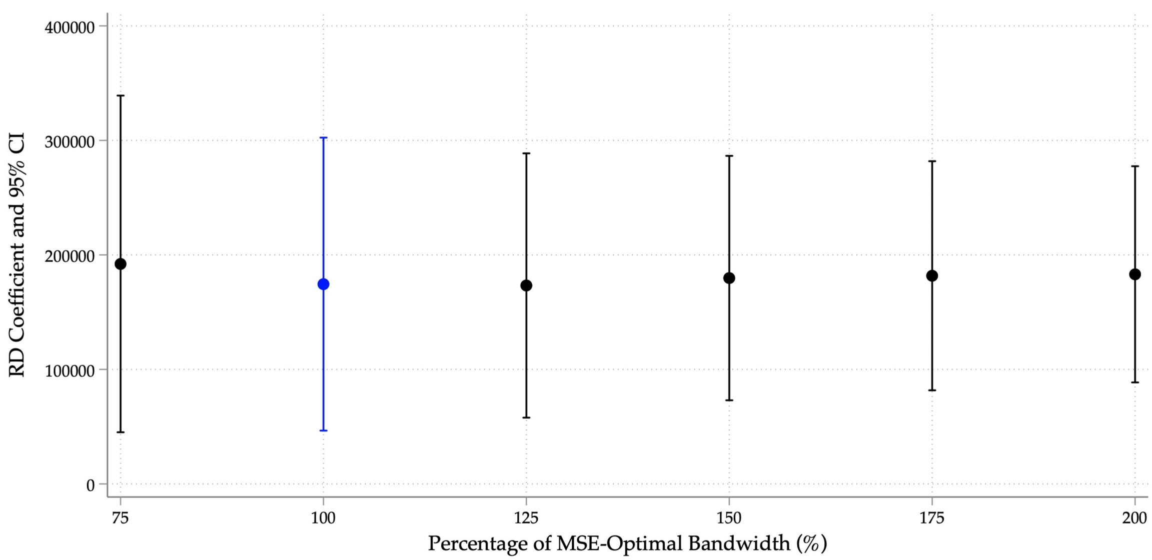
Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test score (SISBEN wealth score) as the running variable, restricting the sample to need- (merit-) eligible students. The dependent variable is formal earnings, measured in multiples of the monthly minimum wage, eight years after high school.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and PILA (MinSalud).

Figure B.16: Formal Earnings (in Constant Pesos) Eight Years after High School

(a) Test Score Cutoff



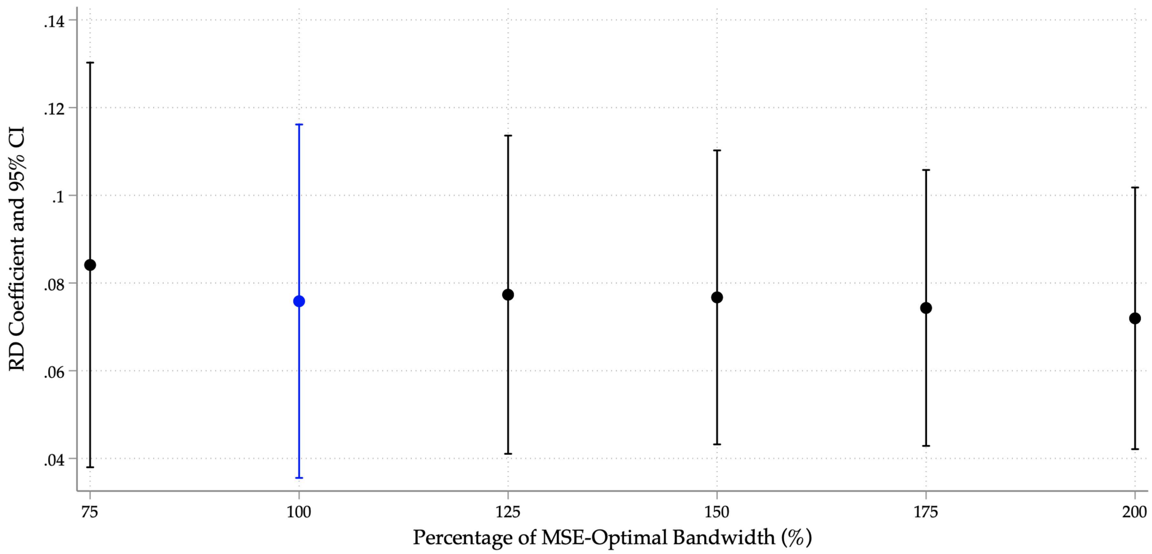
(b) Wealth Cutoff



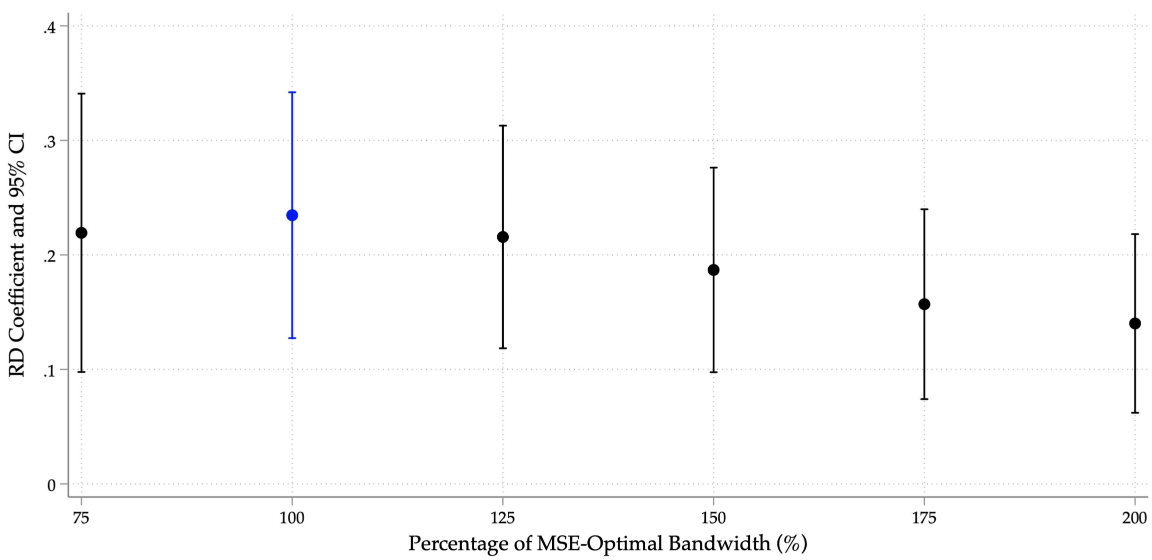
Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test score (SISBEN wealth score) as the running variable, restricting the sample to need- (merit-) eligible students. The dependent variable is formal earnings, measured in December 2021 pesos, eight years after high school.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and PILA (MinSalud).

Figure B.17: Log Formal Earnings Eight Years after High School

(a) Test Score Cutoff



(b) Wealth Cutoff



Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test score (SISBEN wealth score) as the running variable, restricting the sample to need- (merit-) eligible students. The dependent variable is log formal earnings, measured in December 2021 pesos, eight years after high school.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and PILA (MinSalud).

Appendix C Heterogeneity

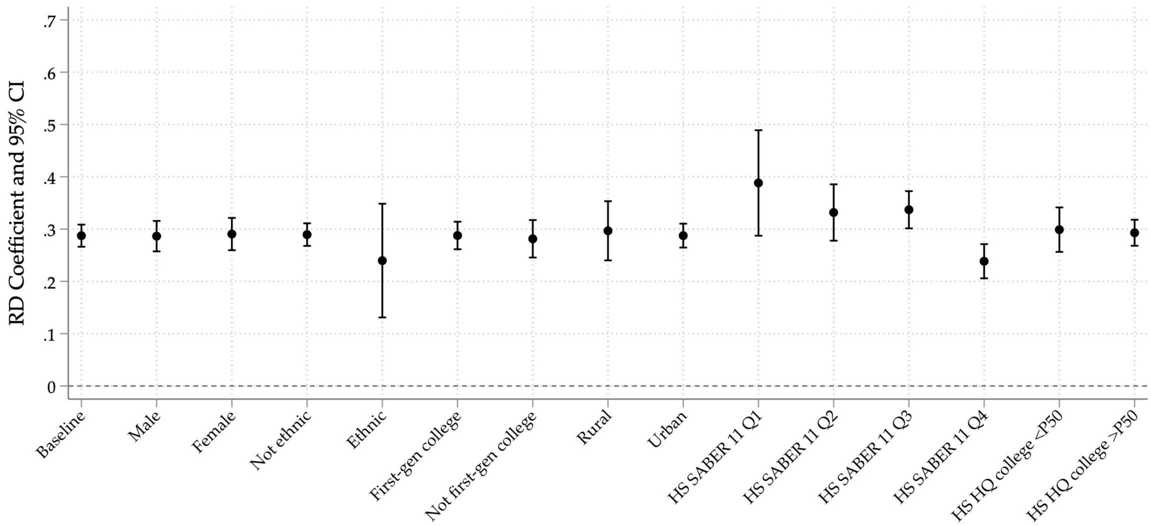
This section provides an overview of the heterogeneous treatment effects of financial aid on students' educational and labor market outcomes. We compare the reduced-form effects of financial aid on college access, quality, bachelor's degree attainment from an HQ college, learning performance, and earnings eight years after high school completion. We analyze these effects by individual, household, and high school characteristics. The results are presented in Figures C.1 to C.5.

Overall, financial aid has positive and significant gains across virtually all baseline characteristics. However, there are three main sources of heterogeneity. First, financial aid has the greatest impact on students from disadvantaged schools. Students graduating from high schools with low test scores and fewer attendees to HQ colleges experience larger gains in college access, graduation, learning, and earnings. However, the effects are noisy at the wealth cutoff due to the limited number of merit-eligible students from these schools (2%). Second, females benefit disproportionately from financial aid in accessing and graduating from HQ colleges. However, females have similar learning and earnings gains to males, as they tend to graduate from fields with lower returns, such as social sciences and humanities (Figure C.6). Third, first-generation college students benefit as much from financial aid as students with college-educated parents.

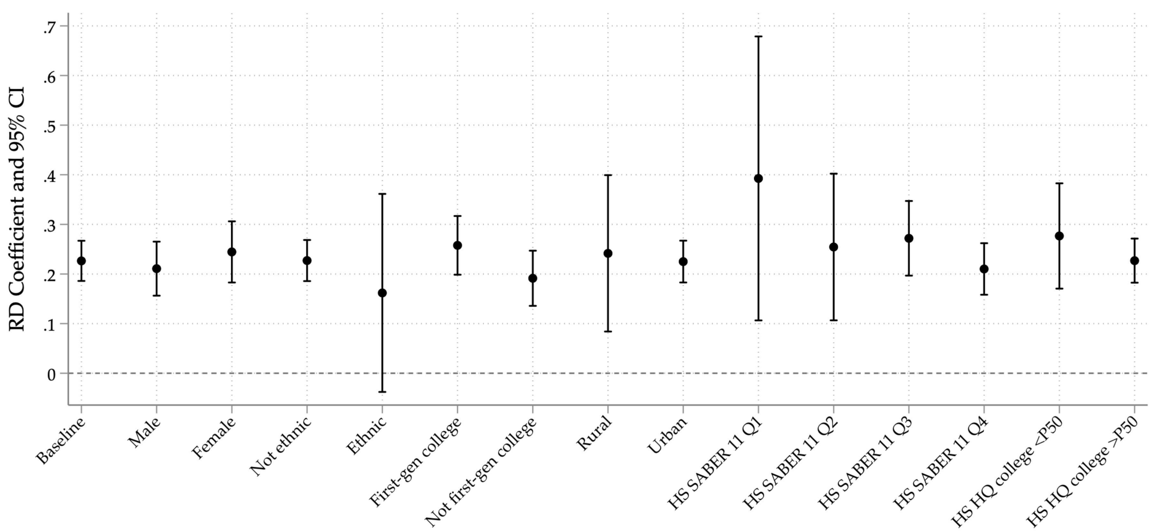
Furthermore, urban students have larger and more precise effects, as the majority of both need-eligible (75%) and merit-eligible (90%) students come from urban areas. However, financial aid also benefits rural students. Regarding ethnicity, the small proportion of self-reported ethnic groups (less than 5% in our study sample) limits the results. However, those who persist in college demonstrate higher learning outcomes, although the impact on earnings remains uncertain.

Figure C.1: Heterogeneous Effects in Immediate Access to Any College

(a) Test Score Cutoff



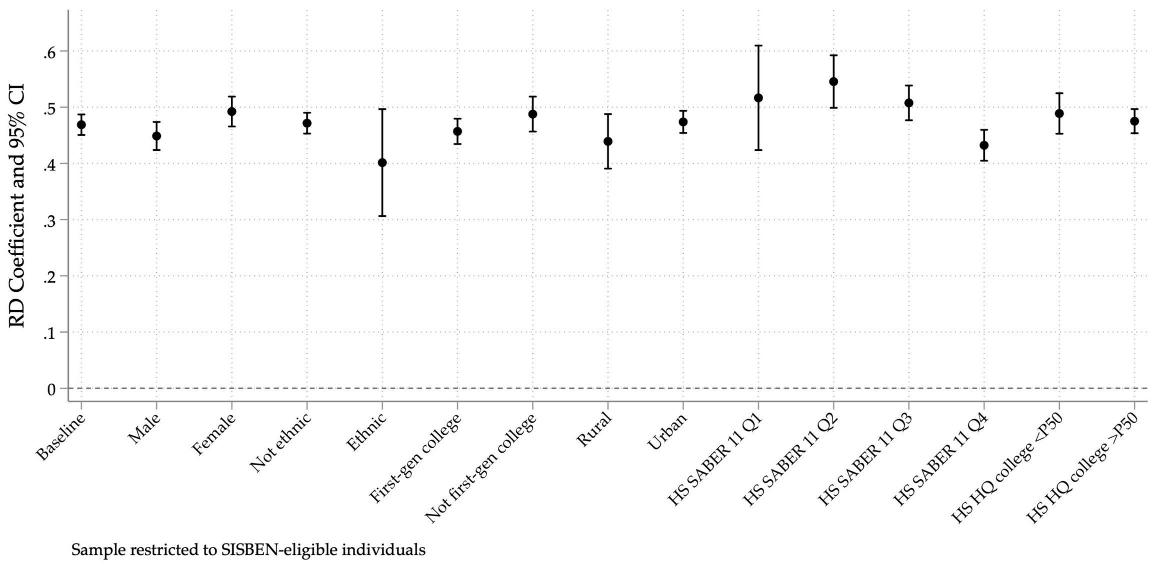
(b) Wealth Cutoff



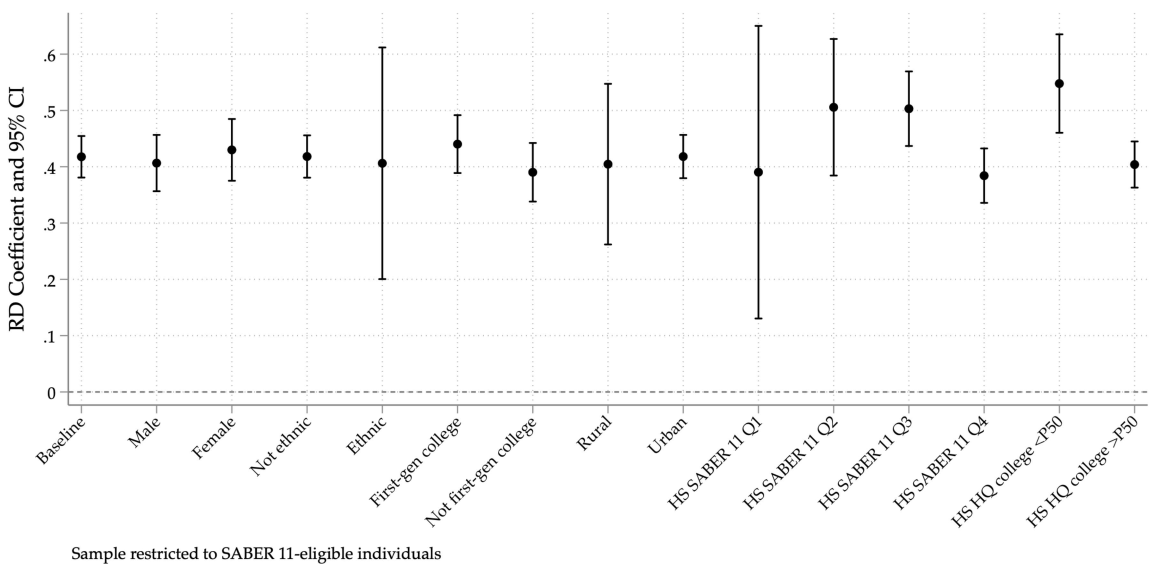
Notes: The figures plot the reduced-form RD coefficient and 95% confidence intervals on immediate access to any college after high school completion. Panel A uses the SABER 11 test score as the running variable, restricting the sample to need-eligible students. Panel B uses the SISBEN wealth index as the running variable, restricting the sample to merit-eligible students.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SNIES (MEN).

Figure C.2: Heterogeneous Effects in Immediate Access to a High-Quality College

(a) Test Score Cutoff



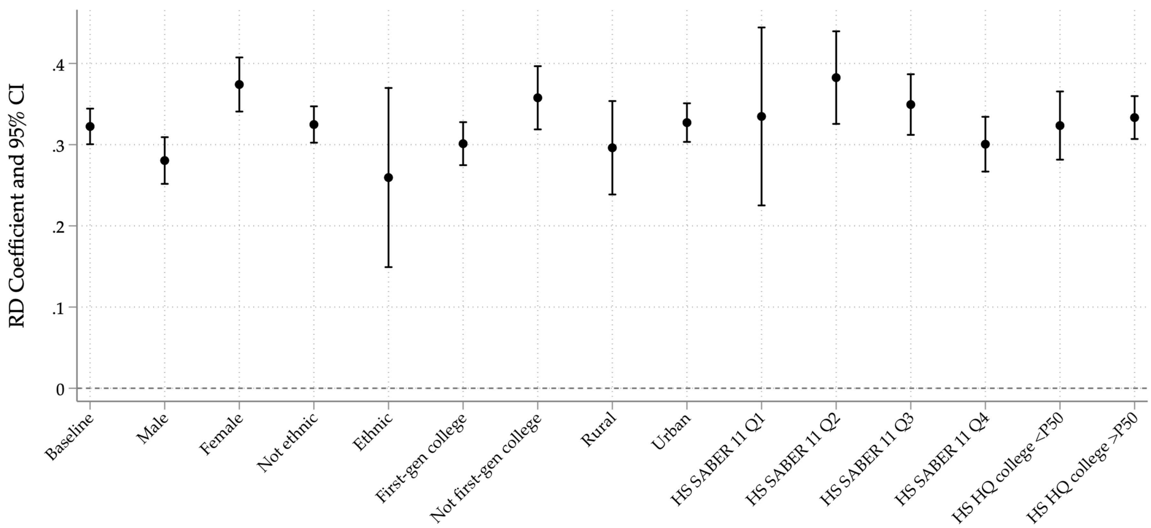
(b) Wealth Cutoff



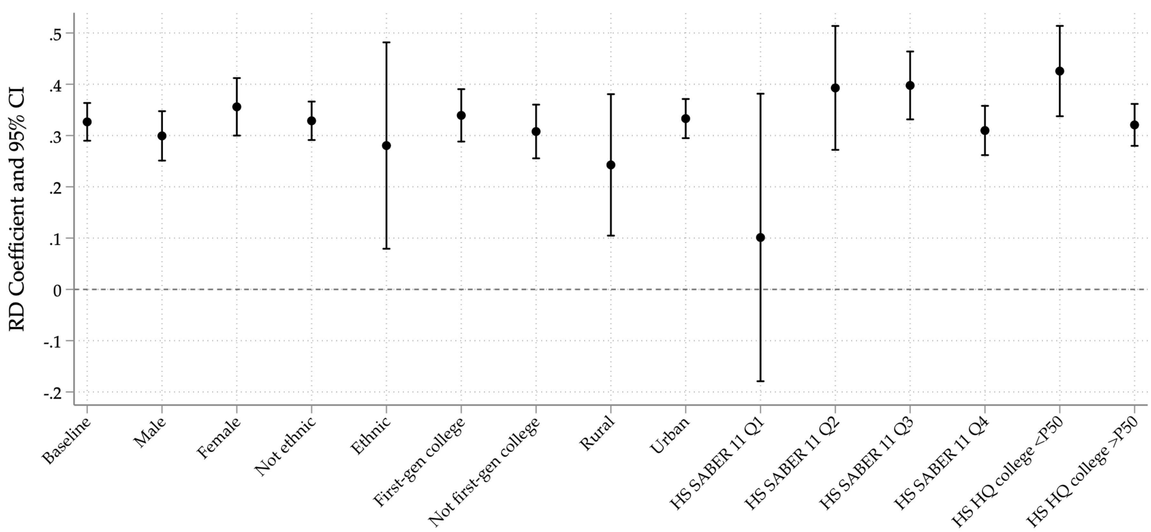
Notes: The figures plot the reduced-form RD coefficient and 95% confidence intervals on immediate access to an HQ college after high school completion. Panel A uses the SABER 11 test score as the running variable, restricting the sample to need-eligible students. Panel B uses the SISBEN wealth index as the running variable, restricting the sample to merit-eligible students.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SNIES (MEN).

Figure C.3: Heterogeneous Effects in Earning a B.A. from a High-Quality College

(a) Test Score Cutoff



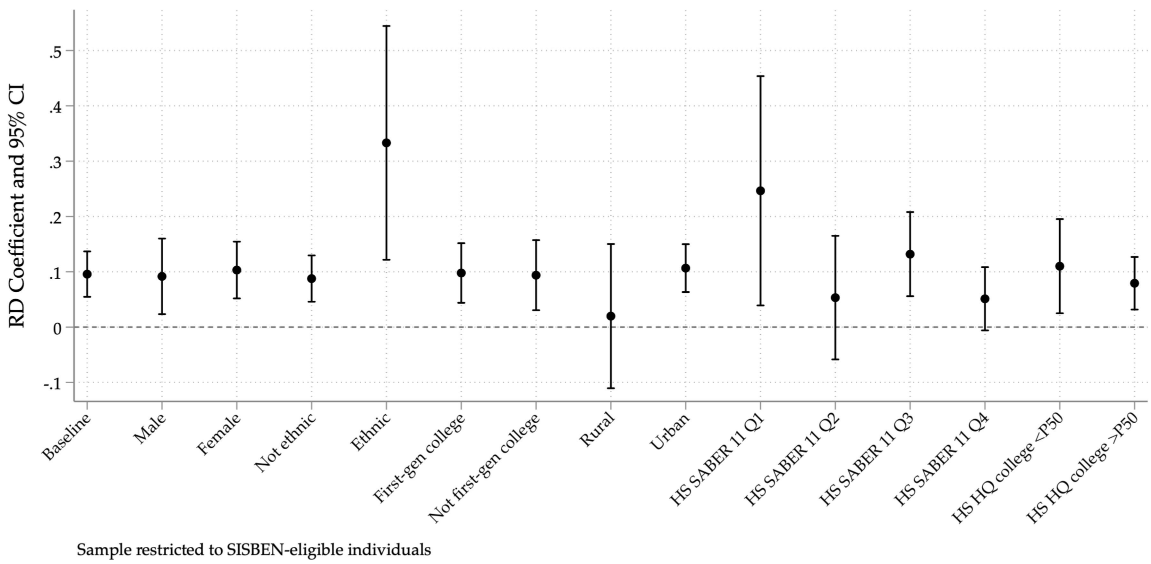
(b) Wealth Cutoff



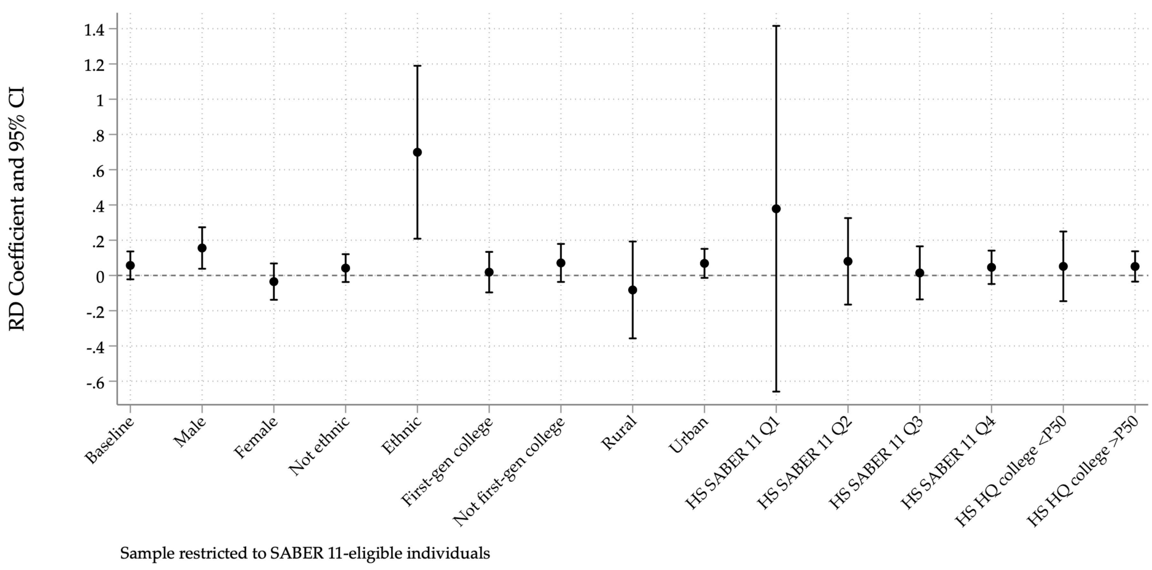
Notes: The figures plot the reduced-form RD coefficient and 95% confidence intervals on the likelihood of earning a bachelor's degree (proxied by taking the SABER PRO exam) from an HQ college within seven years from high school completion. Panel A uses the SABER 11 test score as the running variable, restricting the sample to need-eligible students. Panel B uses the SISBEN wealth index as the running variable, restricting the sample to merit-eligible students.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), SNIES (MEN), and SABER PRO (ICFES).

Figure C.4: Heterogeneous Effects in College Exit Test Scores

(a) Test Score Cutoff



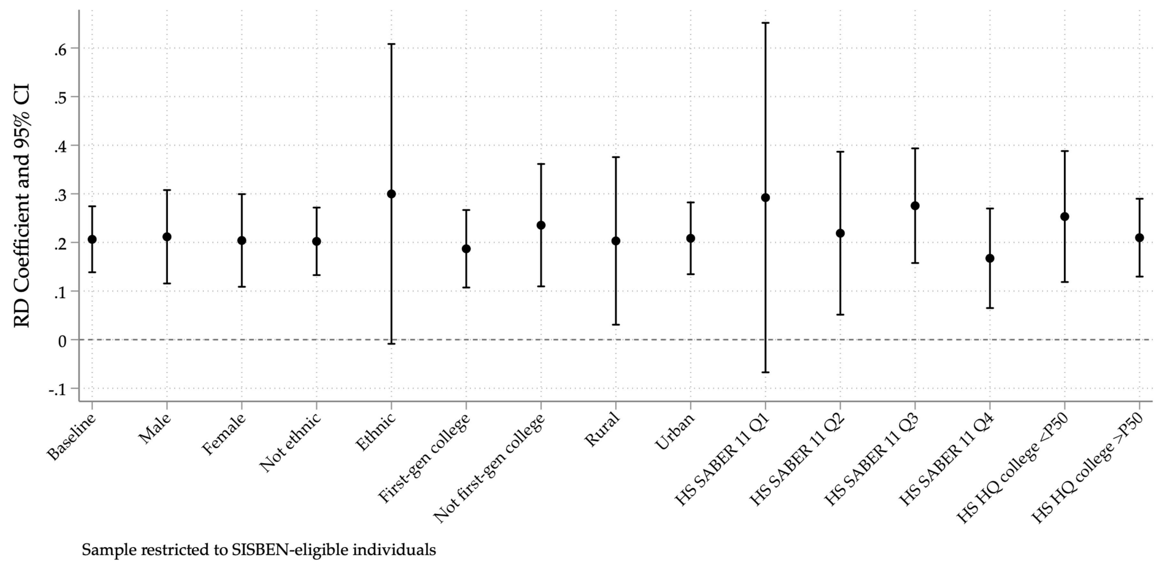
(b) Wealth Cutoff



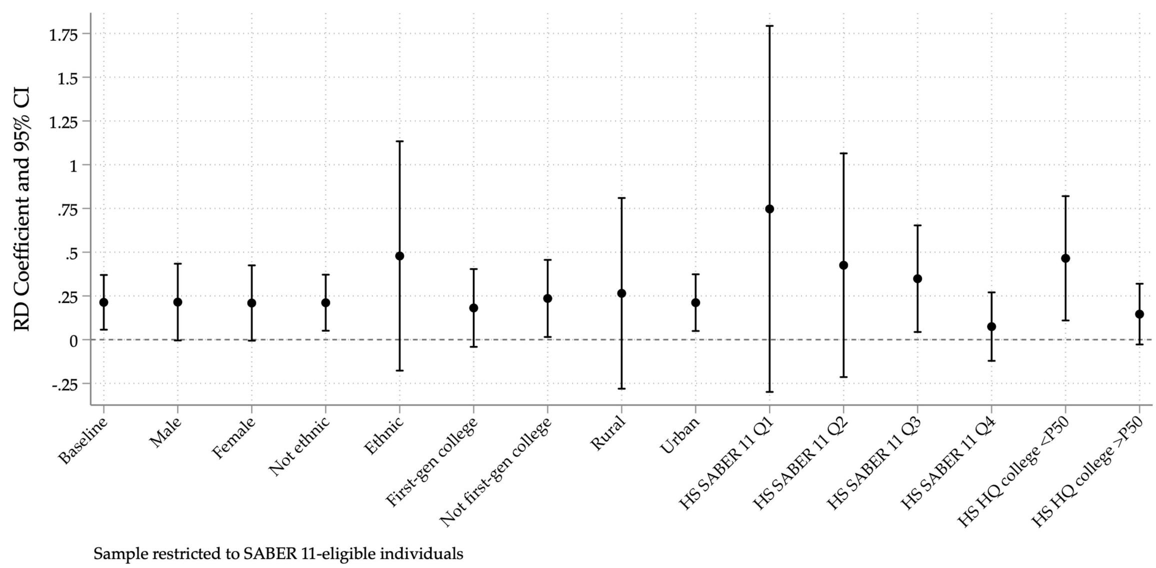
Notes: The figures plot the reduced-form RD coefficient and 95% confidence intervals on the standardized college exit test score for SABER PRO exams taken within five years from high school completion. Panel A uses the SABER 11 test score as the running variable, restricting the sample to need-eligible students. Panel B uses the SISBEN wealth index as the running variable, restricting the sample to merit-eligible students.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), SNIES (MEN), and SABER PRO (ICFES).

Figure C.5: Heterogeneous Effects in Formal Earnings

(a) Test Score Cutoff



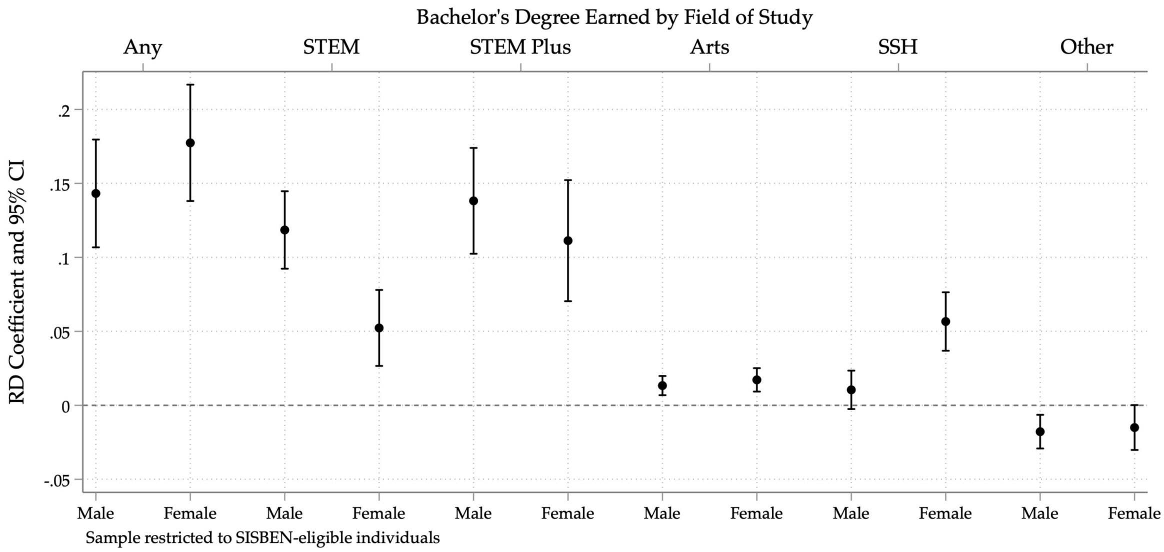
(b) Wealth Cutoff



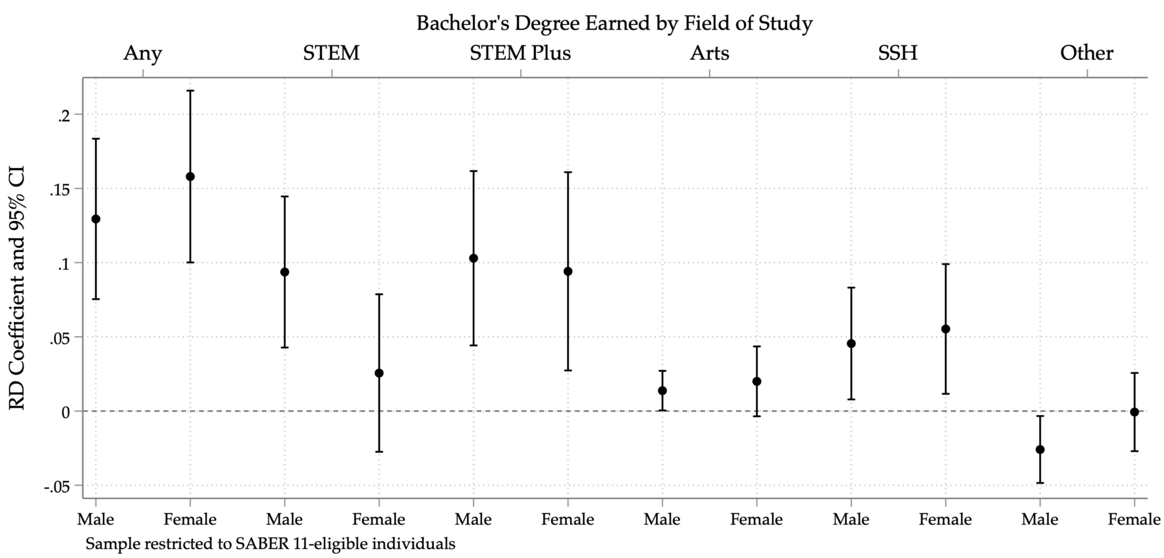
Notes: The figures plot the reduced-form RD coefficient and 95% confidence intervals on formal earnings eight years from high school completion. Earnings are expressed in multiples of the monthly minimum wage and include zeros for individuals without formal employment. Panel A uses the SABER 11 test score as the running variable, restricting the sample to need-eligible students. Panel B uses the SISBEN wealth index as the running variable, restricting the sample to merit-eligible students.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), SNIES (MEN), and PILA (MinSalud).

Figure C.6: Heterogeneous Effects in Earning a B.A. by Gender and Field of Study

(a) Test Score Cutoff



(b) Wealth Cutoff



Notes: The figures plot the reduced-form RD coefficient and 95% confidence intervals on the likelihood of earning a bachelor's degree (proxied by taking the SABER PRO exam) within seven years from high school completion by field of study and sex. Panel A uses the SABER 11 test score as the running variable, restricting the sample to need-eligible students. Panel B uses the SISBEN wealth index as the running variable, restricting the sample to merit-eligible students.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SABER PRO (ICFES).

Appendix D Approximating College "Value-Added"

This section describes how we approximate college graduation, learning, and earning "value-added." Our first objective is to estimate the "value-added" by colleges in terms of graduating students from their programs, giving them knowledge and skills, and increasing students' success in the labor market. Having estimated these college and program-specific productivities, we then use these measures as our outcomes of interest using an RD approach.

We use student-level data from fall 2012 and 2013 test takers to estimate the fixed effects. These cohorts graduated from high school before Colombia introduced SPP. Since we are interested in their outcomes realized within seven or eight years from high school completion, the outcomes will be realized by 2019 to 2021 for these cohorts.

We predict the fixed effects from the following individual-level regression:

$$y_{i,t} = \alpha + \mathbf{X_i}'\Gamma + \delta_{j(i,t)} + \epsilon_{i,t} \tag{3}$$

where $y_{i,t}$ is the outcome for individual i taking the SABER 11 exam in semester t, $\mathbf{X}$ is a vector of baseline covariates, $\delta_{j(i,t)}$ are the college fixed effects based on the first institution attended, and $\epsilon_{i,t}$ is a student-specific error term.

We focus on five main outcomes: (1) any degree attainment, proxied by an indicator for taking the SABER PRO or SABER T&T exams, (2) bachelor's degree attainment, proxied by an indicator for taking the SABER PRO exam, (3) the SABER PRO test score, (4) formal employment, and (5) formal monthly earnings, measured in multiples of the monthly minimum wage.

Our empirical specification includes relevant student demographic information related to these outcomes of interest and selection into specific colleges, majors, and programs. When estimating the model at the college level, we drop students attending colleges with fewer than 50 students. This leaves us with 288 colleges. However, prospective students apply to specific college-program pairs from the moment they first apply for access to higher education in Colombia, and programs vary significantly in their selectivity. For this reason, we estimate the "value-added" contributions by more granular cells to account for within-college variation across programs:

1. There are eight study areas (*áreas del conocimiento*) according to SNIES: agriculture and veterinary, arts, education, health, social sciences and humanities, economics and business, engineering and architecture, and math and natural sciences. Following Ferreyra et al. (2020), we drop cells with fewer than 10 students. This leaves us with 1,145 college-field cells.

2. There are 55 study majors (*núcleos básicos del conocimiento*) according to SNIES, e.g.,

economics. Following Ferreyra et al. (2020), we drop cells with fewer than 10 students. This leaves us with 2,653 college-major cells.

3. There are many more study programs, which is the level at which most students apply to college. Following Ferreyra et al. (2020), we drop cells with fewer than 10 students. This leaves us with 4,688 college-program cells.

Thus, we estimate four models replacing the college fixed effect $\delta_{j(i,t)}$ in Specification (3) with a college-field fixed effect $\delta_{j(i,t)f(i,t)}$, a college-major fixed effect $\delta_{j(i,t)m(i,t)}$ or a college-program fixed effect $\delta_{j(i,t)p(i,t)}$.

In addition, we examine how the estimated fixed effects for these three models vary when progressively including a denser set of baseline covariates controlling for differential peer cohort qualities to obtain "value-added" college contributions purged of cohort effects:

- **Model A** controls for individual and household characteristics; specifically, students' age and SABER 11 score (using third-degree polynomials), sex, whether he or she self-identifies as an ethnic minority, household size, socioeconomic stratum, SISBEN score, parental educational attainment, an indicator for the semester in which the student took the SABER 11 exam, and third-degree polynomials of distance to the college. These variables enable controlling for selection bias due to students' choices of colleges, fields, majors, and programs.

- **Model B** adds dummies for high school schedules, private institutions, and being located in an urban area.

- **Model C** includes the high school-by-cohort leave-one-out mean socioeconomic stratum, SABER 11 test scores, and parental education.

- **Model D** adds the leave-one-out average SABER 11 score of the entering cohort in the college (or college-field, college-major, or college-program), which controls for a big part of the selection into colleges (Melguizo et al., 2017).

- **Model E** adds the leave-one-out mean socioeconomic strata and parental education of the cohort in the college (or college-field, college-major, or college-program), as students' outcomes might be influenced by the socioeconomic characteristics of their peers.

- **Model F** includes the leave-one-out mean SISBEN score of the cohort in the college (or college-field, college-major, or college-program).

We begin by examining the impact of including baseline covariates on the estimated college fixed effects, using bachelor's degree attainment as an example. To focus on students who have the opportunity to graduate from four- or five-year undergraduate programs, we exclude those who do not access any such program within six years of high school. Figure D.1 compares the distributions of college fixed effects estimated using Models A through F, while Table D.1 displays the means by college type. A naive model that does not control for $\mathbf{X}$ suggests that HQ *private* colleges have the highest graduation "value-added." However, this finding can be attributed to the fact that these institutions admit students with exceptionally high test scores and privileged socioeconomic backgrounds, who are generally less likely to drop out. When we account for observable differences in Model A, such as baseline test scores and socioeconomic and demographic characteristics, the graduation "value-added" for HQ colleges decreases, indicating significant sorting of students across programs and college types. Furthermore, Models B through F include a more comprehensive set of baseline covariates, resulting in further reductions in the "value-added" of HQ colleges. This indicates that a significant portion of the graduation effect is explained by differences in individual, household, high school, and peer qualities across college-program combinations. Our preferred model, Model F, controls for the fullest set of baseline characteristics. According to this model, LQ *private* colleges exhibit the highest graduation "value-added," while HQ *public* colleges demonstrate the lowest. Overall, these findings highlight the importance of accounting for baseline covariates when estimating college fixed effects and reveal the nuanced factors that contribute to graduation "value-added" across different college types.

Figure D.2 presents a comparison of fixed effects for Model F, focusing on different levels of granularity: colleges, college-field pairs, college-major pairs, and college-program pairs. By analyzing more specific cells, we can observe how the spread in graduation "value-added" across college types decreases. This is because students choose specific programs, and colleges vary in their selectivity across different fields, majors, and programs. Among these levels of analysis, our preferred unit is the college-program pair. This is the level at which most prospective students apply for access to higher education. Examining fixed effects at this level allows us to gain valuable insights into the factors influencing graduation "value-added" and understand the variations across different college-program pairs.

Table D.1 provides a comparison of the college-program fixed effects for various outcomes and different models that control for different levels of baseline covariates. The table reveals three main findings. Firstly, after adjusting for entry test scores, the coefficients on learning "value-added" are relatively stable across models.[33]Furthermore, HQ colleges demonstrate strong performance in imparting knowledge and skills, with HQ *private* institutions exhibiting the highest "value-added" in this aspect. Secondly, when it comes to employment outcomes, *private* colleges, both HQ and LQ, outperform public colleges. Interestingly, HQ *public*

[33] Indeed, conditioning the sample to students close to graduation gets rid of a major source of selection of students by ability and SES.

institutions exhibit the lowest employment "value-added." Thirdly, HQ *private* colleges demonstrate the highest "value-added" in terms of earnings, with a "value-added" twice as large as that of LQ *private* colleges. Conversely, HQ *public* colleges display the lowest "value-added" in terms of earnings compared to all other types of colleges, after accounting for selection across programs and the qualities of peer cohorts. Interestingly, the table also reveals that many colleges and programs have a negative "value-added" for employment and earnings. This implies that students' labor market outcomes eight years after high school would have been better if they had not attended any college.

Table D.2 presents the reduced-form RD coefficients for various educational and labor market outcomes, along with the estimates based on the college-program "value-added." Figures D.3 to D.7 visualize these effects using Model F. It is important to note that the college-program fixed effects are estimated using pre-policy cohorts (2012 and 2013), while the RD sample consists of the post-policy cohort (2014).

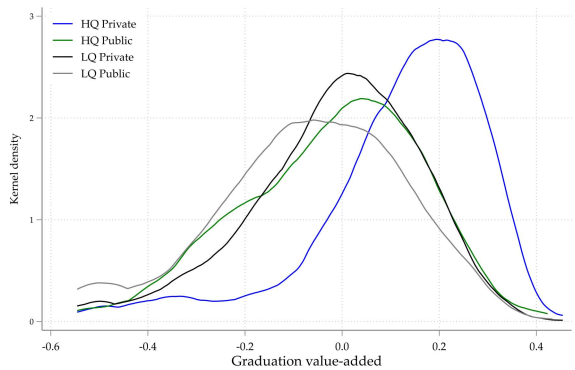
There are three key findings. Firstly, the shift in financial aid from short-cycle programs to bachelor's programs, which are more challenging to complete, would have been expected to result in reduced degree attainment and only a slight increase in bachelor's degree attainment. (The predicted impact is twice as large for students near the wealth cutoff compared to those near the test score cutoff, as the former switched away from HQ public colleges with lower graduation "value-added.") Surprisingly, financial aid significantly increased the likelihood of graduating from these programs. This suggests that the attainment gains are not driven by the specific college-program combinations chosen by students but may be influenced by the strong graduation incentives of the SPP program.

Secondly, financial aid influenced students to opt for college-program combinations with higher learning "value-added." The effect is notably larger for students near the test score cutoff, as they were more inclined to attend LQ colleges, which exhibit lower learning "value-added" according to Table D.1. However, the predicted effect exceeds the observed impact of financial aid at both cutoffs, indicating that financial aid encouraged some students who might have dropped out to successfully graduate.

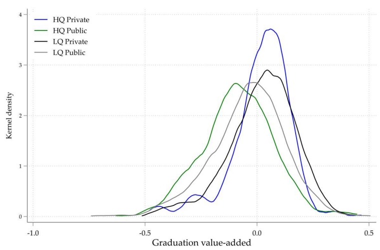
Thirdly, financial aid directed students toward colleges with higher earnings "value-added," although the effects are approximately half the size of the observed effects. This implies that the gains experienced by financial aid recipients surpass the average returns associated with the college-program combinations they choose to attend.

Figure D.1: The Distribution of College Fixed Effects for Four-Year Degree Attainment By Baseline Controls

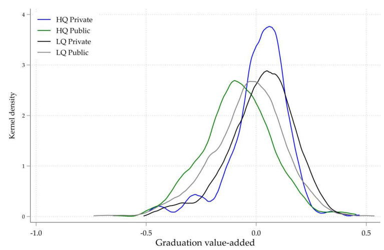
(a) No controls



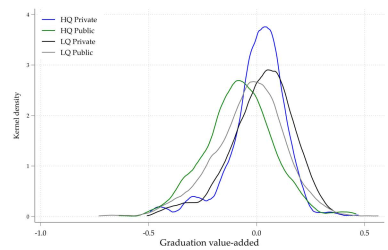
(b) Model A



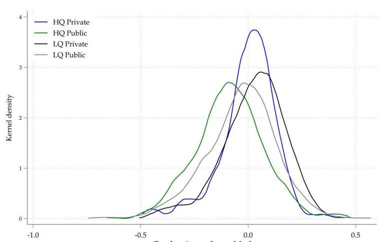
(c) Model B



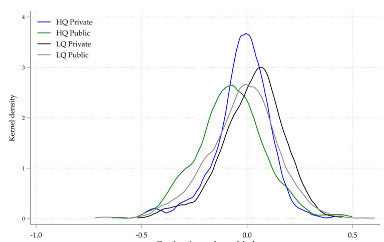
(d) Model C



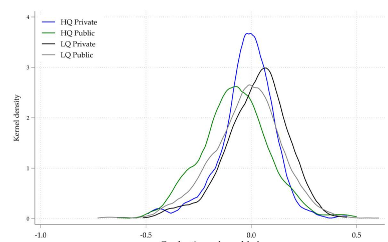
(e) Model D



(f) Model E



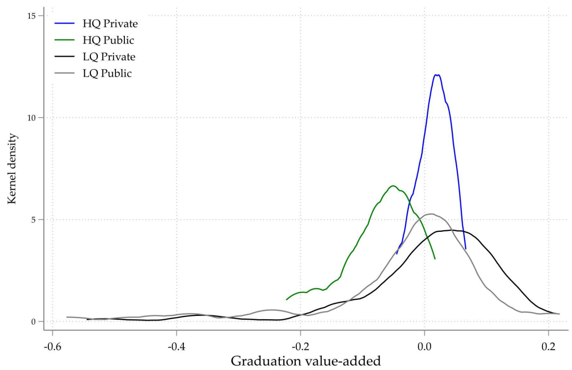
(g) Model F



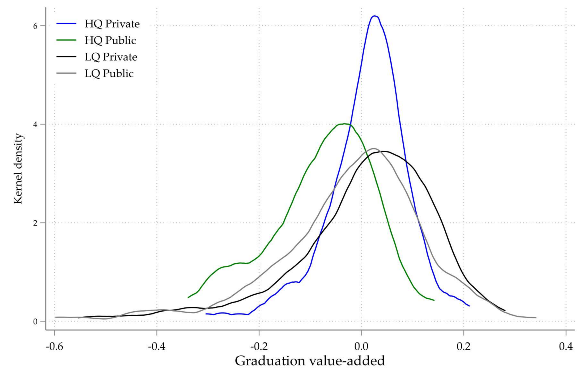
Notes: The figure plots the distribution of college-program fixed effects $\hat{\delta}_{j(i,t)p(i,t)}$ estimated using Specification (1), where the outcome variable is the likelihood of taking a SABER PRO exam within seven years from high school completion. The fixed effects are plotted separately by college type, and models A through F progressively add baseline covariates. The sample is restricted to students who ever attended a four- or five-year undergraduate program within six years from high school completion. Table D.1 reports the mean fixed effects by college type.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), SNIES (MEN), and SABER PRO (ICFES).

Figure D.2: Graduation Productivities of Colleges, Fields, Majors, and Programs

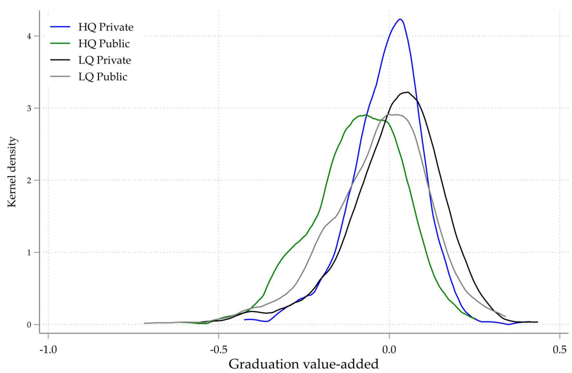
(a) College Fixed Effects



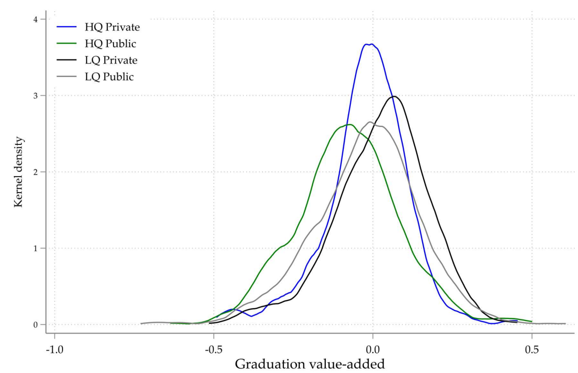
(b) College-Field Fixed Effect



(c) College-Major Fixed Effect



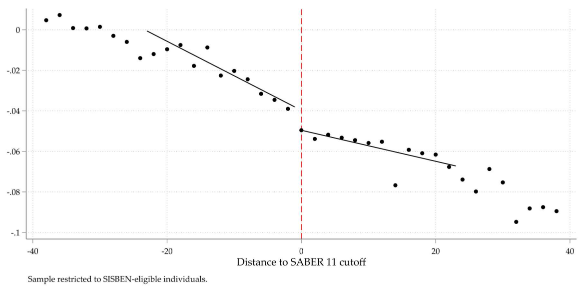
(d) College-Program Fixed Effect



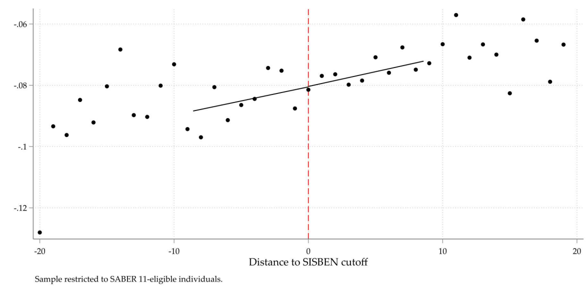
Notes: The figure plots the distribution of college, college-field, college-major, and college-program fixed effects estimated using Specification (1) and Model F where the outcome variable is the likelihood of taking a SABER PRO exam within seven years from high school completion. The fixed effects are plotted separately by college type.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), SNIES (MEN), and SABER PRO (ICFES).

Figure D.3: SPP's Impact on College-Program Graduation "Value Added"

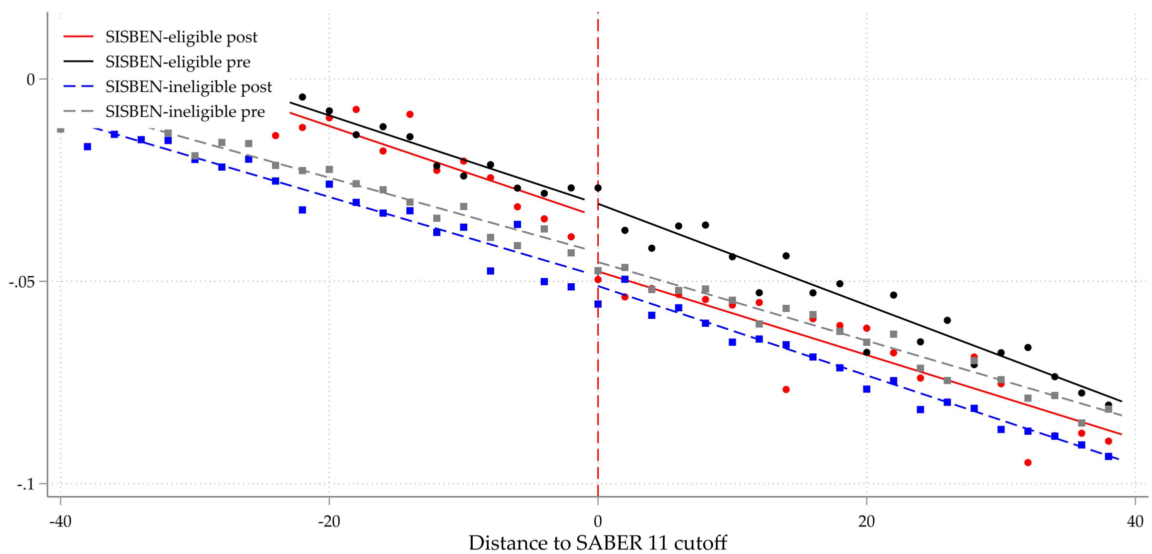
(a) Test Score Cutoff



(b) Wealth Cutoff



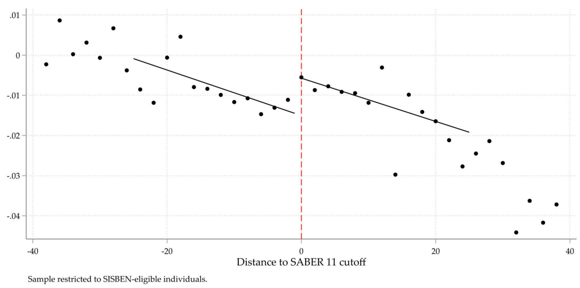
(c) Impacts on Equity



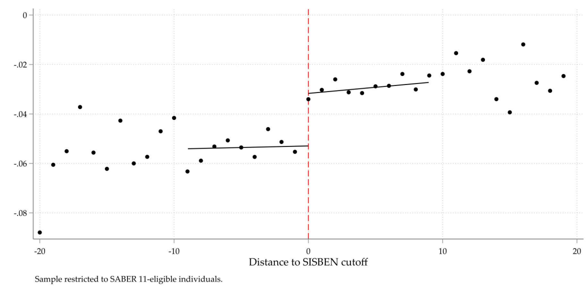
Notes: The figures presented depict the average college-program fixed effect based on Model F for any degree completion (measured by taking the SABER PRO and SABER T&T exams) within seven years after completing high school. Panels A and B plot this outcome based on the proximity to the test score and wealth cutoff, respectively. The results are summarized in Column (2) of Table VI. Panel C compares the series from Panel A (highlighted in red) and a placebo series of SISBEN-eligible students from 2012 and 2013, which predates the expansion of financial aid (represented in black). Additionally, pre- and post-policy outcomes for SISBEN-ineligible students are displayed in gray and blue, respectively.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), SNIES (MEN), SABER T&T (ICFES), and SABER PRO (ICFES).

Figure D.4: SPP's Impact on College-Program Bachelor's Graduation "Value Added"

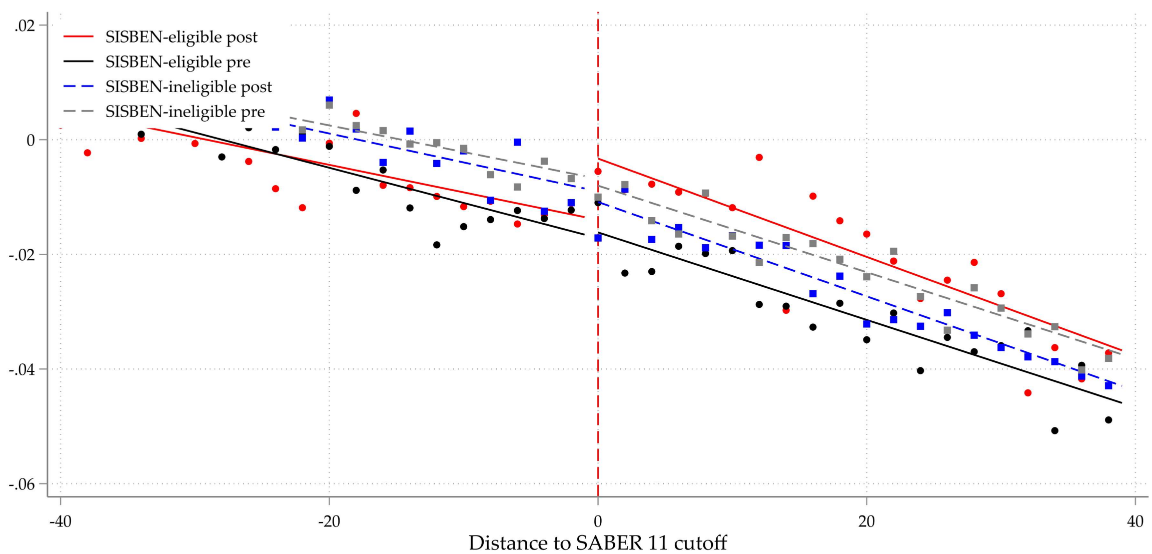
(a) Test Score Cutoff



(b) Wealth Cutoff



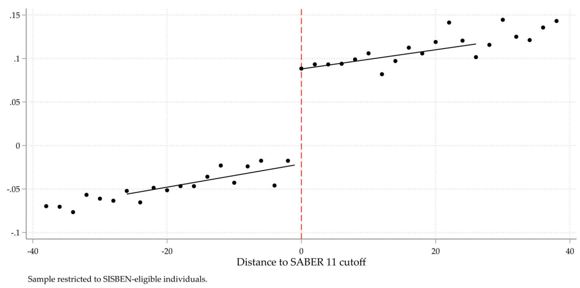
(c) Impacts on Equity



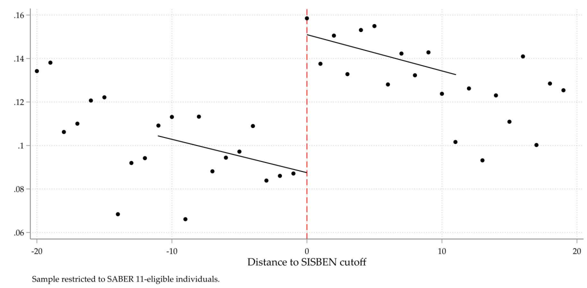
Notes: The figures presented depict the average college-program fixed effect based on Model F for bachelor's degree completion (measured by taking the SABER PRO and SABER T&T exams) within seven years after completing high school. Panels A and B plot this outcome based on the proximity to the test score and wealth cutoff, respectively. The results are summarized in Column (4) of Table VI. Panel C compares the series from Panel A (highlighted in red) and a placebo series of SISBEN-eligible students from 2012 and 2013, which predates the expansion of financial aid (represented in black). Additionally, pre- and post-policy outcomes for SISBEN-ineligible students are displayed in gray and blue, respectively.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), SNIES (MEN), and SABER PRO (ICFES).

Figure D.5: SPP's Impact on College-Program Learning "Value Added"

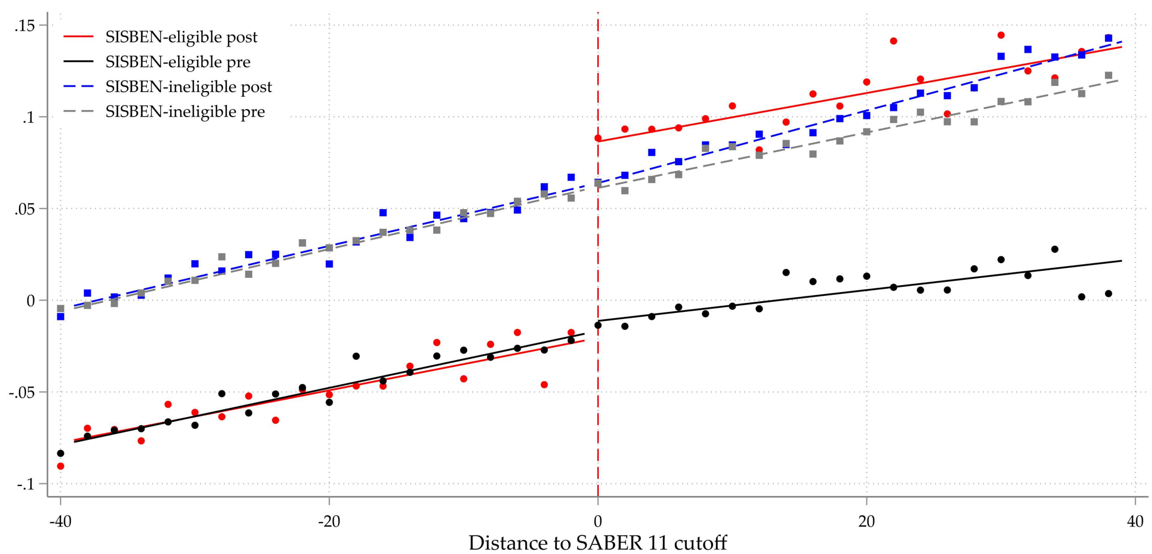
(a) Test Score Cutoff



(b) Wealth Cutoff



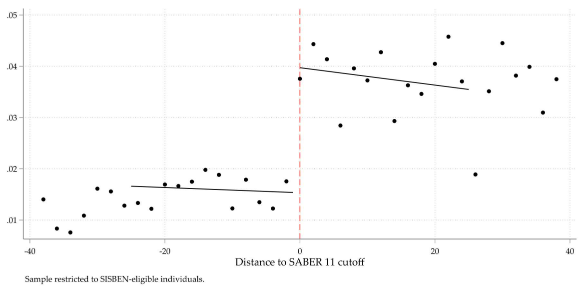
(c) Impacts on Equity



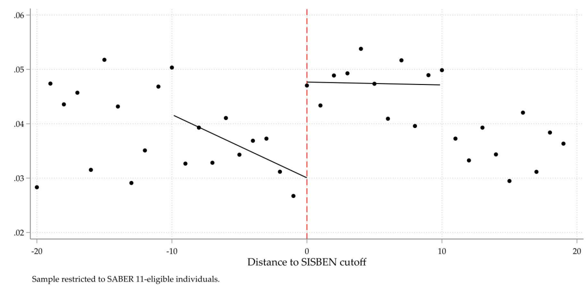
Notes: The figures presented depict the average college-program fixed effect based on Model F for SABER PRO scores within seven years after completing high school. Panels A and B plot this outcome based on the proximity to the test score and wealth cutoff, respectively. The results are summarized in Column (6) of Table VI. Panel C compares the series from Panel A (highlighted in red) and a placebo series of SISBEN-eligible students from 2012 and 2013, which predates the expansion of financial aid (represented in black). Additionally, pre- and post-policy outcomes for SISBEN-ineligible students are displayed in gray and blue, respectively.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), SNIES (MEN), and SABER PRO (ICFES).

Figure D.6: Financial Aid's Impact on College-Program Employment "Value Added"

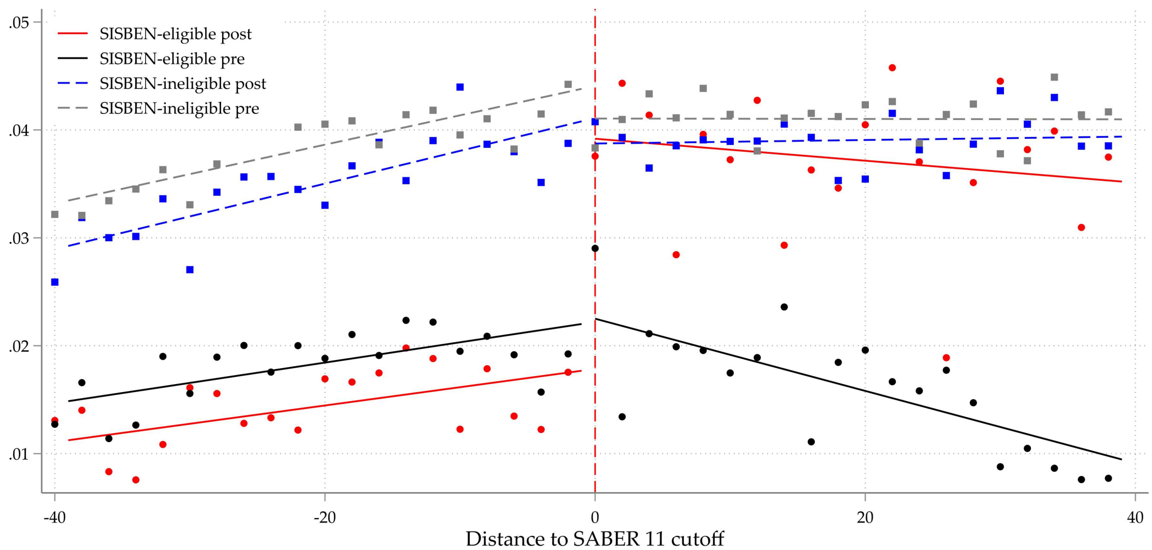
(a) Test Score Cutoff



(b) Wealth Cutoff



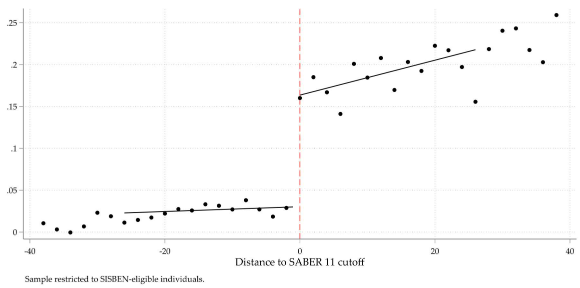
(c) Impacts on Equity



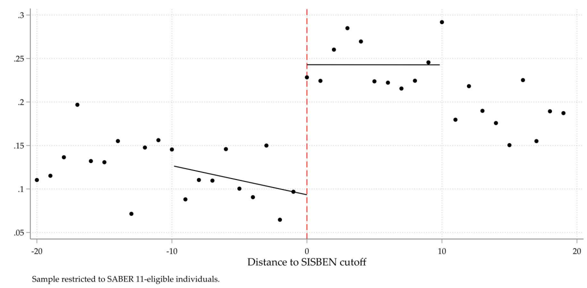
Notes: The figures presented depict the average college-program fixed effect based on Model F for formal employment eight years after completing high school. Panels A and B plot this outcome based on the proximity to the test score and wealth cutoff, respectively. The results are summarized in Column (6) of Table VI. Panel C compares the series from Panel A (highlighted in red) and a placebo series of SISBEN-eligible students from 2012 and 2013, which predates the expansion of financial aid (represented in black). Additionally, pre- and post-policy outcomes for SISBEN-ineligible students are displayed in gray and blue, respectively.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), SNIES (MEN), and PILA (MinSalud).

Figure D.7: Financial Aid's Impact on College-Program Earnings "Value Added"

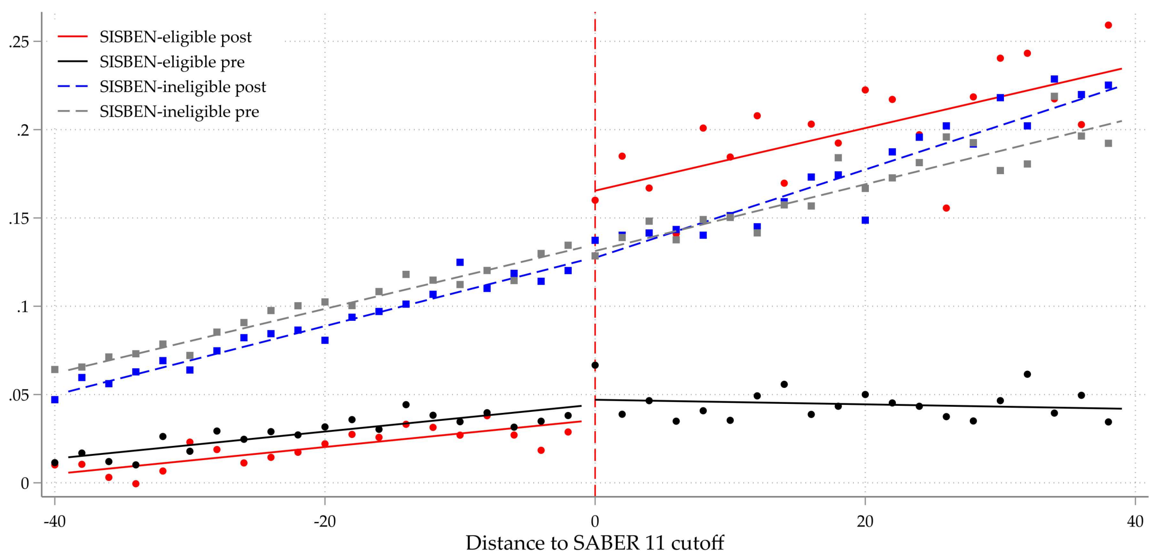
(a) Test Score Cutoff



(b) Wealth Cutoff



(c) Impacts on Equity



Notes: The figures presented depict the average college-program fixed effect based on Model F for formal monthly earnings eight years after completing high school. Panels A and B plot this outcome based on the proximity to the test score and wealth cutoff, respectively. The results are summarized in Column (10) of Table VI. Panel C compares the series from Panel A (highlighted in red) and a placebo series of SISBEN-eligible students from 2012 and 2013, which predates the expansion of financial aid (represented in black). Additionally, pre- and post-policy outcomes for SISBEN-ineligible students are displayed in gray and blue, respectively.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), SNIES (MEN), and PILA (MinSalud).

Table D.1: Average College-Program Fixed Effects by College Type

	HQ		LQ	
	Private	Public	Private	Public
	(1)	(2)	(3)	(4)
Any degree attainment				
No controls	0.110	-0.027	-0.029	0.008
A	0.000	-0.086	-0.029	0.043
B	-0.004	-0.087	-0.030	0.045
C	-0.020	-0.084	-0.030	0.047
D	-0.025	-0.088	-0.030	0.049
E	-0.065	-0.086	-0.036	0.061
F	-0.056	-0.087	-0.033	0.058
Bachelor's degree attainment				
No controls	0.158	-0.014	-0.002	-0.067
A	0.044	-0.093	0.031	-0.030
B	0.038	-0.093	0.031	-0.027
C	0.026	-0.089	0.031	-0.024
D	0.022	-0.093	0.033	-0.023
E	-0.003	-0.087	0.032	-0.011
F	-0.001	-0.089	0.032	-0.012
SABER PRO score				
No controls	0.441	0.393	-0.270	-0.226
A	0.139	0.079	-0.048	-0.080
B	0.139	0.079	-0.048	-0.080
C	0.142	0.077	-0.049	-0.082
D	0.148	0.084	-0.053	-0.085
E	0.176	0.071	-0.057	-0.103
F	0.180	0.070	-0.056	-0.108
Employment				
No controls	0.175	0.016	0.107	0.059
A	0.096	-0.039	0.088	0.072
B	0.094	-0.040	0.086	0.070
C	0.077	-0.045	0.083	0.066
D	0.081	-0.042	0.084	0.066
E	0.046	-0.050	0.068	0.064
F	0.074	-0.044	0.081	0.063
Earnings				
No controls	0.669	0.149	0.217	0.078
A	0.409	-0.046	0.180	0.102
B	0.406	-0.049	0.176	0.099
C	0.379	-0.056	0.171	0.094
D	0.389	-0.047	0.173	0.095
E	0.311	-0.064	0.139	0.092
F	0.358	-0.057	0.159	0.088

Notes: This table presents the average college-by-program fixed effects by college type for different educational and labor market outcomes estimated using Specification (3).
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), SNIES (MEN), SABER PRO (ICFES), SABER T&T (ICFES), and PILA (MinSalud).

Table D.2: The Impact of Financial Aid on the College-Program "Value Added"

	Running variable					
	Panel A: SABER 11			**Panel B:** SISBEN		
	Coef.	SE	N	Coef.	SE	N
	(1)	(2)	(3)	(4)	(5)	(6)
Any degree attainment	0.032	(0.013)	130,376	0.079	(0.026)	19,471
Attainment VA: No controls	0.064	(0.004)	130,376	0.059	(0.008)	19,471
Attainment VA: A	0.020	(0.004)	130,376	0.030	(0.007)	19,471
Attainment VA: B	0.017	(0.004)	130,376	0.028	(0.007)	19,471
Attainment VA: C	0.009	(0.004)	130,376	0.018	(0.007)	19,471
Attainment VA: D	0.007	(0.004)	130,376	0.017	(0.007)	19,471
Attainment VA: E	-0.015	(0.004)	130,376	-0.002	(0.007)	19,471
Attainment VA: F	-0.010	(0.004)	130,376	0.002	(0.007)	19,471
Bachelor's degree attainment	0.062	(0.016)	68,416	0.082	(0.023)	17,605
Bachelor's VA: No controls	0.078	(0.005)	68,416	0.065	(0.008)	17,605
Bachelor's VA: A	0.036	(0.004)	68,416	0.045	(0.006)	17,605
Bachelor's VA: B	0.032	(0.004)	68,416	0.042	(0.006)	17,605
Bachelor's VA: C	0.026	(0.004)	68,416	0.034	(0.006)	17,605
Bachelor's VA: D	0.025	(0.004)	68,416	0.033	(0.006)	17,605
Bachelor's VA: E	0.008	(0.004)	68,416	0.020	(0.006)	17,605
Bachelor's VA: F	0.009	(0.004)	68,416	0.021	(0.006)	17,605
SABER PRO score	0.054	(0.019)	35,493	0.021	(0.033)	12,488
SABER PRO score VA: No controls	0.200	(0.014)	35,374	0.085	(0.026)	12,466
SABER PRO score VA: A	0.081	(0.006)	35,374	0.042	(0.010)	12,466
SABER PRO score VA: B	0.080	(0.006)	35,374	0.041	(0.010)	12,466
SABER PRO score VA: C	0.081	(0.006)	35,374	0.045	(0.010)	12,466
SABER PRO score VA: D	0.084	(0.006)	35,374	0.046	(0.010)	12,466
SABER PRO score VA: E	0.103	(0.006)	35,374	0.062	(0.010)	12,466
SABER PRO score VA: F	0.108	(0.007)	35,374	0.064	(0.011)	12,466
Employment	0.044	(0.013)	284,782	-0.004	(0.025)	21,219
Employment VA: No controls	0.065	(0.004)	284,782	0.044	(0.007)	21,219
Employment VA: A	0.034	(0.003)	284,782	0.026	(0.006)	21,219
Employment VA: B	0.034	(0.003)	284,782	0.026	(0.006)	21,219
Employment VA: C	0.026	(0.003)	284,782	0.019	(0.006)	21,219
Employment VA: D	0.028	(0.003)	284,782	0.020	(0.006)	21,219
Employment VA: E	0.012	(0.003)	284,782	0.007	(0.006)	21,219
Employment VA: F	0.025	(0.003)	284,782	0.017	(0.006)	21,219
Earnings (in min wage)	0.223	(0.036)	284,782	0.226	(0.083)	21,219
Earnings VA: No controls	0.252	(0.013)	284,782	0.227	(0.029)	21,219
Earnings VA: A	0.160	(0.011)	284,782	0.167	(0.025)	21,219
Earnings VA: B	0.160	(0.011)	284,782	0.167	(0.025)	21,219
Earnings VA: C	0.148	(0.011)	284,782	0.156	(0.024)	21,219
Earnings VA: D	0.151	(0.011)	284,782	0.157	(0.025)	21,219
Earnings VA: E	0.116	(0.011)	284,782	0.127	(0.024)	21,219
Earnings VA: F	0.137	(0.011)	284,782	0.146	(0.024)	21,219

Notes: This table displays the reduced-form RD coefficients for different educational and labor market outcomes, alongside the corresponding estimates based on the college-program "value-added."
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), SNIES (MEN), SABER PRO (ICFES), SABER T&T (ICFES), and PILA (MinSalud).