Financial Aid and Social Mobility: Evidence From Colombia's *Ser Pilo Paga*[*]

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Abstract

We study the effects of financial aid on human capital and social mobility. In 2014, Colombia implemented a nationwide financial aid program covering the tuition of four-year undergraduate programs at 33 "high-quality" universities. We estimate effects on educational and labor market outcomes realized seven years after high school completion. We leverage the program's discontinuous assignment rules based on test scores and household poverty using a regression discontinuity design and identify effects away from these discontinuities using difference-in-differences. First, financial aid has a long-lasting expansion of college access and quality, exposing students to colleges with high learning and earnings productivity. Moreover, it boosts social mobility by expanding college attainment, learning, and earnings and slashes the wealth gaps in attainment, learning, and earnings among equally-achieving students. Crucially, these sizable benefits are not offset by corresponding losses for aid-*ineligible* students. As a result, financial aid improves both equity and efficiency. Thanks to financial aid, colleges act as "engines of social mobility" rather than as "bastions of privilege."
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1 Introduction

The high college-earnings premium suggests that college attainment improves financial well-being (Card, 1999; Goldin and Katz, 2008; OECD, 2018; Oreopoulos and Petronijevic, 2013). However, low-socioeconomic status (SES) individuals are less likely to attend college, and those who do are more likely to access low-quality colleges with lower completion rates and returns (Chetty et al., 2020; Ferreyra et al., 2017; Lovenheim and Smith, 2022). As a result, governments seek to promote social mobility by providing extensive financial aid to low-SES students, especially those who excel academically (Hoxby and Avery, 2013).

Notwithstanding, debates abound regarding financial aid's prioritization and efficacy (Dynarski et al., 2022; Nguyen et al., 2019). Indeed, the mobility potential of aid ultimately depends on its ability to improve college attainment and, thereby, earnings. However, the evidence on aid's impacts beyond initial enrollment is scant and mixed. For instance, some researchers find positive effects on degree attainment (Angrist et al., 2021; Bettinger, 2015), others find null effects (Marx and Turner, 2018; Park and Scott-Clayton, 2018; Scott-Clayton, 2011), and yet others find *negative* results if aid diverts students toward lower-quality colleges (Cohodes and Goodman, 2014). Furthermore, only a handful of studies examine impacts on earnings and, again, some researchers find positive effects (Bettinger et al., 2019; Black et al., 2020; Denning et al., 2019) while others obtain null results (Bucarey et al., 2020; Eng and Matsudaira, 2021; Scott-Clayton and Zafar, 2019). These differences across studies may be driven by variations in program characteristics (e.g., eligibility criteria, generosity), whether program beneficiaries displace other students from college, and how severely financial constraints bind. Arguably, financial aid could help low-income high-achievers succeed in contexts with imperfect financial markets and sizable returns to college quality, but there is little evidence from such settings.

This paper studies the effect of financial aid on human capital investments and social mobility. We leverage variation from Colombia, a country that in 2014 transitioned from having very little financial aid to full aid for high-achieving and low-income students thanks to an unprecedented nationwide and large-scale program called "Ser Pilo Paga" (SPP). The SPP loan covered the total tuition cost of attending a four- or five-year undergraduate program in one of Colombia's 33 government-certified "high-quality" universities and provided a modest stipend. In addition, beneficiaries who completed college did not have to pay back the loan and effectively received a grant. The program blended merit- and need-based approaches: eligible applicants scored in the top 9.5% of Colombia's national standardized high school exit exam, SABER 11, *and* belonged to the country's poorest 52.8% of households, according to SISBEN, Colombia's proxy-means testing instrument.[1]

We estimate the effects of financial aid on educational and labor market outcomes realized seven years after high school completion. We use rich administrative data linking the population of high school test-takers to all postsecondary attendees, financial aid beneficiaries, college exit test-takers, and formal workers before and after the SPP program. First, we estimate causal effects using a regression discontinuity (RD) approach that leverages the discontinuous assignment rules based on the standardized high school exit test score and the household wealth index. We compare the effects for the populations affected by the two assignment rules, as they differ considerably in SES, parental educational attainment, and academic achievement. Furthermore, we examine the equity implications of expanding financial aid by comparing outcomes for low- and high-SES students before and after the policy rollout. Lastly, we identify effects away from the program's strict eligibility discontinuities to shed light on the overall effects of expanding financial aid on educational and labor market outcomes. Using difference-in-differences, we compare these outcomes for the entire cohort of students and across the distributions of wealth and test scores before and after Colombia's expansion of financial aid.

There are four key findings. First, financial aid has a substantial and persistent effect on college access, suggesting that imperfect financial markets prevented investments in human capital. Six years after completing high school, merit-eligible students who are barely need-eligible are 4.9 percentage points (p.p.) more likely to have accessed college, a 5.7% gain relative to the control group. The enrollment gain is bigger for lower-SES students who are barely merit-eligible: 9.6 p.p. or 12.4%. Crucially, while the control group accesses low-quality colleges, financial aid permanently expands access to high-quality colleges: the reduced-form coefficient six years after high school is 36.3 p.p. (101%) and 43.5 p.p. (241%), respectively. Furthermore, reducing financial barriers enables students to choose between college and program types, and students disproportionately choose *private* colleges.

[1] SPP annually benefited around 10,000 students or 40,000 individuals between 2014 and 2017. In 2018, a new administration modified SPP and renamed it "Generación E–Equidad." This paper focuses on the first cohort of students who took the high school exit exam in the fall semester of 2014. In a companion paper, Londoño-Vélez et al. (2022) studies how changing the targeting instrument affects the potential equity-efficiency tradeoff when designing financial aid policy.

Second, financial aid bolsters social mobility. The reduced-form estimate on the unconditional probability of earning a postsecondary degree within seven years from high school completion is 6.2 p.p. (10.6%) and 7.7 p.p. (11.6%) at the merit and need cutoffs, respectively. Moreover, the instrumental variables (IV) estimate, which scales these reduced-form effects by the likelihood of receiving financial aid, is 10.6 p.p. and 12.4 p.p., respectively. Indeed, we find no evidence that students motivated to enroll in college due to financial aid persist and graduate at lower rates than students who would have enrolled without aid. On the contrary, financial aid boosts college attainment, largely thanks to its sizable graduation incentives, as dropouts must repay the loan. The IV estimate on the likelihood of earning a bachelor's degree from a high-quality private university is above 59 p.p., a more than 1924% increase for the most vulnerable population of compliers.

Furthermore, financial aid improves students' human capital by gearing students to colleges that teach them more knowledge and skills. As a result, need-eligible individuals who are barely merit-eligible score 7.6% of a standard deviation better in Colombia's nationwide college graduation exam, a 17% increase relative to the control mean. The improved human capital, coupled with students attending colleges that are more productive at getting them high-paying jobs, boosts students' early-career labor market outcomes. For instance, aid increases formal monthly earnings seven years after high school by $55.55 to $59.77, representing about one-third of the control group's mean.

Third, expanding financial aid has important implications for equity. Specifically, financial aid eligibility closes the sizable SES gap in college attainment, learning, and earnings among equally-achieving students caused by imperfect financial markets and a socioeconomically segregated higher education system. Using the terms coined by Pallais and Turner (2006), colleges can act less as "bastions of privilege" and more as "engines of social mobility" thanks to financial aid.

Fourth, while financial aid's benefits could have little impact on overall outcomes if program beneficiaries displace non-beneficiaries from college, we find that the gains are not zero-sum. First, high-quality private colleges expanded capacity and admitted more students to accommodate the increased demand. As a result, high-SES students do not experience declines in overall college attendance or formal earnings. On the contrary, as the best-performing high-SES students attend colleges with similarly high-ability peers, this raises their human capital and, consequently, their earnings. Second, lesser-performing low-SES students are *more* likely to attend college because they fill in the empty seats left by the SPP recipients at low-quality colleges; consequently, their earnings also improve. As a result, the large gains in enrollment, graduation, learning, and earnings experienced by low-SES high-achievers thanks to financial aid are *not* offset by corresponding losses for aid-ineligible students. Instead, financial aid seems to have increased efficiency, improving outcomes for the entire cohort of students.

This paper contributes to the small but growing literature on the effects of financial aid on outcomes beyond enrollment—like degree attainment and earnings—which has found mixed results.[2] First, we study a setting where binding financial constraints prevented college access for low-income individuals. As a result, relaxing financial constraints boosts degree completion by roughly five times the effect sizes found in a recent meta-analysis in the United States (Nguyen et al., 2019). Moreover, the effects are substantial for degrees attained in high-quality private colleges, whose high tuition discouraged access for low-income students.[3] Second, we highlight two essential design features contributing to the policy's substantial mobility effects: (1) requiring beneficiaries to attend high-quality universities exposed students to colleges and programs with high learning and earnings productivity, and (2) forgiving the student loan upon graduation incentivized degree completion.

In addition, we contribute to the extensive literature on the returns of college quality on education and labor market outcomes.[4] Our setting offers three key advantages over previous papers. First, we leverage sizable variation in students' likelihood of attending high-quality colleges thanks to strict discontinuities in people's ability to pay and a nationwide reform that massively expanded access to these colleges. Thus, while previous papers compare the effects of attending a particular college vis-a-vis the next-best alternative, the counterfactual outcome in our setting is attending no college or one substantially lower in quality. Second, we follow students

[2] Angrist et al. (2021); Bettinger et al. (2019); Black et al. (2020); Bucarey et al. (2020); Card and Solis (2022); Castleman and Long (2016); Clotfelter et al. (2018); Cohodes and Goodman (2014); Denning et al. (2019); Fack and Grenet (2015); Scott-Clayton and Zafar (2019); Solis (2017).

[3] These results suggest that straightforward ability to pay accounted for a large part of the sizable role that SES played in college choice in our setting—beyond inability to take advantage of financial aid opportunities (Hoxby and Turner, 2014; Hoxby and Avery, 2013). In addition, imperfect information might have played a role as students learn which colleges are "high-quality" (Dynarski et al., 2021).

[4] Andrews et al. (2016); Anelli (2020); Barrera-Osorio and Bayona-Rodríguez (2019); Black and Smith (2004); Black et al. (forthcoming); Bleemer (2021); Canaan and Mouganie (2018); Dale and Krueger (2014, 2002); Dillon and Smith (2020); Goodman et al. (2017); Hoekstra (2009); Jia and Li (2021); Kane and Rouse (1995); Kane (2003); Lovenheim and Smith (2022); MacLeod et al. (2017); Mountjoy (2022); Mountjoy and Hickman (2021); Saavedra (2009); Sekhri (2020); Zimmerman (2014, 2019).

beyond college, linking national high school exam records, college data, and social security data to consider labor market outcomes. Critically, we link students to any college, major, and formal job in the country.[5] Third, Colombia measures learning at the end of high school *and* college, so we can use comparable measures across all students that are relevant for students, colleges, and employers. We show that higher-quality colleges raise individuals' productivity, proxied by their performance in the national college graduation exam. In addition, this enables us to isolate the earnings effects of college quality coming from human capital.

The remainder of this paper is organized as follows. Section 2 provides some institutional background. Section 3 describes the data and the RD methodology and offers the RD validity checks. Section 4 presents the impacts of financial aid on college access, persistence and attainment, learning performance, and labor market outcomes. Section 5 examines the mechanisms. Section 6 studies the overall effects of financial aid for the entire cohort of students. Finally, Section 7 concludes.

2 Higher Education in Colombia and SPP

There are roughly 300 higher education institutions in Colombia, consisting of professional technical institutions, technological institutions, technological schools, university institutions, and universities. We henceforth refer to all these different types of higher education institutions simply as "colleges." Colleges in Colombia offer two- or three-year "technical and technological" programs and four- or five-year "professional" programs, akin to associate's and bachelor's degrees in the United States.[6]

Programs and colleges vary considerably in selectivity, quality, and tuition fees. Unlike in the United States, undergraduate admissions decisions in Colombia are largely based on applicants' performance in the national standardized high school exit exam, called SABER 11. SABER 11 evaluates individuals' knowledge in areas such as mathematics, physics, chemistry, biology, language, philosophy, social science, and English. This exam is taken by more than 90% of high school seniors regardless of

[5] Thus, unlike US studies based on state-level administrative earnings, out-of-state migration does not bias our estimates (Foote and Stange, 2022).

[6] Universities and university institutions can offer either bachelor's or associate's programs, while the other college types only offer associate's programs. Some researchers use the term "short-cycle programs" to refer to the two- or three-year "technical and technological" programs, following UNESCO's International Standard Classification of Education.

whether they intend on applying to college. Prospective students apply to a college-major pair and admissions are semi-annual because high schools operate on two different academic calendars: nearly 85% of high school students begin classes in the spring term, while 15%—mostly from elite private high schools—begin courses in the fall.

To recruit students, all programs and colleges must obtain the Ministry of Education's *Qualified Registry* of minimum quality standards and renew this status every seven years. Colleges can also voluntarily apply for a certificate of *High-Quality Accreditation* (henceforth HQA), a "peer review" process designed to encourage continuous self-evaluation, self-regulation, and institutional/program improvement (OECD, 2016).[7] HQA proxies quality of education provision, as measured by standardized test scores and graduates' wage profiles (Camacho et al., 2016). All programs offered by colleges with HQA will automatically earn HQA, while colleges without HQA can nevertheless have programs achieving HQA. However, by 2014, only 9% of programs and 12% of colleges achieved HQA (OECD, 2016). Among the 43 colleges with HQA, 33 were universities, and 10 were not universities. Twelve of the 33 universities with HQA were public, and 21 were private. We henceforth refer to the 33 universities with HQA by 2014 as "high-quality" colleges and all other colleges as "low-quality" colleges.

A unique feature of the Colombian higher education system is that, since 2010, students from all undergraduate programs and colleges must take a standardized exam to graduate (Law 1324/2009). This enables judgments about the "value added" by individual colleges in a way attempted by scant other countries (OECD, 2016). Students who have successfully passed 75% of their academic credits for their professional program must take SABER PRO. Students from technical and technological programs are also required to take a standardized exam: before 2016, they take SABER PRO while, after 2016, they take a separate exam, called SABER T&T. SABER PRO and SABER T&T include five generic competency tests—writing, critical reading, quantitative reasoning, English, and citizenship competencies—and test several "common competencies" relevant to the program studied. Students with the highest scores in the program-specific component earn an academic distinction.

Lastly, there is a wide variation in tuition fees across colleges, as plotted in

[7] The HQA status is awarded by the National Accreditation Council, composed of members from the academic and scientific community, and lasts from three to ten years, after which colleges must apply for re-accreditation.

Figure A.1. *High-quality* private colleges are twice as expensive as *low-quality* private colleges and charge expensive tuition fees even by international standards (OECD and The World Bank, 2012). Moreover, the tuition fees for high-quality *private* colleges are over tenfold those of their *public* equivalents because the latter offer free or heavily discounted tuition fees thanks to substantial subsidies from the central government. Furthermore, *high-quality* public colleges are more generously funded and charge even lower tuition fees than *low-quality* public colleges.

Despite progress in the past decades, student loans and grant aid remain substantially less developed in Colombia than in other OECD countries, and very few private colleges offer financial assistance. The high college costs and scant financial aid available squeeze many low-income Colombian students out of collegiate opportunities. Access depends on students' financial resources, and, for those who attend college, sorting into private and selective colleges is strongly defined by the tuition rates they charge (Riehl et al., 2018). While a minority of extremely high-performing students access the highly competitive and near-free high-quality public colleges, most low-income students attend the cheaper low-quality colleges (Ferreyra et al., 2017). As students sort across college types based on their ability to pay, higher education in Colombia becomes severely socioeconomically segregated.

The SPP Financial Aid Program

On October 1, 2014, the Colombian central government announced *Ser Pilo Paga* (roughly, "hard work pays off" in Spanish), a merit-based financial aid program for low-income students. SPP is a comprehensive and publicly funded student loan program that covers the total tuition cost of attending any four- or five-year degree-awarding undergraduate program at any one of Colombia's 33 universities with HQA. The central government transfers the cost of each beneficiary to the university. In addition, beneficiaries receive a stipend worth one monthly minimum wage every six months, but this amount can increase to four minimum wages if the student moves to a different metropolitan area to attend college. Moreover, SPP includes an incentive component because the loan is forgiven if the student graduates.

SPP blends merit- and need-based approaches. Applicants must satisfy three conditions to become eligible. First, they must score above a cutoff in SABER 11 in the fall term of the year they graduate high school. For the first cohort of SPP recipients, this meant scoring at least 310 out of 500, i.e., among the top 9.5% of test scores in 2014

(Figure I, Panel A).[8] Second, applicants must come from a disadvantaged household, measured by the government's main proxy-means testing instrument to target social welfare program recipients, SISBEN. The student's SISBEN wealth index must be below a cutoff that varies with geographic location: 57.21 in the 14 main metropolitan areas; 56.32 in other urban areas; and 40.75 in rural areas (Figure I, Panel B).[9] Around 52.8% of test-takers are need-eligible based on their SISBEN index, meaning they both have a SISBEN score and their score is below the relevant cutoff. Therefore, SPP is more restrictive in terms of merit than need. Third, applicants must receive admission from a high-quality university.

Crucially, SPP was announced by surprise nearly two months *after* students had taken SABER 11. Individuals were informed that they could be eligible for SPP based on their test scores when receiving their exam results some weeks before most colleges' application deadlines. For this reason, students could not manipulate their test scores or wealth index to become eligible for SPP, lending credence to our identifying assumption of quasi-random assignment for students close to the eligibility cutoffs, which we validate in Section 3.1. Between 2014 and 2018, SPP benefited some 40,000 students or roughly 10,000 individuals annually. In addition to its large scale, a massive government advertisement campaign made SPP one of Colombia's most popular social programs.

3 Data and Methodology

We use administrative data from six main sources:

1. The population of high school 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,

[8] Due to rising program take-up and binding budget constraints, the government subsequently increased this test score cutoff to 318 in 2015 (i.e., top 8%), 342 in 2016 (i.e., top 4%), and 348 in 2017 (i.e., top 3%). Londoño-Vélez et al. (2022) investigates the equity and efficiency effects of making financial aid more merit-selective.

[9] SISBEN uses data from a proxy-means survey to assign households a single and continuous score from 0 to 100 (poorest to richest) based on housing quality, possession of durables, public utility services, and human capital endowments, among others. SPP's SISBEN cutoffs coincide with eligibility cutoffs of other social programs, such as the conditional cash transfer program "Familias en Acción" and humanitarian aid for victims of Colombia's armed conflict.

sex) for all SABER 11 test-takers of the fall semesters of 2012, 2013, and 2014, that is, before and after the expansion of financial aid.

2. The universe of households from the Department of National Planning's *Sistema de Identificación de Potenciales Beneficiarios de Programas Sociales* (SISBEN). SISBEN uses data from a proxy-means survey to assign households a single and continuous score from 0 to 100 (poorest to richest). The SISBEN database is available from 2012 to 2014.

Together, these two sources allow the identification of the eligible population, i.e., students who are merit- and need-eligible. In addition, the following source enables us to identify program beneficiaries:

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 and their loan repayment behavior.

Lastly, the following three sources enable us to measure key outcomes of interest:

4. The population of postsecondary attendees from the Ministry of Education's *Sistema Nacional de Información de la Educación Superior* (SNIES) tracks students in the postsecondary education system. We have SNIES microdata from 2013 to 2020, which provide a wealth of information at the student-by-semester level on enrollment status, institution and type of program attended (e.g., undergraduate major, graduate study), academic performance (credits and courses passed), persistence, and degree completion.

5. The population of college exit test-takers from ICFES. This source includes information for SABER PRO and SABER T&T, the mandatory exams for graduation from professional, technical, and technological programs. Information from SABER PRO is available from 2013 to 2021, while SABER T&T is available from 2016 to 2021, after students from technical and technological programs have had a separate examination. Since 2016, SABER PRO is offered annually while SABER T&T is offered each semester. Both exams include five generic competency tests—writing, critical reading, quantitative reasoning, English, and citizenship competencies—and field-specific components relevant

to the major studied (e.g., economics, biology). We focus on the five generic competency tests. For each student, we added the scores obtained in the five generic modules, and then we standardized the sum to a zero mean and standard deviation of one for the students taking the test in 2016. The test scores between 2013 and 2021 are comparable over time.

6. The population of formal workers using social security records from Colombia's Ministry of Health and Social Protection's *Planilla Integrada de Liquidación de Aportes* (PILA). This dataset represents the census of all individual-by-month contributions to healthcare, pension funds, and workers' compensations. The information is available at the individual-by-month level for April, August, and December 2013 through 2021. It includes information on payroll, earnings, days worked, and employer characteristics (e.g., firm size, sector, location) for all formal workers in Colombia. Notably, we do not observe earnings for non-employed or informal workers.

A total of 574,259 individuals took the SABER 11 exam in the second semester of 2014. Around 11,000 of these individuals (2% of test-takers) had attended some college before (re-)taking SABER 11 that semester, so we drop these individuals from the RD sample. Our main analysis is based on the remaining 563,027 individuals. Of these individuals, 297,279 (52.8%) are need-eligible based on SISBEN, while 53,636 (9.5%) are merit-eligible based on SABER 11.

3.1 RD Design and Validity

To estimate the causal effect of financial aid on our outcomes of interest, we exploit the SABER 11 and SISBEN cutoffs using an RD design. Let $Z_i = 1(R_i > k)$ be an indicator for SPP eligibility, where k is the point of a discontinuous assignment rule (SABER 11 score, SISBEN). In addition to providing the government proof of satisfying these need- and merit-based conditions, applicants must also show they have been granted admission at a high-quality university to receive SPP. However, expecting financial aid might cause students to apply (and earn admission) to a high-quality university. Therefore, we will define eligibility based only on test scores and households' poverty index.

Denote D_i as an indicator for whether an individual is a beneficiary of SPP, which depends on the SISBEN wealth index and the SABER 11 test score. This multidimensional RD setting can separately identify two types of compliers: (1) need-eligible students around the test score cutoff and (2) merit-eligible students around the need cutoff (Figure A.2 offers an illustration). However, we report estimates separately, collapsing the discontinuity into a single dimension for each student by defining the distance of SABER 11 (SISBEN) scores from the eligibility cutoff, given SISBEN- (SABER 11-) eligibility status. We prefer this univariate approach over the weighted average of the two RD effects: the two discontinuities capture different student populations who might be differentially affected by financial aid.

Specifically, the RD using the *test score* as the running variable identifies the effects by comparing students scoring near the 91st percentile of the test score distribution, who, on average, are very poor; the control group is in the 31st percentile of the wealth distribution (Table A.1). By contrast, the RD using the *wealth index* as the running variable identifies effects comparing more well-off students—near the 53rd percentile of the wealth distribution—who score even better in the exam; the control group scores above the 95th percentile (Table A.1).[10] Since the latter population has *both* higher SES *and* scholastic achievement, it may be less financially constrained and more likely to attend college. As a result, we might expect larger effects for the former population than for the latter.

We base our primary RD analysis on students who took the high school exit exam in the fall semester of 2014. The first cohort of SPP guarantees the highest internal validity because students were informed about the financial aid program *after* they had taken the SABER 11 exam in 2014, alleviating concerns about non-random sorting across the test score cutoff. In addition, students could not retake SABER 11 to become eligible. By contrast, students in the following years react to the program by exerting more effort in standardized exams, as Laajaj et al. (2022) and Bernal and Penney (2019) show. Younger cohorts may also request an evaluation from local authorities for inclusion in SISBEN, which would induce non-random sorting across the need criterion.

Supporting our identifying assumption, the histograms in Figure I suggest there is no manipulation of SABER 11 or SISBEN for fall 2014 test-takers. Notwithstanding, we formally test for manipulation of the running variable using the local polynomial density estimator proposed by Cattaneo et al. (2020, 2016). The resulting robust-

[10] For instance, Table A.1 shows that merit-eligible students near the need threshold have smaller families, more educated parents, and a higher SES than need-eligible students near the merit threshold. They are also more likely to attend full-day schooling at private high schools and live in urban areas.

corrected p-values are 0.823 with SABER 11 as R_i, and 0.413 with SISBEN as R_i (Figure A.3), confirming there is no statistical evidence of systematic manipulation of the running variable. In addition, Table A.1 shows that the observable covariates are balanced around the two discontinuities. Using SABER 11 as the running variable, we cannot reject the null of no statistical difference for all but three of the 40 baseline characteristics. Using SISBEN as the running variable, we cannot reject the null for 28 of the 40 baseline characteristics.[11] Moreover, we cannot reject the joint null hypothesis of balance in covariates around the two discontinuities.

Figure II plots the likelihood of receiving SPP against the running variable R_i, the SABER 11 score for those eligible by SISBEN (Panel A), and the SISBEN score for those eligible by SABER 11 (Panel B). Three striking results emerge from this figure. First, the sharp eligibility rules made only a handful of people below the cutoffs receive SPP. Second, the program take-up was high: SPP receipt is 58.3% at the merit cutoff and 64.5% at the need cutoff. The higher take-up rate at the need cutoff is consistent with this complier population—characterized by higher SES and even higher test scores—being more likely to apply and receive admission from a high-quality university. Third, there is incomplete take-up because students may not have applied to or been admitted by a high-quality university that semester or may not have applied to the SPP program.[12]

Lastly, to examine the equity implications of expanding financial aid, we compare the outcomes for 2014 test-takers against those of students taking this exam in the fall semesters of 2012 and 2013. The 2012 and 2013 cohorts serve as a placebo check since the SPP program did not exist before 2014. Indeed, as the results below will show, the outcomes of interest are continuous around the threshold for these cohorts of students.

4 The Effects of Financial Aid On Social Mobility

This section presents the effects of financial aid on enrollment (Section 4.1), persistence and degree attainment (Section 4.2), learning performance (Section 4.3), and early-career labor market outcomes (Section 4.4). Later, we show heterogeneity

[11] Table A.1 shows less balance using SISBEN than SABER 11 as the running variable, possibly since, as mentioned in footnote 9, SPP's need cutoffs coincide with those used by other social programs in Colombia like "Familias en Acción."

[12] 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.

in the effects of financial aid (Section 4.5).

4.1 Enrollment Within Six Years from High School Completion

We first estimate the effect of financial aid on the likelihood of ever attending college within zero to six years from high school completion. First, Figure III compares need-eligible students above and below the merit cutoff. Panel A shows that financial aid eligibility dramatically expands immediate postsecondary enrollment by 28.7 p.p., from 41.4% for control students to 70.1%, representing a 69.5% increase.[13] However, over time, more students eventually attend (at least some) college: Panel B shows that the likelihood of ever attending college among barely-eligible students has increased by 16.8 p.p. within zero to six years from high school (from 70.1% to 86.9%). This increase is even larger for barely-ineligible students, whose access has expanded by 36 p.p. (from 41.4% to 77.3%). Consequently, Panel C shows that the enrollment effect falls over time as aid-ineligible students "catch up," from 28.7 p.p. (69.5%) to 18.2 p.p. (29.9%) one year later and 13.0 p.p. (18.3%) two years later, stabilizing at around ten p.p. (14.0%) three years after high school completion. Notwithstanding, the effect is sizable even six years after high school completion: the reduced-form RD coefficient is 9.6 p.p. and highly statistically significant (see Table I).

These patterns are similar when comparing the merit-eligible students above and below the need cutoff (Figure A.4): the reduced-form RD coefficient falls from 22.6 p.p. (42.3%) immediately after high school to 12.2 p.p. (16.7%) one year larger, stabilizing at roughly 5.7 p.p. (5.8%) after three years from high school completion. Relative to students near the merit cutoff, students near the need cutoff have higher SES and test scores. As a result, they may be less financially constrained and more likely to attend college. Indeed, control students below the need cutoff are more likely to attend some college within six years from high school (85.1%) compared to control students below the merit cutoff (77.3%). Correspondingly, the reduced-form RD coefficient is smaller in both percentage points and percentage terms, though it remains sizable and highly significant.

The remainder of this section decomposes the enrollment effects of financial aid by the type of college and program attended. First, Figure IV compares outcomes for

[13] Using data from SPADIES to trace students along the postsecondary pipeline, Londoño-Vélez et al. (2020) estimate a 32 p.p. increase in immediate enrollment. By contrast, we estimate a 28.7 p.p. increase because control students are more likely to attend SENA, Colombia's largest college for vocational training, which is included in our SNIES data but excluded in SPADIES.

need-eligible students around the merit cutoff (Figure A.5 reports the effects for merit-eligible students near the need cutoff). Panel A plots the reduced-form RD coefficient and 95% confidence intervals on the likelihood of accessing a "high-quality" versus a "low-quality" college within zero to six years of high school completion.[14] Financial aid substantially improves access to high-quality colleges: aid-eligible students are 46.8 p.p. more likely to attend a high-quality college immediately after high school, a 489% increase compared to 9.6% among the control group. Moreover, this improvement is long-lasting: the reduced-form RD coefficient is 43.5 p.p. (241%) six years after high school. In addition, financial aid persistently pushes students out of low-quality colleges. The RD coefficient nearly doubles over time, from -18.0 p.p. to -33.6 p.p. within six years from high school; control students are increasingly likely to attend low-quality colleges, while aid-eligible students are not. Therefore, financial aid has a lasting effect on shifting students out of no-college and low-quality education and into high-quality education, consistent with imperfect financial markets preventing investments in human capital.

Private colleges drive the improvement in college quality. The reduced-form RD coefficient on attending a high-quality *private* college within six years from high school is 47.2 p.p. at the merit cutoff and 47.4 p.p. at the need cutoff (Figure A.6 and Table I). Crucially, while this effect is nearly identical across the two populations of compliers, it is driven by different combinations of shifts out of no college, low-quality colleges, and high-quality *public* colleges. For compliers near the merit cutoff (who, as described above, are in the poorest tertile of the wealth distribution), there is a larger shift out of no college and low-quality colleges. By contrast, for compliers near the need cutoff (who, as described above, have higher test scores and SES), there is a greater shift out of high-quality *public* colleges: the coefficient is -11.5 p.p. compared to -3.9 p.p. Indeed, these extremely high-achieving students are more likely to attend high-quality *public* colleges (the control group's mean is 26.4% compared to 14.0% at the merit cutoff). As a result, financial aid radically changes the type of college students attend and does so differently for the two populations of compliers. As the following sections will show, these changes in college types have important consequences for students' educational and labor market outcomes.

Lastly, Panel B of Figure IV plots the reduced-form RD coefficient by program duration, comparing access to four- or five-year programs and two- or three-year

[14] Throughout this paper, "high-quality" refers to the 33 universities with HQA and "low-quality" refers to all other colleges.

programs. Financial aid has a permanent increase in the duration of the program attended. For need-eligible students near the merit cutoff, the long-term gain in access to four- or five-year programs is 21.2 p.p. (37.8%), while access to two- or three-year programs has dropped by 12.1 p.p. (56.6%). Students at the need cutoff (who have higher test scores and SES) are much more likely to attend four- or five-year programs: the control group's mean is 70.3% compared to 56.2%. Notwithstanding, they too substantially change the type of program attended: the long-term gain in access to four- or five-year programs is 14.6 p.p. (20.8%)

4.2 Persistence and Degree Attainment

We first investigate whether financial aid affects students' college persistence by comparing enrollment in the years after high school completion. For students near the merit cutoff, financial aid eligibility increases college attendance one year after high school by 18.2 p.p. (Figure A.7, Panel A). Moreover, aid also increases college persistence: five years after high school completion, the RD coefficient remains highly significant at 13.5 p.p. or 26.6% relative to the control mean. Since the enrollment effects were half the size for merit-eligible students near the need cutoff, the persistence effect is also roughly half as large for this population: 6.9 p.p. or 11.1% five years after high school (Figure A.7, Panel B). However, six years after high school completion, both RD coefficients become zero and non-significant: many aid-eligible students have graduated college, and aid-ineligible students remain in college, as they have taken longer to enroll.

Next, we focus on college attainment. Figure V examines the effect of financial aid eligibility on the likelihood of earning a bachelor's degree within seven years after high school. Panel A plots this probability as a function of the distance to the merit cutoff for need-eligible students. Test scores strongly predict degree attainment: a need-eligible student scoring 40 points above the merit cutoff (i.e., at the 98th percentile) is 28 p.p. more likely to earn a bachelor's degree than a student scoring just below the cutoff (i.e., at the 90th percentile) and nearly 44 p.p. more likely than a student scoring 40 points below the cutoff (i.e., at the 71st percentile). Moreover, financial aid eligibility raises degree attainment by 15.6 p.p., a 38.8% increase relative to the control mean (Table II).[15] The IV estimate, which scales the reduced-form coefficient by the first stage of 58.3%, shows that financial aid increases bachelor's

[15] As for the other outcomes, this effect is stable across RD bandwidth choices (Figure B.7).

degree attainment by 26.8 p.p. or 66.5% relative to the control mean (Table III). Similarly, for merit-eligible students near the need cutoff, the IV estimate is 22.5 p.p. or 41.2% relative to the control mean (Figure A.8 and Table III).

Panel B of Figure V illustrates the equity implications of expanding financial aid and offers a placebo test. First, the figure compares need-eligible students who took the high school exit exam in the fall semesters of 2012 and 2013, i.e., before the expansion of financial aid (in black). These test-takers are just as likely to earn a bachelor's degree if they score below the merit cutoff as test-takers from 2014 (in red). Moreover, their likelihood of earning a bachelor's degree is constant at the threshold: the RD coefficient is close to zero and non-significant (the coefficient is 0.2 p.p. and the p-value is 0.841), further confirming that the increased degree attainment is caused by financial aid. Second, we compare the series against need-*ineligible* students before and after the expansion of financial aid (in gray and blue, respectively). These "high-SES" students do not qualify for SPP because (1) they do not have a SISBEN score or (2) their SISBEN score is above SPP's cutoff. Consistent with their higher SES, need-*ineligible* students are around ten p.p. more likely to earn a bachelor's degree than need-eligible students before the rollout of aid. This attainment gap persists throughout the test score distribution. In contrast, the expansion of financial aid makes low-SES students significantly more likely to earn a bachelor's degree than high-SES students—a dramatic improvement in equity.

More than half of the gain in earning a bachelor's degree is in the STEM fields (i.e., engineering, biological and biomedical sciences, mathematics and statistics, physical sciences, and medicine). While the overall reduced-form estimate at the merit cutoff is 15.6 p.p. (38.8%), the coefficient is 8.6 p.p. (62.1%) for STEM fields and 12.3 p.p. (41.0%) when adding other majors like Architecture, Business, Economics, and Psychology (dubbed "STEM-Plus" in Table II). The effect is 3.2 p.p. (62.1%) for social sciences and humanities, and 1.6 p.p. (352.0%) for the arts (Table II).[16] Similarly, nearly 70% of the attaingment gain is in the STEM-Plus fields at the need cutoff.

Since financial aid expanded access to *high-quality* colleges, Figure VI compares the likelihood of earning a bachelor's degree from a high- versus low-quality college. Panel A shows that degree attainment from a high-quality college increases by 32.3 p.p., a 333% gain since only 9.7% of control students graduates from these selective

[16] Some low-quality colleges do not report students' field of study. Since financial aid shifts people away from low-quality colleges, Column (14) of Table II shows a reduction in the likelihood of earning a bachelor's degree with a missing field of study.

universities. The IV estimate is 55.4 p.p. or 570%—a dramatic improvement in human capital accumulation. Moreover, this effect persists throughout the test score distribution: aid drastically improves college quality even for students scoring among the top 2% of test scores. As a result, low-SES students are significantly more likely to graduate from a high-quality college than high-SES students after the expansion of financial aid. In addition, since aid shifts students away from low-quality colleges, Panel B shows that the likelihood of earning a bachelor's degree from a low-quality college drops by 16.1 p.p. or 53.1%.

Because financial aid shifts students away from two- or three-year programs, graduation from these shorter programs drops by -10.1 p.p. (-54.8%) at the merit cutoff and by -6.0 p.p. (-54.1%) at the need cutoff. Notwithstanding, the likelihood of earning *any* degree—either a two- or three-year degree or a four- or five-year degree—*increases* by 6.2 p.p. (10.6%) at the merit cutoff and by 7.7 p.p. (11.6%) at the need cutoff (Table II). Notably, students at the need cutoff experience a larger graduation effect than students at the merit cutoff, despite being less induced to access college. As Section 5 will show, changing the type of college attended drastically affected the graduation rate for this population of compliers.

Figure VII decomposes the effect on *any* degree attainment by college type and shows that high-quality *private* colleges drive the gain in overall college attainment. Indeed, the likelihood of earning a degree from a high-quality *private* college increases by 35.0 p.p. at the merit cutoff, a more than tenfold increase relative to a control mean of 3.4%. The boost is similarly sizable at the need cutoff: 38.7 p.p. or 527% relative to a mean of 7.3% for the control group.

Table IV reports the effects of financial aid on other educational outcomes. For example, Column (1) shows that, by expanding college access and persistence and program duration, financial aid eligibility raised the total number of years students attended an undergraduate program by 0.51 to 0.76 years, or 13.2% to 22.8%, depending on the complier population. This gain exists despite aid eligibility reducing students' time to graduation by 0.13 to 0.19 years (2.4% to 3.6%), depending on the complier population, as Columns (2) through (6) show. The reduced time to graduation is partly explained by students' flight to high-quality *private* colleges, which have shorter bachelor's degrees; their median program lasts nine semesters compared to ten for high-quality *public* colleges.

Lastly, Column (7) shows that financial aid eligibility also improves the likelihood of continuing into graduate studies. While graduate education is rare in our data—

only 0.8% of control students at the merit cutoff attend graduate studies within six years from high school completion—aid increases it by 0.5 p.p. or 62% relative to the control mean. Expectedly, this point estimate is three times larger for merit-eligible students at the need cutoff, who have higher test scores and SES: 1.6 p.p. or 106%.

4.3 Learning Performance

The previous sections showed that financial aid expands students' access and degree attainment from high-quality colleges. This section shows that this, in turn, raises students' accumulation of human capital, proxied by their learning performance in Colombia's mandatory college exit exam. This exam includes five generic competency tests—writing, critical reading, quantitative reasoning, English, and citizenship competencies—and field-specific components relevant to the major studied (e.g., economics, biology). We focus on the five generic competency tests, which are roughly analogous to the SABER 11 exam.

We first present the effects of financial aid on college exit test scores for bachelor's programs within five years from high school completion. Since financial aid beneficiaries attend college immediately after high school, most take this exam within five years. Figure VIII plots this outcome as a function of the distance to the high school test score cutoff for need-eligible students. Panel A shows that *high school* scores correlate strongly with *college* scores. For example, students at the 90th percentile of the high school test score distribution perform 65% of a standard deviation (σ) higher than students at the 71st percentile. Moreover, financial aid eligibility increases learning performance: the reduced-form RD coefficient is 0.096σ or 22.7% relative to the control mean (Table IV).[17] The IV estimate, which scales the reduced-form effect by the first stage, suggests that financial aid increases students' learning performance by 0.119σ or 28.2% (Table III).

Panel B offers a placebo test and illustrates the equity implications of expanding financial aid on students' learning performance by comparing test scores for need-eligible and need-ineligible students before and after SPP. Strikingly, an SES learning gap emerges in college: despite performing equally well in high school, high-SES students (in gray) perform better in college than low-SES students (in black) before SPP. For instance, Panel A shows that need-ineligible students barely below the merit cutoff perform at least 0.05σ better than low-SES students. Moreover, this gap persists

[17] This effect is stable across RD bandwidth choices (Figure B.12).

throughout the test score distribution and widens for the top 5% of high school test scores. As we will show below, the SES gap in college quality fully explains the learning performance gap. Notwithstanding, the expansion of financial aid improves low-SES students' test scores (in red), eliminating the learning performance gap between low- and high-SES students (in blue).

Since the results from above showed that control students take longer to access college, they will also take longer to take the college exit exam. For this reason, Figure A.10 considers all exams taken within seven years from high school completion. The reduced form estimate is 0.056σ, and the IV estimate is 0.076σ (the first stage is now 73.2% instead of 80.4%). Critically, the effect remains highly significant and economically meaningful: the IV estimate implies a 17.0% boost in learning performance relative to the control mean.

As expected, merit-eligible students near the need cutoff perform twice as well in the college exit exam: the control group for the latter scores 0.843σ above the mean, or almost twice the scores for need-eligible students near the merit cutoff (Table III). In spite of their very high performance, financial aid seems to positively affect their exam performance: the IV coefficients for exams taken within five and seven years are 0.059σ and 0.044σ, respectively. Moreover, Panel B of Figures A.11 and A.12 clearly shows this positive effect when comparing their scores relative to similar students in previous cohorts. Unfortunately, however, the RD coefficients are imprecisely estimated and we cannot reject the null of no effect.

Lastly, note that Colombia's college exit exam is taken only by students who have successfully completed three-quarters of their bachelor's degree. Since we do not observe college exit test scores for students who drop out or never attend a bachelor's program, this implies that our measure of learning performance, based on college exit scores, is available only for students about to earn a bachelor's degree. Moreover, the control group is positively selected: these students are close to earning a bachelor's degree despite being ineligible for aid. As a result, the RD estimate represents a lower bound effect on student learning performance.

4.4 Early-Career Labor Market Outcomes

This section estimates the effects of financial aid on early-career labor market outcomes. We focus on formal employment and earnings seven years after high school completion.

Panel A of Figure IX plots formal employment seven years after high school completion as a function of the distance to the merit cutoff for need-eligible students. Nearly 51% of control students have a formal job seven years after high school. Moreover, aid eligibility increases formal employment by 3.9 p.p, which is a 7.7% increase relative to the control mean (Table V). The IV estimate is 6.7 p.p. or 13.2% (Table III). Panel B compares the reduced-form effects on formal employment from zero to seven years after high school completion. Aid eligibility reduces formal employment one to four years after high school, consistent with aid recipients being likelier to attend college not work while in school. However, the trend reverses five years after high school: the RD coefficient jumps close to zero and becomes marginally significant. Further, six years after high school, the effect is zero and non-significant, which is also consistent with aid eligible and ineligible students being equally likely to attend college six years after high school (Figure A.7). By contrast, seven years after high school, aid-eligible students are significantly *more* likely to be formally employed.

Panel A of Figure X plots monthly formal earnings seven years after high school completion as a function of the distance to the merit cutoff for need-eligible students. We express earnings as multiples of the monthly minimum wage and assign zero formal earnings to individuals who are not formally employed. Panel A shows that test scores strongly correlate with formal earnings: a need-eligible student scoring in the top 2% of test scores earns twice as much as a student scoring in the 71st percentile. Moreover, financial aid eligibility raises formal earnings by 14.9 p.p., a 20.3% increase relative to the control mean of 73.4% of a minimum wage. The IV estimate is 25.5 p.p., implying a 34.8% increase. Panel B compares the effects on formal earnings from zero to seven years after high school completion. Again, earnings drop while students are likely to be in college but increase dramatically seven years after high school. Crucially, the earnings boost in the seventh year is significantly larger than the temporary earnings reduction in years one through four. These results are consistent with graduates from high-quality colleges enjoying a significant earnings premium.

Relative to need-eligible students near the merit cutoff, merit-eligible students near the need cutoff—who, again, have higher test scores and SES—are more likely to be formally employed and receive higher earnings.[18] Notwithstanding, the impact of financial aid is very similar across the two populations of compliers: the effect at

[18] For instance, Table V shows that need-eligible students near the merit cutoff earn 73.4% of the minimum wage (or 145% when conditioning the sample to individuals with a formal job). By contrast, merit-eligible students near the need cutoff earn 97.1% of the monthly minimum wage (or 174% conditional on formal employment).

the need cutoff is 4.1 p.p. for formal employment (Figure A.13) and 17.9 p.p. for formal earnings (Figure A.14).[19] Moreover, Table V, reporting aid's effects on other measures of formal earnings, shows that financial aid eligibility increases earnings by 131,329.30 pesos ($32.42) at the merit threshold and 155,972.40 pesos ($38.50) at the need threshold. Furthermore, the effect remains positive and statistically significant when conditioning the sample to individuals who earn positive formal earnings, suggesting the earnings effects are not solely driven by the increased likelihood of being formally employed.

Figure XI offers a placebo test and illustrates the equity implications of expanding financial aid on students' formal earnings by comparing earnings for need-eligible and ineligible students before and after SPP. Despite performing equally well in high school, high-SES students (in gray) earn more than low-SES students (in black) before SPP. Moreover, this gap persists throughout the test score distribution and widens for the top decile of high school test scores. As we will show below, the SES gap in college quality fully explains the earnings gap. Notwithstanding, the expansion of financial aid raises low-SES students' formal earnings (in red), eliminating the earnings gap between low- and high-SES students (in blue).[20]

The last column of Table V reports the effect of financial aid eligibility on the time between college graduation and the first formal job, measured in four-month periods. Need-eligible students just below the merit cutoff take 12.3 months (3.083×4) to be formally employed after graduation, while merit-eligible students below the need cutoff take 11.5 months. Moreover, financial aid seems to slightly shorten this period by 6.1% (-0.188/3.083) for the former group, but this effect is not significant.

Lastly, it is worth discussing how focusing on early-career formal labor market outcomes might affect our results. On the one hand, measuring earnings using social security data in a setting where some individuals work in informal jobs (meaning they do not have the right to a pension when they retire), could potentially bias our results upward. This occurs because informal workers with positive earnings will appear with zero formal earnings in our data, and control individuals could be more likely to work informally. However, informality is likely uncommon in our study population.

[19] Panel B of Figure A.14 shows that merit-eligible students at the need cutoff—with higher test scores and SES—are less likely to work the first four years after high school since they are likelier to attend college. As for the other complier population, the trend reverses after year four, but they graduate college sooner: the RD coefficient is zero and not significant by year five.

[20] The COVID-19 pandemic affects the 2013 cohort's earnings seven years after high school completion, i.e., in 2020. For this reason, Figure A.15 reproduces Figure XI using the 2012 cohort as the comparison group and shows that the pre- and post-reform series fully overlap.

Indeed, data from CEDLAS and The World Bank show that the informality rate is only 7.9% for workers with more than 13 years of education, and SPP's selective merit-based eligibility cutoffs imply that both treated and control groups are very high achieving and attain (at least some) college education: more than 77% of control students at the merit cutoff and over 85% of control students at the need cutoff attain at least 13 years of education (Table I). In addition, the informality rate is decreasing in student test scores. On the other hand, our estimates of early-career earnings effects could be biased downward. First, financial aid increases the likelihood of being enrolled in a graduate program and students in graduate school appear with zero formal earnings in our data. Moreover, given the high return to graduate education, the positive earnings effects of financial aid are likely to *increase* in the following years once these students obtain their graduate degrees. Second, research by MacLeod et al. (2017) shows that the return to high-reputation colleges *increases* with experience, suggesting that reputation matters beyond signaling individual skill. As a result, our RD coefficient is likely to continue increasing over time.

4.5 Heterogeneity

This section briefly discusses the heterogeneous treatment effects of financial aid on students' educational and labor market outcomes. In particular, we compare the reduced-form effects of financial aid on immediate access to a high-quality college, bachelor's degree attainment from a high-quality college, learning performance, and earnings seven years after high school completion by individual, household, and high school characteristics.

First, we investigate the potential equity-efficiency tradeoff when designing aid programs. On the one hand, targeting high-achieving students through merit aid may yield high enrollment and graduation effects, especially if test scores strongly predict admission and persistence in high-quality colleges. However, if test scores correlate strongly with socioeconomic background, then merit-based targeting can come at the cost of reaching the poor (Barrera-Osorio and Filmer, 2016). On the other hand, need-based aid can reach more disadvantaged students and induce more schooling per additional dollar (Colas et al., 2021). However, if socioeconomically underprivileged students can hardly access and succeed in high-quality universities, one might question the usefulness of that approach.

Our setting allows us to address this potential equity-efficiency tradeoff by contrasting the impacts of financial aid across the distributions of merit and need. For students near the need cutoff, we decompose merit-eligible students by above- and below-median SABER 11 scores and compare effects between students scoring in the top 5% of test scores and those scoring in the next 5% (i.e., percentiles 90.5 to 95). For students near the merit cutoff, we decompose need-eligible students by above- versus below-median SISBEN scores and compare effects between Colombia's poorest 26% of households and the next 26% (i.e., percentiles 27–52).[21]

Figure XII plots the main results. First, the effects of financial aid drastically vary across the test score distribution and between educational versus labor market outcomes. On the one hand, the effects on high-quality access and degree attainment *decrease* with test scores: the better-performing students benefit less from financial aid than lesser-performing students within the top decile of test scores, perhaps due to ceiling effects. Notwithstanding, even the top 5% of students benefit significantly from financial aid: the reduced-form RD coefficient is 35.4 p.p. and highly significant. On the other hand, the earnings effect seems to *increase* with test scores. This gain is not due to increased productivity: the better-performing students do not experience a differential learning gain.

In addition, the effects of financial aid vary across the SES distribution. While the gains in access to high-quality colleges are roughly constant across SISBEN scores, a positive SES gradient emerges in degree attainment: higher-SES students are more likely to succeed in high-quality colleges. Similarly, the earnings effect is slightly *increasing* in SES.[22]

Lastly, we compare the effects of financial aid by sex, ethnicity, parental education, rural or urban, and high school characteristics (Figures A.17 through A.20). The gains from financial aid are positive and significant across virtually all baseline characteristics. However, there are three main sources of heterogeneity. First, financial aid has its largest effects on students in the most disadvantaged schools: the gains in college access, graduation, learning, and earnings are larger for students graduating

[21] In addition, we compare effects across household socioeconomic strata as an alternative measure of SES. Colombia's socioeconomic stratification system explicitly stratifies households from 1 to 6 by affluence based on neighborhood and dwelling characteristics (1 being the poorest). SISBEN and strata are highly correlated: 56% of SISBEN-eligible students are in stratum 1, 34% are in stratum 2, 9% are in stratum 3, and 1% are in strata 4, 5, and 6. Moreover, 70% of students from the bottom SISBEN tertile are in stratum 1.

[22] Figure A.16 compares effects across the socioeconomic strata and, again, shows a flat or positive SES gradient in earning a bachelor's degree from a high-quality college and earnings. By contrast, students from stratum 1 (the poorest stratum) experience the biggest learning gains.

from high schools with low baseline test scores and few graduates attending high-quality colleges.[23] Second, females disproportionately benefit from financial aid in accessing and graduating from high-quality colleges. However, females experience similar learning and earnings gains to males since they are less likely to graduate from STEM degrees and more likely to graduate from social sciences and humanities (Figure A.21), which have lower returns. Third, first-generation college students seem to benefit as much from financial aid as students with college-educated parents.[24]

5 Mechanisms

This section estimates the portion of the above-documented educational and labor market gains from financial aid explained by how productive colleges and programs are in terms of graduating their students, teaching them knowledge and skills, and getting them high-paying jobs. We consider each measure of productivity separately as particular college-program combinations might perform better at specific educational and labor market outcomes. For instance, the college-program contributions that add the highest value in terms of degree attainment may not be the ones that add value in terms of general knowledge and skills or in terms of enabling students to get jobs and earn higher salaries.

We leverage that Colombian students apply to specific college-program pairs from the moment they first apply for access to higher education. Programs select students based on SABER 11 test scores and vary significantly in their selectivity. For this reason, we estimate the "value-added" contributions to students' outcomes by granular college-program pairs. Appendix C discusses details of this empirical approach; here, we summarize the key steps. First, to estimate the college and program fixed effects, we use information from students who took the SABER 11 exam in the fall semesters of 2012 and 2013, before the SPP policy rollout. We are interested

[23] The effects are noisy at the need cutoff, since only 2% of merit-eligible students graduate from the worse-performing schools.

[24] We examined two additional sources of heterogeneity: ethnicity and urban versus rural. First, fewer than 5% of students in our study sample self-report to be part of an ethnic group (e.g., indigenous peoples, Afro-Colombians), making the results noisy. Notwithstanding, there is some evidence that the access and attainment effects are lower for this population, but those who persist in college learn significantly more (although we cannot ascertain whether this translates to higher earnings). Second, students from urban areas have larger and more precisely estimated effects, since 75% of need-eligible students and 90% of merit-eligible students are from urban areas. Notwithstanding, both rural and urban individuals benefit from financial aid.

in the following students outcomes realized seven years after high school completion: (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.

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

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

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

Our empirical specification includes relevant student sociodemographic information related to these outcomes of interest and capturing students' selection across colleges and programs. First, we control for a student's SABER 11 score using a third-degree polynomial. Second, we include the leave-one-out average 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, identifying the "added value" contributed by each college-program pair. Third, we use a rich vector of baseline sociodemographic covariates correlated with the outcomes of interest and influencing students' selection across programs.[25] Fourth, since students may select across programs based on their high school peers, we include the leave-one-out mean socioeconomic stratum, parental education, and SABER 11 test scores at the high school-cohort level. Finally, students' outcomes might be affected by the socioeconomic characteristics of their college-program peers. For this reason, we include the leave-one-out mean socioeconomic stratum, parental education, SISBEN score, and SABER 11 test scores at the college-program-cohort level, which controls for differential peer cohort qualities to obtain college contributions purged of peer effects. Thus, our approach estimates colleges' and programs' productivity after

[25]Specifically, we include student demographic information (sex, ethnic minority, third-degree polynomials of age, and a dummy for the SABER 11 semester), 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).

controlling for baseline ability, sociodemographic characteristics, student selection across programs, and peer cohort qualities.

There is wide variation in programs' productivities $\hat{\delta}_{jp}$ across college types, and the distribution and ranking of programs change drastically with the outcome of interest. Appendix C describes the estimated patterns and reports robustness checks; here, we briefly summarize the main findings. First, the most productive low-quality private colleges graduate more students from their programs than all other college types, while high-quality public colleges have the lowest graduation productivity. Second, high-quality colleges give students more knowledge and skills than low-quality colleges, and the top high-quality *private* colleges are more productive in teaching skills than the top high-quality *public* colleges. Third, high-quality *private* colleges are better at getting students higher-paying jobs. By contrast, students from high-quality *public* colleges tend to make less than other students after controlling for selection and cohort effects.

Armed with these estimated college-program fixed effects $\hat{\delta}_{jp}$, we then estimate the effect of financial aid on the productivity of the colleges and programs students attend using the RD specification and the $\hat{\delta}_{jp}$ as the outcome variables. Moreover, we quantify the portion of the graduation, learning, and earnings gains caused by financial aid that is explained by aid causing students to attend more productive programs. We do this by comparing the RD coefficients for each outcome and the estimated college-program's graduation, learning, and earnings productivity.

First, we focus on graduation from any undergraduate program and bachelor's programs specifically. Column (1) of Table VI reports the effect of financial aid eligibility on the likelihood of earning any college degree for students who ever access college (since access is a prerequisite for graduation). For need-eligible students near the merit cutoff, the attainment effect is half the effect size reported in Column (1) of Table II—this implies that one-half of the estimated attainment gain is due to expanding access. The remainder is driven by the program's two crucial design features, namely, (1) the debt forgiveness policy generating strong incentives to graduate (as dropouts must repay the loan) and (2) the program shifted students to colleges and programs with different graduation productivity. To isolate the latter effect, Column (2) plots the RD coefficient on the college-program graduation productivity. Financial aid shifted students to high-quality colleges, which tend to graduate *fewer* students than the low-quality colleges they would have otherwise attended; therefore, colleges' graduation productivity cannot explain the large impact of financial aid on college attainment. Instead, the program's substantial graduation incentives are likely driving the attainment gains.[26] This is especially the case for merit-eligible students near the need cutoff, for whom the program's graduation incentives drive virtually all of their attainment gain, with access and college-program graduation productivity playing a negligible role.

Since financial aid specifically expanded bachelor's degree attainment, Columns (3) and (4) condition the sample to students accessing this type of program. For need-eligible students near the merit cutoff, more than half of the gain in bachelor's degree attainment is thanks to expanded access, a small share is thanks to colleges' graduation productivity, and the remainder owes to the program's graduation incentives. The results are similar for merit-eligible students near the need cutoff. However, graduation productivity plays a larger role for these students because the program was more likely to shift them from high-quality *public* to high-quality *private* colleges, which tend to graduate more students from their programs.

Next, we focus on learning performance for students taking the SABER PRO exams within seven years from completing high school. Columns (5) and (6) of Table VI compare the RD coefficient when the dependent variable is the standardized college exit test score and the estimated learning college-program productivity, respectively.[27] The large, positive, and significant coefficient in Column (6) suggests that the learning gain caused by financial aid is fully explained by shifting students to colleges that teach them more knowledge and skills. Notwithstanding, the actual learning effect is smaller than the effect size predicted by the aid-induced change in college-program productivity because SPP induced less able students to graduate, arguably due to its graduation incentives.

Lastly, we turn to impacts on early-career labor market outcomes. We express the college-program productivity $\hat{\delta}_{jp}$s in Columns (8) and (10) relative to students with no college experience. Columns (7) and (8) of Table VI focus on formal employment seven years after high school, while Columns (9) and (10) focus on formal earnings. The labor market estimates are similar in magnitude to the estimated labor market productivity, and their confidence intervals overlap. Thus, the labor market gains

[26] SPP may have also expanded graduation thanks to its stipend to beneficiaries. Notwithstanding, this stipend—worth US$40 per month for most beneficiaries—is tiny compared to the sizable debt they must repay if they drop out of college.

[27] Compared to Column (9) of Table IV, the point estimate in Column (5) of Table VI is very similar, and the confidence intervals overlap. Indeed, the sample is based on soon-to-be college graduates in both cases.

caused by financial aid are primarily due to aid shifting students to colleges that are more productive at getting students jobs with higher salaries.[28]

6 Welfare

The sizable gains from financial aid in college access, attainment, learning, and earnings might have little impact on overall outcomes if program beneficiaries displace non-beneficiaries from college (i.e., a zero-sum admission game). Therefore, this section examines the welfare effects of financial aid for all high school test takers. Specifically, we calculate the overall access, attainment, learning, employment, and earnings impacts on the entire cohort of students—low- and high-SES—to examine who gains and who loses from financial aid.

Using difference-in-differences, we compare outcomes for individuals who took the SABER 11 exam in the fall semesters of 2012, 2013, and 2014; that is, before and after Colombia's expansion of financial aid. Because the effects of financial aid vary across the test score distribution, we compare students across SABER 11 deciles. Specifically, students in the top decile are merit-eligible for SPP and potentially directly affected by the policy. By contrast, students in the ninth decile are barely ineligible and potentially displaced by SPP recipients, since colleges admit students based on SABER 11 scores. Lastly, students scoring in deciles 1 through 8 are unlikely to be admitted to high-quality colleges before and after SPP and are, therefore, likely unaffected by the policy. The following specification captures this intuition:

$$\begin{aligned} y_{i(d,t)} = \alpha &+ \delta_1 \cdot 1(\text{Decile } 9)_i + \delta_2 \cdot 1(\text{Decile } 10)_i + \lambda_1 \cdot 1(2013)_i + \lambda_2 \cdot 1(2014)_i \\ &+ \gamma_1 \cdot 1(\text{Decile } 9 \times 2013)_i + \gamma_2 \cdot 1(\text{Decile } 10 \times 2013)_i \\ &+ \beta_1 \cdot 1(\text{Decile } 9 \times 2014)_i + \beta_2 \cdot 1(\text{Decile } 10 \times 2014)_i + \theta_m + \epsilon_i \end{aligned} \tag{2}$$

where $y_{i(d,t)}$ is the outcome for individual i taking SABER 11 in year t and scoring in decile d, $1(\text{Decile } 9)_i$ and $1(\text{Decile } 10)_i$ are the SABER 11 decile dummies (with deciles 1 through 8 being the omitted category), $1(2013)_i$ and $1(2014)_i$ are the year dummies (2012 is the omitted category), θ_m are baseline municipality fixed effects, and ϵ_i is

[28]The fact that the estimate in Column (10) is slightly smaller than in Column (9) should be taken with a grain of salt because, as we discuss in Appendix C, programs' estimated earnings productivities are sensitive to controlling for SISBEN. In particular, disregarding SISBEN leads to an effect on programs' earnings productivity that is closer to that in Column (9).

the individual-specific error term. The identifying assumption of this difference-in-difference specification is that the trends between the treated and control groups would be similar in the absence of the policy. We can support this assumption by showing no pre-trends: since SPP should not affect outcomes before its rollout in 2014, γ_1 and γ_2 should be close to zero and not statistically significant. By contrast, the effects of financial aid are captured by the coefficients on the $1(\text{Decile } 9 \times 2014)_i$ and $1(\text{Decile } 10 \times 2014)_i$ interactions terms. Specifically, β_2 captures the direct effect of the policy. By contrast, β_1 captures the "spillover" effect. For instance, if SPP beneficiaries push out students scoring just below SPP's test score cutoff from college, β_1 will be negative and statistically significant.

In addition, the effects of financial aid are likely to vary across the SES distribution because individuals' ability to pay plays a crucial role in college choice. For this reason, we present results separately by SISBEN eligibility. Nearly 53% of students are SISBEN-eligible, and 47% are SISBEN-ineligible, which means they do not qualify for SPP based on their socioeconomic needs because (1) they do not have a SISBEN score or (2) their SISBEN score is above SPP's cutoff. We hereafter refer to SISBEN-eligible and SISBEN-ineligible students as low- and high-SES, respectively.[29]

Table VII presents the results from Specification (2) when the outcome of interest is college access. Each column reports effects separately by college type and years from high school completion. First, Panel A focuses on the impact of financial aid on low-SES students. Reassuringly, $\hat{\gamma}_1$ and $\hat{\gamma}_2$ are close to zero and primarily not statistically significant, consistent with parallel trends before the policy. By contrast, $\hat{\beta}_2$ is large and highly significant, with financial aid causing large enrollment effects on both the extensive and intensive margins. Consistent with the RD results, financial aid permanently expands college access for low-SES high-achievers who would have attended no college. Moreover, it shifts attendance from low- to high-quality colleges (and, to a lesser extent, from high-quality public to private colleges).

By contrast, $\hat{\beta}_1$ captures the spillover effect for lesser-performing low-SES students. The coefficient on immediate access to high-quality colleges is zero and not significant. Indeed, college quality was not immediately affected for these students because they were most likely to attend *low*-quality colleges. Moreover, as financial aid shifted SPP

[29] We present results separately by socioeconomic strata in Tables A.2, A.3, and A.4. Notably, almost three-fourths of SPP beneficiaries are from strata 1 and 2, one-quarter are from stratum 3, and fewer than 3% are from strata 4-6. Therefore, we divide the population of high school test-takers into three groups: low-SES students (strata 1 and 2), medium-SES students (stratum 3), and high-SES students (strata 4, 5, and 6).

recipients out of these institutions, the lesser-performing low-SES students filled the empty seats, and their likelihood of attending low-quality colleges improved. As a result, their overall probability of accessing college *increased*.

Next, Panel B presents the results for high-SES students. Despite their ineligibility for SPP's financial aid, there is no sign that program beneficiaries displaced top-performing high-SES students from high-quality colleges: we can reject a negative effect on college quality with a 95% confidence interval. Indeed, these top-performing high-SES students are unaffected by the increased entry competition because high-quality private colleges expanded their seats to accommodate the heightened demand (Londoño-Vélez et al., 2020). Notwithstanding, *lesser*-performing high-SES students are temporarily displaced by SPP recipients: $\hat{\beta}_1$ is negative and significant for both high-quality college access and any college access. Over time, however, these students eventually access low-quality colleges. As a result, the effect on any college access fades, becoming nearly zero and non-significant six years after high school. Notably, the 0.9 p.p. loss in college quality for these students is minuscule in magnitude, representing only two percent of the quality gain experienced by low-SES high-achievers and half the quality gain experienced by high-SES high-achievers. Moreover, as we will show below, despite being "low-quality," the counterfactual colleges attended by these high-SES students have very similar productivity in terms of graduation, learning, employment, and earnings—a key result that will explain the lack of negative educational and labor market impacts on this population.

Next, Table VIII reports the overall effects of financial aid on degree attainment, proxied by taking Colombia's mandatory college exit exam within seven years after high school. Unfortunately, the cohorts graduating from high school in 2013 and 2014 are less likely to graduate from college by 2020 and 2021, respectively, than the 2012 cohort is to graduate college by 2019 because the COVID-19 pandemic increased college dropouts and delayed degree attainment. This means that, unfortunately, we cannot interpret the impacts on graduation causally due to pre-trends. Notwithstanding, Panel A appears to reiterate the results from the RD analysis: low-SES high-achievers are significantly more likely to earn an undergraduate degree thanks to a gain in bachelor's degree attainment at high-quality private colleges. By contrast, lesser-performing low-SES experience no significant change in their likelihood of earning a bachelor's degree and a slight drop in their chance of an associate's degree. The overall effect on degree attainment is negative, but it should be taken with a grain of salt because of the pre-trend.

Panel B shows the results for high-SES students. Consistent with their improved access to high-quality colleges, the likelihood of graduating from these institutions also improves for high-SES high-achievers. By contrast, since lesser-performing students experienced a slight loss in college quality, their chance of graduating from a high-quality college also drops by 1.2 p.p. The effect on any degree attainment is slightly negative for this group, but, again, it should be taken with a grain of salt because of the pre-trends.

Turning to overall impacts on learning, the last columns of Table VIII show that low-SES high-achievers experience a sizable learning improvement thanks to the gain in college quality, as the previous sections showed. Similarly, lesser-performing low-SES also seem to learn more, although their boost is smaller and less precisely estimated. To understand this result, Table IX presents the overall effect of financial aid on colleges and programs' learning productivity as well as other measures of productivity, as described in Section 5 and Appendix C. The lesser-performing low-SES do not experience a change in access to high-quality colleges nor in the college-program learning productivity, so institutional quality and productivity cannot explain their improved learning. Similarly, their likelihood of earning a bachelor's degree is unaffected, so the learning effect cannot be explained by selection into exam-taking. Instead, the results suggest that the students who filled the empty seats left by SPP recipients at low-quality colleges performed better than the counterfactual students would have—an increase in efficiency.

Panel B reports the learning effects for high-SES students. Lesser-performing high-SES students experience a small drop in learning performance, partly due to a slight loss in college learning productivity (Table IX). Notwithstanding, the coefficient becomes zero and non-significant seven years after high school. By contrast, high-SES high-achievers experience a learning gain, and Table IX shows that this effect is not caused by changes in their college-program learning productivity. Instead, their learning gain may be driven by a positive peer effect from financial aid causing them to attend colleges with higher-achieving peers. However, the coefficient is only positive and significant seven years after high school, so this result should be interpreted cautiously.

Finally, Table X presents the overall effects of financial aid on formal labor market outcomes measured seven years after high school. Panel A shows that financial aid substantially improved formal employment and earnings for low-SES high-achievers, consistent with the results from the RD analysis. Moreover, since lesser-performing low-SES are slightly more likely to access college, their earnings also moderately improve, although this positive result is non-significant when measuring earnings in multiples of the minimum wage or using the natural logarithm. In any case, we can definitively reject that the labor market effect for these lesser-performing low-SES students is negative.

Panel B shows similarly positive and significant labor market effects on high-SES students. Indeed, while lesser-performing high-SES students were slightly displaced them from high-quality colleges, the "low-quality" colleges they attended had very similar employment and earnings productivity (Table IX). This, again, suggests an increase in efficiency. In addition, the highest-performing high-SES students experience particularly sizable and precisely estimated gains in labor market outcomes. Since financial aid did not affect the earning and employment productivity of the colleges and programs they attend either, these high-SES students likely earn more thanks to their learning gain caused by attending programs with higher-ability peers.

Taken together, the results suggest that expanding financial aid had no displacement effect on overall college attendance and, as a result, no negative impact on student learning, employment, and earnings. On the one hand, lesser-performing low-SES students experienced an *increase* in overall college attendance because they filled in the empty seats left by the SPP recipients at low-quality colleges; therefore, their labor market outcomes improved. On the other hand, high-quality private colleges admitted more students to accommodate the increased demand, and the displaced lesser-performing high-SES students eventually accessed colleges ineligible to receive SPP beneficiaries that, nevertheless, had similar learning and earning productivity. As a result, the large gains in enrollment, graduation, learning, and earnings experienced by low-SES high-achievers thanks to financial aid are *not* offset by corresponding losses for students *ineligible* for aid. Instead, financial aid generated an increase in efficiency and net social gains, improving outcomes for the entire cohort of students.

7 Conclusions

This paper studied the effects of financial aid on human capital investments and social mobility. We leveraged rich administrative data from Colombia, where a large-scale program massively expanded financial aid for low-income high achievers. Using an RD approach, we estimated the effects of financial aid on educational and labor market outcomes realized seven years after high school completion. We found that imperfect financial markets prevent investments in human capital, affecting low-SES students' ability to access college—especially *high-quality* colleges. As a result, expanding financial aid raised human capital accumulation by boosting college access and quality. Crucially, there is no indication that these students suffered from attending more selective colleges. On the contrary, they graduate at higher rates and learn more skills. Thanks to more human capital accumulation, financial aid boosted social mobility, raising students' employment and salaries in the formal labor market.

Using difference-in-differences, we showed that these gains are not offset by corresponding losses from students ineligible to receive financial aid. First, high-quality private colleges expanded their capacity. Moreover, the few lesser-performing students who temporarily lost access to high-quality colleges eventually offset it with higher enrollment rates at other colleges of similar productivity. As a result, there was little change in overall college enrollment and no evidence of negative earnings effects for this group. On the contrary, the financial aid expansion improved efficiency, boosting outcomes for the entire cohort of students. Moreover, it closed the SES gap in college attainment, learning, and earnings among equally-achieving students. Therefore, financial aid improved efficiency and equity.

There are several caveats and directions for future research. First, the financial aid program we study shifted students toward elite colleges that are more costly for students if they drop out. Understanding the effects of more debt incurred on dropouts is crucial to assess the benefits of the program relative to their measured costs. However, this requires observing outcomes realized many years (possibly decades) after dropout, which is currently not feasible. Second, future research should compare the labor market returns for SPP beneficiaries and non-beneficiaries to understand whether elite colleges produce differential labor market outcomes by SES, as in Zimmerman (2019).

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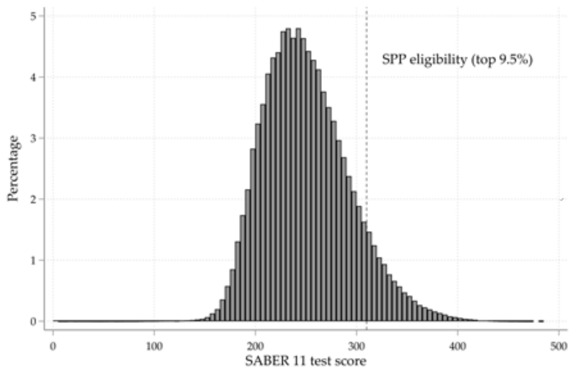
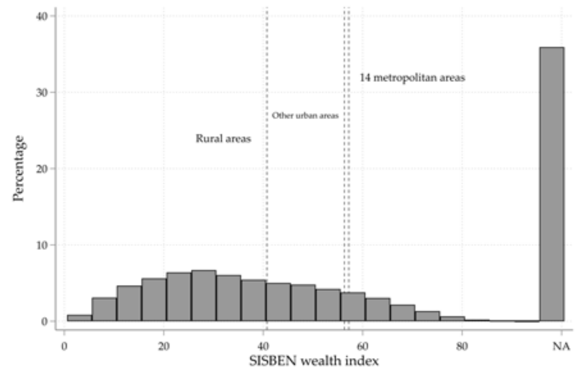
Figures and Tables

Figure I: SPP Eligibility Conditions

(a) Merit: SABER 11 test score $\geq 310/500$



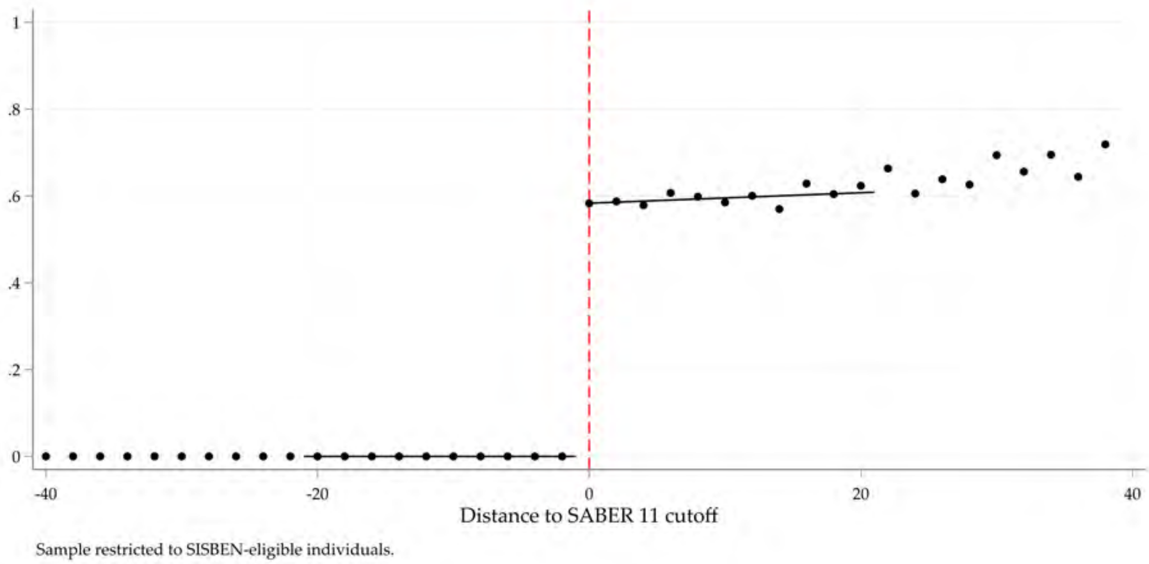
(b) Need: SISBEN wealth index < threshold



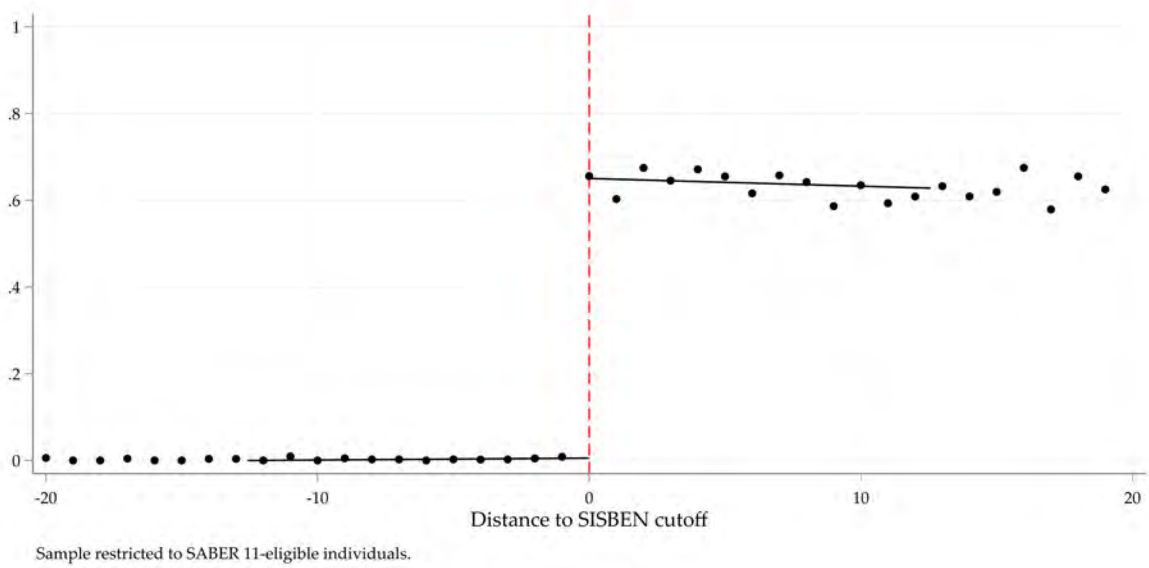
Notes: To be eligible for SPP financial aid program, students must score 310 or more out of 500 (i.e., top 9.5%) in the national standardized high school exit exam, SABER 11. In addition, applicants' household wealth index, SISBEN, must be below a threshold (i.e., bottom 52.8%). These figures show the distribution of SABER 11 test scores (Panel A) and SISBEN poverty index (Panel B) for Fall 2014 test-takers. The red vertical lines represent the SPP eligibility cutoffs. The figures suggest that both variables are distributed smoothly around the eligibility cutoffs. In Panel B, the SISBEN eligibility cutoff varies by the applicant's geographic location. Roughly one-third of test-takers are not in SISBEN (e.g., individuals that do not receive welfare) and, therefore, do not have a SISBEN score; these individuals appear in Panel B as "N/A".
Sources: Authors' calculations based on SABER 11 (ICFES) and SISBEN (DNP).

Figure II: Discontinuity in the Probability of Receiving SPP Financial Aid

(a) Merit-Based Eligibility



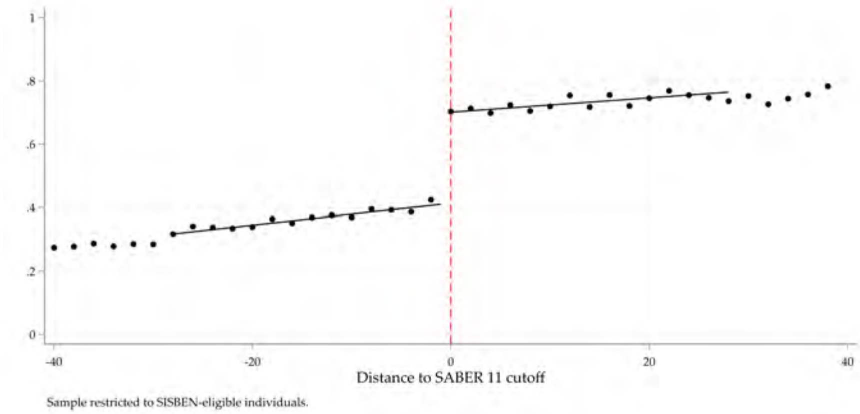
(b) Need-Based Eligibility



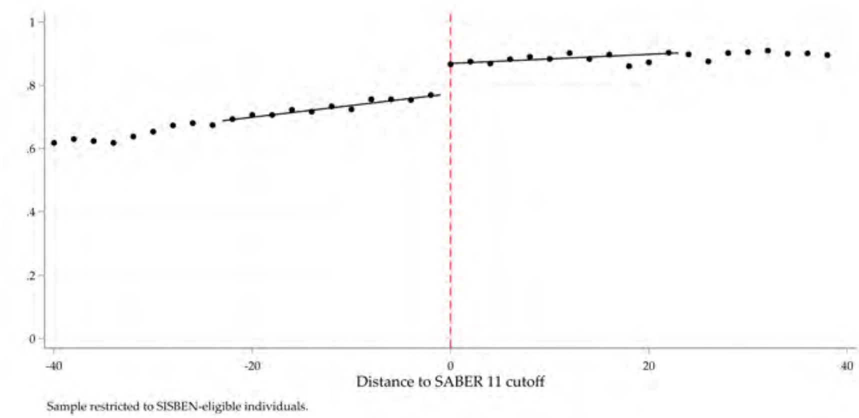
Notes: The figures plot the take-up rate, that is, the probability of receiving SPP financial aid program as a function of the distance to the SABER 11 (Panel A) and SISBEN (Panel B) eligibility cutoffs, restricting the sample to need- and merit-eligible students, respectively. The probability of being 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 III: Enrollment Within Zero and Six Years from High School (Merit Cutoff)

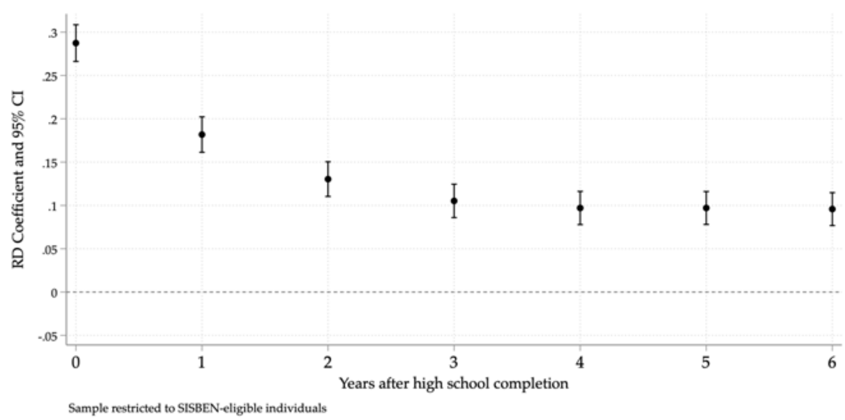
(a) Enrollment Within Zero Years



(b) Enrollment Within Six Years



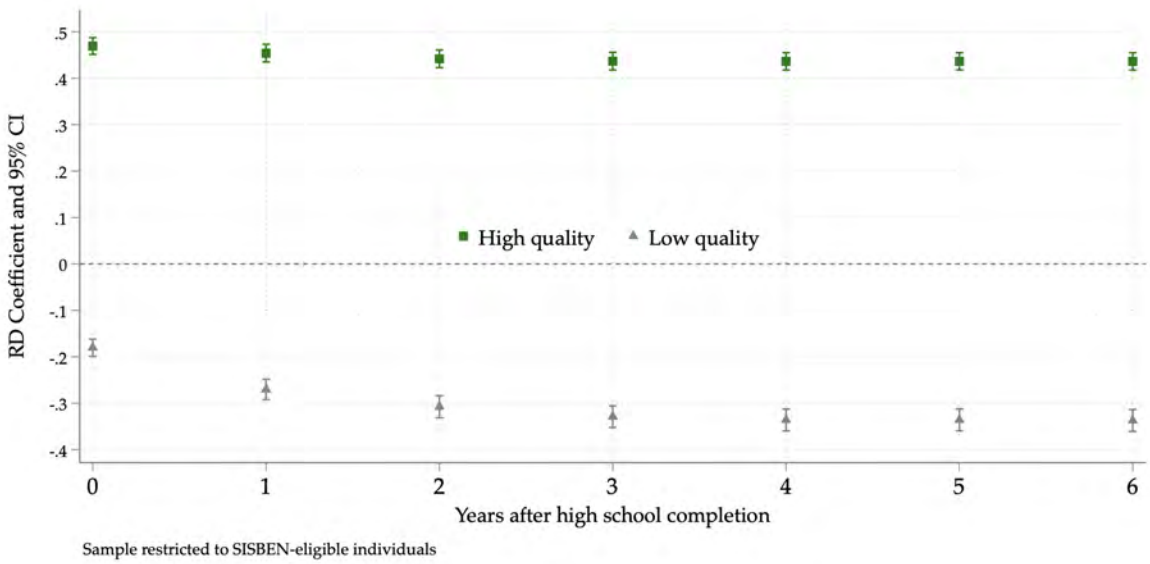
(c) Enrollment Effect Falls Over Time But Stabilizes after Three Years



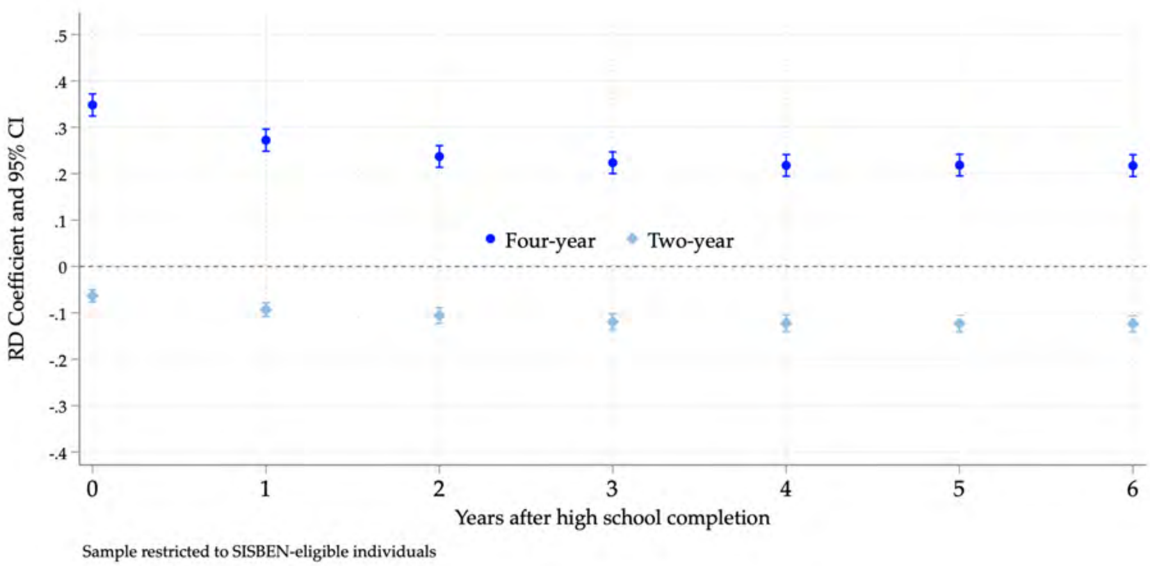
Notes: Panels A and B plot the probability of ever attending college within zero and six years after high school completion, respectively, as a function of the distance to the merit cutoff (for need-eligible students). Panel C plots the RD coefficients over time. Figure A.4 shows similar effects using 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 IV: Enrollment by College Quality and Program Duration (Merit Cutoff)

(a) High- versus Low-Quality College



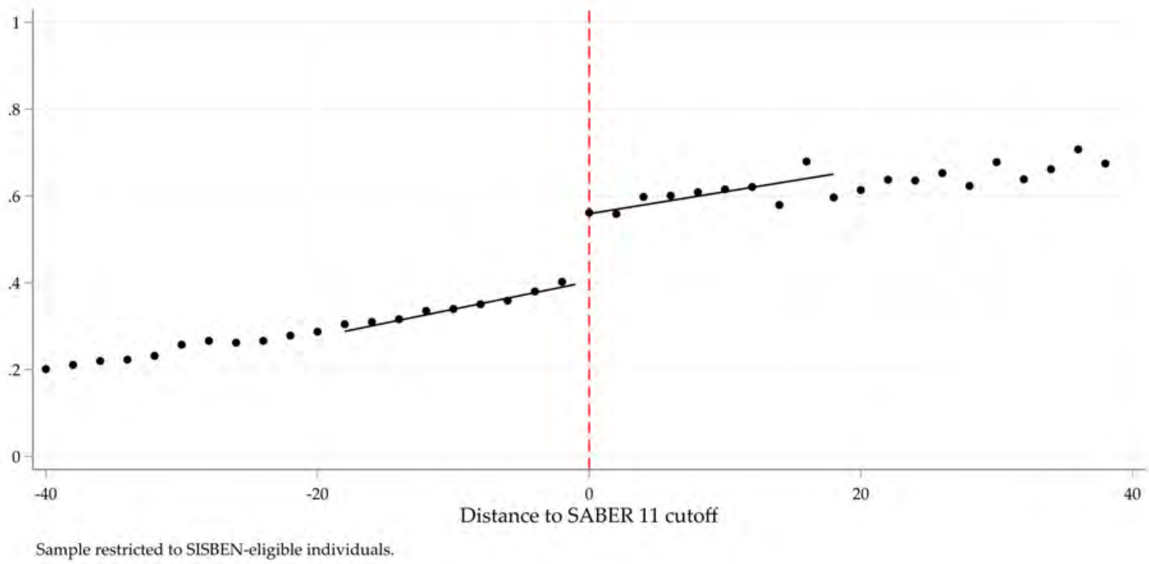
(b) Four- (or five-)year program versus two- (or three-)year program



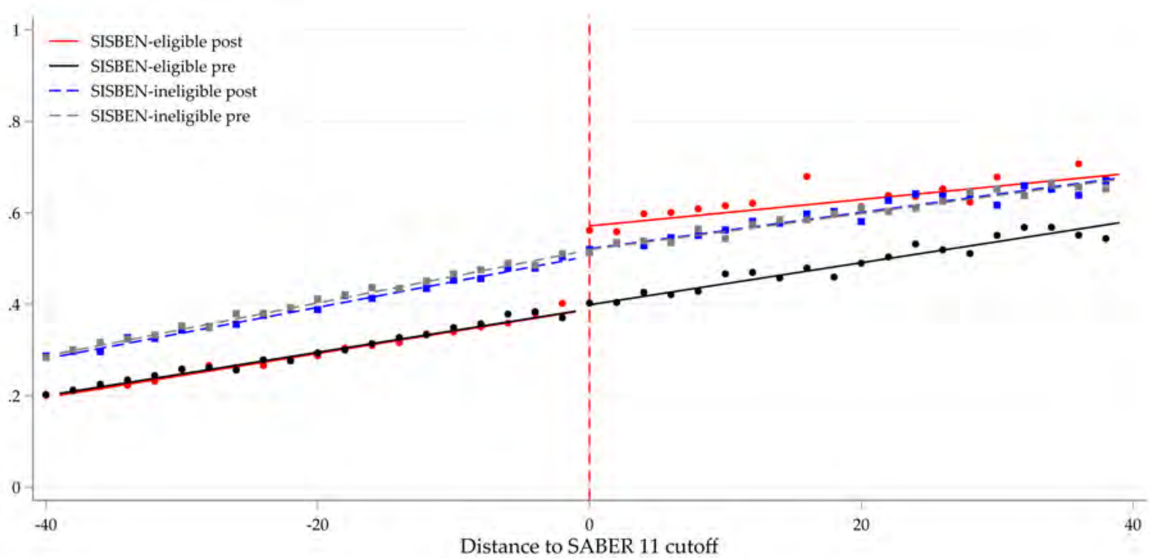
Notes: The figures decompose the enrollment effects over time from Figure III by college quality and program duration based on the merit discontinuity (for need-eligible students). Panel A plots the RD coefficient on the probability of ever attending a high- or low-quality college. Panel B plots the RD coefficients on the probability of ever attend a four- (or five-)year program or a two- (or three-)year program. Figure A.5 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 V: Bachelor's Degree Earned Within Seven Years from High School

(a) Merit Cutoff



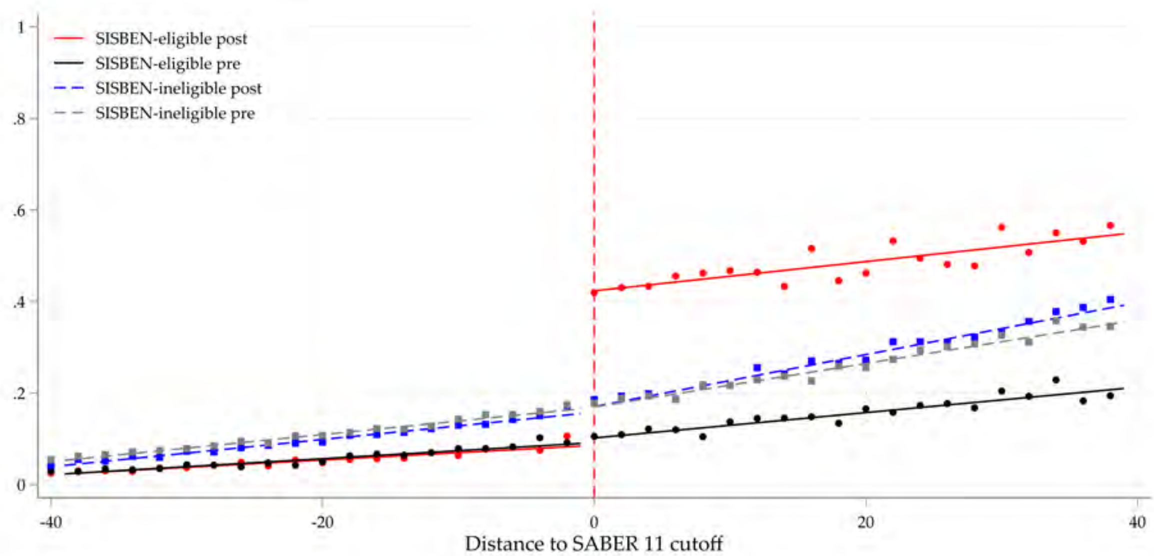
(b) Placebo



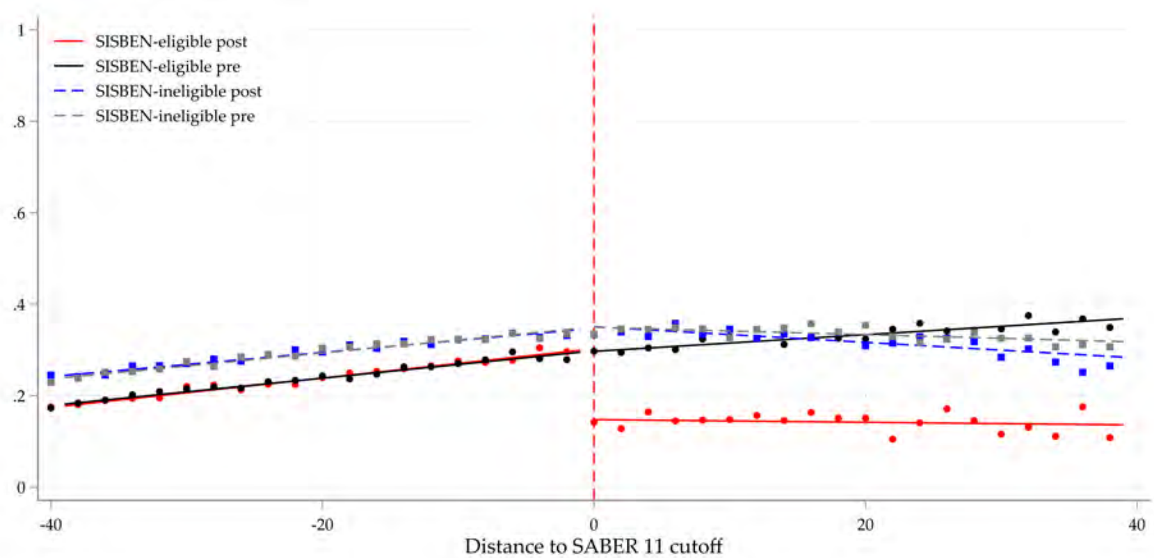
Notes: The figures plot the likelihood of earning a bachelor's degree (proxied by taking the SABER PRO exam) within seven years from high school completion as a function of the distance to the merit cutoff. Panel A restricts the sample to need-eligible students (Table II reports the reduced-form estimate). Panel B shows the equity implications of expanding financial aid by comparing the series from Panel A (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.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SABER PRO (ICFES).

Figure VI: Bachelor's Degree Attainment by College Quality (Merit Cutoff)

(a) High Quality



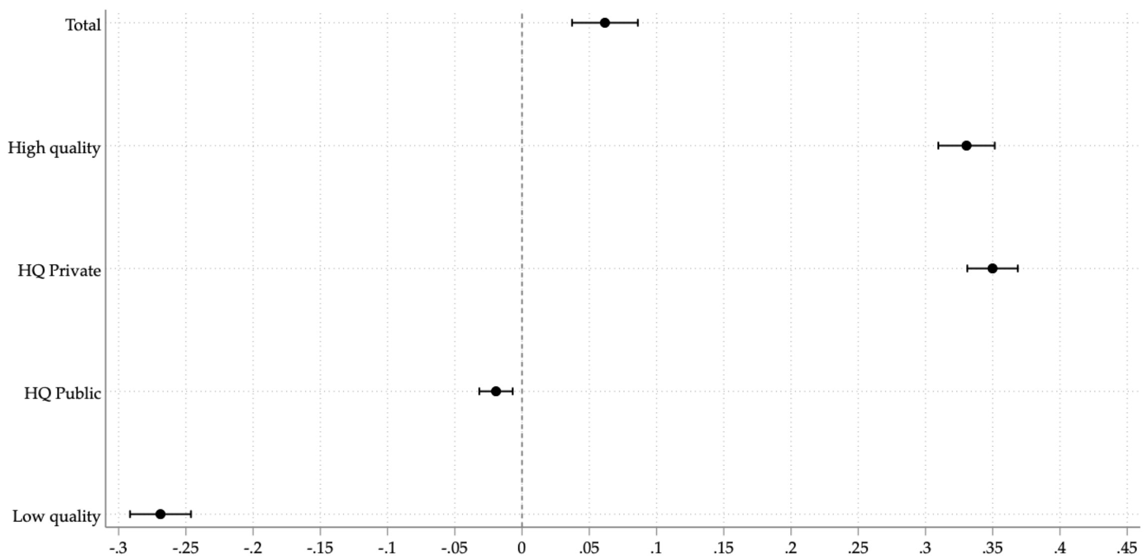
(b) Low Quality



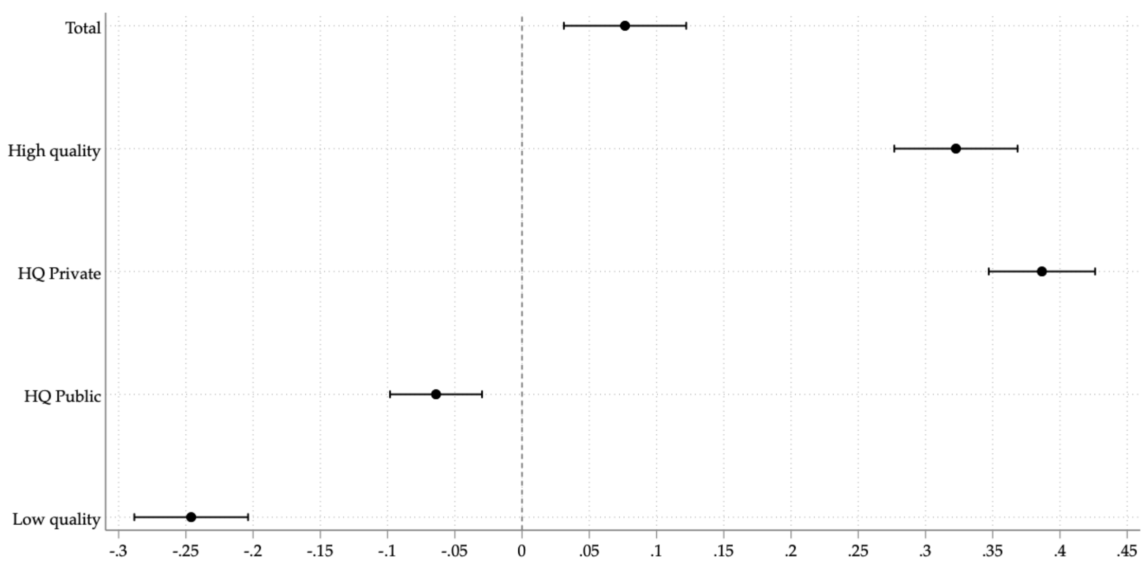
Notes: The figures decompose bachelor's degree attainment (proxied by taking the SABER PRO exam) by high- and low-quality colleges in Panels A and B, respectively. The figures show the equity implications of expanding financial aid by comparing need-eligible students from 2014 (in red) and three placebo 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 VII: The Effect on Degree Attainment is Driven by High-Quality Private Colleges

(a) Merit Cutoff



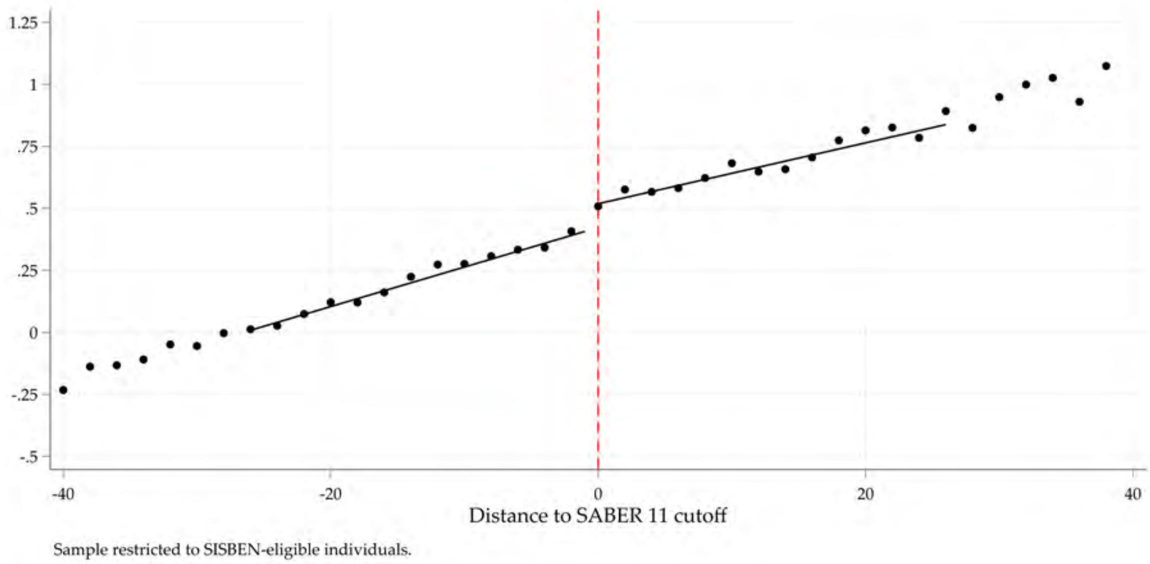
(b) Need Cutoff



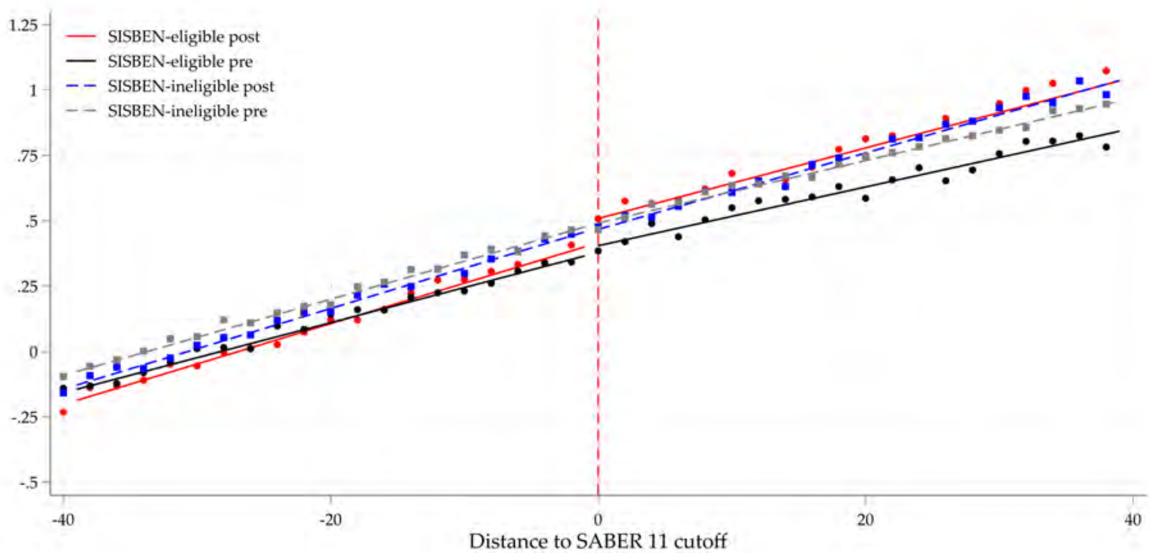
Notes: The figures plot the RD coefficients and 95% confidence intervals, decomposing any degree attainment (proxied by taking SABER PRO or SABER T&T exams) by high-quality, low-quality, private, and public colleges. Panel A (B) uses SABER 11 (SISBEN) as the running variable and restricts the sample 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.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), SABER PRO (ICFES), and SABER T&T (ICFES).

Figure VIII: Standardized College Exit Test Score Within Five Years from High School

(a) Merit Cutoff



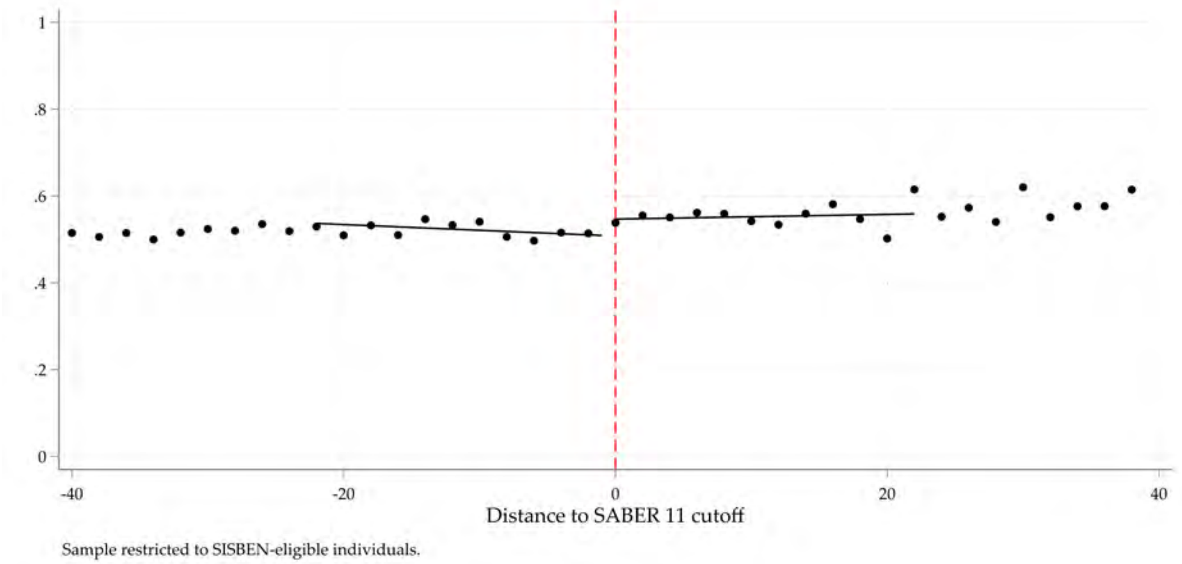
(b) Placebo



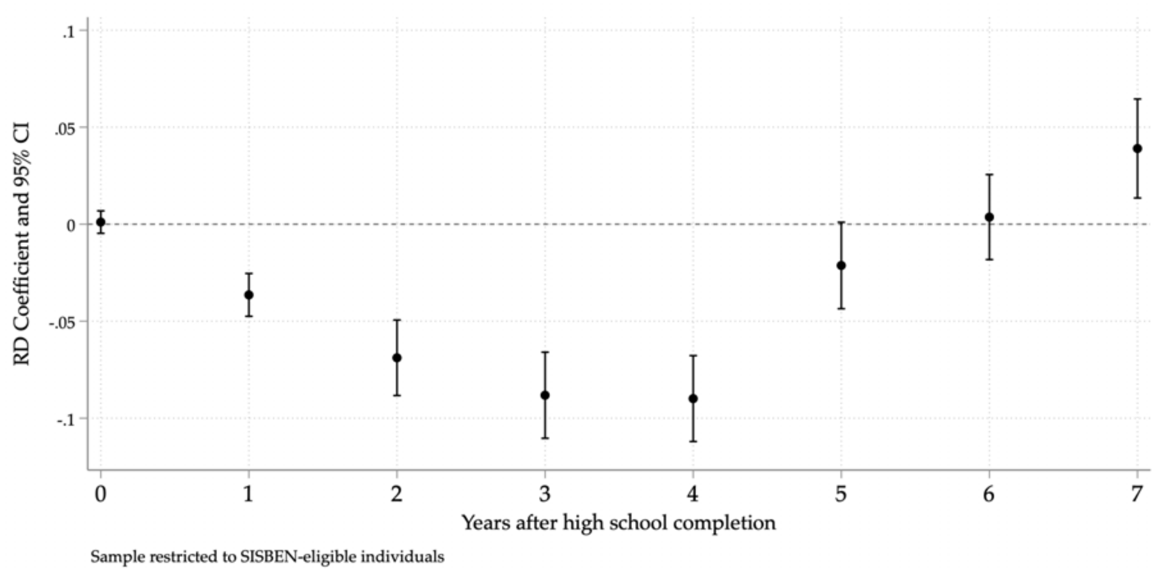
Notes: The figures plot students' performance in Colombia's mandatory standardized college exit exam, SABER PRO, within five years from high school completion as a function of the distance to the merit cutoff. Panel A restricts the sample to need-eligible students (Table IV reports the reduced-form estimate). Panel B shows the equity implications of expanding financial aid by comparing the series from Panel A (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.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SABER PRO (ICFES).

Figure IX: Formal Employment (Merit Cutoff)

(a) Seven Years after High School Completion



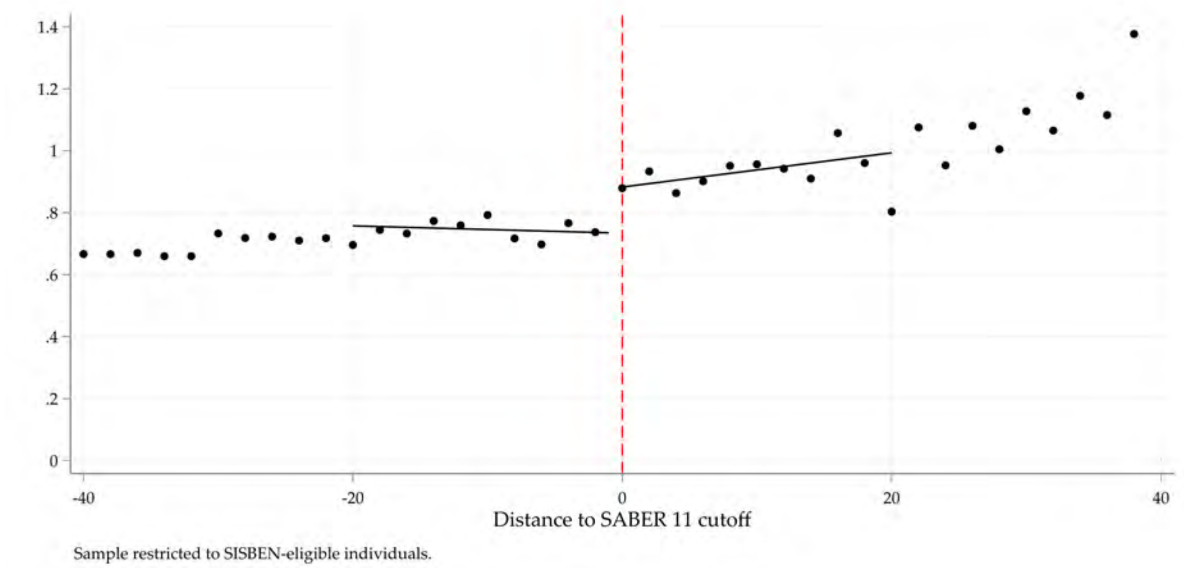
(b) Over Time



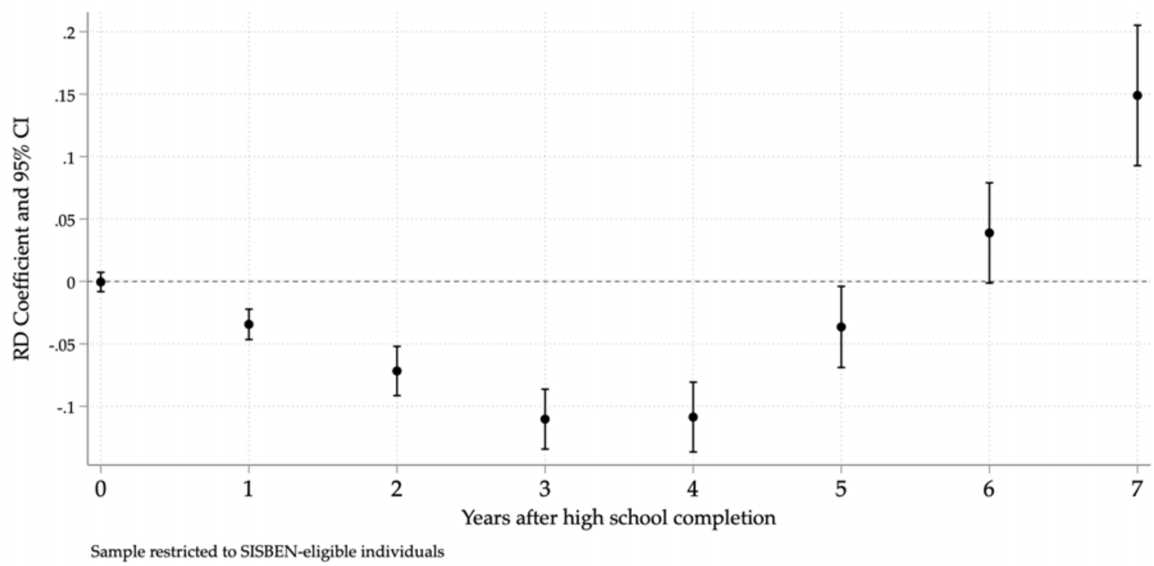
Notes: Panel A plots the probability of formal employment seven years after high school completion as a function of the distance to the merit cutoff (for need-eligible students). Panel B plots the RD coefficient over time. Figure A.13 shows similar effects using SISBEN as the running variable. Table V reports the reduced-form estimates.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and PILA (MinSalud).

Figure X: Formal Earnings (Merit Cutoff)

(a) Seven Years after High School Completion

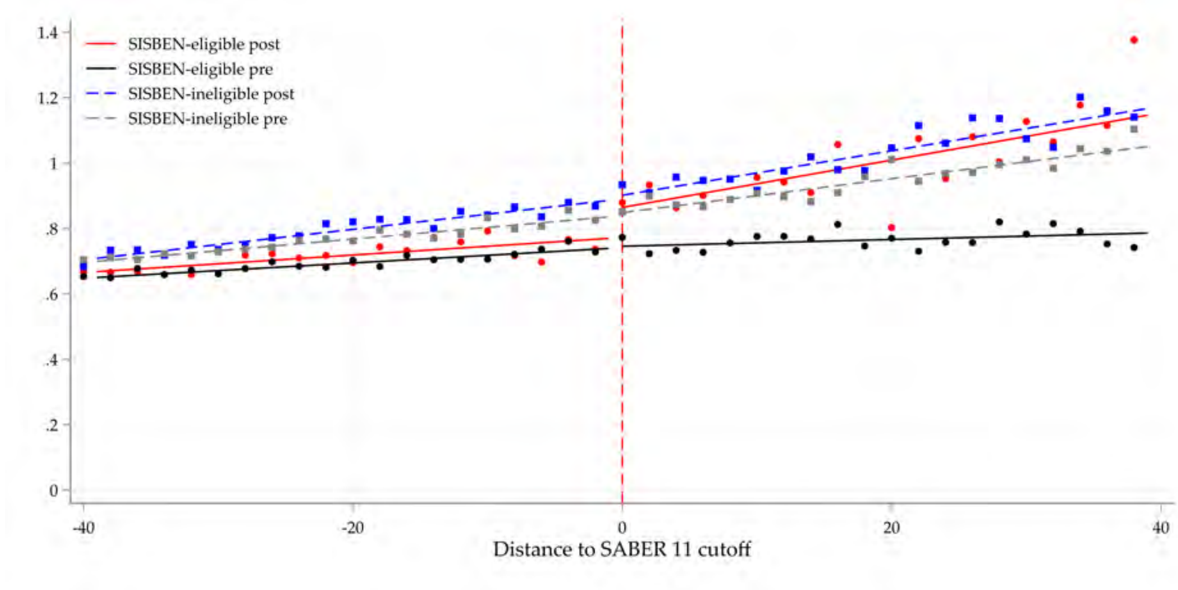


(b) Over Time



Notes: Panel A plots individuals' formal earnings (expressed as multiples of the monthly minimim wage) seven years after high school completion as a function of the distance to the merit cutoff (for need-eligible students). Individuals without formal employment are assigned zero earnings. Panel B plots the RD coefficient over time. Figure A.14 shows similar effects using SISBEN as the running variable. Table V reports the reduced-form estimates.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and PILA (MinSalud).

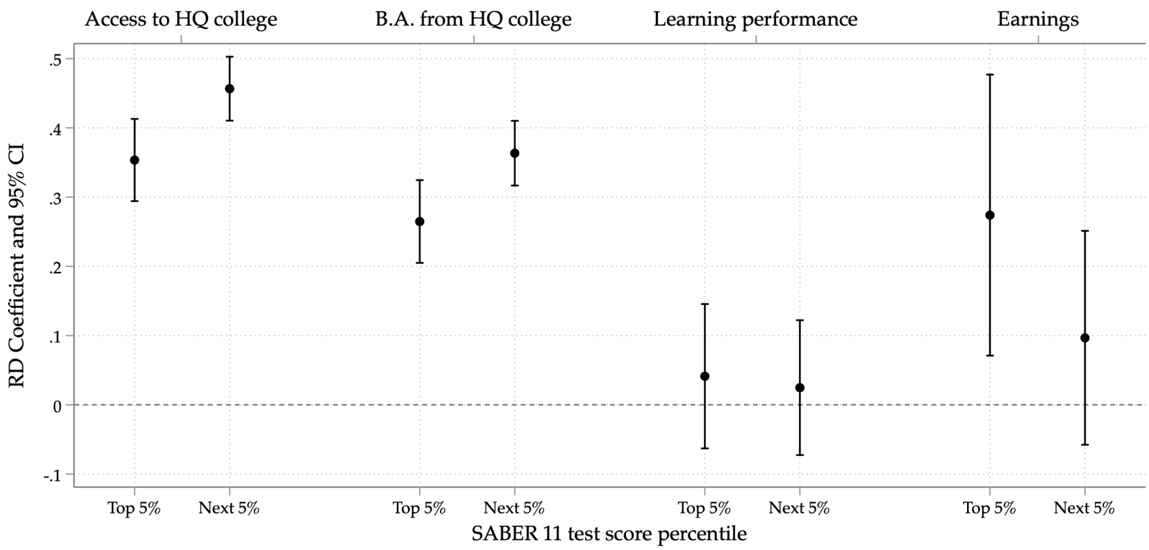
Figure XI: Financial Aid Closes the SES Gap in Formal Earnings



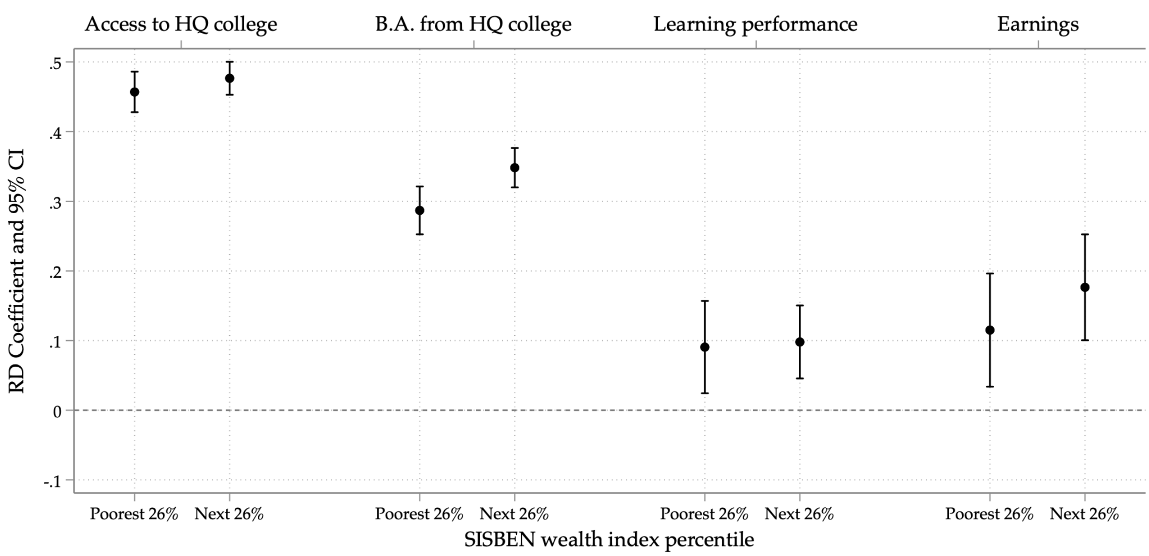
Notes: This figure plots individuals' formal earnings seven years after high school completion as a function of the distance to the merit cutoff. Earnings are expressed in multiples of the monthly minimim wage include zeros for individuals without formal employment. The figure shows the equity implications of expanding financial aid by comparing need-eligible students who took SABER 11 in 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.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and PILA (MinSalud).

Figure XII: The Impact of the Targeting Instrument

(a) Heterogeneity by Merit (Need Cutoff)



(b) Heterogeneity by Need (Merit Cutoff)



Notes: The figures compare the reduced-form RD coefficient and 95% confidence intervals across the distribution of merit (Panel A) and need (Panel B). Panel A uses the SISBEN wealth index as the running variable and compares effects by above- versus below-median test scores within merit-eligible students (i.e., the top 9.5% of test scores). Panel B uses the SABER 11 test score as the running variable and compares effects by above- versus below-median SISBEN scores within need-eligible students (i.e., the poorest 52.8% of households).
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), SABER PRO (ICFES), 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	0.468	0.470	-0.004	-0.180	-0.065	-0.118	-0.062	0.346	0.096	0.435	0.472	-0.039	-0.336	-0.120	-0.218	-0.121	0.212
	(0.011)	(0.009)	(0.009)	(0.006)	(0.009)	(0.006)	(0.007)	(0.007)	(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	0.214	0.110	0.307	0.773	0.181	0.044	0.140	0.587	0.200	0.391	0.214	0.562
Observations										297,279								
BW loc. poly.	28.48	29.71	25.41	24.56	27.36	30.26	33.66	23.61	21.68	23.33	35.22	23.26	25.54	22.33	26.60	27.86	22.81	20.62
Effect obs. control	29,368	30,526	24,714	23,070	27,607	32,363	37,647	21,963	18,948	21,963	41,192	21,963	24,714	20,459	25,871	27,607	20,459	17,966
Effect obs. treat	11,214	11,339	10,576	10,299	11,002	11,576	12,061	10,107	9,489	10,107	12,330	10,107	10,576	9,815	8,796	11,002	9,815	9,317
***Panel B:** SISBEN is the running variable*																		
Reduced form	0.226	0.420	0.477	-0.055	-0.190	-0.078	-0.113	-0.062	0.290	0.049	0.363	0.474	-0.115	-0.308	-0.125	-0.182	-0.097	0.146
	(0.021)	(0.019)	(0.015)	(0.015)	(0.017)	(0.011)	(0.015)	(0.010)	(0.020)	(0.016)	(0.019)	(0.018)	(0.018)	(0.021)	(0.015)	(0.018)	(0.013)	(0.019)
Mean control	0.535	0.241	0.073	0.169	0.293	0.113	0.181	0.093	0.442	0.851	0.359	0.096	0.264	0.492	0.199	0.293	0.148	0.703
Observations										22,552								
BW loc. poly.	11.24	12.41	14.68	12.14	11.55	11.86	10.89	11.40	12.21	9.33	12.43	11.36	11.04	9.62	10.38	10.48	12.21	9.66
Effect obs. control	4,674	5,049	5,667	4,983	4,802	4,904	4,540	4,723	5,000	4,005	5,056	4,708	4,599	4,093	4,383	4,405	4,995	4,114
Effect obs. treat	4,797	5,230	6,021	5,158	4,907	5,051	4,652	4,849	5,181	4,012	5,240	4,837	4,717	4,130	5,234	4,478	5,179	4,149

Notes: This table presents the reduced-form effect of financial aid 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., high-quality, low-quality) 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 need-eligible 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 need-eligible individuals, financial aid 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	0.323	0.345	-0.016	-0.161	-0.066	-0.080	0.086	0.123	0.016	0.032	-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	0.097	0.031	0.063	0.304	0.108	0.148	0.138	0.301	0.005	0.051	0.048
Observations								297,279						
BW loc. poly.	22.71	22.97	18.78	19.70	28.25	20.94	30.76	30.68	25.16	25.08	17.78	26.38	27.21	24.59
Effect obs. control	20,459	20,459	15,683	16,562	29,368	17,966	32,363	32,363	24,714	24,714	14,367	25,871	27,607	23,070
Effect obs. treat	9,815	9,815	8,796	8,987	11,214	9,317	11,576	11,576	10,576	10,576	8,464	10,754	11,002	10,299
***Panel B:** SISBEN is the running variable*														
Reduced form	0.077	-0.060	0.145	0.328	0.393	-0.066	-0.182	-0.080	-0.085	0.064	0.100	0.015	0.055	-0.015
	(0.023)	(0.015)	(0.020)	(0.019)	(0.017)	(0.014)	(0.017)	(0.012)	(0.013)	(0.019)	(0.023)	(0.006)	(0.015)	(0.009)
Mean control	0.661	0.111	0.546	0.240	0.074	0.167	0.305	0.123	0.137	0.236	0.409	0.015	0.069	0.045
Observations								22,552						
BW loc. poly.	8.72	7.42	12.43	13.13	12.86	12.53	12.53	11.16	11.09	12.28	10.63	12.52	8.07	10.52
Effect obs. control	3,738	3,199	5,053	5,260	5,166	5,087	5,087	4,630	4,606	5,011	4,454	5,084	3,466	4,424
Effect obs. treat	3,761	3,162	5,234	5,484	5,371	5,264	5,264	4,772	4,735	5,196	4,537	5,263	3,450	4,496

Notes: This table presents the reduced-form effect of financial aid eligibility 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 low-quality 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 work	Formal earnings (includes zeros)		
	Any college	High-quality college		Program duration		Any degree	Two-year degree	Four-year degree			Five years	Seven years		in constant pesos	in monthly min. wages	in natural logarithm
		Any	Private	Two Years	Four Years			Any college	High-quality college: Any	High-quality college: Private						
	(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	0.364	0.106	-0.173	0.268	0.554	0.593	0.119	0.076	0.067	225,059.50	0.255	0.109
	(0.016)	(0.012)	(0.011)	(0.015)	(0.020)	(0.021)	(0.015)	(0.023)	(0.016)	(0.012)	(0.026)	(0.025)	(0.022)	(42,959.05)	(0.049)	(0.032)
First stage	0.583	0.579	0.583	0.583	0.583	0.583	0.583	0.584	0.584	0.581	0.804	0.732	0.583	0.584	0.584	0.637
	(0.009)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)	(0.008)	(0.011)	(0.011)	(0.009)	(0.009)	(0.010)	(0.011)
Mean control	0.773	0.181	0.044	0.214	0.562	0.584	0.184	0.403	0.097	0.031	0.423	0.448	0.508	645,803.90	0.734	13.901
Observations						297,279					23,059	41,430		297,279		131,719
BW loc. poly.	23.335	35.224	23.263	22.808	20.623	22.714	22.966	18.781	19.696	28.255	26.531	24.858	22.043	20.400	20.346	29.182
Effect obs. control	21,963	41,192	21,963	20,459	17,966	20,459	20,459	15,683	16,562	29,368	4,491	7,350	20,459	17,966	17,966	15,966
Effect obs. treat	10,107	12,330	10,107	9,815	9,317	9,815	9,815	8,796	8,987	11,214	4,576	6,186	9,815	9,317	9,317	6,274
***Panel B:** SISBEN is the running variable*																
IV	0.081	0.565	0.741	-0.151	0.232	0.124	-0.100	0.225	0.506	0.610	0.059	0.044	0.054	242,131.70	0.277	0.155
	(0.026)	(0.031)	(0.025)	(0.024)	(0.031)	(0.038)	(0.022)	(0.038)	(0.033)	(0.029)	(0.046)	(0.047)	(0.039)	(92,823.59)	(0.106)	(0.073)
First stage	0.634	0.635	0.634	0.633	0.635	0.634	0.634	0.635	0.635	0.633	0.801	0.740	0.635	0.635	0.635	0.665
	(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)	(0.017)	(0.017)	(0.024)
Mean control	0.849	0.357	0.094	0.145	0.704	0.659	0.114	0.546	0.240	0.069	0.809	0.842	0.562	853,093.60	0.967	14.012
Observations	22,552										9,047	13,694		22,552		12,831
BW loc. poly.	8.305	8.443	8.258	7.795	8.510	8.135	8.075	8.663	8.579	7.505	12.116	10.209	8.475	8.772	8.768	7.567
Effect obs. control	3,560	3,623	3,544	3,337	3,650	3,481	3,466	3,721	3,676	3,219	1,572	2,386	3,637	3,754	3,749	1,811
Effect obs. treat	3,544	3,607	3,523	3,331	3,643	3,475	3,450	3,720	3,675	3,209	2,376	2,851	3,622	3,781	3,778	1,938

Notes: This table presents the instrumental variables estimates of the effect of financial aid on educational and labor market outcomes realized up to seven 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 seven 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			
			Any	Private	Public			Five years	Seven years
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
***Panel A:** SABER 11 is the running variable*									
Reduced form	0.758	-0.125	-0.213	0.118	-0.174	0.063	0.005	0.096	0.056
	(0.063)	(0.038)	(0.076)	(0.121)	(0.106)	(0.072)	(0.002)	(0.021)	(0.019)
Mean control	3.319	5.213	5.272	4.921	5.466	5.192	0.008	0.423	0.448
Observations	297,279	22,476	8,809	6,499	2,310	13,667	297,279	23,059	41,430
BW loc. poly.	18.96	25.41	23.34	18.83	26.38	23.61	29.91	26.53	24.86
Effect obs. control	15,683	3,986	795	227	573	2,830	30,526	4,491	7,350
Effect obs. treat	8,796	5,342	4,336	3,313	565	756	11,339	4,576	6,186
***Panel B:** SISBEN is the running variable*									
Reduced form	0.507	-0.190	-0.242	0.011	-0.088	0.047	0.016	0.057	0.033
	(0.113)	(0.062)	(0.082)	(0.114)	(0.128)	(0.112)	(0.007)	(0.040)	(0.035)
Mean control	3.836	5.234	5.254	4.940	5.405	5.218	0.016	0.804	0.843
Observations	22,552	10,691	8,261	6,322	1,939	2,430	22,552	9,047	13,694
BW loc. poly.	7.94	9.17	8.91	8.76	9.34	14.72	9.37	9.89	10.03
Effect obs. control	3,421	1,365	636	255	399	1,023	4,011	1,320	2,359
Effect obs. treat	3,385	2,227	1,932	1,610	311	414	4,024	1,969	2,804

Notes: This table presents the reduced-form estimates of the effect of financial aid 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 work	Formal earnings (includes zeros)			Time to first formal job
		in constant pesos	in monthly min. wages	in natural logarithm	
	(1)	(2)	(3)	(4)	(5)
***Panel A:** SABER 11 is the running variable*					
Reduced form	0.039	131,329.30	0.149	0.069	-0.188
	(0.013)	(25,210.93)	(0.029)	(0.020)	(0.094)
Mean control	0.508	645,803.90	0.734	13.901	3.083
Observations	297,279	297,279	297,279	131,719	29,073
BW loc. poly.	22.04	20.40	20.35	29.18	34.20
Effect obs. control	20,459	17,966	17,966	15,966	6,267
Effect obs. treat	9,815	9,317	9,317	6,274	2,934
***Panel B:** SISBEN is the running variable*					
Reduced form	0.041	155,972.40	0.179	0.110	-0.292
	(0.020)	(55,843.68)	(0.064)	(0.043)	(0.178)
Mean control	0.559	856,502.70	0.971	14.016	2.875
Observations	22,552	22,552	22,552	12,831	5,110
BW loc. poly.	12.95	9.88	9.88	9.80	9.84
Effect obs. control	5,215	4,196	4,194	2,336	705
Effect obs. treat	5,412	4,237	4,232	2,507	1,133

Notes: This table presents the reduced-form estimates of the effect of financial aid on early-career labor market outcomes using an RD design. The outcomes in Columns (1)–(4) are measured seven 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 $32.42 and the control mean is $159.41 including zeros and $313.98 excluding zeros. Column (5) 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: The Effect of Financial Aid on Educational and Labor Market Outcomes Explained by College-Program Fixed Effects

	College attainment				College exit test score		Formal labor market outcomes			
	Any degree		Four-year degree				Employment		Earnings	
	Outcome	FE	Outcome	FE	Outcome	FE	Outcome	FE	Outcome	FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
***Panel A:** SABER 11 is the running variable*										
Reduced form	0.032	-0.010	0.063	0.011	0.054	0.108	0.043	0.038	0.159	0.113
	(0.013)	(0.004)	(0.016)	(0.004)	(0.019)	(0.006)	(0.014)	(0.004)	(0.030)	(0.009)
Observations	130,353	130,353	68,426	68,426	35,489	35,371	284,755	284,755	284,755	284,755
***Panel B:** SISBEN is the running variable*										
Reduced form	0.079	0.001	0.081	0.023	0.022	0.065	0.032	0.036	0.185	0.127
	(0.026)	(0.007)	(0.023)	(0.006)	(0.034)	(0.011)	(0.023)	(0.006)	(0.064)	(0.018)
Observations	19,463	19,463	17,601	17,601	12,485	12,461	21,211	21,211	21,211	21,211

Notes: This table presents the portion of the reduced-form estimates on educational and early-career labor market outcomes explained by the college-program graduation, learning, employment, and earnings productivities using an RD design. The outcomes are measured within seven years from high school completion in Columns (1) through (6) and seven 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 fixed effect 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 C 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: The Overall Impact of Financial Aid on College Access

	Enrollment within zero years from high school					Enrollment within six years from high school				
	Any	High-quality college			Low-	Any	High-quality college			Low-
	college	Any	Private	Public	Quality	college	Any	Private	Public	Quality
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: *SISBEN-eligible*										
Decile 9	0.172	0.051	0.015	0.037	0.120	0.295	0.104	0.025	0.079	0.191
	(0.003)	(0.002)	(0.001)	(0.001)	(0.003)	(0.003)	(0.002)	(0.001)	(0.002)	(0.004)
Decile 10	0.330	0.158	0.037	0.121	0.172	0.380	0.245	0.050	0.195	0.135
	(0.004)	(0.003)	(0.002)	(0.003)	(0.004)	(0.004)	(0.004)	(0.002)	(0.004)	(0.004)
Decile 9 x 2013	0.013	0.001	0.000	0.000	0.012	0.005	-0.001	-0.002	0.001	0.006
	(0.005)	(0.002)	(0.001)	(0.002)	(0.004)	(0.005)	(0.003)	(0.002)	(0.003)	(0.005)
Decile 10 x 2013	-0.010	0.008	0.006	0.003	-0.018	0.013	0.013	0.005	0.008	-0.001
	(0.006)	(0.005)	(0.002)	(0.004)	(0.005)	(0.005)	(0.005)	(0.003)	(0.005)	(0.006)
Decile 9 x 2014	0.025	0.002	0.000	0.002	0.022	0.007	-0.004	-0.003	0.000	0.011
	(0.005)	(0.002)	(0.001)	(0.002)	(0.004)	(0.004)	(0.003)	(0.002)	(0.003)	(0.005)
Decile 10 x 2014	0.224	0.416	0.429	-0.013	-0.191	0.084	0.368	0.432	-0.064	-0.283
	(0.006)	(0.005)	(0.004)	(0.004)	(0.005)	(0.004)	(0.005)	(0.004)	(0.005)	(0.006)
N	814,002									
Panel B: *SISBEN-ineligible*										
Decile 9	0.194	0.084	0.052	0.032	0.110	0.246	0.143	0.080	0.063	0.103
	(0.003)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.003)
Decile 10	0.341	0.263	0.142	0.122	0.077	0.300	0.358	0.188	0.170	-0.058
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Decile 9 x 2013	-0.014	-0.002	0.002	-0.004	-0.012	0.003	0.000	0.004	-0.003	0.003
	(0.004)	(0.002)	(0.002)	(0.001)	(0.004)	(0.003)	(0.003)	(0.003)	(0.002)	(0.004)
Decile 10 x 2013	-0.019	0.006	0.012	-0.006	-0.025	0.009	0.012	0.014	-0.002	-0.003
	(0.004)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Decile 9 x 2014	-0.015	-0.007	-0.004	-0.003	-0.008	-0.002	-0.009	-0.005	-0.005	0.007
	(0.004)	(0.002)	(0.002)	(0.001)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)	(0.004)
Decile 10 x 2014	-0.030	0.010	0.012	-0.002	-0.040	0.010	0.018	0.017	0.001	-0.008
	(0.004)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
N	850,855									

Notes: This table compares the overall effects of financial aid on college access across the distributions of SES and test scores using the difference-in-difference specification (2). Panels A and B focus on low- and high-SES students, defined as being eligible and ineligible for financial aid based on their SISBEN score. Moreover, students in "Decile 10" are eligible for financial aid based on their SABER 11 test score, while students in deciles nine and below are not; therefore, they are potentially "displaced" from colleges by the better-performing financial aid recipients. Robust standard errors are in parentheses.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SNIES (MEN).

Table VIII: The Overall Impact of Financial Aid on College Attainment and Learning

	Degree attainment within seven years from high school completion							College exit test score if exam taken within	
	Any	Two-year	Four-year degree						
			Any	High-quality college			Low-quality	Five	Seven
	Degree	degree	college	Any	Private	Public	college	years	years
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: *SISBEN-eligible*									
Decile 9	0.280	0.056	0.223	0.057	0.016	0.040	0.167	0.602	0.611
	(0.004)	(0.003)	(0.003)	(0.002)	(0.001)	(0.001)	(0.003)	(0.011)	(0.008)
Decile 10	0.386	-0.025	0.411	0.168	0.042	0.126	0.244	1.115	1.113
	(0.004)	(0.003)	(0.004)	(0.003)	(0.002)	(0.003)	(0.004)	(0.011)	(0.008)
Decile 9 x 2013	-0.016	-0.014	-0.002	-0.003	0.000	-0.003	0.002	0.000	0.009
	(0.005)	(0.004)	(0.005)	(0.002)	(0.001)	(0.002)	(0.004)	(0.015)	(0.011)
Decile 10 x 2013	-0.019	-0.005	-0.014	0.002	0.003	-0.002	-0.016	-0.003	0.004
	(0.006)	(0.004)	(0.006)	(0.005)	(0.003)	(0.004)	(0.006)	(0.016)	(0.011)
Decile 9 x 2014	-0.018	-0.009	-0.008	-0.005	-0.001	-0.005	-0.003	0.023	0.047
	(0.005)	(0.004)	(0.005)	(0.002)	(0.001)	(0.002)	(0.004)	(0.014)	(0.010)
Decile 10 x 2014	0.018	-0.083	0.102	0.286	0.326	-0.040	-0.184	0.071	0.082
	(0.006)	(0.004)	(0.006)	(0.005)	(0.004)	(0.004)	(0.005)	(0.014)	(0.011)
N				814,002				57,006	108,519
Panel A: *SISBEN-ineligible*									
Decile 9	0.274	0.001	0.273	0.099	0.063	0.037	0.174	0.597	0.601
	(0.003)	(0.002)	(0.003)	(0.002)	(0.001)	(0.001)	(0.002)	(0.007)	(0.005)
Decile 10	0.380	-0.085	0.465	0.292	0.166	0.126	0.173	1.197	1.181
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.006)	(0.005)
Decile 9 x 2013	-0.017	-0.008	-0.009	-0.004	0.003	-0.007	-0.005	-0.031	-0.012
	(0.004)	(0.003)	(0.004)	(0.003)	(0.002)	(0.001)	(0.004)	(0.010)	(0.008)
Decile 10 x 2013	-0.023	-0.011	-0.012	0.002	0.010	-0.008	-0.014	-0.030	-0.016
	(0.003)	(0.002)	(0.004)	(0.003)	(0.003)	(0.002)	(0.003)	(0.009)	(0.007)
Decile 9 x 2014	-0.029	-0.010	-0.019	-0.012	-0.004	-0.008	-0.008	-0.024	0.008
	(0.004)	(0.003)	(0.004)	(0.002)	(0.002)	(0.001)	(0.004)	(0.010)	(0.008)
Decile 10 x 2014	-0.037	-0.016	-0.022	0.006	0.014	-0.008	-0.028	-0.005	0.039
	(0.003)	(0.002)	(0.004)	(0.003)	(0.003)	(0.002)	(0.003)	(0.009)	(0.007)
N				850,855				118,962	215,076

Notes: This table presents the overall effects of financial aid on college attainment and learning performance across the distributions of SES and test scores using the difference-in-difference specification (2). See the notes under Table VII for other details.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SABER PRO (ICFES).

Table IX: The Overall Impact of Financial Aid on Colleges' and Programs' Productivity

	Measures of college-program specific productivity				
	Any attainment (1)	Bachelor's attainment (2)	College exit test score (3)	Formal employment (4)	Formal earnings (5)
***Panel A:** SISBEN-eligible*					
Decile 9	-0.045	-0.017	0.058	0.030	0.043
	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)
Decile 10	-0.091	-0.044	0.105	0.015	0.003
	(0.001)	(0.002)	(0.003)	(0.001)	(0.003)
Decile 9 x 2013	-0.004	0.000	-0.006	-0.002	-0.006
	(0.002)	(0.002)	(0.003)	(0.001)	(0.003)
Decile 10 x 2013	-0.005	-0.001	0.004	0.000	-0.002
	(0.002)	(0.002)	(0.003)	(0.002)	(0.004)
Decile 9 x 2014	-0.003	0.000	-0.005	-0.003	-0.008
	(0.002)	(0.002)	(0.003)	(0.001)	(0.003)
Decile 10 x 2014	-0.015	0.014	0.085	0.037	0.116
	(0.002)	(0.002)	(0.003)	(0.002)	(0.004)
N	317,533	166,709	83,829	736,867	736867
***Panel B:** SISBEN-ineligible*					
Decile 9	-0.038	-0.013	0.056	0.027	0.052
	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)
Decile 10	-0.084	-0.046	0.124	0.023	0.066
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Decile 9 x 2013	-0.002	-0.001	-0.003	0.000	-0.001
	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)
Decile 10 x 2013	-0.004	-0.002	-0.003	-0.001	-0.002
	(0.001)	(0.001)	(0.002)	(0.001)	(0.003)
Decile 9 x 2014	-0.002	0.000	-0.004	-0.001	-0.003
	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)
Decile 10 x 2014	-0.007	-0.003	0.003	-0.001	0.000
	(0.001)	(0.001)	(0.002)	(0.001)	(0.003)
N	439,777	297,024	175,547	760,383	760,383

Notes: This table presents the overall effects of financial aid on the graduation, learning, employment, and earnings productivities of the colleges and programs attended by students across the distributions of SES and test scores using the difference-in-difference specification (2). See the notes under Table VII for other details. Appendix C details how these college-program productivities are computed.
Sources: Authors' calculations based on SABER 11 (ICFES) , SNIES (MEN), SISBEN (DNP), SABER PRO (ICFES), SABER T&T (ICFES), and PILA (MinSalud).

Table X: The Overall Impact of Financial Aid on Formal Labor Market Outcomes

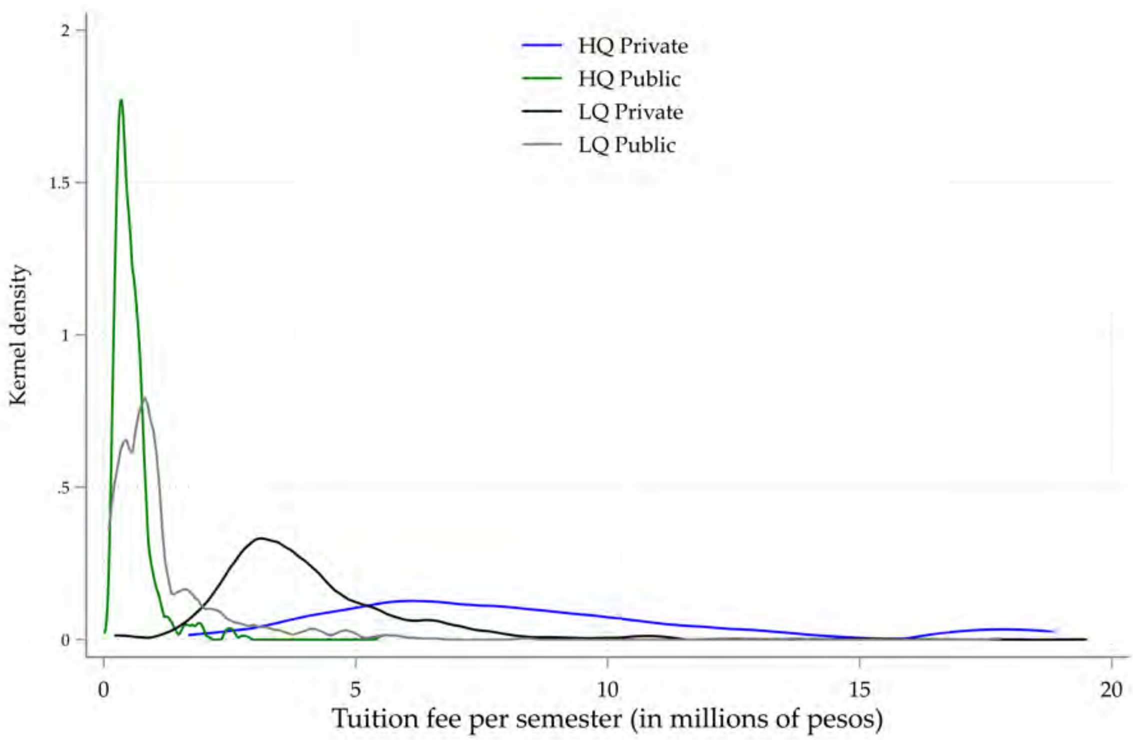
	Formal work	Formal earnings (includes zeros)		
		in constant pesos	in monthly min. wages	in natural logarithm
	(1)	(2)	(3)	(4)
***Panel A:** SISBEN-eligible*				
Decile 9	0.062	127,501.375	0.181	0.134
	(0.004)	(4,675.451)	(0.007)	(0.006)
Decile 10	0.051	197,487.609	0.278	0.239
	(0.004)	(7,018.632)	(0.010)	(0.008)
Decile 9 x 2013	-0.008	-11,351.156	-0.032	-0.019
	(0.005)	(6,745.950)	(0.009)	(0.009)
Decile 10 x 2013	-0.015	-18,693.543	-0.049	-0.013
	(0.006)	(9,957.202)	(0.013)	(0.012)
Decile 9 x 2014	0.002	39,171.582	0.004	0.003
	(0.005)	(7,121.743)	(0.009)	(0.008)
Decile 10 x 2014	0.044	198,668.469	0.166	0.086
	(0.006)	(11,281.120)	(0.014)	(0.011)
N		814,002		355,680
***Panel B:** SISBEN-ineligible*				
Decile 9	0.038	130,299.469	0.186	0.142
	(0.003)	(4,115.709)	(0.006)	(0.005)
Decile 10	0.048	295,193.375	0.417	0.321
	(0.003)	(5,021.630)	(0.007)	(0.005)
Decile 9 x 2013	-0.004	-7,702.790	-0.029	-0.009
	(0.004)	(6,098.134)	(0.008)	(0.008)
Decile 10 x 2013	-0.001	848.245	-0.038	-0.007
	(0.004)	(7,367.577)	(0.010)	(0.008)
Decile 9 x 2014	0.004	50,996.074	0.013	0.016
	(0.004)	(6,738.333)	(0.008)	(0.008)
Decile 10 x 2014	0.010	122,911.344	0.048	0.036
	(0.004)	(8,310.565)	(0.010)	(0.008)
N		850,855		404,485

Notes: This table presents the overall effects of financial aid on formal labor market outcomes across the distributions of SES and test scores using the difference-in-difference specification (2). See the notes under Table VII for other details.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and PILA (MinSalud).

Online Appendix

Appendix A Additional Figures and Tables

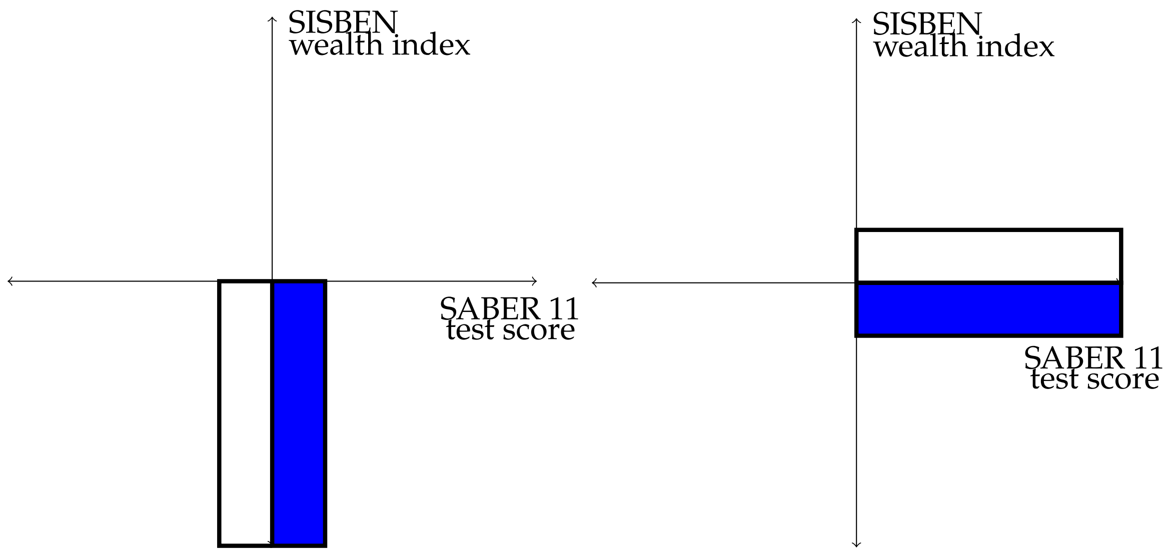
Figure A.1: Tuition Fees



Notes: The figure plots the distribution of semesterly tuition fees for "active" bachelor's programs separately by college type. Tuition fees for public colleges may vary depending on student SES.
Sources: Authors' calculations based on SNIES (MEN).

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

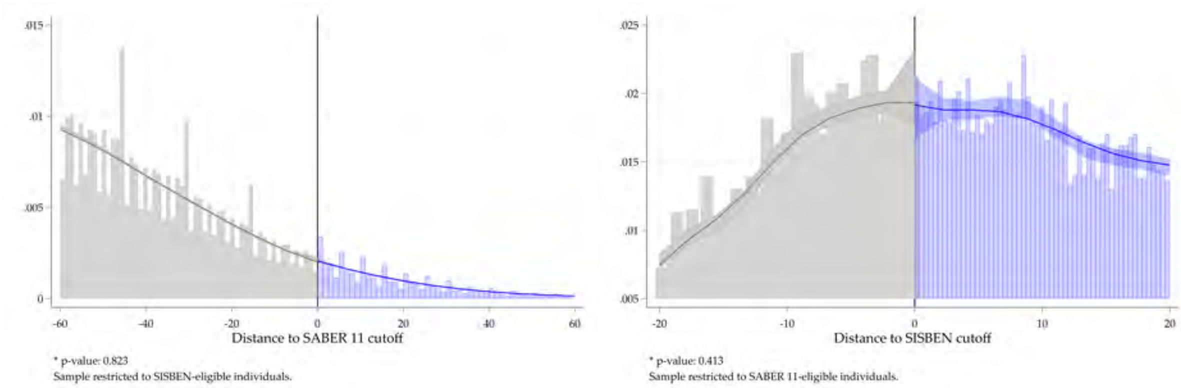
(a) SABER 11 as the running variable (b) SISBEN as the running variable



Notes: This figure illustrates the two types of compliers of the need- and merit-based financial aid program SPP. Panel A uses the SABER 11 test score as the running variable and compares need-eligible students who are barely merit-eligible (in blue) and merit-ineligible (in white). Panel B uses the SISBEN wealth index as the running variable and compares merit-eligible students who are barely need-eligible (in blue) and need-ineligible (in white).

Figure A.3: Manipulation Testing based on Density Discontinuity

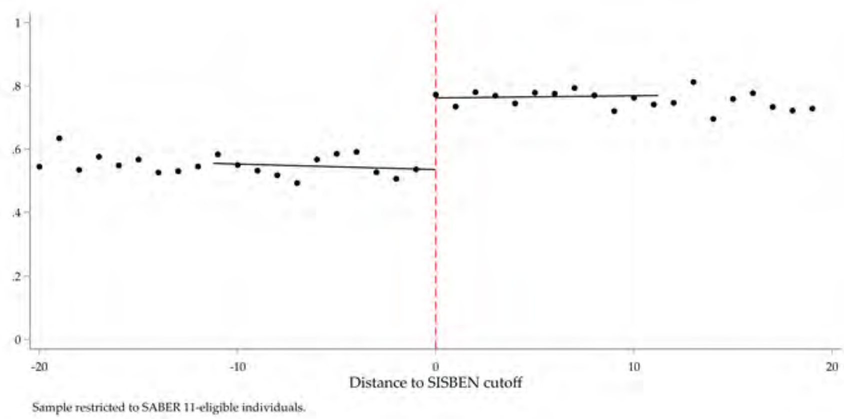
(a) SABER 11 as the running variable (b) SISBEN as the running variable



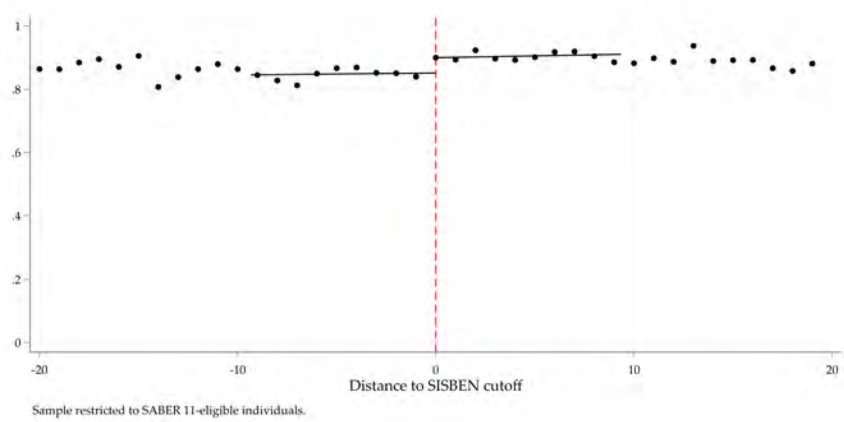
Notes: This figure tests for manipulation of the running variable based on density discontinuity. All results are estimated with package rddensity (Cattaneo et al., 2016) using an unrestricted model and a triangular kernel function, and employ the jackknife standard errors estimator. Panel A restricts 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: Enrollment Within Zero and Six Years from High School (Need Cutoff)

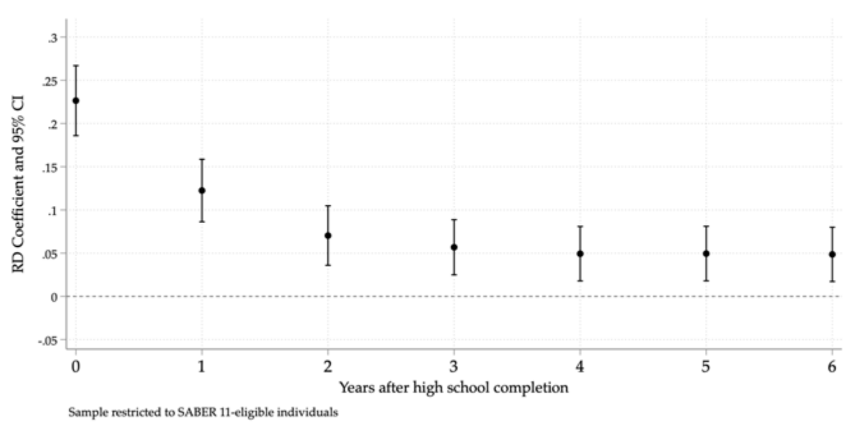
(a) Enrollment Within Zero Years



(b) Enrollment Within Six Years



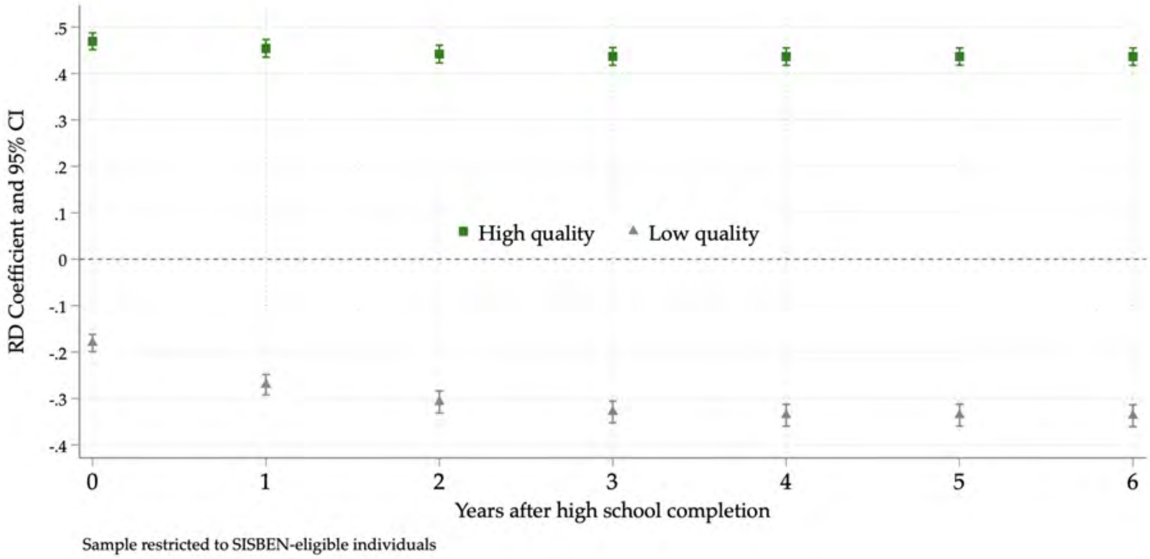
(c) Enrollment Effect Falls Over Time But Stabilizes after Three Years



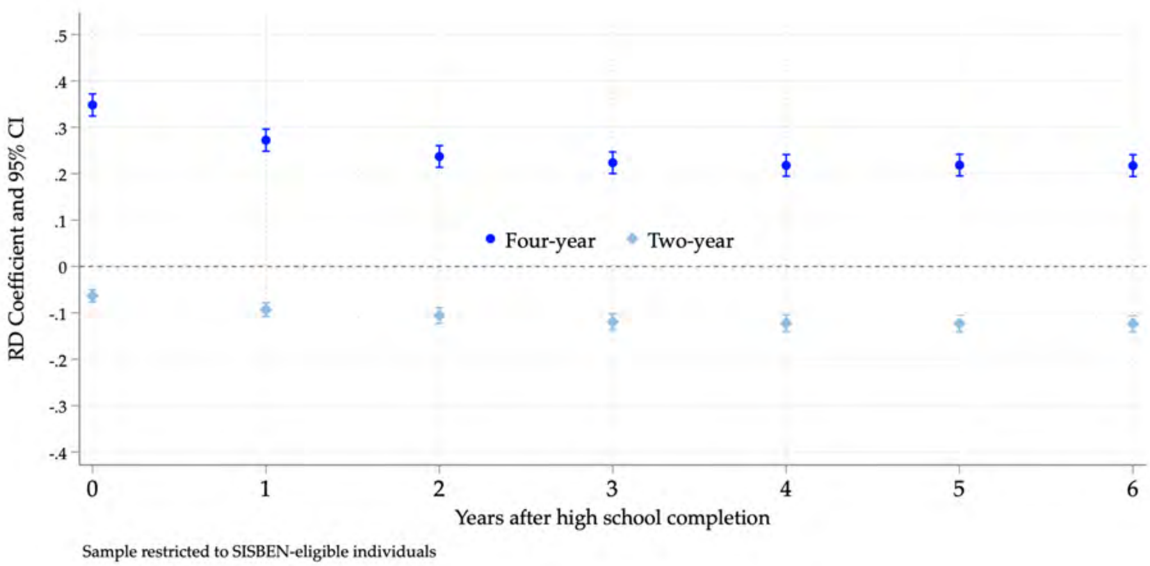
Notes: Panels A and B plot the probability of ever attending college within zero and six years after high school completion, respectively, as a function of the distance to the need cutoff (for merit-eligible students). Panel C plots the RD coefficients over time. Figure III shows similar effects using 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.5: Enrollment by College Quality and Program Duration (Need Cutoff)

(a) High- versus Low-Quality College



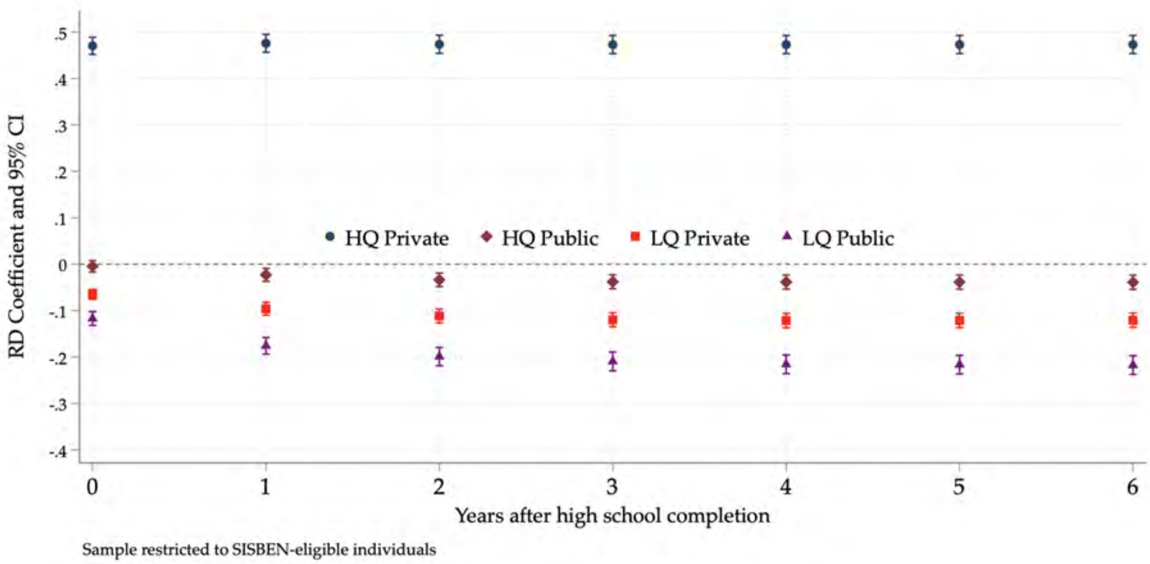
(b) Four- (or five-)year program versus two- (or three-)year program



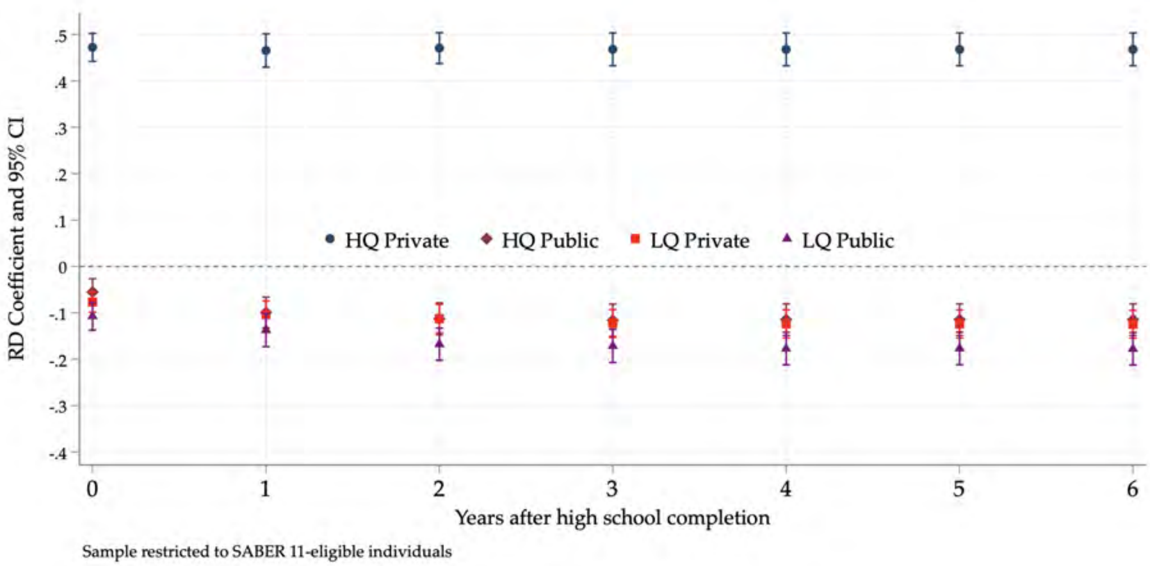
Notes: The figures decompose the enrollment effects over time from Figure A.4 by college quality and program duration based on the need discontinuity (for merit-eligible students). Panel A plots the RD coefficient on the probability of ever attending a high- or low-quality college. Panel B plots the RD coefficients on the probability of ever attend a four- (or five-)year program or a two- (or three-)year program. Figure IV 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.6: Enrollment by College Type: High- vs. Low-Quality and Private vs. Public

(a) Merit Cutoff



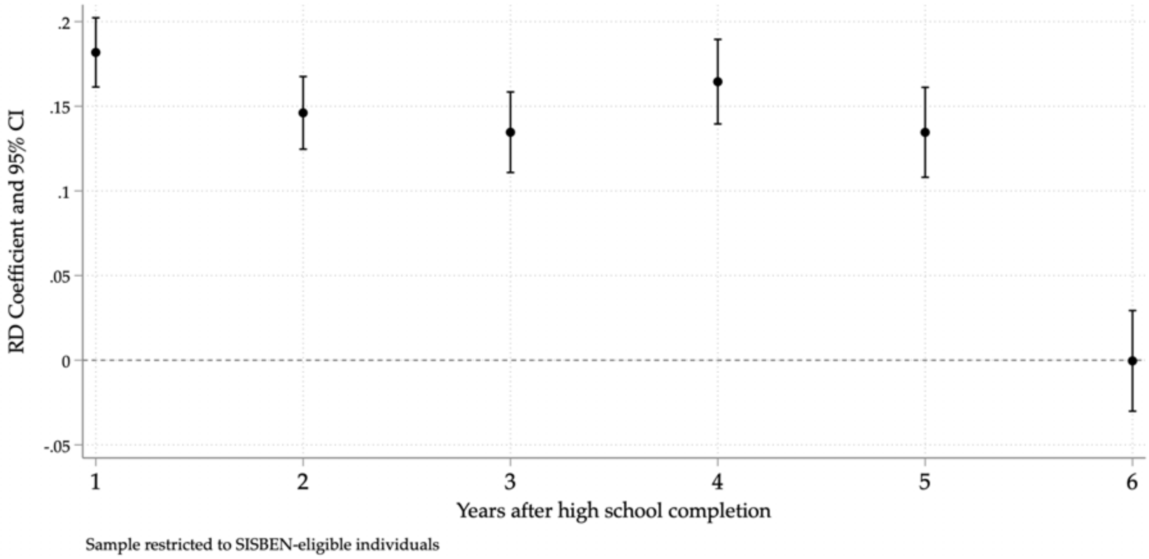
(b) Need Cutoff



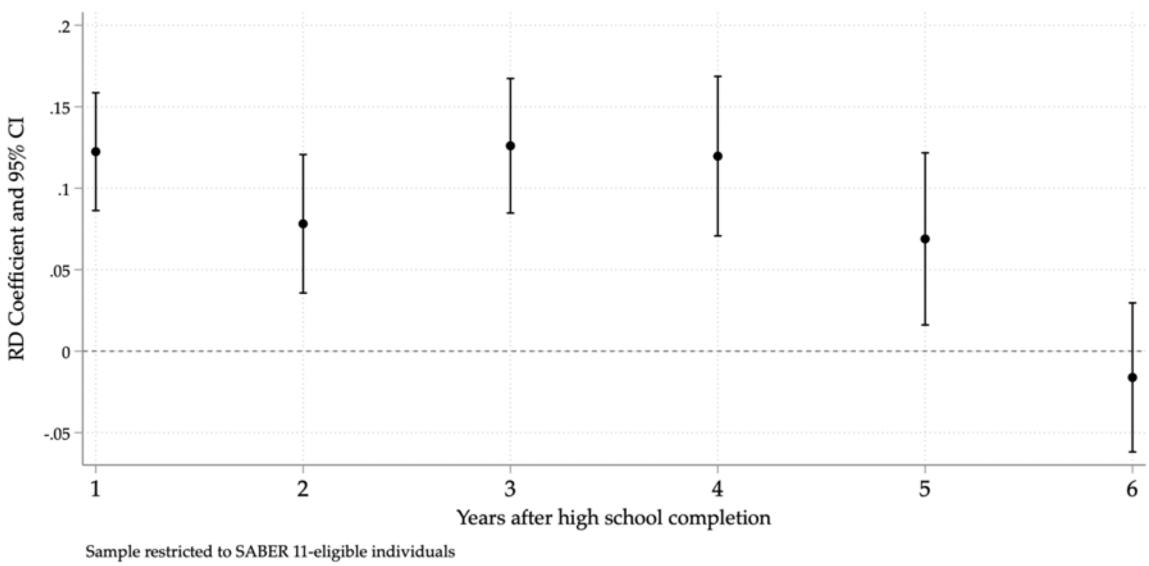
Notes: The figures decompose the enrollment effects over time by college quality and whether the institution is public or private. Panel A plots the RD coefficient based on the merit discontinuity (for need-eligible students), while Panel B plots the RD coefficient based on the need discontinuity (for 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.7: Persistence: Likelihood of Being Enrolled in College Over Time

(a) Merit Cutoff



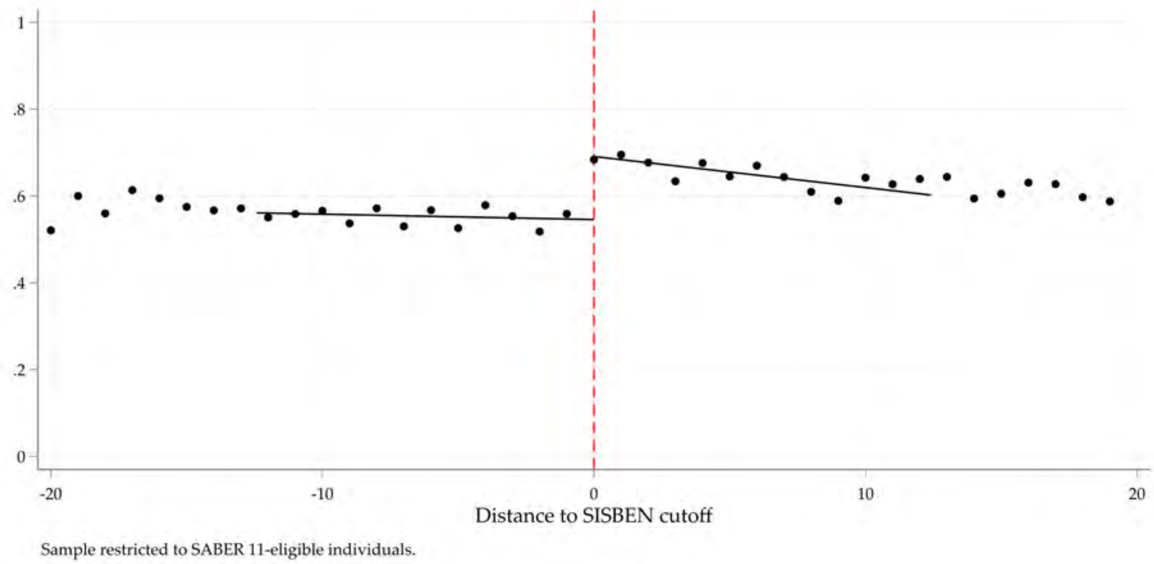
(b) Need Cutoff



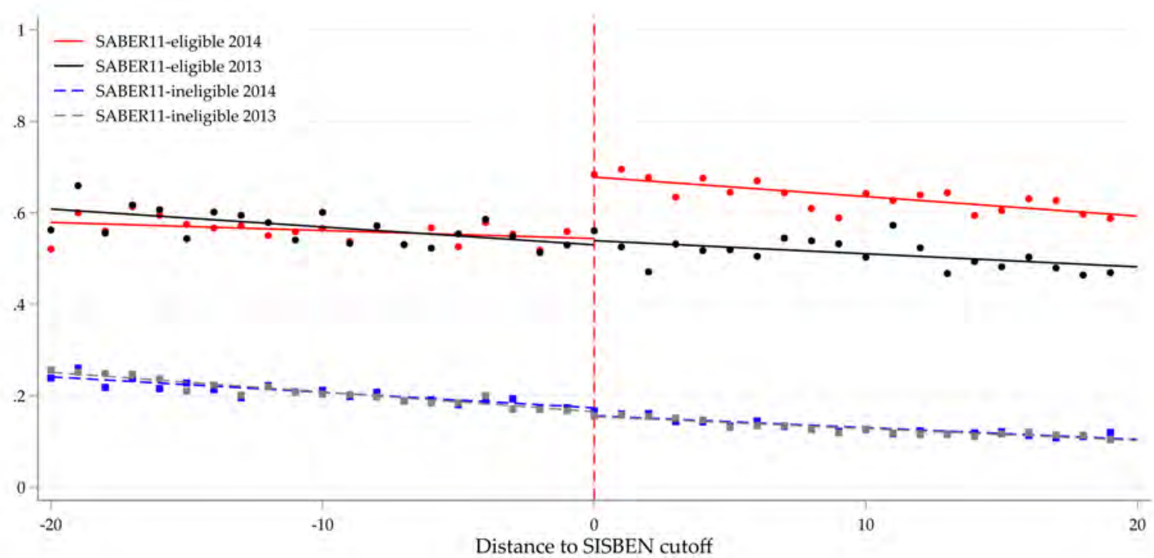
Notes: The figures plot the RD coefficient and 95% confidence intervals on the likelihood of being enrolled in college in a given year one to six years after high school completion. Panel A (B) uses SABER 11 (SISBEN) as the running variable and restricts the sample to need- (merit-) eligible students.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

Figure A.8: Bachelor's Degree Earned Within Seven Years from High School

(a) Need Cutoff



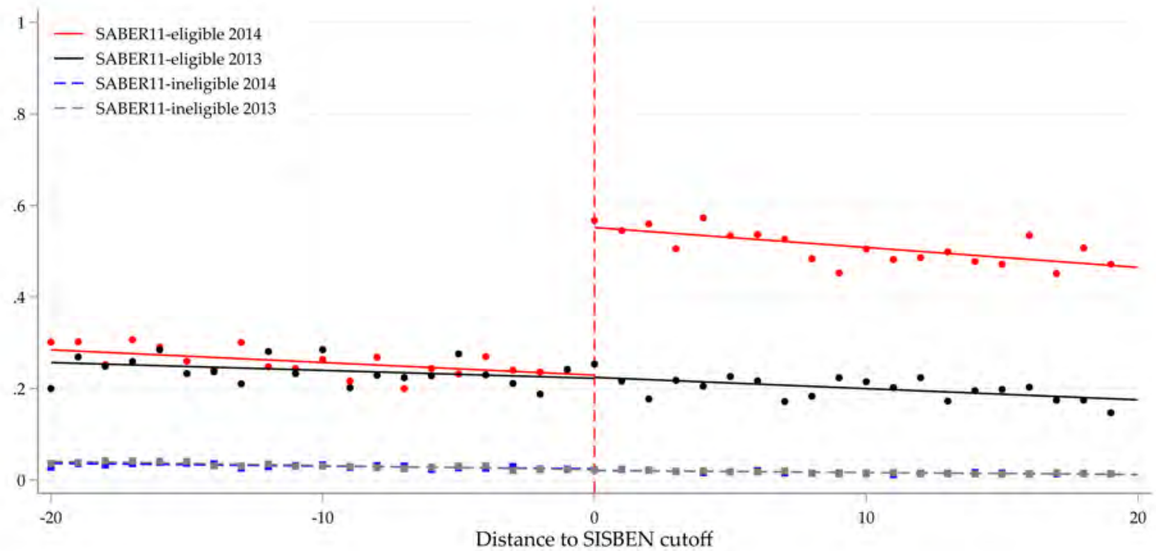
(b) Placebo



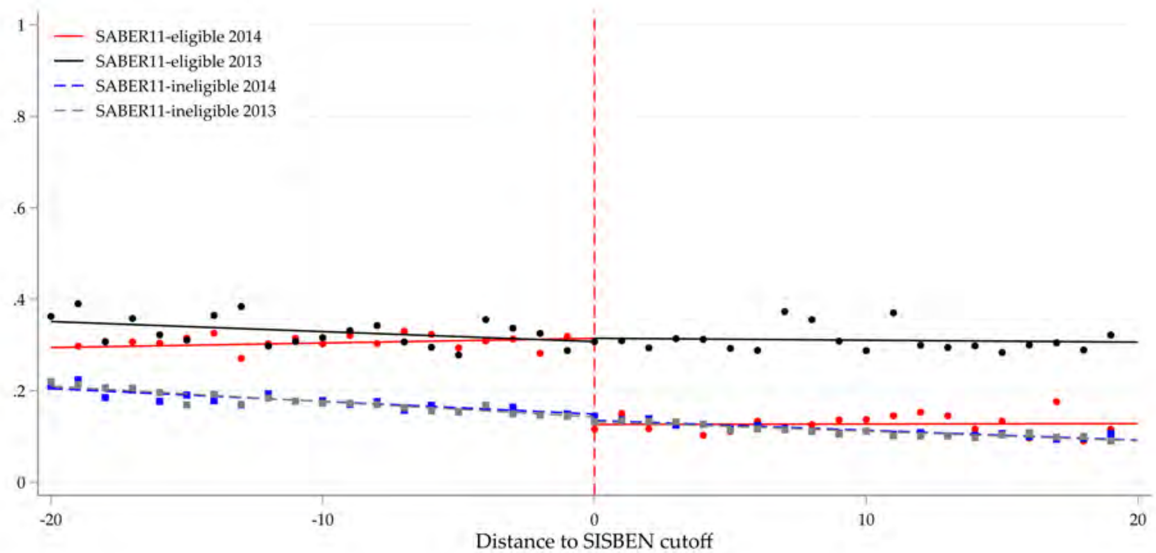
Notes: The figures plot the likelihood of earning a bachelor's degree (proxied by taking the SABER PRO exam) within seven years from high school completion as a function of the distance to the need cutoff. Panel A restricts the sample to merit-eligible students. Panel B compares that series (in red) with several placebo series: SABER 11-eligible students in 2013 (in black), SABER 11-ineligible students in 2013 (in gray), and SABER 11-ineligible students in 2014 (in blue). Table II reports the reduced-form estimate.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SABER PRO (ICFES).

Figure A.9: Bachelor's Degree Attainment by College Quality (Need Cutoff)

(a) High Quality



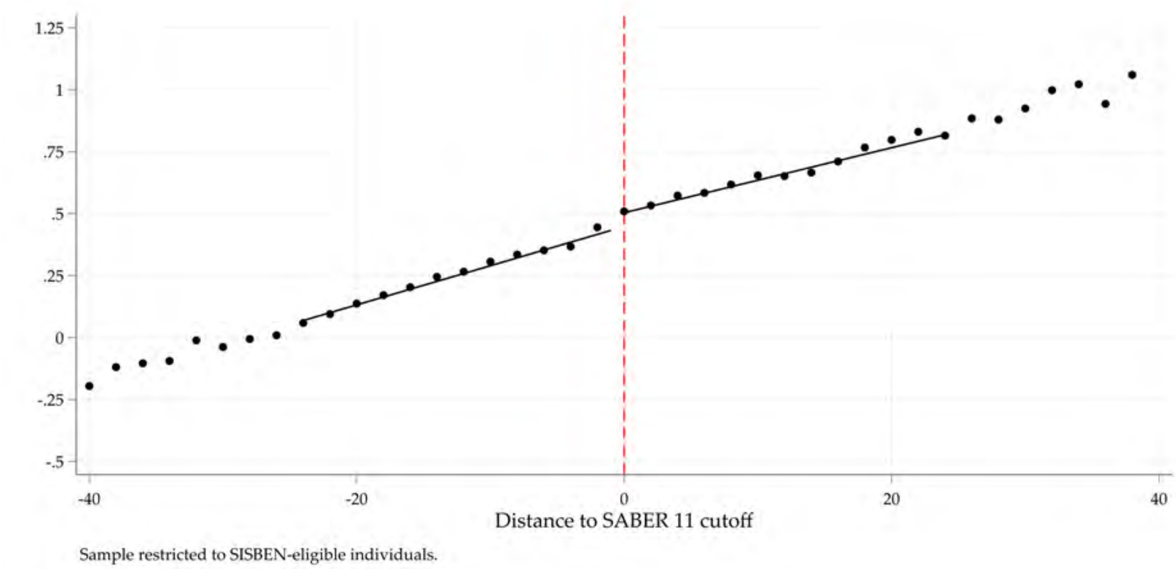
(b) Low Quality



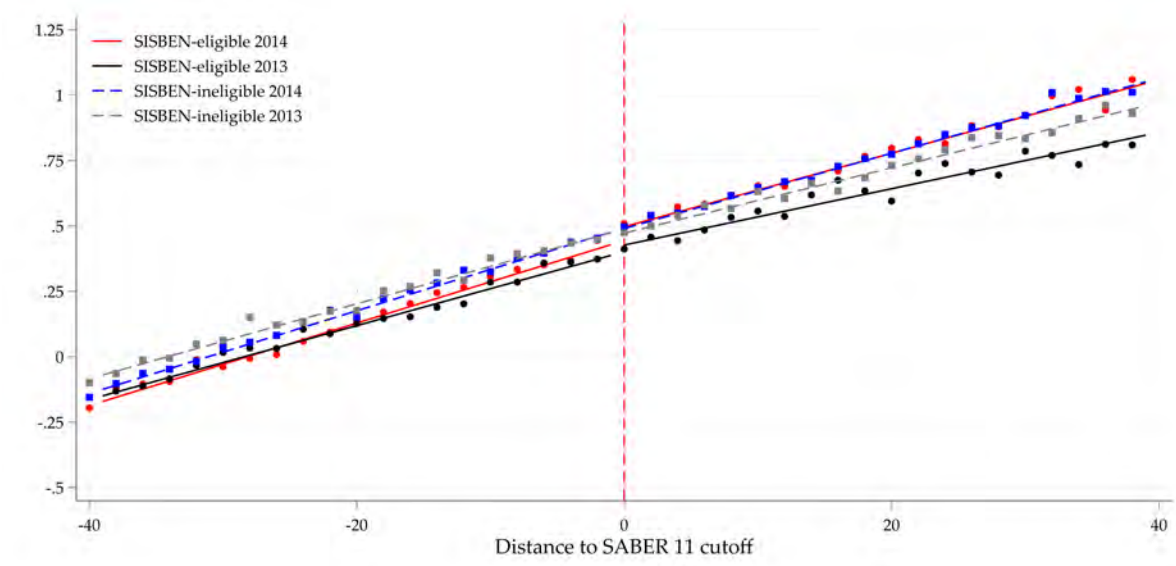
Notes: The figures decompose bachelor's degree attainment (proxied by taking the SABER PRO exam) by high- and low-quality colleges in Panels A and B, respectively. See the notes under Figure V for other details.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SABER PRO (ICFES).

Figure A.10: Standardized College Exit Test Score Within Seven Years from High School

(a) Merit Cutoff



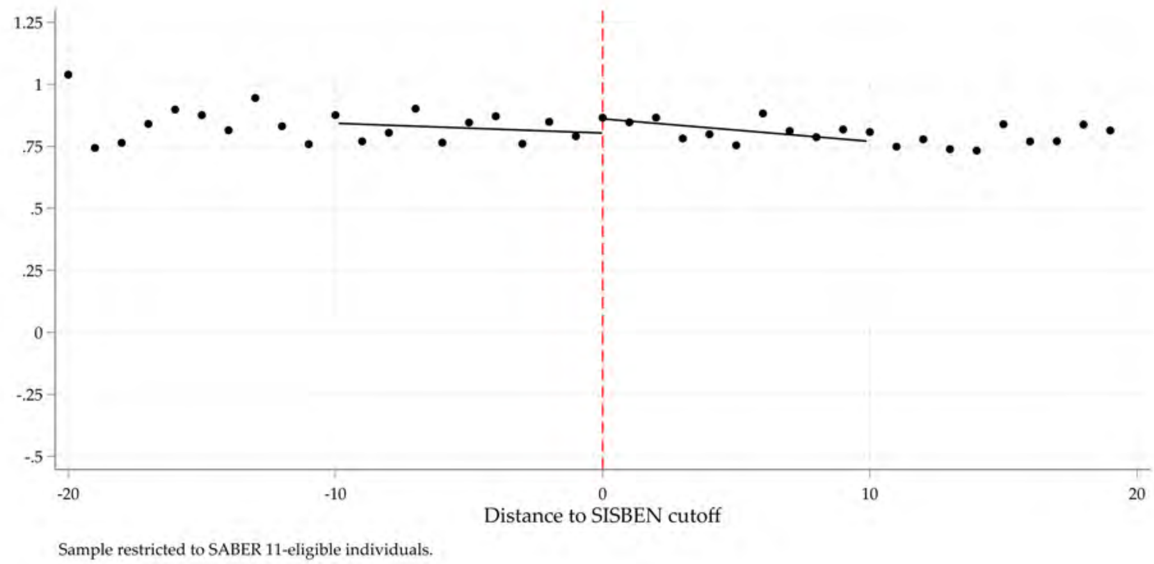
(b) Placebo



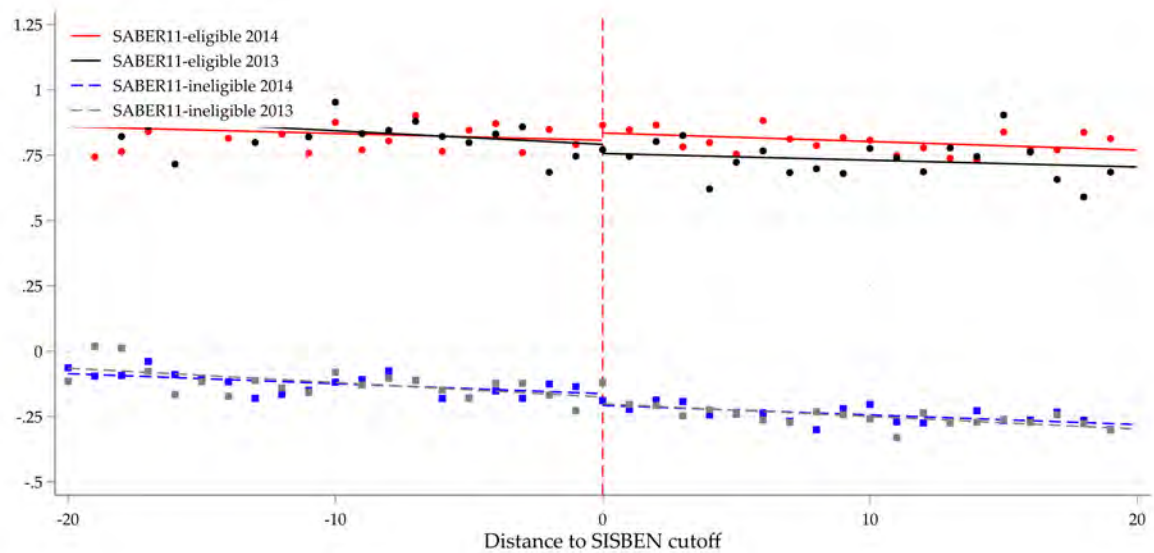
Notes: The figures plot students' performance in Colombia's mandatory standardized college exit exam, SABER PRO, within seven years from high school completion as a function of the distance to the merit cutoff. Panel A restricts the sample to need-eligible students. Panel B compares that series (in red)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

Figure A.11: Standardized College Exit Test Score Within Five Years from High School

(a) Need Cutoff



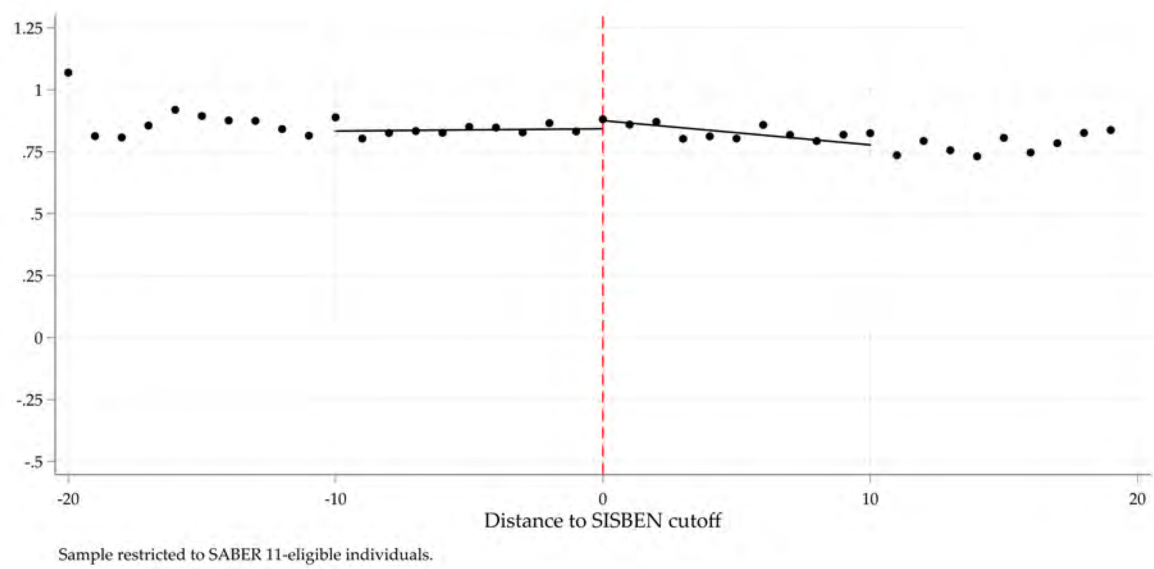
(b) Placebo



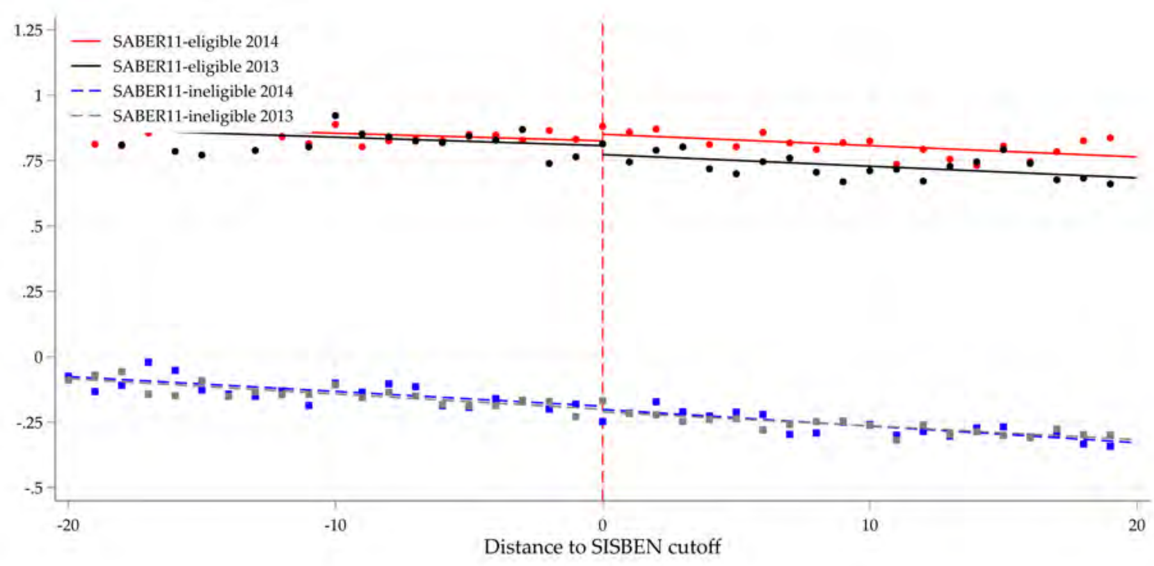
Notes: The figures plot students' performance in Colombia's mandatory standardized college exit exam, SABER PRO, within five years from high school completion as a function of the distance to the need cutoff. Panel A restricts the sample to merit-eligible students. Panel B compares that series (in red)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

Figure A.12: Standardized College Exit Test Score Within Seven Years from High School

(a) Need Cutoff



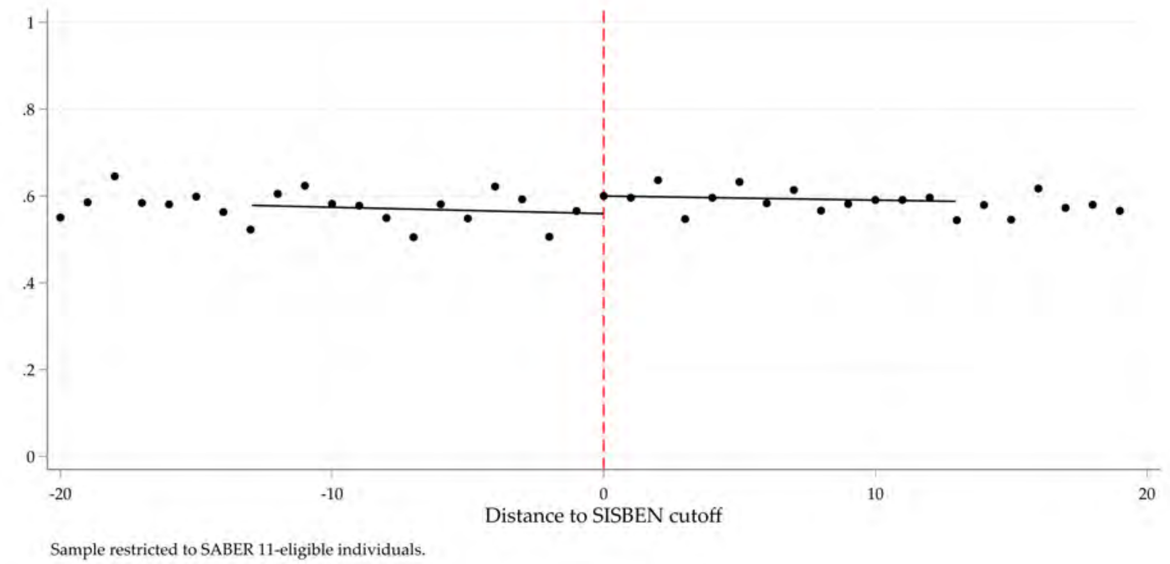
(b) Placebo



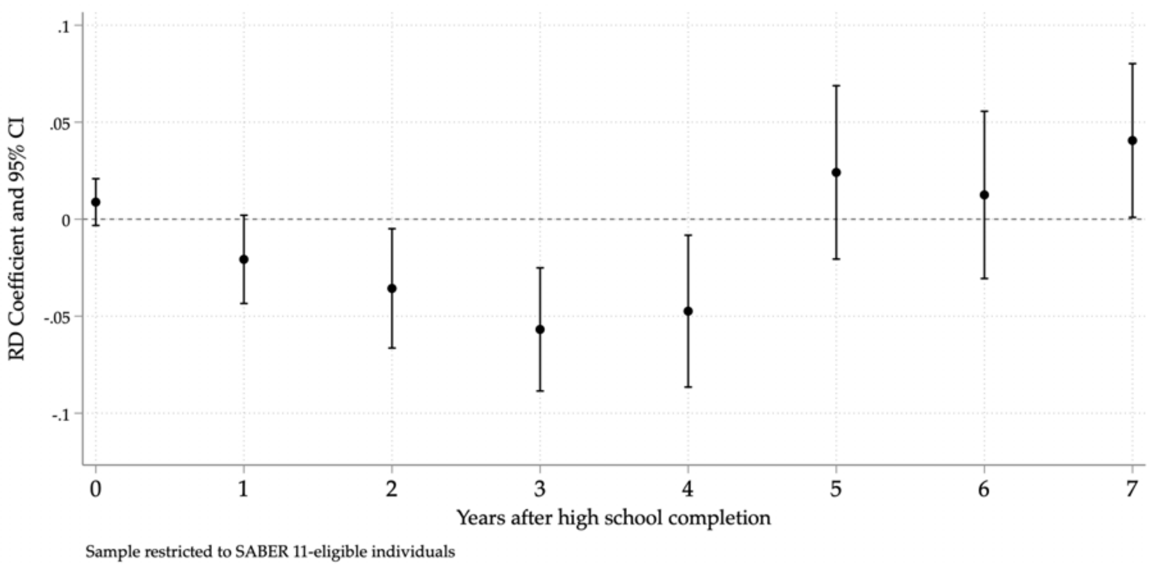
Notes: The figures plot students' performance in Colombia's mandatory standardized college exit exam, SABER PRO, within seven years from high school completion as a function of the distance to the need cutoff. Panel A restricts the sample to merit-eligible students. Panel B compares that series (in red)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

Figure A.13: Formal Employment (Need Cutoff)

(a) Seven Years after High School Completion



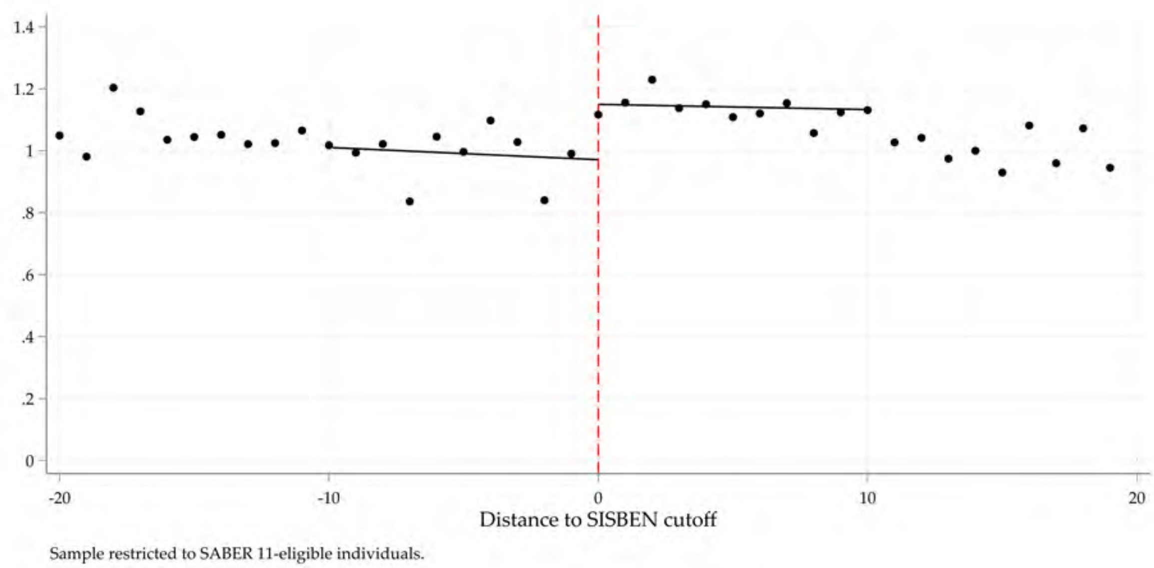
(b) Over Time



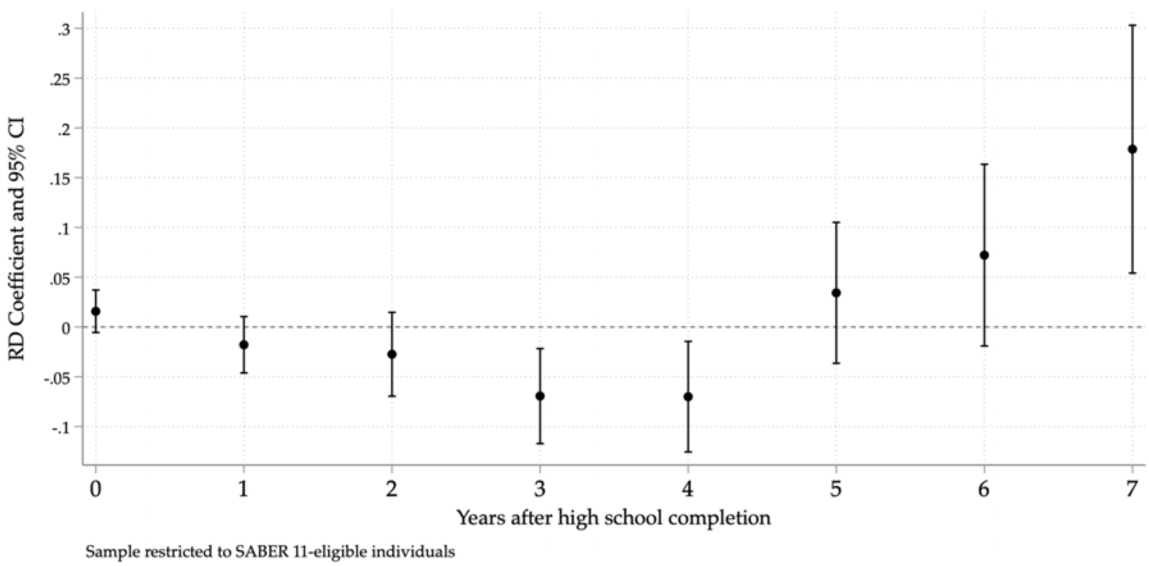
Notes: Panel A plots the probability of formal employment seven years after high school completion as a function of the distance to the need cutoff (for merit-eligible students). Panel B plots the RD coefficient over time. Table V reports the reduced-form estimates.
Sources: Authors' calculations based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

Figure A.14: Formal Earnings (Need Cutoff)

(a) Seven Years after High School Completion

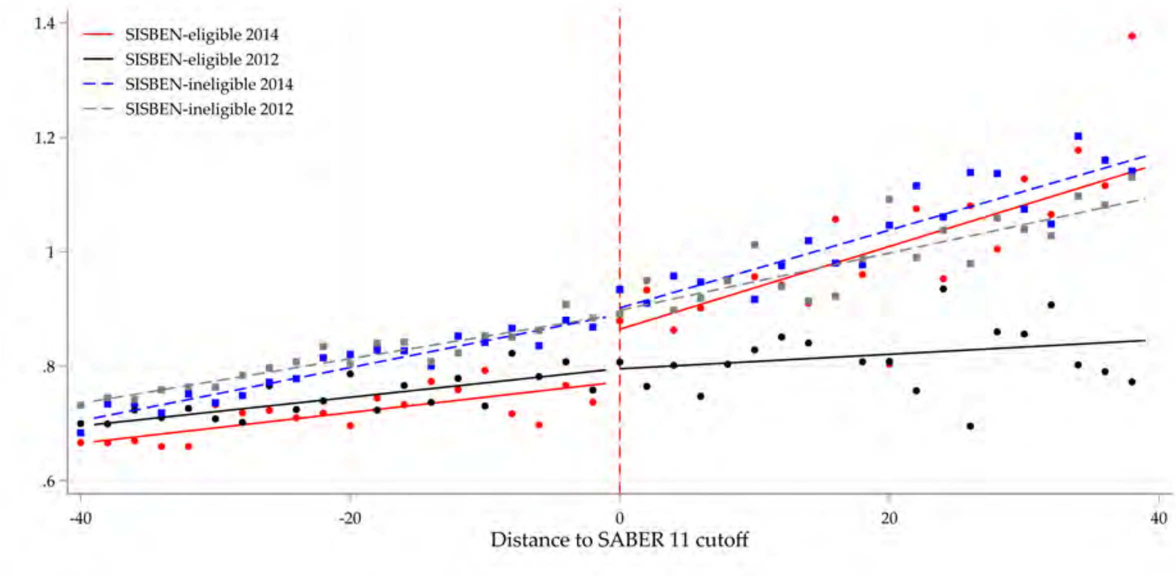


(b) Over Time



Notes: Panel A plots individuals' formal earnings (expressed as multiples of the monthly minimim wage) seven years after high school completion as a function of the distance to the need 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).

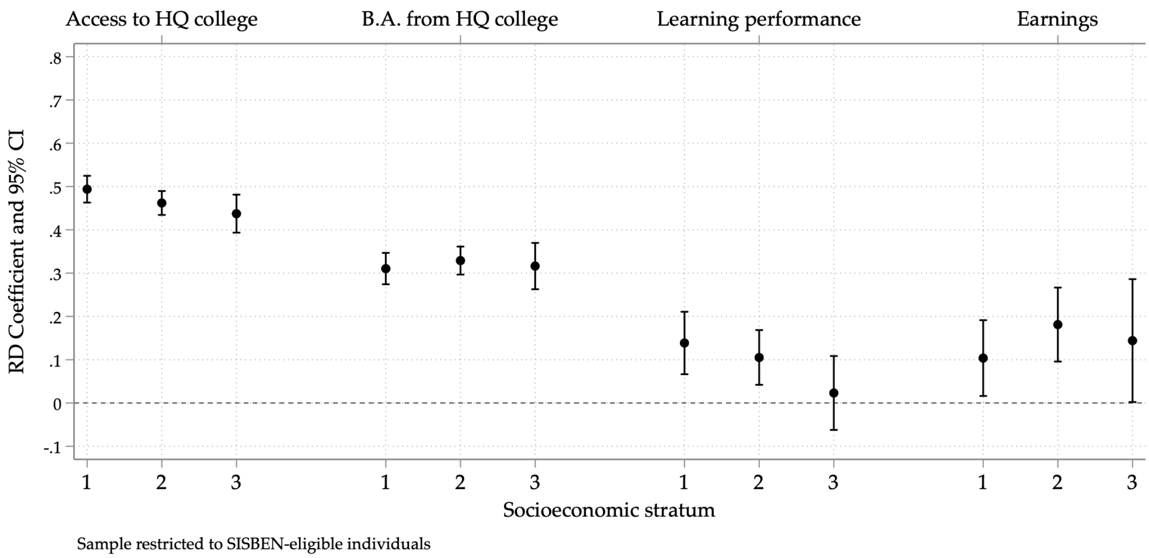
Figure A.15: Formal Earnings Using 2012 as the Comparison Group (Merit Cutoff)



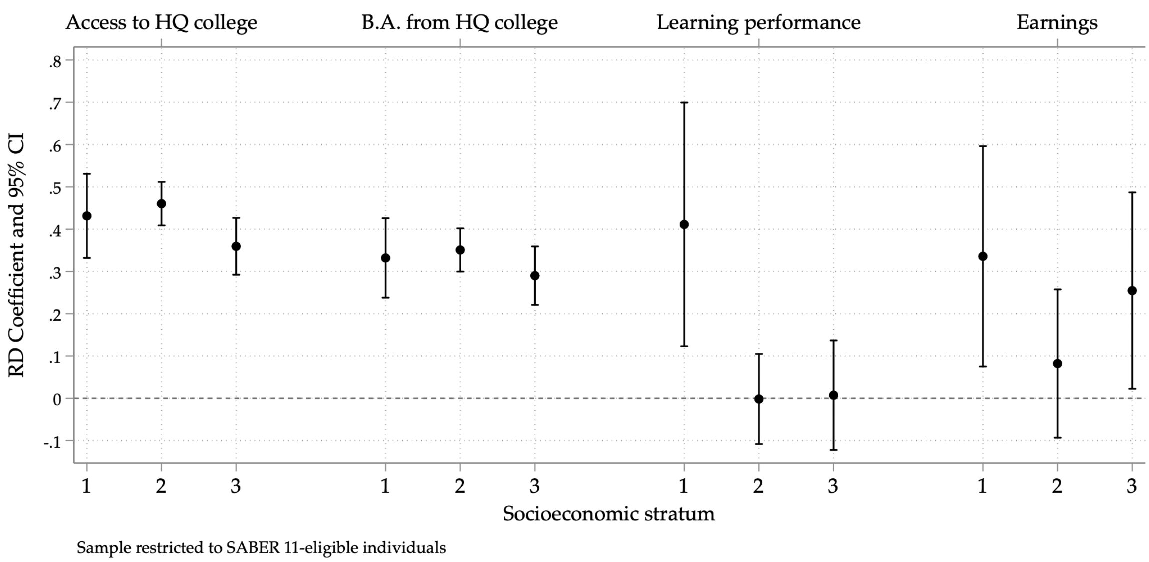
Notes: This figure plots individuals' formal earnings seven years after high school completion as a function of the distance to the merit cutoff. Earnings are expressed in multiples of the monthly minimim wage include zeros for individuals without formal employment. The figure shows the equity implications of expanding financial aid by comparing need-eligible students who took SABER 11 in 2014 (in red) and three placebo series: SISBEN-eligible and SISBEN-ineligible students from 2012 (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.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and PILA (MinSalud).

Figure A.16: Heterogeneity by Socioeconomic Stratum

(a) Merit Cutoff



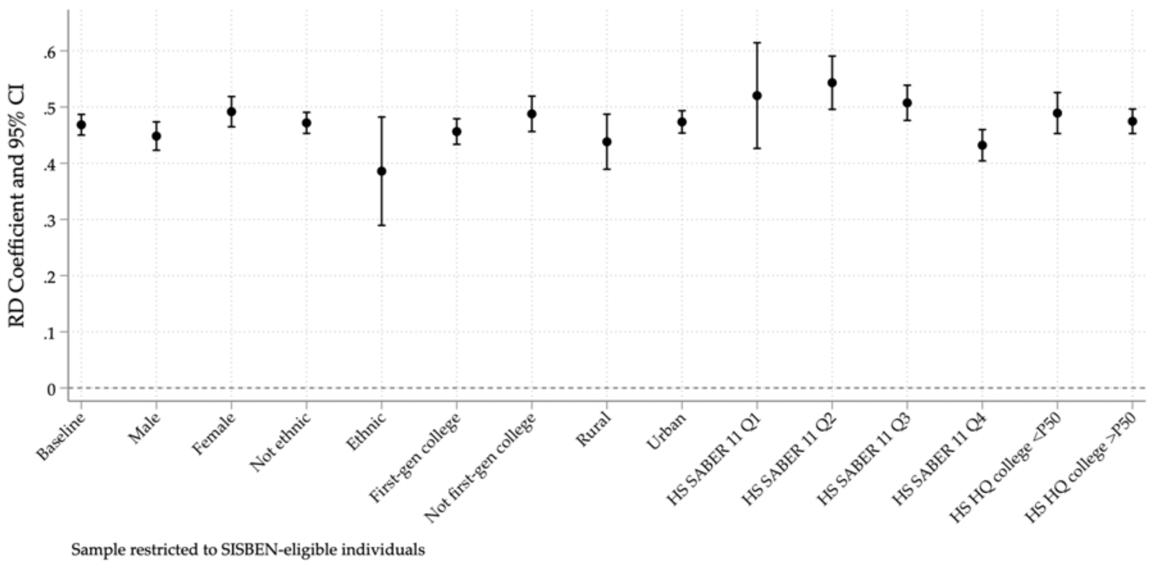
(b) Need Cutoff



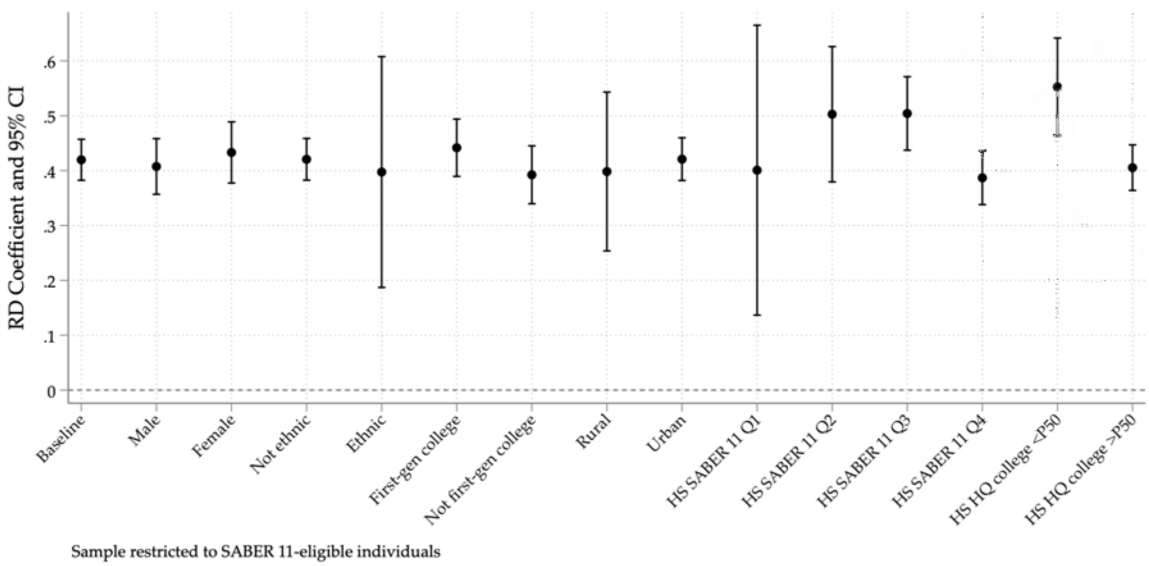
Notes: The figures compare the reduced-form RD coefficient and 95% confidence intervals across the distribution of socioeconomic strata. Panel A uses the SABER 11 test score as the running variable, restricting the sample to need-eligible students. Panel B uses the SISBEN wealth index as the running variable, restricting the sample to merit-eligible students.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), SABER PRO (ICFES), and PILA (MinSalud).

Figure A.17: Heterogeneous Effects in Immediate Access to a High-Quality College

(a) Merit Cutoff



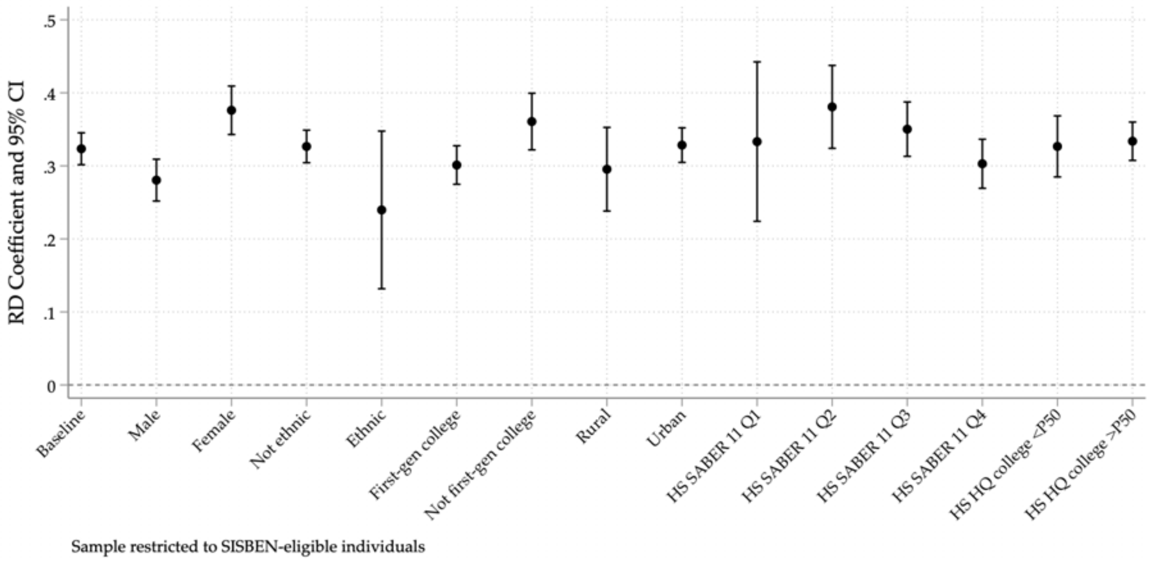
(b) Need Cutoff



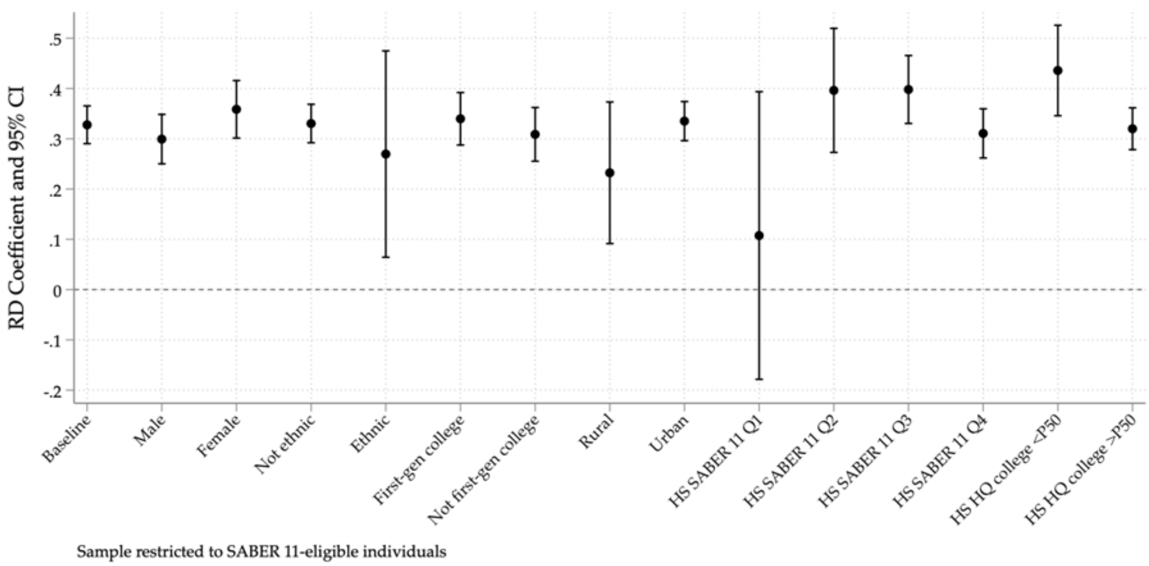
Notes: The figures plot the reduced-form RD coefficient and 95% confidence intervals on immediate access to a high-quality college after high school completion. Panel A uses the SABER 11 test score as the running variable, restricting the sample to need-eligible students. Panel B uses the SISBEN wealth 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 A.18: Heterogeneous Effects in Earning a B.A. from a High-Quality College

(a) Merit Cutoff



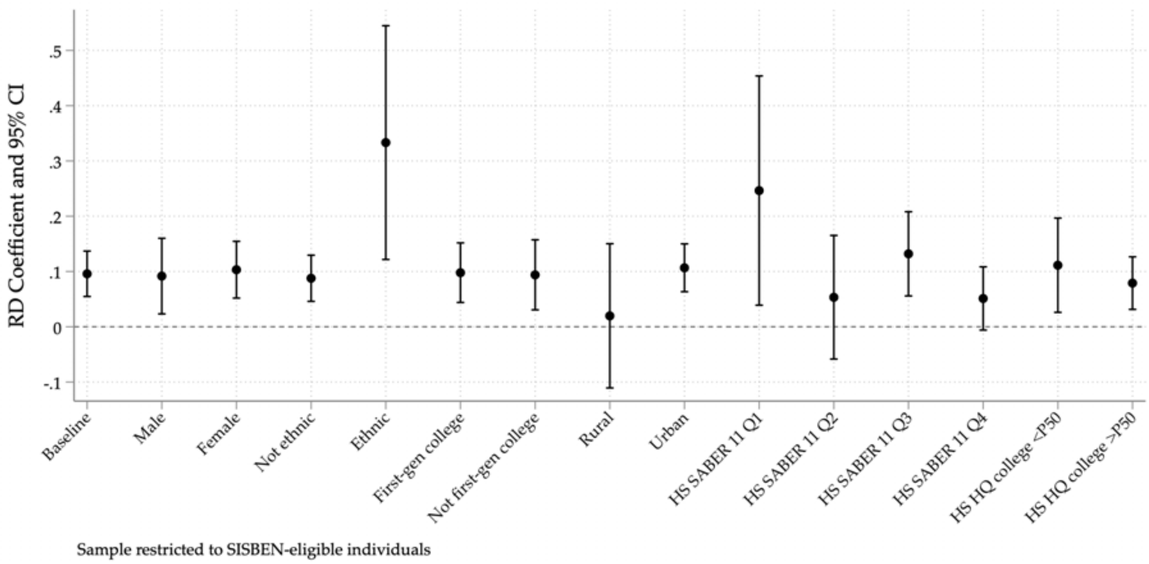
(b) Need Cutoff



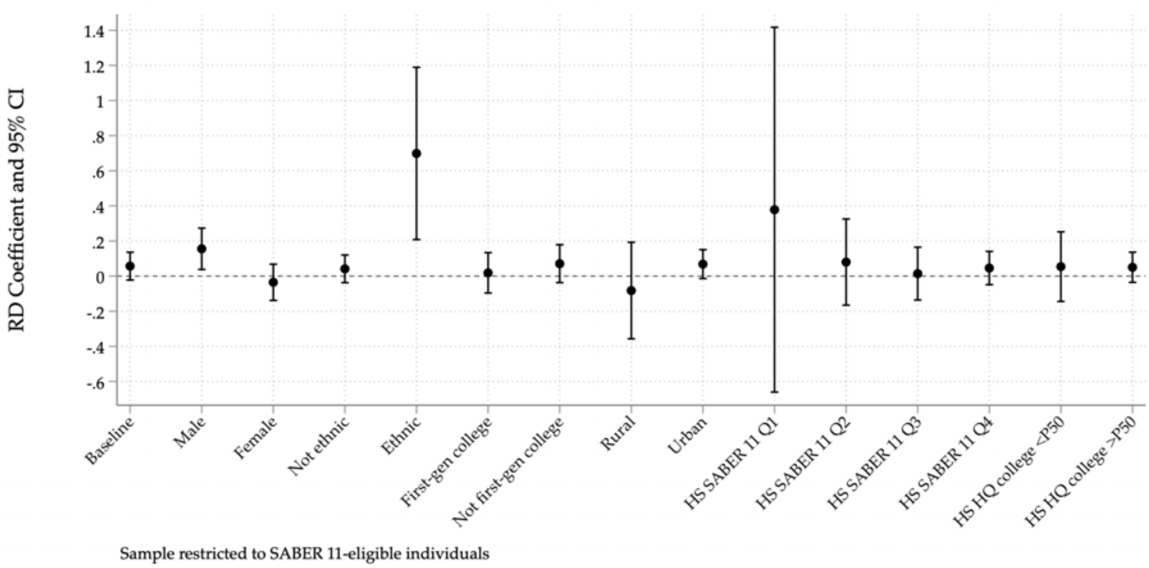
Notes: The figures plot the reduced-form RD coefficient and 95% confidence intervals on the likelihood of earning a bachelor's degree (proxied by taking the SABER PRO exam) from a high-quality college within seven years from high school completion. Panel A uses the SABER 11 test score as the running 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 A.19: Heterogeneous Effects in College Exit Test Scores

(a) Merit Cutoff



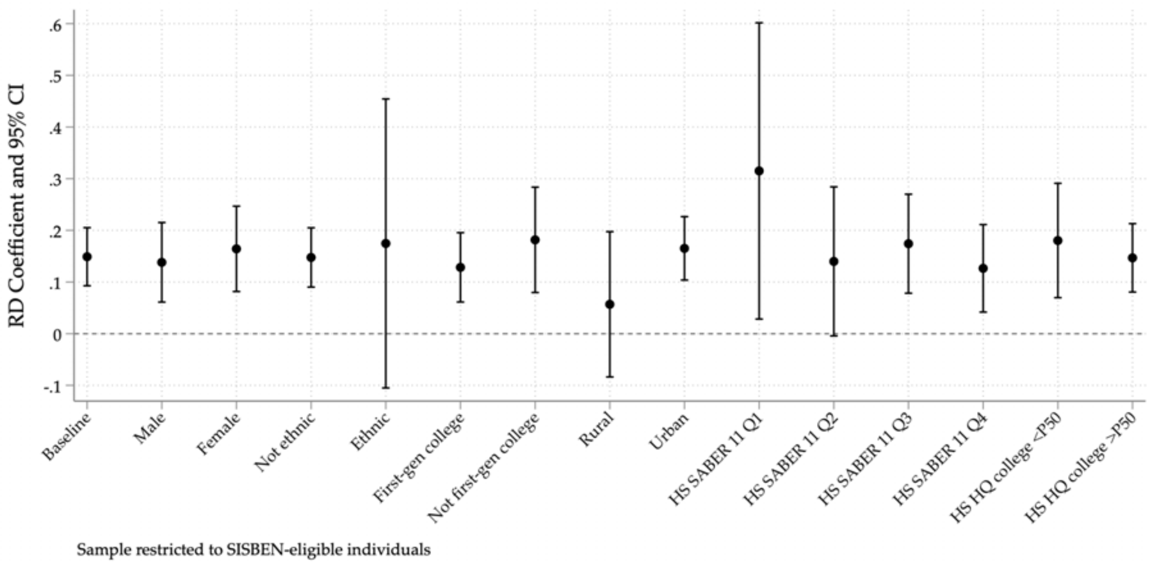
(b) Need Cutoff



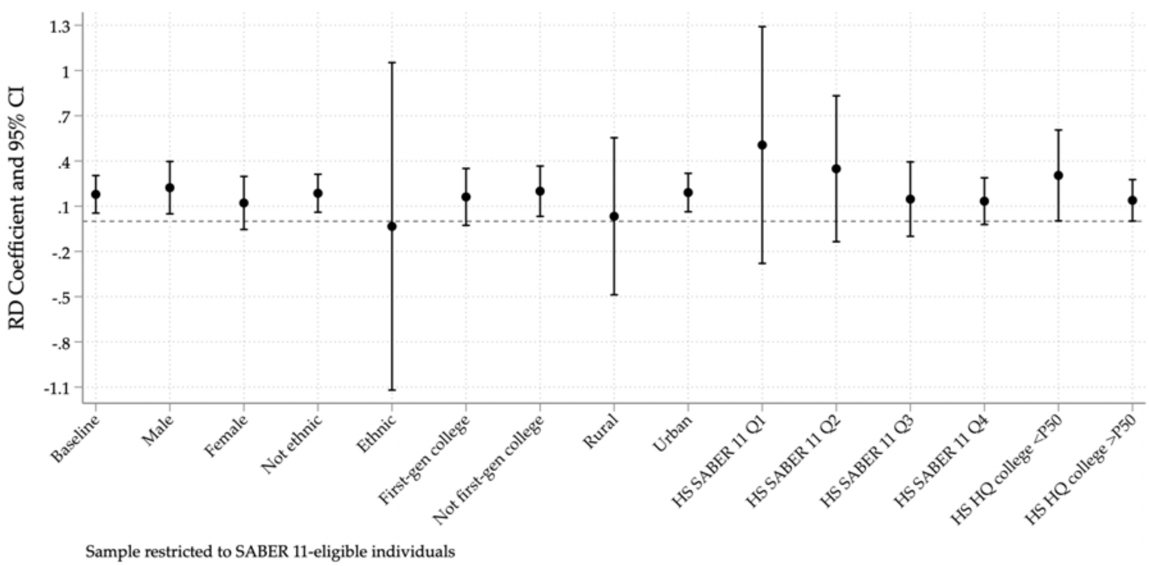
Notes: The figures plot the reduced-form RD coefficient and 95% confidence intervals on the standardized college exit test score for SABER PRO exams taken within five years from high school completion. Panel A uses the SABER 11 test score as the running variable, restricting the sample to need-eligible 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 A.20: Heterogeneous Effects in Formal Earnings

(a) Merit Cutoff



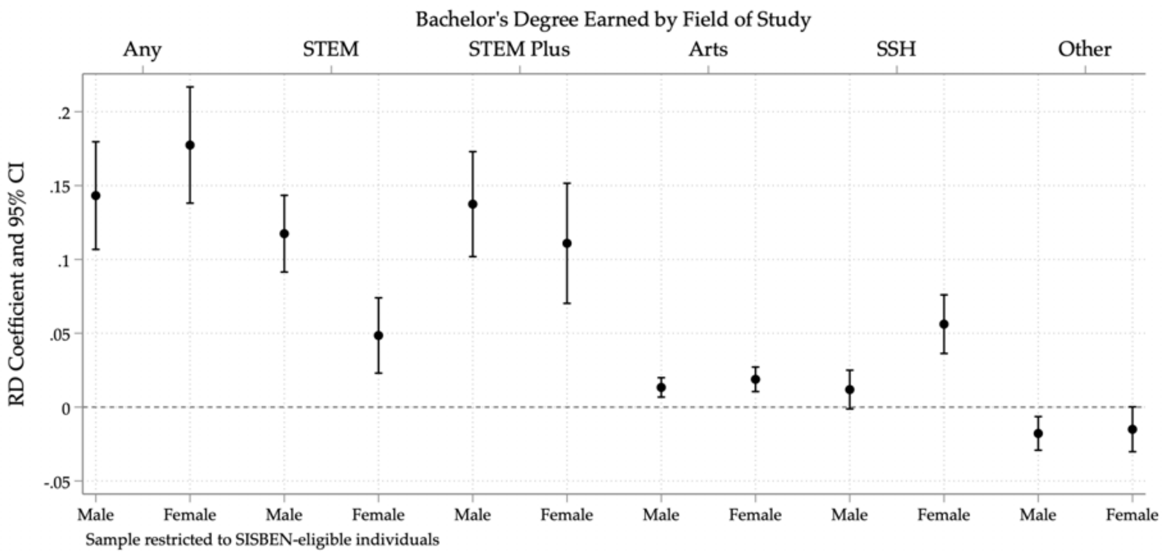
(b) Need Cutoff



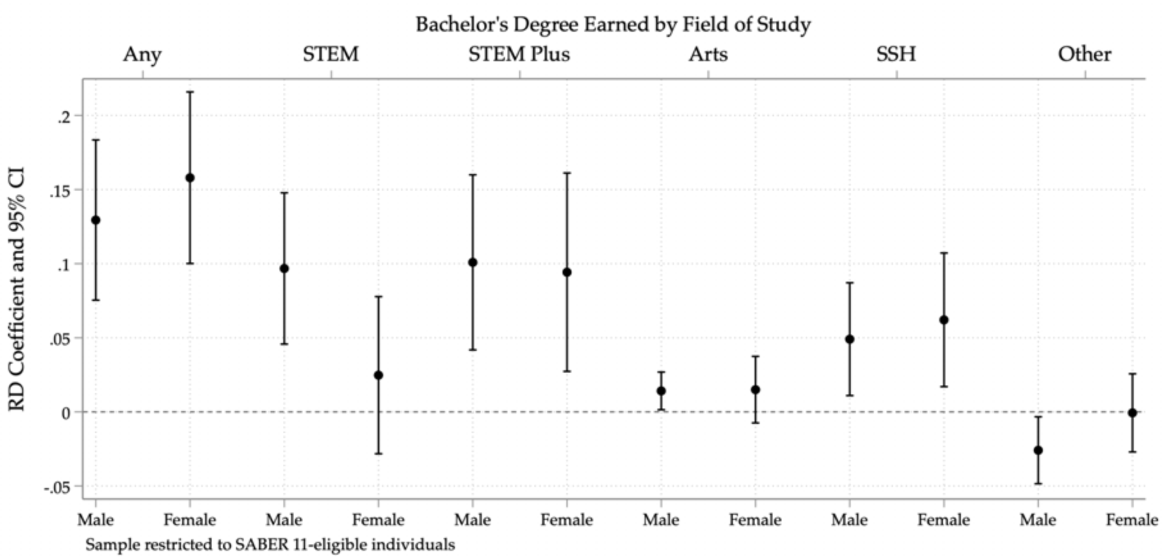
Notes: The figures plot the reduced-form RD coefficient and 95% confidence intervals on formal earnings seven years from high school completion. Earnings are expressed in multiples of the monthly minimum wage and include zeros for individuals without formal employment. Panel A uses the SABER 11 test 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 A.21: Heterogeneous Effects in Earning a B.A. by Gender and Field of Study

(a) Merit Cutoff



(b) Need 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).

Table A.1: 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.2: The Overall Impact of Financial Aid on College Access

	Enrollment within zero years from high school					Enrollment within six years from high school				
	Any college	High-quality college			Low-Quality	Any college	High-quality college			Low-Quality
		Any	Private	Public			Any	Private	Public	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
***Panel A:** Strata 1-2*										
Decile 9	0.178	0.052	0.016	0.036	0.126	0.288	0.103	0.027	0.076	0.185
	(0.003)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.003)
Decile 10	0.328	0.161	0.038	0.123	0.167	0.363	0.244	0.052	0.191	0.120
	(0.003)	(0.002)	(0.001)	(0.002)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)
Decile 9 x 2013	0.007	-0.001	0.000	-0.001	0.008	0.007	-0.003	-0.002	-0.001	0.010
	(0.004)	(0.002)	(0.001)	(0.002)	(0.003)	(0.004)	(0.003)	(0.001)	(0.002)	(0.004)
Decile 10 x 2013	-0.007	0.006	0.005	0.001	-0.013	0.017	0.010	0.002	0.008	0.007
	(0.005)	(0.003)	(0.002)	(0.003)	(0.004)	(0.004)	(0.004)	(0.002)	(0.004)	(0.004)
Decile 9 x 2014	0.015	-0.001	0.000	-0.001	0.016	0.010	-0.007	-0.003	-0.003	0.016
	(0.004)	(0.002)	(0.001)	(0.001)	(0.003)	(0.003)	(0.003)	(0.001)	(0.002)	(0.004)
Decile 10 x 2014	0.111	0.219	0.224	-0.005	-0.109	0.061	0.201	0.227	-0.026	-0.140
	(0.005)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)
N					1,283,122					
***Panel B:** Stratum 3*										
Decile 9	0.166	0.082	0.048	0.034	0.083	0.181	0.135	0.070	0.065	0.046
	(0.004)	(0.003)	(0.002)	(0.002)	(0.004)	(0.004)	(0.003)	(0.003)	(0.002)	(0.004)
Decile 10	0.298	0.237	0.099	0.137	0.062	0.227	0.322	0.128	0.194	-0.095
	(0.004)	(0.003)	(0.002)	(0.003)	(0.004)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)
Decile 9 x 2013	-0.015	-0.003	0.000	-0.003	-0.011	0.007	0.000	0.002	-0.003	0.008
	(0.006)	(0.004)	(0.003)	(0.002)	(0.006)	(0.005)	(0.005)	(0.004)	(0.003)	(0.006)
Decile 10 x 2013	-0.023	0.005	0.016	-0.011	-0.028	0.009	0.009	0.019	-0.010	0.000
	(0.006)	(0.005)	(0.004)	(0.004)	(0.005)	(0.004)	(0.005)	(0.004)	(0.004)	(0.006)
Decile 9 x 2014	-0.017	-0.007	-0.005	-0.002	-0.010	-0.001	-0.009	-0.006	-0.004	0.008
	(0.006)	(0.004)	(0.003)	(0.002)	(0.006)	(0.005)	(0.005)	(0.004)	(0.003)	(0.006)
Decile 10 x 2014	0.010	0.078	0.088	-0.010	-0.068	0.026	0.072	0.091	-0.019	-0.047
	(0.006)	(0.005)	(0.004)	(0.004)	(0.005)	(0.004)	(0.005)	(0.004)	(0.004)	(0.006)
N					297,993					
***Panel C:** Strata 4–6*										
Decile 9	0.131	0.125	0.103	0.022	0.006	0.113	0.189	0.156	0.034	-0.077
	(0.008)	(0.007)	(0.006)	(0.003)	(0.007)	(0.007)	(0.008)	(0.007)	(0.004)	(0.008)
Decile 10	0.246	0.322	0.225	0.097	-0.076	0.151	0.397	0.275	0.122	-0.246
	(0.006)	(0.005)	(0.005)	(0.003)	(0.005)	(0.005)	(0.006)	(0.006)	(0.003)	(0.006)
Decile 9 x 2013	-0.042	-0.011	-0.004	-0.007	-0.031	-0.014	-0.003	0.001	-0.004	-0.010
	(0.012)	(0.009)	(0.009)	(0.003)	(0.010)	(0.009)	(0.011)	(0.010)	(0.005)	(0.012)
Decile 10 x 2013	-0.025	-0.007	-0.006	-0.001	-0.018	-0.006	0.000	-0.004	0.004	-0.006
	(0.009)	(0.008)	(0.007)	(0.004)	(0.008)	(0.007)	(0.008)	(0.008)	(0.005)	(0.009)
Decile 9 x 2014	-0.036	-0.018	-0.019	0.001	-0.018	-0.003	-0.013	-0.023	0.009	0.010
	(0.012)	(0.009)	(0.009)	(0.004)	(0.010)	(0.010)	(0.011)	(0.010)	(0.005)	(0.012)
Decile 10 x 2014	-0.016	0.005	0.004	0.001	-0.022	0.007	0.012	0.006	0.006	-0.005
	(0.009)	(0.008)	(0.007)	(0.004)	(0.008)	(0.007)	(0.008)	(0.008)	(0.005)	(0.009)
N					83,742					

Notes: This table presents the overall effects of financial aid on college access using Specification (2). Robust standard errors are in parentheses.
Sources: Authors' calculations based on SABER 11 (ICFES) and SNIES (MEN).

Table A.3: The Overall Impact of Financial Aid on College Attainment and Learning

	Degree attainment within seven years from high school completion							College exit test score if exam taken within	
	Any	Two-year	Four-year degree						
			Any	High-quality college			Low-quality	Five	Seven
	Degree	degree	college	Any	Private	Public	college	years	years
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
***Panel A:** Strata 1-2*									
Decile 9	0.281	0.049	0.232	0.059	0.019	0.040	0.173	0.618	0.623
	(0.003)	(0.002)	(0.003)	(0.001)	(0.001)	(0.001)	(0.002)	(0.008)	(0.006)
Decile 10	0.383	-0.029	0.412	0.170	0.044	0.126	0.242	1.138	1.128
	(0.003)	(0.002)	(0.003)	(0.002)	(0.001)	(0.002)	(0.003)	(0.008)	(0.006)
Decile 9 x 2013	-0.015	-0.008	-0.007	-0.008	-0.001	-0.006	0.001	-0.003	0.014
	(0.004)	(0.003)	(0.004)	(0.002)	(0.001)	(0.002)	(0.003)	(0.011)	(0.008)
Decile 10 x 2013	-0.016	-0.008	-0.008	0.000	0.001	-0.001	-0.008	-0.018	-0.004
	(0.004)	(0.003)	(0.005)	(0.003)	(0.002)	(0.003)	(0.004)	(0.012)	(0.009)
Decile 9 x 2014	-0.017	-0.006	-0.011	-0.009	-0.002	-0.007	-0.003	0.005	0.035
	(0.004)	(0.003)	(0.004)	(0.002)	(0.001)	(0.001)	(0.003)	(0.011)	(0.008)
Decile 10 x 2014	-0.006	-0.053	0.047	0.148	0.170	-0.021	-0.102	0.038	0.062
	(0.004)	(0.003)	(0.005)	(0.004)	(0.003)	(0.003)	(0.004)	(0.011)	(0.008)
N				1,283,122				93,515	178,525
***Panel B:** Strata 3*									
Decile 9	0.233	-0.007	0.241	0.093	0.056	0.037	0.148	0.554	0.568
	(0.004)	(0.003)	(0.004)	(0.003)	(0.002)	(0.002)	(0.004)	(0.010)	(0.008)
Decile 10	0.324	-0.084	0.408	0.257	0.118	0.140	0.151	1.139	1.131
	(0.004)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.010)	(0.007)
Decile 9 x 2013	-0.022	-0.008	-0.014	-0.004	0.001	-0.005	-0.010	-0.031	-0.025
	(0.006)	(0.005)	(0.006)	(0.004)	(0.003)	(0.002)	(0.006)	(0.015)	(0.011)
Decile 10 x 2013	-0.026	-0.004	-0.022	-0.003	0.011	-0.014	-0.019	-0.030	-0.016
	(0.005)	(0.004)	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)	(0.014)	(0.010)
Decile 9 x 2014	-0.034	-0.006	-0.028	-0.012	-0.005	-0.007	-0.016	0.006	0.025
	(0.006)	(0.005)	(0.006)	(0.004)	(0.003)	(0.002)	(0.006)	(0.014)	(0.011)
Decile 10 x 2014	-0.034	-0.020	-0.014	0.049	0.069	-0.019	-0.064	0.011	0.054
	(0.005)	(0.003)	(0.005)	(0.005)	(0.004)	(0.003)	(0.005)	(0.013)	(0.010)
N				297,993				55,065	97,942
***Panel C:** Strata 4-6*									
Decile 9	0.193	-0.036	0.229	0.162	0.140	0.022	0.067	0.573	0.549
	(0.008)	(0.004)	(0.008)	(0.007)	(0.007)	(0.002)	(0.008)	(0.018)	(0.013)
Decile 10	0.295	-0.068	0.364	0.379	0.275	0.104	-0.015	1.208	1.187
	(0.006)	(0.003)	(0.006)	(0.005)	(0.005)	(0.003)	(0.006)	(0.015)	(0.011)
Decile 9 x 2013	-0.004	0.002	-0.006	0.001	0.002	0.000	-0.008	-0.053	-0.018
	(0.012)	(0.006)	(0.012)	(0.010)	(0.010)	(0.003)	(0.011)	(0.025)	(0.019)
Decile 10 x 2013	-0.025	-0.003	-0.022	-0.006	-0.002	-0.004	-0.016	-0.022	-0.016
	(0.009)	(0.004)	(0.009)	(0.008)	(0.007)	(0.004)	(0.008)	(0.021)	(0.016)
Decile 9 x 2014	-0.022	-0.004	-0.018	-0.017	-0.020	0.003	-0.001	-0.060	0.002
	(0.012)	(0.006)	(0.012)	(0.010)	(0.009)	(0.003)	(0.011)	(0.025)	(0.019)
Decile 10 x 2014	-0.012	0.002	-0.014	-0.001	0.003	-0.004	-0.013	-0.016	0.045
	(0.009)	(0.004)	(0.009)	(0.008)	(0.007)	(0.004)	(0.008)	(0.021)	(0.016)
N				83,742				27,388	47,128

Notes: This table presents the overall effects of financial aid on college attainment and learning performance using Specification (2). Robust standard errors are in parentheses.
Sources: Authors' calculations based on SABER 11 (ICFES) and SABER PRO (ICFES).

Table A.4: The Overall Impact of Financial Aid on Formal Labor Market Outcomes

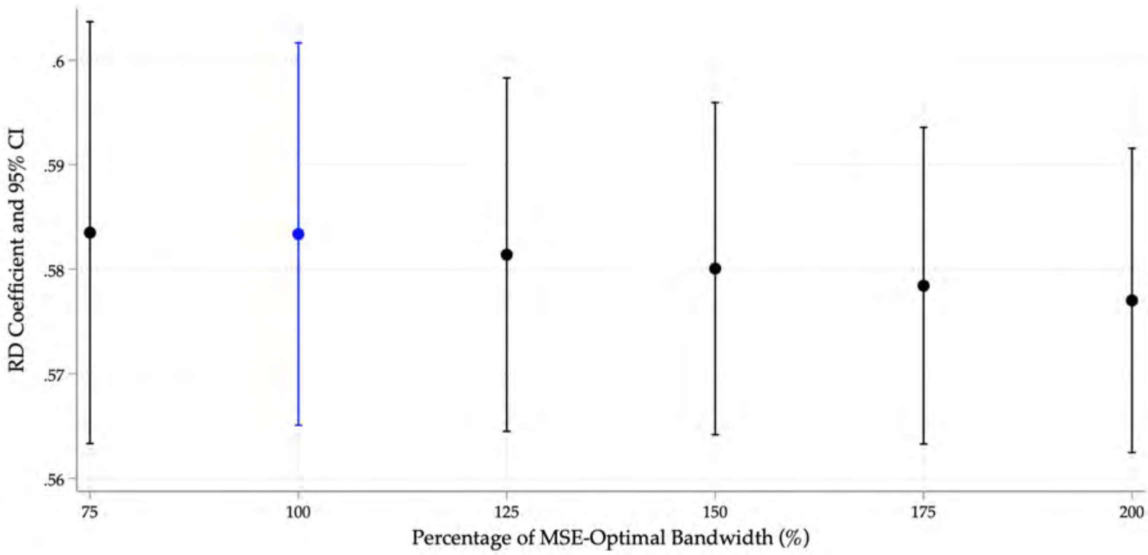
	Formal work	Formal earnings (includes zeros)		
		in constant pesos	in monthly min. wages	in natural logarithm
	(1)	(2)	(3)	(4)
Panel A: *Strata 1-2*				
Decile 9	0.056	126,063.602	0.179	0.138
	(0.003)	(3,609.558)	(0.005)	(0.005)
Decile 10	0.055	203,863.297	0.287	0.240
	(0.003)	(5,219.790)	(0.007)	(0.006)
Decile 9 x 2013	-0.004	-11,653.798	-0.032	-0.025
	(0.004)	(5,261.189)	(0.007)	(0.007)
Decile 10 x 2013	-0.014	-14,535.699	-0.044	-0.011
	(0.005)	(7,606.616)	(0.010)	(0.009)
Decile 9 x 2014	0.006	45,897.039	0.012	0.003
	(0.004)	(5,745.590)	(0.007)	(0.007)
Decile 10 x 2014	0.026	140,952.094	0.098	0.046
	(0.005)	(8,814.028)	(0.011)	(0.009)
N		1,283,122		558,476
Panel B: *Stratum 3*				
Decile 9	0.031	120,537.656	0.168	0.123
	(0.004)	(6,365.231)	(0.009)	(0.008)
Decile 10	0.033	250,292.531	0.348	0.289
	(0.004)	(7,201.961)	(0.010)	(0.008)
Decile 9 x 2013	-0.007	531.975	-0.013	0.019
	(0.006)	(9,510.604)	(0.013)	(0.012)
Decile 10 x 2013	-0.002	8,400.750	-0.017	0.007
	(0.006)	(10,618.742)	(0.014)	(0.012)
Decile 9 x 2014	-0.002	32,089.725	0.004	0.015
	(0.006)	(10,130.944)	(0.013)	(0.012)
Decile 10 x 2014	0.011	116,954.414	0.067	0.047
	(0.006)	(11,765.030)	(0.015)	(0.011)
N		297,993		157,738
Panel C: *Strata 4-6*				
Decile 9	0.053	176,297.484	0.247	0.189
	(0.008)	(14,866.575)	(0.021)	(0.020)
Decile 10	0.101	450,058.125	0.629	0.439
	(0.007)	(13,118.759)	(0.018)	(0.015)
Decile 9 x 2013	-0.013	-31,109.395	-0.061	0.006
	(0.012)	(21,082.564)	(0.028)	(0.029)
Decile 10 x 2013	-0.004	-26,874.051	-0.088	-0.001
	(0.009)	(18,618.025)	(0.025)	(0.022)
Decile 9 x 2014	-0.007	34,172.875	-0.010	0.045
	(0.012)	(22,929.004)	(0.029)	(0.028)
Decile 10 x 2014	0.008	117,942.859	0.011	0.041
	(0.009)	(20,432.920)	(0.025)	(0.022)
N		83,742		43,951

Notes: This table presents the overall effects of financial aid on formal labor market outcomes using Specification (2). Robust standard errors are in parentheses.
Sources: Authors' calculations based on SABER 11 (ICFES) and PILA (MinSalud).

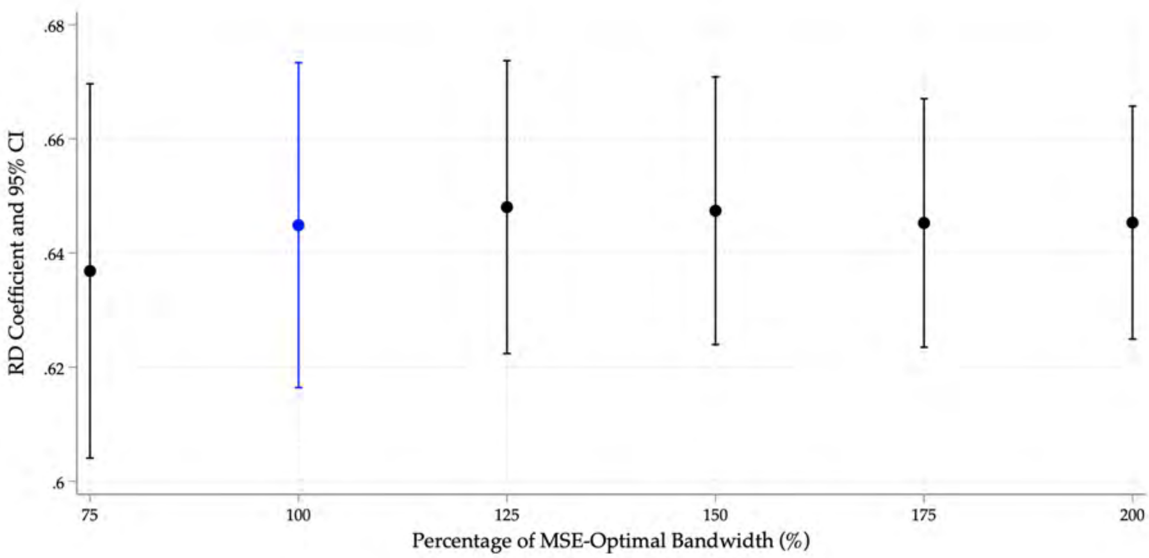
Appendix B Robustness to RD Bandwidth Selection

Figure B.1: Probability of Receiving SPP Financial Aid

(a) Merit Cutoff



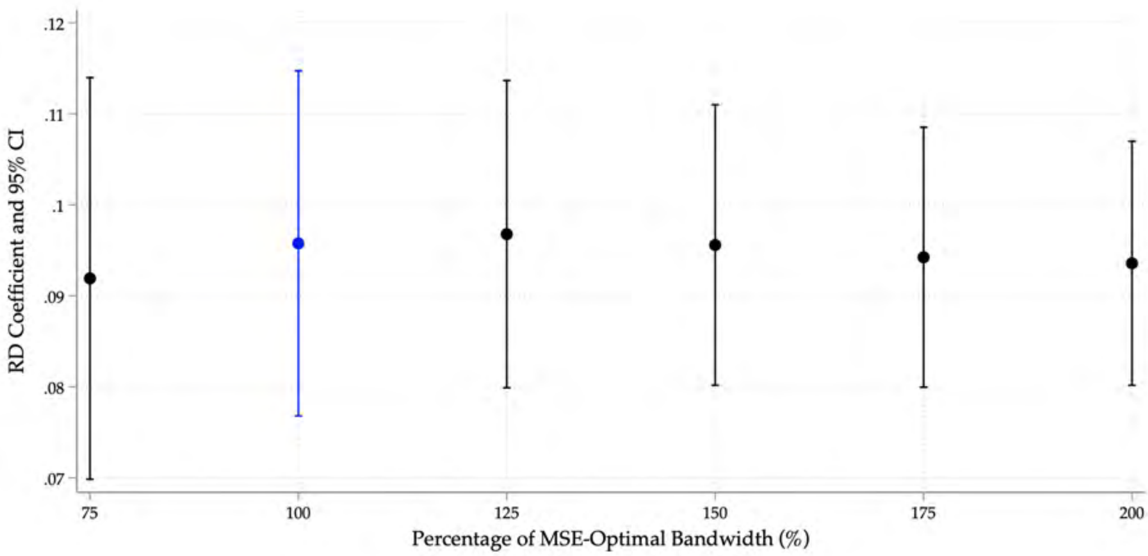
(b) Need Cutoff



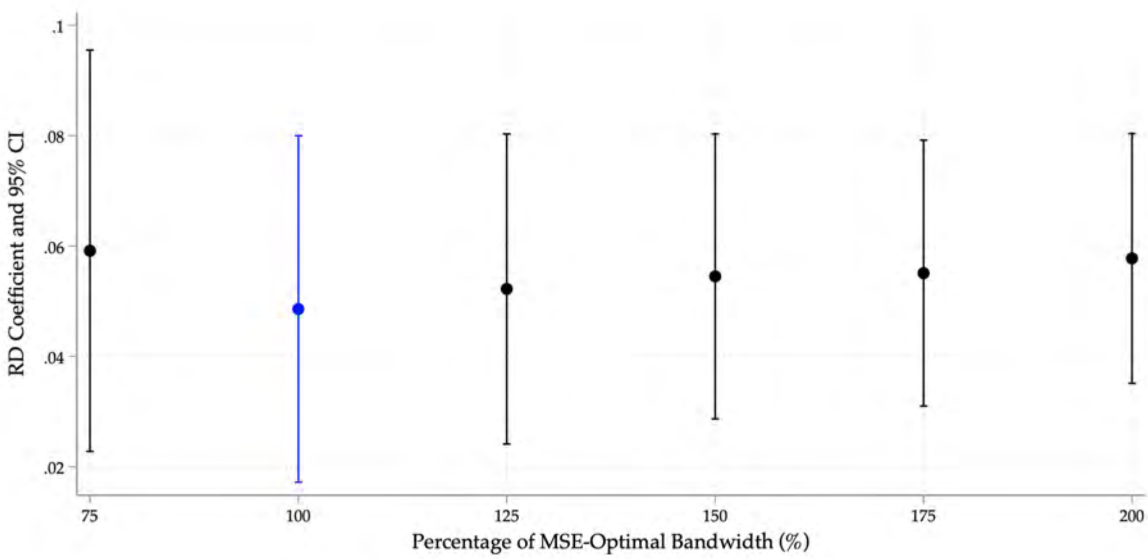
Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test 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) Merit Cutoff



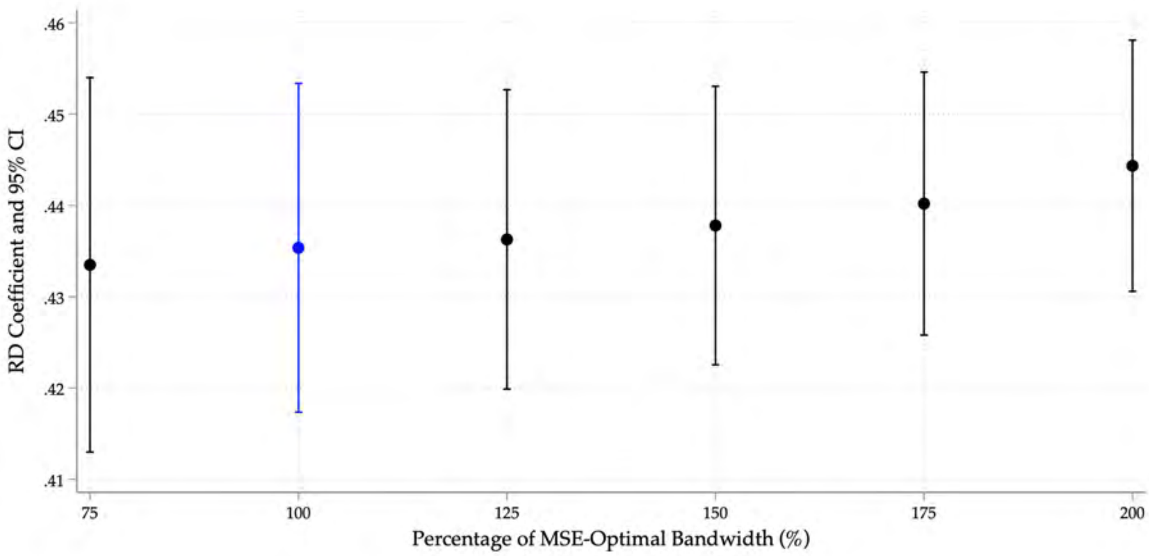
(b) Need Cutoff



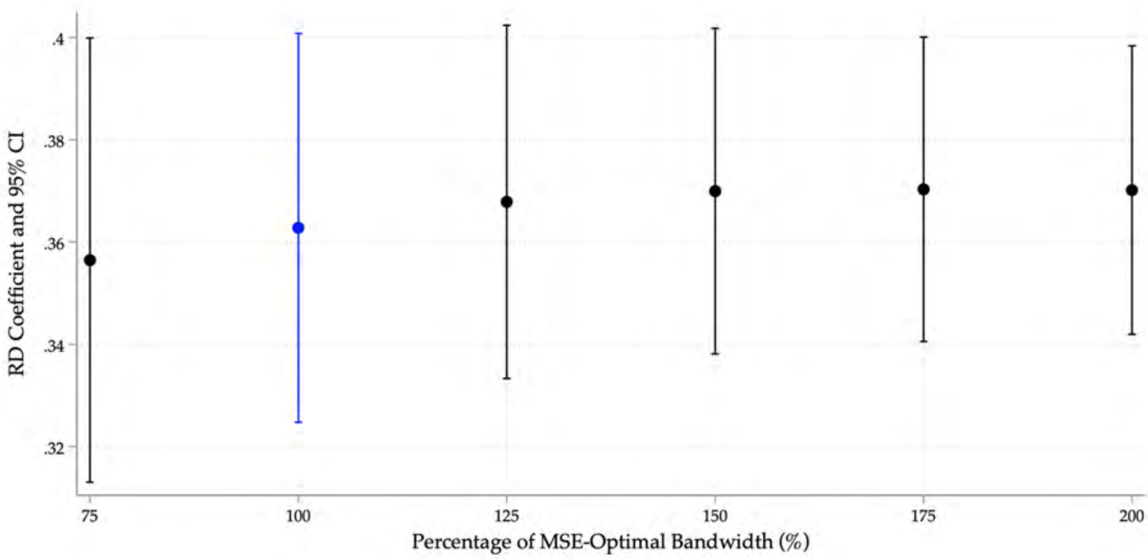
Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test 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) Merit Cutoff



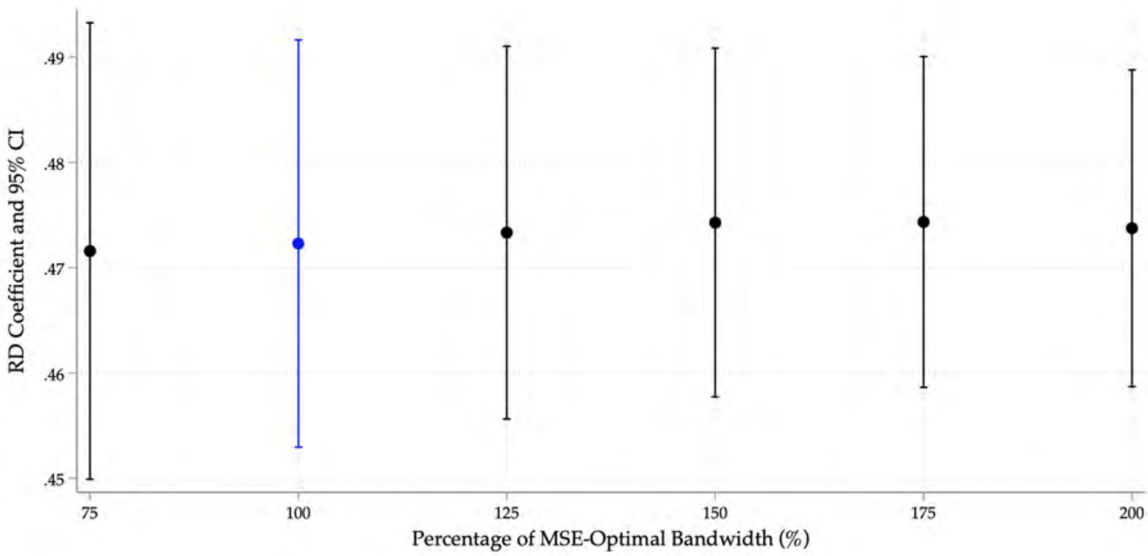
(b) Need Cutoff



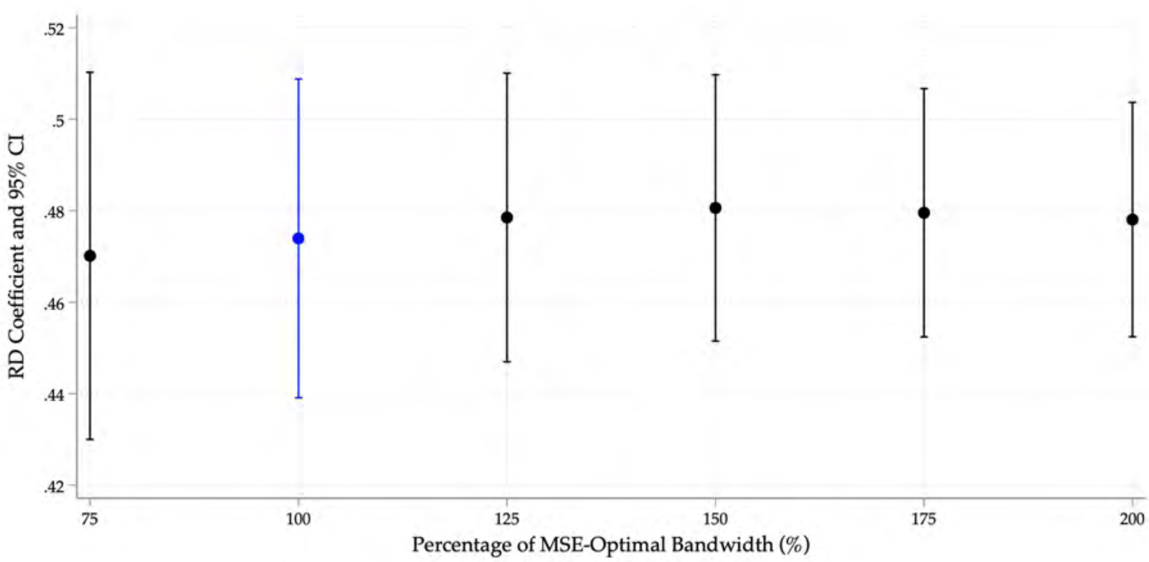
Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test score (SISBEN wealth score) as the running variable, restricting the sample to need- (merit-) eligible students. The dependent variable is the likelihood of accessing a high-quality 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) Merit Cutoff



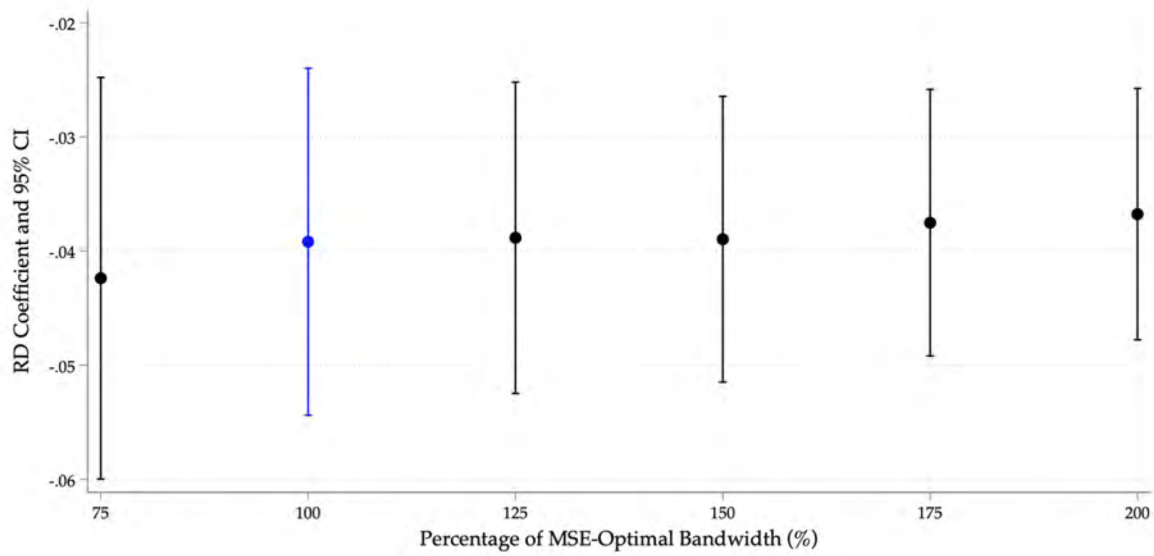
(b) Need Cutoff



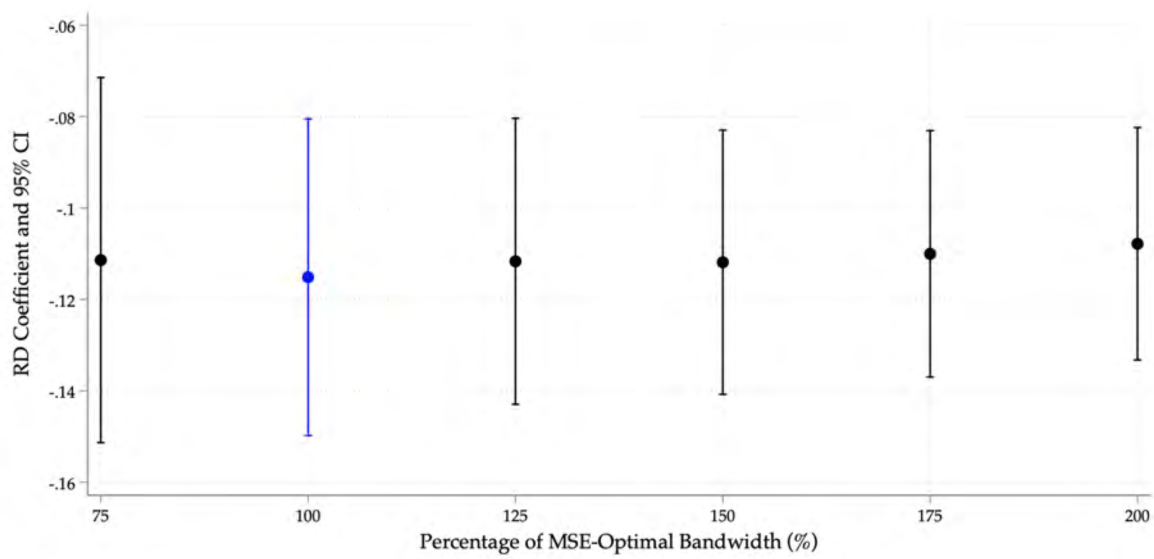
Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test score (SISBEN wealth score) as the running variable, restricting the sample to need- (merit-) eligible students. The dependent variable is the likelihood of accessing a high-quality 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) Merit Cutoff



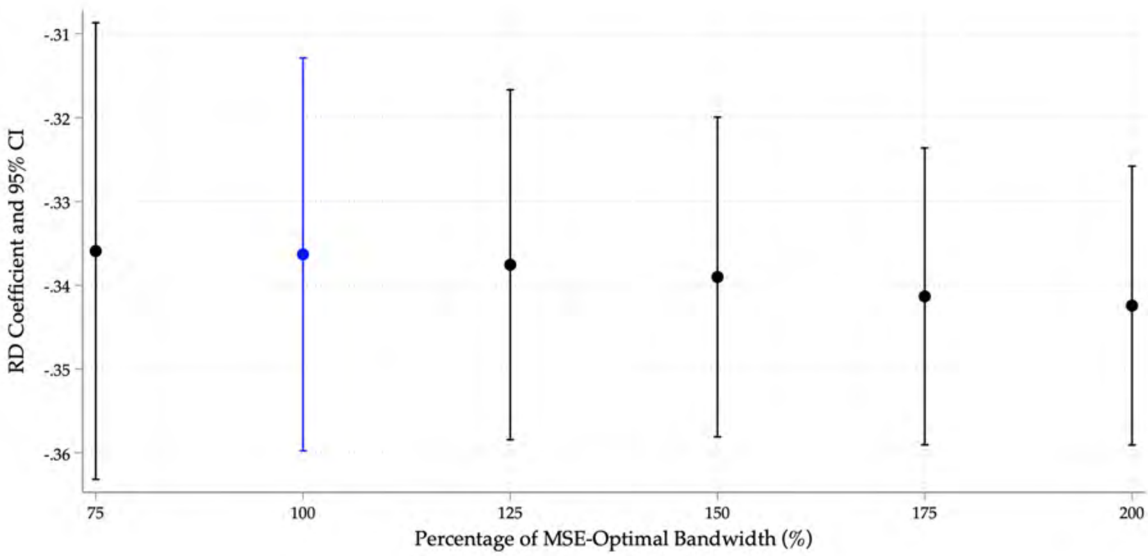
(b) Need Cutoff



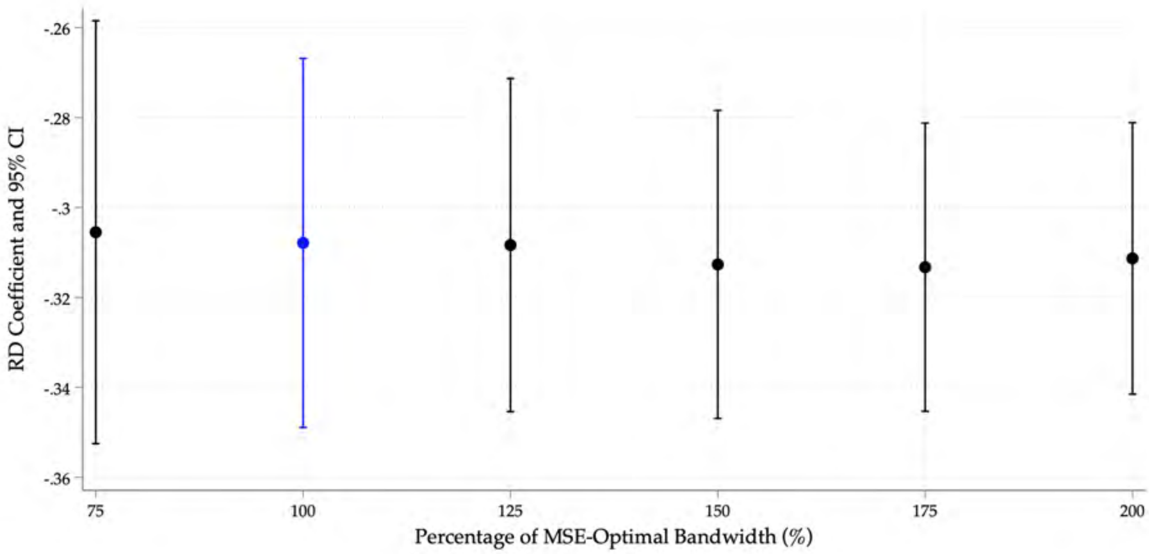
Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test score (SISBEN wealth score) as the running variable, restricting the sample to need- (merit-) eligible students. The dependent variable is the likelihood of accessing a high-quality 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) Merit Cutoff



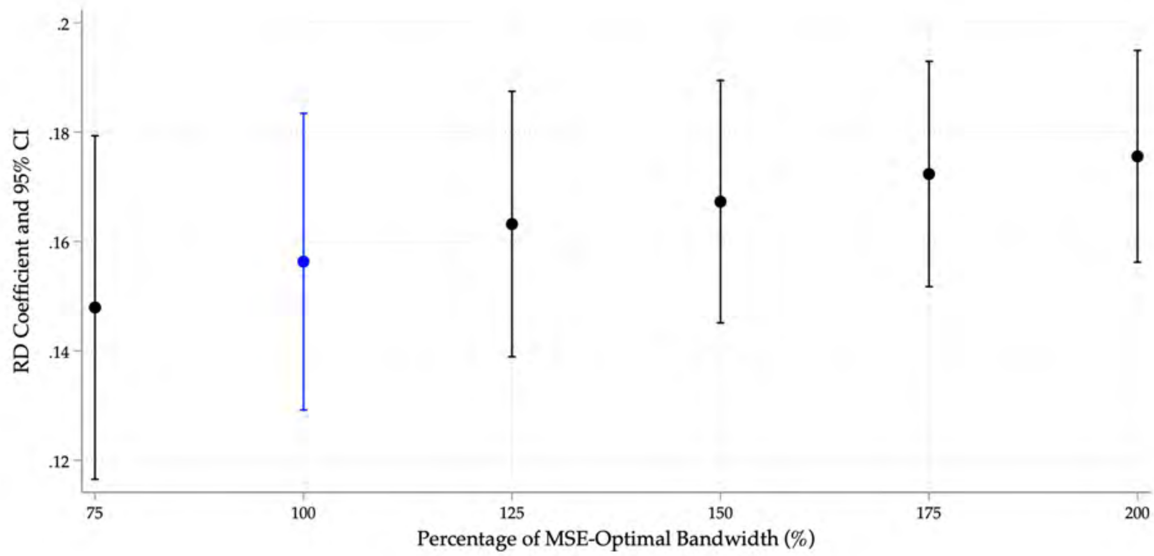
(b) Need Cutoff



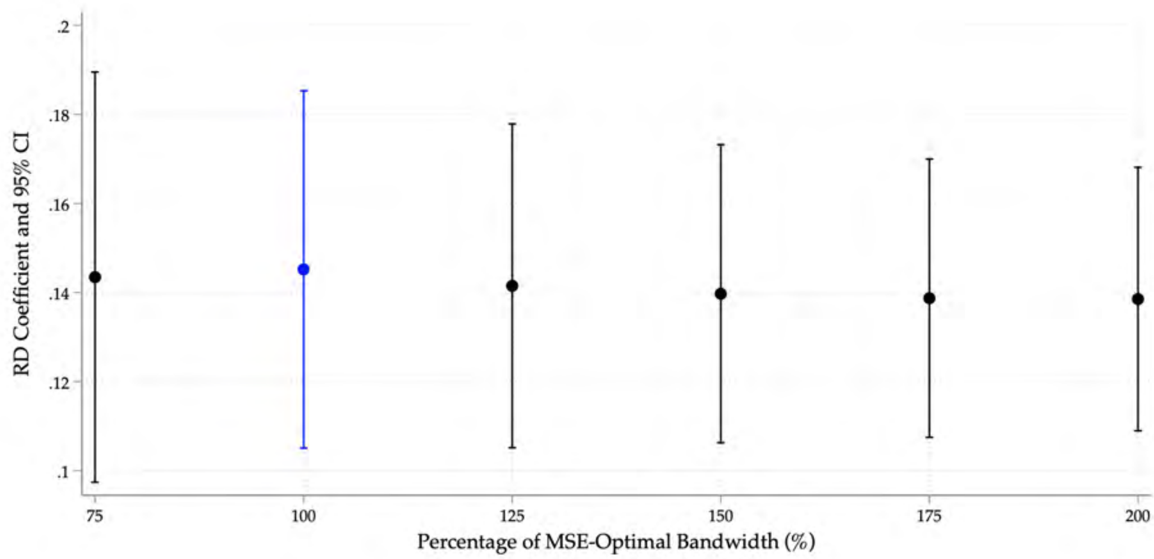
Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test score (SISBEN wealth score) as the running variable, restricting the sample to need- (merit-) eligible students. The dependent variable is the likelihood of accessing a low-quality 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) Merit Cutoff



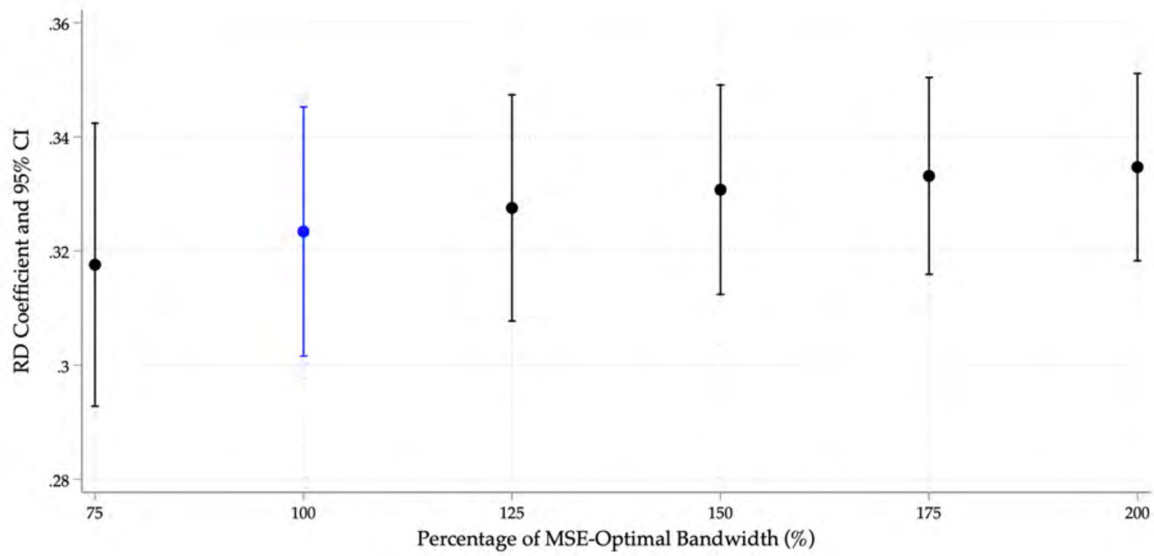
(b) Need Cutoff



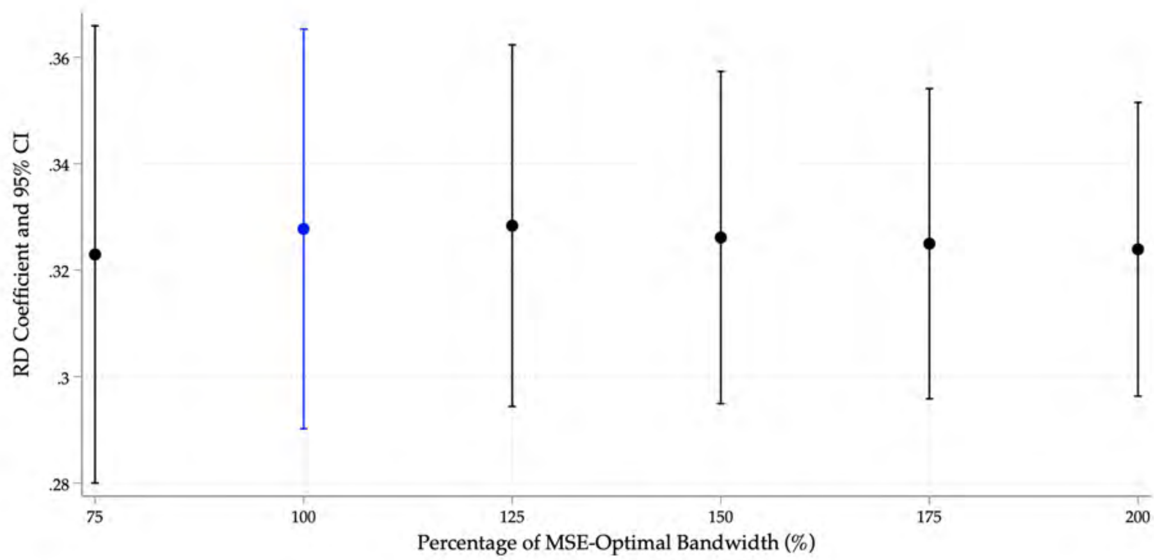
Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test 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) Merit Cutoff



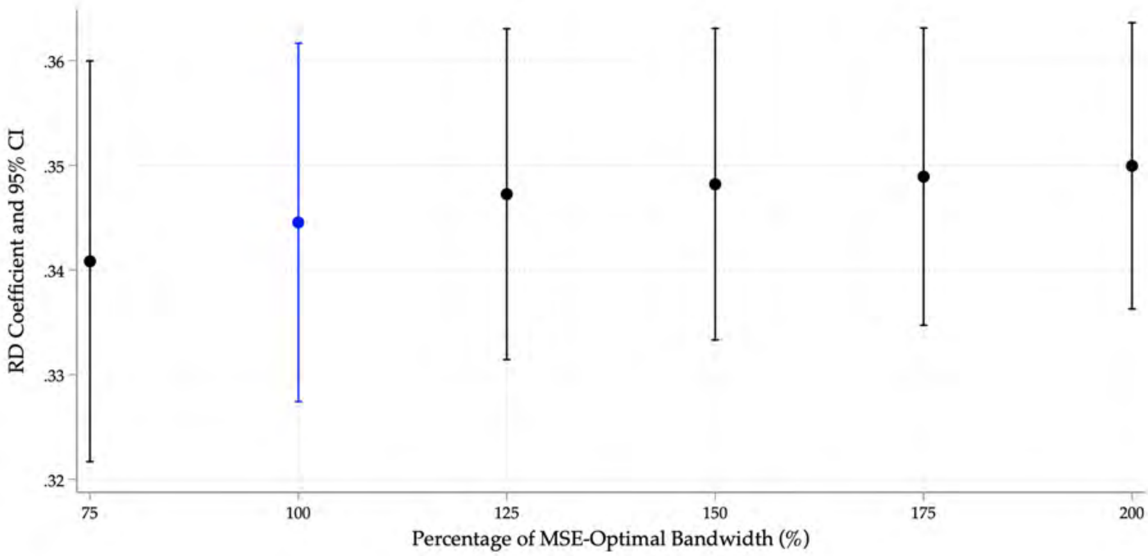
(b) Need Cutoff



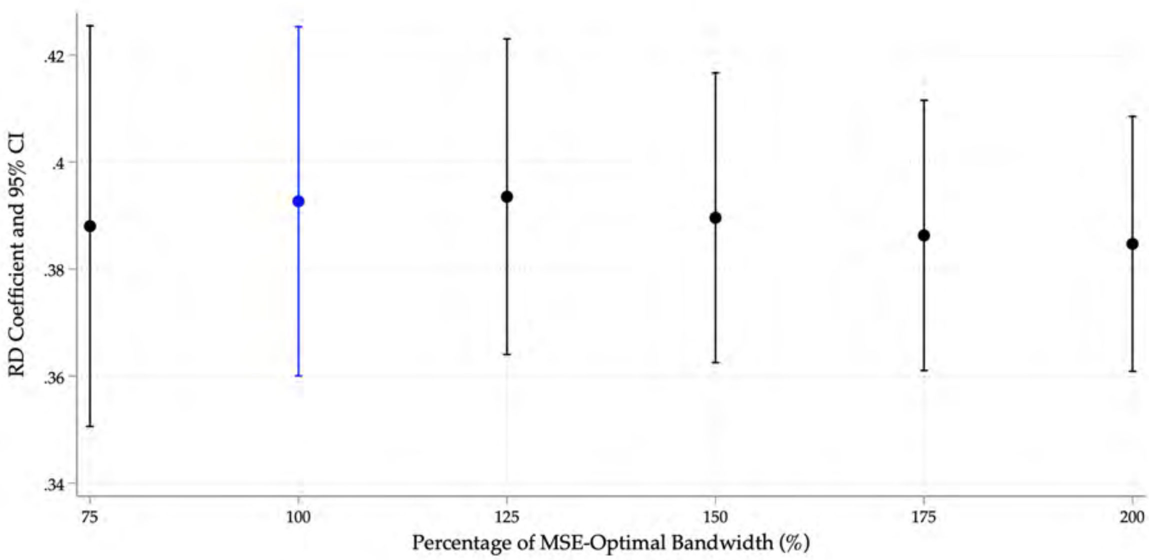
Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test 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 a high-quality 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) Merit Cutoff



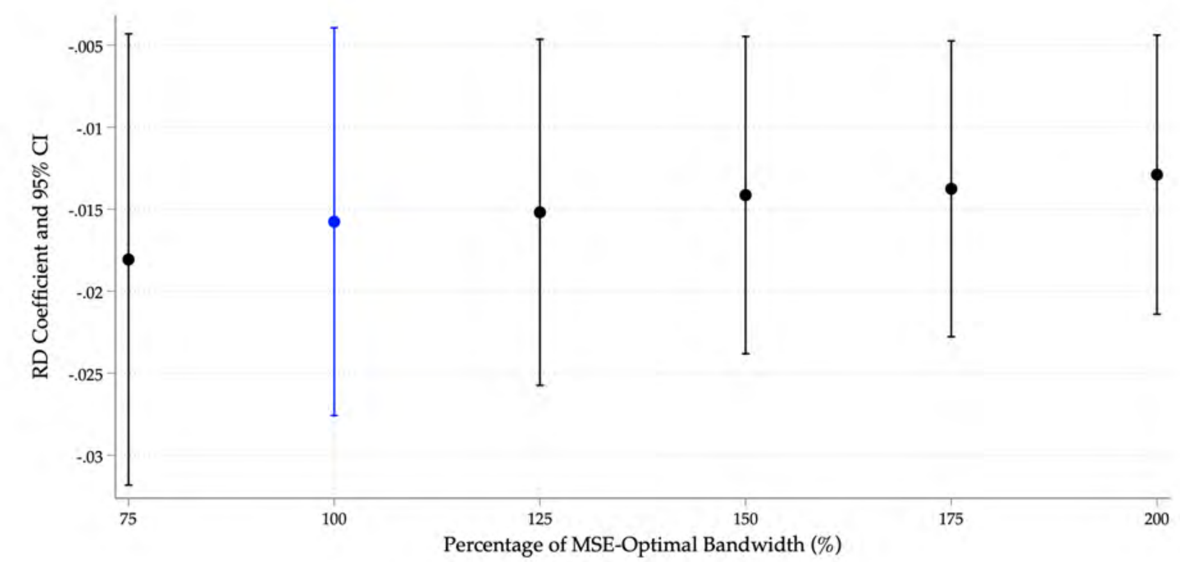
(b) Need Cutoff



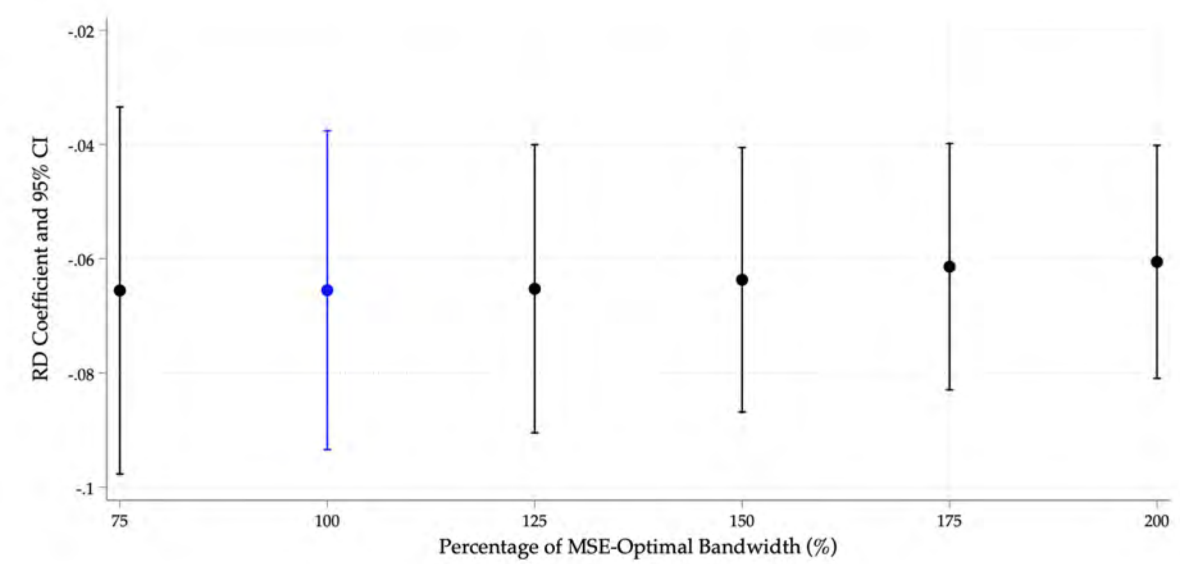
Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test 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 a high-quality 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) Merit Cutoff



(b) Need Cutoff

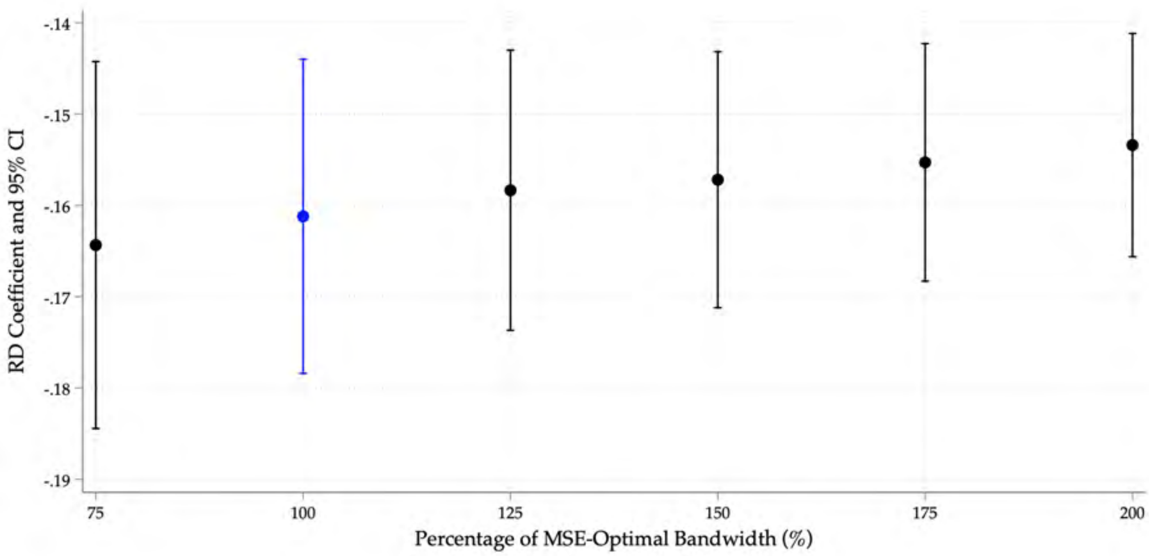


Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test 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 a high-quality public college within seven years from high school completion.

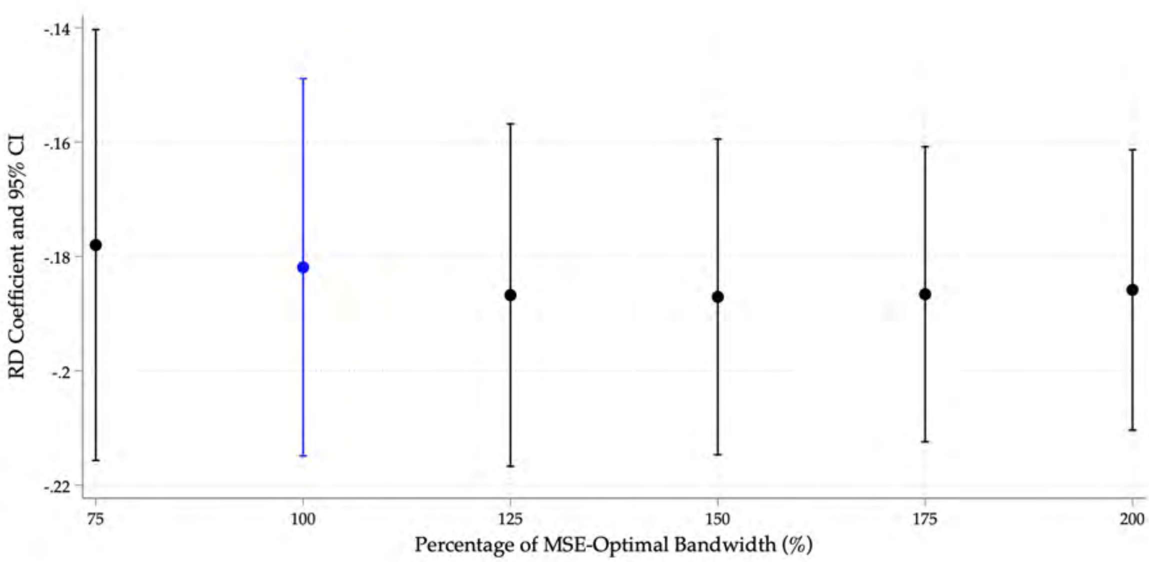
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and SABER PRO (ICFES).

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

(a) Merit Cutoff



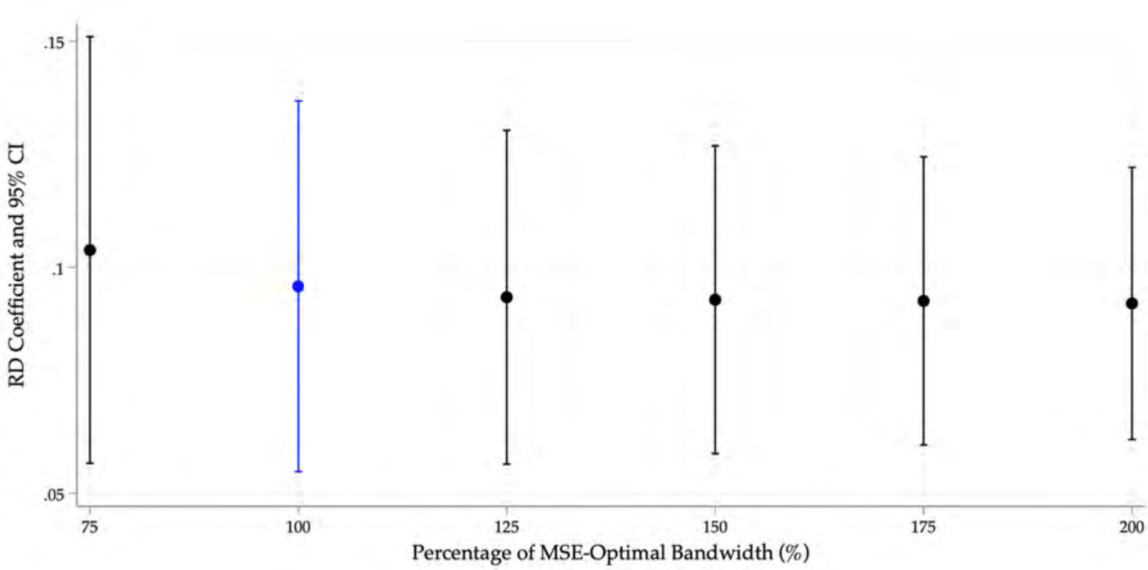
(b) Need Cutoff



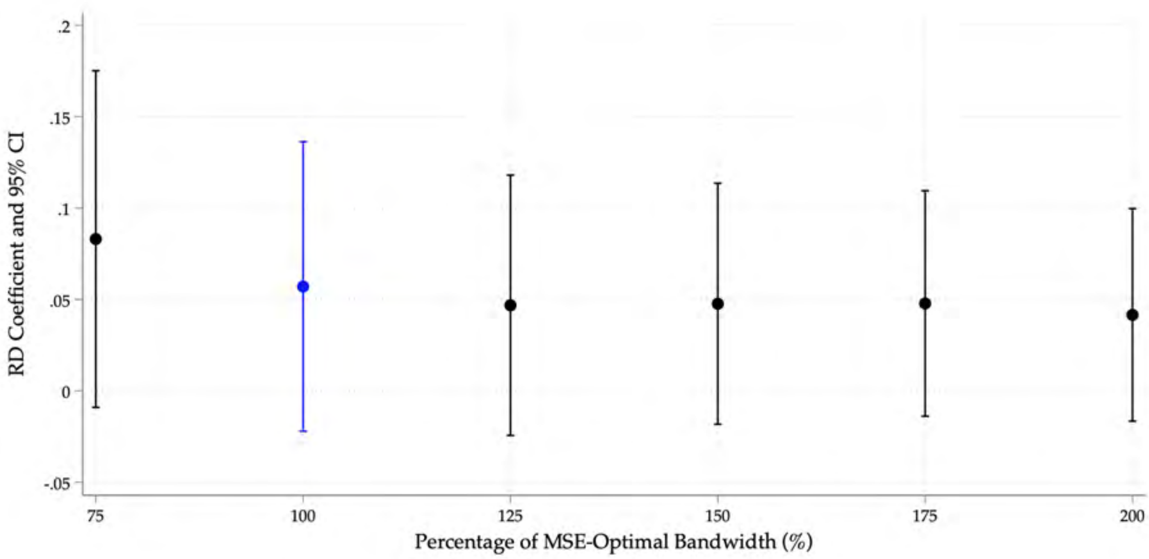
Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test 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 a low-quality 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) Merit Cutoff



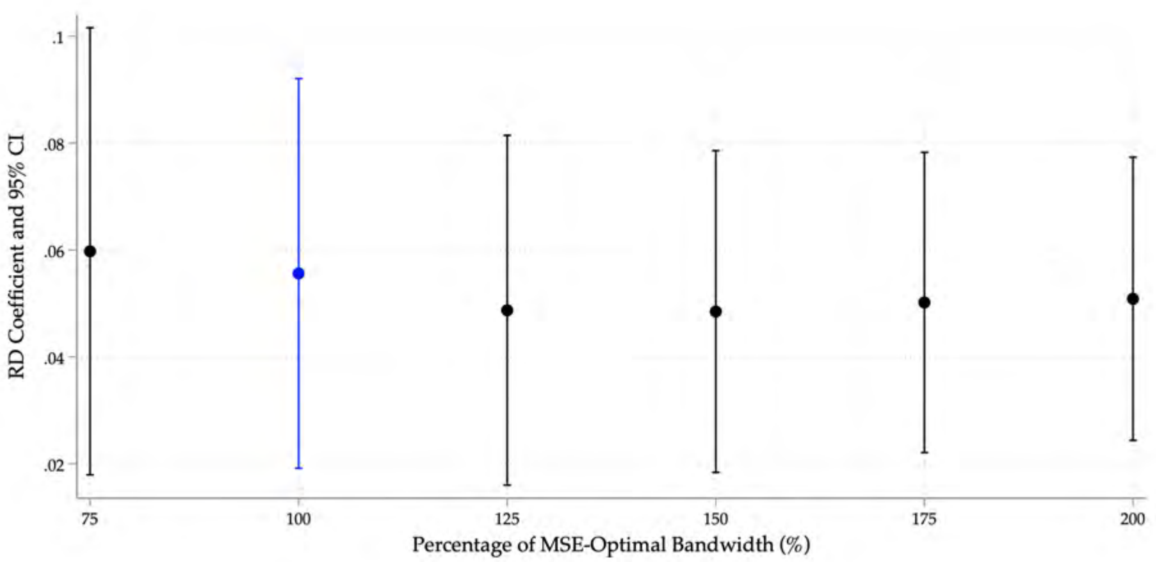
(b) Need Cutoff



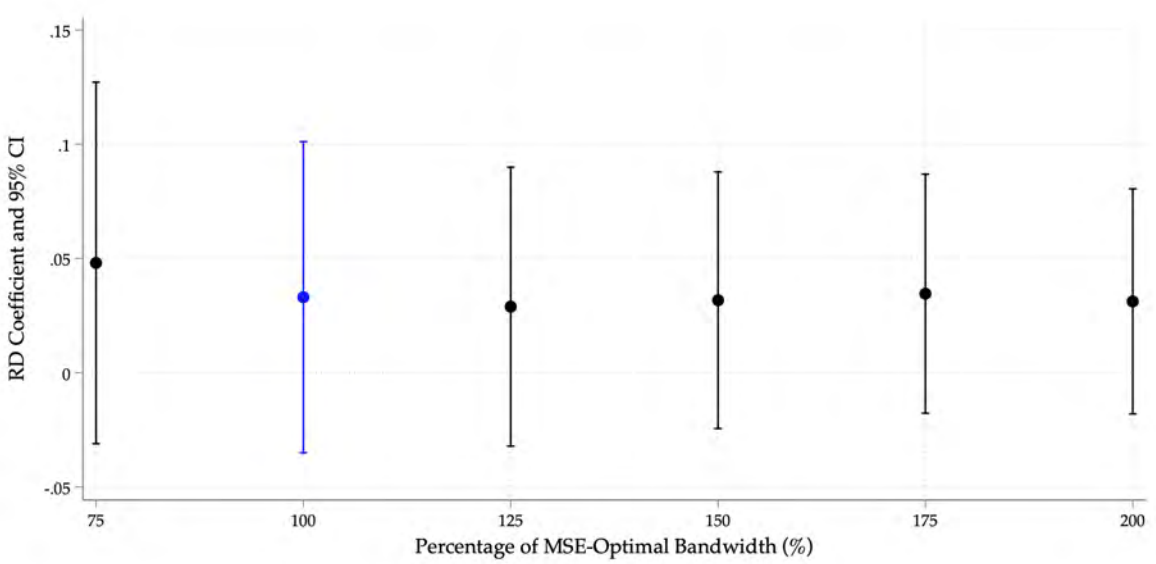
Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test 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) Merit Cutoff



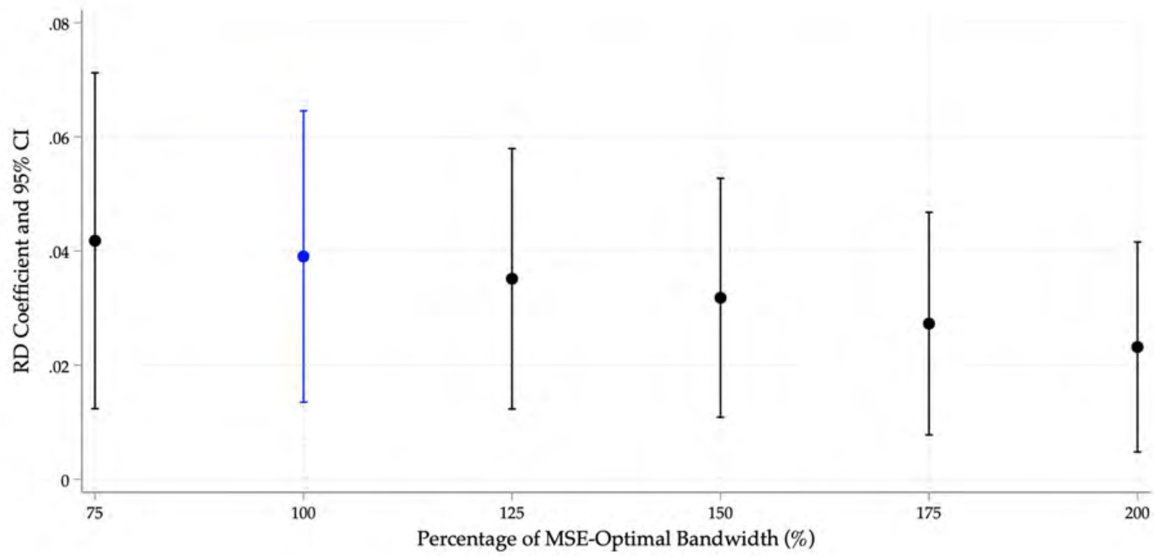
(b) Need Cutoff



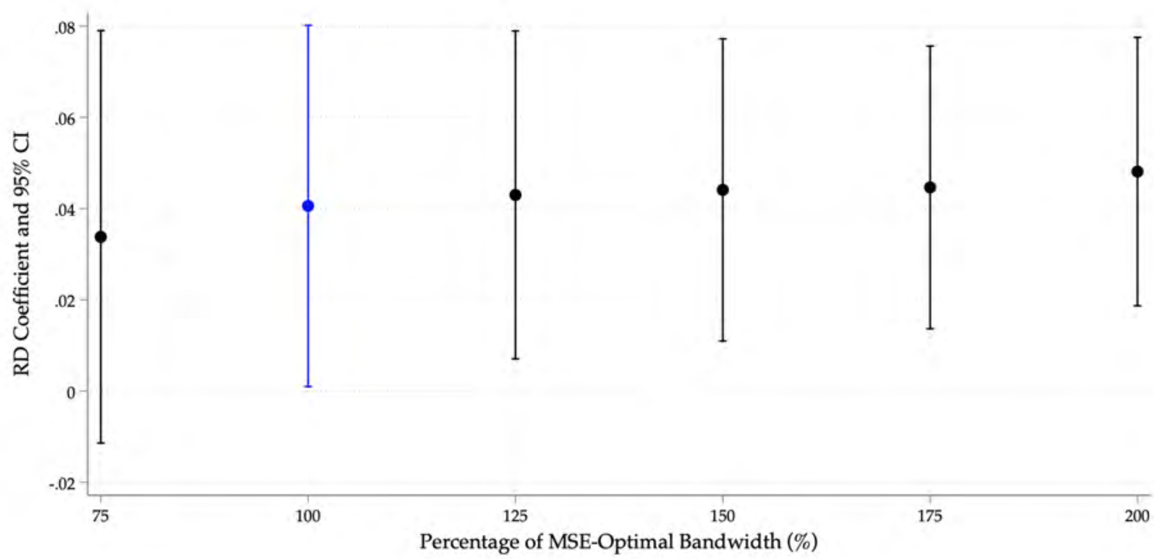
Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test 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 Seven Years after High School

(a) Merit Cutoff



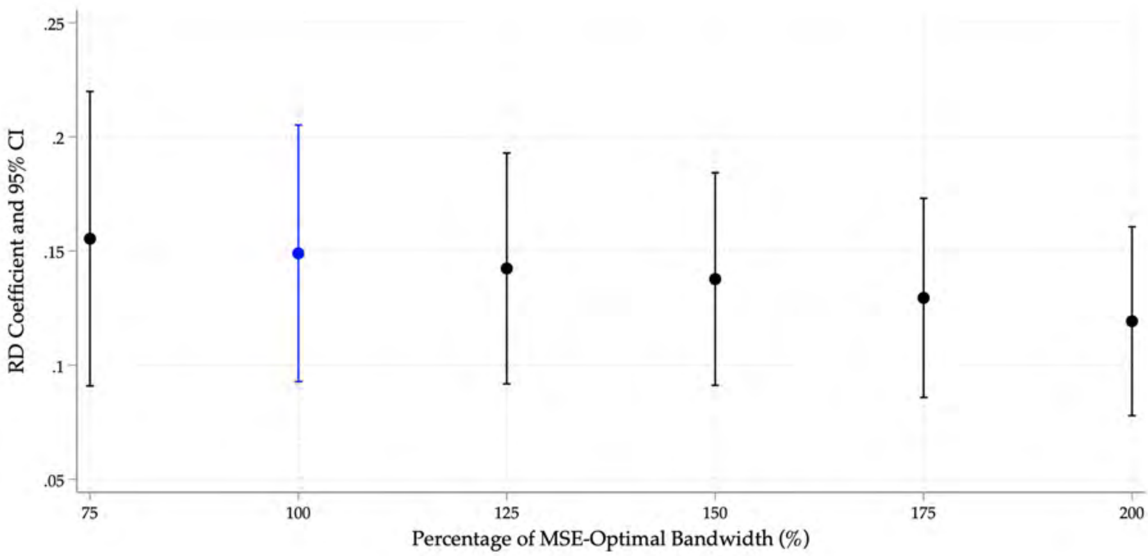
(b) Need Cutoff



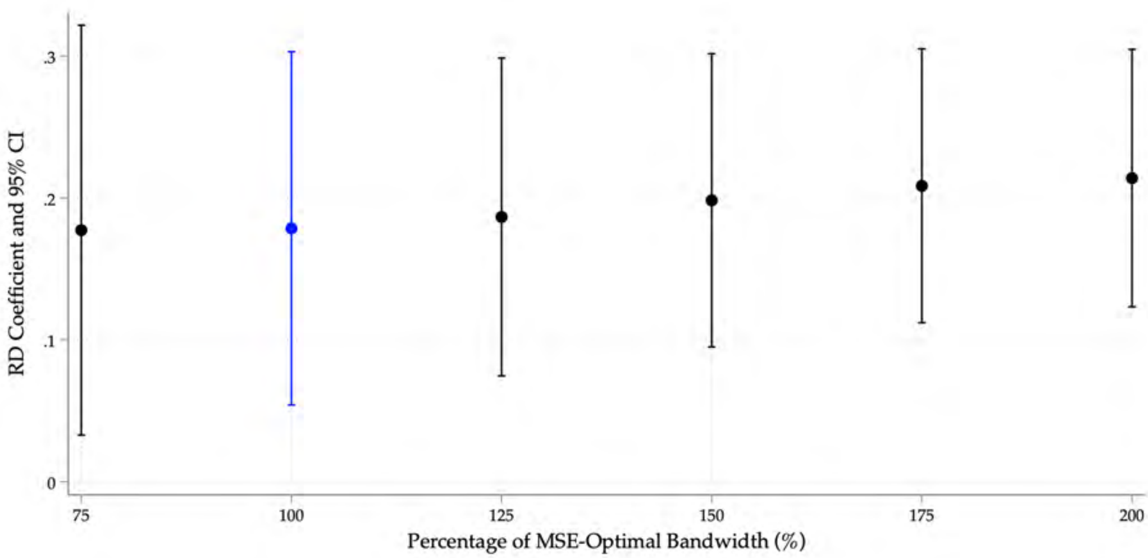
Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test 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 seven 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) Seven Years after High School

(a) Merit Cutoff



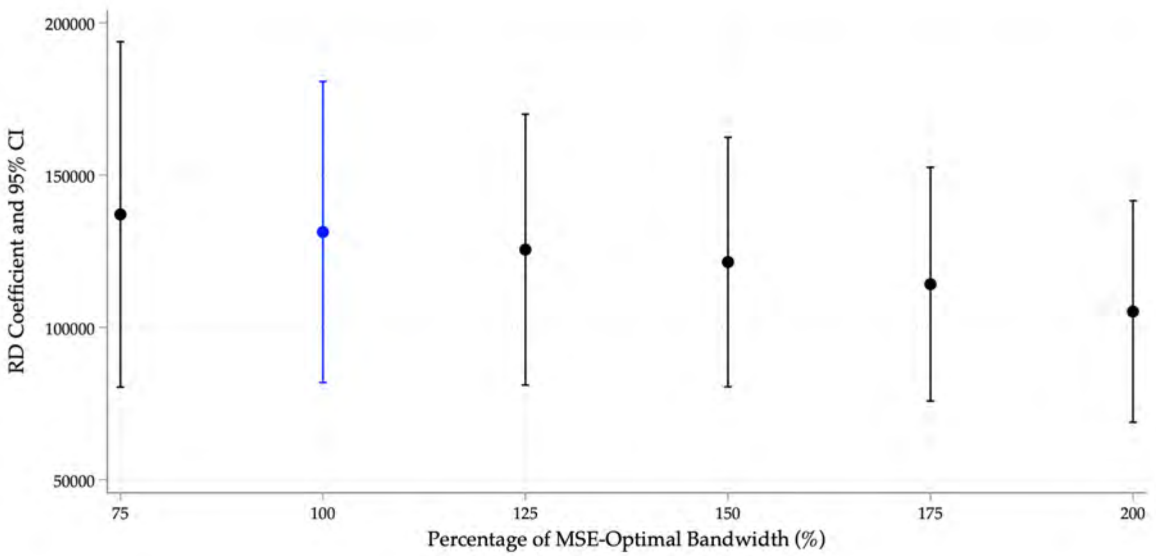
(b) Need Cutoff



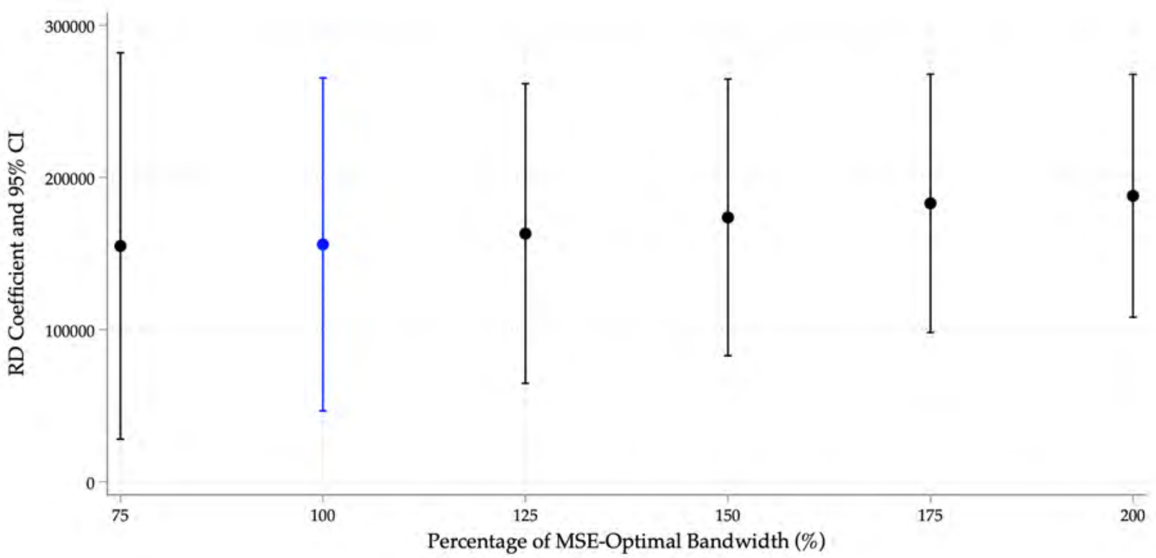
Notes: The figures plot the reduced-form, conventional RD coefficient, and 95% confidence intervals for different bandwidth choices estimated with package rdrobust (Cattaneo et al., 2014). The series in blue is the mean squared error (MSE)-optimal selected bandwidth. Panel A (B) uses the SABER 11 test 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, seven 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) Seven Years after High School

(a) Merit Cutoff



(b) Need 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, seven years after high school.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and PILA (MinSalud).

Appendix C College Value Added

This section describes the outcome variables and measures we employ to approximate college graduation, learning, and earning productivity.

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. This enables estimating the portion of the effect on educational and labor market outcomes explained by the college, major, and program fixed effects.

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 years from high school completion, the outcomes will be realized by 2019 and 2020 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, outcomes like graduation, learning, and earnings vary substantially across colleges and programs. For this reason, we consider more granular cells to account for within-college variation across programs. First, SNIES defines eight study areas (*áreas del conocimiento*): agriculture and veterinary, arts, education, health, social sciences and humanities, economics and business, engineering and architecture, and math and natural sciences. Second, SNIES defines 55 study majors (*núcleos básicos del conocimiento*), e.g., economics. Lastly, the most granular level is at the college-program level—the level at which most students apply to college. 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 $\delta_{j(i,t)p(i,t)}$. When estimating the model at the college-field, college-major, and college-program levels, we follow Ferreyra et al. (2020) and drop cells with fewer than 10 students. This leaves us with 1,145 college-field cells, 2,653 college-major cells, and 4,688 college-program cells.

In addition, we examine how the estimated fixed effects for these three models vary when progressively including a denser set of baseline covariates. 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). Because students' outcomes might be influenced by the socioeconomic characteristics of their peers, 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). Lastly, Model F includes the leave-one-out mean SISBEN score of the cohort in the college (or college-field, college-major, or college-program). Thus, Models C through F enable progressively controlling for differential peer cohort qualities to obtain "value-added" college contributions purged of cohort effects.

First, we examine how the inclusion of baseline covariates affects the estimated college fixed effects, using bachelor's degree attainment as an illustration. For this outcome, we exclude students who do not access any four- or five-year undergraduate program within six years from high school since access is a prerequisite for graduation from these programs. Figure C.1 compares the distributions of the college fixed effects estimated using Models A through F. The college fixed effects are somewhat sensitive to the inclusion of baseline controls, although the qualitative results are the same across the different models. Our preferred model is Model F, which controls for a rich vector of baseline characteristics of individuals, households, high schools, and college peers. Panel F of Figure C.1 shows there is wide variation in colleges' contribution to getting students to complete their bachelor's degrees, which is particularly substantial among low-quality colleges. Indeed, dropout rates are exceptionally high at some low-quality colleges. However, the most productive low-quality colleges contribute more to bachelor's degree attainment than high-quality colleges. Moreover, high-quality *public* colleges contribute less to degree attainment than high-quality *private* colleges.

In Colombia, students apply to specific college-program pairs from the moment they first apply for access to higher education, and programs vary significantly in their selectivity. For this reason, we estimate the "value-added" contributions to students' graduation outcomes by college-field pairs and the more granular college-field, college-major, and college-program pairs. Figure C.2 compares the distribution of fixed effects for college fixed effects, college-field fixed effects, college-major fixed effects, and college-program fixed effects using Model F. Replacing the college fixed effects with college-field, college-major, or college-program fixed effects significantly reduces the spread in graduation productivities across college types because students self-select across programs and colleges vary widely in their selectivity across majors. Panel D shows that low-quality private colleges have a larger graduation productivity, while some high-quality *public* colleges have the lowest graduation productivity.

Next, we estimate the "value-added" contributions to students' learning performance and compare the distribution of fixed effects across colleges, fields, majors, and programs. For this outcome, we exclude students who do not take the exam from any four- or five-year undergraduate program within seven years from high school. Figure C.3 compares the distribution of these fixed effects by college type using Model F. Replacing the college fixed effects with college-field, college-major, or college-program fixed effects in Specification (3) shrinks the differences across college types because, again, programs vary significantly in their selectivity within colleges. Panel D shows that high-quality colleges give students more knowledge and skills than low-quality colleges. Moreover, high-quality *private* colleges are more productive in teaching skills than high-quality *public* colleges.

Next, we estimate the contributions to students' formal employment seven years after high school. We assign students who do not access any college to a fake college identifier and express the estimated fixed effects relative to students with no college experience. Figure C.4 compares the distribution of the different types of fixed effects by college type using Model F. Panel A shows that high-quality *private* colleges have larger employment productivity. However, replacing the college fixed effects with more granular cells that account for within-college, across major differences in Panel C shrinks the differences across college types. Panel D shows that private colleges are better at getting students jobs than public colleges. Crucially, after adjusting for selection across programs, there is little difference in employment productivity between high- and low-quality colleges.

We obtain similar results when focusing on formal earnings in Figure C.5. Indeed, the earnings premium from high-quality private colleges shrinks as we move progressively to smaller cells. Notwithstanding, Panel D shows that students who attended the most productive high-quality *private* colleges earn more than other individuals even after adjusting for selection across programs and peer cohort qualities. Students from the most productive low-quality private colleges are the next highest earners. By contrast, students from high-quality *public* colleges tend to earn less than other students, after controlling for selection and cohort effects.

Interestingly, Figure C.5 shows that some colleges have *negative* earnings productivity, meaning students' earnings seven years after high school would have been higher had they not attended college. Moreover, Figures C.6 through C.8 condition the estimation sample to students who access college, earn a college degree, and earn a bachelor's degree. The qualitative results remain similar, but restricting the sample rescales the fixed effects and changes the x-axis.

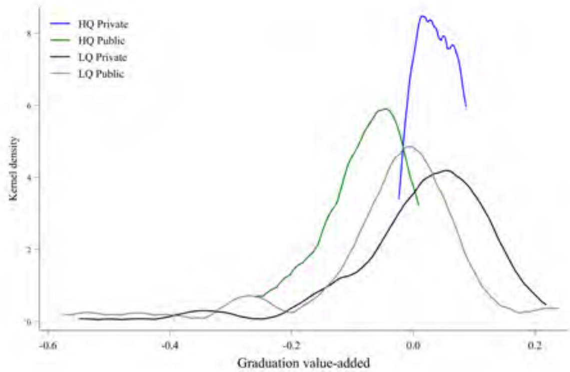
Having estimated these fixed effects based on the pre-policy cohorts, the next step is use these fixed effects for the fall 2014 cohort and compare the change in "value-added" of colleges, fields, majors, and programs attended and the equivalent RD coefficient for each outcome. This enables understanding the portion of the effect of financial aid explained by changes in college graduation, learning, and earning productivities.

Tables C.1 through C.4 compare the reduced-form RD coefficient for each outcome and the college, college-field, college-major, and college-program fixed effects across Models A through F. The effects on any degree attainment and bachelor's attainment are sensitive to the inclusion of baseline covariates, suggesting that a large part of the graduation effect is explained by differences in peer and cohort qualities (ability and SES). By contrast, the coefficients on learning performance are relatively stable across specifications and models. Indeed, conditioning the sample to students close to graduation gets rid of a major source of selection of students by ability and SES. Furthermore, the effects on labor market outcomes increase when controlling for college cohort qualities. Because students self-select substantially across programs, the models including college-program fixed effects are less sensitive to controlling for cohort qualities than the models that do not consider selection across programs.

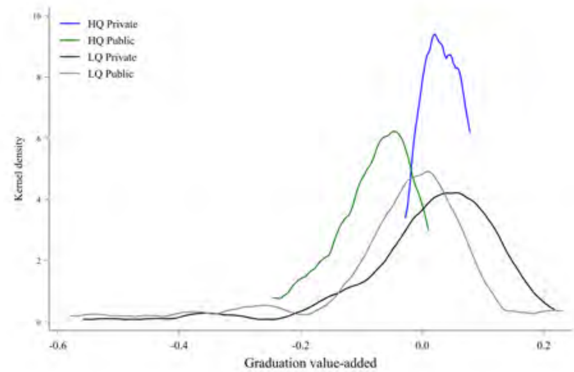
Model F using college-program fixed effects is our preferred specification: it controls for selection into colleges and programs—which is the level at which most students compete for college entry—and uses the richest vector of baseline covariates that enables controlling for baseline ability, selection across programs, and peer cohort qualities.

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

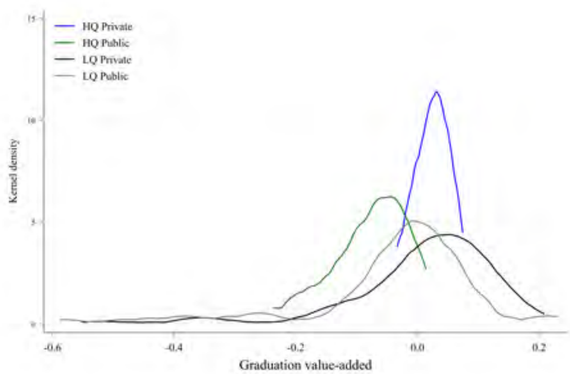
(a) Model A



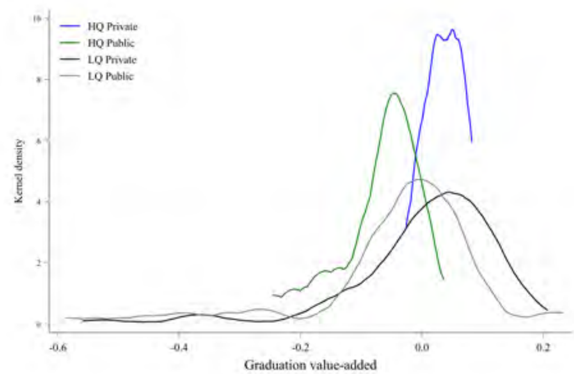
(b) Model B



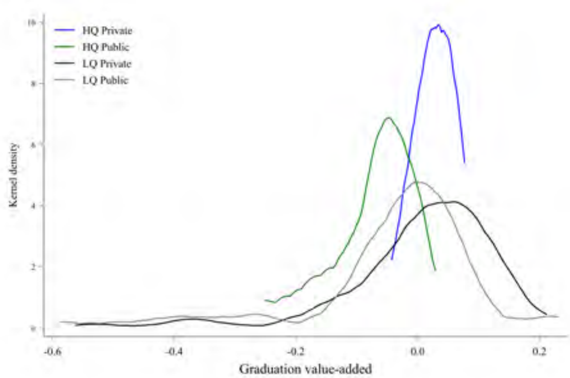
(c) Model C



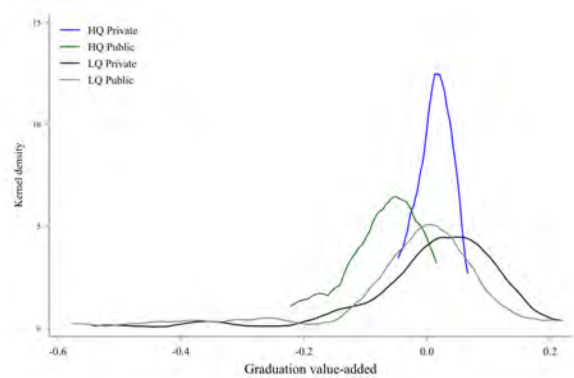
(d) Model D



(e) Model E



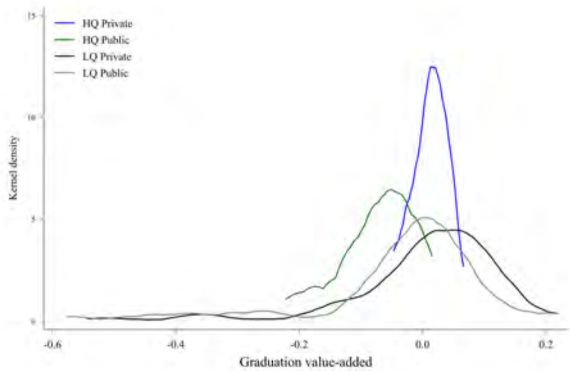
(f) Model F



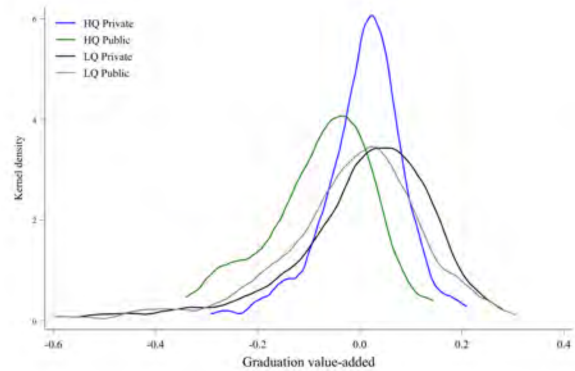
Notes: The figure plots the distribution of college fixed effects $\hat{\delta}_j$ estimated using Specification (1) where the outcome variable is the likelihood of taking a SABER PRO exam within seven years from high school completion. Models A through F progressively add baseline covariates. The fixed effects are plotted separately by college type. The sample is restricted to students who ever attended a four- or five-year undergraduate program within six years from high school completion.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), SNIES (MEN), and SABER PRO (ICFES).

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

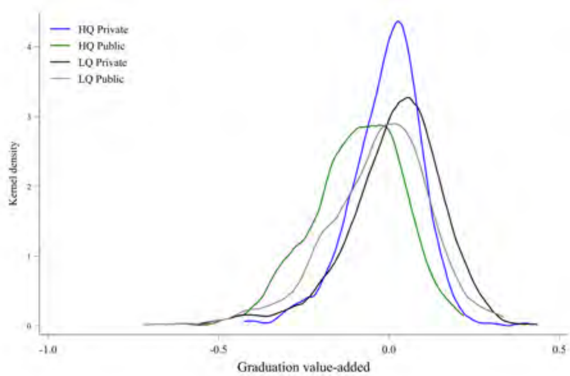
(a) College Fixed Effects



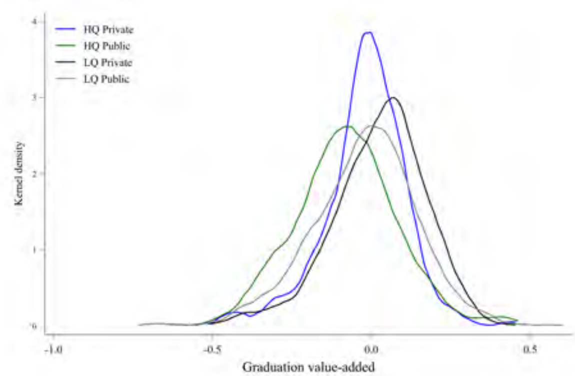
(b) College-Field Fixed Effect



(c) College-Major Fixed Effect



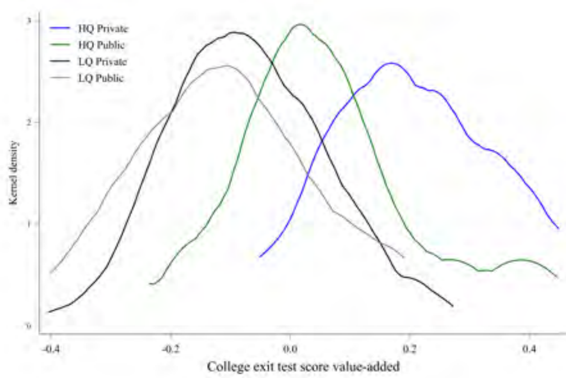
(d) College-Program Fixed Effect



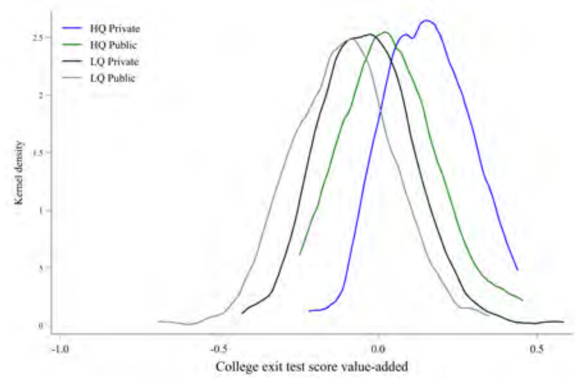
Notes: The figure plots the distribution of college, college-field, college-major, and college-program fixed effects estimated using Specification (1) and Model F where the outcome variable is the likelihood of taking a SABER PRO exam within seven years from high school completion. The fixed effects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

Figure C.3: Learning Productivities of Colleges, Fields, Majors, and Programs

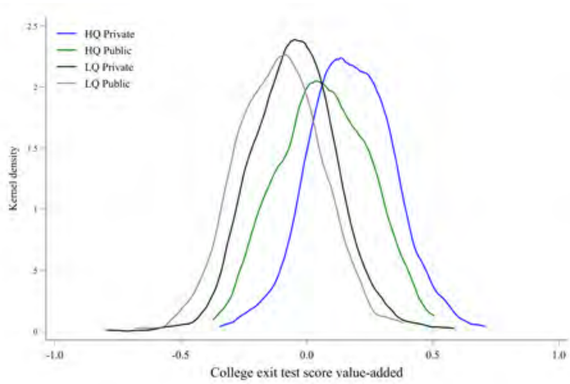
(a) College Fixed Effects



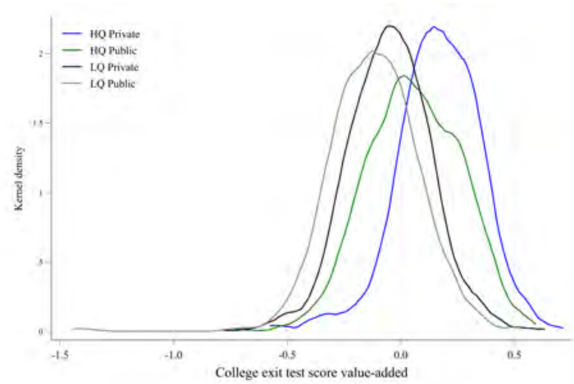
(b) College-Field Fixed Effect



(c) College-Major Fixed Effect

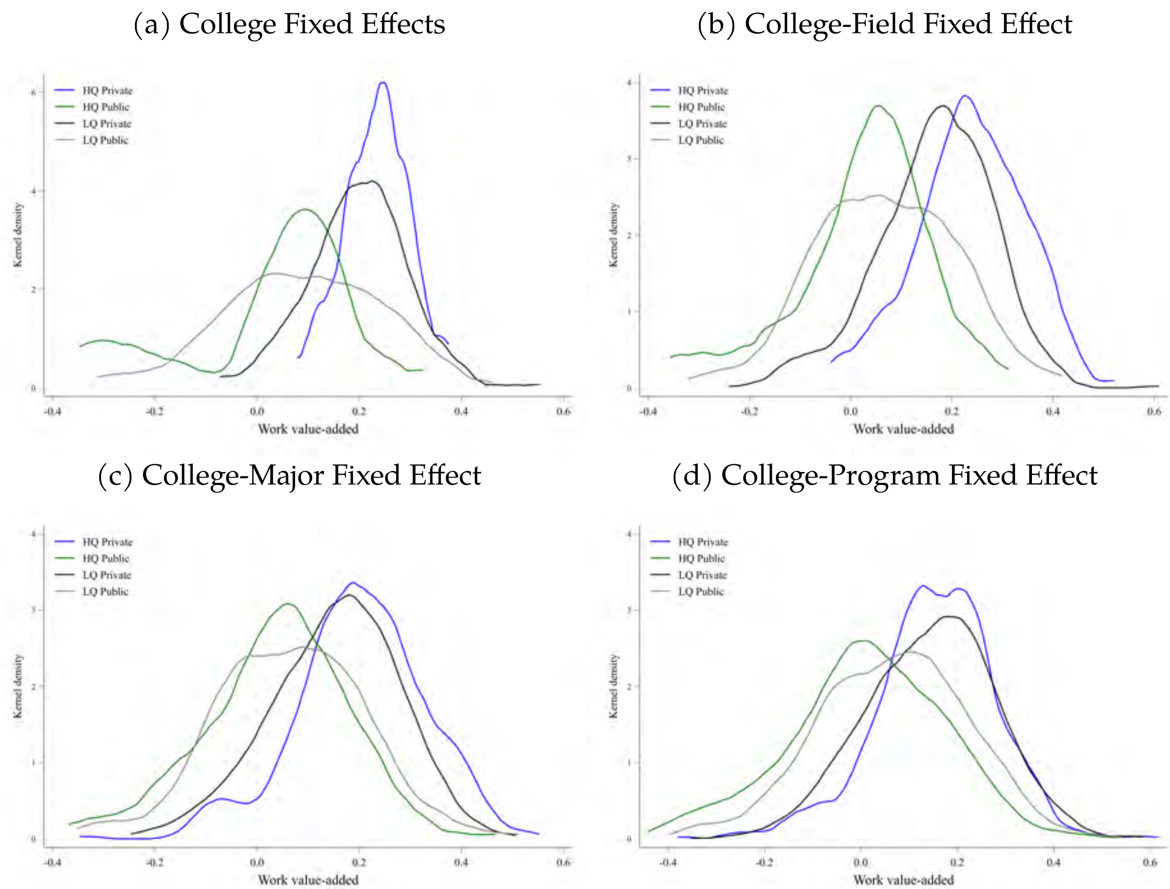


(d) College-Program Fixed Effect



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 SABER PRO score for exams taken 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).

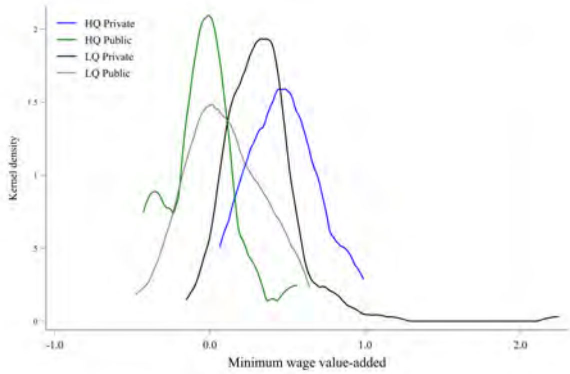
Figure C.4: Employment Productivities of Colleges, Fields, Majors, and Programs



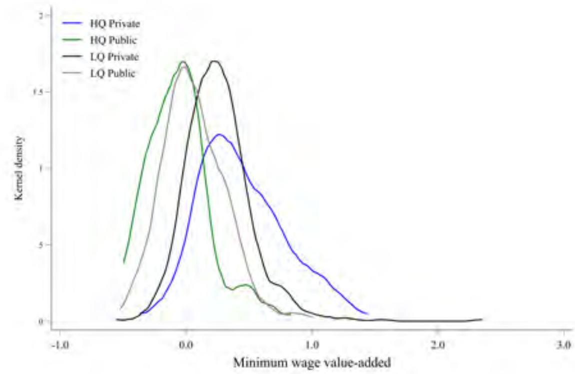
Notes: The figure plots the distribution of college, college-field, college-major, and college-program fixed effects estimated using Specification (1) and Model F where the outcome variable is formal employment seven years from high school completion. The fixed effects are plotted separately by college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

Figure C.5: Earnings Productivities of Colleges, Fields, Majors, and Programs

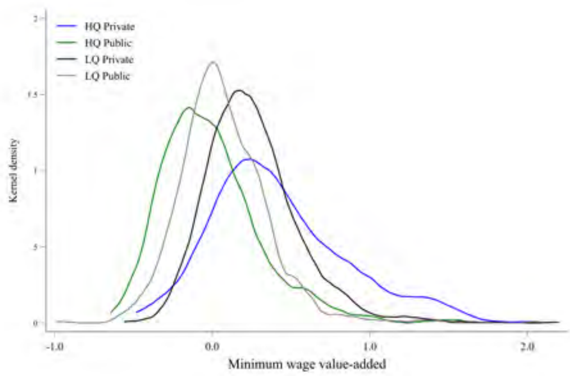
(a) College Fixed Effects



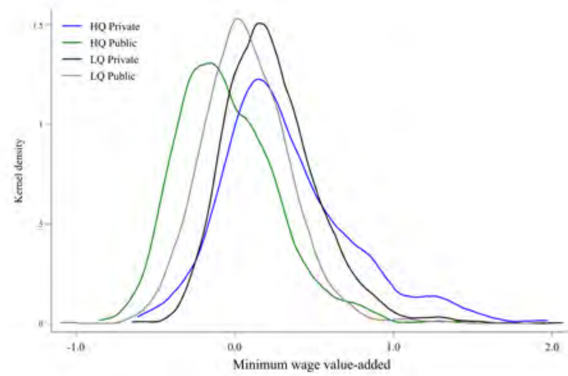
(b) College-Field Fixed Effect



(c) College-Major Fixed Effect



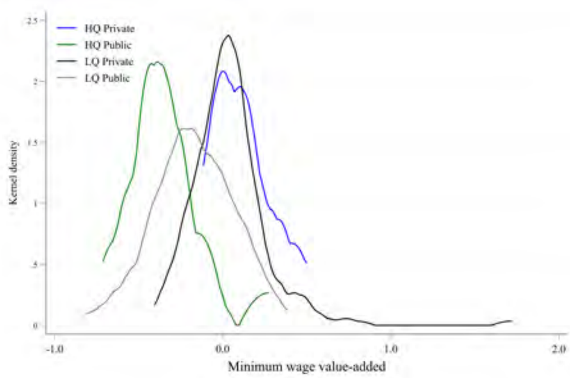
(d) College-Program Fixed Effect



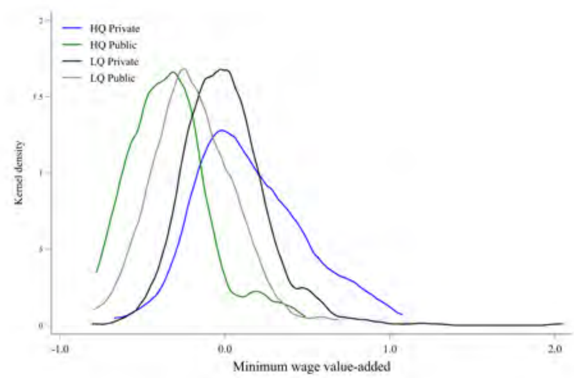
Notes: The figure plots the distribution of college, college-field, college-major, and college-program fixed effects estimated using Specification (1) and Model F where the outcome variable is formal monthly earnings seven years from high school completion, measured in multiples of the monthly minimum wage. The fixed effects are plotted separately by college type.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and PILA (MinSalud).

Figure C.6: Figure C.5 Conditional on Accessing College

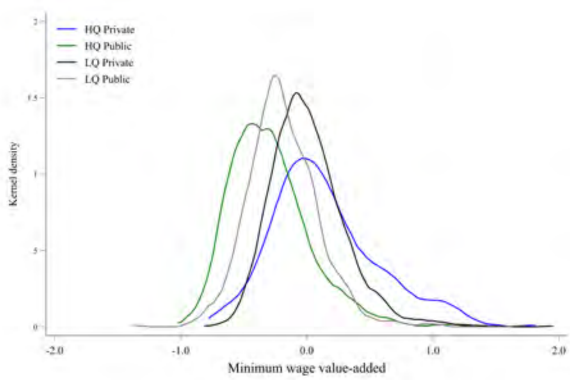
(a) College Fixed Effects



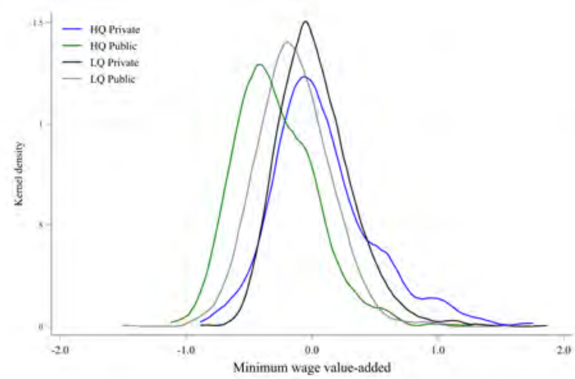
(b) College-Field Fixed Effect



(c) College-Major Fixed Effect



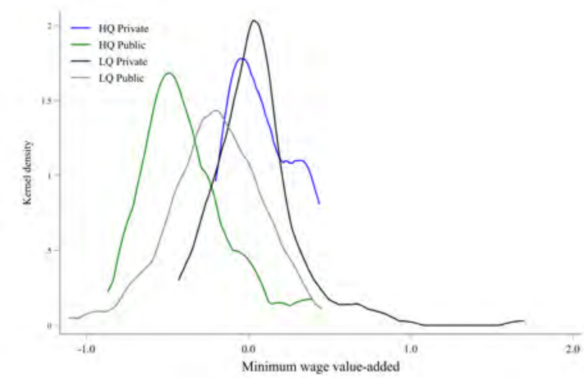
(d) College-Program Fixed Effect



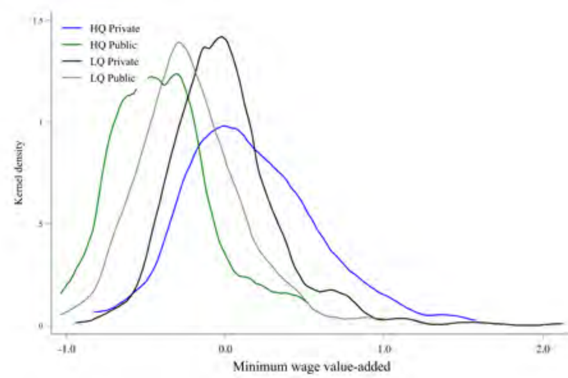
Notes: The figure plots the distribution of college, college-field, college-major, and college-program fixed effects estimated using Specification (1) and Model F where the outcome variable is formal monthly earnings seven years from high school completion, measured in multiples of the monthly minimum wage. The fixed effects are plotted separately by college type. The sample is restricted to students who accessed college within six years from high school.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and PILA (MinSalud).

Figure C.7: Figure C.5 Conditional on Earning Any College Degree

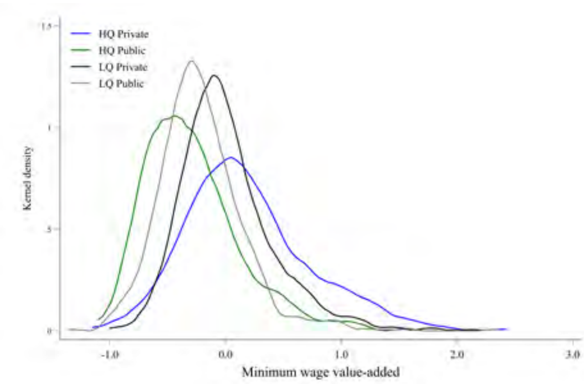
(a) College Fixed Effects



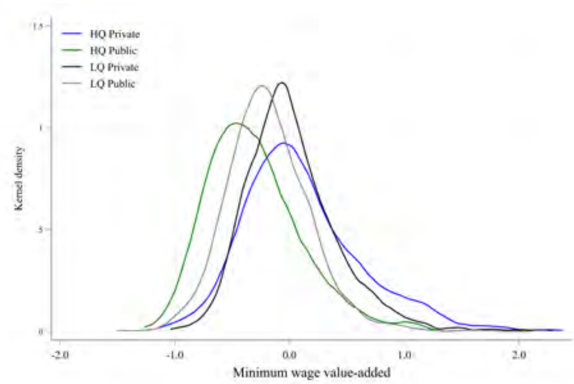
(b) College-Field Fixed Effect



(c) College-Major Fixed Effect



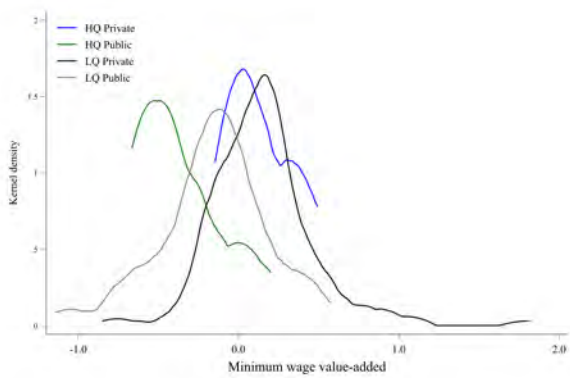
(d) College-Program Fixed Effect



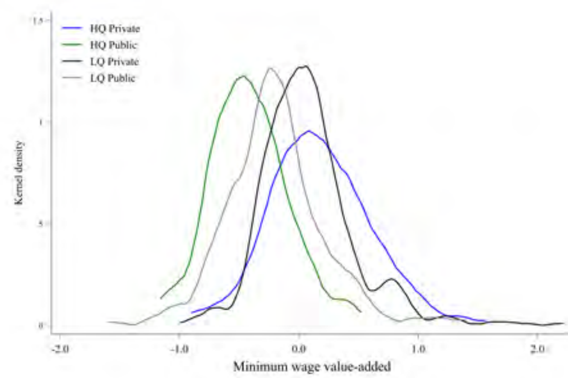
Notes: The figure plots the distribution of college, college-field, college-major, and college-program fixed effects estimated using Specification (1) and Model F where the outcome variable is formal monthly earnings seven years from high school completion, measured in multiples of the monthly minimum wage. The fixed effects are plotted separately by college type. The sample is restricted to students who earned any college degree within seven years from high school.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and PILA (MinSalud).

Figure C.8: Figure C.5 Conditional on Earning a Bachelor's Degree

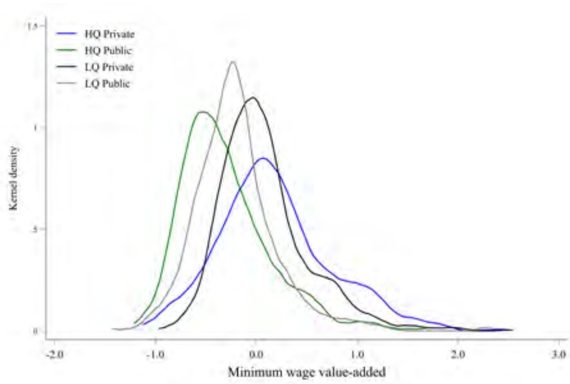
(a) College Fixed Effects



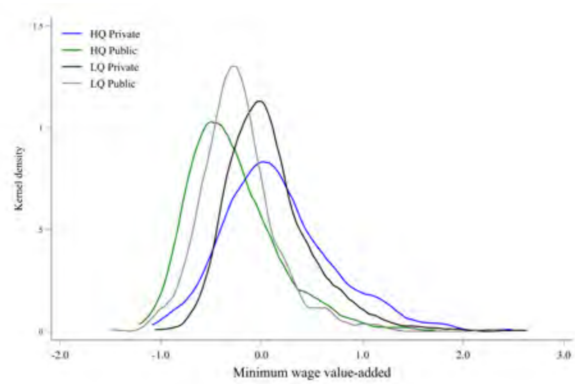
(b) College-Field Fixed Effect



(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 formal monthly earnings seven years from high school completion, measured in multiples of the monthly minimum wage. The fixed effects are plotted separately by college type. The sample is restricted to students who earned a bachelor's degree within seven years from high school.
Sources: Authors' calculations based on SABER 11 (ICFES), SISBEN (DNP), and PILA (MinSalud).

Table C.1: The Effect Explained by College Fixed Effects

	Running variable					
	Panel A: SABER 11			**Panel B:** SISBEN		
	Coef.	SE	N	Coef.	SE	N
	(1)	(2)	(3)	(4)	(5)	(6)
Any degree attainment	0.031	(0.013)	133,158	0.075	(0.026)	19,786
Attainment VA: A	0.015	(0.002)	133,158	0.028	(0.005)	19,786
Attainment VA: B	0.012	(0.002)	133,158	0.025	(0.005)	19,786
Attainment VA: C	0.004	(0.002)	133,158	0.016	(0.004)	19,786
Attainment VA: D	0.023	(0.002)	133,158	0.030	(0.004)	19,786
Attainment VA: E	-0.026	(0.002)	133,158	-0.012	(0.004)	19,786
Attainment VA: F	-0.017	(0.002)	133,158	-0.006	(0.004)	19,786
Bachelor's degree attainment	0.060	(0.016)	69,701	0.078	(0.022)	17,891
Bachelor's attainment: A	0.032	(0.003)	69,701	0.047	(0.004)	17,891
Bachelor's attainment: B	0.028	(0.003)	69,701	0.043	(0.004)	17,891
Bachelor's attainment: C	0.022	(0.002)	69,701	0.035	(0.004)	17,891
Bachelor's attainment: D	0.028	(0.002)	69,701	0.039	(0.004)	17,891
Bachelor's attainment: E	0.023	(0.002)	69,701	0.036	(0.004)	17,891
Bachelor's attainment: F	0.012	(0.002)	69,701	0.025	(0.003)	17,891
SABER PRO score	0.058	(0.019)	35,981	0.015	(0.034)	12,680
SABER PRO score VA: A	0.087	(0.004)	35,977	0.047	(0.007)	12,680
SABER PRO score VA: B	0.087	(0.004)	35,977	0.047	(0.007)	12,680
SABER PRO score VA: C	0.088	(0.004)	35,977	0.051	(0.007)	12,680
SABER PRO score VA: D	0.108	(0.005)	35,977	0.064	(0.009)	12,680
SABER PRO score VA: E	0.109	(0.005)	35,977	0.066	(0.009)	12,680
SABER PRO score VA: F	0.133	(0.006)	35,977	0.081	(0.010)	12,680
Employment	0.044	(0.014)	287,560	0.032	(0.023)	21,534
Employment VA: A	0.025	(0.003)	287,560	0.027	(0.004)	21,534
Employment VA: B	0.027	(0.003)	287,560	0.029	(0.004)	21,534
Employment VA: C	0.021	(0.003)	287,560	0.024	(0.004)	21,534
Employment VA: D	0.093	(0.004)	287,560	0.080	(0.007)	21,534
Employment VA: E	0.182	(0.006)	287,560	0.166	(0.010)	21,534
Employment VA: F	0.044	(0.003)	287,560	0.043	(0.005)	21,534
Earnings (in min wage)	0.160	(0.030)	287,560	0.193	(0.064)	21,534
Earnings VA: A	0.115	(0.006)	287,560	0.129	(0.010)	21,534
Earnings VA: B	0.117	(0.006)	287,560	0.131	(0.010)	21,534
Earnings VA: C	0.108	(0.006)	287,560	0.124	(0.010)	21,534
Earnings VA: D	0.190	(0.007)	287,560	0.187	(0.013)	21,534
Earnings VA: E	0.326	(0.011)	287,560	0.317	(0.019)	21,534
Earnings VA: F	0.125	(0.007)	287,560	0.133	(0.012)	21,534

Notes: This table presents the portion of the effects explained by colleges' educational and labor market productivities 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 C.2: The Effect Explained by College-Field Fixed Effects

	Running variable					
	Panel A: SABER 11			**Panel B:** SISBEN		
	Coef. (1)	SE (2)	N (3)	Coef. (4)	SE (5)	N (6)
Any degree attainment	0.031	(0.013)	133,164	0.078	(0.026)	19,773
Attainment VA: A	0.020	(0.003)	133,164	0.032	(0.006)	19,773
Attainment VA: B	0.018	(0.003)	133,164	0.030	(0.006)	19,773
Attainment VA: C	0.010	(0.003)	133,164	0.020	(0.005)	19,773
Attainment VA: D	0.024	(0.003)	133,164	0.031	(0.005)	19,773
Attainment VA: E	-0.015	(0.003)	133,164	-0.003	(0.005)	19,773
Attainment VA: F	-0.009	(0.003)	133,164	0.002	(0.005)	19,773
Bachelor's degree attainment	0.061	(0.016)	69,667	0.080	(0.022)	17,876
Bachelor's attainment: A	0.040	(0.003)	69,664	0.050	(0.005)	17,876
Bachelor's attainment: B	0.036	(0.003)	69,664	0.047	(0.005)	17,876
Bachelor's attainment: C	0.030	(0.003)	69,664	0.039	(0.005)	17,876
Bachelor's attainment: D	0.036	(0.003)	69,664	0.043	(0.005)	17,876
Bachelor's attainment: E	0.023	(0.003)	69,664	0.032	(0.005)	17,876
Bachelor's attainment: F	0.020	(0.003)	69,664	0.029	(0.005)	17,876
SABER PRO score	0.059	(0.019)	35,974	0.016	(0.034)	12,668
SABER PRO score VA: A	0.087	(0.005)	35,958	0.046	(0.008)	12,667
SABER PRO score VA: B	0.086	(0.005)	35,958	0.046	(0.008)	12,667
SABER PRO score VA: C	0.087	(0.005)	35,958	0.050	(0.008)	12,667
SABER PRO score VA: D	0.090	(0.005)	35,958	0.051	(0.008)	12,667
SABER PRO score VA: E	0.101	(0.005)	35,958	0.061	(0.009)	12,667
SABER PRO score VA: F	0.115	(0.006)	35,958	0.070	(0.009)	12,667
Employment	0.043	(0.014)	287,566	0.030	(0.023)	21,521
Employment VA: A	0.020	(0.003)	287,566	0.022	(0.005)	21,521
Employment VA: B	0.021	(0.003)	287,566	0.024	(0.005)	21,521
Employment VA: C	0.015	(0.003)	287,566	0.019	(0.005)	21,521
Employment VA: D	0.043	(0.003)	287,566	0.040	(0.005)	21,521
Employment VA: E	0.143	(0.005)	287,566	0.130	(0.009)	21,521
Employment VA: F	0.059	(0.003)	287,566	0.063	(0.006)	21,521
Earnings (in min wage)	0.159	(0.030)	287,566	0.194	(0.063)	21,521
Earnings VA: A	0.093	(0.007)	287,566	0.113	(0.013)	21,521
Earnings VA: B	0.096	(0.007)	287,566	0.115	(0.013)	21,521
Earnings VA: C	0.086	(0.007)	287,566	0.107	(0.013)	21,521
Earnings VA: D	0.109	(0.007)	287,566	0.125	(0.013)	21,521
Earnings VA: E	0.262	(0.010)	287,566	0.262	(0.018)	21,521
Earnings VA: F	0.134	(0.008)	287,566	0.156	(0.014)	21,521

Notes: This table presents the portion of the effects explained by colleges' educational and labor market productivities 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 C.3: The Effect Explained by College-Major Fixed Effects

	Running variable					
	Panel A: SABER 11			**Panel B:** SISBEN		
	Coef.	SE	N	Coef.	SE	N
	(1)	(2)	(3)	(4)	(5)	(6)
Any degree attainment	0.031	(0.013)	132,678	0.082	(0.026)	19,637
Attainment VA: A	0.017	(0.003)	132,678	0.027	(0.007)	19,637
Attainment VA: B	0.014	(0.003)	132,678	0.025	(0.007)	19,637
Attainment VA: C	0.006	(0.003)	132,678	0.015	(0.006)	19,637
Attainment VA: D	0.010	(0.003)	132,678	0.018	(0.006)	19,637
Attainment VA: E	-0.012	(0.003)	132,678	-0.001	(0.006)	19,637
Attainment VA: F	-0.004	(0.003)	132,678	0.004	(0.006)	19,637
Bachelor's degree attainment	0.061	(0.016)	69,335	0.082	(0.022)	17,747
Bachelor's attainment: A	0.036	(0.004)	69,332	0.045	(0.006)	17,747
Bachelor's attainment: B	0.032	(0.004)	69,332	0.042	(0.006)	17,747
Bachelor's attainment: C	0.027	(0.004)	69,332	0.034	(0.006)	17,747
Bachelor's attainment: D	0.026	(0.004)	69,332	0.034	(0.006)	17,747
Bachelor's attainment: E	0.016	(0.004)	69,332	0.025	(0.006)	17,747
Bachelor's attainment: F	0.016	(0.004)	69,332	0.026	(0.006)	17,747
SABER PRO score	0.055	(0.019)	35,813	0.018	(0.035)	12,576
SABER PRO score VA: A	0.082	(0.006)	35,731	0.048	(0.009)	12,555
SABER PRO score VA: B	0.082	(0.006)	35,731	0.047	(0.009)	12,555
SABER PRO score VA: C	0.083	(0.005)	35,731	0.051	(0.009)	12,555
SABER PRO score VA: D	0.089	(0.006)	35,731	0.054	(0.010)	12,555
SABER PRO score VA: E	0.103	(0.006)	35,731	0.067	(0.010)	12,555
SABER PRO score VA: F	0.109	(0.006)	35,731	0.069	(0.010)	12,555
Employment	0.043	(0.014)	287,080	0.028	(0.023)	21,385
Employment VA: A	0.021	(0.004)	287,080	0.016	(0.006)	21,385
Employment VA: B	0.022	(0.004)	287,080	0.018	(0.006)	21,385
Employment VA: C	0.016	(0.003)	287,080	0.013	(0.006)	21,385
Employment VA: D	0.035	(0.003)	287,080	0.026	(0.006)	21,385
Employment VA: E	0.096	(0.004)	287,080	0.081	(0.008)	21,385
Employment VA: F	0.046	(0.003)	287,080	0.045	(0.006)	21,385
Earnings (in min wage)	0.160	(0.030)	287,080	0.192	(0.063)	21,385
Earnings VA: A	0.100	(0.009)	287,080	0.112	(0.017)	21,385
Earnings VA: B	0.102	(0.009)	287,080	0.114	(0.017)	21,385
Earnings VA: C	0.093	(0.009)	287,080	0.106	(0.017)	21,385
Earnings VA: D	0.111	(0.009)	287,080	0.119	(0.017)	21,385
Earnings VA: E	0.196	(0.010)	287,080	0.196	(0.019)	21,385
Earnings VA: F	0.123	(0.009)	287,080	0.140	(0.017)	21,385

Notes: This table presents the portion of the effects explained by colleges and majors' educational and labor market productivities 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 C.4: The Effect Explained by College-Program Fixed Effects

	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)	130,353	0.079	(0.026)	19,463
Attainment VA: A	0.020	(0.004)	130,353	0.030	(0.007)	19,463
Attainment VA: B	0.018	(0.004)	130,353	0.028	(0.007)	19,463
Attainment VA: C	0.009	(0.004)	130,353	0.018	(0.007)	19,463
Attainment VA: D	0.007	(0.004)	130,353	0.016	(0.007)	19,463
Attainment VA: E	-0.016	(0.004)	130,353	-0.003	(0.007)	19,463
Attainment VA: F	-0.010	(0.004)	130,353	0.001	(0.007)	19,463
Bachelor's degree attainment	0.063	(0.016)	68,426	0.081	(0.023)	17,601
Bachelor's attainment: A	0.037	(0.004)	68,426	0.046	(0.006)	17,601
Bachelor's attainment: B	0.033	(0.004)	68,426	0.043	(0.006)	17,601
Bachelor's attainment: C	0.027	(0.004)	68,426	0.035	(0.006)	17,601
Bachelor's attainment: D	0.026	(0.004)	68,426	0.035	(0.006)	17,601
Bachelor's attainment: E	0.009	(0.004)	68,426	0.021	(0.006)	17,601
Bachelor's attainment: F	0.011	(0.004)	68,426	0.023	(0.006)	17,601
SABER PRO score	0.054	(0.019)	35,489	0.022	(0.034)	12,485
SABER PRO score VA: A	0.081	(0.006)	35,371	0.043	(0.010)	12,461
SABER PRO score VA: B	0.081	(0.006)	35,371	0.043	(0.010)	12,461
SABER PRO score VA: C	0.082	(0.006)	35,371	0.046	(0.010)	12,461
SABER PRO score VA: D	0.085	(0.006)	35,371	0.047	(0.010)	12,461
SABER PRO score VA: E	0.104	(0.006)	35,371	0.065	(0.010)	12,461
SABER PRO score VA: F	0.108	(0.006)	35,371	0.065	(0.011)	12,461
Employment	0.043	(0.014)	284,755	0.032	(0.023)	21,211
Employment VA: A	0.023	(0.004)	284,755	0.017	(0.006)	21,211
Employment VA: B	0.024	(0.004)	284,755	0.018	(0.006)	21,211
Employment VA: C	0.018	(0.004)	284,755	0.013	(0.006)	21,211
Employment VA: D	0.029	(0.004)	284,755	0.020	(0.006)	21,211
Employment VA: E	0.071	(0.004)	284,755	0.057	(0.007)	21,211
Employment VA: F	0.038	(0.004)	284,755	0.036	(0.006)	21,211
Earnings (in min wage)	0.159	(0.030)	284,755	0.185	(0.064)	21,211
Earnings VA: A	0.108	(0.009)	284,755	0.112	(0.018)	21,211
Earnings VA: B	0.109	(0.009)	284,755	0.114	(0.018)	21,211
Earnings VA: C	0.100	(0.009)	284,755	0.106	(0.018)	21,211
Earnings VA: D	0.105	(0.009)	284,755	0.109	(0.018)	21,211
Earnings VA: E	0.160	(0.010)	284,755	0.157	(0.018)	21,211
Earnings VA: F	0.113	(0.009)	284,755	0.127	(0.018)	21,211

Notes: This table presents the portion of the effects explained by colleges and programs' educational and labor market productivities 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).