Who benefits from tuition-free, top-quality universities? Evidence from Brazil☆

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ARTICLE INFO

ABSTRACT

This paper investigates the long-term impact on earnings of attending a tuition-free, top-quality university in Brazil. We identify the causal effect through a sharp discontinuity in an admission process based on test scores. If admitted, low-income students are found to increase their earnings by 26% ten years later. However, admission has a small and insignificant effect on high-income students. The difference between income groups is not explained by educational attainment, program choice, or selection into better-paying jobs. The evidence suggests that most low-income applicants, if not admitted, still graduate from college but with much lower returns to education. High-income applicants who just miss the cutoff, however, can find other opportunities such that earnings trajectories are unchanged. Our results underscore the role of affordable higher education in promoting social mobility.

1. Introduction

The number of students pursuing higher education (HE) has rapidly grown worldwide, powered by a rising demand from low-income, first-generation students (Marginson, 2016). However, public and non-profit universities have struggled to accommodate this rising demand. According to UNESCO (2017), underprivileged students have disproportionally enrolled in private for-profit institutions that charge higher than average tuition fees but deliver a lower than average quality. If promoting equal opportunities and social mobility are goals of government-funded universities (Chetty et al., 2017), it is imperative to understand how access to subsidized institutions compares to the best outside option available for different groups.

To examine this issue, we estimate the effect of admission to a tuition-free, highly-selective institution on the future earnings of low- and high-income students in Brazil. Brazilian public universities have never charged tuition fees, and the number of qualified applicants has been greater than their capacity since the late 1960s (Binelli & Menezes-Filho, 2019; de Mello e Souza, 1991). As test scores on admission exams were used to allocate the limited spots, only the students with the highest test scores have been admitted. As a result, elite groups have been historically over-represented in these institutions, as access to high-quality basic schooling and private preparatory programs provides advantages in the admission process. In contrast, most college students from low-income backgrounds attend for-profit institutions where they must pay tuition (McCowan, 2007; Ristoff, 2014; World Bank, 1986).[1]

To estimate the impact of attending a public institution in Brazil, we exploit a discontinuity in the admission for the *Universidade Federal de Pernambuco* (UFPE), a flagship university in the Northeast — the region with the highest income inequality in the country.[2] Data come

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[1] This gap began to close with the implementation of quotas in public universities for Afro-descendants, indigenous peoples, and students from public high schools in the 2010s (Mello, 2022).

[2] UFPE is considered a flagship university because it is the one receiving the most support from the federal government in the state of Pernambuco. Moreover, it has the second largest budget among public universities in the Northeast.

from cohorts of applicants in 2006 and 2007. Given its high quality, reputation, and policy of free tuition, this university is a top choice for students in the region, with only 10% of the applicants admitted. Its admission process is strictly based on the candidates' scores in an entrance exam, which they take all at the same time, only once a year. To take the exam, candidates must first apply to a subject-specific program of study at the university. They cannot apply to more than one program in the same year. After the exam, the highest-scoring applicants are invited to join the program until all the available spots are filled.

In each program of study, we compare the last student invited to enroll with the first student left out, for a set of outcomes related to earnings ten years after the entrance exam, in 2016 and 2017. By applying a regression discontinuity design (RDD), we find that the salary of low-income students employed in the formal sector increases by 26% ten years after admission.formalintro On the contrary, the salary of high-income students is barely affected. We further show that the effect on low-income students is higher in programs with less competitive admission. Namely, low-income students with lower test scores, whose college options are more restricted, are more affected. These results suggest that the average alternative to attending a selective tuition-free university is considerably worse for low-scoring applicants with limited resources.herefore, expanding access to affordable quality HE would have a greater impact on these students than on wealthier or higher performing students.

Our findings are robust to the use of alternate bandwidths around the admission cutoff, other than the one calculated by Calonico-Cattaneo-Titiunik (CCT) procedure (Calonico et al., 2014). We also show that predetermined characteristics are balanced around the cutoff and find no evidence of density discontinuity, as confirmed by the McCrary's 2008 test. While women make up a larger share of the student body, we do not find significant heterogeneity by gender in the returns to admission to the selective university in earnings. Moreover, admission is found to have no significant impact on formal employment, job tenure, and work experience in the long run for either students from low-income or high-income families.

As far as our data permit, we investigate four reasons for the difference in wage premia between income groups. First, the effect on high-income students will be null if non-admitted candidates apply again in the following years and eventually graduate from the flagship university. Hence, in the high-income group, both treated and untreated groups would hold the degree from the same university. Our findings, though, do not support this hypothesis. Once admitted, high-income students are 60 percentage points (p.p.) more likely to ever graduate at the flagship, which is similar to the effect on low-income students.

Second, the effect on low-income students will be positive if non-admitted candidates are less likely to hold a college degree in the future. Namely, a difference in educational attainment would explain the effect of admission on low-income students. Our findings confirm that low-income students are 8.5 p.p. more likely to have a college degree if admitted to the tuition-free university. For high-income students, the effect is close to zero. Despite the difference between low- and high-income groups, the effect on the educational attainment of low-income students is not large enough to explain the effect on future earnings.[3]

The third potential mechanism is related to program choice. It could be the case that low-income students enroll in programs in which UFPE offers a higher wage premium. Indeed, compared to applicants' outside option, programs with a higher presence of low-income students yield higher returns to admission at UFPE than other programs. However,

within these programs, the effect on the formal earnings of low-income students is six times as large as the effect on the high-income group. In other words, high-income applicants have better outside options than the low-income ones even among those who apply to the same programs.

Finally, it might be the case that non-admitted candidates pursue a very different career path when they enroll in other institutions. Accordingly, the effect on future earnings would be determined by differences in employers and occupations. Although admission to the flagship university increases the chances of students finding a top-paying job, the average effect on the earnings of low- and high-income groups does not considerably change after disentangling it from occupation and firm effects.[4]

These results are consistent with the idea that affordable quality education improves and/or signals the skills of students, with large impacts for individuals from low-income backgrounds.[5] In the context of Brazil, the universal tuition-waiver policy in public institutions benefits low-income students, giving them an outstanding opportunity for social mobility. For high-income applicants, though, admission to the tuition-free university has insignificant effects, possibly due to the applicants' access to better private education or more expansive social networks (or both). A question left for future research is whether the universal tuition waiver is the most effective instrument to attract the highest performing students to public universities. Another limitation of our study regards the unexplored heterogeneity across students, programs, and outside options. In this paper, we only focus on the average gap between students' best option and second-best option, but we do not examine how free access to quality education improves the matching between students and degrees.

Our work is related to a large literature on the labor market returns to attending selective institutions. A number of studies show they provide higher returns (Andrews et al., 2016; Black & Smith, 2004, 2006; Dale & Krueger, 2002, 2014) and explore different mechanisms such as differences in formal employment rates and the quality of peers (Anelli, 2020; Jia & Li, 2021), quality and type of major (Anelli, 2020; Hastings et al., 2013), human capital and signaling (Hoekstra, 2009), and peer ties (Zimmerman, 2019). Their evidence indicates that the effects are mainly concentrated among men and more privileged students. Notably, in these other studies, universities are not free, and tuition fees often play a distinct role. The present study shows that low-income students, both men and women, benefit most from attending a free top-quality university.

Our study complements other scholarship in Brazil that has examined the impact of admission via the implementation of quotas in public universities for Afro-descendants, indigenous peoples and students from public high schools starting in the mid 2010s. Using data from another public university in Brazil, Francis-Tan and Tannuri-Pianto (2018) examine whether admission affects the future earnings of racial quota applicants. Their results also demonstrate high returns for populations that were historically less represented, finding that Afro-Brazilian students increase educational attainment and earnings eight years after admission through affirmative action (AA). Our study, focusing on income background rather than race, goes one step further and shows that the wage premium for underrepresented groups is not fully explained by educational attainment or by occupation and firm selection.

Our study also complements the work of Mello (2022) and Otero et al. (2021). Both papers show that the AA policy in Brazil expanded

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[3] The effect of admission on educational attainment is not higher because the studied period coincides with the expansion of a student loan program in Brazil named FIES (*Fundo de Financiamento Estudantil*), which helped low-income students to pay for private institutions.

[4] The fixed effects for occupations and firms are calculated using the universe of workers employed by firms directly or indirectly connected to the applicants in our sample. The estimated model also controls for worker fixed effects.

[5] Unlike MacLeod et al. (2017), we cannot separate the effect of acquired skills from the effect of signaled skills and reputation.

the access of historically excluded populations and low-income students to public universities. In particular, Mello (2022) shows that the AA policy has offset a centralized application policy that increased the number of more privileged students in tuition-free institutions. On the other hand, Otero et al. (2021) conclude that the AA policy had negligible effects on students' income. Their study, though, does not estimate the effect on the observed salaries of treated students. Instead, they estimate the effect on predicted earnings, which are obtained from the correlation between studied major and salary in a pre-policy cohort. Using an exogenous variation in admission, the present paper indicates that the effect on the observed earnings of low-income students is rather large and significant.

Finally, our study contributes to a growing literature on academic mismatch and intergenerational mobility. Our findings are linked with descriptive evidences from Hoxby and Avery (2013) showing that high-achieving students from low-income families usually do not apply to elite colleges, but those who matriculate achieve equivalent academic progress as their high-income counterparts. We are in line with Chetty et al. (2017, 2020) by showing with causal evidence that low-income students have high returns to attending elite higher-education institutions. Unlike these studies, we show that, for these high-performing applicants, the impact of attending a top-quality public university in Brazil is larger for those with lower test scores in the low-income group.

The remainder of the paper is structured as follows. Section 2 presents the institutional background, with details on the HE policy in Brazil and the admission process in the public university. Section 3 describes the data sources, constructed variables, and sample. Section 4 explains the empirical strategy. In Section 5, we discuss the main empirical results and explore some potential mechanisms. Section 6 concludes the paper.

2. Institutional background

Brazil has one of the highest income inequality in the world, and unequal access to education has long been pointed as the main determinant (Fishlow, 1972; Lam & Levison, 1991; Langoni, 1973). Unlike other Latin American countries, when Brazil was a colony, the creation of universities was forbidden. Then, higher education was a privilege for those who could afford studying abroad (de Mello e Souza, 1991). With the creation of public, tuition-free universities in the 1950s and 1960s, the cost of attending college reduced drastically. However, access to those institutions remained unequal.

Since 1911, HE institutions in Brazil were legally mandated to admit students using an entrance exam, called *vestibular*. The purpose was to establish the minimum knowledge required for candidates to enter college. In 1960s, however, the number of eligible candidates exceeded the number of places available, and public universities did not expand their capacity as fast as demand. At the same time, regulations on the establishment of private institutions were relaxed. According to Schwartzman (1988), three groups were established in the 1980s: research-oriented public universities with 14,000 faculty members with doctoral degrees and 40,000 graduate students; other public universities with 45,000 full-time instructors, holding some academic degree, serving 450,000 students; and private institutions with 60,000 instructors, mostly adjuncts and without academic qualification, serving 850,000 students.

The combination of better faculty, no-tuition, and admission criteria resulted in students from the top 20% of the income distribution taking half the spaces in public universities. The bottom decile comprised only 1% of the students (World Bank, 1986). Despite some improvements promoted by AA policies, the social gap in public institutions persists (Ferreyra et al., 2017; Ristoff, 2014). Between 2004 and 2015, the share of college students in public institutions did not change, staying around 24%. However, most of the growing demand in the four lowest income quintiles was absorbed by private institutions, while most of the growing demand in the top quintile was absorbed by the public

ones.[6] According to Mello (2022), the AA policy adopted in the 2010s increased the number of low socioeconomic status students in public universities, but the concurrent introduction of a centralized admission process have offset this impact.

2.1. The flagship university

Founded in 1948, UFPE is a flagship university in the Northeastern region of Brazil and one of the top ten tuition-free institutions in the country. According to the Ministry of Education, UFPE has consistently had the highest assessments in the North and Northeast since 1995. Given its high quality and reputation, it is the top choice of almost every high school student in the state of Pernambuco. Table A1 of the Appendix shows the ranking of higher education institutions in Pernambuco; UFPE stands out for its quality and free tuition. Depending on the program of study, time to graduation at UFPE varies between three and six years, with most programs requiring at least four years.

Its main campus is located in the metropolitan area of Recife (MAR), and 84% of its candidates come from this area. MAR is the largest, second-richest, and second-most unequal metropolitan area in the Northern and Northeastern regions.[7] It had more than 4 million people and only 13% of the population between 16 and 24 years old were enrolled in tertiary education in 2016. From the population of college students, 70% were enrolled in private institutions.

2.1.1. Admissions process

About 95% of its undergraduate students are admitted through the *vestibular*, which is held only once a year.[8] Some 68% of the candidates are students who have recently graduated from high school. Half of them are taking the *vestibular* for the first time and the other half are retaking it because they were not admitted the year before. The minority consists of candidates who came from other institutions or study programs (12%), graduated from the adult education program (2.5%), or have not studied for a while (17.5%). In fact, anyone with a high school diploma or equivalent can apply to the university; the chances of being accepted depend uniquely on the test score.

The admissions process requires candidates to choose their study program when they apply. That is, they are not admitted to the university as a whole, but to a particular undergraduate program offered by the institution. They cannot apply to multiple programs in the same year. To switch programs, the student has to retake the *vestibular* and compete for a place in the new program. A very few students, less than 5%, who have already been enrolled in an HE institution are able to skip the entrance exam and join a program that is short of junior and senior students.

With the vestibular serving as the singular criteria for admission, applying to UFPE was very time-consuming. The *vestibular* had two rounds, both lasting about a week, requiring applicants to travel to Recife (or another campus) twice. The first round assessed students' general knowledge and eliminates about 40% of the candidates.[9] In the second round, the remaining candidates were tested in Portuguese, a foreign language, and three other subjects that were particularly required for the study program. The final admission score was a weighted average of the first- and second-round scores. Finally, each program admitted those candidates with the best final scores until all the places were taken. On average, only 10% of the applicants were admitted per program.

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[6] Source: SEDLAC.

[7] Source: IBGE.

[8] In 2015, all programs began to adopt the new national entrance process (the Unified Selection System, SISU) to public universities in Brazil, ending institution-specific exams.

[9] Since 2010, the first round has been replaced by the National High School Exam (ENEM), which has a similar structure and does not require applicants to travel to Pernambuco.

3. Data and descriptive statistics

3.1. Data sources and sample

Our data come from three different sources. The first is the admission committee (COVEST), which provides information on every applicant in 2006 to 2007. The second is UFPE's Academic Information System (*Sistema de Informações e Gestão Acadêmica*, SIGA), which provides information on students' enrollment, grades and status. The third is the Annual Social Information Report (*Relação Anual de Informações Sociais*, RAIS) from the Ministry of Labor, which contains information on every registered employee in Brazil.

3.1.1. Applications and admission score

The COVEST data include the test scores from the first and second rounds and the final admission score. We standardize the admission score using the admission cutoff — i.e., the final score of the last student enrolled in the program — and the standard deviation per program and year. The COVEST data also include the number of times each candidate did the entrance exam in the past, motivation to enter the program, previous studies, and a long list of characteristics, such as age, gender, race, employment, and parents' education.

Following a definition from the Ministry of Education, we classify as 'low-income' those that report a monthly household income below 1,000 BRL, which corresponds to 5,500 USD a year. In 2016, about 54% of households in Brazil were below this income level. Also, we classify as 'high-income' those with monthly household income above 2,000 BRL (11,000 USD/year). In the whole country, only 21% of the households had income above this threshold in 2016. As an robustness check, we also group candidates based on parental education (having a college degree or not).

3.1.2. College enrollment and transcripts

SIGA provides detailed information on all the students enrolled in 2002–2014, regardless of when they enter and leave the institution. With these data, we verify whether admitted students enrolled in the university in the same year they originally applied. This information is used to measure compliance to the treatment. We also check whether and when a student graduated from this institution.

3.1.3. Earnings, occupation, and educational attainment

In Brazil, every registered firm is legally required to annually report every worker employed in the previous year, with information on salary, number of months worked, and education level. This information is available on RAIS. Using students' social security numbers (*Cadastro de Pessoa Física*, CPF), we match the two previous data sources with RAIS to obtain their earnings, occupation, and years of schooling for every year from 2002 to 2017. For the students who graduated from UFPE, according to SIGA, 96% are reported to hold a college degree on RAIS.

Individual earnings are calculated as the sum of all salaries received within 12 months, deflated to December 2017 using the Extended Consumer Price Index (IPCA). For each year, individuals are considered formally employed if they are found on RAIS, regardless of how many weeks and hours they worked.[10] For each employed worker, we also calculate their experience (number of months employed since their first formal occupation) and tenure (number of months working for their current employer).

In this paper, all the results, including the ones on educational attainment, are restricted to applicants who are found to be formally employed in the future. Therefore, we do not estimate the effect of

admission on student's total income, which would also include earnings from informal jobs and self-employment. According to the Brazilian National Household Survey (*Pesquisa Nacional por Amostra de Domicílios*, PNAD), this restriction implies that we potentially ignore the earnings of 24% of the applicants.[11] However, we show in Section 5.1 that the restriction is not a concern because the share of students formally employed is balanced at the admission cutoff. Similarly, for educational attainment, the analysis is restricted to students who were formally employed.

Another concern is whether formal employees complement their salaries with informal and entrepreneurial earnings. According to PNAD, less than 3% of the formal employees in Pernambuco, who hold a high school degree, have a second job as an informal employee or self-employed.

3.2. Descriptive statistics

Table 1 displays descriptive statistics split in three groups: all candidates, admitted candidates, and non-admitted candidates. The group of non-admitted candidates only includes those who passed to the second round of the entrance exam. Even so, less than 20% of the applicants in our sample enrolled at the flagship university, but about 85% enrolled in some HE institution. Among admitted students, 86% enrolled at the flagship university after admission. These numbers confirm the high level of selectivity of the university.

Compared to non-admitted candidates, those admitted are less likely to come from a public high school and from the low-income group. Also, their parents are more likely to have a college degree. Ten years later, the earnings of admitted students are 40% higher than the earnings of non-admitted candidates.

4. Empirical strategy

4.1. General framework

Estimating credible effects of attending a highly selective university is difficult due to many sources of selection bias. Students' observed and unobserved traits are correlated with this opportunity. To circumvent these challenges, we apply an RDD at the admission cutoff (Hahn et al., 2001; Imbens & Lemieux, 2008). Since admission is strictly based on a test score, taken only once a year, the last student admitted to the university is very similar to the first candidate left out. The only difference between them is the right to attend the flagship institution.

However, the candidates do not apply to the university as a whole. Instead, they apply to one (and only one) of its undergraduate programs. Given that each program has a different cutoff every year, we follow Pop-Eleches and Urquiola (2013) and Zimmerman (2019) and stack the sample across years and programs. Then we standardize the admission scores within year-program cohorts so that each cutoff is equal to zero. According to Cattaneo et al. (2016), this standardizing-and-pooling approach yields consistent estimates for the local average treatment effect (LATE).

Formally, let x_{ikt} be the admission score of candidate i who applies to program k in year t, and $\underline{x}_{kt}$ be the score of the last student joining this program that year. If $x_{ikt} \geq \underline{x}_{kt}$, then the candidate may enroll in the university. But if $x_{ikt} < \underline{x}_{kt}$, then the candidate cannot, under any circumstances, start the program s/he applied to. Let y_{ikt} be the future log earnings of candidate i, who applies to program k in year t. This variable may also represent any other future outcome, such as educational attainment and work experience. Then the LATE of

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[10] Workers that are not found on RAIS are not necessarily unemployed. They can also be unregistered. In either case, we consider their absence from the records as a sign of underemployment.

[11] If we only consider workers in the state of Pernambuco, who are 25 years or older, and who attended at least one year of HE, about 5% of them are informal employees, 7% are informally self-employed, 12% own a formal business, and 9% are unemployed.

Table 1
Descriptive statistics.

	All		Admitted		Non-admitted	
	mean	s.d.	mean	s.d.	mean	s.d.
	(1)	(2)	(3)	(4)	(5)	(6)
Standardized admission score	−0.901	1.127	0.716	0.634	−1.332	0.786
Enrolled after admission (same year)	0.182	0.386	0.862	0.345	0.000	0.000
Graduated at flagship university*	0.131	0.337	0.620	0.485	0.000	0.014
Enrolled in some college*	0.847	0.360	0.918	0.275	0.827	0.378
Graduated in some college*	0.723	0.448	0.838	0.369	0.690	0.463
Formally employed*	0.641	0.480	0.685	0.465	0.629	0.483
Log earnings**	10.43	0.983	10.73	0.929	10.34	0.981
Migration**	0.183	0.387	0.203	0.402	0.178	0.382
Female	0.578	0.494	0.542	0.498	0.587	0.492
Age	19.95	2.565	20.08	2.448	19.91	2.594
Number of previous attempts	1.896	0.965	2.122	0.968	1.835	0.955
Attended preparatory program	0.504	0.500	0.607	0.489	0.477	0.499
Living in metropolitan area	0.885	0.319	0.923	0.267	0.875	0.331
Attended public high school	0.248	0.432	0.237	0.425	0.251	0.434
Income brackets						
Low income (< 1,000 BRL/month)	0.380	0.485	0.326	0.469	0.395	0.489
High income (> 2,000 BRL/month)	0.334	0.471	0.388	0.487	0.319	0.466
One parent has college degree	0.453	0.498	0.509	0.500	0.439	0.496
One parent is underemployed	0.199	0.399	0.175	0.380	0.205	0.403
One parent is an entrepreneur	0.205	0.404	0.226	0.418	0.200	0.400
Main reason to choose program of study						
Career prestige	0.028	0.165	0.016	0.125	0.031	0.174
Quality of the program	0.086	0.281	0.088	0.284	0.086	0.280
Personal self-fulfillment	0.546	0.498	0.579	0.494	0.537	0.499
Other reasons	0.340	0.474	0.317	0.465	0.346	0.476
Main reason to apply to flagship university						
No tuition fees	0.369	0.483	0.347	0.476	0.375	0.484
University prestige	0.273	0.446	0.297	0.457	0.267	0.442
Other reasons	0.358	0.479	0.356	0.479	0.358	0.479
Number of observations	20,827		4,390		16,437	

This table presents the mean and standard deviation (s.d.) of dependent variables and covariates used in this study. The sample excludes applicants who did not pass to the second round of the admission exam. The sample is also split between applicants that were 'admitted' and 'non-admitted' after the admission exam. * in the tenth year after applying to the university. ** if employed in the tenth year after applying to the university.

admission to a candidate's earnings is given by the following sharp regression discontinuity (SRD) estimand:

$$\tau_{SRD} = \lim_{x \downarrow \underline{x}} E\left(y_{ikt}|x_{ikt} \geq \underline{x}_{kt}\right) - \lim_{x \uparrow \underline{x}} E\left(y_{ikt}|x_{ikt} < \underline{x}_{kt}\right). \quad (1)$$

Since not every admitted candidate enrolls in the university, the LATE of enrollment in the flagship university is given by the following fuzzy regression discontinuity (FRD) estimand:

$$\tau_{FRD} = \frac{\tau_{SRD}}{\lim_{x \downarrow \underline{x}} \Pr\left(z_{ikt} = 1|x_{ikt} \geq \underline{x}_{kt}\right) - \lim_{x \uparrow \underline{x}} \Pr\left(z_{ikt} = 1|x_{ikt} < \underline{x}_{kt}\right)}, \quad (2)$$

where z_{ikt} is equal to one if candidate i enrolls in program k in year t and zero otherwise, and $\Pr(.)$ is a probability function. As shown in Fig. 1, the probability of enrollment below the cutoff, $\Pr\left(z_{ikt} = 1|x_{ikt} < \underline{x}_{kt}\right)$, is zero. Above the cutoff, the probability is around 85%.

Identifying these LATEs relies on the fact that students around the cutoff are similar in every aspect. Still, a few issues, if existed, could violate this condition. First, the performance in the admission exam might depend on how far the candidates are from the cutoff. Second, non-admitted students could take the exam multiple times until they pass. Third, the university could apply a second admission criterion based on soft information. Fourth, other institutions could use the admission score for the flagship university as an admission criterion, making admitted candidates more likely to reject the flagship offer than the non-admitted.

Fortunately, none of these issues applies to our setting. All the candidates take the exam at the same time, nobody can retake it in the same year, and the cutoff is unknown until all the scores are released. Also, for any institution, applying an admission criterion other than its own admission score is against the law. The admission process in higher

education in Brazil is transparent, requiring all institutions to publicly disclose the ranking of candidates.

Despite the favorable institutional framework, the sample must still satisfy local continuity assumptions to validate the RDD. Accordingly, we apply the McCrary (2008) test for density continuity. Figure A1 of the Appendix shows no evidence for a discontinuity in the density of candidates at the cutoff. Moreover, Table A2 shows that no covariate measured at the time of the exam is significantly discontinuous. Therefore, candidates' characteristics look balanced at the cutoff. The same continuity conditions also hold for the sub-samples that we study — i.e., low- and high-income groups. For the interpretation of the effects on these groups, it also helps that their enrollment rates are very similar (see Fig. 1).

Another potential issue with our approach is the endogeneity of the admission cutoff. According the de Chaisemartin and Behaghel (2020), when the cutoff is defined by the last students accepting the offer, the compliance to the offer becomes higher in the treated group than in the untreated one. To re-balance the compliance rate, they point out that excluding the last student enrolling each program from the sample would be enough. Accordingly, for each program cohort, we exclude the applicant whose admission score, x_{ikt}, is equal to the admission cutoff, $\underline{x}_{kt}$.[12]

Although Cattaneo et al. (2016) state that the standardizing-and-pooling approach is consistent, they also argue that settings like ours, including many program cohorts, can be used to consistently examine heterogeneous effects. Similar to Cattaneo et al.'s example of mayoral elections, we further split the sample into types of programs to verify some of the potential mechanisms. Unfortunately, given our sample

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[12] The non-exclusion of these candidates does not change our findings. Results are available upon request.
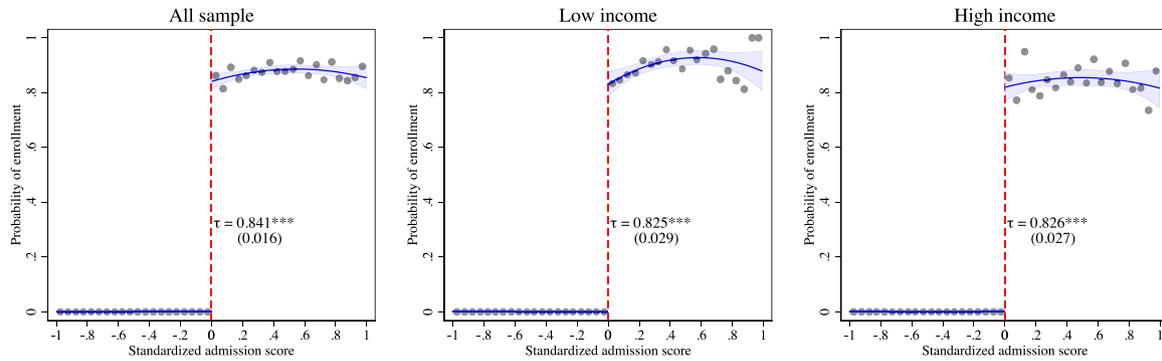
Fig. 1. Relationship between admission score and enrollment.
Admission score is standardized using the admission cutoff and the standard deviation per program and year. The first graph on the left includes all applicants in our sample. In the other two graphs, sample is split based on the income reported on the application. The dependent variable equals 1 if the applicant enrolled at the university in the same year and 0 otherwise. Points are unconditional means of the dependent variable within a range of 0.05 standard deviations of the score. The SRD is estimated using triangular kernel with the bandwidth selected using the CCT procedure. τ is the SRD estimate, with standard errors clustered at the applicant level in parenthesis. ***, **, * represent statistical significance at 1%, 5% and 10% levels, respectively.

size, we cannot split the sample of high- and low-income students into more than three groups without compromising the statistical power of our estimates.

4.2. Regression discontinuity estimator

To estimate LATEs (1) and (2), we apply locally weighted regressions (LWR) with a triangular kernel function as follows. Let $k_{ikt} = \max\left[0, \left(1 - \left|x_{ikt} - \underline{x}_{kt}\right|/b\right)\right]$ be the kernel weight for candidate i, who applies to program k in year t, given a bandwidth b. First, we estimate the following LWR estimator on each side of the cutoff:

$$\hat{\mu}_-^y = (1 \ \ 0)\left(X'W_-X\right)^{-1} X'W_-Y,$$

$$\hat{\mu}_+^y = (1 \ \ 0)\left(X'W_+X\right)^{-1} X'W_+Y.$$

where Y is a $n\times1$ vector of values for y_{ikt}, X is a $n\times2$ matrix of values for $(1, x_{ikt} - \underline{x}_{kt})$, W_- is a $n\times n$ diagonal matrix with diagonal elements equal to $\mathbf{1}\left(x_{ikt} < \underline{x}_{kt}\right) \cdot k_{ikt}$, and W_+ is a $n \times n$ diagonal matrix with diagonal elements equal to $\mathbf{1}\left(x_{ikt} \geq \underline{x}_{kt}\right) \cdot k_{ikt}$.

Then the estimator for the SRD estimand is:

$$\hat{\tau}_{SRD} = \hat{\mu}_+^y - \hat{\mu}_-^y - \hat{B}^y\left(b, b^*\right) \qquad (3)$$

and the estimator for the FRD estimand is:

$$\hat{\tau}_{FRD} = \frac{\hat{\tau}_{SRD}}{\hat{\mu}_+^z - \hat{\mu}_-^z - \hat{B}^z\left(b, b^*\right)}. \qquad (4)$$

where b is the optimal main bandwidth and b^* is the optimal pilot bandwidth. Following Calonico et al. (2014), we select the bandwidth using a minimum square error (MSE) procedure. The bias estimator, $\hat{B}^z(.)$, adjusts the LWR estimates for a large, MSE-optimal bandwidth. See Calonico et al. (2014) for details of the bias correction and robust variance. Since we use two years of exams, applicants may appear twice in our sample. For this reason, we cluster the standard errors at the applicant level.[13]

To narrow the list of potential mechanisms to viable candidates, some regressions gradually include control variables, as proposed by Calonico et al. (2019). Since some of these covariates can be considered "bad" controls, as defined by Angrist and Pischke (2009), the purpose of the exercise is not pin down the mechanism through which admission affects earnings. Instead, it is only intended to eliminate potential candidates for which inclusion in the model do not change the LATE.

4.3. Decomposition of earnings into firm, position, and worker effects

The effect of admission on future earnings might be driven by the selection of students into better-paying firms or better-paying occupations. To verify this mechanism, we decompose their expected salary into worker, position, and firm fixed-effects using the Abowd-Kramarz-Margolis (AKM) model (Abowd et al., 1999).

In this model, the log wage of student i working for firm j with position g in year T, w_{iT}, is determined as follows:

$$w_{iT} = \theta_T + \psi_{i,gj(i)} + \varepsilon_{iT}, \qquad (5)$$

where θ_T is the year-specific effect, ε_{iT} is a zero-mean random term, and $\psi_{i,gj(i)}$ is the worker–firm-position fixed effect, which is further decomposed into three effects:

$$\psi_{i,gj(i)} = \alpha_i + \gamma_{gj(i)} + u_{i,gj(i)}. \qquad (6)$$

The first term in the above equation, α_i, represents the worker fixed effect; the second term, $\gamma_{gj(i)}$, is the firm-position fixed effect; and the third term, $u_{i,gj(i)}$, is an orthogonal residual representing the effect of matching worker with firm and position.

We further decompose the firm-position fixed effect, $\gamma_{gj(i)}$, into three components:

$$\gamma_{gj(i)} = \lambda_{j(i)} + \eta_{g(i)} + e_{gj(i)}, \qquad (7)$$

where $\lambda_{j(i)}$ is the firm fixed effect, $\eta_{g(i)}$ is the position effect, and $e_{gj(i)}$ is an orthogonal term that captures the interaction between firm and position.

The three equations above are estimated in three steps. First, we estimate Eq. (5) using a within-group estimator. In the second and third steps, we estimate Eqs. (6) and (7), respectively, using Guimaráes and Portugal's 2010 two-way fixed effect estimator, weighted by the number of observations in each combination of worker–firm-position and firm-position. These equations are estimated using the full set of workers who have ever worked for firms that have ever employed students in our sample. For those workers, we also consider their salary when employed by other firms.

For our results below, we focus on the effect of admission on three variables: firm compensation, $\lambda_{j(i)}$; firm-position (or job) compensation, which is the sum of the firm-position effect, $\gamma_{gj(i)}$, and the firm-position-worker effect, $u_{i,gj(i)}$; and individual compensation, which is the sum of the worker effect, α_i, and the idiosyncratic compensation ε_{iT}. We acknowledge that our decomposition may suffer from a limited mobility bias. According to Bonhomme et al. (2023), this bias should reduce the contribution of firm effects in explaining earnings inequality. Therefore,
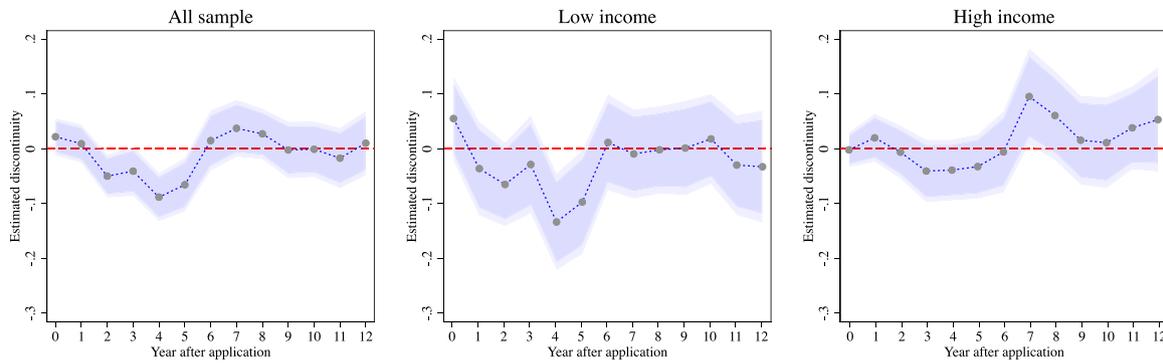
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[13] We also present results excluding the second time the applicant shows up in the sample.

Fig. 2. Effect of admission on formal employment over the years.
This figure presents SRD estimates for the effects of admission on formal employment up to 12 years after application. The first graph on the left includes all applicants in our sample. In the other two graphs, sample is split based on the income reported on the application. The dependent variable equals 1 if applicant is found on RAIS in the given year and 0 otherwise. Each point in these graphs represents a SRD estimate, estimated using triangular kernel with the bandwidth selected using the CCT procedure. All regressions control for gender, year, and program fixed effects. Shaded areas represent robust confidence interval at 90% (darker) and 95% (lighter) levels.

it might be biased towards the effect of admission on firm selection and against the effect on workers' skills. In our estimated model, the firm effects account for 20% of the implied variance, which is consistent with the estimates presented by Gerard et al. (2021).

4.4. Regression discontinuity for quantile treatment effects

After the decomposition described above, we estimate the quantile effects of admission on each variable — i.e., firm compensation, job compensation, and individual compensation. By estimating quantile effects instead of LATEs, we can examine how the distribution of potential employers, job opportunities, and skills changes after admission. The LATE estimates would not tell us whether the effects are driven by improvements at the bottom or at the top of the distribution, but the quantile effects do.

The quantile treatment effects of admission are estimated using Frandsen et al.'s 2012 nonparametric estimator for RDD. This estimator is similar to one described in Section 4.2, expect that $\hat{\mu}_+^y$ and $\hat{\mu}_-^y$ are estimated for different quantiles using Abadie et al.'s 2002 instrumental-variable approach.

5. Results

Our results are divided into three parts. First, we show that the sample of applicants who are formally employed in the future is balanced at the admission cutoff. Then, we present the effect of admission to the flagship university on formal earnings ten years later. Finally, we test for potential mechanisms that could explain this effect. Robustness tests are presented in Appendix.

5.1. Formal employment

Since all the results below are restricted to a sample of applicants found on RAIS, first we must show that the probability of being a formal employee is not affected by admission to the flagship university. In this regard, Fig. 2 presents the LATE of admission on this probability for each year after the entrance exam. That is, each point in these graphs comes from a separate SRD.

The three graphs in Fig. 2 confirm that admission has nearly no effect on formal employment in the long run.[14] This result means that we can confidently compare candidates who were formally employed ten years later in terms of earnings and educational attainment. Still, it is worth stressing that our results and conclusion only consider students who become formally employed, which represent 76% of the labor force with at least one year of college.

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[14] For reference, Table A3 of the Appendix presents the formal employment rates just below the admission cutoff.

Table 2
Effects of admission and enrollment on log earnings ten years after application.

	All	Female	Male
	(1)	(2)	(3)
Panel A. All sample			
Admission	0.150**	0.140*	0.129
	(0.064)	(0.072)	(0.106)
Enrollment	0.178**	0.163**	0.154
	(0.076)	(0.083)	(0.127)
N. of observations	13,352	7,514	5,838
Panel B. Low income			
Admission	0.242**	0.223*	0.302
	(0.108)	(0.116)	(0.203)
Enrollment	0.297**	0.260*	0.400
	(0.132)	(0.134)	(0.266)
N. of observations	5,674	3,397	2,277
Panel C. High income			
Admission	0.014	−0.058	0.116
	(0.110)	(0.147)	(0.158)
Enrollment	0.016	−0.069	0.132
	(0.130)	(0.174)	(0.181)
N. of observations	3,764	1,930	1,834

This table presents the SRD estimates for admission effect and the FRD estimates for enrollment effect on log earnings. Earnings are measured as the sum of all salaries received for 12 months, ten years after the application. Sample is restricted to applicants who were formally employed ten years later. In Panels B and C, sample is split based on the income reported on the application. In columns (2) and (3), samples are restricted based on gender. SRDs and FRDs are estimated using triangular kernel with the bandwidth selected using the CCT procedure. All regressions control for gender, cohort, and program fixed effects. Standard errors, clustered at student level, are in parentheses. ***, **, * represent statistical significance at 1%, 5% and 10% levels, respectively.

5.2. Effect on earnings

The left-hand graph in Fig. 3 shows the relationship between the admission score and formal earnings ten years after applying to college. As expected, this relationship is positive. In addition, we observe a clear discontinuity at the admission cutoff. On average, being admitted to the flagship university increases by 14% the future earnings of candidates who are close to the cutoff.

Beyond the average, the middle graph in Fig. 3 shows that the effect of admission on the earnings of low-income applicants is about 26%, which is twice as high as the average effect. On the other hand, the effect on high-income applicants (right-hand graph) is small and insignificant. In addition to the effect of admission, column (1) of Table 2 presents the FRD estimate for the effect of enrollment for these two groups. If enrolled after admission, the future earnings of low-income students increase by 30%. For high-income students, the wage
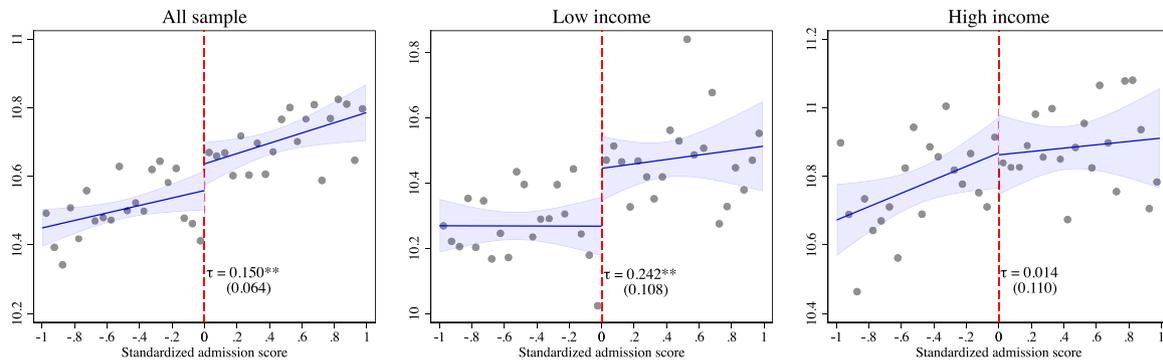
Fig. 3. Relationship between admission score and log earnings ten years after application.
The first graph on the left includes all applicants in our sample who were formally employed ten years after application. In the other two graphs, sample is split based on the income reported on the application. Admission score is standardized using the admission cutoff and the standard deviation per program and year. The dependent variable is the log of annual earnings ten years after candidates applied to the university. Points are unconditional means of the dependent variable within a range of 0.05 standard deviations of the score. The SRD is estimated using triangular kernel with the bandwidth selected using the CCT procedure. All regressions control for gender, year, and program fixed effects. τ is the SRD estimate, with standard errors clustered at the applicant level in parenthesis. ***, **, * represent statistical significance at 1%, 5% and 10% levels, respectively.

Table 3
Effects of admission and enrollment on education and log earnings ten years after application.

	Dependent variable					
	College enrollment	College graduation	Graduation at flagship	Log earnings		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. All sample						
Admission	0.052**	0.038	0.614***	0.127**	0.123**	0.125**
	(0.022)	(0.028)	(0.023)	(0.063)	(0.062)	(0.062)
N. of observations	13,352	13,352	20,790	13,352	13,352	13,352
Enrollment	0.062**	0.045	0.731***	0.150**	0.145**	0.148**
	(0.026)	(0.033)	(0.023)	(0.075)	(0.073)	(0.073)
N. of observations	13,352	13,352	20,790	13,352	13,352	13,352
Panel B. Low income						
Admission	0.107***	0.091*	0.572***	0.207*	0.203*	0.202*
	(0.038)	(0.049)	(0.042)	(0.107)	(0.106)	(0.106)
N. of observations	5,674	5,674	7,866	5,674	5,674	5,674
Enrollment	0.132***	0.113*	0.696***	0.254*	0.250*	0.248*
	(0.046)	(0.060)	(0.044)	(0.131)	(0.129)	(0.129)
N. of observations	5,674	5,674	7,866	5,674	5,674	5,674
Panel C. High income						
Admission	0.007	0.011	0.626***	0.014	0.011	0.011
	(0.031)	(0.036)	(0.035)	(0.113)	(0.112)	(0.111)
N. of observations	3,764	3,764	6,908	3,764	3,764	3,764
Enrollment	0.008	0.014	0.759***	0.016	0.013	0.013
	(0.036)	(0.043)	(0.035)	(0.132)	(0.132)	(0.131)
N. of observations	3,764	3,764	6,908	3,764	3,764	3,764
Control variables						
College enrollment				Yes		Yes
College graduation					Yes	Yes

This table presents the SRD estimates for admission effect and the FRD estimates for enrollment effect on (1) enrollment in some college, (2) graduation in some college, (3) graduation at the flagship university, and (4)-(6) log earnings. Earnings are measured as the sum of all salaries received for 12 months, ten years after the application. Except in column (3), samples are restricted to applicants who were formally employed ten years later. In Panels B and C, sample is split based on the income reported on the application. SRDs and FRDs are estimated using triangular kernel with the bandwidth selected using the CCT procedure. All regressions control for gender, cohort, and program fixed effects. Columns (4)-(6) include additional controls for education. Standard errors, clustered at student level, are in parentheses. ***, **, * represent statistical significance at 1%, 5% and 10% levels, respectively.

premium of attending the flagship university is close to zero. A similar difference is found if we split students based on parental education. Tables A4 of the Appendix shows that the effect is large and significant if neither parent holds a college degree and small and insignificant if either parent holds a college degree.[15]

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[15] If we split the sample based on type of high school (i.e., public or private), we do not find much difference — see Table A5 of Appendix. The reason is that a great number of private schools does not have a high socioeconomic status. Likewise, some public schools are very selective (e.g., military schools), attracting students with high socioeconomic status. See Fontanive et al. (2021).

In columns (2) and (3) of Table 2, we split the sample by gender. The effect on low-income men is about 40%, which is larger than the effect on low-income women, about 26%. Although lacking statistical significance for men, we consider that the effects are large and important for both genders in the low-income group. In the high-income group, neither gender is significantly affected. For the sake of preserving the statistical power, we do not split our results by gender when investigating potential mechanisms.

The results in Fig. 3 and Table 2 are robust to a series of changes in our sample and estimation procedure. All the robustness checks are found in Appendix. For instance, Table A6 shows that the differences between high- and low-income groups persist if our LWR includes

Table 4
Effects of admission and enrollment on log earnings by type of program.

	Share of low-income students		Competition for admission	
	Low	High	Low	High
	(1)	(2)	(3)	(4)
Panel A. All sample				
Admission	0.096	0.364***	0.153	0.207**
	(0.108)	(0.104)	(0.094)	(0.105)
Enrollment	0.117	0.425***	0.177*	0.256**
	(0.131)	(0.121)	(0.107)	(0.130)
N. of observations	6,221	7,131	7,207	6,145
Panel B. Low income				
Admission	0.093	0.495***	0.304**	0.164
	(0.267)	(0.129)	(0.148)	(0.168)
Enrollment	0.123	0.587***	0.370**	0.207
	(0.349)	(0.156)	(0.177)	(0.212)
N. of observations	2,149	3,525	3,229	2,445
Panel C. High income				
Admission	0.081	−0.072	−0.156	0.220
	(0.192)	(0.161)	(0.165)	(0.160)
Enrollment	0.090	−0.087	−0.182	0.259
	(0.217)	(0.194)	(0.194)	(0.189)
N. of observations	1,836	1,928	1,681	2,083

This table presents the SRD estimates for admission effect and the FRD estimates for enrollment effect on log earnings. Earnings are measured as the sum of all salaries received for 12 months, ten years after the application. Sample is restricted to applicants who were formally employed ten years later. In Panels B and C, sample is split based on the income reported on the application. In each column, samples are split based on the characteristics of the program students apply. In columns (1) and (2), the split is at the median share of low-income students admitted to the program. Samples in column (1) include only candidates that applied to programs below this median, while samples in column (2) includes only those above the median. In columns (3) and (4), the split is at the median round-one score of admitted students. Samples in column (3) include only candidates that applied to programs below this median, while samples in column (4) includes only those above the median. SRDs and FRDs are estimated using triangular kernel with the bandwidth selected using the CCT procedure. All regressions control for gender, cohort, and program fixed effects. Standard errors, clustered at student level, are in parentheses. ***, **, * represent statistical significance at 1%, 5% and 10% levels, respectively.

second-degree polynomials. They also persist if we only compare the next-to-last student in the program with the first student out (one-to-one matching).[16] If anything, the point estimates for low-income students are even larger under these alternative models, while the estimates for high-income students remain insignificant. Compared to alternative specifications, the linear (first-degree polynomial) LWR, in Table 2, yields more conservative estimates with lower standard errors.

Furthermore, our results could rather be driven a reduction in earnings right below the admission cutoff (see Fig. 3). To address this issue, we reestimate the effects excluding observations that are too close to the cutoff. Table A7 of the Appendix shows that the resulting estimates are consistent with the main results. Another related concern is that the estimated effects are too sensitive to the bandwidth choice in LWRs. Figure A2 of the Appendix confirms that our estimates for both income groups are robust under smaller and larger bandwidths.

It might also be the case that the earnings discontinuity is observed for other admission scores, which are not the cutoffs. In Figure A3 of the Appendix, we present the *z*-statistics for the estimates using placebo cutoffs. The graphs confirm that our results cannot be replicated by other random thresholds. As a final robustness test, among applicants who appear in our sample twice, we exclude the first time they were not admitted. In addition, we exclude applicants who were admitted twice. Estimates reported in Table A8 of the Appendix are overall similar to our main results.

5.3. Potential mechanisms

In this section, we explore some of the mechanisms that could explain the different effects between low- and high-income groups. We verify four potential channels: educational attainment, program choice,

employment, and access to better-paying jobs. In the Appendix, we also present results controlling for migration — see Table A9. Overall, none of these channels fully explain our main findings.

5.3.1. Educational attainment

The first mechanism to be explored is educational attainment. One may argue that, for low-income applicants, admission to a tuition-free institution is the only opportunity they have to attend college. Thus, the difference in the effect on earnings between groups might be explained by their years of schooling, regardless of the institution these groups attend.

Columns (1) and (2) of Table 3 confirm that the admission to the flagship university does not make high-income applicants more likely to go to college. If a low-income applicant is admitted, though, their probability of going to college increases by 10 p.p. and their probability of graduating from some HE institution increases by 8.5 p.p. Still, as we control for educational attainment, the effect of admission on earnings only drops by 16% for low-income students — see columns (4) and (5). This reduction in the admission effect does not necessarily imply that a college degree increases students' earnings see Angrist & Pischke, 2009. However, it suggests that, if anything, the role of educational attainment in explaining our main findings is very limited.[17]

Another hypothesis is that, if not admitted, high-income candidates apply again to the flagship university in the following year — a privilege that low-income applicants would not have. In that case, the effect of admission on the probability of graduating from the flagship university would be much smaller for the high-income group. However, column (3) of Table 3 shows that this effect is very similar between groups — about 57 p.p. for the high-income group and 61 p.p. for the low-income group.

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[16] For this method, following de Chaisemartin and Behaghel (2020), we do not consider the last student admitted to each program cohort.

[17] If educational attainment were the sole driver of our main results, a back-of-the-envelope calculation would suggest a college wage premium close to 270% for low-income students.

(a) Formal Experience
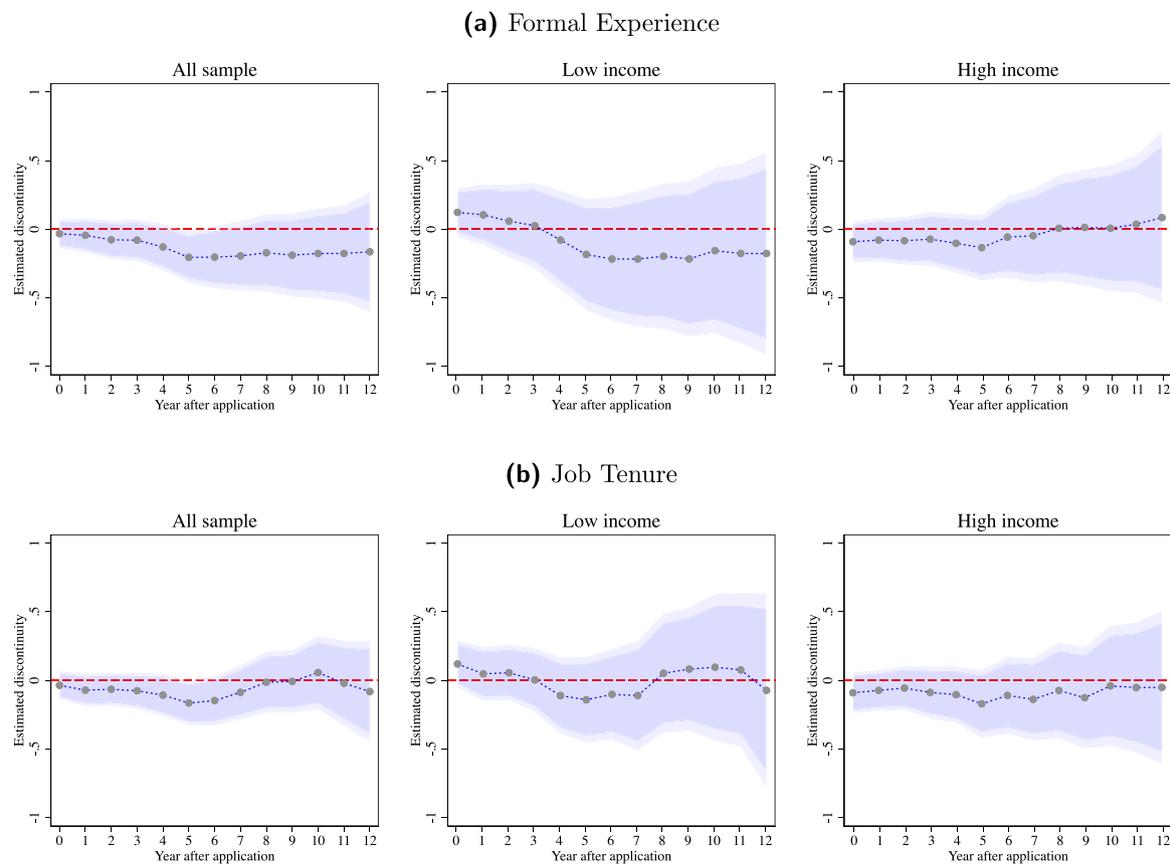

(b) Job Tenure

Fig. 4. Effect of admission on formal experience and job tenure over the years.
This figure presents SRD estimates for the effects of admission on formal work experience in panel (a) and job tenure in panel (b) up to 12 years after application. Graphs on the left-hand side include all applicants in our sample. In the other graphs, sample is split based on the income reported on the application. Formal experience is calculated as the cumulative number of days, divided by 365.25, an applicant is found employed on RAIS. Job tenure is calculated as the consecutive number of days, divided by 365.25, an applicant has worked for their current employer. Each point in these graphs represents a SRD estimate, estimated using triangular kernel with the bandwidth selected using the CCT procedure. All regressions control for gender, year, and program fixed effects. Shaded areas represent robust confidence interval at 90% (darker) and 95% (lighter) levels.

Furthermore, Figure A4 of the Appendix confirms that some non-admitted candidates reapply and enroll in to the flagship university in the following year. As a result, the admission effect on enrollment drops from 85 p.p. to less than 60 p.p. after one year, staying at the same level afterwards. Although, this reduction in the first-stage effect makes our estimates for the enrollment effect more conservative, it does not seem to differ between high- and low-income groups.

Therefore, differences in educational attainment and future enrollment in the flagship university are not large enough to explain the differences in the admission effect on the earnings of low- and high-income students. For low-income students, our results suggest that the earning gains of attending other institutions are not as high as the gains of attending the flagship university.

5.3.2. Program choice
Another mechanism that could explain the difference between groups is program choice. It could be that low-income students are admitted to programs that yield higher wage premia, compared to their outside option. To verify this mechanism, Table 4 presents four separate regressions for each panel (i.e., 'all sample,' 'low income,' and 'high income'). In each column, the effects are estimated using a different sub-sample of applicants, grouped based on the type of program they chose. Given the smaller sample sizes and larger standard errors, the results below should be taken with a grain of salt because no difference is statistically significant at 10%.

In columns (1) and (2), we split the programs at the median share of admitted low-income students. These columns show that the point estimate for the effect on earnings is larger in programs with a higher

share of low-income students. However, the effect on high-income students who enroll in these programs is small and insignificant. Thus, even within those programs, we still observe a considerable difference between low- and high-income groups. This difference is also robust if we split the programs into quartiles instead of high and low shares — see columns (1)-(3) in Table A10 of Appendix.

In columns (3) and (4) of Table 4, we split the programs at the median first-round score of admitted students. If above the median, we consider that admission to the program is highly competitive. Otherwise, it is less competitive.[18] Those columns show that the point estimate for the effect on low-income students is greater and only significant in less competitive programs. This result suggests that high-performing low-income students, who were at the cusp of entering more selective programs, are not affected by admission as much as those low-income students who almost make the cutoff for less competitive programs. Columns (4)-(6) in Table A10 of the Appendix confirm that this difference is robust if we split the programs into quartiles of median first-round scores.

Overall, these results suggest that the difference between high- and low-income groups is not mechanically driven by the composition of students in the chosen programs. Moreover, they reveal that the average gap between best and second-best option is larger for low-income candidates with lower test scores. For high-performing low-income students, who just made the cutoff to enter highly selective programs,

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[18] We cannot use the final admission score to compare different programs because the second-round exams are specific to the subject.

(a) Firm Compensation

(b) Job Compensation
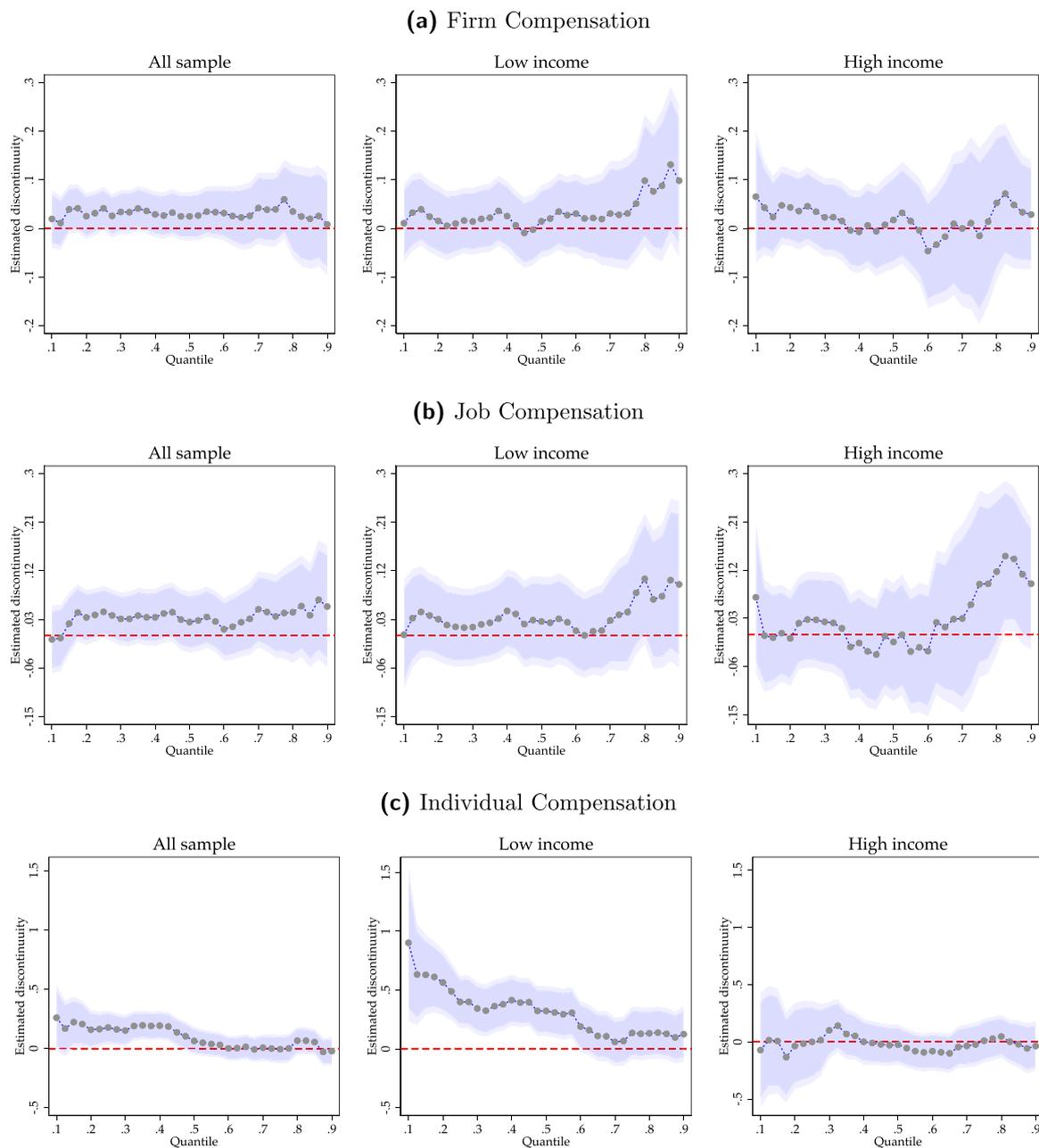

(c) Individual Compensation

Fig. 5. Quantile effects of admission on firm compensation, job compensation, and individual compensation ten years after application.
Each point in these graphs represents a quantile effect of admission, estimated using Frandsen et al.'s 2012 SRD estimator. Dependent variables derive from an AKM model, described in Section 4.3. The dependent variable in panel (a) is the fixed effect of firms for which applicants worked ten years after application. In panel (b), it is the sum of firm-position and firm-position-worker fixed effects. In panel (c), it is the difference between applicants' log earnings and the sum of firm-position and firm-position-worker fixed effects. Graphs on the left-hand side include all applicants in our sample who were formally employed ten years later. In the other graphs, sample is split based on the income reported on the application. Shaded areas represent robust confidence interval at 90% (darker) and 95% (lighter) levels.

the average outside option is not significantly worse than going to the flagship university.

5.3.3. Employment and work experience

A potential channel through which admission affects earnings is employment. With this respect, Fig. 2 shows that admission has nearly no effect on the probability of having a formal job in the long run.

However, short-term impacts on employment might have cumulative effects on work experience. To verify this mechanism, we estimate the effect of admission on years of formal experience and tenure at the current job for each year after candidates apply to college. In Fig. 4, each point represents a separate SRD. They confirm that in the long

run, cumulative effects on formal experience and job tenure are neither large nor significant.[19]

Another hypothesis is that admission might affect the number of days employed within a year. Columns (1) and (2) of Table 5 indicates that this is not the case. Students do not seem to work more days in a formal job due to admission to the flagship university. On the other hand, as we restrict our sample to those who work for more than 182 days, the point estimates for the admission effect become smaller — see columns (3) and (4) of Table 5. Therefore, the admission effect

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[19] Table A11 of the Appendix also shows that controlling for these two variables does not change the estimated effects on earnings.

Table 5

Effects of admission and enrollment on days worked and log earnings ten years after application.

	Dependent variable			
	Share of days worked		Log earnings	
	All applicants	Formally employed	Employed 183 days or more	Employed whole year
	(1)	(2)	(3)	(4)
Panel A. All sample				
Admission	0.018	0.024	0.043	0.032
	(0.023)	(0.017)	(0.047)	(0.046)
Enrollment	0.021	0.029	0.051	0.038
	(0.027)	(0.021)	(0.055)	(0.054)
N. of observations	20,790	13,353	11,880	9,781
Panel B. Low income				
Admission	0.041	0.030	0.124*	0.115*
	(0.039)	(0.028)	(0.068)	(0.069)
Enrollment	0.048	0.037	0.153*	0.142*
	(0.045)	(0.034)	(0.084)	(0.085)
N. of observations	7,866	5,675	5,107	4,283
Panel C. High income				
Admission	0.010	0.008	−0.076	−0.135
	(0.044)	(0.030)	(0.096)	(0.093)
Enrollment	0.012	0.010	−0.090	−0.156
	(0.053)	(0.036)	(0.114)	(0.108)
N. of observations	6,908	3,764	3,262	2,617

This table presents the SRD estimates for admission effect and the FRD estimates for enrollment effect on the proportion of days on which applicants were formally employed in the tenth year after the application, in columns (1)-(2), and log earnings, in columns (3) and (4). Earnings are measured as the sum of all salaries received for 12 months, ten years after the application. Samples in columns (2)-(3) are restricted to applicants who were formally employed ten years later. In column (3), sample is also restricted to those who were formally employed for 183 days or more. In column (4), sample is also restricted to those who were formally employed for 365 days or more. In Panels B and C, samples are split based on the income reported on the application. SRDs and FRDs are estimated using triangular kernel with the bandwidth selected using the CCT procedure. All regressions control for gender, cohort, and program fixed effects. Standard errors, clustered at student level, are in parentheses. ***, **, * represent statistical significance at 1%, 5% and 10% levels, respectively.

on earnings appears to be more pronounced on low-income students with higher risk of unemployment. This heterogeneity is in line with the evidence that in the low-income group, low-performing students are more affected than high-performing students (see Table 4).[20]

5.3.4. Access to better jobs

The fourth and last mechanism that we test is whether low-income students increase their future earnings by working for better-paying firms or having better-paying occupations. It might be the case that admitted students have access to jobs that the non-admitted do not. Moreover, non-admitted students might pursue other careers, different from the ones intended when they apply to the flagship university. If either mechanism plays an important role, then our results should be explained by differences in employers and occupations.

To explore these differences, we first decompose the salary of workers into worker, occupation, and firm fixed-effects using the Abowd–Kramarz–Margolis (AKM) model (Abowd et al., 1999). This model is estimated using the full set of workers who have ever worked for firms that have ever employed students in our sample. For those workers, we also consider their salary when employed by other firms. The estimated AKM model is described in Section 4.3.

In Table 6, we present the average effects on firm compensation in column (1), job (i.e., firm plus position) compensation in column (2), and individual compensation in column (3). Columns (1) and (2) indicate that, on average, the effect of admission on job selection is small and insignificant. Accordingly, column (3) confirms that the average effect of admission on salary is fully explained the individual component — i.e., their earnings excluding the firm and position

effects. Therefore, our main findings seem to be driven by differences in workers' skills within jobs.

Since we are also interested in changes in the distribution of job opportunities, our results are now focused on quantile effects of admission, described in Section 4.4. In Fig. 5, we present the estimated quantile effect on firm compensation in panel (a), job compensation in panel (b), and individual compensation in panel (c). Panel (a) shows that, for the low-income students with the highest-paying employers, the admission makes them find even better firms, which pay around 10% more. Likewise, panel (b) shows that, if admitted, the high-income students with the highest-paying jobs tend to find positions that pay about 12% more. Nevertheless, the effect of admission on job selection is small and insignificant for the other students.

Panel (c) of Fig. 5 confirms that most of the effect on the future earnings of low-income students comes from an increase in their residual compensation. In addition, we find that the impact is larger at the bottom of the earnings distribution. This result is consistent with the heterogeneity presented in Table 4 – i.e., larger effects on low-performing low-income students – and in Table 5 — i.e., smaller effects on low-income students employed for a longer period of time.

6. Conclusion

The demand for HE has rapidly increased in developed and developing countries, along with the costs of attending college. The rising costs tend to be more damaging to low-income students, who lack financial resources to be able to access the full array of options. Accordingly, a policy that increases access to top-quality institutions and makes them more affordable for underprivileged students could effectively promote social mobility. To examine this premise, we estimate the impact of admission to an affordable, top-quality university in Brazil and compare effects between low- and high-income students.

By applying a regression discontinuity design, we find that admission to a highly selective public university increases the earnings of

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[20] After restricting the sample to those employed for 12 months or more, the difference between low-income students in less and more competitive programs disappears. See Table A12 of Appendix.

Table 6
Effects of admission and enrollment on firm compensation, job compensation, and individual compensation ten years after application.

	Dependent variable		
	Firm compensation	Job compensation	Individual compensation
	(1)	(2)	(3)
Panel A. All sample			
Admission	0.035	0.033	0.122*
	(0.028)	(0.028)	(0.069)
Enrollment	0.040	0.038	0.143*
	(0.033)	(0.033)	(0.081)
N. of observations	9,952	10,090	9,952
Panel B. Low income			
Admission	0.036	0.034	0.291***
	(0.041)	(0.040)	(0.103)
Enrollment	0.046	0.044	0.369***
	(0.051)	(0.050)	(0.129)
N. of observations	4,256	4,299	4,256
Panel C. High income			
Admission	0.034	0.039	−0.036
	(0.048)	(0.051)	(0.132)
Enrollment	0.038	0.045	−0.040
	(0.054)	(0.058)	(0.148)
N. of observations	2,760	2,815	2,760

This table presents the SRD estimates for admission effect and the FRD estimates for enrollment effect on firm, job, and individual components of applicants' earnings. Sample is restricted to applicants who were formally employed ten years later. Dependent variables derive from an AKM model, described in Section 4.3. The dependent variable in column (1) is the fixed effect of firms for which applicants worked ten years after application. In column (2), it is the sum of firm-position and firm-position-worker fixed effects. In column (3), it is the difference between applicants' log earnings and the sum of firm-position and firm-position-worker fixed effects. In Panels B and C, samples are split based on the income reported on the application. SRDs and FRDs are estimated using triangular kernel with the bandwidth selected using the CCT procedure. All regressions control for gender, cohort, and program fixed effects. Standard errors, clustered at student level, are in parentheses. ***, **, * represent statistical significance at 1%, 5% and 10% levels, respectively.

low-income students by 26% ten years later. Surprisingly, gains in educational attainment barely explain this effect. Although most of the non-admitted low-income applicants also graduate from college, their returns to HE are much lower. This gap in returns is even larger for applicants who are nearly admitted to less competitive programs, confirming these students' lack of HE opportunities. Furthermore, our results show that access to better-paying employers and occupations does not explain the effect of admission on earnings. Instead, this effect is explained by the acquired and signaled skills of attending the public university.

On the other hand, admission to the public university does not significantly affect the earnings of high-income students and those admitted to more selective programs. This result indicates that these students would likely find similar opportunities if not admitted to the public university. It also implies that expanding access to affordable quality institutions to these groups would not yield substantial returns. A question for future research is whether tuition discounts are effective in attracting high-income and high-performing students to public universities.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

The data that has been used is confidential.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.econeducrev.2023.102423.

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