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The efficacy of large-scale affirmative action at elite universities

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Abstract. We study the effects of affirmative action at an elite Brazilian university that adopted race- and income-based quotas for 45 percent of its admission slots. We link admission records to national employer-employee data to examine how the policy affected the careers of both its targeted beneficiaries and the university's other students. For students admitted through affirmative action, the policy led to a modest increase in early-career earnings that faded as their careers progressed. Conversely, the adoption of affirmative action caused a large and persistent decrease in earnings for the university's most highly ranked students. We present evidence that these negative earnings effects are driven by a reduction in human capital accumulation and a decline in the value of networking.

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This paper examines the efficacy of large-scale affirmative action at elite universities. There is growing pressure on top universities to promote intergenerational mobility by increasing the racial and socioeconomic diversity of their student bodies. Recent work has found that affirmative action in state university systems can raise the earnings of underrepresented minority students (Bleemer, 2022). But there is limited evidence on the earnings effects of affirmative action at the most elite universities, and at the scale that would be necessary to equalize the representation of minority and low-income students (Chetty et al., 2020).

The mechanisms through which affirmative action can affect earnings inequality are likely to depend on the scale of the policy and on the selectivity of the university that adopts it. The debate on affirmative action often centers on the question of how it affects graduation rates (Arcidiacono et al., 2016; Bagde et al., 2016). This debate is less relevant at elite universities because *all* students on the margin of admission are high achieving, and most will earn a degree regardless of where they enroll (Bowen and Bok, 1998). The benefits of attending an elite school are more likely to come from the material students learn and the peer networks they form during college. Large-scale affirmative action may alter these benefits by changing how professors teach (Duflo et al., 2011), the nature of peer interactions (Carrell et al., 2013), and the value of networking (Zimmerman, 2019; Michelman et al., 2022).

We study an elite university in Brazil that adopted race- and income-based quotas for nearly half of its student body. We link admissions and labor market data to ask how affirmative action affected the careers of both its targeted beneficiaries and other students at the university. For students admitted through affirmative action, the policy led to a modest increase in early-career earnings that faded as their careers progressed. Conversely, the adoption of affirmative action caused a large and persistent decrease in earnings for the university's most highly-ranked students. We show that these earnings effects are due in part to a reduction in learning and to a decline in the value of networking.

The setting for our paper is Rio de Janeiro State University (UERJ), which is one of the most prestigious universities in Brazil. Brazil's higher education system is heavily privatized and contains over 2,000 colleges, but the most selective schools are public universities like UERJ. UERJ is consistently ranked among the top 15 universities nationally. In some years, over 100,000 students take UERJ's entrance exam to compete for roughly 5,000 admission slots. Thus in terms of national prestige and the difficulty of gaining admission, UERJ is comparable to elite private colleges in the United States.

UERJ was one of the first universities in Brazil to adopt affirmative action, and it did so at a large scale. Historically, white students from private high schools were disproportionately likely to gain admission through UERJ's entrance exam. In 2004, UERJ began reserving 45 percent of slots in each major for Black and public high school students from low-income families. Students admitted through affirmative action were high-achieving relative to the

Brazilian population, but the policy was necessary to give them preference over applicants from the general pool with higher exam scores. This policy led to a sudden and dramatic increase in the racial and socioeconomic diversity of UERJ's student body.

We collected data on the schooling and labor market outcomes of students who applied to UERJ before and after the adoption of affirmative action (AA). Our base dataset contains the entrance exam scores and admission outcomes of all UERJ applicants in 1995–2001 (pre-AA) and 2004–2011 (post-AA). We link these data to UERJ enrollment/graduation records, and to Brazil's national employer-employee dataset for the years 2003–2019.

Our analysis exploits a unique feature of UERJ's policy that created variation in exposure to affirmative action across majors. Admission to UERJ is major-specific, and while the fraction of slots reserved for affirmative action was the same in each major, the *take-up* of these slots varied significantly. In UERJ's most prestigious majors, the number of applicants in the Black and public school tracks typically exceeded the reserved quotas, so affirmative action students made up 45 percent of the incoming class. The reserved quotas often went unfilled in less-selective programs, and UERJ would fill open seats from the general applicant pool. Thus the share of enrollees who were from an affirmative action track was as low as 10–20 percent in some programs.

We use two empirical strategies to identify the effects of affirmative action on both its intended beneficiaries and on other UERJ students. In majors with high take-up of affirmative action, we use a regression discontinuity (RD) design that compares applicants above and below the admission score cutoffs (Hoekstra, 2009; Kirkebøen et al., 2016). Our RD design identifies the returns to attending UERJ for applicants who were marginally-admitted through the affirmative action tracks (as well as those in the general track).

Our second strategy exploits variation in affirmative action take-up to identify the policy's "spillover" effects on other UERJ students. We use a simple difference-in-differences (DD) design that estimates changes in outcomes between the pre- and post-AA cohorts, and across majors with higher and lower take-up rates. In this analysis, we focus on a sample of *top enrollees* whose entrance exam scores were high enough to gain admission to UERJ regardless of whether affirmative action existed in their cohort. For these highly-ranked students, our DD design identifies the effects of a 19 percentage point increase in the share of their classmates who were from an affirmative action track.

We have two main findings. First, for students admitted through affirmative action, enrolling in UERJ led to a modest increase in early-career earnings that faded as their careers progressed. Our RD estimates show that UERJ enrollment caused a 14 percent increase in affirmative action students' hourly wages measured 6–9 years after application. But the earnings gain for affirmative action students faded over time, and it was close to zero measured 13 years after application. Further, attending UERJ had no impact on the likelihood that affirmative action students earned a college or postgraduate degree.

Second, the adoption of affirmative action lowered the earnings of UERJ's highly-ranked students. In our DD analysis, we find that top enrollees' hourly wages decreased by 14 percent in majors with high affirmative action take-up relative to those with lower take-up. This negative earnings effect persisted throughout the period of 6–13 years after UERJ enrollment. Our estimates imply that a one percentage point increase in the share of affirmative action enrollees reduced top students' wages by 0.7 percent.

We present evidence on two mechanisms for the negative effect of affirmative action on top enrollees' earnings. First, we use data from Brazil's field-specific college exit exam, the ENADE, to show that affirmative action reduced the learning of UERJ's top students. Specifically, the ENADE scores of UERJ graduates declined in the post-AA cohorts relative to other public universities. These performance declines also appear at high quantiles of the score distribution, and we do not find significant changes in the demographics or admission scores of UERJ's top students. This suggests that the decrease in ENADE scores partly reflects a reduction in top enrollees' human capital accumulation during college.

Second, the negative earnings effects were also due in part to a decrease in the value of UERJ's peer and alumni networks. To test for networking effects, we use our employer-employee data to define outcome variables that indicate when a UERJ applicant obtained a job at a firm that hired a graduate (the "alum") from the program that the applicant applied to. We show that UERJ enrollment has a large causal effect on the likelihood of employment with other UERJ alumni, suggesting that networking is an important part of the value of attending the university. Since affirmative action students typically obtained lower-paying jobs than general track students, the value of these network connections declined more in majors with high exposure to the policy. Specifically, we find that greater exposure to affirmative action reduced top enrollees' likelihood of obtaining high-paying jobs that are affiliated with UERJ alumni from the general track.

Our paper contributes to research on the efficacy of affirmative action for reducing earnings inequality. There is a large literature on affirmative action in university admissions, but there is limited evidence on the earnings impacts of these policies (Arcidiacono et al., 2015).[1] The most related paper to ours is Bleemer (2022), which examines a ban on affirmative action in the University of California system. This ban affected enrollment at many schools that vary widely in selectivity, including two- and four-year state colleges. Bleemer finds

[1] Other research on affirmative action looks primarily at its effects on student body diversity or graduation rates (Cortes, 2010; Backes, 2012; Hinrichs, 2012; Kapor, 2015; Arcidiacono et al., 2016; Bagde et al., 2016). This is true of most work on affirmative action in Brazil (Francis and Tannuri-Pianto, 2012; Ribeiro, 2016; Estevan et al., 2019; Vieira and Arends-Kuenning, 2019; Ribeiro and Estevan, 2021; Mello, 2022), with the exception of Francis-Tan and Tannuri-Pianto (2018).

that affirmative action increased minority students' earnings as well as their likelihood of completing a bachelor's degree, which suggests that educational attainment is a key mechanism for the earnings gains. Consistent with this, papers on the effects of admission to state university *systems* typically find large increases in both earnings and degree attainment for disadvantaged students (Zimmerman, 2014; Smith et al., 2020; Bleemer, 2021).

Our findings suggest that affirmative action may be less effective at reducing earnings inequality at elite universities. There is less scope for affirmative action to affect degree attainment for applicants on the margin of admission to elite schools, which is consistent with our null effects on educational attainment. Many of the benefits of attending elite schools, such as advanced coursework or networking, are likely to depend on student body composition, and thus can change with large-scale affirmative action. Further, minority and low-income individuals are likely to face barriers to career advancement in the labor market, which can reduce their benefits from university networks. Consistent with this hypothesis, we find that UERJ's affirmative action policy increased disadvantaged students' access to high-paying firms early in their careers, but this effect also faded over time.[2]

Our paper is also novel in examining the spillover effects of affirmative action on students who are not targeted by the policy. Several papers examine how a university's racial or socioeconomic diversity affects the outcomes of other students (Daniel et al., 2001; Hinrichs, 2011; Arcidiacono et al., 2012; Carrell et al., 2019).[3] Identification is a central challenge in these papers, as there is extensive sorting of students into colleges. Our empirical design identifies spillovers under weaker assumptions, and we present evidence on both learning and networking mechanisms. Our estimated spillover effect is similar in magnitude to Arcidiacono and Vigdor (2010)'s estimate of the effects of the minority student share on the earnings of other students at selective U.S. colleges. The efficacy of affirmative action depends on relative returns for targeted and untargeted students (Durlauf, 2008; Bertrand et al., 2010; Black et al., 2020) as well as any spillover effects. The existence of spillovers means that the true effects of large-scale admission reforms can differ from those estimated using existing student/college matches (Chetty et al., 2020; Otero et al., 2021).

Our paper proceeds as follows. Section 1 describes UERJ's affirmative action policy and our data. Sections 2 and 3 present our RD analysis of the direct effects of affirmative action on disadvantaged students. Sections 4 and 5 present our DD analysis of the policy's spillover effects on untargeted students. Section 6 concludes.

[2] Our findings are similar to those in Zimmerman (2019) and Michelman et al. (2022); both papers find that high-income students are the primary beneficiaries of peer and alumni connections at elite schools. Our paper is unique in examining how network mechanisms affect the efficacy of affirmative action policies, and our data allows us to examine early-career employment in a broad set of firms.

[3] A related literature examines student/college match effects in graduation and earnings outcomes (Andrews et al., 2016; Dillon and Smith, 2018; Riehl, 2019; Mountjoy and Hickman, 2020).

1. Context and data

1.1. **UERJ and higher education in Brazil.** Our setting is an elite public university in Brazil called Rio de Janeiro State University, or UERJ (*Universidade do Estado do Rio de Janeiro*). It is one of the oldest and most prestigious universities in Brazil; UERJ ranked 11th nationally in a 2012 ranking by the newspaper *Folha*. UERJ is part of Brazil's system of *state universities*, which are funded and managed by the governments of each state. Brazil also has *federal universities* that are controlled by the federal government. State and federal universities are highly-regarded and tuition-free, and thus admissions are highly competitive. At UERJ, the number of applicants is often 10–20 times greater than the number of slots. Yet these schools are a small share of the Brazilian higher education system; there are more than 2,000 colleges in the country, and roughly two-thirds of students attend a private institution.

UERJ typically offers 40–50 undergraduate majors each year in a variety of fields. Students apply to specific programs. Admission is determined by a two-round entrance exam that the university administers near the end of each year. The first round consists of a qualifying exam that is common to all applicants. Students who pass the qualifying exam take field exams in several subjects that depend on the major they are applying to. Admissions are based on a weighted average of an applicant's field exam scores. The highest-scoring applicants are admitted up to a cutoff that is determined by the program's capacity.

1.2. **Data.** Our analysis matches two administrative datasets from UERJ to Brazil's national employer-employee records. Our base dataset is a list of all individuals who applied to UERJ in the years 1995–2001 and 2004–2011.[4] We focus on applicants who passed the first-round exam, which is the relevant sample of potential admits for our analyses. We observe the program individuals applied to, their overall admission score, and their admission outcome. In some cohorts, we also observe demographic characteristics and subject scores on the entrance exams. Appendix B.1 provides details on the variables we use in our analysis and the cohorts in which they are available.

Our second dataset covers all students who enrolled in UERJ from 1995–2011. These records contain the student's program, enrollment date, status as of 2020 (graduated, dropped out, or still enrolled), and final year in the program.

Lastly, we use the 2003–2019 years of Brazil's employer-employee dataset, the RAIS (*Relação Anual de Informações Sociais*). This dataset is assembled by the Ministry of Labor and covers the universe of formal-sector jobs in Brazil. Worker variables include demographics, educational attainment, occupation, hours worked, and monthly earnings. It also includes information on the firm's number of employees, industry, and location.

[4] UERJ does not have application records for the 2002–2003 cohorts.

We link the UERJ and RAIS datasets using individuals' national ID numbers (*Cadastro de Pessoas Físicas*). For individuals with missing ID numbers, we use names and birthdates to link the datasets. Appendix B.2 provides details on the merge process and match rates.

1.3. **Affirmative action at UERJ.** Historically, white students from private high schools were overrepresented at state and federal universities, while Black and public high school students were underrepresented. This was mainly due to the fact that advantaged students typically earned higher scores on the schools' entrance exams.[5] The lack of diversity was a source of contention because these elite universities are publicly-funded and free to attend.

UERJ was one of the first Brazilian universities to address this disparity through affirmative action. In 2003, the state government of Rio de Janeiro passed a law that required UERJ to reserve seats for students from underrepresented groups. Only two other top public universities had affirmative action at the time, and both were located in other states (Júnior and Daflon, 2014). Other universities adopted race- and/or income-based quotas in subsequent years (Ferman and Assunção, 2005; Vieira and Arends-Kuenning, 2019), and a 2012 national law mandated affirmative action at all federal universities. But UERJ was the only elite university in Rio de Janeiro with affirmative action for much of the 2000s.

UERJ implemented the policy by reserving 45 percent of seats in each program for low-income applicants from disadvantaged groups. Historically there was a single admission track for each major, but in 2004 UERJ added three affirmative action tracks per program.[6] 20 percent of slots in each major were reserved for public high school applicants. Another 20 percent of slots were reserved for Black applicants.[7] Lastly, 5 percent of slots were reserved for students from other disadvantaged groups (e.g., disabled and indigenous applicants). To be eligible for an affirmative action track, applicants also had to be from a low-income family, and they were required to submit tax records to verify their income status.[8] Applicants who did not meet these criteria could apply through the general track, which governed the remaining 55 percent of seats. Within each track, admissions were still based solely on field exam scores, and there was no affirmative action in the first-round qualifying exam.

Although the fraction of reserved slots was the same in each major, the *take-up* of these slots varied significantly. We illustrate this in Figure 1, which plots the share of affirmative

[5] Other factors contributed to the underrepresentation of Black and public school students, e.g., exam access and information about the admission process (Hoxby and Avery, 2013; Machado and Szerman, 2021).

[6] UERJ first introduced affirmative action in the 2003 cohort following the state law. In this cohort, there were only two admission tracks—low-income and general—and each track reserved some seats for Black applicants. The quota system described in the text was in place for all cohorts in 2004–2011.

[7] In Brazil, race is commonly classified in five groups: *branco* (white), *pardo* (brown), *preto* (Black), *amarelo* (yellow), and indigenous. UERJ's race-based quota was reserved for individuals who self-identified as Black; this occasionally differs from their racial identity reported in the entrance exam or RAIS data.

[8] In 2004, for example, applicants had to be from families whose per capita income was below R$300 per month (Zoninsein and Júnior, 2008), which was roughly 40 percent of the national GDP per capita.

action enrollees in the 2004–2011 cohorts (y-axis) against a measure of each program's selectivity (x-axis). In highly-selective programs like Law and Medicine, the number of applicants to each track typically exceeded the reserved quota, so affirmative action students made up 45 percent of the incoming class. In less selective programs like Math and Teaching, the number of affirmative action applicants was frequently less than the reserved quota, and UERJ would fill the remaining slots from the general track. Thus the share of affirmative action enrollees in these programs was as low as 10–20 percent. The low take-up was attributable to both the lower desirability of these programs and the fact that UERJ had strict criteria for affirmative action eligibility.[9]

UERJ's admission policy gave a large implicit preference to affirmative action students. Figure 2 plots the distribution of admission scores for 2004–2011 applicants in the Black, public school, and general tracks, where scores are standardized to be mean zero and standard deviation (SD) one among *all* applicants to a given program/cohort. Vertical lines displays the mean cutoff score in each track, which is the mean of the standardized scores for the last students admitted to each program/cohort. The mean cutoff was -0.5 in the public school track, -0.6 in the Black track, and $+0.9$ in the general track. Thus the last admitted students in the affirmative action tracks typically scored 1.5 standard deviations below the last admitted student in the general track. The number of general applicants was also ten times greater than the number of affirmative action applicants, so the policy gave Black and public school applicants priority over many individuals with higher scores.

1.4. **Samples.** We create two samples to analyze the impacts of affirmative action at UERJ. In Sections 2–3, we use a regression discontinuity (RD) design that compares the outcomes of admitted and rejected applicants. In Sections 4–5, we use a difference-in-differences (DD) design that compares the outcomes of enrollees in programs with higher and lower take-up rates of affirmative action. We construct our samples to be suitable for these two analyses. Appendix B.4 provides details on our sample construction.

Our RD sample includes programs in which we can identify the effects of admission to UERJ in the affirmative action tracks. Some UERJ programs had few Black and public school applicants, so in many cohorts there were no rejected students in these tracks. Since we cannot implement our RD design in these cases, we restrict our RD sample to programs where the Black and public school quotas typically filled up. Specifically, our RD sample includes the 24 programs in which 30 percent or more of the 2004–2011 enrollees were from an affirmative action track (i.e., programs above the horizontal line in Figure 1). In these

[9] Affirmative action applicants had to meet both the quota category requirement (Black or public high school) and the low-income requirement. As a result, most Black and public high school students who applied to UERJ were *not* eligible for an affirmative action track. All applicants also had to pass the first-round qualifying exam, for which there was no affirmative action.

programs, we also exclude any cohort/application-track pair in which there are fewer than five applicants below the admission threshold (see Appendix Tables B2–B4).[10]

Our DD sample includes all programs that UERJ offered both before and after 2004.[11] This includes the 24 programs in our RD sample plus 19 other programs with lower rates of affirmative action take-up. We focus on a sample of *top enrollees* that we define in Section 4, which are students who could have attended UERJ regardless of whether affirmative action existed in their cohort.

Table 1 shows summary statistics for our RD and DD samples. Panel A includes programs in both samples, and Panel B includes programs that are only in our DD sample. Our RD sample includes all of UERJ's health programs as well as many humanities and social science majors (e.g., History, Journalism, and Social Work). Our DD sample includes many teacher-training programs in different subjects, but it also includes Economics, Math, and several engineering majors. We display statistics separately for applicants in pre-AA cohorts (column A) and to the four tracks in the post-AA cohorts (columns B–E). Affirmative action applicants were disadvantaged relative to general applicants as measured by mother's education and family income. They were also older on average and more likely to be female.

2. RD specification

2.1. **Regression model.** We use a two-stage least squares (2SLS) RD model to estimate the returns to enrolling in UERJ:

$$E_{ip} = \theta D_{ip} + \alpha x_{ip} + \psi D_{ip} x_{ip} + \gamma_p + \varepsilon_{ip} \quad \text{if } |x_{ip}| \leq h^Y \tag{1}$$

$$Y_{ip} = \beta E_{ip} + \tilde{\alpha} x_{ip} + \tilde{\psi} D_{ip} x_{ip} + \tilde{\gamma}_p + \tilde{\varepsilon}_{ip} \quad \text{if } |x_{ip}| \leq h^Y. \tag{2}$$

Y_{ip} is an outcome for individual i who applied to UERJ in application pool p. Application pools are defined by a program, cohort, and admission track. The endogenous treatment variable, E_{ip}, is an indicator that equals one if the applicant enrolled in the UERJ program and cohort that they applied to. We instrument for UERJ enrollment with D_{ip}, which is an indicator for having an admission score above the final cutoff for application pool p.

We use a local linear specification to estimate returns for applicants on the margin of admission. Equations (1)–(2) include an interaction between D_{ip} and the running variable, x_{ip}, which is individual i's admission score in application pool p. We normalize x_{ip} to equal zero for the last admitted student, and so that it has SD one in the population of all applicants to a given program/cohort. Our regressions include the subset of applicants

[10] Although the quotas almost always filled up in the general track, we restrict to the same programs in our RD sample of general applicants so that it is comparable to the Black and public school samples. Our RD sample excludes all applicants in the disabled/indigenous track, as these quotas were rarely filled.

[11] UERJ re-organized a few large programs into sub-programs during our sample period. In these cases we combine sub-programs into one program in our DD analysis. See Appendix Tables B2–B4.

whose admission scores are within h^Y standard deviations of the admission threshold. Our benchmark results use the Calonico et al. (2014) bandwidth computed separately for each outcome Y; Appendix Tables A3–A5 show that our main results are robust to different bandwidths. We include fixed effects for each application pool, γ_p, and cluster standard errors at the individual level.[12]

Our focus is on outcomes for affirmative action students, so we estimate equations (1)–(2) using applicants in the Black and public school tracks. Our main estimates pool across the two affirmative action tracks and the 24 UERJ majors in order to increase power. We also present estimates for general track applicants in the pre- and post-AA cohorts, and we examine heterogeneity by field of study.

2.2. **Identification assumptions and balance tests.** Our identification relies on the standard RD, instrumental variable, and local average treatment effect (LATE) assumptions.

The main RD assumption requires that applicants' admission scores are effectively randomly assigned near the admission cutoffs. There is little scope for applicants to manipulate their exam scores, but a potential issue is that UERJ engages in waitlist admissions to fill the available seats. UERJ sends initial admission offers in January, and admits have several weeks to accept or decline their offer. UERJ then attempts to fill the remaining open seats through multiple rounds of waitlist admissions with applicants who have the next highest scores. Our instrument and running variable, D_{ip} and x_{ip}, are defined by the final threshold in each application pool. This creates the possibility of non-random sorting around the cutoff; the last admitted student may be particularly likely to accept an admission offer, and this tendency may be correlated with potential outcomes. Appendix B.3 for describes UERJ's admission process in detail.

We find no evidence that the RD assumption is violated for affirmative action applicants using the standard balance tests. Appendix Table A1 presents RD estimates from regressions that use applicant characteristics as dependent variables (e.g., age, gender, race, and qualifying exam scores). We cannot reject the hypothesis that these coefficients are jointly equal to zero ($p = 0.88$). We find similar results combining these characteristics into an index of predicted wages (Appendix Figure A1). There is no evidence of a discontinuity in the density of admission scores using the McCrary (2008) test (Appendix Figure A2). These results corroborate our prior that the waitlist is less likely to be an issue in the Black and public school tracks because most applicants accepted their admission offer.

We also find covariate balance for general track applicants, but we find a statistically significant decrease in the density of admission scores at the threshold using the McCrary test.

[12] Clustering addresses the fact that some individuals appear in our sample multiple times because they applied to UERJ more than once.

UERJ's yield was lower in the general track, so there was more scope for non-random sorting from waitlist admissions. Thus our RD results for general applicants should be interpreted with some caution. Reassuringly, we find similar results in "donut hole" regressions that drop applicants near the threshold (Appendix Tables A3–A5).

We also make the standard instrumental variable and LATE assumptions (Angrist et al., 1996). Instrument relevance is satisfied because the UERJ enrollment rate increases sharply at the admission threshold (Table 2, Panel A). The exclusion restriction requires that our instrument affects outcomes only through the channel of enrolling in UERJ. This could be violated if, for example, admission to UERJ caused individuals to apply to other schools. We cannot rule out this possibility, but we believe our results are primarily attributable to UERJ enrollment, particularly in the affirmative action tracks where the first-stage coefficient is large. The monotonicity assumption requires that there are no applicants who would attend UERJ if and only if they were *below* the cutoff, which is plausible in our setting.

Under these assumptions, the β coefficient from equation (2) can be interpreted as the average causal effect of attending UERJ for marginally-admitted "compliers." These compliers are students who would have enrolled if and only if they scored above the cutoff. This estimand measures the returns to UERJ enrollment relative to the educational choices that students would have made if they were not admitted, which is relevant for evaluating the efficacy of UERJ's affirmative action policy for reducing earnings inequality.

3. Direct effects of affirmative action

3.1. **Organization of RD results.** This section presents RD estimates of the returns to attending UERJ for affirmative action students.

Table 2 presents our main results on graduation rates and post-college earnings. We examine outcomes 6–9 years after individuals' applied to UERJ to capture their initial jobs after (potential) graduation, and also 10–13 years after application to capture longer-run effects.[13] To shed light on mechanisms, Table 3 shows how admission to UERJ affected applicants' college selectivity, fields of study, and educational attainment. Lastly, Table 4 examines the role of UERJ's alumni networks in these returns. Figure 3 displays RD graphs for our main outcomes; these graphs show the reduced-form effects of admission to UERJ by plotting means of each outcome in 0.1 SD bins of the standardized admission score.

In each table/figure, we show results for three groups: applicants in the pre-AA cohorts (1995–2001), general track applicants in the post-AA cohorts (2004–2011), and affirmative

[13] All of our RD regressions include one observation per applicant. For earnings outcomes, we use the applicant's mean real earnings over the periods of 6–9 or 10–13 years after application. For binary outcomes, we use the maximum over each period, so our estimates reflect ever having a job with those characteristics. Most UERJ students who graduate do so in 4–6 years (see Appendix Figure A3).

action applicants (Black/public school tracks pooled). We focus on the outcomes of affirmative actions students, but we present results in the general tracks for comparison.

3.2. **Graduation and earnings.** For Black and public school applicants, the likelihood of enrolling in UERJ increased by 69 percentage points at the admission threshold. Panel A of Table 2 displays estimates of our first-stage coefficients, θ, from equation (1). The first stage for affirmative action applicants (column F) is more than twice as large as that for general applicants (columns B and D). UERJ's "yield" was high in the Black and public school tracks because most other universities in Rio did not have affirmative action during 2004–2011. In the general track, marginal admits would typically have been competitive for admission to other top colleges in the area (see Section 3.4).

Our first finding is that marginal enrollees in the affirmative action tracks were only slightly less likely to graduate than those from the general track. Panel B of Table 2 displays 2SLS RD coefficients, β, from equation (2). The first row shows the effects of enrolling in a UERJ program on the likelihood of graduating from that program by nine years later. 64 percent of marginal enrollees in the Black and public school tracks graduated by this time, as compared with 68–71 percent of marginal enrollees in the general tracks. The similarity of these graduation rates is striking since marginal affirmative action admits scored 1.5 standard deviations below those in the general track on the entrance exam (Figure 2).

We find that enrolling in UERJ did not affect the likelihood that affirmative action applicants worked in the formal sector. In Panel B of Table 2, our measure of formal employment is an indicator for appearing in the RAIS data at any time 6–9 years after application. The formal employment rate is 73 percent for marginally-rejected Black and public school applicants (column E), and the 2SLS RD estimate is close to zero (column F).

Affirmative action students experienced a modest increase in early-career earnings from attending UERJ. Panel B of Table 2 shows 2SLS estimates on log hourly wages and mean monthly earnings measured 6–9 years after application. UERJ enrollment caused a 14 percent increase in the mean hourly wages of marginal Black and public school admits (see also Panel C of Figure 3). The gain in monthly earnings was $110 (in 2019 U.S. dollars), which is similar as a percentage of mean earnings below the cutoffs. Both of these estimates are statistically significant at $p < 0.05$, but they are relatively modest in magnitude. The monthly earnings coefficient is roughly one-fifth of the gap between marginally-rejected applicants in the general and affirmative action tracks ($1,391 vs. $817). Thus affirmative action students still had much lower earnings than their classmates from the general track.

Further, the initial earnings gain for affirmative action students did not persist later into their careers. Panel C of Table 2 shows 2SLS estimates for graduation and labor market outcomes measured 10–13 years after application. For affirmative action students, the effect of UERJ enrollment on log hourly wages measured 10–13 years later is only 0.024 log points, and it is not statistically different from zero. Appendix Table A2 shows that we can reject equality of the early- and later-career wage coefficients at $p < 0.05$. The gain in monthly earnings for affirmative action students also declined to $56 in the later period, although this estimate is not statistically distinguishable from the early-career return. Appendix Figure A4 shows that the wage gains for affirmative action students decreased both over time (holding the sample of cohorts fixed) and across cohorts (holding potential experience fixed).

We do not find significant effects of UERJ enrollment on earnings for general track applicants (columns B and D in Table 2). In the cohorts with affirmative action, we find a *negative* effect of UERJ enrollment on general applicants' early-career monthly earnings (Panel B), but this negative return is not significant in the later time period (Panel C). These limited earnings effects may be due to the fact that marginally-rejected general track applicants often enrolled in other selective colleges, as we show in Section 3.4.

3.3. **Heterogeneity by field of study.** Figure 4 shows how our graduation and earnings effects vary by field of study. Related work finds that the benefits of admission to selective colleges vary significantly by major (Hastings et al., 2013; Kirkebøen et al., 2016). To explore these patterns in our data, we group UERJ's programs into five fields of study (see Table 1), and estimate our RD regressions separately by applicant group and field.[14] Figure 4 plots 2SLS RD coefficients for UERJ graduation (Panel A) and monthly earnings (Panel B) measured 10–13 years later, which are analogous to the estimates in Panel C of Table 2. Appendix Table A7 displays heterogeneity in all of our main outcomes by field of study.

We find suggestive evidence that affirmative action students had larger earnings gains in health and business programs, although these estimates are underpowered. The point estimates suggest that UERJ enrollment raised affirmative action students' monthly earnings by about $300 in both business and health. These RD coefficients are about 40% of the magnitude of the general/AA earnings gap below the threshold, although the estimates are imprecise and neither is statistically significant. UERJ's health and business programs have high graduation rates, and over 70 percent of marginal affirmative action enrollees completed a degree. In natural science and humanities programs, affirmative action students graduated at much lower rates, and we find no evidence of earnings gains. The general/AA gap in graduation rates is largest in natural science, consistent with research that finds that academic preparation is particularly important in STEM programs (Arcidiacono, 2004).

[14] We follow UERJ's classification of its programs into four fields: health, humanities, natural sciences, and social sciences (https://www.uerj.br/ensino/cursos-de-graduacao). But we group accounting, business administration, and economics into a separate field of "business" because networking may be especially important in these programs (Zimmerman, 2019). We do not classify UERJ's law program as "business" because roughly 70 percent its graduates obtain jobs in the public sector, which is similar to other social science programs.

3.4. **College selectivity, major choice, and educational attainment.** To provide context for the above results, we examine how admission to UERJ affected the selectivity of individuals' colleges, their major choice, and their overall educational attainment. While UERJ is an elite school, it exists in a highly-competitive market. The federal university in Rio de Janeiro, UFRJ, is even more selective than UERJ for most programs. UFRJ ranked 3$^{\text{rd}}$ in a 2012 national ranking by the newspaper *Folha*, while UERJ ranked 11$^{\text{th}}$. There are three other selective federal universities in the Rio suburbs, and more than five private universities in the city itself (see Appendix Table A9). Access to these other colleges differed significantly across admission tracks because UERJ was the only school in Rio with affirmative action during most of our sample period. Further, applicants to a particular UERJ program may have also chosen a different major if they enrolled in another school.

We examine effects on college and major choice using data from Brazil's higher education census (*Censo da Educação Superior*), which covers all colleges in the country. We do not have access to ID numbers in this dataset, so we match it to our sample of UERJ applicants using exact day of birth, gender, and year of enrollment. These variables do not uniquely identify individuals, so we define our dependent variables as the *total* number of students at a particular university or major that have the same birthdate, gender, and enrollment year as the UERJ applicant.[15] This fuzzy merge adds noise to our dependent variables, reducing the precision of our RD coefficients. In addition, individual-level census data does not exist prior to 2009, so we can only include 2009–2011 UERJ applicants in this analysis. Appendix B.5 provides details on our merge with the higher education census.

With these caveats, we find evidence that UERJ's affirmative action policy allowed Black and public school applicants to attend a more selective college. Panel A of Table 3 displays θ coefficients from our reduced-form RD specification (1), which estimates the effects of *admission* to UERJ. The number of UERJ enrollees in the census data increases by 0.88 at the affirmative action thresholds (column F), which is broadly similar to our first stage estimate of 0.69 in Table 2. We do not find effects on enrollment in UFRJ, other federal universities in Rio, or private universities in the top 100 of the *Folha* ranking. Instead, we find that the number of enrollees in lower-ranked Rio universities falls by roughly 0.5 at the affirmative action thresholds. Although these estimates are noisy, they match our prior that many affirmative action applicants would not have gained admission to other top universities, and thus likely had less-selective private schools as their fallback option.

Admission to UERJ also had a large effect on the majors of affirmative action applicants. In the last two rows of Panel A, our dependent variables measure the total number of enrollees in Rio de Janeiro universities with the same major as the one that the UERJ applicant

[15] For the average UERJ applicant in our data, there are 29 students with the same birthdate, gender, and enrollment year across all Rio de Janeiro universities.

applied to. The number of Rio-area enrollees with the same major as the applicant increases by 0.35 at the affirmative action thresholds using 2-digit major codes, and it increases by 0.46 enrollees using 3-digit majors codes. This suggests that a significant number of affirmative action enrollees would have chosen a different major if they were not admitted to UERJ. These changes in field of study are an important mechanism for our RD earnings estimates, but such changes are relevant for the evaluation of affirmative action policies in any context where individuals may choose different majors at different schools.[16]

Despite the effects on college selectivity and major choice, affirmative action students were no more likely to earn a college or postgraduate degree when they enrolled in UERJ. In Panel B of Table 3, we use the RAIS data to define three binary measures of educational attainment: 1) a college degree during the period of 6–9 years after UERJ application; 2) a college degree by 2019; and 3) a postgraduate degree by 2019.[17] We find no effects on any of these outcomes. Affirmative action applicants were disadvantaged relative to general UERJ applicants, but they were high-achieving relative to most Brazilian students. 71 percent of marginally-rejected Black and public school applicants earned a college degree by 2019 (column E), which is a high rate even by the standards of developed countries.

For general track applicants, admission to UERJ reduced the likelihood of enrolling in other top federal and private universities in Rio (Panel A of Table 3). Thus general track "compliers" would likely have attended other selective universities if they had been rejected. This may be why UERJ enrollment did not significantly affect their earnings (Table 2).

3.5. **Employment with UERJ alumni.** One of the benefits of attending an elite university is networking with peers and alumni (Rivera, 2016). Elite university networks can improve students' access to high-paying jobs through many channels, including on-campus recruiting (Weinstein, 2021), referrals (Calvo-Armengol and Jackson, 2004), and school reputation (MacLeod and Urquiola, 2015). But prior research has found that the benefits of networking at elite universities accrue primarily to high-SES students (Zimmerman, 2019; Michelman et al., 2022), so it is unclear whether affirmative action students would also benefit from these networks.

To examine network mechanisms, we use the RAIS data to define outcome variables that indicate when UERJ applicants obtained jobs at firms that are affiliated with other UERJ alumni. Specifically, consider a UERJ applicant i who applied to major m. We define applicant i as obtaining a job at an *alumni firm* if their firm employed another individual j

[16] Ng and Riehl (2020) also find that admission to a selective university has a large effect on applicants' major choice in Colombia, another Latin American country with decentralized college admissions.
[17] A caveat is that we only observe these outcomes for applicants who appear in the RAIS, but we find no evidence of selection into this dataset for affirmative action applicants (Table 2 and Appendix Table A1).

who graduated from major m (the "alum").[18] Our simplest network outcome is an indicator equal to one if the applicant's firm ever hired another alum, but we define many different versions of this variable based on the alum's characteristics, the timing of their employment, and the concentration of alumni at the firm.[19] We use major-specific networks because students in the same program often take classes together and work in similar labor markets.

Mean wages at firms affiliated with UERJ alumni firms were 0.44 log points higher than those at other firms in our sample (Appendix Table A11), suggesting that increased access to these firms may raise an individual's earnings. Appendix Table A10 shows that firms with the most UERJ alumni were big public entities like Rio de Janeiro City Hall and the State Secretary of Education. Firms with the highest alumni concentration include financial organizations like Accenture and the Brazilian Development Bank, and branches of the multinational petroleum company Petrobras.

Attending UERJ significantly increased the likelihood that affirmative action students obtained jobs at firms affiliated with other UERJ alumni. Panel A of Table 4 shows that marginal affirmative action enrollees were 14 percentage points more likely to work at a firm affiliated with any UERJ alum measured 6–9 years after application (see also Panel E of Figure 3). This is a 29 percent increase from the mean rate of employment at alumni firms for marginally-rejected Black and public school applicants (48 percent). Affirmative action enrollees were also 6 percentage points more likely to work at firms with a high concentration of UERJ alumni, defined as those that hired 10 or more alumni per 1000 workers (second row of Panel A).[20] This coefficient is 50 percent of the below-threshold mean, which shows that the effects of attending UERJ on the *odds* of employment are larger for firms with a high concentration of alumni. We also find large and significant effects on employment at alumni firms for general track applicants (columns B and D).

The remaining rows in Panel A show that UERJ enrollment increased access to specific firms *within* narrowly-defined labor markets. Since UERJ admission affected applicants' major choices (Table 3), it is possible that the effects on access to alumni firms are driven by major-specific human capital accumulation rather than by networking. To examine this possibility, we ask whether the effects on employment with other alumni are stronger at

[18] All of our network outcomes are leave-individual-out; even if an applicant completed a UERJ degree, these variables equal one only if there is *another* alum affiliated with that firm. Following Gerard et al. (2021), we define firms at the *establishment* level (organization × branch) using 14-digit CNPJ codes. See Appendix B.1 for details.

[19] Our variable definitions allow applicants to be both beneficiaries and benefactors of UERJ's alumni network. For example, an applicant could be co-employed with a UERJ alum if they got a job from the alum's referral, or they themselves referred the alum to the firm. Our outcomes are similar in spirit to Zimmerman (2019)'s measures of co-leadership rates, but we consider firm-level employment rather than leadership positions.

[20] We use the firm's mean size over all years of our data. For example, firms with 10 alumni per 1000 workers include those with a mean size of 100 employees that hired at least one UERJ program alum.

the firm or the labor market level. Specifically, we use outcome variables that measure the number of UERJ alumni per 1000 workers at both the firm and labor market levels, where we define a labor market as a municipality × 5-digit industry code. Across all applicant groups, the effects of UERJ enrollment on alumni concentration at the firm level are larger than those at the labor market level. Further, we find significant effects on the *difference* between the firm and labor market alumni concentration (last row of Panel A). Thus the presence of UERJ alumni at a firm is a strong predictor of an applicant's employment outcome even within the set of firms in the same location and industry. This suggests that our results are not solely driven by major-specific human capital accumulation.

As further evidence of networking, Figure 5 displays heterogeneity in the RD coefficients for the number of UERJ alumni per 1000 workers at the applicant's firm(s). These estimates are analogous to the third row of Panel A in Table 4, except we pool across all three applicant groups. Panel A of Figure 5 shows that the effects on alumni concentration are much larger for private firms than for public firms. Networking is likely more important at private firms because most public firms in Brazil use exams to hire workers (Mocanu, 2022). Similarly, we find larger effects in fields where graduates typically go into the private sector (business and engineering) than in fields where graduates often work in government (humanities and social sciences). Panel B of Figure 5 shows that the effects are largest for alumni from the same cohort as the applicant, and for alumni who work at the firm at the same time as the applicant. These results are consistent with referral and recruiting mechanisms, which are likely to be stronger when alumni know each other, or when they overlap in their employment.

Yet the benefits of UERJ's alumni network fade over individuals' careers, particularly for affirmative action students. Panel B of Table 4 shows RD estimates for the same alumni firm outcomes as in Panel A, but we measure employment 10–13 years after UERJ application. In all applicant groups, the effects of UERJ enrollment on employment with alumni are smaller in this later period. This decline is most pronounced for affirmative action students (column F), and the RD coefficients are not statistically different from zero for most outcomes. This suggests that alumni networks are most important for initial job placement, and that their influence declines as individuals form new networks in the labor market.

3.6. **Discussion.** We conclude our RD analysis by discussing how our results relate to other research on affirmative action and college selectivity.

Our graduation results show that most affirmative action students succeeded academically at UERJ. Related work argues that students admitted through affirmative action have lower graduation rates—particularly in STEM fields—because they are relatively less-prepared than their classmates (Arcidiacono et al., 2016). UERJ graduation rates are high by Brazilian standards, and most programs in our RD sample are in non-STEM fields (Table 1). Thus relative academic preparation may be less important for degree completion in our setting. Yet we also find no evidence of *positive* effects of UERJ's affirmative action policy on the likelihood of earning a college degree. One possibility is that any negative effects of mismatch in academic preparation were offset by positive effects of UERJ's greater resources, and the net effect on degree attainment was zero.

Our results differ from work that finds that disadvantaged students have large earnings returns to college selectivity. Zimmerman (2014) and Smith et al. (2020) find that low-income students experienced earnings gains from admission to US state university systems. Bleemer (2022) finds declines in earnings for underrepresented minority applicants when affirmative action was banned in the University of California system. The magnitudes of the earnings effects in these papers are significantly larger than our estimate of the early-career wage return for affirmative action students.[21] Each of these papers finds that disadvantaged students were more likely to earn a four-year degree when they attended selective colleges, suggesting that educational attainment is a key driver of the earnings results. This may explain why we find smaller returns for affirmative action students in the short run, and no longer-run earnings effects.

Our earnings results are more similar to those in Zimmerman (2019), who finds that disadvantaged students did not experience long-run income gains from attending elite Chilean universities. Our data allows us to examine early-career outcomes, and we find that affirmative action policy increased students' likelihood of employment at firms affiliated with UERJ alumni by 14 percentage points (Table 4). Taken together with the OLS wage premium for alumni firms (0.44 log points), these estimates suggests that increased access to alumni firms can explain nearly half of the early-career wage gains for affirmative action students (0.132 log points). But individuals' networks change as they progress in their careers, and Black and low-income students likely faced barriers to network formation in the labor market. Universities only have so much influence on their students' outcomes after they graduate, which limits the scope for affirmative action to reduce earnings inequality at schools whose value depends on networking.

4. DD specification

4.1. **Top enrollee sample.** To estimate the effects of affirmative action on untargeted students, we construct a sample of *top enrollees* who could have attended UERJ regardless of whether affirmative action existed in their cohort. For each major m, we define N_m to be

[21] Zimmerman (2014) finds that *admission* to a college in the Florida state system increased individuals' earnings by 22 percent, and admission increased the likelihood of enrolling by roughly 50 percent. Bleemer (2022) finds that enrollment in selective UC colleges declined by eight percentage points for minority applicants, and earnings fell by 0.05 log points. These imply much larger returns to selective college enrollment than our estimate of the early-career return for affirmative action students (14 percent).

the minimum number of students who enrolled through the *general* track in any cohort in 1995–2011.[22] Our top enrollee sample includes the N_m enrollees with the highest admission scores in each cohort. This is a balanced panel at the major level that includes N_m enrollees from every cohort. This sample roughly corresponds to the top half of students in each program/cohort, as 55 percent of slots were reserved for general applicants.[23] Appendix Table A12 provides summary statistics for our top enrollee sample.

4.2. **Regression model and identification assumptions.** For identification, we exploit variation in the take-up of affirmative action across UERJ's majors, as depicted in Figure 1. We use this variation in a difference-in-differences (DD) specification:

$$Y_{imc} = \gamma_m + \gamma_{cf(m)} + \pi[\text{ExposureToAA}_m \times \text{Post}_c] + \varepsilon_{imc}. \tag{3}$$

Y_{imc} is an outcome for individual i who enrolled in major m and cohort c. Our variable of interest is the interaction between a major's exposure to affirmative action and a dummy for post-AA cohorts ($\text{ExposureToAA}_m \times \text{Post}_c$). Our benchmark results use a binary measure of exposure that equals one if the share of affirmative action enrollees in the 2004–2011 cohorts was 30 percent or higher (the horizontal line in Figure 1). We include major and cohort fixed effects, and cluster standard errors at the major level.

Our key identification assumption is that the outcomes of enrollees in majors with more and less exposure would have followed parallel trends in the absence of affirmative action. A potential concern is that Brazil experienced a recession in the mid-2010s, which may have had heterogeneous impacts across UERJ's majors. To address this, we interact the cohort dummies, γ_c, with fixed effects for the field of study groups, $f(m)$, defined in Table 1: business, health, humanities, natural sciences, and social sciences. These interactions restrict our identification to comparisons between majors in the same field, which were more likely to be similarly affected by macroeconomic conditions. Below we present event study and robustness results to test the validity of our identification strategy.

We estimate equation (3) in our sample of top enrollees to examine the effects of UERJ's affirmative action policy on untargeted students. In this case, the π coefficient measures how affirmative action changed top enrollees' outcomes in more-affected majors relative to less-affected majors. We refer to these π coefficients as "spillover" effects because they reflect the impacts of affirmative action students' enrollment on top enrollees' outcomes (Arcidiacono and Vigdor, 2010). For comparison, we also present DD estimates in the sample of all other enrollees; these coefficients reflect the intended impacts of affirmative

[22] In other words, we compute the number of general track enrollees in each major m and cohort c, N_{mc}, and then define $N_m = \min_{c\in\{1995,\ldots,2011\}} N_{mc}$. On average, N_m is 53 percent of the total number of enrollees.
[23] Our top enrollee sample includes affirmative action students who were among the top N_m in their cohort. In practice, 93 percent of top enrollees were admitted through the general track (Table 1).

action on student body diversity in addition to any spillovers. Note that these DD coefficients reflect changes in outcomes for the average UERJ enrollee in each sample, whereas our RD coefficients measured outcomes for marginal enrollees relative to their outcomes if they were not admitted.

Panel A of Table 5 shows that our DD specification identifies the effects of a 19 percentage point increase in the affirmative action share in an individual's program/cohort. This is a large effect on diversity relative to the scale of affirmative action at many elite US universities, but it is similar to the magnitude of Chetty et al. (2020)'s proposed admission reform.

5. Spillover effects of affirmative action

5.1. **Potential spillover mechanisms.** Tables 5–6 present our main results on the spillover effects of affirmative action. Column (A) in each table shows the dependent variable mean for top enrollees in the pre-AA cohorts (1995–2001). Our main results are the DD coefficients, π, for top enrollees in column (B). Column (C) shows DD estimates for other enrollees, and column (D) shows DD estimates for all enrollees pooled.

Our dependent variables reflect a variety of channels through which affirmative action could affect the outcomes of untargeted students. Table 5 examines whether affirmative action impacted the composition of top enrollees. Research finds that families prefer schools with high-achieving peers (Abdulkadiroğlu et al., 2020). Thus UERJ's admission policy may have induced some students to attend other colleges, particularly since it was the first university in Rio de Janeiro to adopt affirmative action. To test for these effects, Table 5 uses the demographic characteristics and entrance exam scores of top enrollees as dependent variables. We also combine these characteristics into a single index using the predicted values from a log wage regression.

Panels A–B of Table 6 show effects on graduation and earnings. Affirmative action increased the racial and socioeconomic diversity of UERJ's student body, which may have impacted the types of jobs its students could gain access to through networking (Zimmerman, 2019). Panels C–D of Table 6 examine networking mechanisms using our alumni firm outcome variables. Affirmative action also reduced the average level of academic preparation of UERJ's student body, which may have impacted professors' level of instruction (Duflo et al., 2011) or the benefits of peer interactions (Sacerdote, 2001). We test for these learning mechanisms in Table 7 using data from Brazil's college graduation exam (described below).

5.2. **Characteristics of UERJ enrollees.** We do not find significant effects of exposure to affirmative action on observable characteristics of top enrollees. Column (B) in Table 5 shows that the DD coefficients for top enrollees' age, gender, and race are small and statistically insignificant (Panel B). We also find insignificant effects on top enrollees' scores on the field entrance exam and their overall admission score (Panel C).[24] The DD estimate for predicted hourly wage based on these characteristics is small (−0.03 log points) and statistically insignificant (Panel D). Thus, the composition of top enrollees in more- and less-affected majors did not diverge significantly with the introduction of affirmative action.

A possible explanation for this finding is that prospective students may not have known that the take-up of affirmative action would differ across UERJ's majors. Students were surely aware of the admission policy, but our DD analysis nets out school-level changes in the characteristics of top enrollees. Before enrolling, it may have been hard to know that the share of affirmative action students would be, for example, 15 percentage points lower in Economics than in Business on average. Thus our findings do not rule out the possibility that affirmative action deterred some students from attending UERJ.

By contrast, affirmative action had a large impact on the demographics and test scores of non-top enrollees (column C of Table 5). In majors with greater exposure to affirmative action, the student bodies became more racially diverse, older, and lower-ability as measured by entrance exam scores. This reflects the admission preference given to affirmative action students and the intended effects on diversity.

5.3. **Labor market outcomes.** Our main finding is that greater exposure to affirmative action reduced the post-college earnings of top enrollees. Column (B) in Table 6 shows that the mean hourly wage of top enrollees measured 6–9 years later declined by 14 percent in more-affected majors relative to less-affected majors (Panel B). The DD estimate is similar in magnitude using mean monthly earnings as the dependent variable (−170 USD). Panel A of Figure 6 shows an event-study version of this result. The log hourly wage coefficient for top enrollees (red line) drops sharply between the last pre-AA cohort (2001) and the first post-AA cohort (2004). The wage coefficient declines further over subsequent cohorts, reaching a magnitude of −0.20 log points by the 2011 cohort.

This negative earnings effect persisted over the next four years of top enrollees' careers. Appendix Table A13 shows the same outcomes as in Table 6, but we measure them 10–13 years after application. For top enrollees, the DD coefficients are −0.12 for log hourly wages and −224 for mean monthly earnings. These are similar to the estimates measured 6–9 years after application.

The negative earnings effect for top enrollees was largely driven by a reduction in firm quality as measured by firm average wages. The DD estimate for log firm mean hourly wage is −0.095 for top enrollees, which is 70 percent of the magnitude of the individual wage coefficient. The event-study coefficients for firm average wage also decline sharply in the first cohort with affirmative action (Panel B of Figure 6). We find no effect of exposure to

[24] We standardize scores to mean zero and SD one in the population of all UERJ enrollees in a given cohort.

affirmative action on the graduation rates of top enrollees (Panel A of Table 6), suggesting that the earnings effect is not driven by changes in educational attainment. The DD estimate for employment in any formal sector job is negative and marginally significant (-0.027), but it is relatively small compared to the mean formal employment rate (0.74).

Appendix Figure A5 provides support for our key identification assumption of parallel trends. This assumption could be violated if exposure to affirmative action was correlated with wage growth in industries that typically hired students from these majors. To test for this possibility, we first compute the mean hourly wage in each industry $\times$ year pair using all workers in the RAIS data. We then compute a weighted average of these industry $\times$ year means for each UERJ major, where the weights are the share of pre-AA top enrollees who were employed in each industry. Panel A of Appendix Figure A5 shows that, across all years of our data, these industry mean wages trended similarly between majors with more and less exposure to affirmative action. Panel B presents an event study version of this analysis; in the years in which post-AA graduates entered the labor market (2009–2019), we find a small and statistically insignificant decrease in industry mean wages (-0.02 log points) in more-affected majors relative to less-affected majors. This suggests that our earnings results in Table 6 are not driven by different rates of development across sectors of the Brazilian economy, or by heterogeneous impacts of the mid-2010s recession.

Appendix Table A15 shows that our results for top enrollees are robust to a variety of other specification checks. Our estimates for top enrollees' hourly wages do not change significantly if we restrict our regression sample to pre-recession years (column B), or if we include program-specific linear trends estimated in the pre-AA cohorts (column C). Controlling for student demographics and entrance exam scores only slightly reduces the magnitude of the wage coefficient (column D), consistent with the small compositional effects identified in Table 5. We continue to find effects on individual and firm mean wages when we compare programs in the same quartile of selectivity (column E), as defined by the x-axis in Figure 1, or if we exclude the field of study controls (column F). Lastly, our results are similar when we use a continuous measure of each major's affirmative action exposure (column G); in this specification, we define ExposureToAA_m as the share of all enrollees in 2004–2011 cohorts who were from an affirmative action track (i.e., the y-axis in Figure 1).

We find even larger declines in earnings for lower-ranked enrollees in majors with greater exposure to affirmative action (column C of Table 6). The DD estimate for hourly wages is larger in magnitude than the predicted wage effect based on individual characteristics (-0.212 vs. -0.154), suggesting that spillover effects may have also reduced the wages of affirmative action students.

5.4. **Networking mechanisms.** To examine the role of networking in our earnings results, Panels C–D of Table 6 use dependent variables that measure employment in firms affiliated with UERJ alumni. We define these alumni firm outcomes in the same way as in our RD analysis (Section 3.5), but we compute different versions based on the alum's cohort and application track. This allows us to ask whether affirmative action changed the types of jobs that UERJ students obtained through networking.

In Panel C of Table 6, we find that affirmative action reduced the likelihood that top enrollees obtained jobs at firms that hired UERJ alumni from the pre-AA cohorts. In the first row of Panel C, the outcome variable is an indicator for employment at firms that hired an alum from the 1995–2001 cohorts of the enrollee's program. For top enrollees, the likelihood of employment at firms with pre-AA alumni declined by 5.5 percentage points in more-affected majors relative to less-affected majors (column B). Conversely, top enrollees in the most-affected majors became 4.9 percentage points more likely to work at firms that hired *only* alumni from the post-AA cohorts (second row of Panel C).

Panel D of Table 6 shows that affirmative action also reduced top enrollees' likelihood of employment with general track alumni from their own cohort. In the first row of Panel D, the dependent variable is an indicator for employment at firms that hired a general track alum from the same cohort as the enrollee. For top enrollees, the likelihood of employment with same-cohort alumni from the general track declined by 9.8 percentage points in more-affected majors relative to less-affected majors. The bottom three rows of Panel D show that top enrollees were instead more likely to work with alumni from a different cohort, or with alumni from an affirmative action track.[25] Specifically, greater exposure to affirmative action increased the likelihood of employment with general track alumni from a different cohort by 4.2 percentage points, and it increased the likelihood of employment at firms that hired *only* affirmative action alumni by 4.6 percentage points.

Affirmative action may have altered top enrollees' job outcomes through a number of different networking channels. The findings in Panel C could arise if employers that typically hired from UERJ changed their recruiting behavior (Weinstein, 2021) or their expectations on the ability of UERJ graduates (MacLeod et al., 2017) in response to affirmative action. In Panel D, the results are more likely to be driven by mechanisms where peer connections are important, such as referrals or information sharing. If peer connections matter for UERJ students' job outcomes, then our results are not surprising because students in majors with high exposure to affirmative action had fewer general track peers in their cohort.

[25] We define the outcomes in Panels C–D of Table 6 to be non-overlapping. For example, in the second row of Panel D, the dependent variable equals one only if the firm did *not* hire a general track alum from the enrollee's own cohort.

Since general track alumni typically obtained higher-paying jobs, the results in Panels C–D suggest that affirmative action reduced the value of UERJ's alumni networks. Appendix Table A11 shows that mean hourly wages were roughly 0.2 log points higher at firms that hired pre-AA alumni than at firms that hired only post-AA alumni. Similarly, jobs with general track alumni also had a wage premium of about 0.2 log points relative to firms that hired only alumni from an affirmative action track. This suggests that a decline in the value of networking can partly explain the negative effects of affirmative action on top enrollees' earnings. We quantify the magnitude of these effects in the discussion section below.

5.5. **Learning mechanisms.** To examine the role of learning mechanisms, we use data from Brazil's national college exit exam, the ENADE (*Exame Nacional de Desempenho dos Estudantes*). The ENADE is a field-specific exam that is designed to measure the quality of higher education programs. It has been administered every year since 2004, although each field is tested every three years on a staggered schedule.[26] The exams consist of both a field-specific component and a general component that is common across all fields. The government assigns ratings to each higher education program based in part on their students' ENADE scores, so many universities ask students who are close to graduation to take the exam (Pedrosa et al., 2013). But the ENADE is typically low stakes from the student's perspective, as most universities, including UERJ, do not require students to pass the exam in order to graduate.

Table 7 shows how affirmative action affected the characteristics and performance of UERJ's ENADE exam takers. This table presents difference-in-differences (DD) estimates that compare UERJ to other federal and state universities that did not implement affirmative action until 2012 or later.[27] The sample includes students who took the ENADE exam in 2004–2015. Column (A) shows the means of each outcome in the pre-AA cohorts at UERJ; we define 2004–2006 as the pre-AA period since most students who took the ENADE exam in these years enrolled prior to 2003. Column (B) displays DD estimates for all exam takers, which are the coefficients on an indicator for UERJ interacted with an indicator for the post-AA cohorts (2007–2015).[28] We do not have the ENADE data linked to our UERJ records

[26] For example, the Medicine test was administered in 2004, 2007, 2010, etc., and the Economics test was administered in 2006, 2009, 2012, etc.

[27] See Appendix Table A16 for details on our ENADE sample and the exam fields.

[28] Our DD specification for Table 7 is

$$Y_{mjt} = \gamma_{mj} + \gamma_{mt} + \pi[\text{UERJ}_j \times \text{Post}_t] + \varepsilon_{mjt}. \tag{4}$$

Regressions are at the exam field (m) by institution (j) by year (t) level, with observations weighted by the number of exam takers. We include institution fixed effects, γ_{mj}, and year fixed effects, γ_{mt}, and we interact both with field dummies so that identification comes only from within-field comparisons. The coefficient of interest, π, is on an indicator for UERJ interacted with an indicator for the post-AA cohorts (2007–2015). We cluster standard errors at the institution level.

at the individual level, so we cannot estimate this regression in our top enrollee sample. As an alternative, column (C) restricts the sample to white students who attended private high schools, who were not eligible for affirmative action. Column (D) presents results for exam takers who were non-white and/or attended a public high school.

In Panel A of Table 7, we find that the number of UERJ students who took the ENADE exam did not change significantly when it adopted affirmative action. In the pre-AA cohorts, 36 UERJ students took each ENADE field exam on average (column A). The number of exam takers in the post-AA cohorts increased by only four students at UERJ relative to other universities (column B). The change in the number of UERJ exam takers is not statistically significant measured either in levels or in logs. But affirmative action significantly increased the diversity of UERJ's exam takers relative to those at other universities, as intended. The bottom rows of Panel A show that UERJ's post-AA exam takers were 13 percentage points less likely to be white or to have attended a private high school.

Panel B of Table 7 shows that affirmative action decreased the performance of UERJ students on the ENADE exam. ENADE scores are expressed as the proportion of correct answers, and the overall score is a weighted average of the field-specific and general components. In the pre-AA cohorts, the average UERJ student correctly answered 66 percent of the general questions and 52 percent of the field-specific questions (column A). On both components, the proportion of correct answers declined by four percentage points in the post-AA cohorts at UERJ relative to the same cohorts at other universities (column B). The decline in UERJ's overall scores (−3.8pp) is 26 percent of the standard deviation of scores in the full population of ENADE exam takers (14.4pp).

Notably, we also find declines in the ENADE scores of UERJ's white students from private high schools, who were not targeted by the affirmative action policy. The estimates in column (B) of Table 7 are likely attributable, at least in part, to the intended effects of affirmative action on student body diversity. Yet column (C) shows that affirmative action also reduced the scores of UERJ's white private high school students by roughly two percentage points (Panel B). It is possible that this result is also driven by compositional effects, but we do not find significant changes in the demographics of UERJ's white students from private high schools (Panel A).[29] All else equal, one would expect *positive* selection in this sample because the bar for admission was higher in the cohorts with affirmative action.

Figure 7 shows that the decline in UERJ's ENADE performance appears even at the highest quantiles of the score distribution. This figure plots DD coefficients in which the dependent variables measure quantiles of ENADE scores within each exam field × institution

[29] We find some evidence that UERJ's white private high school students were *higher* income after the adoption of affirmative action (Panel A of Table 7). But we do not place much weight on this result because income is measured relative to Brazil's national minimum wage, which varies significantly over time.

$\times$ year cell (rather than the mean score, as in Table 7). We find that the decline in UERJ's scores is largest at low quantiles, consistent with the compositional effects of affirmative action. Yet the 80th and 90th percentiles of UERJ's score distribution also declined by about two percentage points on both exam components.

The findings in Figure 7 and column (C) of Table 7 suggest that affirmative action reduced the learning of UERJ's top students. At high quantiles and in the white private high school sample, the declines in ENADE performance are not likely to be attributable to changes in the set of students who gained admission under affirmative action. Further, we find no evidence of negative selection into this sample as measured by demographic characteristics. Thus we take these results as evidence that the negative effects of affirmative action on top enrollees' earnings are partly driven by learning spillovers.[30]

5.6. **Discussion.** Our results show that UERJ's adoption of affirmative action reduced the earnings of students who were not targeted by the policy. Our point estimates suggest that a 19 percentage point increase in the affirmative action share led to a 14 percent decrease in the wages of highly-ranked students. This is similar in magnitude to Arcidiacono and Vigdor (2010)'s finding that a one percentage point increase in the share of minority students at selective U.S. colleges is associated with a 0.8 percent decrease in other students' earnings. The negative effects on top students' earnings may have been even larger in UERJ's most selective majors since the affirmative action share was 45 percent.

In a back-of-the-envelope calculation, our estimates for compositional, networking, and learning mechanisms can jointly explain two-thirds of the decrease in top enrollees' hourly wages. Although we do not find statistically significant effects on the characteristics of top enrollees, our point estimate for the log wage index in Panel D of Table 5 (-0.033) is 25 percent of our main estimate on log wages (-0.132). If we combine the DD estimates for access to alumni firms (Panels C–D of Table 6) with the OLS wage premia associated with these jobs (Appendix Table A11), we can explain 10–17 percent of the overall wage effect. We do not have the ENADE exam scores linked to wages, but Reyes (2022) finds that a one percentage point increase in the proportion of correct answers on Brazil's national college entrance exam (ENEM) is associated with a 0.02 log point increase in early-career wages. Assuming that the relationship between correct answers and wages is the same on the ENADE exam, the decline in overall scores for white private high school students in Panel B of Table 7 (2.2pp) can explain 32 percent of the overall wage effect. Appendix B.6 provides details on these back-of-the-envelope calculations.

[30] Appendix Table A17 shows that our ENADE findings are similar (but less precise) if we use a modified version of equation (4) that also compares UERJ majors with more and less exposure to affirmative action.

6. Conclusion

This paper examined the direct and spillover effects of affirmative action at the State University of Rio de Janeiro (UERJ), which is one of the most prestigious universities in Brazil. For students admitted through affirmative action, we found that attending UERJ caused a modest increase in early-career earnings, but these gains faded as their careers progressed. Conversely, the affirmative action policy led to a large decrease in the earnings of UERJ's other students, and these negative effects persisted up to 13 years later.

Our findings show that affirmative action may be less effective at reducing income disparities at elite universities. There is compelling evidence that colleges with greater resources can help disadvantaged students graduate, and that this channel can lead to a persistent gain in earnings. But this mechanism is less relevant at elite universities because all students on the margin of admission are high achieving, and many are likely to earn a degree regardless of where they enroll. The efficacy of affirmative action may be lower if the value of an elite education depends on networking, particularly in countries like Brazil where there is substantial racial and socioeconomic segregation in the labor market. Indeed, we found that attending UERJ increased affirmative action students' access to high-paying jobs at the start of their careers, but this benefit also faded over time.

In addition, our results show why elite universities may be hesitant to unilaterally admit a large number of students from disadvantaged backgrounds. Elite schools around the world use admission policies that favor high-achieving and wealthy students (Arcidiacono et al., 2022). These policies allow faculty to teach material at a more advanced level, and they allow students to interact with well-connected peers. We found that both of these mechanisms are important at UERJ, as affirmative action reduced the human capital accumulation of highly-ranked students and led to a decline in the value of alumni and peer connections. Universities that care about their reputations may be reluctant to adopt large-scale affirmative action in the absence of government intervention or social pressure. At UERJ, in fact, affirmative action came about through a state law rather than by university initiative.

Other policies may be necessary to improve the efficacy of affirmative action at elite universities. Policies that reduce discrimination in the labor market may help to lessen elite universities' preference for wealthy students. Further, a significant increase in the diversity of students at *many* elite universities could meaningfully reduce segregation in high-paying jobs. This was the aim of a 2012 national law in Brazil, which required that all federal public universities adopt affirmative action at a similar scale as that at UERJ. Our results show that the effects of such admission reforms are hard to estimate ex ante, so we hope future research will shed light on their efficacy.

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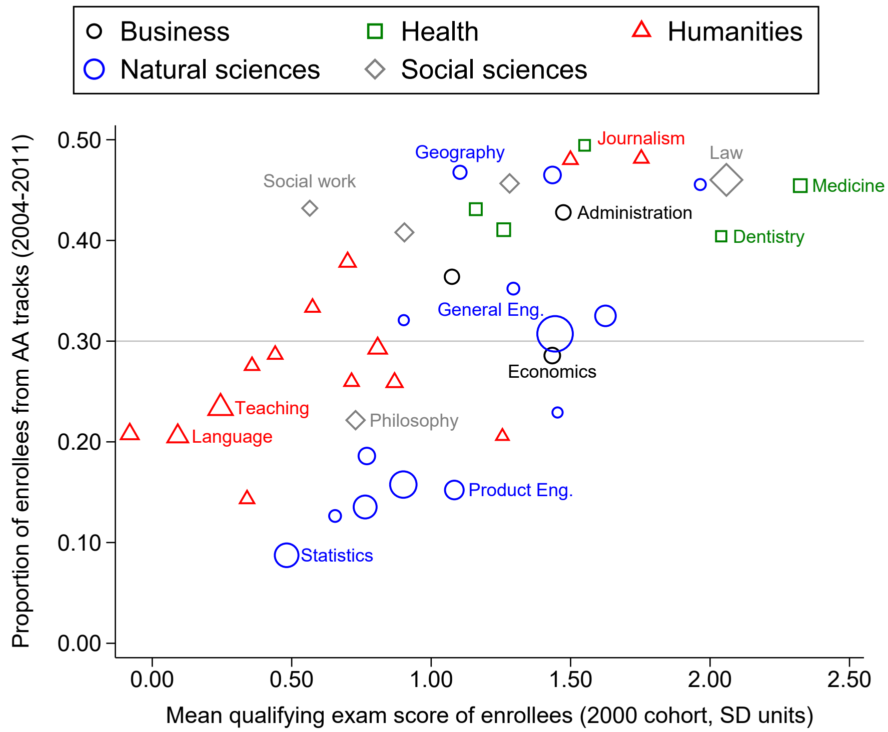


Figure 1. Take-up of affirmative action and program selectivity

Notes: This figure plots exposure to affirmative action (y-axis) and selectivity (x-axis) for each UERJ program in our sample. The y-axis displays the fraction of enrollees in the 2004–2011 cohorts who entered through an affirmative action track. The x-axis displays the mean score on the 2000 qualifying exam for students who enrolled in each program. We compute each applicant's average score across subjects in the exam, and standardize the scores to mean zero and SD one in the entire population of qualifying exam takers. The figure does not display two programs in our sample for which we do not have scores in the 2000 qualifying exam (mechanical engineering and production engineering). Marker sizes are proportional to the number of enrollees.

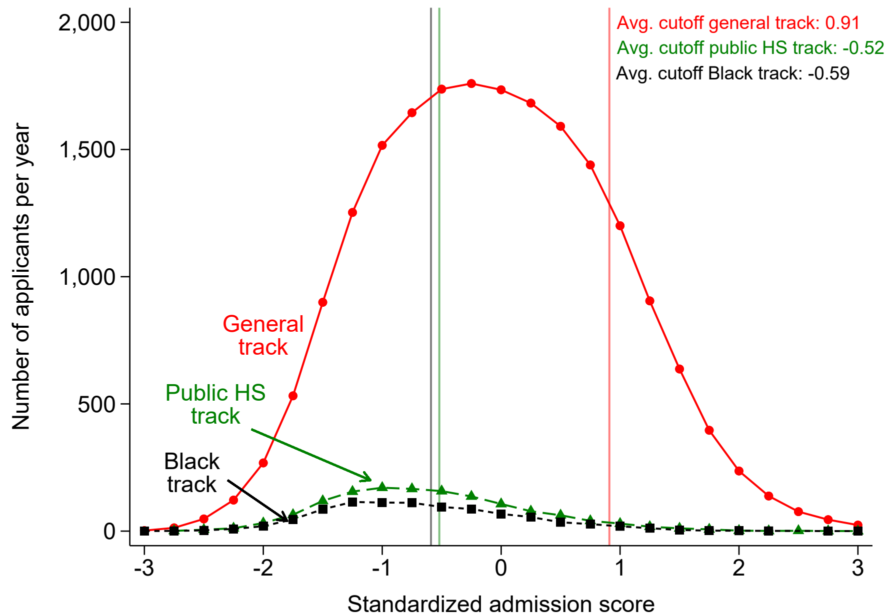
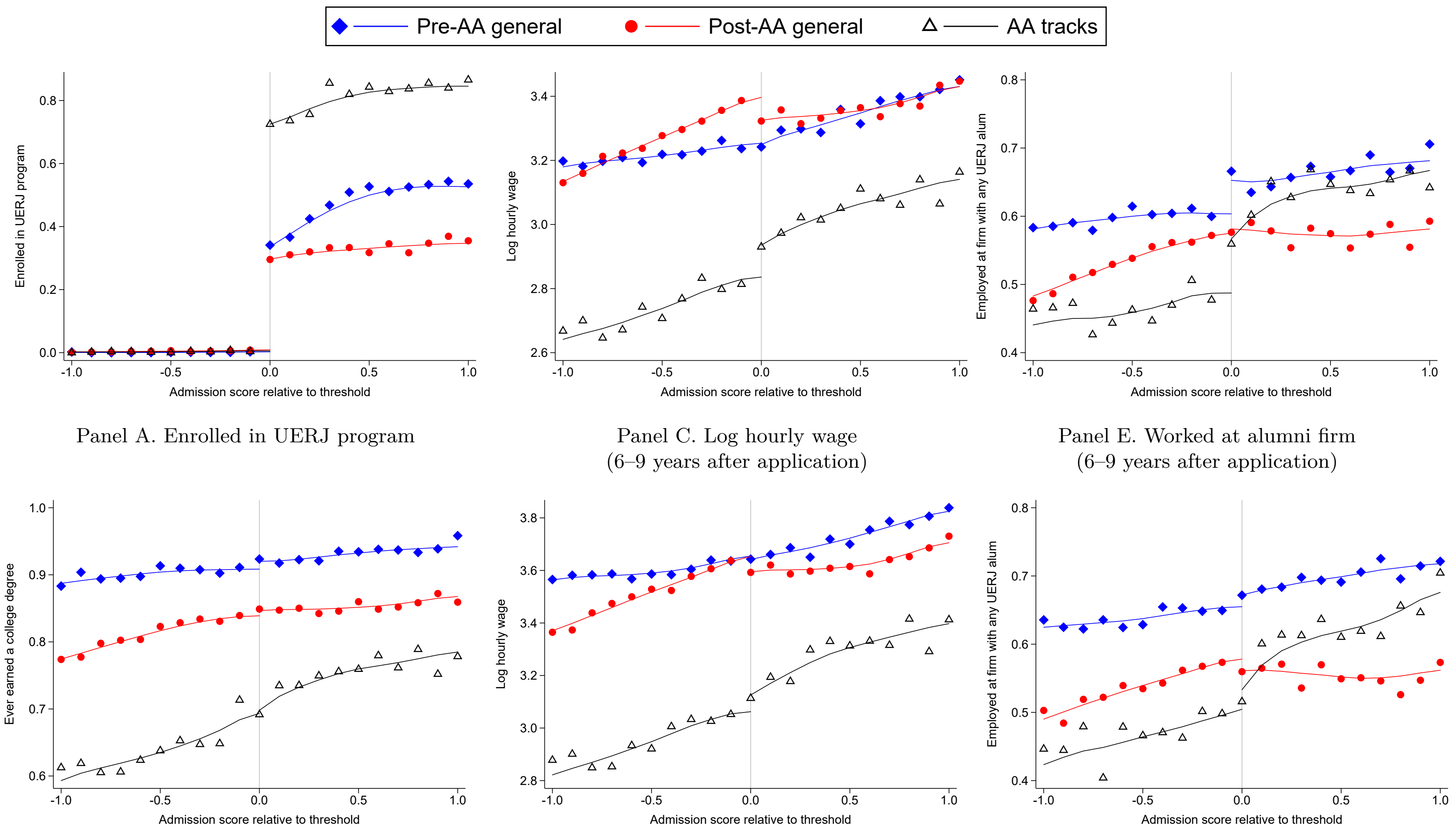


Figure 2. Admission score distribution and mean cutoff by application track (2004–2011)

Notes: This figure shows the distribution of standardized admission scores for applicants in each application track. The sample includes the 24 programs in our RD sample (Panel A of Table 1). We standardize scores to be mean zero and SD one in the population of all applicants in the same program/cohort, and plot distributions in 0.25 SD bins of the standardized score. Vertical lines represent the (applicant-weighted) average admission cutoff in each track. The cutoffs are equal to the standardized scores of the last admitted students.



Panel A. Enrolled in UERJ program

Panel C. Log hourly wage (6–9 years after application)

Panel E. Worked at alumni firm (6–9 years after application)

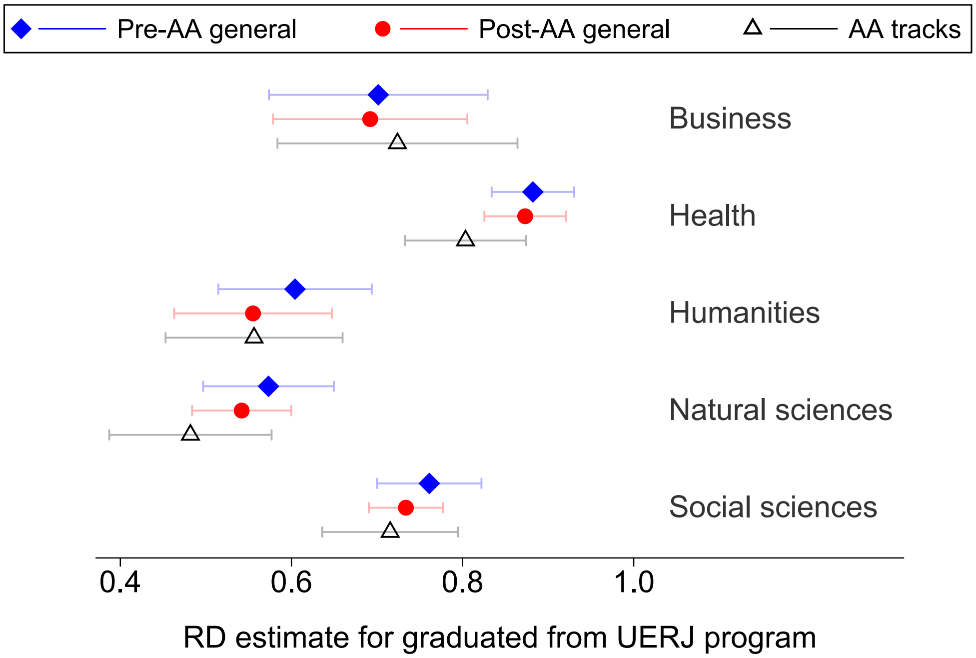
Panel B. Ever earned a college degree

Panel D. Log hourly wage (10–13 years after application)

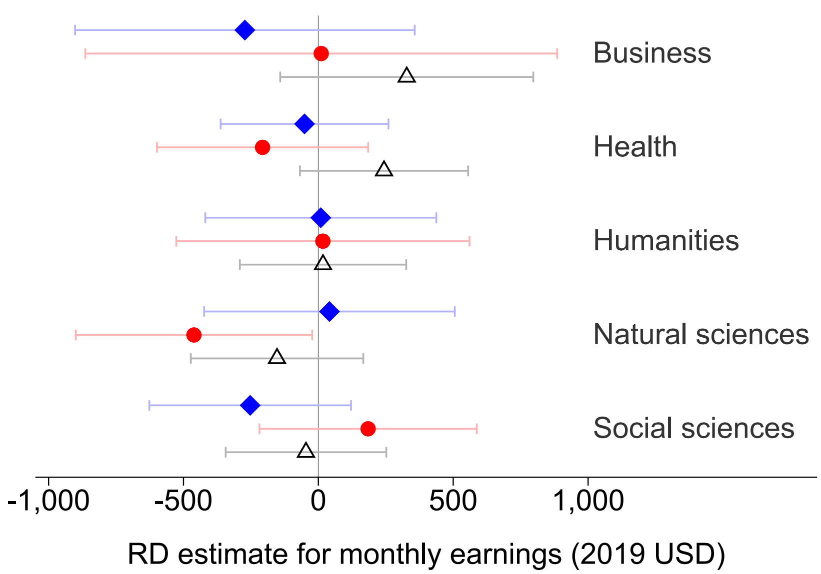
Panel F. Worked at alumni firm (10–13 years after application)

FIGURE 3. RD graphs for UERJ enrollment, earnings, and employment at alumni firms

Notes: This figure presents RD graphs for pre-AA general applicants (blue diamonds), post-AA general applicants (red circles), and Black/public school applicants (black triangles). The x-axis in each panel is an applicant's standardized admission score normalized to zero at the cutoff. The variable on the y-axis of each graph is listed in the panel title. Outcomes in Panels C and E are measured 6–9 years after individuals applied to UERJ. Outcomes in Panels D and F are measured 10–13 years after application. Markers depict means in 0.1 SD bins of the standardized score. Lines are predicted values from local linear regressions estimated separately above and below the threshold with a triangular kernel.



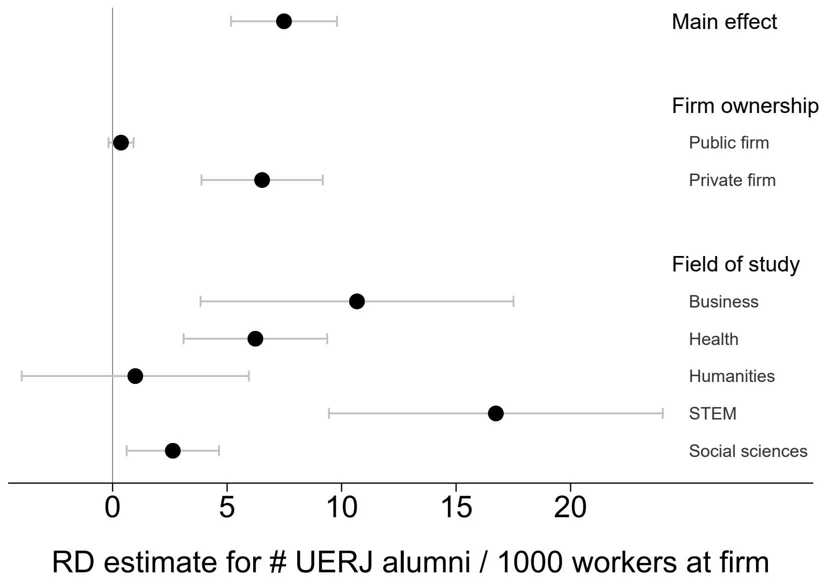
Panel A. Graduated from UERJ program



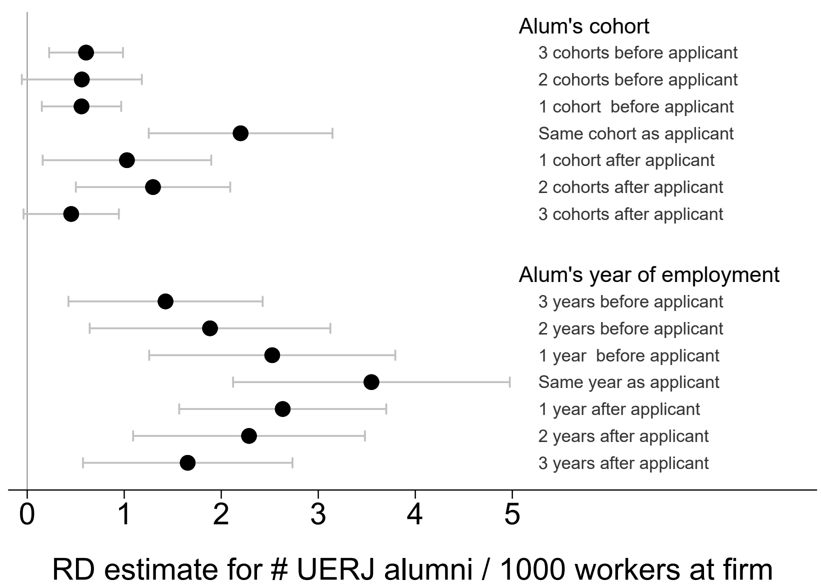
Panel B. Monthly earnings (2019 USD)

FIGURE 4. Heterogeneity in RD estimates by field of study

Notes: This figure displays RD estimates for the effects of UERJ enrollment on program completion and monthly earnings by applicant group and field of study. We estimate our 2SLS RD regression (2) separately for each of our three applicant groups (pre-AA general, post-AA general, and affirmative action) and for the five field of study groups in Panel A of Table 1 (business, health, humanities, natural sciences, and social sciences). In Panel A, the dependent variable is an indicator for graduating from the UERJ program by 13 years after applying. In Panel B, the dependent variable is mean monthly earnings (in 2019 U.S. dollars) 10–13 years after applying. Markers depict the RD coefficients, β, coefficients from these regressions, and horizontal bars are 95 percent confidence intervals using standard errors clustered at the individual level.



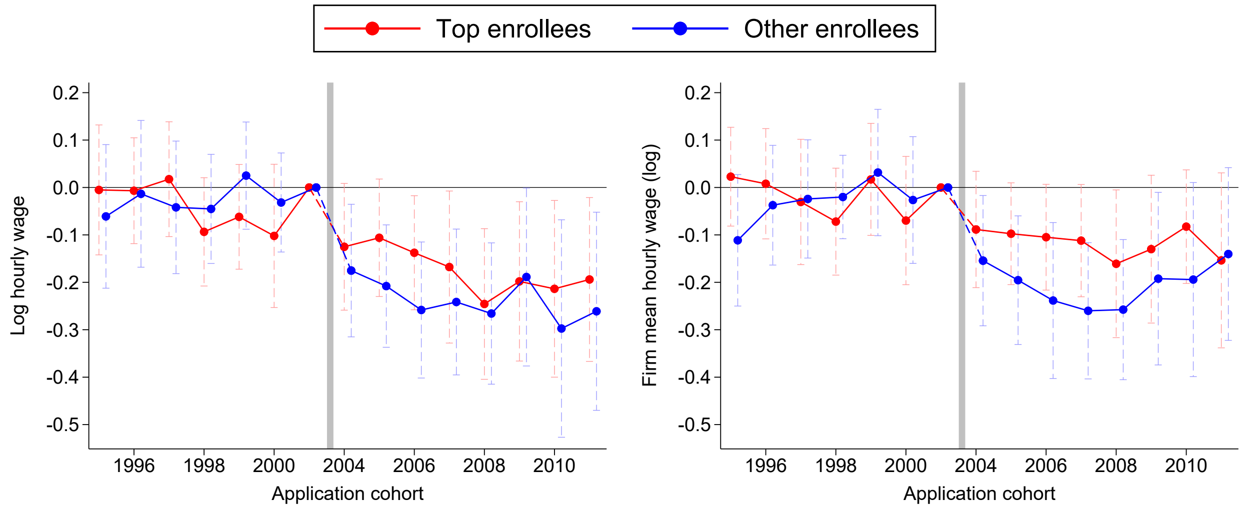
Panel A. Firm ownership and field of study



Panel B. Cohort and employment timing

FIGURE 5. Heterogeneity in RD estimates for alumni per 1000 workers at the firm

Notes: This figure displays RD estimates for the effects of UERJ enrollment on the mean number of alumni per 1000 workers at the applicants' firms measured 6–9 years after application. These estimates are analogous to the 2SLS RD coefficients in the third row of Panel A, except we estimate equation (2) pooling across all applicant groups. Panel A shows the main effect across all applicants, and separate estimates for the five field of study groups in Panel A of Table 1. Panel A also shows estimates in which we interact the dependent variable with an indicator for public or private firms. Panel B shows estimates in which we compute the dependent variables separately using alumni who enrolled in UERJ in each cohort from 3 years before to 3 years after the applicant's cohort. Panel B also shows estimates in which we also compute the dependent variables using alumni who worked at the firm in each year from 3 years before to 3 years after the applicant. Markers depict the RD coefficients, β, coefficients from these regressions, and horizontal bars are 95 percent confidence intervals using standard errors clustered at the individual level.



Panel A. Log hourly wage

Panel B. Firm mean hourly wage (log)

FIGURE 6. Event study estimates for individual and firm mean hourly wages 6–9 years after application

Notes: This figure plots coefficients from an event-study version of our DD regression (3). These are π_c coefficients that we estimate by replacing Post_c with dummies for each cohort (omitting 2001). Dashed lines are 95% confidence intervals using standard errors clustered at the program level. The dependent variables are log hourly wage (Panel A) and firm mean log hourly wage (Panel B), each measured 6–9 years after application. We estimate regressions separately for top enrollees (red markers) and other enrollees (blue markers).

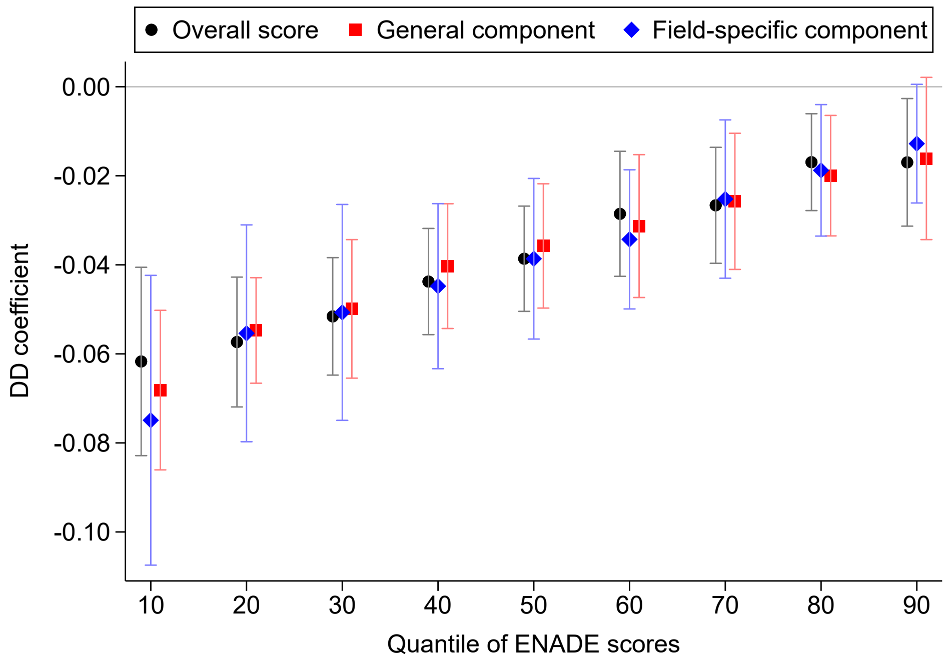


Figure 7. Effects of affirmative action at UERJ on quantiles of ENADE scores

Notes: This table displays DD estimates of the effect of UERJ's affirmative action policy on quantiles of its graduates ENADE exam scores. We estimate the same DD regressions as in Panel B of Table 7, but the dependent variables are quantiles of ENADE scores within each institution × program × cohort cell (rather than mean ENADE scores). Markers represent the DD coefficients, with the quantile depicted on the x-axis and the coefficient value on the y-axis. Vertical bars are 95 percent confidence intervals using standard errors clustered at the institution level.

TABLE 1. Summary statistics for RD and DD samples

Panel A. Programs in both RD and DD samples

	(A)	(B)	(C)	(D)	(E)
	1995–2001	2004–2011 cohorts			
Sample sizes and characteristics of all applicants	General track	General track	Public high school	Black	Other AA
Total applicants	95,659	159,408	10,996	7,263	318
Applicants in RD sample	93,930	159,383	9,624	5,600	0
Enrollees in DD sample	15,512	11,588	4,465	3,241	211
Top enrollees in DD sample	7,932	8,922	362	178	2
Female	0.50	0.55	0.60	0.60	0.48
Age at applicantion	20.75	20.28	21.88	23.04	24.30
White (UERJ data)		0.64	0.49	0.03	0.35
White (RAIS data)	0.78	0.67	0.57	0.15	0.48
Mother has a high school degree		0.85	0.49	0.56	0.54
HH income > 1.5× min. wage		0.82	0.35	0.35	0.45

Included programs (24 in total):

Business: Accounting, Business Administration.

Health: Biological sciences, Dentistry, Medicine, Nursing, Nutrition.

Humanities: Greek/Latin/Literature, History Ed. (SGO), Journalism, Psychology.

Natural sciences: Chemical Engineering, Chemistry, Computer Science, General Engineering, Geography, Geology, Industrial Design, Mechanical Engineering, Production Engineering.

Social sciences: History, Law, Social Science, Social Work.

Panel B. Programs in DD sample only

	(A)	(B)	(C)	(D)	(E)
	1995–2001	2004–2011 cohorts			
Sample sizes and characteristics of all applicants	General track	General track	Public high school	Black	Other AA
Total applicants	47,633	50,553	4,374	2,118	58
Applicants in RD sample	0	0	0	0	0
Enrollees in DD sample	13,765	14,105	2,469	1,326	38
Top enrollees in DD sample	8,534	9,179	495	253	9
Female	0.56	0.53	0.62	0.63	0.57
Age at applicantion	22.34	21.62	22.54	24.09	26.24
White (UERJ data)		0.59	0.49	0.03	0.32
White (RAIS data)	0.75	0.65	0.60	0.20	0.47
Mother has a high school degree		0.78	0.45	0.52	0.43
HH income > 1.5× min. wage		0.74	0.28	0.30	0.25

Included programs (19 in total):

Business: Economics.

Humanities: Art, Biological Sciences (SGO), English/German/Japanese, Geography Ed. (SGO), Language (SGO), Math Ed. (SGO), Teaching, Teaching (DDC), Physical Ed., Spanish/French/Italian.

Natural sciences: Cartographic Engineering, Math, Mechanical Engineering (NF), Oceanography, Physics, Production engineering (RES), Statistics.

Social sciences: Philosophy.

Notes: This table reports summary statistics for UERJ applicants to programs in our sample. Panel A includes programs that are in our RD and DD samples (24 programs). Panel B includes programs in our DD sample only (19 programs). Programs are at UERJ's main campus in Rio unless denoted with parentheses. Column (A) includes applicants in the pre-AA cohorts. Columns (B)–(E) include applicants to the four admission tracks in the post-AA cohorts. See Appendices B.1 and B.4 for details on variable definitions and the sample.

Table 2. RD estimates of the effects of UERJ enrollment on graduation and earnings

	(A)	(B)	(C)	(D)	(E)	(F)
	1995–2001 general track		2004–2011 general track		2004–2011 AA tracks	
Dependent variable	Mean below	RD coef	Mean below	RD coef	Mean below	RD coef
Panel A. First stage						
Enrolled in UERJ program	0.003	0.313^{***}	0.008	0.292^{***}	0.004	0.689^{***}
		(0.010)		(0.006)		(0.014)
N	3,234	17,519	4,012	47,838	543	6,121
Panel B. Graduation and earnings 6–9 years after application (2SLS)						
Graduated from UERJ program	0.002	0.711^{***}	0.003	0.677^{***}	0.004	0.640^{***}
		(0.017)		(0.013)		(0.018)
Formal employment	0.627	0.064^{**}	0.672	−0.031	0.729	−0.002
		(0.029)		(0.027)		(0.026)
Log hourly wage	3.237	−0.003	3.387	−0.079	2.813	0.132^{***}
		(0.050)		(0.049)		(0.044)
Monthly earnings (2019 USD)	1,356.069	0.295	1,390.819	-153.473^{**}	816.821	110.230^{**}
		(75.313)		(77.290)		(49.546)
N (formal employment)	3,234	37,794	4,012	55,030	543	8,147
N (log hourly wage)	2,027	24,564	2,694	32,972	394	6,100
Panel C. Graduation and earnings 10–13 years after application (2SLS)						
Graduated from UERJ program	0.002	0.718^{***}	0.003	0.693^{***}	0.003	0.661^{***}
		(0.017)		(0.014)		(0.021)
Formal employment	0.693	0.032	0.686	−0.026	0.714	0.037
		(0.027)		(0.031)		(0.039)
Log hourly wage	3.636	0.005	3.637	0.005	3.052	0.024
		(0.054)		(0.058)		(0.063)
Monthly earnings (2019 USD)	2,005.191	−84.946	1,757.947	−99.418	1,041.942	56.577
		(94.587)		(109.084)		(75.202)
N (formal employment)	3,234	39,134	2,974	41,138	388	4,320
N (log hourly wage)	2,237	24,273	2,021	26,407	273	3,746

Notes: This table presents RD estimates for the effects of UERJ enrollment on graduation, formal employment, and earnings. Columns (A), (C), and (E) show means of each dependent variable for applicants in each group who scored $(-0.1, 0)$ SDs below the cutoff. In Panel A, columns (B), (D), and (F) show reduced-form RD coefficients, θ, from equation (1), which measure the effects of UERJ admission on UERJ enrollment. In Panels B and C, these columns show 2SLS RD coefficients, β, from equation (2), which measure the effects of UERJ enrollment on graduation and labor market outcomes. Panel B measures outcomes 6–9 years after application, and Panel C measures outcomes 10–13 years after application. Parentheses contain standard errors clustered at the individual level. We use the Calonico et al. (2014) bandwidth for each outcome, so sample sizes differ across dependent variables.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3. RD estimates for enrollment in other universities and degree attainment

	(A)	(B)	(C)	(D)	(E)	(F)
	1995–2001 general track		2004–2011 general track		2004–2011 AA tracks	
Dependent variable	Mean below	RD coef	Mean below	RD coef	Mean below	RD coef
Panel A. Enrollment in Rio de Janeiro universities (reduced form, 2009–2011 cohorts only)						
# enrolled in UERJ			1.465	0.271***	1.051	0.880***
				(0.037)		(0.088)
# enrolled in UFRJ			3.369	−0.147***	2.381	−0.111
				(0.057)		(0.137)
# enrolled in other federal universities			4.407	−0.165**	3.181	0.041
				(0.083)		(0.168)
# enrolled in a top-100 private university			5.154	−0.176**	4.312	0.147
				(0.077)		(0.164)
# enrolled in other private universities			5.110	−0.041	5.181	−0.457**
				(0.062)		(0.229)
# enrolled in same progam area (2-dig)			3.448	0.120**	2.647	0.351**
				(0.059)		(0.138)
# enrolled in same progam area (3-dig)			1.661	0.192***	1.367	0.459***
				(0.040)		(0.082)
N (enrolled in UERJ)			1,553	19,895	215	2,757
Panel B. Educational attainment measured in RAIS (2SLS)						
Any college degree, 6-9 years later	0.731	0.044	0.785	0.006	0.636	−0.002
		(0.032)		(0.029)		(0.038)
Ever earned a college degree	0.911	0.012	0.839	0.026	0.713	0.010
		(0.017)		(0.025)		(0.033)
Ever earned a graduate degree	0.107	−0.004	0.069	−0.017	0.051	−0.006
		(0.020)		(0.017)		(0.015)
N (ever college degree)	2,417	32,718	2,925	36,617	415	5,978

Notes: This table presents RD estimates for enrollment in Rio de Janeiro universities and educational attainment.

In Panel A, columns (B), (D), and (F) show reduced-form RD coefficients, θ, from equation (1). The dependent variables are the *total* number of enrollees in a given group of universities or field of study who share the applicant's birthdate, gender, and enrollment year. We measure these totals in Brazil's higher education census (see Appendix B.5). We categorize universities into four groups based on ownership and selectivity: 1) The federal university in the municipality of Rio de Janeiro (UFRJ); 2) The three other federal universities in the suburbs of Rio de Janeiro (UFF, UFRRJ, UNIRIO); 3) Private universities in the municipality of Rio de Janeiro that ranked in the top 100 of the 2012 *Folha* ranking (PUC-Rio, UNESA); and 4) Other private universities in the municipality of Rio de Janeiro (UGF, UVA, UCAM, Universo, UCB). We categorize fields of study using 2- and 3-digit census major codes. Regressions include only 2009–2011 UERJ applicants, and they include gender and age dummies to increase precision.

In Panel B, columns (B), (D), and (F) show 2SLS RD coefficients, β, from equation (2). Regressions include all UERJ cohorts, and the dependent variables are indicators for educational attainment measured in the RAIS data.

In both panels, columns (A), (C), and (E) show means of each dependent variable for applicants in each group who scored $(-0.1, 0)$ SDs below the cutoff. Parentheses contain standard errors clustered at the individual level. We use the Calonico et al. (2014) bandwidth for each outcome, so sample sizes differ across dependent variables.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4. RD estimates for employment at alumni firms

	(A)	(B)	(C)	(D)	(E)	(F)
	1995–2001 general track		2004–2011 general track		2004–2011 AA tracks	
Dependent variable	Mean below	RD coef	Mean below	RD coef	Mean below	RD coef
Panel A. Employment 6–9 years after application (2SLS)						
Employed at firm with any UERJ alum	0.600	0.118***	0.572	0.070**	0.477	0.137***
		(0.033)		(0.034)		(0.038)
Employed at firm with 10+ alumni per 1000 workers	0.210	0.092***	0.215	0.102***	0.124	0.062**
		(0.025)		(0.032)		(0.029)
# UERJ alumni per 1000 workers at firm	6.561	4.652***	7.120	9.738***	3.334	8.523**
		(1.624)		(2.583)		(3.580)
# UERJ alumni per 1000 workers in labor market	5.021	1.646**	5.333	0.718	3.817	4.639***
		(0.774)		(1.000)		(1.353)
# UERJ alumni / 1000 workers at firm (net of labor market)	1.540	3.505**	1.786	7.279**	−0.483	5.186*
		(1.390)		(2.989)		(2.867)
N (any alum)	2,029	25,324	2,698	34,127	396	5,060
Panel B. Employment 10–13 years after application (2SLS)						
Employed at firm with any UERJ alum	0.649	0.059*	0.573	0.028	0.498	0.080*
		(0.033)		(0.037)		(0.044)
Employed at firm with 10+ alumni per 1000 workers	0.201	0.065***	0.173	0.056*	0.116	0.000
		(0.024)		(0.032)		(0.030)
# UERJ alumni per 1000 workers at firm	5.873	3.648***	5.620	5.954**	3.224	−0.092
		(1.078)		(3.025)		(2.767)
# UERJ alumni per 1000 workers in labor market	4.421	0.433	4.515	0.316	2.960	2.382***
		(0.693)		(0.940)		(0.894)
# UERJ alumni / 1000 workers at firm (net of labor market)	1.452	3.179***	1.105	4.552	0.264	−0.821
		(1.159)		(2.819)		(1.601)
N (any alum)	2,242	25,535	2,039	28,217	277	4,179

Notes: This table presents RD estimates for the effects of UERJ enrollment on employment at alumni firms. Columns (A), (C), and (E) show means of each dependent variable for applicants in each group who scored $(-0.1, 0)$ SDs below the cutoff. Columns (B), (D), and (F) show 2SLS RD coefficients, β, from equation (2), which measure the effects of UERJ enrollment on employment outcomes. Panel A measures outcomes 6–9 years after application, and Panel B measures outcomes 10–13 years after application. In the first two rows of each panel, the dependent variables are indicators for employment at alumni firms in any year during this period. In the last three rows, the dependent variables are the mean number of alumni in the applicant's firms or labor markets during each period per 1000 workers. We define labor markets as a municipality $\times$ 5-digit industry code, and we compute the number of workers in each firm and labor market using its mean size over all years of our data. See Section 3.5 for details on the definitions of our alumni firm variables.

Parentheses contain standard errors clustered at the individual level. We use the Calonico et al. (2014) bandwidth for each outcome, so sample sizes differ across dependent variables.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5. DD estimates of the effects of AA exposure on student body composition

	(A)	(B)	(C)	(D)
	Pre-AA mean	DD coefficients		
Dependent variable	Top enrollees	Top enrollees	Other enrollees	All enrollees
Panel A. Exposure to affirmative action				
Prop. of classmates from AA tracks	0.000	0.189***	0.192***	0.191***
		(0.017)	(0.018)	(0.017)
Panel B. Demographic characteristics				
Age at application	21.921	0.191	0.666***	0.427*
		(0.312)	(0.229)	(0.216)
Female	0.501	0.032	0.038*	0.036*
		(0.022)	(0.021)	(0.019)
White	0.810	0.013	−0.121***	−0.060***
		(0.018)	(0.025)	(0.016)
Brown	0.156	0.000	0.043**	0.023*
		(0.012)	(0.017)	(0.012)
Black	0.025	−0.005	0.077***	0.041***
		(0.010)	(0.012)	(0.008)
Panel C. Admission exam scores (standardized in population of all enrollees)				
Field exam writing score	0.178	−0.045	−0.246***	−0.128***
		(0.043)	(0.046)	(0.046)
Mean field exam subject score	0.151	−0.029	−0.182**	−0.128*
		(0.064)	(0.084)	(0.074)
Admission score	0.270	−0.080	−0.498***	−0.274**
		(0.112)	(0.143)	(0.115)
Panel D. Predicted log wage based on characteristics and scores				
Predicted log wage	3.298	−0.023	−0.161***	−0.087***
		(0.029)	(0.043)	(0.032)
Predicted log wage (if in RAIS)	3.251	−0.033	−0.154***	−0.093***
		(0.028)	(0.043)	(0.032)
N (enrollees)	16,466	35,866	30,854	66,720

Notes: This table displays DD estimates of the effect of affirmative action exposure on student characteristics. Column (A) shows the mean of each dependent variable for top enrollees in the 1995–2001 cohorts. Columns (B)–(D) display estimates of π from equation (3) for top enrollees, other enrollees, and all enrollees. The dependent variables are:

- Panel A. The proportion of enrollees in an individual's program/cohort who were from an affirmative action track.
- Panel B. Demographic characteristics of enrollees. For the early cohorts of our data, we use gender/race measured in the RAIS data (see Appendix Table A1).
- Panel C. Applicants' field exam scores and their overall admission score. We normalize scores to be mean 0/SD 1 in the population of all UERJ enrollees in UERJ in a given cohort. The field score regressions include cohort dummies interacted with dummies for the set of subject tests that the applicant took (which vary by major).
- Panel D. The predicted value from a regression of log hourly wage (6–9 years after application) on each of the variables in Panels B–C.

Parentheses contain standard errors clustered at the program level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6. DD estimates for graduation, employment, and earnings 6–9 years after application

	(A)	(B)	(C)	(D)
	Pre-AA mean	DD coefficients		
Dependent variable	Top enrollees	Top enrollees	Other enrollees	All enrollees
Panel A. Graduation and formal employment				
Graduated from UERJ program	0.556	0.013	0.006	0.011
		(0.021)	(0.028)	(0.019)
Formal employment	0.734	-0.027^{*}	-0.012	-0.019
		(0.015)	(0.015)	(0.014)
Panel B. Earnings				
Log hourly wage	3.245	-0.132^{***}	-0.212^{***}	-0.178^{***}
		(0.045)	(0.062)	(0.049)
Monthly earnings (2019 USD)	1,380.558	-169.838^{***}	-272.989^{***}	-225.286^{***}
		(53.057)	(89.500)	(67.991)
Firm mean hourly wage (log)	3.316	-0.095^{**}	-0.183^{***}	-0.146^{***}
		(0.035)	(0.051)	(0.038)
Panel C. Employment at firms with pre- and post-AA alumni				
Pre-AA alumni	0.602	-0.055^{**}	-0.044	-0.054^{**}
		(0.023)	(0.033)	(0.026)
Only post-AA alumni	0.067	0.049^{**}	0.036	0.043^{**}
		(0.023)	(0.023)	(0.021)
Panel D. Alumni firm employment by application track and cohort				
General track alumni from same cohort	0.451	-0.098^{***}	-0.072^{**}	-0.088^{***}
		(0.021)	(0.028)	(0.021)
General track alumni from diff. cohort	0.233	0.042^{**}	0.004	0.021
		(0.016)	(0.017)	(0.012)
Only AA alumni from same cohort	0.000	0.036^{***}	0.051^{***}	0.044^{***}
		(0.009)	(0.007)	(0.007)
Only AA alumni from diff. cohort	0.012	0.010^{**}	0.014^{***}	0.013^{***}
		(0.005)	(0.004)	(0.004)
N (enrollees)	16,466	35,866	30,854	66,720
N (wage observations)	12,062	26,445	22,975	49,420

Notes: This table displays DD estimates of the effect of affirmative action exposure on graduation, earnings, and employment at alumni firms measured 6–9 years after application. Column (A) shows the mean of each dependent variable for top enrollees in the 1995–2001 cohorts. Columns (B)–(D) display estimates of π from equation (3) for top enrollees, other enrollees, and all enrollees. The dependent variables are defined similarly to those in Tables 2 and 4. In Panel C, we define alumni firms using graduates from the pre- and post-AA cohorts. In Panel D, we define alumni firms using the alum's cohort (same or different than the applicant's cohort) and application track (general or AA). We define the outcomes in Panels C–D to be non-overlapping, i.e., the variables in the lower rows of each panel equal one only if the firm did not hire alumni who meet the criteria for the higher rows. Parentheses contain standard errors clustered at the program level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7. DD estimates for UERJ's ENADE exam taker characteristics and scores

	(A)	(B)	(C)	(D)
	UERJ pre-AA mean	DD coefficients		
Dependent variable	All students	All students	White private HS students	Other students
Panel A. Characteristics of ENADE exam takers				
# exam takers per program × year	36.086	4.322	-7.926^{***}	9.142^{***}
		(3.368)	(0.934)	(2.835)
Log # exam takers	3.508	0.057	-0.430^{***}	0.319^{***}
		(0.072)	(0.072)	(0.071)
White	0.716	-0.132^{***}		-0.066^{***}
		(0.022)		(0.020)
Private HS student	0.570	-0.131^{***}		-0.040^{***}
		(0.024)		(0.011)
Female	0.526	−0.005	−0.009	−0.016
		(0.014)	(0.015)	(0.015)
Age	26.520	0.661^{*}	0.042	0.700^{**}
		(0.384)	(0.243)	(0.325)
Mother has a HS degree	0.692	-0.086^{***}	0.016	-0.081^{***}
		(0.021)	(0.011)	(0.022)
HH income > 3x min. wage	0.899	-0.031^{**}	0.038^{***}	-0.058^{**}
		(0.013)	(0.010)	(0.023)
Panel B. ENADE scores (proportion correct answers)				
Overall score	0.553	-0.038^{***}	-0.022^{**}	-0.051^{***}
		(0.007)	(0.009)	(0.015)
Field-specific component	0.519	-0.037^{***}	-0.021^{**}	-0.048^{**}
		(0.008)	(0.010)	(0.018)
General component	0.657	-0.041^{***}	-0.026^{**}	-0.059^{***}
		(0.006)	(0.012)	(0.011)
N (programs × years)	36	1,664	1,664	1,664
N (exam takers)	1,059	61,112	16,851	37,992

Notes: This table displays DD estimates of the effect of affirmative action on the characteristics (Panel A) and scores (Panel B) of UERJ's ENADE exam takers. The sample includes 2004–2015 ENADE exam takers from UERJ and other federal and state universities that did not implement affirmative action until 2012 or later. See Appendix Table A16 for details on our ENADE sample and the exam fields. Column (A) shows dependent variable means for UERJ exam takers in 2004–2006. The other columns show π coefficients from the DD regression:

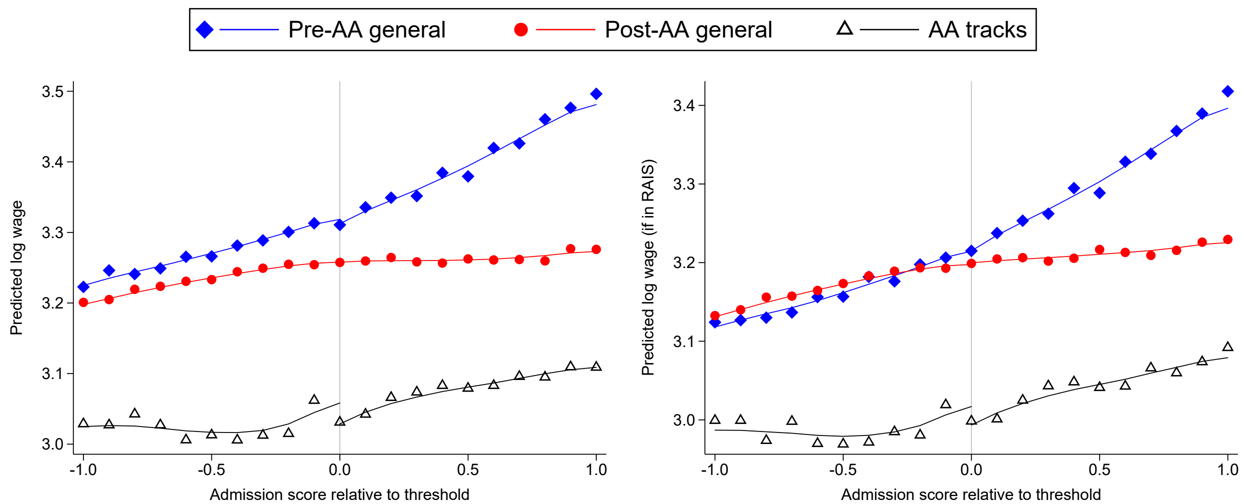
$$Y_{mjt} = \gamma_{mj} + \gamma_{mt} + \pi[\text{UERJ}_j \times \text{Post}_t] + \varepsilon_{mjt}.$$

Regressions are at the exam field (m) by institution (j) by year (t) level, with observations weighted by the number of exam takers. (In the first two rows of Panel A, we weight by the number of 2004–2006 exam takers in each mj cell.) We include field × institution dummies, γ_{mj}, field × year dummies, γ_{mt}, and an indicator for UERJ interacted with an indicator for the 2007–2015 cohorts, $\text{UERJ}_j \times \text{Post}_t$. Columns (B)–(D) include all students, white students from private high schools, and non-white and/or public high school students, respectively. Parentheses contain standard errors clustered at the institution level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix — For Online Publication

A. Appendix figures and tables

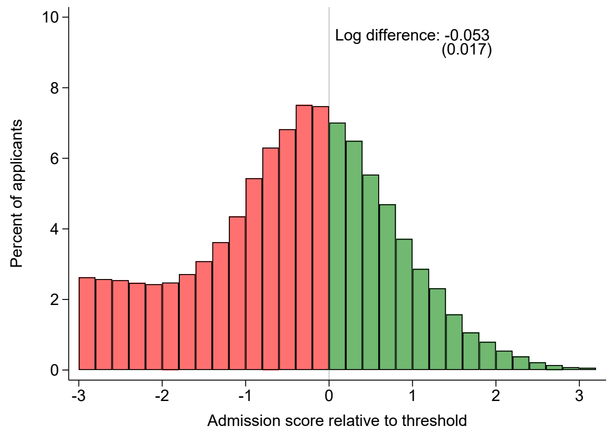


Panel A. All applicants

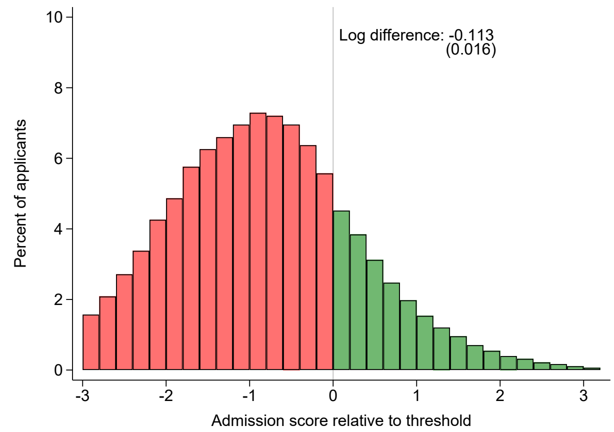
Panel B. Applicants who appear in the RAIS

Figure A1. Predicted log wage based on applicant characteristics

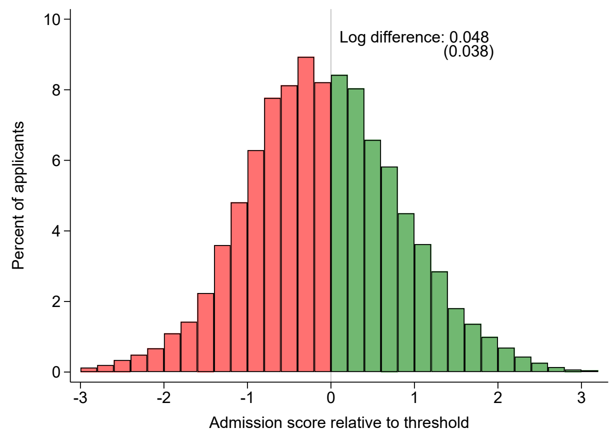
Notes: This figure presents RD graphs for pre-AA general applicants (blue diamonds), post-AA general applicants (red circles), and Black/public school applicants (black triangles). The x-axis in each panel is an applicant's standardized admission score normalized to zero at the cutoff. The dependent variable on the y-axis is the predicted value from a regression of log hourly wage (6–9 years after application) on student characteristics (age, gender, race, mother's education, family income, score on the writing component of the field exam, and qualifying exam score). Markers depict means in 0.1 SD bins of the standardized score. Lines are predicted values from local linear regressions estimated separately above and below the threshold with a triangular kernel.



Panel A. General track (1995–2001)



Panel B. General track (2004–2011)



Panel C. AA tracks (2004–2011)

FIGURE A2. Density of admission scores relative to the threshold

Notes: This figure shows the density of admission scores relative to the cutoff. The x-axis is a student's admission score normalized to zero at the cutoff of the relevant application pool. The y-axis shows the percent of applicants within 0.20 SD unit bins of the admission score. We restrict the figure to only display normalized scores within three SD of the cutoff. We also exclude applicants whose score defines the cutoff.

Panel A shows the distribution of admission scores for pre-AA general applicants, Panel B for post-AA general applicants, and Panel C for Black/public school applicants.

Each figure displays the estimated the log difference in height at the threshold using the McCrary (2008) density test. The standard error is shown in parentheses.

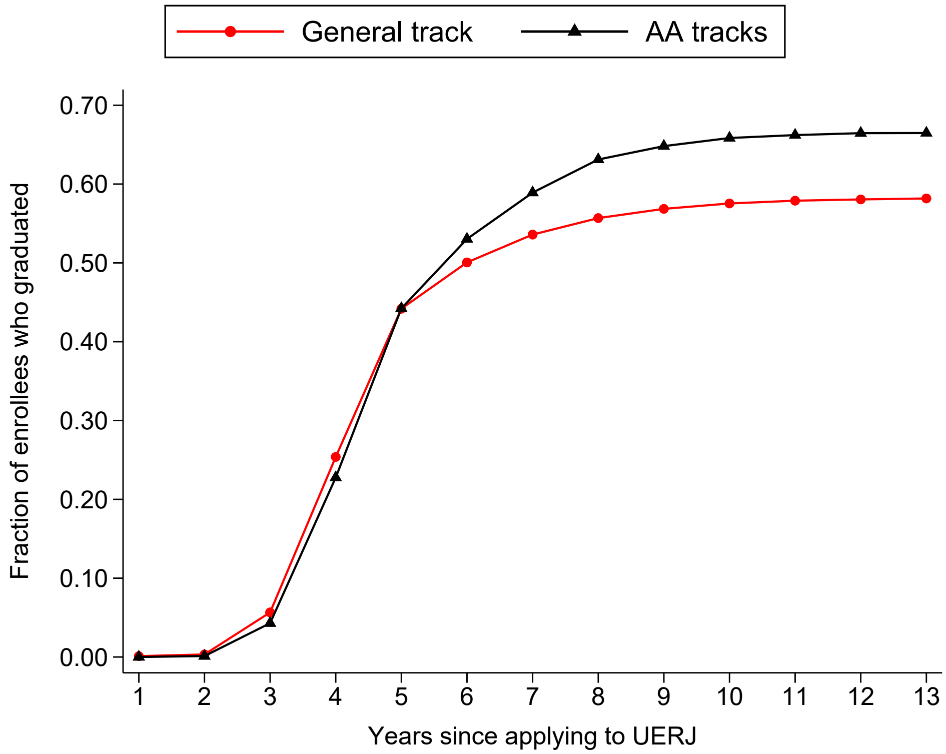
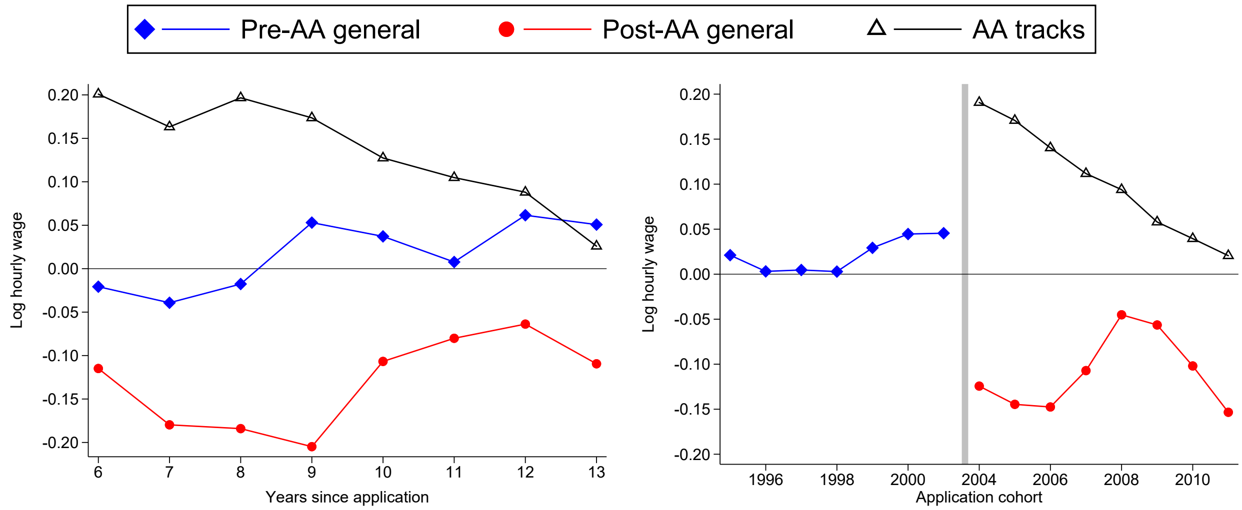


FIGURE A3. UERJ graduation rate by year since application

Notes: This figure show the empirical cumulative distribution function of the graduation rate of students in programs in our RD sample (Panel A of Table 1). We plot separately the graduation rate of general track enrollees (red line) and Black/public school enrollees (black line).



Panel A. By years since application
(1997–2006 cohorts)

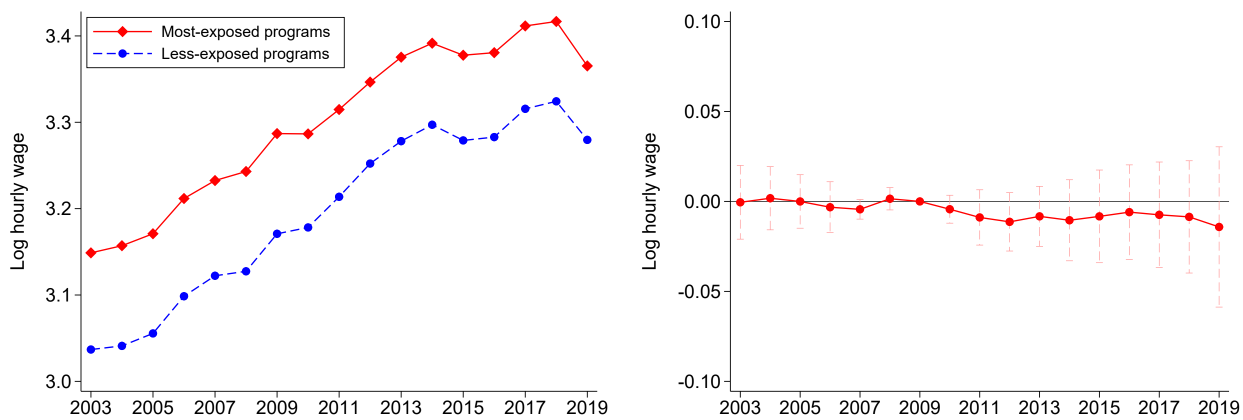
Panel B. By cohort
(Wages measured 6–9 years later)

FIGURE A4. RD estimates for log hourly wages by years since application and cohort

Notes: This figure displays 2SLS RD coefficients, β, for pre-AA general applicants (blue diamonds), post-AA general applicants (red circles), and Black/public school applicants (black triangles).

Panel A plots β coefficients for log hourly wages measured at different years since individuals applied to UERJ. To smooth estimates, we use the applicant's three-year average wage as the dependent variable in each regression (years $t-1$, t, and $t+1$). We include only 1997–2006 cohorts since we observe their outcomes in each of 6–13 years later.

Panel B plots β coefficients for log hourly wages estimated in different application cohorts. To smooth estimates, we include three adjacent cohorts for each regression (cohorts $t-1$, t, and $t+1$). All regressions use mean log hourly wage measured 6–9 years after application as the dependent variable.



Panel A. Industry mean wage in majors w/ more and less AA exposure

Panel B. Event study of industry mean wage

FIGURE A5. Wage trends in industries that employed pre-AA top enrollees

Notes: This figure shows how hourly wages trended over time in industries that employed top enrollees from the pre-AA cohorts. We begin by computing the mean hourly in each (5-digit) industry j and year t using all workers in the RAIS data; we denote this industry × year mean wage by $\bar{w}_{jt}$. We then take our sample of top UERJ enrollees in the pre-AA cohorts (1995–2001), and compute the share of individuals in each major m who were employed in industry j measured 6–9 years after UERJ application; we denote these shares s_{mj}, with $\sum_j s_{mj} = 1$ for each m. Lastly, we define $\bar{w}_{mt} = \sum_j s_{mj} * \bar{w}_{jt}$ as the industry mean wage for UERJ major m in year t, where this mean is computed using the pre-AA employment shares in each industry.

In Panel A, we plot the average value of $\bar{w}_{mt}$ in majors with more- (red line) and less- (blue line) exposure to affirmative action in each year from $t = 2003$ to 2019. More-exposed programs are those in which the share of affirmative action enrollees in the 2004–2011 cohorts was 30 percent or higher (Panel A of Table 1). Less-exposed programs are those in which the share of affirmative action enrollees in the 2004–2011 cohorts was below 30 percent (Panel B of Table 1).

Panel B plots π_t coefficients from the following event study regression:

$$\bar{w}_{mt} = \gamma_m + \gamma_{tf(m)} + \pi_t \text{ExposureToAA}_m + \varepsilon_{mt},$$

where γ_m are program fixed effects, $\gamma_{tf(m)}$ are year × field of study fixed effects, and π_t are coefficients on the interaction between year dummies (omitting 2009) and an indicator for more-exposed programs, ExposureToAA_m. Observations in this event study are weighted by the number of pre-AA top enrollees in each major m. Vertical dashed lines are 95 percent confidence intervals with standard errors clustered at the program level.

Table A1. RD balance tests

Dependent variable (cohorts observed)	(A)	(B)	(C)	(D)	(E)	(F)
	1995–2001 general track		2004–2011 general track		2004–2011 AA tracks	
	Mean below	RD coef	Mean below	RD coef	Mean below	RD coef
Panel A. Applicant characteristics						
Female (2004–2011 cohorts)			0.530	0.001 (0.008)	0.602	0.001 (0.021)
Female (measured in RAIS) (1995–2011 cohorts)	0.486	0.008 (0.010)	0.518	0.012 (0.009)	0.600	0.001 (0.023)
White (2007–2011 cohorts)			0.696	0.002 (0.010)	0.343	−0.022 (0.028)
White (measured in RAIS) (1995–2011 cohorts)	0.790	0.011 (0.009)	0.714	0.003 (0.008)	0.436	−0.027 (0.020)
Brown (2007–2011 cohorts)			0.212	−0.009 (0.009)	0.313	0.023 (0.030)
Brown (measured in RAIS) (1995–2011 cohorts)	0.173	−0.008 (0.010)	0.216	−0.010 (0.007)	0.321	0.028 (0.022)
Age at application (1995–2011 cohorts)	20.608	0.181** (0.081)	20.043	0.037 (0.072)	22.306	−0.446* (0.262)
Mother has HS degree (2007–2011 cohorts)			0.901	−0.002 (0.007)	0.534	0.001 (0.033)
HH income > 1.5x min. wage (2007–2011 cohorts)			0.886	−0.007 (0.007)	0.341	0.008 (0.026)
Writing score (SD units) (1995–2001, 2007–2011 cohorts)	0.174	0.020 (0.015)	0.477	0.011 (0.016)	−0.202	0.011 (0.052)
Qualifying exam score (SD units) (1995–2001 cohorts)	−0.148	−0.009 (0.008)				
Joint balance test (p value)		0.110		0.411		0.875
Panel B. Predicted log wage based on applicant characteristics						
Predicted log wage (1995–2011 cohorts)	3.313	−0.004 (0.006)	3.254	0.001 (0.003)	3.062	−0.003 (0.010)
Predicted log wage (if in RAIS) (1995–2011 cohorts)	3.206	0.004 (0.006)	3.193	0.003 (0.004)	3.019	−0.006 (0.011)
N	3,234	27,610	4,012	45,731	543	6,410
N (if in RAIS)	2,027	17,027	2,694	30,315	394	4,303

Notes: This table presents RD balance tests. Columns (A), (C), and (E) show means of each dependent variable for applicants in each group who scored $(-0.1, 0)$ SDs below the cutoff. Columns (B), (D), and (F) display reduced-form RD coefficients, θ, from equation (1), using the dependent variable listed in the row header.

The last row in Panel A reports the p values from F tests that the coefficients on all covariates are jointly equal to zero. Parentheses contain standard errors clustered at the individual level. We use the Calonico et al. (2014) bandwidth for each outcome, so sample sizes differ across dependent variables.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2. Effects of UERJ enrollment on job outcomes measured 10–13 years later

	(A)	(B)	(C)	(D)	(E)	(F)
	RD coefficient measured 10–13 years later			*Change* in RD coefficients from 6–9 to 10–13 years later		
Dependent variable	1995–01 general	2004–11 general	AA tracks	1995–01 general	2004–11 general	AA tracks
Panel A. Formal employment and earnings (2SLS)						
Formal employment	0.032	−0.026	0.037	−0.032	0.005	0.038
	(0.027)	(0.031)	(0.039)	(0.021)	(0.028)	(0.036)
Log hourly wage	0.005	0.005	0.024	0.008	0.084	-0.108^{**}
	(0.054)	(0.058)	(0.063)	(0.046)	(0.052)	(0.055)
Monthly earnings (2019 USD)	−84.946	−99.418	56.577	−85.241	54.056	−53.652
	(94.587)	(109.084)	(75.202)	(71.026)	(87.121)	(60.324)
N (log hourly wage)	24,273	26,407	3,746	48,837	59,379	9,846
Panel B. Employment at firms that hired UERJ alumni (2SLS)						
Employed at firm with any UERJ alum	0.059^{*}	0.028	0.080^{*}	-0.059^{*}	−0.042	−0.057
	(0.033)	(0.037)	(0.044)	(0.032)	(0.036)	(0.042)
Employed at firm with 10+ alumni / 1000 workers	0.065^{***}	0.056^{*}	0.000	−0.027	−0.046	-0.062^{**}
	(0.024)	(0.032)	(0.030)	(0.024)	(0.032)	(0.031)
# UERJ alumni / 1000 workers at firm	3.648^{***}	5.954^{**}	−0.092	−1.004	−3.784	-8.615^{**}
	(1.078)	(3.025)	(2.767)	(1.497)	(2.742)	(3.514)
# UERJ alumni / 1000 workers in labor market	0.433	0.316	2.382^{***}	-1.213^{*}	−0.402	-2.257^{*}
	(0.693)	(0.940)	(0.894)	(0.712)	(0.877)	(1.289)
# UERJ alumni / 1000 workers at firm (demeaned)	3.179^{***}	4.552	−0.821	−0.326	−2.727	-6.007^{*}
	(1.159)	(2.819)	(1.601)	(1.434)	(2.782)	(3.105)
N (any alum)	25,535	28,217	4,179	50,859	62,344	9,239

Notes: This table presents RD estimates for employment and earnings measured 10–13 years after application. Columns (A)–(C) show 2SLS RD coefficients, β, from equation (2) for each applicant group. Columns (D)–(F) show the difference in the 2SLS RD coefficients between the periods of 6–9 and 10–13 years after application. Parentheses contain standard errors clustered at the individual level. We use the Calonico et al. (2014) bandwidth for each outcome, so sample sizes differ across dependent variables.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3. Robustness of RD estimates — General track (1995–2001)

	(A)	(B)	(C)	(D)	(E)
	RD coefficients by bandwidth, h^Y				
	1.0× CCT	0.5× CCT	1.5× CCT	Donut RD	Adding controls
Panel A. First stage					
Enrolled in UERJ program	0.313***	0.354***	0.311***	0.253***	0.310***
	(0.010)	(0.014)	(0.008)	(0.021)	(0.010)
N	17,519	8,880	25,848	9,101	17,519
Panel B. Returns to UERJ enrollment 6–9 years later (2SLS)					
Graduated from UERJ program	0.711***	0.708***	0.719***	0.714***	0.713***
	(0.017)	(0.022)	(0.015)	(0.037)	(0.017)
Formal employment	0.064**	0.104**	0.072***	0.059	0.016
	(0.029)	(0.042)	(0.022)	(0.043)	(0.019)
Log hourly wage	−0.004	0.012	0.027	−0.004	−0.006
	(0.050)	(0.074)	(0.038)	(0.055)	(0.049)
Monthly earnings (2019 USD)	0.440	103.639	66.956	−30.020	−2.036
	(75.269)	(114.267)	(57.390)	(92.678)	(74.054)
N (employment regression)	37,794	20,162	51,674	29,030	37,794
N (wage regression)	24,567	13,140	33,612	24,481	24,567
Panel C. Returns to UERJ enrollment 10–13 years later (2SLS)					
Graduated from UERJ program	0.718***	0.729***	0.729***	0.709***	0.720***
	(0.017)	(0.022)	(0.015)	(0.038)	(0.017)
Formal employment	0.032	0.033	0.042**	−0.003	−0.008
	(0.027)	(0.039)	(0.021)	(0.039)	(0.014)
Log hourly wage	0.005	−0.037	0.018	0.006	0.004
	(0.054)	(0.077)	(0.041)	(0.060)	(0.054)
Monthly earnings (2019 USD)	−84.105	−83.191	30.639	−115.173	−85.290
	(94.586)	(145.009)	(73.007)	(135.201)	(94.117)
N (employment regression)	39,133	21,003	53,108	31,666	39,133
N (wage regression)	24,273	12,851	33,695	24,847	24,273

Notes: This table display RD coefficients using different specifications of our estimating equation. The coefficients are estimated on the sample of general track applicants in the pre-AA cohorts (1995–2001).

Columns (A)–(C) display the estimated RD coefficients using different sample bandwidths. Column (A) reproduces our baseline specification, which uses the Calonico et al. (2014) (CCT) optimal bandwidth for each outcome. In Column (B), we use a bandwidth half the size of the optimal CCT bandwidth. In Column (C), we use a bandwidth twice as large as the CCT bandwidth. In Column (D), we exclude applicants with an admission score within 0.05 SD of the cutoff. In Column (E), we include controls for age, gender, race, mother's educational attainment, family income, score on the writing component of the field exam, and qualifying exam score.

Panel A displays the first-stage effect, which the estimated θ from equation (1). Panels B–C display 2SLS RD coefficients, β, from equation (2). Parentheses contain standard errors clustered at the individual level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4. Robustness of RD estimates — General track (2004–2011)

	(A)	(B)	(C)	(D)	(E)
	RD coefficients by bandwidth, h^Y				
	1.0× CCT	0.5× CCT	1.5× CCT	Donut RD	Adding controls
Panel A. First stage					
Enrolled in UERJ program	0.292***	0.288***	0.295***	0.296***	0.292***
	(0.006)	(0.008)	(0.005)	(0.008)	(0.006)
N	47,838	24,477	69,856	36,957	47,838
Panel B. Returns to UERJ enrollment 6–9 years later (2SLS)					
Graduated from UERJ program	0.677***	0.647***	0.684***	0.715***	0.677***
	(0.013)	(0.017)	(0.011)	(0.016)	(0.013)
Formal employment	−0.030	−0.029	−0.025	−0.061*	0.012
	(0.027)	(0.038)	(0.022)	(0.033)	(0.016)
Log hourly wage	−0.080	−0.123*	−0.038	−0.046	−0.081*
	(0.049)	(0.069)	(0.041)	(0.057)	(0.049)
Monthly earnings (2019 USD)	−163.811**	−223.150**	−75.055	−106.698	−157.841**
	(77.383)	(105.344)	(63.181)	(97.113)	(76.054)
N (employment regression)	55,110	28,308	80,093	45,622	55,110
N (wage regression)	32,966	16,930	47,911	31,844	32,966
Panel C. Returns to UERJ enrollment 10–13 years later (2SLS)					
Graduated from UERJ program	0.693***	0.658***	0.709***	0.736***	0.694***
	(0.014)	(0.019)	(0.012)	(0.019)	(0.014)
Formal employment	−0.026	0.008	−0.007	−0.035	−0.008
	(0.031)	(0.043)	(0.026)	(0.037)	(0.021)
Log hourly wage	0.011	−0.072	−0.022	0.049	0.010
	(0.057)	(0.081)	(0.048)	(0.068)	(0.057)
Monthly earnings (2019 USD)	−102.941	−231.406	−53.098	−33.774	−100.881
	(109.073)	(152.445)	(89.843)	(135.372)	(108.029)
N (employment regression)	41,128	21,285	59,285	34,320	41,128
N (wage regression)	26,540	13,748	38,220	23,707	26,540

Notes: This table display RD coefficients using different specifications of our estimating equation. The table is structured similarly to Table A3, but the coefficients are estimated on the sample of general track applicants in the post-AA cohorts (2004–2011). See notes to Table A3 for details.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5. Robustness of RD estimates — Affirmative action tracks

	(A)	(B)	(C)	(D)	(E)
	RD coefficients by bandwidth, h^Y				
	1.0× CCT	0.5× CCT	1.5× CCT	Donut RD	Adding controls
Panel A. First stage					
Enrolled in UERJ program	0.689***	0.708***	0.713***	0.626***	0.689***
	(0.014)	(0.019)	(0.012)	(0.030)	(0.014)
N	6,121	3,262	8,606	3,308	6,121
Panel B. Returns to UERJ enrollment 6–9 years later (2SLS)					
Graduated from UERJ program	0.640***	0.642***	0.643***	0.660***	0.638***
	(0.018)	(0.023)	(0.015)	(0.034)	(0.018)
Formal employment	−0.002	−0.008	−0.013	0.022	−0.010
	(0.026)	(0.037)	(0.021)	(0.038)	(0.017)
Log hourly wage	0.132***	0.123**	0.130***	0.161***	0.125***
	(0.044)	(0.062)	(0.036)	(0.058)	(0.043)
Monthly earnings (2019 USD)	110.230**	114.289*	112.040***	202.213***	108.147**
	(49.523)	(66.820)	(40.360)	(76.211)	(48.984)
N (employment regression)	8,147	4,459	11,011	6,276	8,147
N (wage regression)	6,100	3,311	8,203	5,405	6,100
Panel C. Returns to UERJ enrollment 10–13 years later (2SLS)					
Graduated from UERJ program	0.661***	0.654***	0.670***	0.653***	0.660***
	(0.021)	(0.028)	(0.018)	(0.041)	(0.021)
Formal employment	0.037	0.060	0.011	0.057	0.025
	(0.039)	(0.055)	(0.031)	(0.054)	(0.031)
Log hourly wage	0.025	0.101	0.052	0.019	0.014
	(0.063)	(0.087)	(0.050)	(0.080)	(0.062)
Monthly earnings (2019 USD)	56.577	69.183	66.656	120.434	45.488
	(75.149)	(102.962)	(59.072)	(104.238)	(74.087)
N (employment regression)	4,320	2,280	6,109	3,958	4,320
N (wage regression)	3,748	2,024	5,240	3,693	3,748

Notes: This table display RD coefficients using different specifications of our estimating equation. The table is structured similarly to Table A3, but the coefficients are estimated on the sample of Black/public school applicants in the post-AA cohorts (2004–2011). See notes to Table A3 for details.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6. RD estimates for alumni firm employment by alum's cohort/track

	(A)	(B)	(C)	(D)	(E)	(F)
	1995–2001 general track		2004–2011 general track		2004–2011 AA tracks	
Dependent variable	Mean below	RD coef	Mean below	RD coef	Mean below	RD coef
Panel A. Employment at firms that hired any UERJ alumni (2SLS)						
Any pre-AA general track alum	0.502	0.091***	0.308	0.035	0.278	0.075**
		(0.034)		(0.032)		(0.038)
Any post-AA general track alum	0.396	0.060*	0.430	0.079**	0.343	0.126***
		(0.031)		(0.035)		(0.037)
Any AA track alum	0.367	0.040	0.398	0.072**	0.376	0.120***
		(0.030)		(0.033)		(0.036)
N (AA track)	2,029	28,066	2,698	36,404	396	5,372
Panel B. Number of UERJ alumni per 1000 workers at firm (2SLS)						
# pre-AA general track alumni	3.216	3.366***	1.676	0.363	0.587	1.242***
		(0.999)		(0.559)		(0.442)
# post-AA general track alumni	1.537	0.354	2.548	5.800***	1.311	2.793
		(0.715)		(2.210)		(2.019)
# AA track alumni	0.961	0.362	1.705	2.017**	0.905	2.039*
		(0.294)		(0.988)		(1.074)
N (AA track)	2,029	32,665	2,698	38,670	396	5,242

Notes: This table presents RD estimates for employment outcomes measured 6–9 years after application. Columns (A), (C), and (E) show means of each dependent variable for applicants in each group who scored $(-0.1, 0)$ SDs below the cutoff. Columns (B), (D), and (F) show 2SLS RD coefficients, β, from equation (2). The dependent variables are indicators for employment at firms that hired any UERJ alum (Panel A) and the number of UERJ alumni at the applicant's firm per 1000 workers (Panel B). These variables are similar to those in Table 4, but we define outcomes separately using pre-AA alumni, post-AA general track alumni, and affirmative action alumni. Parentheses contain standard errors clustered at the individual level. We use the Calonico et al. (2014) bandwidth for each outcome, so sample sizes differ across dependent variables.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7. Heterogeneity in RD estimates by field of study — Affirmative action tracks

	(A)	(B)	(C)	(D)	(E)
	Field of study				
Dependent variable	Business	Health	Human -ities	Natural sciences	Social sciences
Panel A. Returns to UERJ enrollment 6–9 years after applying (2SLS)					
Graduated from UERJ program	0.697***	0.801***	0.528***	0.411***	0.722***
	(0.059)	(0.029)	(0.045)	(0.038)	(0.033)
Formal employment	0.071	−0.055	0.075	0.037	−0.053
	(0.077)	(0.053)	(0.058)	(0.055)	(0.051)
Log hourly wage	0.224*	0.204**	0.079	0.090	0.095
	(0.131)	(0.087)	(0.103)	(0.094)	(0.091)
Monthly earnings (2019 USD)	270.006**	217.408*	97.492	50.829	3.713
	(128.029)	(116.354)	(87.611)	(113.661)	(96.297)
N (formal employment)	784	1,895	1,295	1,681	2,492
N (log hourly wage)	698	1,382	1,009	1,269	1,719
Panel B. Returns to UERJ enrollment 10–13 years after applying (2SLS)					
Graduated from UERJ program	0.724***	0.803***	0.556***	0.482***	0.715***
	(0.072)	(0.036)	(0.053)	(0.048)	(0.041)
Formal employment	0.080	0.018	−0.024	0.105	0.026
	(0.146)	(0.075)	(0.093)	(0.083)	(0.077)
Log hourly wage	0.240	0.172	0.025	−0.055	−0.141
	(0.220)	(0.127)	(0.145)	(0.133)	(0.122)
Monthly earnings (2019 USD)	327.743	243.571	17.347	−153.081	−45.814
	(239.378)	(159.063)	(157.375)	(163.135)	(151.932)
N (formal employment)	423	1,022	709	830	1,336
N (log hourly wage)	403	903	624	727	1,089

Notes: This table displays RD coefficients estimated on the sample of Black/public school applicants. Each column shows the result for applicants to different fields of study. Column (A) shows the results for applicants to business programs; column (B) for health programs; column (C) for humanities programs; column (D) for natural sciences programs, and column (E) for social sciences programs. See Table 1, Panel A, for the programs included in each field of study and Appendix Tables B2–B4 for the number of applicants by program/cohort.

Panels A–B display 2SLS RD coefficients, β, from equation (2). The dependent variables are program completion, formal employment, and earnings, each measured 6–9 years after applying (Panel A) and 10–13 years after applying (Panel B). See Appendix B.1 for variable definitions.

Parentheses contain standard errors clustered at the individual level. We use the Calonico et al. (2014) bandwidth for each outcome, so sample sizes differ across dependent variables.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8. Effects of UERJ enrollment on firm, occupation, industry, and municipality mean wages

	(A)	(B)	(C)	(D)	(E)	(F)
	1995–2001 general track		2004–2011 general track		2004–2011 AA tracks	
Dependent variable	Mean below	RD coef	Mean below	RD coef	Mean below	RD coef
Panel A. Outcomes 6–9 years after application (2SLS)						
Firm mean wage (log)	3.303	0.018	3.475	-0.095^{*}	3.073	0.106^{*}
		(0.043)		(0.053)		(0.062)
Occupation mean wage (log)	3.271	0.028	3.388	−0.062	3.017	0.053
		(0.034)		(0.039)		(0.041)
Industry mean wage (log)	3.219	0.009	3.201	−0.024	3.000	0.044
		(0.031)		(0.037)		(0.036)
Municipality mean wage (log)	3.186	0.009	3.175	−0.013	3.137	−0.005
		(0.017)		(0.018)		(0.020)
N (firm mean wage)	2,024	30,345	2,681	31,087	394	4,306
Panel B. Outcomes 10–13 years after application (2SLS)						
Firm mean wage (log)	3.572	0.093^{*}	3.581	−0.053	3.223	0.049
		(0.053)		(0.062)		(0.071)
Occupation mean wage (log)	3.428	−0.009	3.509	0.025	3.157	−0.071
		(0.033)		(0.041)		(0.053)
Industry mean wage (log)	3.314	0.039	3.271	0.029	3.094	0.018
		(0.034)		(0.040)		(0.044)
Municipality mean wage (log)	3.209	−0.025	3.206	−0.027	3.131	0.017
		(0.021)		(0.020)		(0.023)
N (firm mean wage)	2,236	24,701	2,010	21,071	275	3,133

Notes: This table presents RD estimates for the effects of UERJ enrollment on mean wages at the firm, occupation, industry, and municipality levels. Columns (A), (C), and (E) show means of each dependent variable for applicants in each group who scored $(-0.1, 0)$ SDs below the cutoff. Columns (B), (D), and (F) show 2SLS RD coefficients, β, from equation (2), which measure the effects of UERJ enrollment on mean hourly wages associated with four different characteristics of individuals' jobs: 1) firm; 2) occupation (using 4-digit CBO codes); 3) industry (using 4-digit CNAE codes); and 4) municipality. Panel A measures outcomes 6–9 years after application, and Panel B measures outcomes 10–13 years after application. Parentheses contain standard errors clustered at the individual level. We use the Calonico et al. (2014) bandwidth for each outcome, so sample sizes differ across dependent variables.

Table A9. Summary statistics for Rio de Janeiro universities in 2010

(A)	(B)	(C)	(D)	(E)	(F)	(G)	(H)	(I)
						in 2010 US dollars		
University name	Abbr.	Ownership	*Folha* national ranking	Undergrad enrollment	Graduate enrollment	Annual revenue (millions)	Annual expenses (millions)	Expenses per student
Univ. Federal do Rio de Janeiro	UFRJ	Federal	3	50,342	12,453	1,254.4	1,254.4	19,976
Univ. do Estado do Rio de Janeiro	**UERJ**	**State**	**11**	**30,144**	**5,767**	**463.9**	**465.5**	**12,962**
Pont. Univ. Católica do Rio de Janeiro	PUC-Rio	Private	13	17,061	3,352	291.3	265.8	13,022
Univ. Federal Fluminense	UFF	Federal	15	48,809	5,720	767.9	1,268.3	23,259
Univ. Federal Rural do Rio de Janeiro	UFRRJ	Federal	48	14,826	2,116	308.8	250.0	14,759
Univ. Federal do Estado do Rio de Janeiro	UNIRIO	Federal	67	14,418	934	140.5	109.2	7,112
Univ. Estácio de Sá	UNESA	Private	89	181,832	492	341.0	237.6	1,303
Univ. Gama Filho	UGF	Private	110	21,020	243	94.8	97.1	4,568
Univ. Veiga de Almeida	UVA	Private	147	21,983	184	62.4	55.7	2,512
Univ. Salgado de Oliveira	Universo	Private	155	48,130	173	246.5	114.8	2,377
Univ. Castelo Branco	UCB	Private	160	71,524	0	33.8	33.1	463
Univ. Cândido Mendes	UCAM	Private	168	21,454	458	69.8	83.9	3,827

Notes: This table displays summary statistics for universities in Rio de Janeiro. The sample includes private universities in the municipality of Rio, federal universities in the state of Rio, and UERJ. These are the universities we use to define enrollment outcomes in Table 3.

Columns (A)–(C) show the university's name, abbreviation, and ownership type. Column (D) reports the university's rank in the 2012 national ranking by the newspaper *Folha*. Column (E) shows the number of undergraduate students enrolled in each institution in 2010, which we compute from the individual-level dataset of Brazil's higher education census (*Censo da Educação Superior*). Column (F) shows the number of graduate students at each institution in 2010, which we compute from the CAPES census of graduate programs (*Discentes dos Programas de Pós-Graduação stricto sensu no Brasil*). Columns (G)–(H) report annual revenue and expenses in 2010 (converted to US dollars) from the school-level dataset of the *Censo da Educação Superior*. Column (I) shows annual expenses (column H) divided by total enrollment (columns E + F).

Table A10. Top employers of UERJ alumni

#	Firm	(A) No. UERJ graduates hired	(B) No. alumni hired per 1000 workers	(C) Firm size (mean)	(D) Located in Rio	(E) Public firm	(F) Prop. of employees w/ college	(G) Firm mean hourly wage (2019 USD)
Panel A. Top 10 firms by total number of UERJ alumni employees								
1	City Hall of Rio de Janeiro	1,161	13.30	87,274	Yes	Yes	0.461	6.891
2	State Secretary of Education	1,093	11.97	91,309	Yes	Yes	0.398	3.959
3	State University of Rio de Janeiro (UERJ)	409	56.29	7,266	Yes	Yes	0.690	13.062
4	Brazilian Petroleum (Petrobras - HQ)	384	62.91	6,104	Yes	No	0.780	27.690
5	State Secretary of Health	377	15.35	24,563	Yes	Yes	0.330	3.132
6	State Court of Law	321	21.27	15,093	Yes	Yes	0.718	16.466
7	Center for Payment of the Army	307	1.91	161,115	No	Yes	0.172	6.048
8	Federal University of Rio de Janeiro (UFRJ)	238	22.97	10,362	Yes	Yes	0.715	13.051
9	State Public Ministry	227	71.22	3,187	Yes	Yes	0.740	25.150
10	City Hall of Duque de Caxias	221	17.83	12,395	Yes	Yes	0.874	6.914
–	All other firms	–	–	510	0.777	0.084	0.384	6.971
Panel B. Top 10 firms by number of UERJ alumni hired per 1000 workers								
1	National Bank of Econ. & Social Dev.	217	109.23	1,987	Yes	No	0.875	38.010
2	Accenture	184	107.13	1,718	Yes	No	0.810	13.268
3	Petrobras - EDIHB	176	102.91	1,710	Yes	No	0.841	24.554
4	General Public Defender of the State	142	80.84	1,757	Yes	Yes	0.536	24.971
5	Petrobras - Research Center	137	72.20	1,898	Yes	No	0.693	22.740
6	State Public Ministry	227	71.22	3,187	Yes	Yes	0.740	25.150
7	Petrobras - Vibra Energy	86	68.34	1,258	Yes	No	0.781	20.840
8	TIM Cellular	112	67.67	1,655	Yes	No	0.813	13.542
9	Pedro II Federal Public School	139	63.35	2,194	Yes	Yes	0.828	10.560
10	Petrobras - EDISE	384	62.91	6,104	Yes	No	0.780	27.690
–	All other firms	–	–	537	0.777	0.085	0.384	6.964

Notes: This table displays summary statistics for top employers of UERJ alumni from the programs in our RD sample (Panel A of Table 1). Panel A lists the top ten firms ranked according to column (A), which is the number of UERJ graduates hired across all cohorts in our data. Panel B lists the top ten firms ranked according to column (B), which is the number of UERJ graduates (column A) divided by the firm size (column C) and multiplied by 1000. Column (C) shows the average firm size (number of employees). Column (D) indicates whether the firm is located in the state of Rio. Column (E) indicates whether the firm is public. Column (F) shows the proportion of the firm's employees with a college degree (from any school). Column (G) shows the firm mean hourly wage, measured in 2019 USD. The last row of each Panel shows the average of all other firms that hired at least one UERJ graduate in our sample.

Table A11. OLS regressions on alumni firm variables

Covariate	(A)	(B)	(C)	(D)
	Dependent variable: Log firm mean hourly wage			
Any alumni firm	0.444			
	(0.004)			
Firm w/ 50+ alumni per 1000 workers		0.398		
		(0.011)		
Firm w/ 25−50 alumni per 1000 workers		0.589		
		(0.010)		
Firm w/ 10−25 alumni per 1000 workers		0.639		
		(0.006)		
Firm w/ 5−10 alumni per 1000 workers		0.488		
		(0.007)		
Firm w/ 1−5 alumni per 1000 workers		0.439		
		(0.005)		
Firm w/ 0−1 alumni per 1000 workers		0.303		
		(0.005)		
Pre-AA alumni			0.481	
			(0.004)	
Only post-AA alumni			0.274	
			(0.005)	
General track alumni from same cohort				0.533
				(0.005)
General track alumni from diff. cohort				0.455
				(0.004)
Only AA alumni from same cohort				0.293
				(0.008)
Only AA alumni from diff. cohort				0.070
				(0.010)
Admission score	0.170	0.164	0.173	0.164
	(0.002)	(0.002)	(0.002)	(0.002)
N	549,675	549,675	549,675	549,675

Notes: This table shows OLS estimates of the wage premia associated with employment at UERJ alumni firms. The sample includes all UERJ applicants. Regressions are at the applicant × year level and include observations 6–9 years after UERJ application. The dependent variable is log firm mean hourly wage. We use four types of our alumni firm variables as covariates:

- Column (A): Any alumni firm;
- Column (B): Firms categorized by the number of UERJ alumni they hired relative to their mean size;
- Column (C): Firms that hired any alumni from the pre-AA cohorts (1995–2001) vs. firms that hired alumni *only* from the post-AA cohorts (2004–2011);
- Column (D): Firms defined by the alum's cohort (same or different than the applicant's cohort) and application track (general or AA). We define these outcomes to be non-overlapping, i.e., the variables in the lower rows equal one only if the firm did not hire alumni who meet the criteria for the higher rows.

All regressions control for the applicant's standardized admission score and application pool × calendar year dummies. Parentheses contain standard errors clustered at the individual level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A12. Summary statistics for top enrollee sample

	(A)	(B)	(C)	(D)
	1995–2001 cohorts (Pre-AA)		2004–2011 cohorts (Post-AA)	
	High AA exposure programs	Low AA exposure programs	High AA exposure programs	Low AA exposure programs
Panel A. Demographic characteristics and admission scores				
Female	0.458	0.541	0.484	0.527
Age at application	20.974	22.802	20.595	22.154
White	0.832	0.790	0.709	0.658
Black	0.021	0.028	0.059	0.084
Admission score	0.861	−0.279	1.026	0.094
N	7,915	8,513	9,464	9,930
Panel B. Proportion employed in top 10 industries				
Public administration	0.351	0.386	0.270	0.318
Education	0.082	0.166	0.098	0.198
Services	0.104	0.079	0.133	0.104
Finance	0.072	0.058	0.045	0.042
Health and social services	0.050	0.015	0.067	0.017
Computer activities	0.036	0.023	0.045	0.026
Retail trade	0.024	0.030	0.029	0.035
Telecommunications	0.034	0.020	0.019	0.017
Professional associations	0.023	0.025	0.031	0.023
Extractive industries	0.035	0.020	0.019	0.012
Total in top 10 industries	0.812	0.822	0.756	0.790
N	5,665	6,414	6,660	7,762

Notes: This table reports summary statistics for UERJ top enrollees in our DD sample. Top enrollees are those students whose entrance exam scores were high enough to gain admission to UERJ regardless of whether affirmative action existed in their cohort. Columns (A)–(B) include top enrollees in pre-AA cohorts (1995–2001), and columns (C)–(D) include top enrollees in post-AA cohorts (2004–2011). Columns (A) and (C) include programs in which 30 percent or more of the students in the post-AA cohorts were from an AA track (Panel A of Table 1). Columns (B) and (D) include programs in which less than 30 percent the students in the post-AA cohorts were from an AA track (Panel B of Table 1). Panel A reports the demographic characteristics and admission scores of top enrollees. Panel B reports the fraction of top enrollees employed in the top most common industries, where employment is measured 6–9 years after UERJ application. See Appendices B.1 and B.4 for details on variable definitions and the sample.

Table A13. DD estimates for graduation, employment, and earnings 10–13 years after application

	(A)	(B)	(C)	(D)
	Pre-AA mean	DD coefficients		
Dependent variable	Top enrollees	Top enrollees	Other enrollees	All enrollees
Panel A. Graduation and formal employment				
Graduated from UERJ program	0.568	0.011	0.010	0.010
		(0.018)	(0.028)	(0.018)
Formal employment	0.768	−0.013	−0.010	−0.013
		(0.011)	(0.013)	(0.010)
Panel B. Earnings				
Log hourly wage	3.600	−0.115**	−0.252***	−0.180***
		(0.053)	(0.072)	(0.054)
Monthly earnings (2019 USD)	2,005.914	−224.443**	−469.037***	−337.439***
		(90.068)	(133.630)	(100.875)
Firm mean hourly wage (log)	3.565	−0.114**	−0.191***	−0.153***
		(0.044)	(0.055)	(0.041)
Panel C. Employment at firms with pre- and post-AA alumni				
Pre-AA alumni	0.595	−0.043	−0.023	−0.038
		(0.027)	(0.034)	(0.028)
Only post-AA alumni	0.089	0.034**	0.034	0.034**
		(0.016)	(0.021)	(0.016)
Panel D. Alumni firm employment by application track and cohort				
General track alumni from same cohort	0.463	−0.093***	−0.046	−0.075***
		(0.026)	(0.030)	(0.024)
General track alumni from diff. cohort	0.248	0.046**	0.023	0.033**
		(0.018)	(0.017)	(0.013)
Only AA alumni from same cohort	0.000	0.031***	0.036***	0.033***
		(0.006)	(0.009)	(0.006)
Only AA alumni from diff. cohort	0.012	0.005	0.003	0.005*
		(0.004)	(0.004)	(0.003)
N (enrollees)	16,466	31,016	26,484	57,500
N (wage observations)	12,614	23,381	20,091	43,472

Notes: This table displays DD estimates of the effect of affirmative action exposure on graduation, earnings, and employment at alumni firms measured 10–13 years after application. Column (A) shows the mean of each dependent variable for top enrollees in the 1995–2001 cohorts. Columns (B)–(D) display estimates of π from equation (3) for top enrollees, other enrollees, and all enrollees. The dependent variables are defined similarly to those in Tables 2 and 4. In Panel C, we define alumni firms using graduates from the pre- and post-AA cohorts. In Panel D, we define alumni firms using the alum's cohort (same or different than the applicant's cohort) and application track (general or AA). We define the outcomes in Panels C–D to be non-overlapping, i.e., the variables in the lower rows of each panel equal one only if the firm did not hire alumni who meet the criteria for the higher rows. Parentheses contain standard errors clustered at the program level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A14. DD estimates by field of study — Top enrollees

Dependent variable	(A) Business	(B) Human -ities	(C) Natural sciences	(D) Social sciences
Panel A. Graduation and formal employment				
Graduated from UERJ program	−0.011	0.046	−0.020	0.052
	(0.043)	(0.034)	(0.039)	(0.025)
Formal employment	0.022	−0.021	−0.029	−0.030
	(0.032)	(0.029)	(0.018)	(0.026)
Panel B. Earnings				
Log hourly wage	−0.035	-0.212^{**}	−0.082	-0.181^{*}
	(0.040)	(0.094)	(0.071)	(0.070)
Monthly earnings (2019 USD)	−118.732	-211.522^{***}	−86.695	−301.923
	(91.622)	(60.937)	(77.045)	(185.385)
Firm mean hourly wage (log)	-0.100^{**}	-0.137^{**}	−0.066	−0.079
	(0.018)	(0.055)	(0.072)	(0.058)
Panel C. Employment at firms with pre- and post-AA alumni				
Pre-AA alumni	−0.072	−0.026	−0.062	-0.158^{***}
	(0.034)	(0.032)	(0.037)	(0.023)
Only post-AA alumni	0.035	0.047	0.059	0.088^{**}
	(0.054)	(0.038)	(0.034)	(0.026)
Panel D. Alumni firm employment by application track and cohort				
General track alumni from same cohort	-0.057^{**}	-0.095^{**}	-0.103^{**}	-0.151^{**}
	(0.008)	(0.036)	(0.036)	(0.034)
General track alumni from diff. cohort	-0.049^{**}	0.077^{***}	0.036	0.071^{**}
	(0.007)	(0.021)	(0.024)	(0.016)
Only AA alumni from same cohort	0.043^{*}	0.023	0.046^{***}	0.044^{**}
	(0.011)	(0.020)	(0.008)	(0.010)
Only AA alumni from diff. cohort	0.019	0.011	0.016^{***}	-0.017^{**}
	(0.012)	(0.011)	(0.004)	(0.006)
N (enrollees)	2,895	13,410	11,266	5,445
N (wage observations)	2,343	10,312	8,409	3,515

Notes: This table examines heterogeneity by field of study in our DD estimates for top enrollees. The sample and dependent variables are the same as in column (B) of Table 6, but we estimate regressions for programs in four field of study groups: (A) business, (B) humanities, (C) natural sciences, and (D) social sciences. See Table 1 for the programs included in each field of study group. We cannot estimate our DD specification for health programs because there is no variation in our binary measure of exposure to affirmative action. All outcomes are measured 6–9 years after UERJ application.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A15. Robustness of DD estimates — Top enrollees

Dependent variable	(A) Bench-mark	(B) Pre-crisis years	(C) Linear trends	(D) Demo-graphics	(E) Selectivity controls	(F) No field of study	(G) Actual AA share
Panel A. Graduation and formal employment							
Graduated from UERJ program	0.013	0.004	0.037*	0.013	0.011	0.039*	0.006
	(0.021)	(0.019)	(0.021)	(0.021)	(0.020)	(0.021)	(0.020)
Formal employment	−0.027*	−0.022	−0.039**	0.005	−0.015	−0.044**	−0.023
	(0.015)	(0.014)	(0.019)	(0.008)	(0.021)	(0.020)	(0.015)
Panel B. Earnings							
Log hourly wage	−0.132***	−0.118**	−0.133**	−0.110**	−0.067**	−0.152**	−0.125**
	(0.045)	(0.047)	(0.061)	(0.043)	(0.033)	(0.061)	(0.052)
Firm mean hourly wage (log)	−0.095**	−0.094***	−0.120***	−0.075**	−0.059**	−0.125***	−0.082*
	(0.035)	(0.032)	(0.043)	(0.033)	(0.028)	(0.044)	(0.043)
Panel C. Employment at firms with pre- and post-AA alumni							
Pre-AA alumni	−0.055**	−0.061***	−0.072***	−0.050*	−0.035*	−0.064**	−0.064***
	(0.023)	(0.020)	(0.026)	(0.025)	(0.021)	(0.024)	(0.021)
Only post-AA alumni	0.049**	0.044**	0.080***	0.048**	0.017	0.073***	0.055**
	(0.023)	(0.020)	(0.025)	(0.023)	(0.018)	(0.024)	(0.024)
Panel D. Alumni firm employment by application track and cohort							
General track alumni from same cohort	−0.098***	−0.094***	−0.093***	−0.096***	−0.090***	−0.087***	−0.091***
	(0.021)	(0.016)	(0.024)	(0.022)	(0.022)	(0.025)	(0.023)
General track alumni from diff. cohort	0.042**	0.037**	0.046***	0.045**	0.034	0.044**	0.029
	(0.016)	(0.017)	(0.016)	(0.017)	(0.021)	(0.017)	(0.018)
Only AA alumni from same cohort	0.036***	0.029***	0.041***	0.036***	0.029***	0.041***	0.035***
	(0.009)	(0.009)	(0.007)	(0.008)	(0.008)	(0.007)	(0.008)
Only AA alumni from diff. cohort	0.010**	0.007	0.007	0.010**	0.006**	0.008*	0.011**
	(0.005)	(0.005)	(0.004)	(0.005)	(0.003)	(0.004)	(0.005)
N (enrollees)	35,866	28,591	35,866	35,866	35,866	35,866	35,866
N (wage observations)	26,445	20,889	26,445	26,445	26,445	26,445	26,445

Notes: Column (A) reproduces our benchmark DD results for top enrollees (column B in Table 6). Column (B) includes only outcomes measured in 2003–2014. Column (C) includes program-specific linear trends estimated in the 1995–2001 cohorts. Column (D) includes controls for age, gender, race, qualifying exam score, and writing field exam score. Column (E) includes cohort dummies interacted with dummies for quartiles of program selectivity (x-axis of Figure 1). Column (F) excludes the field of study group interactions, $f(m)$. Column (G) defines ExposureToAA_m as each major's affirmative action share in the 2004–2011 cohorts (y-axis of Figure 1), scaled to represent a 20 percentage point increase. Parentheses contain standard errors clustered at the program level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A16. Number of students in ENADE sample by exam year, field, and university

	Exam year												
Exam field	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	Total
Dentistry	270			156			415			475			1,316
Medicine	342			228			636			738			1,944
Nursing	385			258			479			426			1,548
Nutrition	158			117			255			317			847
Physical education	251			162			141	176		224	288		1,242
Social work	113			116			138			283			650
Biology		647			474			1,014			1,263		3,398
Chemistry		213			262			364			416		1,255
Computation		400			316			373			633		1,722
Engineering I (Civil)		342			454			441			1,036		2,273
Engineering II (Electrical)		333			340			522			730		1,925
Engineering III (Mechanical)		185			205			381			465		1,236
Engineering IV (Chemical)		173			120			180			291		764
Engineering VI (Production)		108			164			270			365		907
Geography		506			531			708			1,098		2,843
History		526			509			600			1,163		2,798
Language		861			905			1,247			1,336		4,349
Math		388			451			420			602		1,861
Philosophy		96			108			140			119		463
Physics		192			185			139			325		841
Social science		286			193			316			399		1,194
Teaching		835			1,396			4,584			3,807		10,622
Accounting			194			614			428			610	1,846
Business			710			1,029			1,018			1,128	3,885
Design			198			288			211			320	1,017
Economics			392			410			305			574	1,681
Law			537			1,226			1,229			1,574	4,566
Psychology			210			266			159			311	946
Social communication			288			316			184			385	1,173
UERJ	141	676	242	124	718	294	192	1,086	253	304	1,643	512	6,185
Other federal & state universities	1,378	5,415	2,287	913	5,895	3,855	1,872	10,789	3,281	2,159	12,693	4,390	54,927
Full sample	1,519	6,091	2,529	1,037	6,613	4,149	2,064	11,875	3,534	2,463	14,336	4,902	61,112

Notes: This table shows the number of students in our ENADE sample for Table 7. The sample includes 2004–2015 ENADE exam takers at UERJ and other federal and state universities that did not implement affirmative action until 2012 or later. We define each university's year of affirmative action adoption as the first year in which more than 10 percent of new enrollees entered through a reserved quota in the *Censo da Educação Superior* data, or, if it is earlier, the year of affirmative action adoption cited in Júnior and Daflon (2014) or Vieira and Arends-Kuenning (2019). The federal universities in our sample are: UFAC, UFAM, UFC, UFCG, UFCSPA, UFERSA, UFLA, UFMS, UFMT, UFPEL, UFRR, UFV, UFVJM, UNIFAL-MG, UNIFAP, UNIFEI, UNIR, and UNIRIO. The state universities in our sample are: UECE, UERJ, UERR, UNESP, UNITINS, and URCA. We exclude fields with no UERJ exam takers, and we drop any institution × major pair that does not have exam takers in every year in which the exam was offered.

TABLE A17. DDD estimates for UERJ's ENADE exam taker characteristics and scores

	(A)	(B)	(C)	(D)
	UERJ pre-AA mean	DDD coefficients		
Dependent variable	All students	All students	White private HS students	Other students
Panel A. Characteristics of ENADE exam takers				
# exam takers per program × year	38.250	−5.024	-8.316^{**}	−4.539
		(13.077)	(3.188)	(11.197)
Log # exam takers	3.557	−0.046	-0.356^{*}	−0.025
		(0.122)	(0.195)	(0.255)
White	0.750	-0.180^{***}		-0.138^{***}
		(0.027)		(0.022)
Private HS student	0.588	-0.133^{***}		-0.061^{***}
		(0.031)		(0.020)
Female	0.511	0.075^{***}	0.026	0.122^{***}
		(0.013)	(0.091)	(0.040)
Age	26.028	0.397	1.018^{*}	0.101
		(0.376)	(0.543)	(0.538)
Mother has a HS degree	0.726	-0.069^{**}	−0.008	−0.051
		(0.026)	(0.022)	(0.056)
HH income > 3x min. wage	0.916	-0.061^{***}	0.055^{*}	-0.106^{***}
		(0.021)	(0.029)	(0.035)
Panel B. ENADE scores (proportion correct answers)				
Overall score	0.566	−0.024	-0.030^{***}	−0.027
		(0.022)	(0.009)	(0.026)
Field-specific component	0.535	−0.020	-0.035^{***}	−0.020
		(0.026)	(0.011)	(0.028)
General component	0.661	-0.035^{**}	−0.014	-0.048^{*}
		(0.013)	(0.009)	(0.024)
N (programs × years)	36	1,664	1,664	1,664
N (exam takers)	747	61,112	16,851	37,992

Notes: This table displays triple-difference (DDD) estimates of the effect of affirmative action on the characteristics (Panel A) and scores (Panel B) of UERJ's ENADE exam takers. The sample includes 2004–2015 ENADE exam takers from UERJ and other federal and state universities that did not implement affirmative action until 2012 or later. See Appendix Table A16 for details on our ENADE sample and the exam fields. Column (A) shows dependent variable means for UERJ exam takers in 2004–2006. The other columns show θ coefficients from the DDD regression:

$$Y_{mjt} = \gamma_{mj} + \gamma_{mt} + \pi_{f(m)}[\text{UERJ}_j \times \text{Post}_t] + \theta[\text{UERJ}_j \times \text{Post}_t \times \text{ExposureToAA}_m] + \varepsilon_{mjt}.$$

Regressions are at the exam field (m) by institution (j) by year (t) level, with observations weighted by the number of exam takers. (In the first two rows of Panel A, we weight by the number of 2004–2006 exam takers in each mj cell.) We include field × institution dummies, γ_{mj}, field × year dummies, γ_{mt}, and an indicator for UERJ interacted with an indicator for the 2007–2015 cohorts, $\text{UERJ}_j \times \text{Post}_t$. We interact $\text{UERJ}_j \times \text{Post}_t$ with dummies for the field of study groups listed in Table 1, $f(m)$ (i.e., business, health, humanities, natural sciences, and social sciences). The variable of interest is $\text{UERJ}_j \times \text{Post}_t \times \text{ExposureToAA}$, where ExposureToAA is an indicator for UERJ programs in which the share of affirmative action enrollees in the 2004–2011 cohorts was 30 percent or higher. Columns (B)–(D) include all students, white students from private high schools, and non-white and/or public high school students, respectively. Parentheses contain standard errors clustered at the institution level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B. Empirical appendix

B.1. **Variable definitions.** This section describes the main variables in our paper.

- **Admission score.** Raw admission scores are based on applicants' subject scores on different subjects of the field exam (*Exame discursivo*), plus bonus points from their qualifying exam performance (*exame de qualificação*). We standardize raw admission scores to represent an individual's distance from the admission cutoff in their application pool in SD units. For this, we subtract the score of the last admitted student in the application pool, and divide by the SD of scores for all applicants to the same program/cohort. We adjust these SDs to be comparable across cohorts because the number of field exam takers varies significantly over time due to changes in UERJ's standards for the qualification exam.
- **Alumni firm employment.** We define UERJ applicant i to major m as obtaining a job at an alumni firm if their firm ever employed another individual j who graduated from major m (the "alum"). We define different versions of this variable based on the alum's cohort, application track, and year of employment. We also define versions that classify firms based on the number of alumni they hired relative to their mean size over all years of our data; for example, firms with 10 alumni per 1,000 employees include those with a mean size of 100 workers that hired at least one UERJ program alum, as well as those with a mean size of 10,000 workers who hired at least 100 alumni.
- **Demographic characteristics.** From the UERJ admission data, we observe age at application, gender, race, mother's education, and household income. Age is available in all cohorts; other characteristics are available only in certain cohorts (see Appendix Table A1). These variables come from a survey that applicants completed as part of the application process. We also use gender and race from the RAIS data, which we observe for any applicant who appears in this dataset. We use indicators for three racial groups: *branco* (white), *pardo* (brown), and *preto* (Black).
- **Field exam subject scores.** An applicant's scores on subjects of the field exam (*exame discursivo*). We use an applicant's writing exam score (which is common to all applicants), and their mean score across 2–4 other subjects (which vary depending on the cohort and major they are applying to). We observe field exam subject scores in the 1995–2001 and 2007–2011 cohorts.
- **Firm.** We define firms at the establishment level. Establishments are identified by their 14-digit CNPJ (short for *Cadastro Nacional da Pessoa Jurídica*, or National Registry of Legal Entities). The CNPJ is a tax identifier for legally incorporated identities. The first eight digits identify the company. The rest of the digits identify the branch or subsidiary of the company.
- **Firm mean wage.** The leave-individual-out mean hourly wage at a given firm.

- **Firm size.** The total number of workers employed by the firm at the end of each year.
- **Formal employment.** An indicator that takes the value one if an applicant appears in the employee-employer matched dataset (RAIS).
- **Hourly wage.** We calculate the hourly rate of each worker as the ratio between a worker's inflation-adjusted monthly earnings and the hours worked per month. Hours worked reflects the number of hours per week at which the firm hired the worker according to the worker's contract, which may differ from the hours actually worked in any given week.
- **Industry mean wage.** The leave-individual-out mean hourly wage of all UERJ applicants working in a given industry. We define industries at the 4- or 5-digit level of the Brazilian National Classification of Economic Activities (*Classificação Nacional de Atividades Economicas*) level.
- **Monthly earnings (2019 USD).** This variable represents a worker's average monthly salary in a given year. To report this variable, establishments have to calculate the worker's total earnings for the year and divide them by the number of months the firm employed the worker. We adjust earnings for inflation using the consumer price index. We express earnings in 2019 US dollars using the Brazilian Real/US Dollar exchange rate.
- **Municipality mean wage.** The leave-individual-out mean hourly wage of all UERJ applicants working at a given municipality. Municipalities are defined by the location of the worker's establishment.
- **Occupation mean wage.** The leave-individual-out mean hourly wage of all UERJ applicants with a given occupation. We define occupations at the 4-digit of the Brazilian Occupational Code Classification (*Classificação Brasileira de Ocupações*) level.
- **Qualifying exam score.** An applicant's standardized score from the qualifying exam (*exame de qualificação*). This exam includes eight subject tests common to all applicants: Biology, Chemistry, Geography, Foreign language (French, Spanish, or English), History, Literature/Portuguese, Mathematics, and Physics. Students that fail to achieve a minimum score on the qualifying exam cannot take the second round discursivo exam. We observe qualifying exam scores in the 1995–2001 cohorts.

We measure graduation and labor market outcomes in two time periods: 6–9 years after application, and 10–13 years after application. For earnings and wage indices, we use the mean value over each four-year period. For binary variables, we use the maximum value over the four year period.

B.2. **Data and merging.** Our base dataset includes a list of all individuals who passed UERJ's first-round admission exam and applied to an undergraduate program in the years 1995–2001 and 2004–2011. This dataset includes the program(s)/cohort that each individual applied to, their admission score in the second exam of the admission process, and their admission decisions. The 2004–2011 records include the track each applicant applied through. In addition, we have access to socioeconomic variables for the 2007–2011 application cohorts.

We combine the UERJ admission records with two individual-level administrative datasets. The first dataset is from UERJ, and it includes the graduation outcomes of all the students who enrolled in UERJ since 1995. These records contain the student's program, enrollment date, status as of December 2020 (i.e., graduated, dropped out, or still enrolled), and final year in the program.

The second administrative dataset is called the RAIS (*Relação Anual de Informações Sociais*), and it includes employment outcomes collected by the Ministry of Labor. We have access to the RAIS for the 2003–2019 period. This dataset has information on all workers with a formal-sector job. The RAIS contains information about both the worker and the firm. Worker information includes demographic variables (e.g., age, gender, and race), educational attainment, occupation, hours worked, and earnings. Firm-level variables include the number of employees, industry, and geographic location.

We merge the admission data with the graduation records using the university ID of each individual. Most individuals match uniquely on the ID, but in cases with duplicated IDs, we corroborate the quality of the matches using individuals' names and programs. We fix a few cases in which different individuals have the same university ID. We match 94.8 percent of individuals in the graduation records to the admission records using the university IDs. We use the names and application years of the remaining unmatched individuals to match them to the graduation records. Overall, we match 97.8 percent of the individuals in the graduation records to the admission records.

Lastly, we link the combined dataset from the above merge to the RAIS dataset using individuals' national ID numbers (*Cadastro de Pessoas Físicas*, or CPF for short), birth dates, and names. For this, we follow a two-step procedure. First, we match individuals for whom we have the CPF available in the UERJ records.[31] Second, for individuals who remain unmatched, we merge them using their names and dates of birth. We define a match from this process as observations that have either: 1) the same CPF number; or 2) the same birth date and an exact name match. We match 77.4 of the individuals in merged UERJ records to at least one year of the RAIS through this process. Out of the matched individuals, 66.1 percent were matched using the CPF, and the remaining 33.9 percent were matched using names and dates of birth.

One way to benchmark the merge rate with the RAIS is to compare it with the share of individuals with similar demographic characteristics who have a formal-sector job in Brazil. To do this, we use data from the 2015 Brazilian household survey (*Pesquisa Nacional por*

[31] The UERJ records contain the CPF nearly all individuals who applied in 2000–2001 and 2004–2011. Before 2000, the CPF is rarely available. Virtually all workers in the 2003–2019 RAIS datasets have a CPF.

Table B1. Timeline of events during the 2010 admission process

Event	Date
First date for applicants to take the qualifying exam	06/21/2009
Results of the qualifier exam are published	07/01/2009
Second date for applicants to take the qualifying exam	09/13/2009
Results of the qualifier exam are published	09/23/2009
Applicants who passed the qualifier exam take the field exam	12/13/2009
Results of the field exam are published	01/16/2010
Results of the field exam are published	01/30/2010
First round of admission offers is sent	01/30/2010
Second round of admission offers is sent	02/12/2010
Admitted students can enroll in first-semester programs	03/02/2010 – 03/03/2010
First day of classes - 1st semester	**03/10/2010**
Third round of admission offers is sent	03/16/2010
Fourth round of admission offers is sent	07/02/2010
Fifth round of admission offers is sent	07/16/2010
Newly admitted applicants can enroll in second-semester programs	07/28/2010 – 07/29/2010
First day of classes - 2nd semester	**08/10/2010**

Notes: This calendar is summarized from information in these two UERJ documents:

- http://sistema.vestibular.uerj.br/portal_vestibular_uerj/arquivos/arquivos2010/ed/03_anexo1_WEB.pdf
- http://sistema.vestibular.uerj.br/portal_vestibular_uerj/arquivos/arquivos2010/calendario/calendario_eq.pdf

Amostra de Domicílios, abbreviated PNAD), which includes information on the informal economy. Our proxy of working in the formal sector is having the right to a pension when retired.[32] The share of economically active individuals aged 25–37 with at least a high-school degree who have a job in the formal sector is 62.4 percent. This suggests that our merge identified most individuals with formal sector jobs.

B.3. **UERJ's admission process.** Applicants can gain admission to UERJ at one of several stages. The admission process begins with applicants taking a common qualifying exam. Applicants who pass this exam then take a field exam. UERJ ranks applicants based on their field exam scores and sends admissions offers to accepted applicants up to the capacity of each program. The remaining applicants are either rejected (if their score in the field exam is below a minimum threshold) or waitlisted. The first admission offers are typically sent in January, and admitted students have several weeks to accept or reject their offer. UERJ sends a second round of admission offers to waitlisted applicants based on the number of offers that were declined. This process is repeated up to five times per application year

[32] International organizations define informality in two different ways. Under the *legal* definition, a worker is considered informal if she does not have the right to a pension when retired. An alternative to the legal definition is the *productive* definition, where a worker is considered informal if she is a salaried worker in a small firm (i.e., it employs less than five workers), a non-professional self-employed, or a zero-income worker. We use the legal definition in the main text. The share of workers with a formal job under the productive definition is slightly lower than the one based on the legal definition.

if there are remaining open seats, and the last admission offers may occur as late as July. Appendix Table B1 provides an example of this process for the 2010 cohort.

The admission thresholds in our RD analysis are given by the admission score of the final student who gained admission in each application pool (after all waitlist offers). Any applicant who scored above this threshold could have been admitted to UERJ, although some of these students chose to enroll in other universities by the time they would have gotten in off the waitlist. Potential for non-random sorting around the admission cutoff arises because applicants have control over whether they accept or reject their admission offer. Students just above the final cutoff may therefore be those who particularly want to attend UERJ. We present tests for non-random sorting around the admission cutoff in Section 2.2.

B.4. **Sample.** Our initial dataset includes all applicants to UERJ undergraduate majors who passed the first-round qualifying exam and who have a valid second-round admission score (i.e., non-missing, non-zero). UERJ has several campuses; its main campus is in the municipality of Rio de Janeiro, and it has five smaller campuses in other municipalities in the state: Baixada Duque de Caixas (DDC), Nova Friburgo (NF), Resende (RES), São Gonçalo (SGO), and Teresópolis (TER). The number of undergraduate programs changes across cohorts of our data because UERJ split some large programs into smaller "sub-programs" and added some new majors.

Our raw data includes 71 different sub-programs across all cohorts and campuses. We group these 71 sub-programs into 43 programs to create a consistent set over time. We create these groups using documentation from UERJ detailing how large programs were divided into sub-programs. We exclude six new majors that UERJ created after the introduction of affirmative action: computing engineering (NF), geography (DDC), math (DDC), pedagogy (SGO), tourism (TER), and actuarial sciences (RIO). Appendix Tables B2-B4 show the 43 programs in our data and the sub-programs that they are derived from.

We use data from these 43 programs to create two different samples to analyze the impacts of UERJ's affirmative action policy. For our RD sample, we exclude programs where fewer than 30 percent of the 2004–2011 students entered through an affirmative action track. The second column in Appendix Tables B2-B4 shows the percent of students that entered through an affirmative action track in each program group during 2004–2011. Bolded figures denote programs where this figure is above 30 percent. 24 programs meet this criteria. Within these programs, we also exclude program-cohort-admission track triplets with fewer than five applicants below the admission threshold. We also exclude all applicants to the disabled/indigenous track since these quotas rarely filled up. In Appendix Tables B2-B4, we highlight in bold the program-cohort pairs in each admission track that satisfy our sample restrictions and appear in our RD sample.

For our DD sample, we focus on applicants who *enrolled* in UERJ. Our DD sample includes the 24 programs in our RD sample plus 19 other programs with lower take-up rates in the affirmative action tracks. These programs are unbolded in Appendix Tables B2-B4.

Table B2. Number of applicants by cohort — General track

#	Program	Prop. AA	Program name(s)	1995	1996	1997	1998	1999	2000	2001	2004	2005	2006	2007	2008	2009	2010	2011
1	Accounting	**0.364**	Accounting	**351**	**463**	**450**	**471**	**476**	**469**	**1160**	**350**	**442**	**502**	**374**	**484**	**492**	**551**	**492**
2	Art	0.287	Artistic education	198	233	210	230	235	234	547								
			Art								384	413	402					
			Art history											114		85	160	127
			Visual arts (bach.)											127				
			Visual arts (license)											125				
			Visual arts												326	334	292	328
3	Biology	**0.494**	Biology	**194**	**295**	**225**	**292**	**297**	**351**	**1899**	**659**	**1059**	**1156**	**973**	**873**	**1160**	**1028**	**1148**
4	Biology (SGO)	0.260	Biology	94	229	151	236	222	235	643	271	246	380	274	325	252	209	227
5	Business	**0.428**	Business	**466**	**583**	**459**	**590**	**590**	**593**	**2200**	**537**	**964**	**983**	**824**	**864**	**1071**	**943**	**1108**
6	Cartographic eng.	0.126	Cartographic eng.	43	112	86	119	117	117	185	69	79	129	104	115	156	218	148
7	Chemical eng.	**0.465**	Chemical eng.						**317**	**897**	**420**	**662**	**811**	**838**	**817**	**1149**	**1128**	**1290**
8	Chemistry	**0.352**	Chemistry	**352**	**474**	340	**352**	**353**	**160**	**408**	**212**	**206**	**336**	**319**	**317**	**363**	**349**	**321**
9	Computer science	**0.325**	Information science	**633**	**705**	**567**	**587**	**590**	**593**	**2029**	**592**	**699**	**775**	**637**	**603**	**665**	**548**	
			Computer science															**742**
10	Dentistry	**0.404**	Dentistry	**357**	**357**	**350**	**356**	**356**	**359**	**1235**	**450**	**447**	**632**	**446**	**458**	**441**	**503**	**605**
11	Economics	0.286	Economics	442	541	408	539	548	547	1355	529	640	664	538	532	752	709	754
12	General eng.	**0.307**	Engineering	1065	**1252**	**1182**	**1407**	**1410**	**1417**									
			Civil eng.							**736**	**291**	**512**	**574**	**511**	**691**	**908**	**905**	**1310**
			Electrical eng.							**2409**	**614**	**922**	**1070**	**695**	**765**	**1048**	**1066**	**1109**
			Textile eng.	18	**153**	42												
13	Geography	**0.468**	Geography	**113**	**119**	**115**	**157**	**198**	**196**	**943**	**337**	**578**	**524**	**523**	**587**	**532**	**544**	**523**
14	Geog. Ed. (SGO)	0.275	Geography	137	214	131	232	237	237	591	291	366	369	259	259	334	225	202
15	Geology	**0.321**	Geology	28	**84**	**65**	**88**	**89**	**86**	**203**	**94**	**144**	**216**	**185**	**264**	**409**	**317**	**329**
16	History	**0.457**	History	**383**	**298**	**288**	**392**	**395**	**393**	**1991**	**723**	**981**	**1007**	**863**	**719**	**828**	**830**	**772**
17	Hist. Ed. (SGO)	**0.333**	History	**202**	**234**	**185**	**238**	**236**	**236**	**592**	**306**	**311**	**412**	**318**	**264**	**292**	**203**	**226**
18	Industrial design	**0.456**	Industrial design	**157**	**174**	**171**	**208**	**175**	**207**	**836**	**357**	**480**	**655**	**517**	**539**	**627**	**627**	**699**
19	Journalism	**0.481**	Social communication	**350**	**355**	**350**	**358**											
			Journalism					**237**	**239**	**1679**	**528**	**717**	**803**	**737**	**672**	**940**	**777**	**1130**
			Public relations					**159**	**239**	**948**	**311**	**469**	**552**	**500**	**479**	**645**	**619**	**717**
20	Language (SGO)	0.205	Language	298	351	326	469	470	475	910	332	334	395	208	246	251	180	233
21	Language I	0.259	Language I	282	267	263	296	292	294	978								
			Literature/English								403	328	403	325	285	318	281	295
			Port./German								48	81	60	49	36	52	46	42
			Port./Japanese								48	65	66	58	62	65	36	35

Table B2. Number of applicants by cohort — General track *(continued)*

#	Program	Prop. AA	Program name(s)	1995	1996	1997	1998	1999	2000	2001	2004	2005	2006	2007	2008	2009	2010	2011
22	Language II	0.293	Language II	317	350	363	388	388	513	1117								
			Port./France								77	166	74	137	78	99	104	78
			Port./Italian								93	165	87	95	80	77	86	74
			Port./Spanish								245	200	312	220	206	232	184	216
23	Language III	**0.379**	Language III	**255**	**322**	**305**	**295**	**296**	**294**	**1212**								
			Port./Greek								**30**	**21**	**63**	**21**	**29**	10	**28**	15
			Port./Latin								**57**	**71**	**50**	**65**	**36**	**47**	**31**	**44**
			Port./Literature								**454**	**348**	**460**	**338**	**295**	**330**	**294**	**305**
24	Law	**0.460**	Law	**1455**	**1477**	**1479**	**1483**	**1496**	**1486**	**5940**	**2079**	**2271**	**3182**	**2487**	**2468**	**2909**	**2884**	**3734**
25	Math	0.158	Math	243	354	537	586	592	589	1181	490	506	515	337	367	367	322	302
26	Math Ed. (SGO)	0.143	Math	88	198	160	235	232	235	283	148	138	142	111	121	118	86	98
27	Mech. eng.	**0.353**	Mech. eng.							**516**	**206**	**371**	**508**	**585**	**505**	**728**	**680**	**822**
28	Mech. eng. (NF)	0.186	Mech. eng.					170	228	143	74	144	238	196	285	334	315	380
29	Medicine	**0.454**	Medicine	**541**	**546**	**546**	**550**	**552**	**551**	**4122**	**1473**	**1749**	**2754**	**2838**	**2025**	**2639**	**2669**	**3971**
30	Nursing	**0.431**	Nursing	**187**	**312**	**274**	**313**	**311**	**389**	**1478**	**563**	**565**	**762**	**475**	**585**	**536**	**507**	**499**
31	Nutrition	**0.411**	Nutrition	236	**316**	**368**	**311**	**390**	**444**	**2203**	**566**	**706**	**818**	**561**	**599**	**689**	**516**	**661**
32	Oceanography	0.229	Oceanography	70	90	78	110	112	116	279	121	483	367	262	268	271	270	228
33	Teaching	0.234	Teaching	557	614	577	623	616	621	1852	724	880	809	511	464	555	451	509
34	Teaching (DDC)	0.208	Teaching	267	251	267	354	347	343	637				143	155	151	138	183
			Teaching I								201	160	225					
			Teaching II								75	51	121					
35	Philosophy	0.222	Philosophy	246	292	264	288	288	287	593	362	272	381	276	228	251	199	186
36	Physical ed.	0.206	Physical education	177	236	238	290	295	352	1611	447	506	600	413	405	470	384	413
37	Physics	0.135	Physics	196	321	299	402	432	434	664	295	410	472	397	289	378	325	320
38	Prod. eng.	**0.384**	Prod. eng.							**694**	**294**	**466**	**539**	**572**	**526**	**696**	**578**	**792**
39	Prod. eng. (RES)	0.152	Prod. eng.	144	284	264	288	290	293	338	185	284	375	301	356	400	372	394
40	Psychology	**0.480**	Psychology	**268**	**390**	**355**	**394**	**391**	**383**	**1527**	**719**	**887**	**984**	**789**	**800**	**920**	**854**	**1138**
41	Social science	**0.408**	Social sciences	**283**	**297**	**285**	**294**	**294**	**293**	**1311**	**472**	**594**	**631**	**628**	**448**	**547**	**468**	**440**
42	Social work	**0.432**	Social work	**261**	**276**	**266**	**276**	**275**	**274**	**1240**	**315**	**374**	**511**	**325**	**324**	**348**	**348**	**427**
43	Statistics	0.087	Statistics	148	286	343	465	468	465	475	203	107	303	134	179	139	190	149

Notes: This table displays the number of applicants in the general track for each program/cohort in our data. See Table B4 for details on the table structure and statistics.

Table B3. Number of applicants by cohort — Public high school track

#	Program	Prop. AA	Program name(s)	2004	2005	2006	2007	2008	2009	2010	2011
1	Accounting	**0.364**	Accounting	74	50	**71**	**41**	**31**	**37**	**50**	**50**
2	Art	0.287	Artistic education								
			Art	84	26	19					
			Art history				9		5	2	4
			Visual arts (bach.)				7				
			Visual arts (license)				10				
			Visual arts					17	16	13	13
3	Biology	**0.494**	Biology	**126**	**70**	**88**	**64**	**38**	**64**	**51**	**48**
4	Biology (SGO)	0.260	Biology	66	19	37	21	19	13	19	25
5	Business	**0.428**	Business	**111**	**53**	**85**	**59**	**54**	**54**	**67**	**75**
6	Cartographic eng.	0.126	Cartographic eng.	12	2	4	3	2	8	5	14
7	Chemical eng.	**0.465**	Chemical eng.	**43**	45	**42**	**34**	27	**56**	**42**	**55**
8	Chemistry	**0.352**	Chemistry	29	11	**24**	**16**	**24**	**13**	17	9
9	Computer science	**0.325**	Information science	**111**	40	**57**	**34**	**43**	**36**	38	
			Computer science								**35**
10	Dentistry	**0.404**	Dentistry	**57**	15	**25**	17	17	**23**	**28**	**30**
11	Economics	0.286	Economics	77	34	40	24	32	30	26	31
12	General eng.	**0.307**	Civil eng.	58	26	**35**	**30**	23	**58**	**47**	**74**
			Engineering								
			Electrical eng.	113	54	**87**	**56**	38	**64**	**72**	**72**
			Textile eng.								
13	Geography	**0.468**	Geography	**80**	**56**	**49**	**49**	**25**	**40**	**32**	20
14	Geog. Ed. (SGO)	0.275	Geography	73	55	35	23	17	20	21	26
15	Geology	**0.321**	Geology	**14**	4	12	4	7	**20**	10	**16**
16	History	**0.457**	History	**174**	**81**	**114**	**84**	**54**	**55**	**53**	**55**
17	Hist. Ed. (SGO)	**0.333**	History	**71**	29	**50**	**27**	24	20	9	24
18	Industrial design	**0.456**	Industrial design	**33**	11	**33**	17	**18**	**33**	**33**	**36**
19	Journalism	**0.481**	Social communication								
			Journalism	**94**	**30**	**54**	**33**	**33**	**36**	**38**	**44**
			Public relations	**37**	**13**	**28**	21	**23**	**25**	**34**	**35**
20	Language (SGO)	0.205	Language	108	26	57	24	21	15	14	16
21	Language I	0.259	Language I								
			Literature/English	53	17	27	9	8	15	19	10
			Port./German		2	1	1		5	1	2
			Port./Japanese	8	3	6	2	1	4	1	5

Table B3. Number of applicants by cohort — Public high school track *(continued)*

#	Program	Prop. AA	Program name(s)	2004	2005	2006	2007	2008	2009	2010	2011
22	Language II	0.293	Language II								
			Port./France	15	18	7	9	1	5	8	3
			Port./Italian	22	13	13	9	6	4	6	7
			Port./Spanish	79	19	30	24	11	11	20	15
23	Language III	**0.379**	Language III								
			Port./Greek	**7**	2	4	3		1	1	1
			Port./Latin	**11**	4	8	4	1	3	4	2
			Port./Literature	**123**	**37**	**67**	33	27	16	20	**25**
24	Law	**0.460**	Law	**284**	**113**	**217**	**132**	**147**	**173**	**198**	**203**
25	Math	0.158	Math	133	41	47	28	23	18	16	9
26	Math Ed. (SGO)	0.143	Math	35	13	18	11	10	3	8	7
27	Mech. eng.	**0.353**	Mech. eng.	**31**	9	16	**17**	**24**	**27**	**38**	**31**
28	Mech. eng. (NF)	0.186	Mech. eng.	16	6	16	9	11	21	18	43
29	Medicine	**0.454**	Medicine	**135**	**61**	**73**	**83**	**65**	**106**	**145**	**189**
30	Nursing	**0.431**	Nursing	**119**	**44**	**74**	**25**	**46**	**52**	**37**	**37**
31	Nutrition	**0.411**	Nutrition	**93**	33	**80**	**50**	**35**	**41**	**29**	**44**
32	Oceanography	0.229	Oceanography	13	19	17	8	6	8	9	10
33	Teaching	0.234	Teaching	284	91	137	74	32	37	40	41
34	Teaching (DDC)	0.208	Teaching				16	9	7	12	9
			Teaching I	59	19	32					
			Teaching II	27	5	11					
35	Philosophy	0.222	Philosophy	70	14	30	18	13	11	15	11
36	Physical ed.	0.206	Physical education	105	27	51	24	26	19	19	25
37	Physics	0.135	Physics	70	25	32	17	13	17	13	15
38	Prod. eng.	**0.384**	Prod. eng.	21	18	13	14	**22**	**39**	21	**39**
39	Prod. eng. (RES)	0.152	Prod. eng.	31	8	17	12	8	17	20	21
40	Psychology	**0.480**	Psychology	**181**	**76**	**85**	**56**	**47**	**62**	**63**	**77**
41	Social science	**0.408**	Social sciences	**75**	36	**49**	**43**	17	20	28	24
42	Social work	**0.432**	Social work	**127**	**56**	**81**	**54**	27	**34**	28	**38**
43	Statistics	0.087	Statistics	31	5	16	1	9	4	3	5

Notes: This table displays the number of applicants in the public high school track for each program/cohort in our data. See Table B4 for details on the table structure and statistics.

Table B4. Number of applicants by cohort — Black track

#	Program	Prop. AA	Program name(s)	2004	2005	2006	2007	2008	2009	2010	2011
1	Accounting	**0.364**	Accounting	**47**	17	25	17	13	23	**33**	**42**
2	Art	0.287	Artistic education								
			Art	53	12	10					
			Art history				3			6	7
			Visual arts (bach.)				3				
			Visual arts (license)				2				
			Visual arts					6	8	14	6
3	Biology	**0.494**	Biology	**64**	**28**	**28**	**32**	14	**39**	**33**	**44**
4	Biology (SGO)	0.260	Biology	28	9	13	8	4	7	7	6
5	Business	**0.428**	Business	**59**	**38**	**42**	17	25	**54**	**42**	**47**
6	Cartographic eng.	0.126	Cartographic eng.	4	1	2	2		2	9	5
7	Chemical eng.	**0.465**	Chemical eng.	26	13	18	16	16	**34**	**43**	**47**
8	Chemistry	**0.352**	Chemistry	**19**	6	10	8	2	14	8	15
9	Computer science	**0.325**	Information science	63	19	**30**	20	17	14	21	
			Computer science								17
10	Dentistry	**0.404**	Dentistry	**42**	5	**18**	12	16	14	**24**	**24**
11	Economics	0.286	Economics	36	14	20	5	6	25	30	22
12	General eng.	**0.307**	Civil eng.	21	7	12	11	9	25	**31**	**70**
			Engineering								
			Electrical eng.	**61**	22	30	25	13	32	40	**62**
			Textile eng.								
13	Geography	**0.468**	Geography	**49**	23	18	14	19	21	**33**	22
14	Geog. Ed. (SGO)	0.275	Geography	52	21	18	12	11	7	15	11
15	Geology	**0.321**	Geology	7	4	4	2	6	**13**	12	**13**
16	History	**0.457**	History	**124**	**37**	**39**	**37**	24	**50**	**36**	**31**
17	Hist. Ed. (SGO)	**0.333**	History	**44**	10	19	11	11	16	11	9
18	Industrial design	**0.456**	Industrial design	**25**	6	**14**	7	8	**21**	8	**18**
19	Journalism	**0.481**	Social communication								
			Journalism	**67**	**13**	**25**	19	**23**	**29**	**31**	**31**
			Public relations	**36**	**22**	**22**	11	8	**25**	**30**	**32**
20	Language (SGO)	0.205	Language	47	15	16	6	3	10	4	9
21	Language I	0.259	Language I								
			Literature/English	31	8	6	4	5	11	7	12
			Port./German	4	1		2	1	1		
			Port./Japanese	6		1	1		2	2	

Table B4. Number of applicants by cohort — Black track *(continued)*

#	Program	Prop. AA	Program name(s)	2004	2005	2006	2007	2008	2009	2010	2011
22	Language II	0.293	Language II								
			Port./France	10	3	2	4	4	2	5	3
			Port./Italian	7	12	3	1	2	2	2	4
			Port./Spanish	36	9	18	17	9	6	7	8
23	Language III	**0.379**	Language III								
			Port./Greek	7		3	1		1	2	1
			Port./Latin	6	4	2	5		3	1	
			Port./Literature	**71**	18	**24**	12	11	13	13	19
24	Law	**0.460**	Law	**271**	**89**	**138**	**122**	**125**	**174**	**192**	**247**
25	Math	0.158	Math	54	16	9	15	10	5	7	6
26	Math Ed. (SGO)	0.143	Math	16	2	1	1	1	1	3	3
27	Mech. eng.	**0.353**	Mech. eng.	**28**	7	10	9	11	**22**	**18**	**27**
28	Mech. eng. (NF)	0.186	Mech. eng.	3	1	2	2		6	7	8
29	Medicine	**0.454**	Medicine	**123**	**38**	**53**	**58**	**48**	**114**	**120**	**187**
30	Nursing	**0.431**	Nursing	**109**	17	**35**	19	20	**32**	**23**	**26**
31	Nutrition	**0.411**	Nutrition	**60**	**24**	**25**	10	16	22	19	28
32	Oceanography	0.229	Oceanography	11	6	9	2	4	2	6	2
33	Teaching	0.234	Teaching	157	40	57	25	16	27	23	34
34	Teaching (DDC)	0.208	Teaching				12	12	15	8	11
			Teaching I	55	13	17					
			Teaching II	17	8	5					
35	Philosophy	0.222	Philosophy	52	11	9	4	2	14	6	4
36	Physical ed.	0.206	Physical education	56	6	24	9	5	6	11	12
37	Physics	0.135	Physics	37	7	10	8	4	9	8	4
38	Prod. eng.	**0.384**	Prod. eng.	9	6	5	3	12	**23**	10	**36**
39	Prod. eng. (RES)	0.152	Prod. eng.	14	5	8	2	1	5	7	7
40	Psychology	**0.480**	Psychology	**123**	**32**	**37**	**28**	28	**43**	**42**	**53**
41	Social science	**0.408**	Social sciences	**56**	19	23	22	22	15	**34**	29
42	Social work	**0.432**	Social work	**119**	**36**	**48**	**27**	30	**41**	22	**52**
43	Statistics	0.087	Statistics	21	4	8		1	2	3	6

Notes: This table displays the number of applicants in the Black track for each program/cohort in our data. The first column shows the 43 programs in our RD and DD samples. The second column shows the proportion of 2004–2011 enrollees in each who were from any affirmative action track (y-axis of Figure 1); **bold** numbers in this column show programs with ExposureToAA$_m$ = 1 in our benchmark DD specification (3). The third column shows the subgroups that comprise each program. Remaining columns show the number of applicants to each program/cohort; **bold** numbers denote program/cohorts that we include in our RD sample.

B.5. **Fuzzy merge of UERJ and higher education census data.** In Section 3, we examine the effects of UERJ enrollment on college selectivity using data from a census of all Brazilian college enrollees (*Censo da Educação Superior*). This subsection describes the merge between UERJ applicants and the higher education census.

We focus on universities in the state of Rio de Janeiro since most Brazilian college students enroll in a university in their home state. We include only 2009–2011 UERJ applicants in this analysis because the higher education census does not exist at the individual level prior to 2009.

We do not observe individuals' ID numbers in the higher education census, so we link the census to the UERJ records using a fuzzy merge based on exact day of birth, gender, and year of enrollment.[33] In the census data, we compute the *total* number of students at a particular university with a given birthdate, gender, and enrollment year. We merge these variables into our UERJ sample using birthdate, gender, and year of *application.* We then use these totals as dependent variables in our RD specification.

The resulting dependent variables reflect the total number of enrollees in a particular university in Rio de Janeiro who have the same birthdate/gender/enrollment-year triplet as a UERJ applicant. The ideal dependent variable—if we could uniquely identify individuals in the census—would be an indicator variable that takes the value one if a given UERJ applicant enrolled in a given university and zero otherwise. If no college student at a university has the same birthdate/gender/enrollment-year triplet as the applicant, we know that the applicant did not enroll in that university in that year (barring errors in the merge variables). However, if one or more enrollees at the university share the same combination of those three variables, we cannot tell with certainty whether the applicant ended up enrolling in the university.[34] Thus our dependent variables contain additional measurement error.

B.6. **Decomposition of DD estimate for top enrollees' log hourly wages.** Our main result in Section 5 is that top enrollees' hourly wages declined by 0.132 log points in UERJ majors with high exposure to affirmative action relative to less-exposed majors (Table 6, Panel B, column B). This section describes back-of-the-envelope calculations on the proportion of this estimate that can be explained by compositional, networking, and learning mechanisms.

First, we find that 25 percent of our main DD estimate can be explained by changes in the observable characteristics of top enrollees. In Panel D of Table 5 we combine applicants'

[33] Each of these variables is available in the public version of the *Censo da Educação Superior* that we downloaded from the website of a Brazilian Ministry of Education agency called INEP (*Instituto Nacional de Estudos e Pesquisas Educacionais Anísio Teixeira*). Some of these variables are no longer available in the current version of this dataset on INEP's website.

[34] For the average UERJ applicant in our data, there are 29 students with the same birthdate, gender, and enrollment year across all Rio de Janeiro universities.

observable characteristics—both demographics and admission scores—into a log wage index, and use this as an outcome variable in our DD specification (3). For individuals who appear in the RAIS data, the observable characteristics of top enrollees declined by 0.033 log wage points in majors with greater exposure to affirmative action relative to less-exposed majors (last row in Panel D of Table 5, column B). Although this point estimate is not statistically significant, it is 25 percent of the magnitude of our main DD estimate for top enrollees' log hourly wages, i.e., $-0.033/(-0.132) = 25\%$.

Next, we find that 10–17 percent of our DD coefficient for top enrollees' hourly wages can be explained by networking mechanisms. We estimate the contribution of networking mechanisms by multiplying the DD estimates for employment in alumni firms (Panels C–D of Table 6) by the OLS wage premia associated with employment in these firms (Appendix Table A11). In Panel C of Table 6, we find that greater exposure to affirmative action reduced top enrollees' likelihood of employment at firms that hired pre-AA alumni by 5.5 percentage points, and it increased their likelihood of employment at firms that hired *only* post-AA alumni by 4.9 percentage points. Appendix Table A11 shows that firms with pre-AA alumni had an hourly wage premium of 0.481 log points relative to other firms in our sample, whereas firms with only post-AA alumni had an hourly wage premium of 0.274 log points. Under the assumption that these OLS wage premia reflect causal effects, the change in access to pre-AA and post-AA alumni firms can explain 10 percent of our main DD estimate for hourly wages, i.e., $(-0.055 * 0.481 + 0.049 * 0.274)/(-0.132) \approx 10\%$. If we do a similar calculation using the estimates from Panel D of Table 6—which measure employment with alumni from different application tracks and cohorts—we find that these estimates can explain 17 percent of our main DD estimate for hourly wages.[35]

Lastly, we estimate that 32 percent of the negative wage effect for top enrollees can be explained by learning mechanisms. We find that affirmative action reduced UERJ's white private high school students' proportion of correct answers on the ENADE exam by 2.2 percentage points (first row in Panel B of Table 7, column C).[36] All else equal, white students from private high schools would have had to obtain *higher* entrance exam scores to be

[35] Specifically, the DD estimates in Panel D of Table 6 and their associated OLS firm wage premia are:

- General track alumni from the same cohort: DD coef = -0.098; OLS wage premium = 0.533;
- General track alumni from different cohorts: DD coef = 0.042; OLS wage premium = 0.455;
- Only AA alumni from the same cohort: DD coef = 0.036; OLS wage premium = 0.293;
- Only AA alumni from different cohorts: DD coef = 0.010; OLS wage premium = 0.070.

Thus we can explain $(-0.098 * 0.533 + 0.042 * 0.455 + 0.036 * 0.293 + 0.010 * 0.070)/(-0.132) \approx 17\%$ of our main DD estimate for hourly wages.

[36] We find a slightly larger point estimate (-3.0pp) in a triple-difference specification that also compares UERJ majors with more- and less-exposure to affirmative action (Appendix Table A17).

admitted to UERJ in the cohorts with affirmative action.[37] Thus we think that the 2.2 percentage point decline in ENADE scores is, if anything, likely to underestimate the decline in scores that we would find in our top enrollee sample. We cannot estimate the relationship between hourly wages and the proportion of correct answers on the ENADE exam because we do not have these two datasets linked at the individual level. As an alternative benchmark, we use Reyes (2022)'s estimate that a one percentage point increase in the proportion of correct answers on Brazil's national college entrance exam (ENEM) is associated with a 0.0192 log point increase in early-career wages. Under the assumption that the relationship between correct answers and wages is the same for the ENEM and ENADE exams, the decline ENADE scores for white private high school students can explain 32 percent of our DD estimate for top enrollees' wages, i.e., $(-2.2 * 0.0192)/(-0.132) = 32\%$.

[37] However, we find limited evidence that affirmative action changed the demographics characteristics of UERJ's white private high school students who took the ENADE exam (Panel A of Table 7).