ALUMNI JOB NETWORKS AT ELITE UNIVERSITIES AND THE EFFICACY OF AFFIRMATIVE ACTION

CECILIA MACHADO[#]
GERMÁN REYES*
EVAN RIEHL[+]

DECEMBER 2021

ABSTRACT. We examine the efficacy of affirmative action at universities whose value depends on peer and alumni networks. We study an elite Brazilian university that adopted race- and income-based affirmative action at a large scale. Using employer-employee data, we show that a key benefit of attending the university is access to high-paying jobs affiliated with its alumni. Affirmative action increased disadvantaged students' access to these firms and raised their early-career earnings. But these benefits faded over time. Further, the increase in student body diversity lowered the job prospects and earnings of the school's most highly ranked students.

[#]Machado: Graduate School of Economics, Getúlio Vargas Foundation (FGV-EPGE), Praia de Botafogo, 190 11º andar, Rio de Janeiro, RJ 22250-900, Brazil (email: cecilia.machado@fgv.br) *Reyes: Department of Economics, Cornell University, 404 Uris Hall, Ithaca, NY 14853, United States (email: gjr66@cornell.edu). [+]Riehl (corresponding author): Department of Economics and ILR School, Cornell University, 266 Ives Hall, Ithaca, NY 14853, United States (email: eriehl@cornell.edu). For helpful comments we thank Sandra Black, Fernanda Estevan, Chao Fu, Adam Kapor, Michael Lovenheim, Ana Ribeiro, Seth Sanders, Fabio Schanaider, and Miguel Urquiola. We are grateful to UERJ for assistance with the data. All errors are our own.

This paper examines the efficacy of 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 large state university systems can raise the earnings of underrepresented minority students (Bleemer, 2020). But there is limited evidence on the earnings effects of affirmative action at the most selective 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 be different at elite universities relative to moderately selective schools. The debate on affirmative action often centers on the question of how it affects the likelihood that students earn a college degree (Arcidiacono et al., 2016; Bagde et al., 2016). This debate is less relevant at elite universities because graduation rates are high, and *all* students on the margin of admission are high achieving (Bowen and Bok, 1998). It is often said that the benefit of attending an elite school comes from peer connections and alumni job networks, and research finds that these benefits accrue primarily to high-income students (Zimmerman, 2019; Michelman et al., 2021). At schools where networking matters, the efficacy of affirmative action depends on whether the value of these networks persists as diversity increases.

We study an elite university in Brazil that adopted large-scale race- and income-based affirmative action. We link admission records to national employer-employee data to show that a key benefit of attending the university is access to high-paying firms affiliated with its alumni. We find that affirmative action increased disadvantaged students' access to these firms and raised their early-career earnings. But both of these benefits faded as their careers progressed. In addition, the increase in student body diversity lowered the job prospects and earnings of the university's most highly ranked students.

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 US.

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 low-income applicants, with separate quotas for Black and public high school students. Students admitted through affirmative action were very 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 dramatic increase in the racial and socioeconomic diversity of UERJ's student body. The scale of affirmative action at UERJ is much larger than that at most US universities, but it is similar in magnitude to the proposed admission policy in Chetty et al. (2020).

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.

The employer-employee data allows us to examine how UERJ's alumni network affects its students' job outcomes. We 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 define different outcomes based on the relationship between the applicant and the alum, the timing of their hires, and the concentration of alumni at the firm. These outcomes capture a variety of channels through which peers and alumni can affect an individual's job prospects, including referrals, search behavior (Marmaros and Sacerdote, 2002), on-campus recruiting (Weinstein, 2021), and reputation effects (MacLeod et al., 2017).

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). This identifies the labor market returns to attending UERJ for marginal admits in each track. Our second strategy exploits variation in affirmative action take-up in a difference-in-differences (DD) design. 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. Our DD regressions estimate changes in top enrollees' outcomes between the pre- and post-AA cohorts, and across majors with higher and lower take-up rates. This design identifies the effects of a 19 percentage point increase in the share of affirmative action enrollees on the outcomes of students who were not targeted by the policy.

We have three main findings. First, affirmative action was successful at getting disadvantaged students jobs at firms affiliated with UERJ's alumni network. In our RD analysis, affirmative action students who enrolled in UERJ were 14 percentage points more likely to work at firms with other alumni measured 6–9 years after application. This employment effect is relatively larger for firms with a high concentration of alumni, and in cases where the applicant and alum were from the same cohort. Firms with UERJ alumni typically paid much higher wages than other firms in our data. This suggests that an important part of the value of attending UERJ comes from alumni job networks, and that affirmative action extended this benefit to Black and low-income students.

Second, this job access increased affirmative action students' early-career earnings, but the earnings gain 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. Admission to UERJ did not affect the likelihood that affirmative action students earned a college degree, and it had limited effects on their occupations or industries of employment. This suggests that the earnings effect was driven primarily by increased access to jobs in UERJ's alumni network. But the earnings gain faded over time, and it was close to zero measured 13 years after application. The effects on job access also decreased over time, suggesting a declining influence of UERJ's network relative to other networks formed in the labor market.

Third, the adoption of affirmative action lowered the job prospects and earnings of UERJ's most highly ranked students. In our DD analysis, we find that top enrollees' earnings decreased in majors with high affirmative action take-up relative to those with lower take-up. Our estimates imply that a one percentage point increase in the share of affirmative action enrollees reduced top students' wages by 0.7 percent, and this effect persisted up to 13 years after enrollment. Greater exposure to affirmative action reduced the rate of employment at jobs affiliated with alumni from the general track and pre-AA cohorts. These jobs paid higher wages than the jobs obtained by affirmative action students, suggesting that the earnings declines were driven by a decrease in the value of network connections. We also find negative spillover effects on the earnings of affirmative action students, although these effects are less-cleanly identified.

Our paper contributes to research on the efficacy of affirmative action for reducing earnings inequality (Bowen and Bok, 1998; Arcidiacono et al., 2015). Bleemer (2020) shows that affirmative action can raise the earnings of underrepresented minority students by studying a ban on the policy in the University of California system. This ban affected enrollment outcomes at many schools that vary widely in selectivity, including two- and four-year state colleges. Bleemer finds that affirmative action increased the likelihood that minority students earned a four-year degree, which suggests that educational attainment is a key mechanism for the earnings gains. Consistent with this, research on the returns to admission to US state university *systems* typically finds large increases in both earnings and degree attainment for disadvantaged students (Zimmerman, 2014; Smith et al., 2020; Bleemer, 2021).[1]

Affirmative action can be less effective at reducing earnings inequality at elite universities whose value depends more on networking. In our sample, the large majority of affirmative action applicants earned a college degree, and admission to UERJ had no effect on the likelihood of degree attainment. This can explain why we find no longer-run effects on affirmative action students' earnings. It is likely that Black and low-income individuals faced other barriers to promotion and networking in their careers. The scope for affirmative action to reduce earnings inequality may be limited if a university's value depends on the networks its graduates form in a segregated labor market.

We also contribute to research on the returns to college selectivity by examining the role of peer and alumni networks (Dale and Krueger, 2002).[2] There is a small literature on network formation in college (Marmaros and Sacerdote, 2002; Mayer and Puller, 2008; Zhu, 2021), but most papers do not have the data necessary to examine both job networks and earnings. Our analysis of elite university networks is most similar to Zimmerman (2019) and Michelman et al. (2021); 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. We find that affirmative action can help disadvantaged students gain access to high-paying firms upon labor market entry, but more research is needed on policies that might help make these initial gains more persistent.

Lastly, our paper is novel in its identification of the spillover effects of affirmative action. 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). Identification is a central challenge in these papers, as there is extensive sorting of students into colleges. Our empirical design allows us to identify spillover effects under weaker assumptions, and the large scale of UERJ's affirmative action policy is unique in the

[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; Mello, 2019; Ribeiro and Estevan, 2021), with the exception of Francis-Tan and Tannuri-Pianto (2018).

[2] Many papers in this literature use RD designs to estimate returns to selective colleges and/or majors (Hoekstra, 2009; Saavedra, 2009; Hastings et al., 2013; Kirkebøen et al., 2016; Canaan and Mouganie, 2018; Anelli, 2018; Ng and Riehl, 2020). Other papers examine student/college match effects (Andrews et al., 2016; Dillon and Smith, 2018; Hoxby, 2018; Riehl, 2019; Mountjoy and Hickman, 2020).

literature. The magnitude of our estimated spillover effect on earnings is similar to that in Arcidiacono and Vigdor (2010), although the authors emphasize learning mechanisms more than networking. 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 significantly from those estimated using the existing distribution of students across colleges (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 2–3 present our RD analysis of the direct effects of affirmative action on disadvantaged students. Sections 4–5 present our DD analysis of the policy's spillover effects on untargeted students. Section 6 concludes.

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 11$^{\text{th}}$ 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 the scores in the field exam tests. 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.[3] 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

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

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.

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.[4] 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.[5] 20 percent of slots in each major were reserved for *public high school* applicants. Another 20

[4] 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, 2017).

[5] 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.

percent of slots were reserved for *Black* applicants.[6] 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.[7] Applicants who did not meet these criteria could apply through the *general* track, which governed the remaining 55 percent of seats.

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 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.[8]

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 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

[6] 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.

[7] In 2004, for example, applicants had to be from families whose per capita income was below $R300 per month (Zoninsein and Júnior, 2008), which was roughly 40 percent of the national GDP per capita.

[8] Affirmative action applicants had to meet both the quota category requirement (Black or public high school) and the low-income requirement. As a result, the large majority of 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.

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.3 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 programs, we also exclude any cohort/application-track pair in which there are fewer than five applicants below the admission threshold (see Appendix Tables B1–B3).[9]

Our DD sample includes all programs that UERJ offered both before and after 2004.[10] 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.

1.5. **Definition of network variables.** A central focus of our analysis is the role of UERJ's peer and alumni networks in the job outcomes of its students. Students may learn about jobs through word of mouth on campus or referrals from fellow alumni (Calvo-Armengol and Jackson, 2004). They may have an advantage in hiring if alumni are involved in candidate screening. Alumni may encourage firms to recruit from their school. Even if the alumni are

[9] 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.

[10] 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 B1–B3.

no longer working at the firm, their past performance can influence employers' expectations on the productivity of a graduate from the same program (MacLeod and Urquiola, 2015).

We use our employer-employee data to define two types of dependent variables that reflect these network mechanisms. In both cases, we define networks at the school × major level; our variables indicate when an applicant to a particular UERJ major ended up at a job that is affiliated with a graduate from that same program. We use major-specific networks because students in the same program take many classes together and often seek employment in similar labor markets. 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.

Our first network outcome variable measures employment at *alumni firms*. This variable indicates whether an applicant to a UERJ program got a job at a firm that ever hired another graduate from that program.[11] 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 ever employed another individual j who *graduated* from major m (the "alum").[12] We also define versions of this variable that identify firms with a high concentration of UERJ alumni.

Our second network variable measures *co-employment* with a UERJ program alum. This variable differs from our alumni firm measure in that it requires the applicant and the alum to be employed at the firm *in the same year*. We define applicant i to major m as co-employed if they worked in the same firm/year pair as another individual j who graduated from major m. Co-employment is less likely to reflect reputation mechanisms than alumni firm employment, and it is more likely to be driven by peer connections. To explore the role of peer ties formed while individuals were in school, we consider co-employment with alumni from both the applicant's own cohort and from different cohorts.

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. Both channels are evidence of network effects. It is hard to isolate either channel, but we present additional results in which we require that the alum worked at the firm before the applicant. We also do not make any restrictions on the years in which the applicant or alum graduated from UERJ. This allows us to capture network effects that arise from internships or part-time jobs while individuals were in still school.

We use these measures as dependent variables in our RD and DD analyses to ask whether attending UERJ had a causal effect on access to jobs affiliated with its alumni network.

[11] Throughout the paper, we define firms at the *establishment* level (organization × branch) using the 14-digit CNPJ codes in the RAIS (as in Gerard et al., 2021). See Appendix B.1 for details.

[12] All of our network outcome variables are leave-individual-out; even if an individual earned a UERJ degree, these variables equal one only if there is *another* alum affiliated with that firm.

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 a linear spline in 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 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 A2–A4 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.[13]

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 pre-AA and post-AA applicants in the general track.

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 by admitting applicants with the next-highest scores in each track. There can be up to five rounds of waitlist admissions, and in some cases final admits receive offers just before the

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

start of the Fall semester.[14] 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.

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 distribution of admission scores using a 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 (see below).

We also find no evidence of covariate imbalance for general track applicants, but the McCrary test shows evidence of a decrease in the density of admission scores at the threshold. 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 A2–A4).

We also make the standard instrumental variable and LATE assumptions (Angrist et al., 1996). Instrument relevance is satisfied because the probability of UERJ enrollment increases sharply at the admission threshold. The exclusion restriction requires that our instrument affects applicants' 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 rules out the possibility that applicants 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 a population of marginally-admitted "compliers," which are students who would have enrolled if and only if they scored above the cutoff.

[14] Most UERJ programs have cohorts that begin in both the Spring and Fall semesters. Applicants can list a preference for one semester, but they are admitted to begin in the other semester if their preferred option has reached capacity. See Appendix B.4 for details on UERJ's admission process.

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.

We begin by examining individuals' graduation rates and early-career jobs. Most UERJ students who graduate do so in 4–6 years (Appendix Figure A3), so we examine outcomes 6–9 years after individuals' applied to UERJ to capture their initial jobs after (potential) graduation.[15] Table 2 presents effects on enrollment in UERJ (our first stage), graduation, and employment at firms affiliated with UERJ alumni. Table 3 shows effects on early-career earnings and other job characteristics. We display RD graphs for several of these outcomes in Figure 3; 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. Table 4 shows how admission to UERJ affected applicants' college selectivity and educational attainment.

Lastly, we examine how the returns to attending UERJ changed as individuals' careers progressed. Table 5 shows employment and earnings effects measured 10–13 years after application. We plot the time profile of the RD earnings coefficients in Figure 4.

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

3.2. **Graduation and early-career jobs.** 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 for Black/public school applicants because most other Rio de Janeiro universities 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 (Section 3.4).

Our first finding is that marginal enrollees from the affirmative action tracks graduated at nearly the same rate as 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. By this time, 64 percent of marginal enrollees in the Black and public school tracks had graduated, as compared with 68–71 percent of marginal enrollees in the general tracks. The similarity of

[15] All of our RD regressions include one observation per applicant. For earnings outcomes, we use the applicant's mean real earnings over the period of 6–9 years after application. For binary outcomes, we use the maximum over this period, so our estimates reflect ever having a job with those characteristics.

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).

Enrolling in UERJ significantly increased the likelihood that affirmative action students worked at firms affiliated with its alumni network. In Panel C of Table 2, our dependent variables are indicators for employment at *alumni firms*, i.e., firms that hired UERJ graduates from the program the applicant applied to (see Section 1.5). For Black and public school students, UERJ enrollment caused a 14 percentage point increase in the rate of employment at firms that ever hired any other alum. We find similar results when we define our dependent variable only using firms that hired an alum before the applicant (second row of Panel C).

The employment effects are relatively larger for firms with a high concentration of UERJ alumni. In the other rows of Panel C, we define alumni firms as those that employed at least n alumni from a given UERJ major per 1000 total employees, where n ranges from 1–25.[16] We find significant effects in all firm categories, but the RD coefficients increase with concentration as a percentage of the mean employment rate for marginally-rejected applicants (column E). For affirmative action students, enrolling in UERJ more than doubled the likelihood of employment at firms with 25 alumni per 1000 workers.

Affirmative action applicants were also more likely to be co-employed with UERJ alumni when they enrolled. Panel D of Table 2 uses our co-employment outcomes (see Section 1.5); these differ from our alumni firm variables in that they require the applicant and alum to be at the firm in the same year. UERJ enrollment increased affirmative action students' likelihood of co-employment by 14 percentage points. We find similar effects on co-employment with alumni from both the applicant's own and other cohorts. The same-cohort estimates are larger relative to the below-threshold means, suggesting that peer ties formed during college are a mechanism for the employment effects. Black and public school enrollees were slightly more likely to be co-employed with other affirmative action alumni than with general track alumni, although we find large causal effects on both outcomes (Appendix Table A5).

Appendix Table A9 provides examples of firms with the largest number and highest concentration of UERJ alumni. Firms that hired the most UERJ graduates 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

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

Development Bank, and branches of the multinational petroleum company Petrobras. These firms paid a significant wage premium over firms that hired other applicants in our sample. On average, mean wages were 55 percent higher at firms with UERJ alumni, and the wage premium was mostly increasing in alumni concentration (see Appendix Table A10).

We also find large effects on access to alumni network jobs for students who enrolled through the general tracks (columns B and D in Table 2). Overall the point estimates tend to be slightly smaller than those for affirmative action applicants, but the general track effects are larger for employment in firms with a high concentration of alumni.

3.3. **Early-career earnings.** Affirmative action students experienced an increase in early-career earnings from enrolling in UERJ. Panel A of Table 3 shows 2SLS estimates on log hourly wages and mean monthly earnings measured 6–9 years after application. Our estimates show that UERJ enrollment caused a 14 percent increase in the mean hourly wages of marginal Black and public school admits. The gain in mean 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 effect is roughly one-fifth of the earnings gap between marginally-rejected applicants in the general and affirmative action tracks (\$1,391 vs. \$817). Thus Black and public school enrollees still had significantly lower earnings than their classmates from the general track.

This earnings gain was driven primarily by access to firms with higher mean wages. Panel B of Table 3 shows how UERJ enrollment affected four different characteristics of applicants' early-career jobs: firm, occupation, industry, and municipality. In each case we define the dependent variable as the leave-out mean log hourly wage of all individuals in our data with that job characteristic. The 2SLS coefficients in column (F) show that UERJ enrollment caused affirmative action applicants to work at firms that paid 11 percent higher wages on average ($p < 0.10$). The magnitude of this effect is 80 percent of the individual wage coefficient in Panel A. We find smaller and insignificant effects on occupation and industry wage indices, and no effect on municipality mean wage.

Taken together, Tables 2–3 suggest that the earnings gain for affirmative action students was driven by increased access to firms in UERJ's alumni network. We find strong effects on employment in high-paying firms with UERJ alumni, and at most modest effects on other job characteristics. Non-network mechanisms could still explain this finding; for example, UERJ may teach a specific type of human capital, and its alumni may be concentrated at firms that value those skills. But it is striking that the presence of alumni at a firm is the strongest predictor of differences in the jobs obtained by marginal admits and marginal rejects. Further, human capital mechanisms are less likely to explain why effects are relatively larger for co-employment with same-cohort peers. This suggests that network mechanisms played an important role in the earnings gains for Black and public school students.

For general track applicants, we find no effect of UERJ enrollment on earnings in the pre-AA cohorts, and some evidence of a *negative* return in the post-AA cohorts (columns B and D in Table 3). For general applicants in the 2004–2011 cohorts, the 2SLS estimates are -0.08 for log hourly wages ($p = 0.10$) and $-\$153$ for monthly earnings ($p = 0.03$). We also find a negative effect of UERJ enrollment on firm mean wages. The decrease in these returns from the pre- to the post-AA cohorts is suggestive of a spillover effect of affirmative action. But our RD analysis is not conclusive on this mechanism because affirmative action also changed the location of the admission cutoffs in the distributions of applicant ability.

3.4. **College selectivity and educational attainment.** To provide context for the above results, we examine how admission to UERJ affected the selectivity of individuals' colleges and their overall educational attainment. While UERJ is an elite school in Brazil, 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. Among these institutions, selectivity is strongly related to financial resources (see Appendix Table A8), which may affect the likelihood that students complete a college degree.

We examine effects on college selectivity 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 that have the same birthdate, gender, and enrollment year as the UERJ applicant.[17] 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.

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 4 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

[17] We match *enrollment year* in the census to *application year* in the UERJ data, which means that we do not capture the behavior of students who reapply to college in a later year. As long as birthdate and gender vary smoothly across the threshold, discontinuities in these total variables are driven by the enrollment outcomes of UERJ applicants. See Appendix B.5 for details on our merge with the higher education census.

estimate of 0.69 in Table 2. We do not find significant 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 private universities in Rio falls by roughly 0.5 at the affirmative action thresholds. These schools are mostly open enrollment, and they have significantly lower spending per student than UERJ. Our estimates are noisy, and we cannot rule out small effects on enrollment in any selectivity category. But the findings match our prior that many Black and public school applicants would not have gained admission to other top universities during this time period.

Despite the increase in selectivity, affirmative action students were no more likely to earn a college or postgraduate degree when they enrolled in UERJ. In Panel B of Table 4, 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.[18] 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 most developed countries. This suggests that most affirmative action students were likely to succeed academically regardless of where they enrolled.

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 4). Thus general track "compliers" would likely have attended other selective universities if they had been rejected by UERJ. This may explain why general applicants did not experience a gain in earnings from attending UERJ (Table 3). In particular, we hypothesize that marginally-rejected general applicants benefited from alumni networks at these other top universities.

3.5. **Later-career jobs and earnings.** Finally, we examine how individuals' jobs and earnings evolved over the next four-year period of their careers. Table 5 shows effects of UERJ enrollment on earnings and employment in alumni network jobs measured 10–13 years after application; all variables are otherwise identical to those in Tables 2–3.[19] Columns (A)–(C) display 2SLS RD coefficients for each applicant group, and columns (D)–(F) show the *change* in the RD coefficients between the periods of 6–9 and 10–13 years later. Figure 4 plots three-year rolling averages of the 2SLS RD estimates for log hourly wages. Panel A shows how these coefficients vary with years since application in a sample for which we observe outcomes in each year (the 1997–2006 cohorts). Panel B shows how returns varied across cohorts using wages measured at a fixed distance from application (6–9 years later).

[18] 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).

[19] Regressions in Table 5 do not include the 2010–2011 cohorts since 2019 is our last year of RAIS data.

We find that the initial earnings gain for affirmative action students faded as their careers progressed. For Black and public school students, the hourly wage return to UERJ enrollment is only two percent measured 10–13 years later (column C in Table 5). This is a decline of 0.11 log points from the early-career wage effect, and the difference in these RD coefficients is significant at $p < 0.05$ (column F). The RD estimate for monthly earnings is 50 percent smaller, although this decrease is not statistically significant. Panel A of Figure 4 shows that the wage return for affirmative action students was above 20 percent measured six years after application, and it faded to near zero by 13 years later. The wage gains were also largest in the first cohort with affirmative action, and decreased significantly by the 2011 cohort (Panel B of Figure 4).

The fade out of the earnings return was partly driven by a declining influence of UERJ's alumni network. Panels B–C of Table 5 show effects of UERJ enrollment on alumni firm employment and co-employment measured 10–13 years later. These estimates are still positive for Black and public school students, but they are smaller than those measured in the early period. Column (F) shows that the magnitudes of these network effects declined by 30–50 percent for most outcomes. The effects on employment in alumni network jobs also declined for applicants in the general tracks (column D–E). This suggests that UERJ's alumni networks are most important for initial job placement, and that their influence decreases as individuals form new networks in the labor market.

3.6. **Discussion.** We conclude our RD analysis by discussing our results in the context of other research on affirmative action and college selectivity.

Our graduation results show that most affirmative action students succeeded academically at UERJ. This contrasts with research that finds that students who benefit from affirmative action have low graduation rates at selective colleges, particularly in STEM fields (Arcidiacono, 2004; Arcidiacono et al., 2016). Most programs in our RD sample are in non-STEM fields (Table 1), and graduation rates are high by Brazilian standards. Relative academic preparation may be less important for degree completion in these programs. Appendix Table A7 shows that affirmative action applicants had the largest earnings returns to health majors, which also had the highest mean graduation rate (79 percent). In UERJ's STEM majors, affirmative action students graduated at a much lower rate (42 percent), and the early-career wage return was close to zero.

Our results also 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 (2020) 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.[20] 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. In our setting, UERJ enrollment had no effect on the likelihood that affirmative action students earned a college degree. This may explain why we find smaller returns in the short run, and no longer-run earnings effects.[21]

Our results speak to the effects of affirmative action at universities whose value depends on job networks more than degree attainment. 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 increased disadvantaged students' access to high-paying firms. But individuals' networks change as they meet new people in their jobs, and Black and low-SES individuals likely faced other barriers to career progression 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.

A difference between our paper and Zimmerman's is that we find no earnings gain for general applicants, and some evidence of negative returns in the post-AA cohorts. The rest of our paper asks whether spillover effects from affirmative action contributed to this finding.

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 the minimum number of students who enrolled through the *general* track in any cohort in 1995–2011.[22] Our top enrollee sample is composed of the N_m students with the highest admission scores who enrolled in each cohort. This is a balanced panel at the major level that includes N_m enrollees from every cohort. This sample contains students whose admission scores were high enough such that they could have enrolled in their major regardless of

[20] 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 (2020) 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).

[21] We find some evidence that the early-career wage gain was larger for more disadvantaged applicants within the affirmative action tracks (Appendix Table A6). But in each of these subgroups, the wage return decreases over time and is statistically insignificant measured 10–13 years after application.

[22] In other words, we compute the number of general track enrollees in each major m and cohort c, N_{mc}, and then define $N_m = \min_{c\in\{1995,\ldots,2011\}} N_{mc}$.

whether UERJ used affirmative action. This is roughly the top half of the incoming class in most programs, as 55 percent of slots were reserved for general applicants.[23]

4.2. **Regression model.** 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.[24] To address this, we interact the cohort dummies, γ_c, with fixed effects for the four field of study groups, $f(m)$, defined in Table 1: health, humanities, natural sciences, and social sciences.[25] These interactions restrict our identification to comparisons between majors in the same field, which were more likely to be similarly affected by macroeconomic fluctuations. 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 spillover effects of affirmative action on untargeted students. In this case, the coefficient π measures how affirmative action changed top enrollees' outcomes in more-affected majors relative to less-affected majors. We also present DD coefficients estimated in the sample of all other enrollees. These estimates are harder to interpret because they reflect both the intended impacts of affirmative action on student body diversity and any spillover effects. But we report these estimates alongside our results for top enrollees to provide evidence on whether spillovers also affected the outcomes of students who were targeted by affirmative action.

Panel A of Table 6 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

[23] Our top enrollee sample includes affirmative action applicants whose exam scores were in the top N_m of their program/cohort. In practice, 93 percent of post-AA top enrollees were general applicants (Table 1).

[24] After a long period of growth, Brazil's real GDP declined by roughly seven percent during 2015–2016.

[25] These fields of study are UERJ's categorization of its faculty areas (https://www.uerj.br/ensino/cursos-de-graduacao/).

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 6–7 present our 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 non-top enrollees, and column (D) shows DD estimates for all enrollees pooled.

Our dependent variables reflect a variety of channels through which affirmative action could impact the outcomes of untargeted students. Table 6 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 6 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.

Table 7 examines effects on graduation and labor market outcomes. Affirmative action may impact the graduation rates of untargeted students through learning spillovers (Sacerdote, 2001) or changes in teaching (Duflo et al., 2011). Earnings effects could arise from many different mechanisms related to human capital accumulation or job access. We focus on the role of UERJ's peer and alumni networks, which became more racially and socioeconomically diverse as a result of affirmative action. We examine network effects using our alumni firm and co-employment outcome variables, defined similarly as in our RD analysis.

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 6 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).[26] 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

[26] We standardize scores to be mean 0/SD 1 in the population of all UERJ enrollees in a given cohort.

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. But the results in Table 6 show that compositional changes are unlikely to explain our DD estimates for top enrollees' labor market outcomes.

By contrast, affirmative action had a significant effect on the demographics and test scores of non-top enrollees (column C of Table 6). 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 7 shows that the mean hourly wage of top enrollees in the most-affected majors declined by 14 percent relative to those in 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 5 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.

The negative earnings effect for top enrollees was largely driven by a reduction in firm quality as measured by the average wage of employees. 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 5). We find no effect of exposure to affirmative action on the graduation rates of top enrollees (Panel A of Table 7), 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).

The effects on firm quality were partly due to a change in the jobs associated with UERJ's alumni network. In Panel C of Table 7, our dependent variables are indicators for employment at firms that hired UERJ alumni from the same program in the pre-AA cohorts.[27] For top enrollees, the likelihood of employment in these firms declined by 5.5 percentage points

[27] These variables are defined using the same leave-individual-out method described in Section 1.5, but we use only alumni from the 1995–2001 cohorts.

in more-affected majors relative to less-affected majors. Panel D shows that exposure to affirmative action reduced the likelihood of co-employment with alumni from the *same* cohort and with alumni from the *general* track. Each of these job characteristics is associated with higher wages (Appendix Table A10), suggesting that affirmative action reduced the value of jobs attained through UERJ's alumni networks. Consistent with this, we find *positive* effects on the rate of co-employment with alumni from the affirmative action tracks.

We find even larger declines in earnings for lower-ranked enrollees in majors with greater exposure to affirmative action (column C of Table 7). 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. Consistent with this, the DD estimates for employment in alumni network jobs are similar for top enrollees and other enrollees (Panels C–D).

The negative spillover effects persisted over the next four years of individuals' careers. Appendix Table A11 shows the same outcomes as in Table 7, but we measure them 10–13 years after application. For top enrollees, the DD estimates are -0.12 for log hourly wages and -0.11 for log firm average wage; these are similar to the estimates measured 6–9 years after application. We continue to find negative effects on employment at firms with pre-AA alumni and on co-employment with general track peers. These estimates are somewhat smaller in magnitude that the early-career estimates, consistent with the fade out of network effects in our RD results.

5.4. **Robustness checks.** Appendix Table A12 shows that our results for top enrollees are robust to specifications that check the validity of our parallel trends assumption. To test if our results are attributable to heterogeneous impacts of Brazil's mid-2010s recession, we estimate the regressions in column (B) of Table 7 using only pre-recession years (2003–2014). The DD estimate for log hourly wages changes only slightly from -0.132 to -0.118, and we find similar effects on other outcomes. Our results are also robust to including program-specific linear trends estimated in the pre-AA cohorts; this shows that our results are not driven by diverging trends in outcomes that predated affirmative action.

Our DD estimates for top enrollees are also robust to controls for student and program characteristics. Controlling for student demographics and entrance exam scores only slightly reduces the magnitude of the log hourly wage coefficient, consistent with the small compositional effects identified in Table 6. We continue to find effects on individual and firm mean wages when we compare programs in the same quartile of selectivity (x-axis in Figure 1), although the magnitudes are smaller because this reduces variation in affirmative action exposure. Lastly, our results are similar when we define ExposureToAA_m as each major's affirmative action share in the 2004–2011 cohorts, i.e., as the y-axis in Figure 1.

5.5. **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 top students. This is similar in magnitude to Arcidiacono and Vigdor (2010)'s estimate of spillover effects at selective US colleges; the authors find that a one percentage point increase in the share of minority students is associated with a 0.8 percent decrease in other students' earnings.[28] 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.

A likely mechanism for these spillovers was a decrease in the value of the jobs attained through UERJ's peer and alumni networks. Exposure to affirmative action reduced the rate of employment at firms that hired pre-AA graduates from the same program. Reputation or recruiting mechanisms may have led some of these firms to hire graduates from other universities. We find no effects on the overall rate of co-employment with UERJ alumni, but students in more-affected majors became less likely to work with a general track alum, and more likely to work with an affirmative action alum. This is not a mechanical effect, but it is also not surprising given the evidence throughout our paper that UERJ's alumni networks matter for its students' job outcomes. Since general track students typically obtained higher-paying jobs than affirmative action students, the value of these network connections declined more in majors with greater exposure to the policy.

In addition, the large scale of UERJ's affirmative action policy may have reduced the earnings of the students that it targeted. The average earnings of lower-scoring enrollees also fell in majors with greater exposure to the policy relative to less-affected majors. It is hard to separate composition and spillover effects in this sample, but the magnitude of the earnings decline was larger than the predicted decrease based on observable characteristics. Our RD results showed that affirmative action students also benefited from access to network jobs, so it is likely that their outcomes were also affected by the decline in the value of network connections. This hypothesis is also consistent with our finding that the early-career returns for affirmative action students were highest in the initial cohorts (Panel B of Figure 4). A smaller-scale affirmative action policy would have admitted fewer disadvantaged students, but it may have led to higher earnings for the students who gained admission.

6. Conclusion

This paper examined the direct and spillover effects of affirmative action at the State University of Rio de Janeiro (UERJ). Using a long panel of university and labor market data, we showed that a key benefit of enrolling in UERJ is access to high-paying jobs affiliated with

[28] Arcidiacono and Vigdor (2010) emphasize human capital spillovers. We cannot rule out this mechanism, but we find no effects of affirmative action exposure on graduation rates (Panel A of Table 7).

its alumni. Affirmative action extended this benefit to Black and low-income students, which increased their early-career earnings. But the effects on access to network jobs faded as their careers progressed, and the associated earnings gain faded as well. Further, the increase in student body diversity had negative spillover effects on the earnings of top UERJ students. These spillover effects were due in part to a reduction in the value of jobs associated with UERJ's alumni network, and they persisted up to 13 years after enrollment.

Our findings show that affirmative action may be less effective at reducing income disparities when the benefits of admission depend on a university's peer and alumni networks. Universities have a direct influence on their students' educational outcomes, and they can help them obtain internships and jobs upon graduation. But their influence lessens after students enter the labor market and begin to form new job networks. In Brazil, as in many countries, there is substantial racial and socioeconomic segregation across industries, firms, and occupations. Disadvantaged individuals likely face barriers to promotion and networking in their careers. The efficacy of affirmative action may be lower if the long-run value of an elite education depends on the evolution of job networks in a segregated labor market.

We do not interpret our results as evidence that affirmative action is ineffective in all settings. 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. In addition, our spillover results show why elite universities may be hesitant to unilaterally admit a large number of students from disadvantaged backgrounds (Arcidiacono et al., 2019), as this can reduce the value of their alumni networks. At UERJ, in fact, affirmative action came about through a state law rather than university initiative.

Our analysis suggests that other policies may be necessary to improve the efficacy of affirmative action at elite universities. At UERJ, affirmative action was successful at boosting the quality of disadvantaged students' early-career jobs. Policies that reduce discrimination in the labor market or promote career advancement for individuals from underrepresented groups may help make these gains persistent. Further, a significant increase in the diversity of students at *many* elite universities might lead to a meaningful reduction in segregation across high-paying firms. 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 spillover results show that the effects of large-scale admission reforms are hard to estimate from existing data, so we hope future research will shed light on their efficacy.

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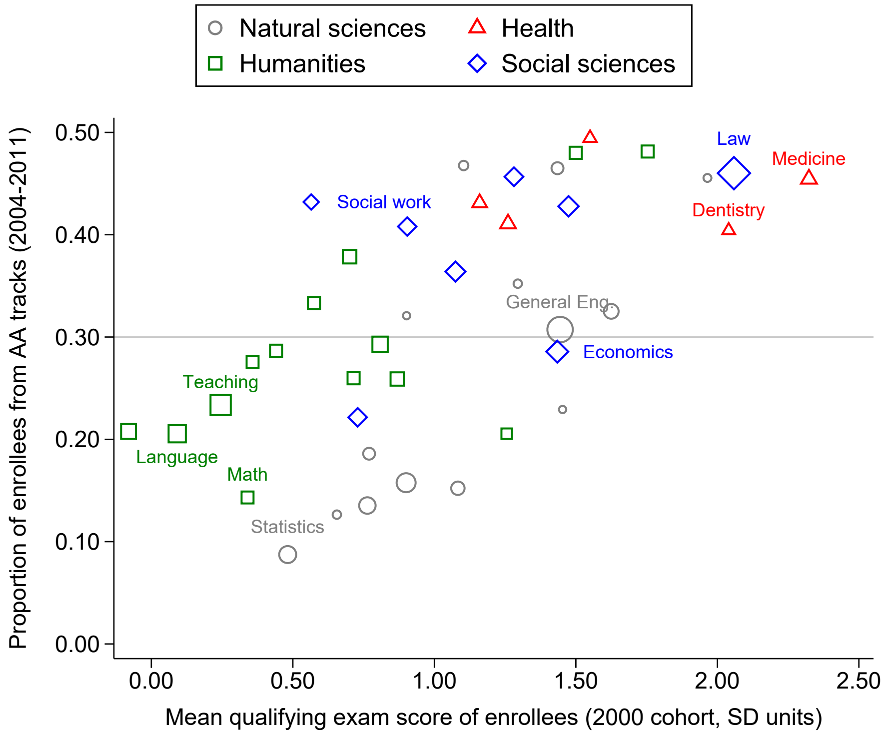


Figure 1. Take-up of affirmative action and program selectivity

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.

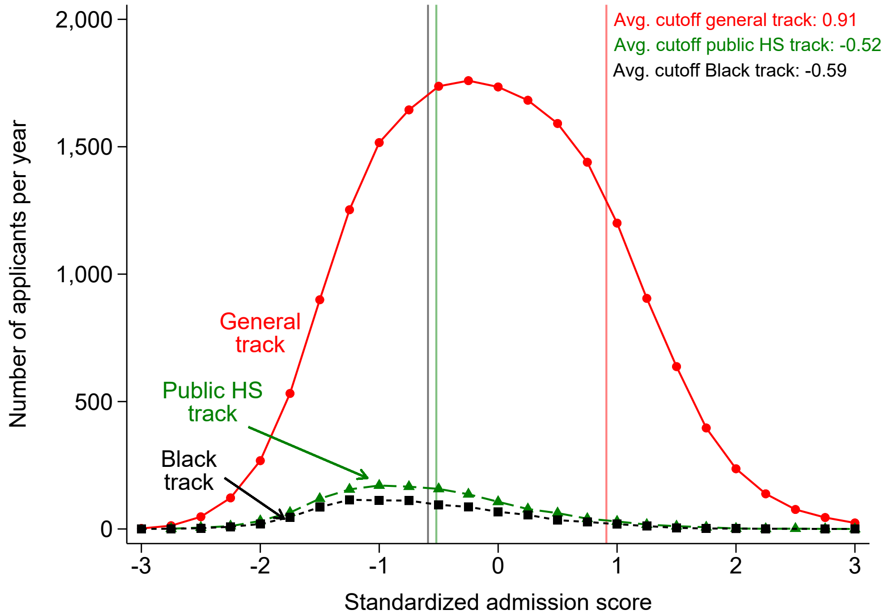
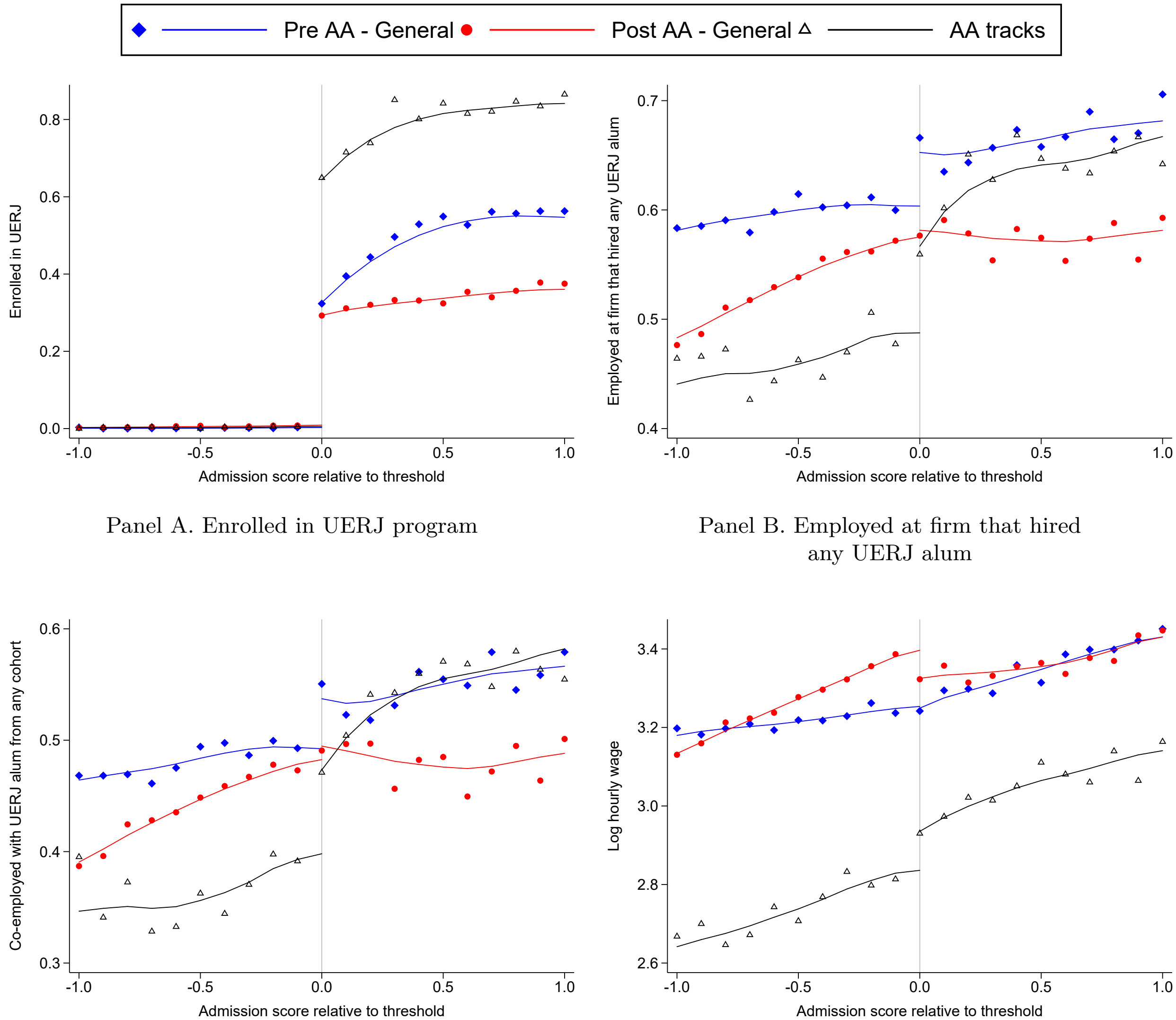


Figure 2. Admission score distribution and mean cutoff by application track (2004–2011)

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

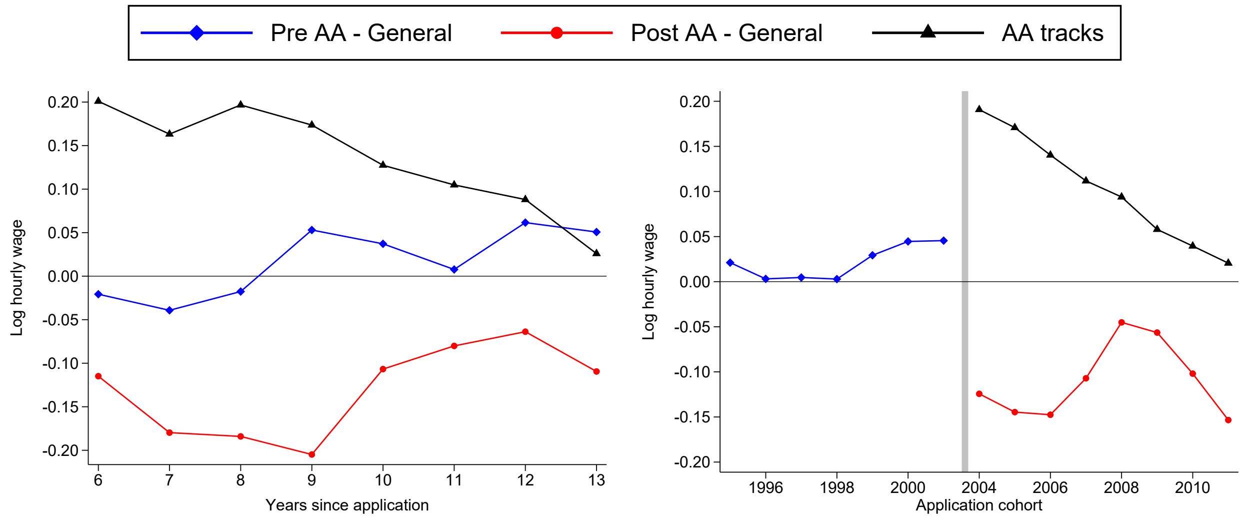
Panel B. Employed at firm that hired any UERJ alum

Panel C. Co-employed with UERJ alum from any cohort

Panel D. Log hourly wage

FIGURE 3. RD graphs for UERJ enrollment, employment, and earnings 6–9 years after application

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 B–D are measured 6–9 years after individuals applied to UERJ. 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. By years since application
(1997–2006 cohorts)

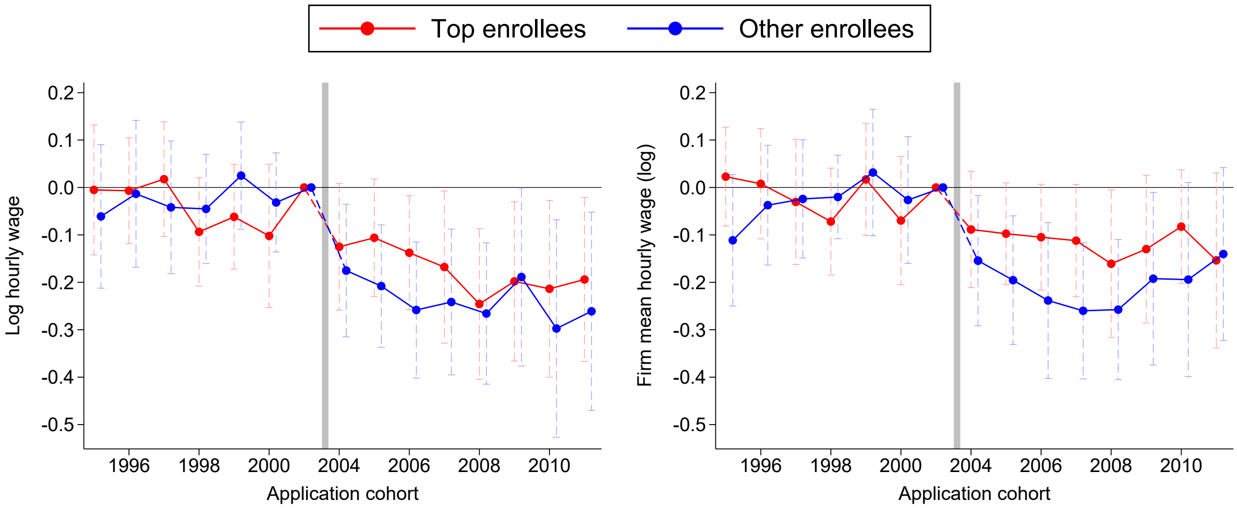
Panel B. By cohort
(Wages measured 6–9 years later)

Figure 4. 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. Log hourly wage

Panel B. Firm mean hourly wage (log)

FIGURE 5. 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).

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):

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: Accounting, Business Administration, 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):

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: Economics, 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.3 for details on variable definitions and the sample.

Table 2. RD estimates for graduation and employment 6–9 years after application

Dependent variable	(A) 1995–2001 general track Mean below	(B) 1995–2001 general track RD coef	(C) 2004–2011 general track Mean below	(D) 2004–2011 general track RD coef	(E) 2004–2011 AA tracks Mean below	(F) 2004–2011 AA tracks 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 formal employment (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)
N (formal employment)	3,234	37,794	4,012	55,030	543	8,147
Panel C. Employment at firms that hired UERJ alumni (2SLS)						
Firm w/ any alum	0.600	0.115^{***}	0.572	0.070^{**}	0.477	0.137^{***}
		(0.033)		(0.034)		(0.038)
Firm w/ alum hired before applicant	0.401	0.042	0.490	0.064^{*}	0.399	0.112^{***}
		(0.031)		(0.034)		(0.032)
Firm w/ 1 alum per 1000 workers	0.467	0.129^{***}	0.476	0.052	0.348	0.164^{***}
		(0.035)		(0.035)		(0.039)
Firm w/ 5 alumni per 1000 workers	0.265	0.099^{***}	0.306	0.119^{***}	0.179	0.104^{***}
		(0.028)		(0.036)		(0.033)
Firm w/ 10 alumni per 1000 workers	0.210	0.099^{***}	0.215	0.098^{***}	0.121	0.064^{**}
		(0.024)		(0.031)		(0.029)
Firm w/ 25 alumni per 1000 workers	0.092	0.065^{***}	0.084	0.086^{***}	0.035	0.049^{***}
		(0.019)		(0.024)		(0.018)
N (any alum)	2,029	25,486	2,698	34,127	396	5,060
Panel D. Co-employment with UERJ alumni (2SLS)						
Alum from any cohort	0.493	0.104^{***}	0.473	0.092^{***}	0.391	0.135^{***}
		(0.036)		(0.035)		(0.038)
Alum from same cohort	0.278	0.062^{**}	0.219	0.099^{***}	0.205	0.091^{***}
		(0.031)		(0.027)		(0.027)
Alum from different cohort	0.474	0.081^{**}	0.461	0.083^{**}	0.391	0.104^{***}
		(0.035)		(0.035)		(0.037)
N (any cohort)	2,029	23,565	2,698	32,909	396	5,009

Notes: This table presents RD estimates for UERJ graduation and employment 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. In Panel A, columns (B), (D), and (F) show reduced-form RD coefficients, θ, from equation (1). In Panels B–D, these columns show 2SLS RD coefficients, β, from equation (2). 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 earnings and job characteristics 6–9 years after application

	(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. Earnings (2SLS)						
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 (log hourly wage)	2,027	24,564	2,694	32,972	394	6,100
Panel B. Wage indices for job characteristics (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

Notes: This table presents RD estimates for earnings and job characteristics 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). In Panel B, the dependent variables are leave-individual-out mean log hourly wages at the firm, 4-digit occupation, 4-digit industry, and municipality level (see Appendix B.1 for details). 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 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 Janerio 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)
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. 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). In Panel B, these columns show 2SLS RD coefficients, β, from equation (2).

In Panel A, the dependent variables are the *total* number of enrollees in a given group of universities who share the applicant's birthdate, gender, and enrollment year. We measure these totals in Brazil's higher education census (*Censo da Educação Superior*); see Section 3.4 and Appendix B.5 for details. We categorize universities into four groups based on ownership and selectivity:

- UFRJ (*Universidade Federal do Rio de Janeiro*), the federal university in the municipality of Rio de Janeiro;
- The three other federal universities in the suburbs of Rio de Janeiro: UFF, UFRRJ, and UNIRIO;
- Private universities in the municipality of Rio de Janeiro that ranked in the top 100 of the 2012 *Folha* national ranking: PUC-Rio, and UNESA;
- Other private universities in the municipality of Rio de Janeiro: UGF, UVA, UCAM, Universo, and UCB.

These regressions include only 2009–2011 UERJ applicants. We include gender and age dummies to increase precision.

In Panel B, the dependent variables are indicators for educational attainment measured in the RAIS data.

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. RD estimates for employment and earnings 10–13 years after application

	(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)						
Firm w/ any alum	0.052	0.025	0.080^{*}	-0.063^{*}	−0.046	−0.057
	(0.036)	(0.038)	(0.044)	(0.034)	(0.036)	(0.042)
Firm w/ 1 alum per 1000 workers	0.096^{***}	0.028	0.107^{***}	−0.033	−0.024	−0.057
	(0.033)	(0.037)	(0.039)	(0.032)	(0.036)	(0.041)
Firm w/ 5 alumni per 1000 workers	0.105^{***}	0.076^{**}	0.037	0.006	−0.043	-0.068^{*}
	(0.029)	(0.034)	(0.038)	(0.027)	(0.036)	(0.036)
Firm w/ 10 alumni per 1000 workers	0.066^{***}	0.059^{*}	−0.008	−0.034	−0.040	-0.073^{**}
	(0.024)	(0.032)	(0.031)	(0.024)	(0.032)	(0.031)
Firm w/ 25 alumni per 1000 workers	0.027	0.031	0.013	-0.038^{*}	-0.055^{**}	-0.037^{*}
	(0.020)	(0.023)	(0.019)	(0.020)	(0.024)	(0.019)
N (any alum)	23,322	27,571	4,179	48,808	61,698	9,239
Panel C. Co-employment with UERJ alumni (2SLS)						
Alum from any cohort	0.052	0.048	0.092^{**}	−0.052	−0.044	−0.043
	(0.033)	(0.035)	(0.039)	(0.035)	(0.038)	(0.040)
Alum from same cohort	0.030	0.074^{**}	0.068^{*}	−0.031	−0.025	−0.023
	(0.030)	(0.030)	(0.039)	(0.029)	(0.030)	(0.035)
Alum from different cohort	0.054^{*}	0.033	0.066^{*}	−0.027	−0.050	−0.038
	(0.032)	(0.034)	(0.039)	(0.034)	(0.037)	(0.040)
N (any cohort)	27,492	31,289	4,718	51,057	64,198	9,727

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 6. DD estimates for 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. AA students in program/cohort	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 7. 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 that hired pre-AA UERJ alumni				
Firm w/ any pre-AA alum	0.602	-0.055^{**}	-0.044	-0.054^{**}
		(0.023)	(0.033)	(0.026)
Firm w/ 1 pre-AA alum per 1000 workers	0.419	-0.050	-0.067^{*}	-0.061^{*}
		(0.030)	(0.036)	(0.031)
Firm w/ 5 pre-AA alumni per 1000 workers	0.226	-0.066^{**}	-0.057	-0.063^{**}
		(0.032)	(0.035)	(0.031)
Firm w/ 10 pre-AA alumni per 1000 workers	0.160	-0.051	-0.035	-0.044
		(0.033)	(0.029)	(0.029)
Panel D. Co-employment with UERJ alumni				
Any alum	0.585	-0.017	0.019	-0.006
		(0.020)	(0.029)	(0.021)
Alum from same cohort	0.384	-0.055^{***}	-0.020	-0.039^{*}
		(0.019)	(0.026)	(0.020)
Alum from general track	0.577	-0.036^{*}	-0.006	-0.029
		(0.019)	(0.029)	(0.020)
Alum from AA track	0.215	0.060^{*}	0.088^{***}	0.071^{**}
		(0.035)	(0.028)	(0.029)
N (enrollees)	16,466	35,866	30,854	66,720
N (wage observations)	12,062	26,445	22,975	49,420

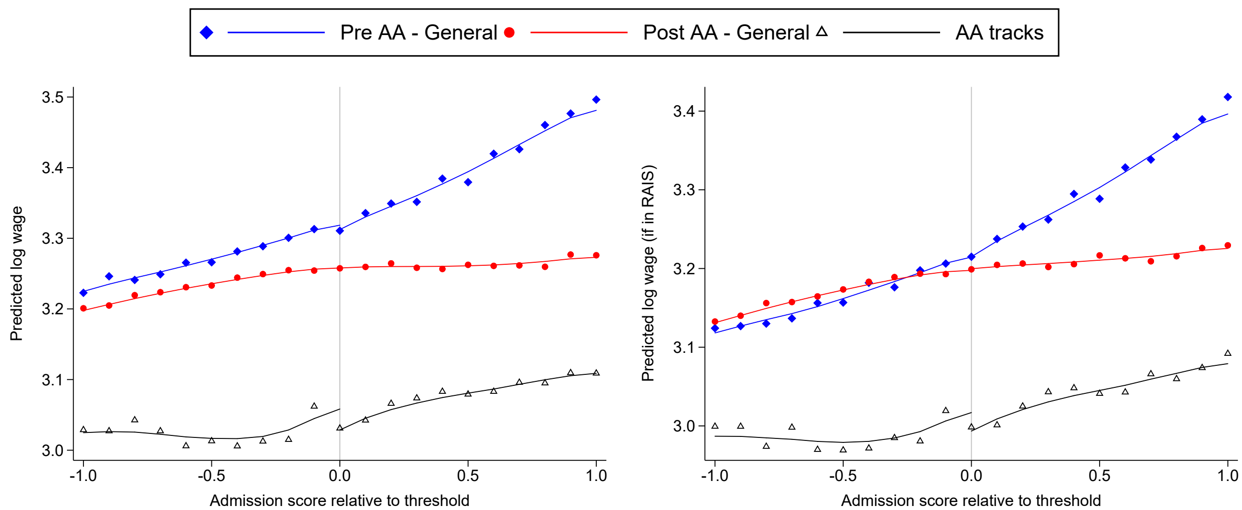
Notes: This table displays DD estimates of the effect of affirmative action exposure on graduation, employment, and earnings measured 6–9 years after application. Column (A) shows the mean of each dependent variable for top enrollees in the 1995–2001 cohorts. Columns (B)–(D) display estimates of π from equation (3) for top enrollees, other enrollees, and all enrollees. The dependent variables are defined similarly to those in Tables 2–3. In Panel C, we define alumni firms using only graduates from the pre-AA cohorts (1995–2001).

Parentheses contain standard errors clustered at the program level.

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

Appendix — For Online Publication

A. Appendix figures and tables

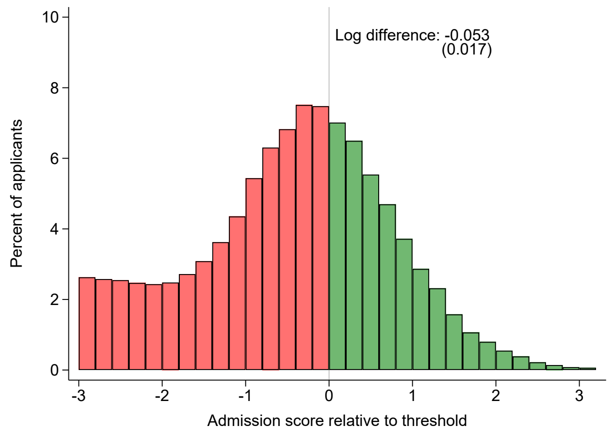


Panel A. All applicants

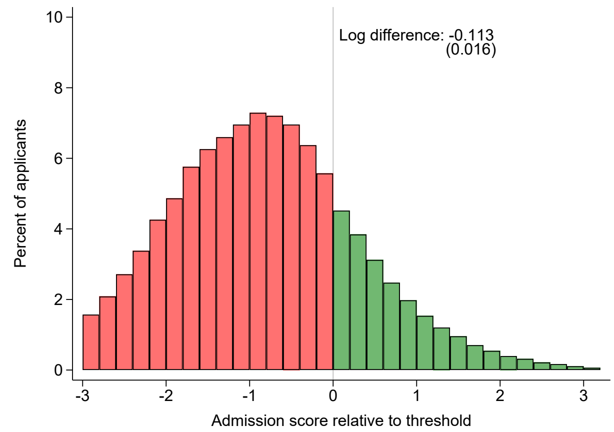
Panel B. Applicants who appear in the RAIS

Figure A1. Predicted log wage based on applicant characteristics

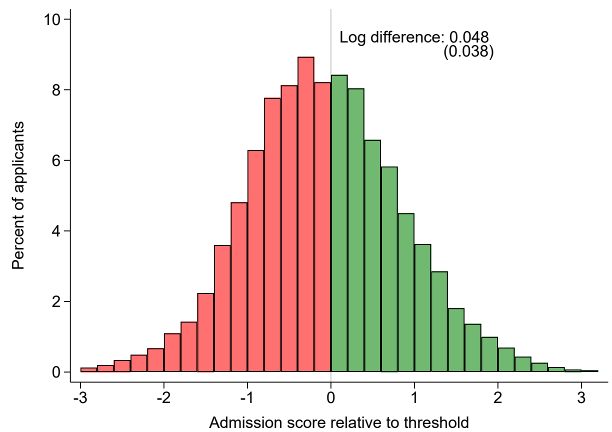
Notes: This figure presents RD graphs for pre-AA general applicants (blue diamonds), post-AA general applicants (red circles), and Black/public school applicants (black triangles). The x-axis in each panel is an applicant's standardized admission score normalized to zero at the cutoff. The dependent 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.

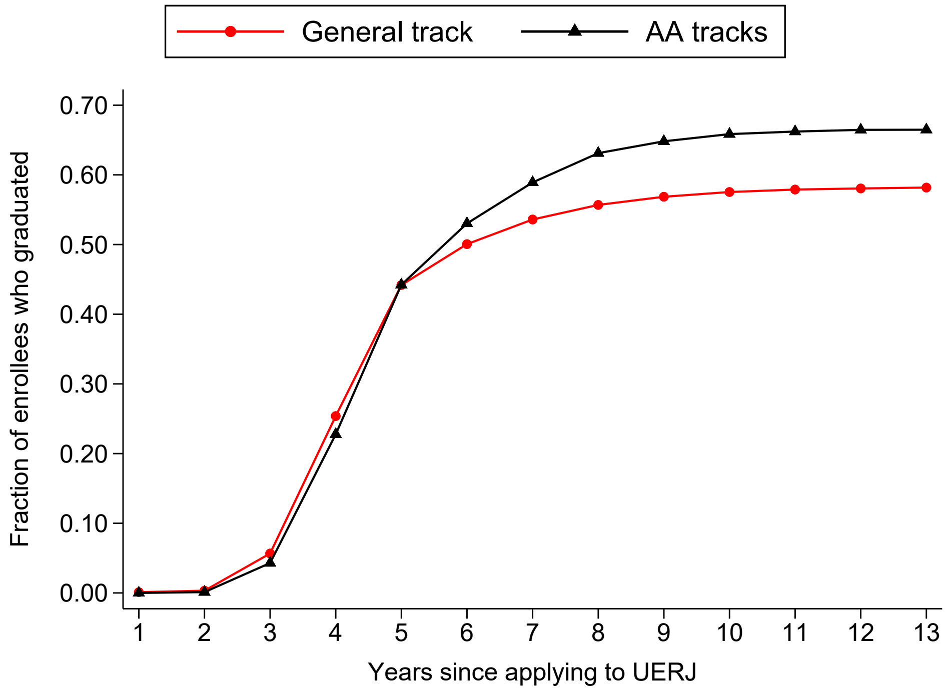


Figure A3. UERJ graduation rate by year since application

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).

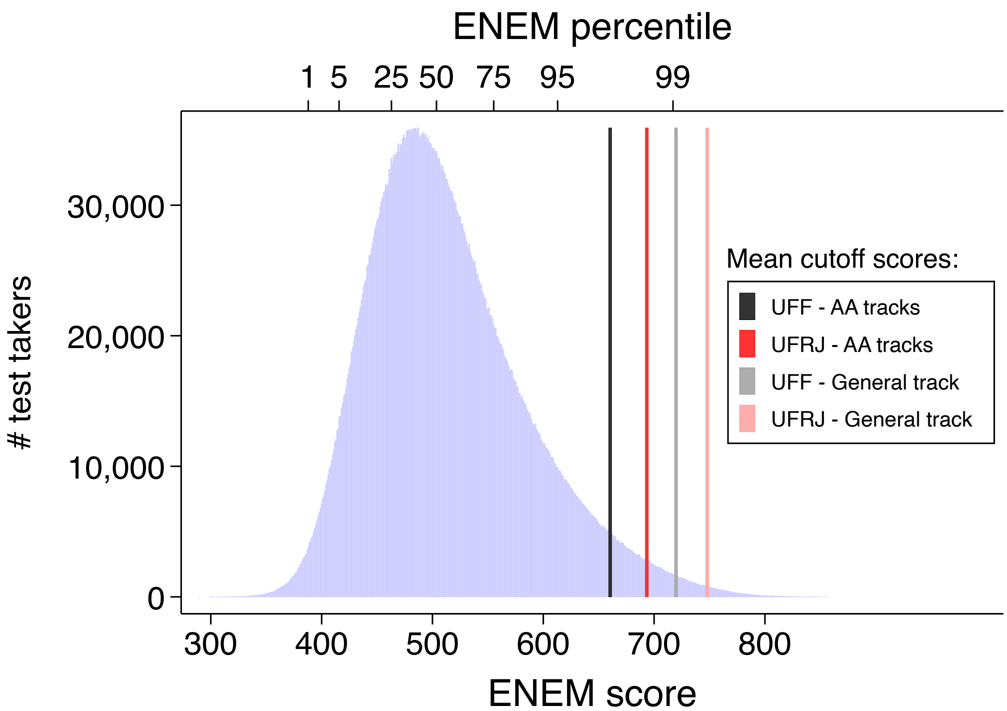


Figure A4. Mean ENEM cutoff scores at UFRJ and UFF (2016)

Notes: Light blue bars show the distribution of ENEM scores from the 2016 test administration. These are each test taker's average score across the five subjects (math, language, natural science, social science, and writing) rounded to the nearest integer. We include only test takers who earned positive scores on all five subjects. The lower x-axis is in ENEM scale score units; the upper x-axis is in percentile units. Data are from the ENEM microdata provided by INEP (*Instituto Nacional de Estudos e Pesquisas Educacionais AnÃsio Teixeira*), available at: https://www.gov.br/inep.

Vertical lines depict mean cutoff scores for Federal University of Rio de Janeiro (UFRJ) and Fluminense Federal University (UFF) from the 2016 administration of the SISU centralized admission system. These are the average cutoff scores across all bachelor's degree programs at each university. Darker lines depict averages across all affirmative action tracks. Lighter lines depict averages for the general track. Data were obtained from SISU (*Sistema de Seleção Unificada*) via a public information request.

Table A1. RD balance tests

Dependent variable (cohorts observed)	(A) 1995–2001 general track Mean below	(B) 1995–2001 general track RD coef	(C) 2004–2011 general track Mean below	(D) 2004–2011 general track RD coef	(E) 2004–2011 AA tracks Mean below	(F) 2004–2011 AA tracks RD coef
Panel A. Applicant characteristics						
Female			0.530	0.001	0.602	0.001
(2004–2011 cohorts)				(0.008)		(0.021)
Female (measured in RAIS)	0.486	0.008	0.518	0.012	0.600	0.001
(1995–2011 cohorts)		(0.010)		(0.009)		(0.023)
White			0.696	0.002	0.343	−0.022
(2007–2011 cohorts)				(0.010)		(0.028)
White (measured in RAIS)	0.790	0.011	0.714	0.003	0.436	−0.027
(1995–2011 cohorts)		(0.009)		(0.008)		(0.020)
Brown			0.212	−0.009	0.313	0.023
(2007–2011 cohorts)				(0.009)		(0.030)
Brown (measured in RAIS)	0.173	−0.008	0.216	−0.010	0.321	0.028
(1995–2011 cohorts)		(0.010)		(0.007)		(0.022)
Age at application	20.608	0.181**	20.043	0.037	22.306	−0.446*
(1995–2011 cohorts)		(0.081)		(0.072)		(0.262)
Mother has HS degree			0.901	−0.002	0.534	0.001
(2007–2011 cohorts)				(0.007)		(0.033)
HH income > 1.5x min. wage			0.886	−0.007	0.341	0.008
(2007–2011 cohorts)				(0.007)		(0.026)
Writing score (SD units)	0.174	0.020	0.477	0.011	−0.202	0.011
(1995–2001, 2007–2011 cohorts)		(0.015)		(0.016)		(0.052)
Qualifying exam score (SD units)	−0.148	−0.009				
(1995–2001 cohorts)		(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	3.313	−0.004	3.254	0.001	3.062	−0.003
(1995–2011 cohorts)		(0.006)		(0.003)		(0.010)
Predicted log wage (if in RAIS)	3.206	0.004	3.193	0.003	3.019	−0.006
(1995–2011 cohorts)		(0.006)		(0.004)		(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. 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 A3. 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 A2, but the coefficients are estimated on the sample of general track applicants in the post-AA cohorts (2004–2011). See notes to Table A2 for details.

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

Table A4. 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 A2, but the coefficients are estimated on the sample of Black/public school applicants in the post-AA cohorts (2004–2011). See notes to Table A2 for details.

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

Table A5. RD estimates for network employment (6–9 years after application) 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 UERJ alumni (2SLS)						
Firm w/ pre-AA general track alum	0.502	0.089^{***}	0.308	0.035	0.278	0.075^{**}
		(0.034)		(0.032)		(0.038)
Firm w/ post-AA general track alum	0.382	0.058^{*}	0.426	0.083^{**}	0.338	0.127^{***}
		(0.031)		(0.035)		(0.038)
Firm w/ AA track alum	0.358	0.042	0.394	0.073^{**}	0.356	0.129^{***}
		(0.030)		(0.033)		(0.037)
N (AA track)	2,029	28,526	2,698	36,705	396	5,073
Panel B. Co-employment with UERJ alumni (2SLS)						
Alum from pre-AA general track	0.462	0.104^{***}	0.352	0.033	0.288	0.078^{**}
		(0.035)		(0.033)		(0.033)
Alum from post-AA general track	0.187	−0.002	0.344	0.082^{**}	0.283	0.084^{**}
		(0.031)		(0.033)		(0.041)
Alum from AA track	0.171	−0.010	0.320	0.067^{**}	0.306	0.094^{***}
		(0.024)		(0.031)		(0.030)
N (AA track)	2,029	27,588	2,698	36,712	396	6,347

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 measure employment at alumni firms (Panel A) and co-employment (B). These variables are similar to those in Panels C–D of Table 2, 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 A6. Heterogeneity in RD estimates by student characteristics — Affirmative action tracks

Dependent variable	(A)	(B)	(C)	(D)	(E)	(F)
	2004–2011 cohorts				2007–2011 cohorts	
	Public school	Black	Males	Females	High SES	Low SES
Panel A. First stage						
Enrolled in UERJ program	0.668***	0.737***	0.688***	0.700***	0.780***	0.710***
	(0.018)	(0.020)	(0.022)	(0.017)	(0.023)	(0.029)
N	4,208	2,664	2,614	4,639	1,916	1,589
Panel B. Returns to UERJ enrollment 6–9 years after applying (2SLS)						
Graduated from UERJ program	0.648***	0.627***	0.574***	0.685***	0.674***	0.625***
	(0.023)	(0.026)	(0.031)	(0.021)	(0.027)	(0.039)
Formal employment	0.025	−0.031	0.012	−0.008	0.059	0.028
	(0.032)	(0.041)	(0.053)	(0.034)	(0.054)	(0.064)
Log hourly wage	0.077	0.153*	0.062	0.138**	0.053	0.091
	(0.060)	(0.080)	(0.091)	(0.059)	(0.094)	(0.113)
Monthly earnings (2019 USD)	109.677*	89.337	72.744	75.737	18.654	79.434
	(56.031)	(84.528)	(108.673)	(54.515)	(102.980)	(94.744)
N (employment regression)	5,313	3,038	2,298	4,799	1,912	1,651
N (wage regression)	3,499	1,765	1,827	3,367	1,225	1,119
Panel C. Returns to UERJ enrollment 10–13 years after applying (2SLS)						
Graduated from UERJ program	0.663***	0.655***	0.578***	0.734***	0.706***	0.667***
	(0.027)	(0.034)	(0.036)	(0.027)	(0.037)	(0.048)
Formal employment	0.040	0.015	0.093	−0.007	0.122	−0.010
	(0.047)	(0.057)	(0.066)	(0.048)	(0.086)	(0.100)
Log hourly wage	−0.015	0.066	0.031	0.058	0.054	−0.045
	(0.084)	(0.126)	(0.123)	(0.076)	(0.176)	(0.169)
Monthly earnings (2019 USD)	37.737	79.456	26.215	95.455	148.320	101.785
	(79.470)	(133.529)	(150.950)	(83.812)	(166.719)	(180.614)
N (employment regression)	3,107	1,765	1,617	2,919	841	789
N (wage regression)	2,149	977	1,207	2,507	390	499

Notes: This table displays RD coefficients estimated on the sample of Black/public school applicants. Column (A) shows results for public high school track applicants; column (B) for Black track applicants; column (C) for males; column (D) for females; column (E) for high-socioeconomic status applicants, defined as those whose mothers finished high school; and column (F) for low-socioeconomic status applicants, defined as those whose mothers did not finish high school.

Panel A displays the first-stage effect, θ, from equation (1). Panels B-C display 2SLS RD coefficients, β, from equation (2). In Panels B–C, the dependent variables are program completion, formal employment, and earnings, measured 6–9 years after applying (Panel B) and 10–13 years after applying (Panel C). 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 A7. Heterogeneity in RD estimates by field of study — Affirmative action tracks

	(A)	(B)	(C)	(D)
	Field of study			
Dependent variable	Health	Humanities	Natural sciences	Social Sciences
Panel A. First stage				
Enrolled in UERJ program	0.711***	0.723***	0.707***	0.650***
	(0.028)	(0.034)	(0.028)	(0.023)
N	1,512	1,025	1,374	2,550
Panel B. Returns to UERJ enrollment 6–9 years after applying (2SLS)				
Graduated from UERJ program	0.794***	0.525***	0.418***	0.714***
	(0.030)	(0.047)	(0.039)	(0.030)
Formal employment	−0.045	0.127*	−0.057	−0.023
	(0.062)	(0.070)	(0.064)	(0.051)
Log hourly wage	0.199*	0.116	0.005	0.093
	(0.102)	(0.121)	(0.110)	(0.087)
Monthly earnings (2019 USD)	211.265*	105.882	55.910	87.951
	(116.896)	(87.709)	(113.028)	(78.088)
N (employment regression)	1,512	1,025	1,374	2,550
N (wage regression)	1,098	798	1,026	1,869
Panel C. Returns to UERJ enrollment 10–13 years after applying (2SLS)				
Graduated from UERJ program	0.803***	0.557***	0.479***	0.718***
	(0.036)	(0.053)	(0.049)	(0.036)
Formal employment	−0.046	−0.016	0.122	0.038
	(0.073)	(0.086)	(0.080)	(0.065)
Log hourly wage	0.220*	−0.021	−0.073	−0.048
	(0.133)	(0.151)	(0.140)	(0.114)
Monthly earnings (2019 USD)	233.633	43.341	−139.594	63.304
	(158.928)	(160.279)	(163.390)	(127.643)
N (employment regression)	1,114	782	906	1,898
N (wage regression)	822	575	671	1,346

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 health programs; column (B) for humanities programs; column (C) for natural sciences programs, and column (D) for social sciences programs. See Table 1, Panel A, for the programs included in each field of study and Appendix Tables B1–B3 for the number of applicants by program/cohort.

Panel A displays the first-stage effect, θ, from equation (1). Panels B-C display 2SLS RD coefficients, β, from equation (2). In Panels B–C, the dependent variables are program completion, formal employment, and earnings, measured 6–9 years after applying (Panel B) and 10–13 years after applying (Panel C). 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. 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 4.

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 A9. 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 A10. OLS regressions on alumni firm and co-employment variables

Covariate	(A)	(B)	(C)	(D)	(E)	(F)
	Dependent variable: Log firm mean hourly wage			Dependent variable: Log individual hourly wage		
Panel A. Wage premia at alumni firms and co-employed jobs						
Any alumni firm	0.444			0.298		
	(0.004)			(0.004)		
Firm w/ 0−1 alumni per 1000 workers		0.303			0.230	
		(0.005)			(0.006)	
Firm w/ 1−5 alumni per 1000 workers		0.439			0.261	
		(0.005)			(0.005)	
Firm w/ 5−10 alumni per 1000 workers		0.488			0.416	
		(0.007)			(0.007)	
Firm w/ 10−25 alumni per 1000 workers		0.639			0.410	
		(0.006)			(0.006)	
Firm w/ 25−50 alumni per 1000 workers		0.588			0.393	
		(0.010)			(0.009)	
Firm w/ 50+ alumni per 1000 workers		0.401			0.210	
		(0.011)			(0.010)	
Co-employment with any alum			0.441			0.288
			(0.004)			(0.004)
N	549,675	549,675	549,675	554,242	554,242	554,242
Panel B. Wage premia by alum's cohort and application track						
Firm w/ pre-AA alumni only	0.479		0.470	0.340		0.333
	(0.007)		(0.007)	(0.007)		(0.007)
Firm w/ post-AA alumni only	0.272		0.241	0.244		0.213
	(0.005)		(0.006)	(0.005)		(0.005)
Firm w/ both pre- and post-AA alumni	0.479		0.425	0.276		0.227
	(0.005)		(0.006)	(0.004)		(0.006)
Co-employment with general track alum		0.417	0.179		0.290	0.139
		(0.006)	(0.007)		(0.006)	(0.007)
Co-employment with AA track alum		0.138	−0.101		0.100	−0.054
		(0.006)	(0.007)		(0.006)	(0.007)
Co-employment with alumni from both tracks		0.386	0.102		0.249	0.090
		(0.005)	(0.006)		(0.005)	(0.006)
N	549,675	549,675	549,675	554,242	554,242	554,242

Notes: This table shows OLS estimates of the wage premia for alumni firms and co-employment with UERJ alumni. In Panel A, the covariates are defined using any UERJ alum. In Panel B, we define covariates separately based on the alum's cohort and application track. The dependent variables are log firm mean hourly wage (columns A–C) and log individual hourly wage (columns D–F). The sample includes all UERJ applicants with an outcome for each year in 6–9 years after application. 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 A11. 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 that hired pre-AA UERJ alumni				
Firm w/ any pre-AA alum	0.595	−0.043	−0.023	−0.038
		(0.027)	(0.034)	(0.028)
Firm w/ 1 pre-AA alum per 1000 workers	0.388	−0.043	−0.044	−0.048
		(0.034)	(0.037)	(0.033)
Firm w/ 5 pre-AA alumni per 1000 workers	0.180	−0.041	−0.051	−0.047
		(0.033)	(0.031)	(0.030)
Firm w/ 10 pre-AA alumni per 1000 workers	0.113	−0.032	−0.027	−0.031
		(0.030)	(0.026)	(0.026)
Panel D. Co-employment with UERJ alumni				
Any alum	0.634	−0.014	0.023	−0.004
		(0.021)	(0.024)	(0.019)
Alum from same cohort	0.398	−0.059**	−0.004	−0.036
		(0.022)	(0.029)	(0.022)
Alum from general track	0.624	−0.031	0.004	−0.022
		(0.022)	(0.024)	(0.019)
Alum from AA track	0.314	0.045**	0.080***	0.057***
		(0.020)	(0.024)	(0.019)
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, formal employment, earnings, and access to UERJ alumni networks. The table structure and variables are the same as in Table 7, but outcomes are measured 10–13 years after application. Parentheses contain standard errors clustered at the program level.

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

Table A12. 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 that hired pre-AA UERJ alumni							
Firm w/ any pre-AA alum	−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)
Firm w/ 1 pre-AA alum per 1000 workers	−0.050	−0.054**	−0.069**	−0.050*	−0.029	−0.071**	−0.045
	(0.030)	(0.026)	(0.030)	(0.030)	(0.034)	(0.030)	(0.030)
Firm w/ 5 pre-AA alumni per 1000 workers	−0.066**	−0.072**	−0.061**	−0.067**	−0.034	−0.065**	−0.048
	(0.032)	(0.028)	(0.029)	(0.030)	(0.030)	(0.028)	(0.029)
Firm w/ 10 pre-AA alumni per 1000 workers	−0.051	−0.053*	−0.040	−0.052	−0.028	−0.048*	−0.030
	(0.033)	(0.029)	(0.028)	(0.031)	(0.025)	(0.028)	(0.028)
Panel D. Co-employment with UERJ alumni							
Alum from same cohort	−0.055***	−0.059***	−0.044*	−0.053**	−0.056***	−0.038	−0.048**
	(0.019)	(0.016)	(0.024)	(0.021)	(0.021)	(0.024)	(0.021)
Alum from general track	−0.036*	−0.043**	−0.023	−0.031	−0.044*	−0.011	−0.040*
	(0.019)	(0.017)	(0.020)	(0.020)	(0.024)	(0.021)	(0.020)
Alum from AA track	0.060*	0.035	0.096***	0.061*	−0.011	0.100***	0.075**
	(0.035)	(0.029)	(0.035)	(0.033)	(0.029)	(0.034)	(0.028)
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 7). 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.

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

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.
- **Co-employment with UERJ alumni.** We define UERJ applicant i to major m as co-employed if they worked in the same firm/year pair as another individual j who graduated from major m (the "alum"). We define different versions of this variable based on the characteristics of the applicant and/or alum, including their cohort and application track.
- **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).
- **Employment at alumni firms.** 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 and/or application track. 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.
- **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-digit 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.[29] 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

[29] 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.

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 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.[30] 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. **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 B1-B3 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 B1-B3 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

[30] 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.

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 B1-B3, 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 B1-B3.

Table B1. 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 B1. 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 B3 for details on the table structure and statistics.

Table B2. 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 B2. 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 B3 for details on the table structure and statistics.

Table B3. 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 B3. 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 $\text{ExposureToAA}_m = 1$ 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.

Table B4. 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

B.4. **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 if there are remaining open seats, and the last admission offers may occur as late as July. Appendix Table B4 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.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. 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. Thus our dependent variables contain additional measurement error.