The Direct and Spillover Effects of Large-Scale Affirmative Action at an Elite Brazilian University

Cecilia Machado, *Escola Brasileira de Economia e Finanças (FGV EPGE)*

Germán Reyes, *Middlebury College and Institute of Labor Economics (IZA)*

Evan Riehl, *Cornell University and National Bureau of Economic Research (NBER)*

We examine the effects of an affirmative action policy at an elite Brazilian university that reserved 45% of admission slots for Black and low-income students. We find that marginally admitted students who enrolled through the affirmative action tracks experienced a 14% increase in early-career earnings. But the adoption of affirmative action also caused a large decrease in earnings for the university's most highly ranked students. We present evidence that the negative spillover effects on highly ranked students' earnings were driven by both a reduction in human capital accumulation and a decline in the value of networking.

For helpful comments we thank Jim Berry, Sandra Black, Fernanda Estevan, Chao Fu, Laura Gee, Adam Kapor, Josh Kinsler, Michael Lovenheim, Tatiana Mocanu, Ana Ribeiro, Eduardo Pontual Ribeiro, Seth Sanders, Fabio Schanaider, Miguel Urquiola, Russell Weinstein, and participants at various seminars and conferences. We are grateful to Universidade do Estado do Rio de Janeiro (UERJ) for assistance with the data. Cecilia Machado acknowledges funding from the Coordenação de Aperfeiçoamento de Pessoal de Nível Superior–Brasil (CAPES)–Finance Code 001. The opinions expressed in this paper are those of the authors alone. Contact the corresponding author,

Submitted February 9, 2023; Accepted June 22, 2023; Electronically published February 11, 2025.

Journal of Labor Economics, volume 43, number 2, April 2025.
 https://doi.org/10.1086/726385

I. Introduction

Top universities face growing pressure to increase their students' racial and socioeconomic diversity. Chetty et al. (2020) argue that large-scale income-based affirmative action at selective colleges could significantly increase intergenerational mobility in the United States. Consistent with this, Bleemer (2022) finds that race-based affirmative action at the University of California increased underrepresented minority students' earnings. Bleemer (2022) argues that the policy improved allocative efficiency because displaced students were not worse off.

These arguments about affirmative action's benefits assume that selective universities can increase diversity without reducing their value added.[1] Yet the value of attending a top college may depend on its student body composition. At schools with high-achieving students, professors can teach courses at an advanced level (Duflo, Dupas, and Kremer 2011), and individuals may learn from peers with similar academic preparation (Carrell, Sacerdote, and West 2013; Arcidiacono, Aucejo, and Hotz 2016). Schools with wealthy student bodies provide access to peers and alumni in high-paying sectors of the economy (Zimmerman 2019; Michelman, Price, and Zimmerman 2022). Employers' recruiting and hiring decisions may depend on the expected ability of a school's students (MacLeod et al. 2017; Weinstein 2018). If these mechanisms are important, significantly increasing the scale of affirmative action can negatively affect all students' outcomes. There is little compelling evidence on the existence and magnitude of such spillovers because isolating variation in the composition of a college's student body is challenging.

We examine the direct and spillover effects of large-scale affirmative action at Rio de Janeiro State University (UERJ), one of Brazil's most prestigious universities. UERJ consistently ranks among the top 15 universities nationally. In some years, more than 100,000 students take UERJ's entrance exam, competing for roughly 5,000 admissions. Thus, UERJ's national prestige and selectivity are comparable to elite US private colleges.

UERJ was among the first Brazilian universities to adopt affirmative action. It did so on a large scale. Historically, white students from private high schools were disproportionately likely to gain admission through UERJ's entrance exam. Starting in 2004, UERJ reserved 45% of slots in each major for Black and public high school students from low-income families. This policy suddenly

Evan Riehl, at eriehl@cornell.edu. Information concerning access to the data used in this paper is available as supplemental material online.

[1] Chetty et al. (2020) write, "We also assume that [our] estimated causal effects do not change under our counterfactual student reallocations, in particular ignoring potential changes in value-added that may arise from having a different group of students (peer effects)" (1626). Similarly, Bleemer's (2022) claim that affirmative action improved allocative efficiency relies on the untested assumption that the policy did not reduce the returns of inframarginal white and Asian students.

and dramatically increased the racial and socioeconomic diversity of UERJ's students.

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 includes entrance exam scores and admission outcomes for all UERJ applicants in 1995–2001 (pre-AA) and 2004–11 (post-AA). We link these data to UERJ enrollment/graduation records and to Brazil's national employer-employee dataset for the years 2003–19.

Our analysis exploits variation in exposure to UERJ's affirmative action policy 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. In UERJ's most prestigious programs, affirmative action students made up 45% of the incoming class because the number of applicants typically exceeded the reserved quotas. The 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 10%–20% in some programs.

We use two empirical strategies to identify the effects of affirmative action on 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 admission score cutoffs (Hoekstra 2009; Kirkebøen, Leuven, and Mogstad 2016). Our RD design identifies the returns to attending UERJ for marginally admitted applicants in each track.

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

We have two main findings. First, for marginally admitted affirmative action students, enrolling in UERJ led to a 14% increase in early-career hourly wages. We find no effects of UERJ enrollment on college degree attainment, but affirmative action enrollees were significantly more likely to obtain jobs at high-paying firms affiliated with UERJ alumni. This suggests that their early-career earnings gains were primarily driven by networking mechanisms. We find that the earnings and networking benefits decreased as affirmative action students' careers progressed, but our later-career results are less powered.

Second, the adoption of affirmative action lowered the earnings of UERJ's highly ranked students. In our DD analysis, top enrollees' hourly wages decreased by 14% in majors with high affirmative action take-up relative to those with lower take-up. This effect persisted up through the end of our data range. We also find declines in earnings for highly ranked underrepresented minority students who could have gained admission to UERJ in the absence of affirmative action. We do not find significant changes in the characteristics and admission scores of top enrollees in more versus less affected majors, although point estimates suggest that compositional changes could play a small role in our results. Instead, we find evidence that the negative spillover effects on earnings were driven by both networking and learning mechanisms. The adoption of affirmative action reduced the likelihood that top enrollees obtained jobs at high-paying firms affiliated with UERJ peers and alumni, and it reduced the performance of UERJ's top students on a college exit exam.

Our findings show that elite universities face a trade-off between promoting upward mobility for disadvantaged students and maintaining sources of their value added that stem from admitting high-achieving and wealthy students. Furthermore, our results suggest that disadvantaged students with the highest admission scores may have been better off with a smaller-scale affirmative action policy.

Our early-career results for affirmative action students are consistent with other evidence that disadvantaged students benefit from attending selective universities. There is a large literature on university affirmative action, but there is limited evidence regarding its earnings impacts (Arcidiacono, Lovenheim, and Zhu 2015).[2] Bertrand, Hanna, and Mullainathan (2010) and Bleemer (2022) find earnings gains for disadvantaged students who were given admission preference at selective colleges in India and the United States. Similarly, Francis-Tan and Tannuri-Pianto (2018) find earnings benefits for male students admitted through reserved quotas at the University of Brasília. Related work finds earnings gains for low-income or minority students who were marginally admitted to US public university systems (Zimmerman 2014; Smith, Goodman, and Hurwitz 2020; Bleemer 2021). Our estimate of the early-career earnings return for affirmative action students—a 14% increase in earnings—is much smaller than analogous estimates from many of these papers.[3] This may be because affirmative action did not affected the educational attainment of UERJ applicants, whereas these papers often find effects on bachelor's degree attainment.

[2] Other research on affirmative action looks primarily at impacts on diversity or graduation rates (Cortes 2010; Backes 2012; Hinrichs 2012; Kapor 2015; Arcidiacono, Aucejo, and Hotz 2016; Bagde, Epple, and Taylor 2016). This is true of most work on affirmative action in Brazil (Francis and Tannuri-Pianto 2012; Ribeiro 2016; Estevan, Gall, and Morin 2019; Vieira and Arends-Kuenning 2019; Otero, Barahona, and Dobbin 2021; Ribeiro and Estevan 2021; Mello 2022).

[3] Zimmerman (2014) finds that admission to the Florida State University system increased the likelihood of enrolling by roughly 50%, and it increased earnings by 22%. Bleemer (2022) finds that an affirmative action ban decreased minority students' enrollment in selective University of California colleges by 8 percentage points, and earnings fell by 0.05 log points. These estimates imply returns to selective college enrollment of roughly 44%–87%.

Our findings are new in showing that affirmative action can benefit disadvantaged students through networking. Zimmerman (2019) and Michelman, Price, and Zimmerman (2022) find that networking is an important mechanism for the long-run earnings benefits of attending elite universities but that only students from advantaged backgrounds benefit from networking. Our data are unique in measuring early-career employment in a broad set of firms. Our results suggest that affirmative action students can also benefit from access to high-wage firms affiliated with alumni, at least early in their careers. We find similar effects for marginal enrollees from the general track, suggesting that networking is an important mechanism in research on the returns to college selectivity (e.g., Dale and Krueger 2002).[4]

Last, our paper is novel in identifying spillover effects of large-scale affirmative action. Several papers examine the efficiency effects of admission policies that benefit disadvantaged students by comparing earnings returns for students who were "pulled in" and "pushed out." The evidence is mixed; Bleemer (2022) finds efficiency gains, Bertrand, Hanna, and Mullainathan (2010) and Riehl (2024) find efficiency losses, and Black, Denning, and Rothstein (2023) find limited earnings effects in either group.[5] A full evaluation of the efficacy of affirmative action must also consider spillover effects on untargeted students (Durlauf 2008). Several papers examine how a university's racial or socioeconomic diversity affects other students' earnings (Daniel, Black, and Smith 2001; Arcidiacono and Vigdor 2010; Hinrichs 2011), but this work relies on strong selection-on-observables assumptions. We find negative earnings spillovers under weaker assumptions, and we present evidence on both learning and networking mechanisms. The existence of spillovers means that the true effects of large-scale admission reforms may differ from those estimated using existing student/college allocations, as in, for example, Chetty et al.'s (2020) "need-affirmative" counterfactual enrollment scenario.

II. Context and Data

A. UERJ and Higher Education in Brazil

Our setting is an elite public university in Brazil called Rio de Janeiro State University (Universidade do Estado do Rio de Janeiro). It is one of the oldest and most prestigious universities in Brazil; UERJ ranked eleventh nationally

[4] There is a large literature on the earnings returns to attending selective colleges and/or majors (Hoekstra 2009; Saavedra 2009; Hastings, Neilson, and Zimmerman 2013; Kirkebøen, Leuven, and Mogstad 2016; Canaan and Mouganie 2018; Hoxby 2018; Anelli 2020; Sekhri 2020; Ng and Riehl 2024). These papers typically cannot examine both job networks and earnings. We contribute to a small literature on network formation in college (Marmaros and Sacerdote 2002; Mayer and Puller 2008; Zhu 2025).

[5] A related literature examines student/college match effects in graduation and earnings outcomes (Andrews, Li, and Lovenheim 2016; Arcidiacono, Aucejo, and Hotz 2016; Dillon and Smith 2020; Mountjoy and Hickman 2020).

in a 2012 ranking by the newspaper *Folha*. UERJ is part of Brazil's system of state universities, which are funded by the governments of each state. Brazil also has a system of federal universities. State and federal universities are highly regarded and tuition-free, and admissions are highly competitive. The number of UERJ applicants is often 10–20 times greater than the number of slots. Most Brazilian students attend one of the nation's 2,000-plus private colleges, which tend to be moderately selective or open enrollment.

UERJ 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 their desired major. Admissions are based on a weighted average of field exam scores. The top-scoring applicants are admitted up to a cutoff determined by the program's capacity.

B. Data

Our analysis matches two UERJ datasets to national employer-employee records. Our base dataset includes all individuals who applied to UERJ in 1995–2001 and 2004–11 (UERJ 2020a).[6] We focus on applicants who passed the first-round exam, which is the relevant sample of potential admits for our analyses. We observe the program individuals applied to, their overall admission score, and their admission outcome. In some cohorts, we observe demographic characteristics and field exam subject scores.[7] Our second dataset contains students who enrolled in UERJ from 1995 to 2011 (UERJ 2020b). This dataset includes the student's program, enrollment date, status as of 2020 (graduated, dropped out, or still enrolled), and final year.

Last, we use the 2003–19 years of Brazil's employer-employee dataset, the Relação Anual de Informações Sociais (RAIS 2021). This dataset covers the universe of formal-sector jobs in Brazil. Worker variables include demographics, educational attainment, occupation, hours worked, and monthly earnings. Firm variables include the firm's industry, location, and number of employees.

We merge the UERJ and RAIS datasets using national ID numbers. For individuals with missing ID numbers, we merge using names and birth dates. See section B.2 of the appendix (available online) for details.

C. Affirmative Action at UERJ

Historically, Black, low-income, and public high school students were underrepresented at state and federal universities, partly because they typically

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

[7] Table B1 (tables A1–A19, B1–B4 are available online) provides details on our variable definitions and data availability.

earned lower scores on the schools' entrance exams.[8] The lack of diversity was contentious because these universities are publicly funded and tuition-free.

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 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 quotas at all federal universities. But UERJ was the only university in Rio de Janeiro with affirmative action for much of the 2000s.

UERJ's policy reserved 45% of seats in each program for low-income applicants from disadvantaged groups. Historically, there was one admission track for each major. In 2004, UERJ added three affirmative action tracks per major.[9] Twenty percent of slots in each major were reserved for public high school applicants. Another 20% of slots were reserved for Black applicants. Five percent of slots were reserved for other disadvantaged groups (e.g., disabled and indigenous applicants). To apply through an affirmative action track, applicants also had to be from a low-income family, as verified by tax records.[10] Applicants who did not meet these criteria could apply through the general track, which governed the remaining slots. Within each track, admissions were based solely on field exam scores.

Although the fraction of reserved slots was the same in each major, the take-up varied significantly. Figure 1 plots the share of affirmative action enrollees in the 2004–11 cohorts (y-axis) against a measure of each program's selectivity (x-axis). In highly selective programs like law and medicine, the reserved quotas usually filled up, so affirmative action students made up 45% of the class. In less selective programs like math and teaching, the number of affirmative action applicants was frequently less than the quota, and UERJ filled open slots from the general track. Thus, the share of affirmative action enrollees was as low as 10%–20% in some programs. The low take-up is attributable to lower desirability of some programs and UERJ's strict criteria for affirmative action eligibility.

UERJ's policy gave a large implicit preference to affirmative action students. Figure 2 plots the distribution of admission scores for 2004–11 applicants in

[8] Other factors likely contributed to limited diversity at selective colleges, such as access to information about the admission process (Hoxby and Avery 2013; Machado and Szerman 2021).

[9] UERJ introduced affirmative action in the 2003 cohort following the state law. There were two admission tracks in 2003—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 of 2004–11.

[10] In 2004, e.g., applicants' per capita family income had to be below BRL 300 per month (Zoninsein and Júnior 2008), which was 40% of national GDP per capita.

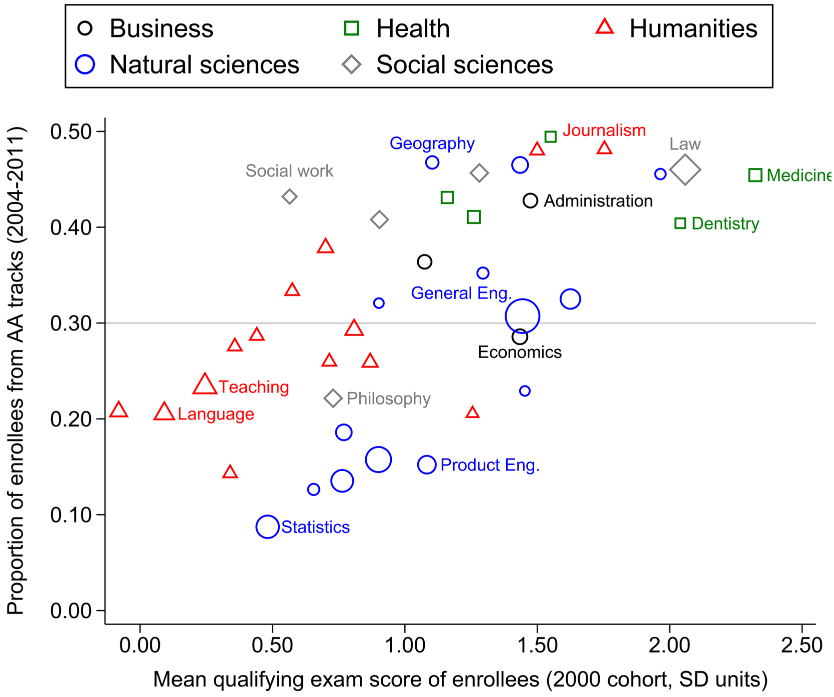

FIG. 1.—Take-up of affirmative action and program selectivity. This figure plots exposure to affirmative action (y-axis) and selectivity (x-axis) for each UERJ program in our sample. The y-axis displays the fraction of enrollees in the 2004–11 cohorts who entered through an affirmative action track. The x-axis displays the mean score on the 2000 qualifying exam for enrollees in each program. We compute each applicant's average score across all exam subjects and standardize to mean 0 and standard deviation 1 in the population of qualifying exam takers. The figure omits two programs 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.

the Black, public school, and general tracks. Scores are standardized to be mean 0 and standard deviation 1 among all applicants to a given program/cohort. Vertical lines show the mean cutoff score in each track, which is the mean score of the last admitted students. The average cutoff is -0.5 in the public school track, -0.6 in the Black track, and $+0.9$ in the general track. Thus, marginally admitted affirmative action students typically scored 1.5 standard deviations below marginal admits in the general track.

D. Samples

We use two samples to analyze the impacts of UERJ's affirmative action policy. In sections III and IV, we use an RD design that compares admitted

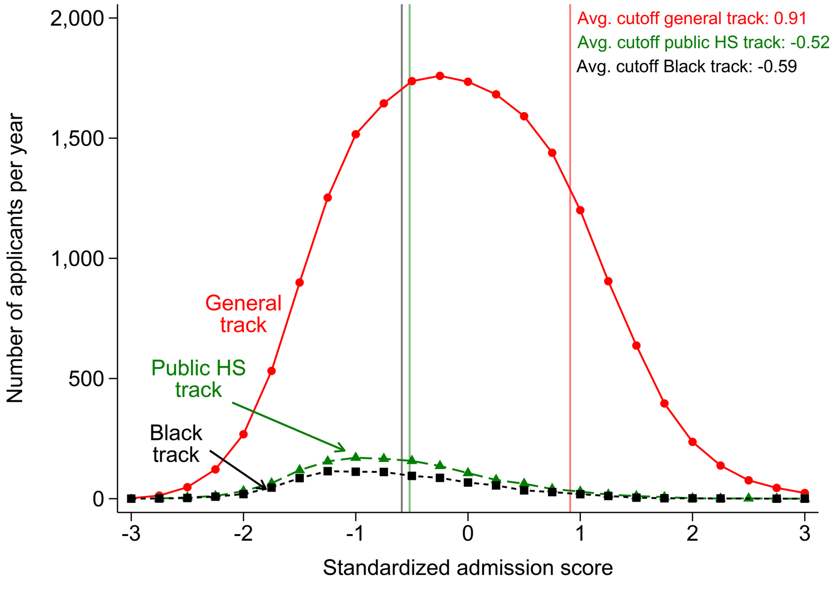

FIG. 2.—Admission score distribution and mean cutoff by application track (2004–11). This figure shows the distribution of standardized admission scores for applicants in each application track. The sample includes the 24 programs in our RD sample (panel A of table 1). We standardize scores to be mean 0 and standard deviation 1 in the population of all applicants in the same program/cohort and plot distributions in 0.25 standard deviation bins of the standardized score. Vertical lines represent the average admission cutoff in each track. HS = high school.

and rejected applicants. In sections IV and V, we use a DD design that compares enrollees in programs with higher and lower take-up rates of affirmative action.

Our RD sample includes programs in which we can estimate returns for marginally admitted affirmative action students. We cannot implement our RD design in cases where there were no rejected students, so we restrict our RD sample to programs where the Black and public school quotas typically filled up. Specifically, our RD sample includes 24 programs in which 30% or more of the 2004–11 enrollees were from an affirmative action track (programs above the horizontal line in fig. 1). In these programs, we also exclude any cohort/application-track pair with fewer than five applicants below the admission threshold (see tables B2–B4).[11]

[11] 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. We exclude the disabled/indigenous track, as these quotas rarely filled. Section B.4 of the appendix provides details on our sample construction.

Our DD sample includes all programs that UERJ offered both before and after 2004.[12] 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 who could have attended UERJ regardless of whether affirmative action existed in their cohort (see sec. V).

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 a wide variety of business, health, engineering, humanities, and social science majors. Our DD sample includes many teacher-training programs, but it also includes economics, math, and several engineering majors. Affirmative action applicants (cols. 3–5) were disadvantaged relative to general applicants (cols. 1, 2), as measured by race, mother's education, and family income.

III. RD Specification

A. Regression Model

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

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

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

The parameter 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 1 if the applicant enrolled in the UERJ program and cohort that they applied to. We instrument for UERJ enrollment with an indicator for an admission score above the final cutoff for application pool p, D_{ip}.

We use a local linear specification to estimate returns for applicants on the admission margin. We include fixed effects for each application pool, γ_p, and an interaction between D_{ip} and the running variable, x_{ip}, which is individual i's admission score in application pool p. We normalize x_{ip} so that it equals 0 for the last admitted student and has standard deviation 1 in the population of all applicants in a program/cohort. Our regression samples include only applicants whose admission scores are within h^Y standard deviations of the admission threshold. Our benchmark results use the Calonico, Cattaneo, and Titiunik (2014) bandwidth computed separately for each outcome Y; tables A4–A6 show that our main results are robust to different bandwidths. We cluster standard errors at the individual level, as some individuals apply to UERJ more than once.

[12] UERJ reorganized a few programs during our sample period. Our DD analysis combines reorganized programs into one program. See tables B2–B4.

Table 1
Summary Statistics for RD and DD Samples

	1995–2001	2004–11 Cohorts			
Sample Sizes and Characteristics of All Applicants	General Track (1)	General Track (2)	Public High School (3)	Black (4)	Other AA (5)
	A. Programs in Both RD and DD Samples (24 Programs)				
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	.50	.55	.60	.60	.48
Age at application	20.75	20.28	21.88	23.04	24.30
White (UERJ data)		.64	.49	.03	.35
White (RAIS data)	.78	.67	.57	.15	.48
Mother has a high school degree		.85	.49	.56	.54
Household income >1.5× minimum wage		.82	.35	.35	.45
	B. Programs in DD Sample Only (19 Programs)				
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	.56	.53	.62	.63	.57
Age at application	22.34	21.62	22.54	24.09	26.24
White (UERJ data)		.59	.49	.03	.32
White (RAIS data)	.75	.65	.60	.20	.47
Mother has a high school degree		.78	.45	.52	.43
Household income >1.5× minimum wage		.74	.28	.30	.25

Note.—This table reports summary statistics for UERJ applicants in our sample. Panel A includes 24 programs that are in our RD and DD samples: accounting, biological sciences, business administration, chemical engineering, chemistry, computer science, dentistry, general engineering, geography, geology, Greek/Latin/literature, history, history education (São Gonçalo [SGO]), industrial design, journalism, law, mechanical engineering, medicine, nursing, nutrition, production engineering, psychology, social science, and social work. Panel B includes 19 programs that are in our DD sample only: art, biological sciences (SGO), cartographic engineering, economics, English/German/Japanese, geography education (SGO), language (SGO), math, math education (SGO), mechanical engineering (Nova Friburgo), oceanography, philosophy, physical education, physics, production engineering (Resende), Spanish/French/Italian, statistics, teaching, and teaching (Duque de Caxias). Programs are at UERJ's main campus in Rio unless denoted with parentheses. Column 1 includes applicants in the pre-AA cohorts. Columns 2–5 include applicants to the four admission tracks in the post-AA cohorts. See table B1 for variable definitions and our grouping of programs into fields of study. See table B4 for our sample definition.

We estimate equations (1) and (2) separately for three groups: pre-AA applicants (1995–2001), post-AA general track applicants (2004–11), and affirmative action applicants. We pool across the Black and public school tracks to increase power. The estimates for affirmative action applicants show how UERJ's policy affected its targeted beneficiaries. The estimates for pre- and post-AA general applicants provide evidence on how the policy changed untargeted students' returns to attending UERJ. However, this evidence is not conclusive because the policy also implicitly raised admission thresholds in the general track.

B. Identification Assumptions and Balance Tests

The main RD identification assumption is that applicants' admission scores are effectively randomly assigned near the thresholds. Applicants have little scope to manipulate their scores, but nonrandom sorting could arise from wait-list admissions. UERJ fills declined seats through multiple rounds of wait-list offers to applicants with the next highest scores (see sec. B.3 of the appendix for details). Our instrument and running variable, D_{ip} and x_{ip}, are defined by the final threshold in each application pool. Thus, the last admitted student may be particularly likely to accept an admission offer, and this tendency may be correlated with potential outcomes.

Balance tests show no evidence that the RD assumption is violated for affirmative action applicants. Table A1 presents estimates from RD regressions that use demographic characteristics and qualifying exam scores as dependent variables. 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 (fig. A1; figs. A1–A6 are available online). There is no evidence of a discontinuity in the density of admission scores using the McCrary (2008) test (fig. A2). These results match our prior that wait-list admissions are unlikely to cause nonrandom sorting in the affirmative action tracks because most applicants accepted their admission offer.

We also find covariate balance for general applicants, but the McCrary (2008) test reveals a statistically significant decrease in the admission score densities at the pre- and post-AA general track thresholds. UERJ's yield was lower in the general track, so there was more scope for nonrandom sorting from wait-list admissions. Thus, our RD results for general applicants should be interpreted with some caution. Reassuringly, our findings are similar in "donut-hole" regressions that drop applicants near the cutoffs (tables A4–A6).

We also make the standard instrumental variable and local average treatment effect assumptions (Angrist, Imbens, and Rubin 1996). Instrument relevance is satisfied because the UERJ enrollment rate increases sharply at the admission threshold (table 2, panel A). The exclusion restriction requires that our instrument affects outcomes only through the channel of enrolling in UERJ. This could be violated if, for example, admission to UERJ caused individuals to apply to other schools. We cannot rule out this possibility, but we believe our results are primarily attributable to UERJ enrollment, particularly in the affirmative action tracks where the first-stage coefficient is large. The monotonicity assumption is plausible because it is unlikely that applicants would have attended UERJ if and only if they were below the cutoff.

Table 2
RD Estimates of the Effects of UERJ Enrollment on Graduation and Earnings

Dependent Variable	1995–2001 General Track: Mean Below (1)	1995–2001 General Track: RD Coef (2)	2004–11 General Track: Mean Below (3)	2004–11 General Track: RD Coef (4)	2004–11 AA Tracks: Mean Below (5)	2004–11 AA Tracks: RD Coef (6)
A. First Stage						
Enrolled in UERJ program	.003	.313***	.008	.292***	.004	.689***
		(.010)		(.006)		(.014)
N	3,234	17,519	4,012	47,838	543	6,121
B. Graduation and Earnings 6–9 Years after Application (2SLS)						
Graduated from UERJ program	.002	.711***	.003	.677***	.004	.640***
		(.017)		(.013)		(.018)
Formal employment	.627	.064**	.672	−.031	.729	−.002
		(.029)		(.027)		(.026)
Log hourly wage	3.237	−.003	3.387	−.079	2.813	.132***
		(.050)		(.049)		(.044)
Monthly earnings (2019 USD)	1,356.069	.295	1,390.819	−153.473**	816.821	110.230**
		(75.313)		(77.290)		(49.546)
N (formal employment)	3,234	37,794	4,012	55,030	543	8,147
N (log hourly wage)	2,027	24,564	2,694	32,972	394	6,100
C. Graduation and Earnings 10–13 Years after Application (2SLS)						
Graduated from UERJ program	.002	.718***	.003	.693***	.003	.661***
		(.017)		(.014)		(.021)
Formal employment	.693	.032	.686	−.026	.714	.037
		(.027)		(.031)		(.039)
Log hourly wage	3.636	.005	3.637	.005	3.052	.024
		(.054)		(.058)		(.063)
Monthly earnings (2019 USD)	2,005.191	−84.946	1,757.947	−99.418	1,041.942	56.577
		(94.587)		(109.084)		(75.202)
N (formal employment)	3,234	39,134	2,974	41,138	388	4,320
N (log hourly wage)	2,237	24,273	2,021	26,407	273	3,746

NOTE.—This table presents RD estimates for the effects of UERJ enrollment on graduation, formal employment, and earnings. Columns 1, 3, and 5 show means of each dependent variable for applicants in each group who scored (−0.1, 0) standard deviations below the cutoff. Columns 2, 4, and 6 show RD coefficients. Panel A reports reduced-form RD coefficients, θ, from eq. (1), which measure the effects of UERJ admission on UERJ enrollment. Panels B and C report 2SLS RD coefficients, β, from eq. (2), which measure the effects of UERJ enrollment on outcomes 6–9 (panel B) and 10–13 (panel C) years after application. Parentheses contain standard errors clustered at the individual level. Sample sizes refer to the dependent variables indicated in parentheses after N.

** $p < .05$.
*** $p < .01$.

Under these assumptions, the β coefficient from equation (2) can be interpreted as the average causal effect of attending UERJ for marginally admitted compliers. Compliers are students who would have enrolled if and only if they scored above the cutoff. This estimand measures the returns to UERJ enrollment relative to the mix of educational choices that students would have made if they were rejected, which is relevant for evaluating the efficacy of affirmative action as a policy to reduce inequality.

IV. Effects of Affirmative Action on Marginal Admits

A. Graduation and Earnings

We begin our RD analysis by examining the effects of UERJ enrollment on graduation rates and earnings. Table 2 presents results for pre-AA general applicants (cols. 1, 2), post-AA general applicants (cols. 3, 4), and affirmative action applicants (cols. 5, 6). Columns 1, 3, and 5 show the means of each dependent variable for applicants who scored just below the threshold (within 0.1 standard deviations). Columns 2, 4, and 6 display RD coefficients. Panel A presents first-stage coefficients, θ, from equation (1). Panels B and C show 2SLS RD coefficients, β, from equations (1) and (2). In panel B, we measure outcomes 6–9 years after UERJ application to capture individuals' initial jobs after (potential) graduation. To examine longer-run effects, panel C measures outcomes 10–13 years after application.[13] Figure 3 presents RD graphs for our main outcomes; these graphs show the reduced-form effects of UERJ admission by plotting means of each outcome in 0.1 standard deviation bins of the standardized admission score.

Panel A of table 2 shows that crossing the admission threshold increased the likelihood that affirmative action applicants enrolled in UERJ by 69 percentage points (col. 6). The first stage for affirmative action applicants is more than double that for general applicants (cols. 2, 4) because most other universities in Rio did not have affirmative action during 2004–11. In the general track, marginal admits would typically have been competitive for admission to other top colleges in the area (see sec. IV.B).

Our first finding is that marginal enrollees in the affirmative action tracks were only slightly less likely to graduate from UERJ than those in the general track. The first row of panel B shows how enrolling in a UERJ program affected the likelihood of graduating from that program by 9 years later. Sixty-four percent of marginal affirmative action enrollees graduated by this time,

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

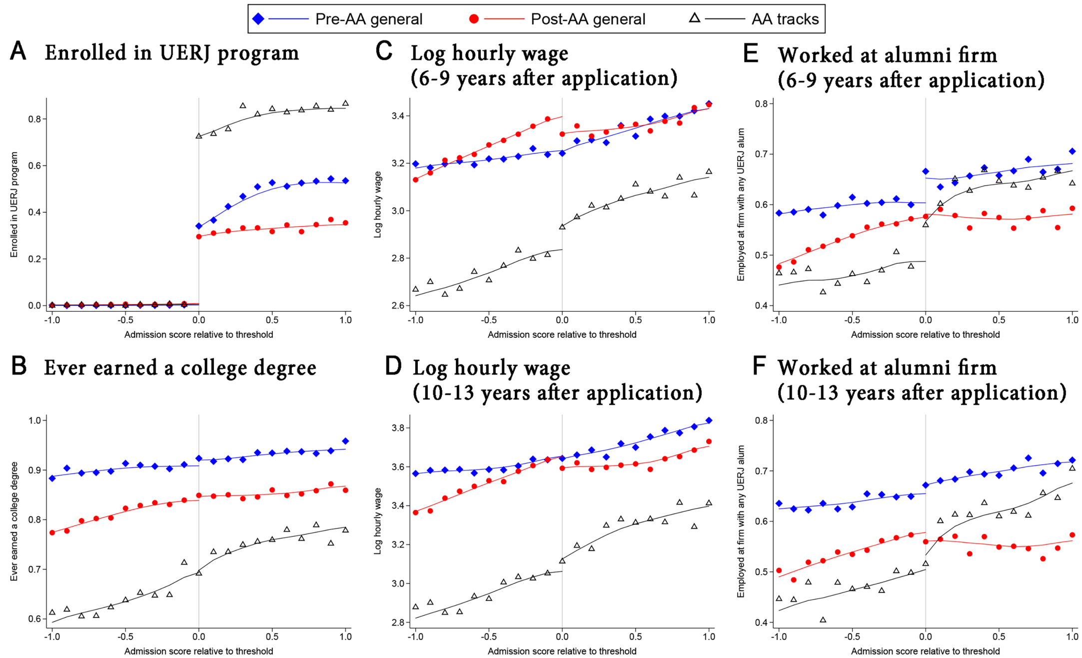

Fig. 3.—RD graphs for UERJ enrollment, earnings, and employment at alumni firms. This figure presents RD graphs for pre-AA general applicants (diamonds), post-AA general applicants (circles), and Black/public school applicants (triangles). The x-axis in each panel is an applicant's standardized admission score normalized to zero at the cutoff. The y-axis plots means of each outcome in 0.1 standard deviation bins of the standardized score. Outcomes are measured 6–9 years after UERJ application in *C* and *E* and 10–13 years after application in *D* and *F*. Lines are predicted values from local linear regressions estimated with a triangular kernel.

compared with 68%–71% of marginal general track enrollees. The similarity of these graduation rates is striking, since the admission scores of marginal affirmative action enrollees were 1.5 standard deviations lower on average.

UERJ enrollment did not significantly affect the likelihood that individuals worked in the formal sector. Our measures of formal employment are indicators for appearing in the RAIS at any time 6–9 or 10–13 years after application. In the affirmative action tracks, the formal employment rates for marginally rejected applicants are above 70% in both time periods (col. 5), and the 2SLS RD coefficients are close to zero (col. 6). For pre-AA applicants, we find a positive and significant effect on early-career formal employment (panel B), but this effect does not persist into the later time period (panel C).

Importantly, affirmative action students experienced an increase in early-career earnings from attending UERJ. UERJ enrollment caused a 14% increase in the mean hourly wages of affirmative action compliers measured 6–9 years after application. The gain in early-career monthly earnings was $110 (in 2019 US dollars). Figure 3*C* shows visual evidence of a discontinuity in the early-career hourly wages of marginally admitted affirmative action students (triangles). The RD coefficient for monthly earnings is roughly one-fifth of the earnings gap between marginally rejected general and affirmative action applicants ($1,391 vs. $817). Thus, UERJ's affirmative action policy meaningfully reduced early-career earnings inequality among applicants on the margin of admission.

We find some evidence that the initial earnings gain for affirmative action students declined as their careers progressed. Panel C of table 2 shows that the effect of UERJ enrollment on affirmative action students' hourly wages declined to 0.024 log points measured 10–13 years later (see also fig. 3*D*). We reject equality of the early- and later-career wage coefficients at $p < .05$ (table A3). The gain in monthly earnings for affirmative action students also declined to $56 in the later period, but this estimate is not statistically distinguishable from the early-career return. Figure A4 shows that the wage gains for affirmative action students decreased both over time (holding the sample of cohorts fixed) and across cohorts (holding potential experience fixed).

For general applicants, we find evidence of a negative early-career return to attending UERJ in the cohorts with affirmative action. We find no significant earnings effects in the pre-AA cohorts (col. 2 of table 2), but UERJ enrollment reduced the early-career hourly wages of 2004–11 general applicants by 8% (col. 4). Similarly, the 2SLS RD estimate for post-AA general applicants' monthly earnings is USD −153, and this estimate is statistically significant at $p < .05$. This suggests that the returns to attending UERJ for non-AA students may have been lower in the cohorts with affirmative action. But this evidence is not conclusive because both earnings coefficients decline in magnitude in the later time period (panel C).

B. College Selectivity and Major Choice

To interpret our earnings results, it is important to understand which college programs UERJ enrollees would have attended if they were not admitted. UERJ is an elite school, but it exists in a highly competitive market. The federal university in Rio de Janeiro, UFRJ, ranked third in a 2012 national ranking by the newspaper *Folha*, while UERJ ranked eleventh. There are three other selective federal universities in the Rio suburbs and more than five private universities in the city itself (see table A8). UERJ applicants in the general and affirmative action tracks differed in the likelihood that they could gain admission to these other colleges during our sample period. Furthermore, applicants to a particular UERJ program may have pursued a different major at another school.

We examine effects on college and major choice using Brazil's higher education census (INEP 2019), which covers all colleges in the country. We do not have access to ID numbers in this dataset, so we match it to our sample of UERJ applicants using exact day of birth, gender, and year of enrollment. These variables do not uniquely identify individuals, so we define our dependent variables as the total number of students at a particular university or major that have the same birth date, gender, and enrollment year as the UERJ applicant. We can only include 2009–11 UERJ applicants in this analysis because individual-level census data do not exist prior to 2009. The fuzzy merge and smaller sample reduce the precision of our RD estimates for this analysis. (See sec. B.5 of the appendix for details.)

With these caveats, we find that UERJ's affirmative action policy allowed disadvantaged applicants to attend a more selective college. Panel A of table 3 displays θ coefficients from our reduced-form RD specification (1), which estimates the effects of UERJ admission. The number of UERJ enrollees in the census data increases by 0.88 at the affirmative action thresholds (col. 6), which is broadly similar to our first-stage estimate of 0.69 in table 2. We do not find effects on enrollment in UFRJ, other federal universities in Rio, or private universities in the top 100 of the *Folha* ranking. Instead, the number of enrollees in lower-ranked Rio universities falls by roughly 0.5 at the affirmative action thresholds. Although these estimates are imprecise, they match our prior that many affirmative action applicants would not have gained admission to other top universities and thus often had less selective private schools as their fallback option.

Admission to UERJ also altered the major choices of affirmative action applicants. In the last two rows of panel A, our dependent variables measure the total number of enrollees in Rio de Janeiro universities with the same major as the one that the UERJ applicant applied to. The number of Rio enrollees with the applicant's major increases at the affirmative action thresholds by 0.35 using two-digit major codes and by 0.46 using three-digit codes. In combination with the RD estimate for the total number of UERJ enrollees

Table 3
RD Estimates for Enrollment in Other Universities and Degree Attainment

Dependent Variable	1995–2001 General Track Mean Below (1)	1995–2001 General Track RD Coef (2)	2004–11 General Track Mean Below (3)	2004–11 General Track RD Coef (4)	2004–11 AA Tracks Mean Below (5)	2004–11 AA Tracks RD Coef (6)
		A. Enrollment in Rio de Janeiro Universities (Reduced Form, 2009–11 Cohorts Only)				
Number enrolled in UERJ			1.465	.271***	1.051	.880***
				(.037)		(.088)
Number enrolled in UFRJ			3.369	−.147***	2.381	−.111
				(.057)		(.137)
Number enrolled in other federal universities			4.407	−.165**	3.181	.041
				(.083)		(.168)
Number enrolled in a top-100 private university			5.154	−.176**	4.312	.147
				(.077)		(.164)
Number enrolled in other private universities			5.110	−.041	5.181	−.457**
				(.062)		(.229)
Number enrolled in same program area (two digit)			3.448	.120**	2.647	.351**
				(.059)		(.138)
Number enrolled in same program area (three digit)			1.661	.192***	1.367	.459***
				(.040)		(.082)
N (number enrolled in UERJ)			1,553	19,895	215	2,757
		B. Educational Attainment Measured in RAIS (2SLS)				
Any college degree, 6–9 years later	.731	.044	.785	.006	.636	−.002
		(.032)		(.029)		(.038)
Ever earned a college degree	.911	.012	.839	.026	.713	.010
		(.017)		(.025)		(.033)
Ever earned a graduate degree	.107	−.004	.069	−.017	.051	−.006
		(.020)		(.017)		(.015)
N (ever college degree)	2,417	32,718	2,925	36,617	415	5,978

NOTE.—This table presents RD estimates for enrollment in Rio de Janeiro universities and educational attainment. Panel A reports reduced-form RD coefficients, θ, from eq. (1). The dependent variables are the total number of enrollees in a given group of universities or field of study who share the applicant's birth date, gender, and enrollment year, as measured in Brazil's higher education census (see sec. B.5 of the appendix). We categorize universities into four groups by ownership and selectivity: (1) the federal university in the Rio de Janeiro municipality (UFRJ); (2) federal universities in the Rio de Janeiro suburbs (Universidade Federal Fluminense, Universidade Federal Rural do Rio de Janeiro, Universidade Federal do Estado do Rio de Janeiro); (3) private universities in the Rio de Janeiro municipality that ranked in the top 100 of the 2012 *Folha* ranking (Pontifícia Universidade Católica do Rio de Janeiro, Universidade Estácio de Sá); and (4) other private universities in the Rio de Janeiro municipality (Universidade Gama Filho, Universidade Veiga de Almeida, Universidade Candido Mendes, Universidade Salgado de Oliveira, Universidade Castelo Branco). Fields of study are defined by two- and three-digit census major codes. The sample is 2009–11 UERJ applicants. Regressions include gender and age dummies to increase precision. Panel B reports 2SLS RD coefficients, β, from eq. (2). The dependent variables are indicators for educational attainment measured in the RAIS. Regressions include all UERJ applicants. The columns are defined in the same way as table 2. Parentheses contain standard errors clustered at the individual level. Sample sizes refer to the dependent variables indicated in parentheses after *N*.

** $p < .05$.
*** $p < .01$.

(0.88), these coefficients suggests that roughly half of affirmative action compliers would have chosen a different major if they were not admitted to UERJ. These changes in field of study are an important mechanism for our RD earnings results, but such changes are relevant for evaluating affirmative action policies in any context where individuals may pursue different majors at different schools.

For general applicants, admission to UERJ reduced the likelihood of enrolling in other top federal and private universities in Rio (table 3, panel A, col. 4). Thus, most general track compliers would likely have attended other selective universities if they had been rejected.

C. Educational Attainment

We next examine whether UERJ enrollment affected the likelihood that individuals earned any college or postgraduate degree. We use the RAIS 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. Panel B of table 3 shows 2SLS RD estimates for these outcomes using regression samples that include all applicants who appear in the RAIS.[14]

We find no effects on educational attainment for both affirmative action and general applicants. Most notably, UERJ enrollment did not affect the likelihood that affirmative action applicants earned a college or postgraduate degree (col. 6). Seventy-one percent of marginally rejected Black and public school applicants earned a college degree by 2019 (col. 5), which is a very high rate by Brazilian standards. This reflects the fact that UERJ's affirmative action applicants were high achieving, even though they were disadvantaged relative to general UERJ applicants. In the general tracks, college degree attainment rates were even higher (cols. 1, 3), and we also find no effects on educational attainment (cols. 2, 4).

D. Employment with UERJ Alumni

As a final potential mechanism, we consider the effects of networking with UERJ peers and alumni. Elite university networks can improve students' access to high-paying jobs through many channels (Rivera 2016), including on-campus recruiting (Weinstein 2022), referrals (Calvo-Armengol and Jackson 2004), and school reputation (MacLeod and Urquiola 2015).

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

[14] We find no evidence that the observable characteristics of UERJ applicants who appear in the RAIS change discontinuously at the admission thresholds (table A1).

"alum"). Our simplest network outcome is an indicator equal to 1 if the applicant's firm ever hired another alum. We define different versions of this variable based on the alum's characteristics, the timing of their employment, and the concentration of alumni at the firm. We use major-specific networks because students in the same program often take classes together and work in similar labor markets.[15]

Attending UERJ significantly increased the likelihood that affirmative action students obtained jobs at firms affiliated with other UERJ alumni. Panel A of table 4 shows that marginal affirmative action enrollees were 13.7 percentage points more likely to work at a firm affiliated with any UERJ alum in the period of 6–9 years after application (see also fig. 3*E*). This is a 29% increase from the mean rate of alumni firm employment for marginally rejected applicants (47.7%). Affirmative action enrollees were more likely to work with both general and affirmative action alumni (second and third rows of panel A). Similarly, attending UERJ increased the proportion of UERJ alumni at affirmative action applicants' firms by 8.5 alumni per 1,000 workers (fourth row of panel A). We also find large effects on early-career employment at alumni firms for general applicants (cols. 2, 4).

Figure 4 presents evidence that the results in table 4 are partly driven by networking mechanisms. It is possible that the above employment effects reflect major-specific human capital accumulation rather than networking, since admission to UERJ affected major choices (table 3). To distinguish between these mechanisms, figure 4 displays heterogeneity in RD estimates for the number of UERJ alumni per 1,000 workers at the applicant's firms (pooling across all applicant groups). We find larger estimates in cases where networking is likely more important. The RD coefficients are larger for employment in small private firms than in large public firms.[16] Similarly, the employment effects are largest for alumni from the applicant's cohort and for alumni who work at the firm at the same time as the applicant. This variation is consistent with referral and recruiting mechanisms, and it is hard to reconcile with major-specific human capital. Table A10 shows that enrolling in UERJ increased access to firms with UERJ alumni even within groups of firms in the same location and industry. In other words, the presence of UERJ alumni is a strong predictor of an applicant's employment outcome even among firms in the same narrowly defined labor market.

Mean wages at firms affiliated with UERJ alumni were 0.44 log points higher than those at other firms in our sample (table A12), suggesting that

[15] All of our network outcomes are leave-individual-out; even if an applicant completed a UERJ degree, these variables equal 1 only if there is another alum affiliated with that firm. Our variable definitions allow applicants to be beneficiaries or benefactors of UERJ's alumni network. For example, an applicant could work at an alumni firm if they got a job from an alum's referral or if they referred an alum.

[16] Networking is likely more important at small private firms because most public firms in Brazil use exams to hire workers (Mocanu 2022).

Table 4
RD Estimates for Employment at Alumni Firms

Dependent Variable	1995–2001 General Track Mean Below (1)	1995–2001 General Track RD Coef (2)	2004–11 General Track Mean Below (3)	2004–11 General Track RD Coef (4)	2004–11 AA Track Mean Below (5)	2004–11 AA Track RD Coef (6)
	A. Employment 6–9 Years after Application (2SLS)					
Employed at firm with any UERJ alum	.600	.118***	.572	.070**	.477	.137***
		(.033)		(.034)		(.038)
Employed at firm with any general track alum	.579	.129***	.540	.076**	.437	.106***
		(.035)		(.034)		(.035)
Employed at firm with any AA track alum	.375	.036	.402	.076**	.386	.129***
		(.031)		(.033)		(.037)
Number of UERJ alumni per 1,000 workers at firm	6.561	4.652***	7.120	9.738***	3.334	8.523**
		(1.624)		(2.583)		(3.580)
Firm mean wage (log)	3.303	.018	3.475	−.095*	3.073	.106*
		(.043)		(.053)		(.062)
N (firm mean wage)	2,024	30,345	2,681	31,087	394	4,306
	B. Employment 10–13 Years after Application (2SLS)					
Employed at firm with any UERJ alum	.649	.059*	.573	.028	.498	.080*
		(.033)		(.037)		(.044)
Employed at firm with any general track alum	.629	.060	.550	.014	.451	.086**
		(.037)		(.038)		(.039)
Employed at firm with any AA track alum	.411	.032	.384	.056	.383	.082*
		(.034)		(.036)		(.044)
Number of UERJ alumni per 1,000 workers at firm	5.873	3.648***	5.620	5.954**	3.224	−.092
		(1.078)		(3.025)		(2.767)
Firm mean wage (log)	3.572	.093*	3.581	−.053	3.223	.049
		(.053)		(.062)		(.071)
N (firm mean wage)	2,236	24,701	2,010	21,071	275	3,133

Note.—This table presents 2SLS RD coefficients, β, from eq. (2), which measure the effects of UERJ enrollment on employment at alumni firms 6–9 (panel A) and 10–13 (panel B) years after application. The first three outcomes are indicators for employment at any firm during each time period with (1) any UERJ alum, (2) any general track alum, and (3) any AA track alum. The fourth outcome is the average number of alumni per 1,000 workers at the applicant's firms over the time period. The fifth outcome is the natural log of the mean hourly wage at the applicant's firms averaged over the time period. The columns are defined in the same way as table 2. Parentheses contain standard errors clustered at the individual level. Sample sizes refer to the dependent variables indicated in parentheses after *N*.

* $p < .10$.
** $p < .05$.
*** $p < .01$.

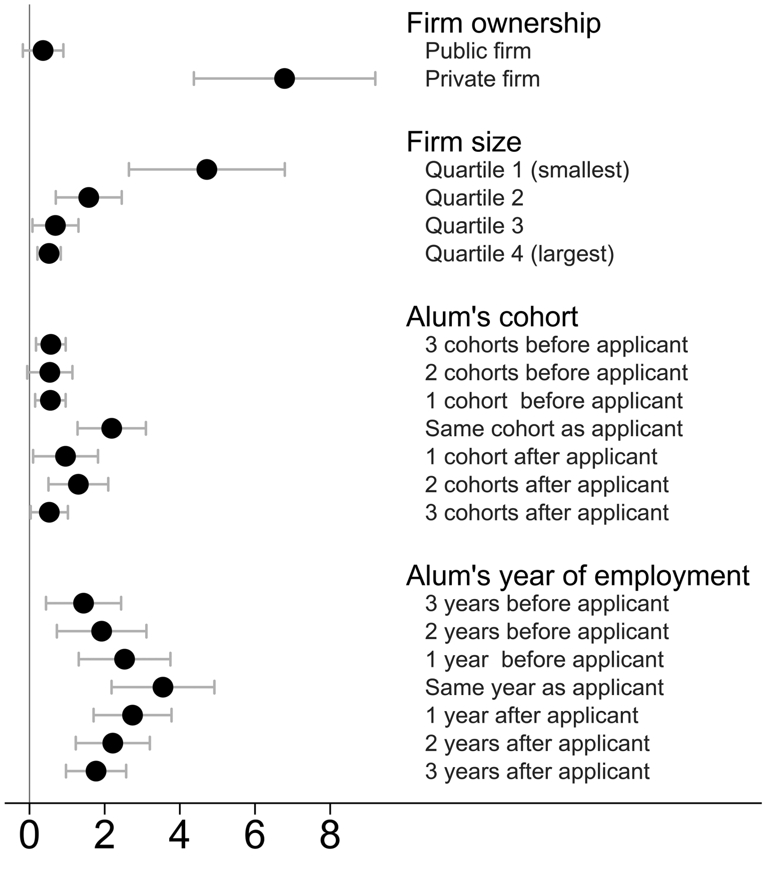

FIG. 4.—Heterogeneity in RD estimates for alumni per 1,000 workers at the firm. This figure displays RD estimates for the effects of UERJ enrollment on the mean number of alumni per 1,000 workers at the applicants' firms measured 6–9 years after application. These estimates are analogous to those in the fourth row of table 4, panel A, but we pool across all applicant groups. We use four types of dependent variables: firm ownership (the firm's mean number of alumni per 1,000 workers interacted with dummies for public and private firms), firm size (the firm's mean number of alumni per 1,000 workers interacted with dummies for quartiles of firm size [number of workers]), alum's cohort (the firm's mean number of alumni per 1,000 workers computed separately using alumni who enrolled in UERJ in each cohort from 3 years before to 3 years after the applicant's cohort), and alum's year of employment (the firm's mean number of alumni per 1,000 workers computed separately using alumni who worked at the firm in each year from 3 years before to 3 years after the applicant). Markers depict RD coefficients, β, from equation (2). Horizontal bars are 95% confidence intervals computed using standard errors clustered at the individual level.

affirmative action enrollees benefited from increased access to these firms.[17] Consistent with this, attending UERJ increased the mean wage at affirmative action compliers' early-career firms by 0.11 log points (last row of panel A, col. 6), which is similar in magnitude to the individual-level wage coefficient (0.13 log points). Notably, UERJ enrollment reduced the average wage at post-AA general applicants firms' by 0.10 log points (col. 4), consistent with their negative earnings effects in table 2.

Yet the benefits of accessing UERJ's alumni network decreased as individuals' careers progressed. Panel B of table 4 shows RD estimates for the same alumni firm outcomes as in panel A, but instead measured 10–13 years after UERJ application. For all outcomes and all applicant groups, the RD estimates are smaller in the later period, and many are not statistically different from zero. This suggests that alumni networks are most important for initial job placement and that their influence declines as individuals progress in the labor market.

E. Discussion

Our graduation results show that most affirmative action students succeeded academically at UERJ. Related work argues that affirmative action may cause disadvantaged students to drop out or switch majors—particularly in STEM fields—because it places them in schools where they are less prepared than their classmates (Arcidiacono, Aucejo, and Hotz 2016). UERJ graduation rates are high by Brazilian standards, and most programs in our RD sample are in non-STEM fields (table 1). Thus, relative academic preparation may be less important for degree completion in our setting.[18] On the other hand, we do not find that affirmative action increased the likelihood that disadvantaged students earned a college degree, as other work has found (Bleemer 2022). One possibility is that negative effects of mismatch in academic preparation were offset by positive effects of UERJ's greater resources, yielding a zero net effect on degree attainment.

Our findings suggest that the early-career earnings gains for affirmative action students were driven partly by networking mechanisms. UERJ's affirmative action policy increased disadvantaged students' access to higher-paying firms affiliated with its alumni (table 4). Taken together with the ordinary least squares (OLS) wage premium for alumni firms (0.44 log points), the RD estimate for alumni firm employment (14 percentage points) can explain nearly half of affirmative action students' early-career wage gains

[17] Table A9 provides examples of alumni firms. Firms with the highest alumni concentration include financial organizations like Accenture and the Brazilian Development Bank, as well as branches of the multinational petroleum company Petrobras.

[18] Affirmative action students' early-career earnings gains were driven by UERJ's health and business programs, which also have high graduation rates. In STEM programs, affirmative action students graduated at much lower rates, and we find no evidence of positive returns. See table A7.

(0.13 log points). Our paper differs from Zimmerman's (2019) and Michelman, Price, and Zimmerman's (2022) findings that the benefits of networking at elite universities accrue only to students from advantaged backgrounds. This difference may arise because our data include early-career outcomes in a broader set of firms, and our results are unclear about whether initial networking benefits are persistent. Furthermore, our estimate of the early-career return for affirmative action students is substantially smaller than that in Bleemer (2022; see n. 3), which may be because we find no effects on college degree attainment.

For general track enrollees, we find some evidence that the early-career return to attending UERJ declined from the pre-AA to the post-AA cohorts (table 2, panel B). This suggests that there may have been negative spillover effects of affirmative action on other UERJ students. In the post-AA cohorts, marginally admitted general applicants were less likely to work at high-paying firms and more likely to work with affirmative action alumni (table 4). Thus, affirmative action may have reduced the value of networking at UERJ because the new disadvantaged students tended to obtain lower-paying jobs. But our RD analysis cannot conclusively identify spillover effects because affirmative action also affected the characteristics of marginally admitted general applicants.[19] To present more compelling evidence on spillover effects of UERJ's policy, we turn to our second empirical strategy.

V. DD Specification

A. Top Enrollee Sample

To estimate the effects of affirmative action on other UERJ 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.[20] Our top enrollee sample is a balanced panel at the major level that includes the N_m enrollees with the highest admission scores in each cohort. Since 55% of slots were reserved for general applicants, this sample contains roughly the top half of the class.

B. Regression Model

For identification, we exploit variation in the take-up of affirmative action across UERJ's majors (fig. 1) in a DD specification:

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

[19] Table A2 shows that post-AA general track compliers were more likely to be nonwhite and younger than pre-AA compliers, although the magnitude of these differences is relatively modest.

[20] In other words, we define $N_m = \min_{c\in\{1995,\dots,2011\}} N_{mc}$, where N_{mc} is the number of general track enrollees in major m and cohort c.

The term 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 1 if the share of affirmative action enrollees in 2004–11 was above 30% (the horizontal line in fig. 1). We include major and cohort fixed effects and cluster standard errors at the major level.

We estimate equation (3) in our sample of top enrollees to examine the effects of affirmative action on untargeted students. In this case, the π coefficient measures how affirmative action changed top enrollees' outcomes in more affected majors relative to less affected majors. We refer to these estimates as "spillover" effects because they reflect the impacts of affirmative action students' enrollment on top enrollees' outcomes (Arcidiacono and Vigdor 2010). We also present DD coefficients for the small subset of top enrollees who are from underrepresented minority (URM) groups; this sheds light on whether affirmative action had an impact on URM students who could have gained admission to UERJ in the absence of the policy.

Our DD specification identifies the effects of a 19 percentage point increase in the fraction of enrollees in an individual's program/cohort who entered through affirmative action (panel A of table 5). This is a large effect on diversity relative to the scale of affirmative action at many US universities, but it is similar to the magnitude of Chetty et al.'s (2020) need-affirmative counterfactual admission policy.

C. Identification Assumptions

Our key identification assumption is that the outcomes of enrollees in more and less affected majors would have followed parallel trends in the absence of affirmative action. A potential concern is that Brazil experienced a recession in the mid-2010s, which may have had heterogeneous impacts across UERJ's majors. To address this, we interact the cohort dummies in equation (3), γ_c, with fixed effects for five field of study groups, $f(m)$: business, health, humanities, natural sciences, and social sciences (see sec. B.1 of the appendix). This restricts identification to comparisons between majors in the same field, which were likely to be similarly affected by macroeconomic conditions.

Figure A6 shows that mean wages evolved similarly in industries that hired UERJ students from majors with more and less exposure to affirmative action. For this figure, we first compute the mean hourly wage in each industry $\times$ year pair using all workers in the RAIS. We then compute a weighted average of these industry $\times$ year means for each UERJ major using the share of pre-AA top enrollees who were employed in each industry as weights. These industry mean wages trended similarly between more and less exposed majors across all years of our data. In the years in which post-AA graduates were in the labor market (2009–19), the change in industry mean

Table 5
DD Estimates of the Effects of Affirmative Action Exposure on Student Body Composition

Dependent Variable	Pre-AA Mean: All Top Enrollees (1)	DD Coefficients: All Top Enrollees (2)	DD Coefficients: URM Top Enrollees (3)	DD Coefficients: Non–Top Enrollees (4)
	A. Exposure to Affirmative Action			
Proportion of classmates from AA tracks	.000	.189***	.208***	.192***
		(.017)	(.021)	(.018)
	B. Demographic Characteristics			
Age at application	21.921	.191	.794	.666***
		(.312)	(.899)	(.229)
Female	.501	.032	.102	.038*
		(.022)	(.078)	(.021)
White	.810	.013		−.121***
		(.018)		(.025)
Brown	.156	.000		.043**
		(.012)		(.017)
Black	.025	−.005	−.009	.077***
		(.010)	(.037)	(.012)
	C. Admission Exam Scores (Standardized in Population of All Enrollees)			
Field exam writing score	.178	−.045	.031	−.246***
		(.043)	(.129)	(.046)
Mean field exam subject score	.151	−.029	.039	−.182**
		(.064)	(.121)	(.084)
Admission score	.270	−.080	.034	−.498***
		(.112)	(.160)	(.143)
	D. Predicted Log Wage Based on Characteristics and Scores			
Predicted log wage	3.298	−.023	−.011	−.161***
		(.029)	(.049)	(.043)
Predicted log wage (if in RAIS)	3.251	−.033	−.009	−.154***
		(.028)	(.053)	(.043)
N (enrollees)	16,466	35,866	1,631	30,854

NOTE.—This table displays DD estimates of the effect of affirmative action exposure on student characteristics. Column 1 shows the mean of each dependent variable for all top enrollees in the 1995–2001 cohorts. Columns 2–4 display estimates of π from eq. (3) for all top enrollees, URM top enrollees, and non–top enrollees. The dependent variables are as follows: for panel A, the proportion of enrollees in an individual's program/cohort who were from an affirmative action track; for panel B, demographic characteristics of enrollees; for panel C, applicants' field exam and overall admission scores, normalized to be mean 0 and standard deviation 1 in the population of all UERJ enrollees in a given cohort (field exam score regressions include dummies for cohorts × applicant's set of subject tests, which vary by major); and for panel D, the predicted value from a regression of log hourly wage on all variables in panels B and C. Parentheses contain standard errors clustered at the program level.

* $p < .10$.
** $p < .05$.
*** $p < .01$.

wages between more and less affected majors is small and statistically insignificant (-0.02 log points). This suggests that our results are not driven by divergent industry growth rates or heterogeneous impacts of the mid-2010s recession. Below we also present event study and robustness results to test our identification assumption.

VI. Spillover Effects of Affirmative Action

A. Characteristics of UERJ Enrollees

We begin our DD analysis by asking whether affirmative action affected the composition of UERJ's top enrollees. Research finds that families prefer schools with high-achieving peers (Abdulkadiroğlu et al. 2020). Thus, UERJ's policy may have induced some students to attend other colleges. To test this hypothesis, table 5 uses UERJ enrollees' demographic characteristics and entrance exam scores as dependent variables in regression (3). Column 1 shows dependent variable means for top enrollees in pre-AA cohorts (1995–2001). Our main results are the DD coefficients, π, for top enrollees in column 2. Column 3 shows DD estimates for URM top enrollees, which we define as top enrollees who identify as Black or indigenous in the RAIS. Column 4 shows DD estimates for students who are not top enrollees.

We do not find significant effects of exposure to affirmative action on top enrollees' observable characteristics. The DD coefficients for top enrollees' age, gender, and race are small and statistically insignificant (table 5, panel B, col. 2). We find no effects on top enrollees' field exam or admission scores (panel C). In panel D, the dependent variables are indices of predicted log wages based on demographic characteristics and admission scores. We find no effect on these predicted wages, and the estimates are similar when we restrict to enrollees who appear in the RAIS. Thus, the composition of top enrollees in more and less affected majors did not diverge significantly with the adoption of affirmative action.[21]

A possible explanation for this finding is that prospective students may not have known that the take-up of affirmative action would differ across UERJ's majors. Students were surely aware of the admission policy, but our DD analysis nets out school-level changes in top enrollees' characteristics. Before enrolling, students may not have known that the affirmative action share would be, for example, 15 percentage points lower in economics than in business. Thus, while affirmative action may have deterred some students from enrolling in any UERJ major, compositional changes are unlikely to fully explain our DD results.

[21] We also find no evidence of differential changes in the composition of top enrollees using socioeconomic status indices based on individuals' first and last names (table A13).

By contrast, in majors with high exposure to affirmative action, the population of non–top enrollees became more racially diverse, older, and lower ability, as measured by entrance exam scores (col. 4 of table 5). This reflects the intended effects of affirmative action on diversity.

B. Labor Market Outcomes

Our main finding is that greater exposure to affirmative action reduced top enrollees' earnings. Table 6 presents DD estimates for graduation and labor market outcomes measured 6–9 years after application using the same table structure as table 5. We find that UERJ's policy reduced the mean hourly wage of top enrollees by 14% in more affected majors relative to less affected majors (panel B, col. 2). The DD estimate for average monthly earnings is similar in magnitude (USD −170). Figure 5*A* shows an event-study version of this result. The hourly wage coefficient for top enrollees (circular markers) drops sharply between the last pre-AA cohort (2001) and the first post-AA cohort (2004), and it declines further to −0.20 log points by the 2011 cohort. These negative effects persist at a similar magnitude for earnings measured 10–13 years after application (see table A14).

The decline in top enrollees' earnings was largely driven by a decline in firm quality, as measured by firm average wages. The DD estimate for log firm mean hourly wage is −0.095 for top enrollees (panel B of table 6), which is 70% of the individual wage coefficient. The event-study coefficients for firm average wage also decline sharply in the first post-AA cohort (fig. 5*B*). Exposure to affirmative action did not affect top enrollees' graduation rates (panel A of table 6), suggesting that the earnings effect is not driven by changes in educational attainment. The DD estimate for formal employment is negative and marginally significant (−0.027), but it is relatively small compared with the mean formal employment rate (0.74).

Table A16 shows that our results for top enrollees are robust to multiple specification checks. Our earnings estimates are similar if we restrict to pre-recession years or if we include program-specific linear trends estimated in the pre-AA cohorts. Controlling for student demographics and entrance exam scores only slightly reduces the DD coefficients, consistent with the small compositional effects in table 5. We continue to find negative effects when we compare programs in the same quartile of selectivity (defined by the x-axis in fig. 1) and when we exclude field of study controls. Last, our results are similar when we use a continuous treatment variable, ExposureToAA_m, which is the share of 2004–11 enrollees who were from an affirmative action track (the y-axis in fig. 1).

Notably, we also find that UERJ's affirmative action policy reduced the earnings of top enrollees from URM groups (col. 3 of table 6). These estimates are imprecise because our top enrollee sample includes only about 1,600 Black and indigenous students. Nonetheless, we find negative and significant point estimates for both individual and firm average wages (panel B). We also find

Table 6
DD Estimates for Graduation, Employment, and Earnings 6–9 Years after Application

Dependent Variable	Pre-AA Mean: All Top Enrollees (1)	DD Coefficients: All Top Enrollees (2)	URM Top Enrollees (3)	Non–Top Enrollees (4)
		A. Graduation and Formal Employment		
Graduated from UERJ program	.556	.013	.013	.006
		(.021)	(.063)	(.028)
Formal employment	.734	−.027*	.076	−.012
		(.015)	(.054)	(.015)
		B. Earnings		
Log hourly wage	3.245	−.132***	−.220**	−.212***
		(.045)	(.107)	(.062)
Monthly earnings (2019 USD)	1,380.558	−169.838***	−28.036	−272.989***
		(53.057)	(100.006)	(89.500)
Firm mean hourly wage (log)	3.316	−.095**	−.334***	−.183***
		(.035)	(.113)	(.051)
		C. Employment at Firms with Pre- and Post-AA Alumni		
Pre-AA alumni	.602	−.055**	−.009	−.044
		(.023)	(.081)	(.033)
Only post-AA alumni	.067	.049**	.019	.036
		(.023)	(.056)	(.023)
		D. Alumni Firm Employment by Application Track and Cohort		
General track alumni from same cohort	.451	−.098***	−.118*	−.072**
		(.021)	(.068)	(.028)
General track alumni from different cohort	.233	.042**	.138	.004
		(.016)	(.086)	(.017)
Only AA alumni from same cohort	.000	.036***	.035**	.051***
		(.009)	(.014)	(.007)
Only AA alumni from different cohort	.012	.010**	−.017	.014***
		(.005)	(.022)	(.004)
N (enrollees)	16,466	35,866	1,631	30,854
N (wage observations)	12,062	26,445	1,323	22,975

NOTE.—This table displays DD estimates of the effect of affirmative action exposure on graduation, earnings, and employment at alumni firms measured 6–9 years after application. The columns are defined in the same way as table 5. The dependent variables are defined similarly to those in tables 2 and 4. In panel C, we categorize firms using alumni from the pre- and post-AA cohorts. In panel D, we categorize firms using the alum's cohort (same or different from the applicant's cohort) and application track (general or affirmative action). The outcomes in panels C and D are nonoverlapping (i.e., variables in the lower rows equal 1 only if the firm did not hire alumni who meet the criteria for the higher rows). Parentheses contain standard errors clustered at the program level.

* $p < .10$.
** $p < .05$.
*** $p < .01$.

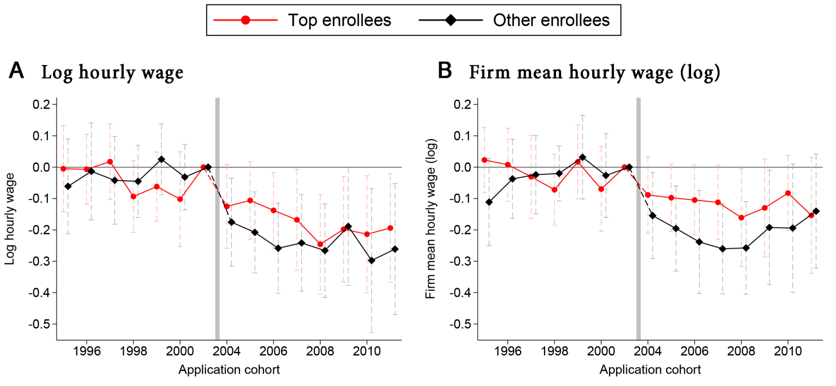

FIG. 5.—Event study estimates for individual and firm mean hourly wages 6–9 years after application. This figure plots π_c coefficients from an event-study version of our DD regression (3), which replaces Post$_c$ with dummies for each cohort (omitting 2001). Dashed lines are 95% confidence intervals computed using standard errors clustered at the program level. The dependent variables are log hourly wage (*A*) and firm mean log hourly wage (*B*) measured 6–9 years after application. Circular markers show estimates for top enrollees. Diamond markers show estimates for other enrollees.

large earnings declines for non–top enrollees in more versus less exposed majors (col. 4). The DD estimate for non–top enrollees' hourly wages (−0.212) is larger in magnitude than the predicted wage effect based on individual characteristics (−0.154). Thus, spillover effects may have also reduced the wages of affirmative action students, although this evidence is suggestive.

C. Networking Mechanisms

To shed light on mechanisms for these spillover effects, we first ask whether affirmative action affected the jobs that UERJ students obtained through networking. Affirmative action may have caused some employers to forgo recruiting at UERJ because it reduced the expected ability of a UERJ student (MacLeod et al. 2017; Weinstein 2018). Furthermore, affirmative action students typically obtained lower-paying jobs than general track students (table 2), so the value of referrals from classmates likely declined in majors with high exposure to the policy.

To test for these mechanisms, panels C and D of table 6 use dependent variables that measure employment at firms that hired UERJ alumni from different cohorts and application tracks.[22] In panel C, the outcome variables are

[22] These alumni firm variables are similar to those in our RD analysis except we define them to be nonoverlapping. For example, in the second row of panel C, the dependent variable equals 1 only if the firm did not hire a pre-AA alum. As in table 4, we require that the applicant and alum are from the same major.

indicators for employment at firms with pre-AA alumni from the enrollee's program versus firms that hired only post-AA alumni. In panel D, the outcome variables are indicators for employment at firms with general track alumni from the enrollee's own cohort versus firms that hired only alumni from other cohorts or from the affirmative action tracks. These variables test whether affirmative action changed the types of firms that hired UERJ students (panel C) and the peer connections that UERJ students used to obtain jobs (panel D). Firms with pre-AA and general track alumni paid significantly higher average wages than those that hired only post-AA or affirmative action alumni (table A12).

We find that affirmative action reduced top enrollees' employment rates at higher-paying alumni network jobs. Top enrollees' likelihood of employment at firms with pre-AA alumni declined by 5.5 percentage points in more versus less affected majors (table 6, panel C, col. 2). This decline was offset by a 4.9 percentage point increase in the rate of employment at firms that hired only post-AA alumni. Similarly, the likelihood of employment with same-cohort general track alumni declined by 9.8 percentage points for top enrollees in more versus less affected majors (panel D, col. 2). Correspondingly, top enrollees in these majors became relatively more likely to work at firms that hired only general track alumni from another cohort (+4.2 percentage points) or only affirmative action alumni (+4.6 percentage points). Thus, employment shifted toward firms with lower average wages (table A12). This suggests that the negative spillover effects of affirmative action on earnings can partly be explained by a decline in the value of networking.

D. Learning Mechanisms

Affirmative action may also have reduced top enrollees' earnings through human capital channels. For example, UERJ students became less academically prepared on average under affirmative action. This may have reduced the benefits of peer interactions or caused professors to teach less advanced material.

To test for learning mechanisms, we use data from Brazil's national college exit exam, the Enade (INEP 2022). The Enade is a field-specific exam that has been administered every year since 2004, although each field is tested every 3 years on a staggered schedule. The government uses Enade scores to rate higher education programs, so many universities ask students to take the exam when they are close to graduation (Pedrosa, Amaral, and Knobel 2013). The Enade is typically low stakes from the student's perspective; it is not a graduation requirement at most universities.

Table 7 shows how affirmative action affected the characteristics and performance of UERJ's Enade participants. This table presents DD estimates that compare 2004–15 exam takers at UERJ to those at other federal and state universities that did not have affirmative action during this period.[23] Column 1

[23] See table A17 for details on our Enade sample and the exam fields.

Table 7
DD Estimates for UERJ's Enade Exam Taker Characteristics and Scores

	UERJ Pre-AA Mean	DD Coefficients		
Dependent Variable	All Students (1)	All Students (2)	White Private High School Students (3)	Other Students (4)
		A. Characteristics of Enade Exam Takers		
Number of exam takers	36.086	4.322	−7.926***	9.142***
		(2.664)	(1.398)	(1.777)
White	.716	−.132***		−.066***
		(.009)		(.009)
Private high school student	.570	−.131***		−.040***
		(.015)		(.009)
Female	.526	−.005	−.009	−.016
		(.011)	(.012)	(.011)
Age	26.520	.661**	.042	.700*
		(.261)	(.209)	(.361)
Mother has a high school degree	.692	−.086***	.016	−.081***
		(.012)	(.012)	(.012)
Household income/ minimum wage	7.724	−1.577***	.292	−1.873***
		(.098)	(.210)	(.117)
Predicted general score	.563	−.008***	.001**	−.007***
		(.001)	(.001)	(.001)
		B. Enade Scores (Proportion Correct Answers)		
Overall score	.553	−.038***	−.022**	−.051***
		(.005)	(.008)	(.006)
Field-specific component	.519	−.037***	−.021**	−.048***
		(.005)	(.009)	(.006)
General component	.657	−.041***	−.026***	−.059***
		(.005)	(.007)	(.007)
N (programs × years)	36	1,664	1,664	1,664
N (exam takers)	1,059	61,112	16,851	37,992

NOTE.—This table displays DD estimates of the effect of affirmative action on the characteristics (panel A) and scores (panel B) of UERJ's Enade exam takers. The sample is 2004–15 Enade participants from UERJ and other federal/state universities that did not implement affirmative action through 2012 (see table A17). Column 1 shows means for UERJ exam takers in 2004–6. Other columns show π coefficients from the following DD regression: $Y_{mjt} = \gamma_{mj} + \gamma_{mt} + \pi[\text{UERJ}_j \times \text{Post}_t] + \varepsilon_{mjt}$. Regressions are at the exam field (m) by institution (j) by year (t) level, with observations weighted by the number of exam takers. (In the first row of panel A, we weight by the number of 2004–6 exam takers in each mj cell.) We include dummies for field × institution, field × year, and UERJ × 2007–15 cohorts ($\text{UERJ}_j \times \text{Post}_t$). Columns 2–4 include all students, white private high school students, and nonwhite and/or public school students. "Predicted general score" is the predicted value from a regression of general component scores on age and dummies for gender, race, private high school, mother's education, father's education, and family income. Parentheses contain standard errors clustered at the institution level.

* $p < .10$.
** $p < .05$.
*** $p < .01$.

shows the means of each outcome in the 2004–6 cohorts at UERJ; we define 2004–6 as the pre-AA period since these Enade cohorts typically enrolled in UERJ prior to 2003. Column 2 displays DD estimates for all exam takers, which are the coefficients on an indicator for UERJ interacted with an indicator for the post-AA cohorts (2007–15).[24] The Enade data are not linked to our UERJ records at the individual level, so we cannot estimate this regression in our top enrollee sample. As an alternative, column 3 restricts the sample to white students from private high schools, who were not eligible for affirmative action. Column 4 presents results for nonwhite and/or public high school exam takers.

Panel A of table 7 shows that affirmative action increased the diversity of UERJ's Enade exam takers, but we do not find compositional changes within the sample of white private school students. The mean number of UERJ exam takers per program/cohort (36 students) did not change significantly with affirmative action (first row of panel A, col. 2), but there were 7.9 fewer white private student students on average (col. 3) and 9.1 more nonwhite and/or public school students (col. 4). Affirmative action significantly increased the racial and socioeconomic diversity of the average UERJ Enade participant (col. 2). However, we do not find significant changes in gender, age, mother's education, or household income within the sample of white private school students (col. 3). The relative change in the composition of UERJ's white private school students is close to zero using an index of predicted Enade scores based on demographic characteristics (last row of panel A).

Panel B of table 7 shows that affirmative action decreased the Enade scores of UERJ students, including within the sample of white private school students. Enade scores are expressed as the proportion of correct answers, and the overall score is a weighted average of its field-specific and general components. For the average UERJ exam taker, affirmative action reduced the proportion of correct answers by 3.8 percentage points (col. 2) from a pre-AA mean of 55% (col. A). This average effect is likely due in part to the policy's effects on diversity. Yet the overall scores of UERJ's white private school students also declined by 2.2 percentage points (col. C). This decline is 15% of a standard deviation of the full distribution of Enade scores (14.4 percentage points). Similarly, figure 6 shows that Enade performance declined

[24] Our DD specification for table 7 is

$$Y_{mjt} = \gamma_{mj} + \gamma_{mt} + \pi[\mathrm{UERJ}_j \times \mathrm{Post}_t] + \varepsilon_{mjt}. \quad (4)$$

Regressions are at the exam field (m) by institution (j) by year (t) level, with observations weighted by the number of exam takers. We include field $\times$ institution dummies, γ_{mj}, and field $\times$ year dummies, γ_{mt}. Thus, identification comes only from within-field comparisons. The coefficient of interest, π, is on an indicator for UERJ interacted with an indicator for the 2007–15 cohorts, $\mathrm{UERJ}_j \times \mathrm{Post}_t$.

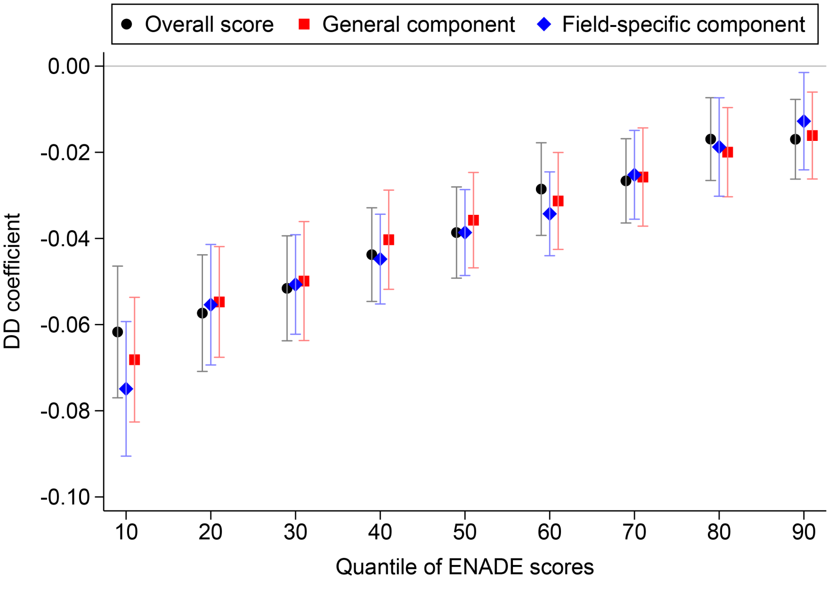

FIG. 6.—Effects of affirmative action at UERJ on quantiles of Enade scores. This figure displays DD estimates of the effect of UERJ's affirmative action policy on quantiles of its graduates' Enade exam scores. These estimate are similar to those in panel B of table 7, but the dependent variables are quantiles of Enade scores within each institution × program × cohort cell. Markers depict the DD coefficient (y-axis) for each quantile (x-axis). Vertical bars are 95% confidence intervals computed using standard errors clustered at the institution level.

by about 2 percentage points at the highest quantiles of UERJ's score distribution.[25] We also find that Enade scores declined in UERJ majors with more exposure to affirmative action relative to majors with less exposure (table A18).

These findings suggest that affirmative action reduced the learning of UERJ's top students. At high quantiles and in the white private school sample, the declines in Enade performance are not likely to be driven by compositional effects. We find no evidence of negative selection in the sample of white private school students, and all else equal, one would expect positive selection within this sample because the bar for admission was higher in cohorts with affirmative action. Thus, these results suggest that the negative effects of affirmative action on top enrollees' earnings were partly driven by learning spillovers.

[25] Figure 6 plots DD coefficients in which the dependent variables are quantiles of Enade scores within each exam field × institution × year cell (rather than mean scores, as in table 7).

E. Discussion

Our point estimates imply that a 1 percentage point increase in the affirmative action share led to a 0.7% decrease in the wages of UERJ's highly ranked students. Thus, the negative effects on top enrollees' earnings were large in majors with the highest exposure to affirmative action. These spillover results are consistent with our RD analysis, which found that general applicants in the post-AA cohorts had a negative early-career earnings return to attending UERJ (table 2).

These spillover effects were driven by a combination of compositional, networking, and learning mechanisms. Although the DD estimates for top enrollee composition are not statistically significant, the point estimate for the log wage index in panel D of table 5 (-0.033) is 25% of our main effect on log wages (-0.132). By combining the DD estimates for access to alumni firms (panels C, D of table 6) with the OLS wage premia for these jobs (table A12), network mechanisms can explain 10%–17% of the overall wage effect. Our Enade dataset is not linked to wages, but Reyes (2023) finds that a 1 percentage point increase in the proportion of correct answers on Brazil's national college entrance exam (ENEM) is associated with a 0.02 log point increase in early-career wages. Assuming that the relationship between correct answers and wages is the same on the Enade exam, the decline in overall scores for white private school students in panel B of table 7 (2.2 percentage points) can explain 32% of the overall wage effect. Taken together, these compositional, networking, and learning effects explain two-thirds of the decrease in top enrollees' hourly wages. (See sec. B.6 of the appendix for details.)

Furthermore, UERJ's adoption of affirmative action reduced the earnings of highly ranked URM students. High-scoring URM students were likely affected by networking and learning spillover effects in the same way as other top enrollees. They may also have faced statistical discrimination from employers when the URM share of the student body increased (Coate and Loury 1993). Thus, our results suggest that highly ranked URM students may have been better off if UERJ's affirmative action policy had been smaller in scale.

VII. Conclusion

This paper documented a trade-off between the direct and spillover effects of affirmative action at UERJ. On the one hand, marginally admitted Black and low-income students who attended UERJ as a result of affirmative action experienced a 14% increase in early-career earnings. This earnings gain was driven not by educational attainment but rather by increased access to high-paying firms affiliated with UERJ alumni. This suggests that the primary benefit of affirmative action at elite universities may be to help disadvantaged students gain access to job networks in high-wage sectors of the economy. Yet we found some evidence that affirmative action students' earnings and networking gains decreased as their careers progressed, suggesting that they faced additional barriers to career advancement in the labor market.

On the other hand, UERJ's affirmative action policy had negative impacts on the careers of its other students, including highly ranked URM students. Our results suggest that a 19 percentage point increase in the share of students admitted through affirmative action led to a 14% decrease in the wages of UERJ's top students. This earnings effect may have been due in part to a change in the composition of UERJ's top students, but it was also driven by negative spillover effects on their learning and a decline in the value of peer networking. These results can explain why elite schools around the world use admission policies that favor high-achieving and wealthy students (Arcidiacono, Kinsler, and Ransom 2022) and why they may be hesitant to unilaterally adopt affirmative action at a large scale.

Our paper shows that elite universities face a trade-off between serving as engines of upward mobility for disadvantaged students and maintaining sources of their value added that stem from admitting high-achieving and wealthy students. An important caveat is that we do not examine nonpecuniary benefits of interacting with classmates from diverse backgrounds (e.g., Carrell, Hoekstra, and West 2019), which can further justify the adoption of large-scale affirmative action.

References

Abdulkadiroğlu, Atila, Parag A. Pathak, Jonathan Schellenberg, and Christopher R. Walters. 2020. Do parents value school effectiveness? *American Economic Review* 110, no. 5:1502–39.

Andrews, Rodney J., Jing Li, and Michael F. Lovenheim. 2016. Quantile treatment effects of college quality on earnings. *Journal of Human Resources* 51, no. 1:200–238.

Anelli, Massimo. 2020. The returns to elite university education: A quasi-experimental analysis. *Journal of the European Economic Association* 18, no. 6:2824–68.

Angrist, Joshua D., Guido W. Imbens, and Donald B. Rubin. 1996. Identification of causal effects using instrumental variables. *Journal of the American Statistical Association* 91, no. 434:444–55.

Arcidiacono, Peter, Esteban M. Aucejo, and V. Joseph Hotz. 2016. University differences in the graduation of minorities in STEM fields: Evidence from California. *American Economic Review* 106, no. 3:525–62.

Arcidiacono, Peter, Josh Kinsler, and Tyler Ransom. 2022. Legacy and athlete preferences at Harvard. *Journal of Labor Economics* 40, no. 1:133–56.

Arcidiacono, Peter, Michael Lovenheim, and Maria Zhu. 2015. Affirmative action in undergraduate education. *Annual Review of Economics* 7, no. 1:487–518.

Arcidiacono, Peter, and Jacob L. Vigdor. 2010. Does the river spill over? Estimating the economic returns to attending a racially diverse college. *Economic Inquiry* 48, no. 3:537–57.

Backes, Ben. 2012. Do affirmative action bans lower minority college enrollment and attainment? Evidence from statewide bans. *Journal of Human Resources* 47, no. 2:435–55.

Bagde, Surendrakumar, Dennis Epple, and Lowell Taylor. 2016. Does affirmative action work? Caste, gender, college quality, and academic success in India. *American Economic Review* 106, no. 6:1495–521.

Bertrand, Marianne, Rema Hanna, and Sendhil Mullainathan. 2010. Affirmative action in education: Evidence from engineering college admissions in India. *Journal of Public Economics* 94, no. 1:16–29.

Black, Sandra E., Jeffrey T. Denning, and Jesse Rothstein. 2023. Winners and losers? The effect of gaining and losing access to selective colleges on education and labor market outcomes. *American Economic Journal: Applied Economics* 15, no. 1:26–67.

Bleemer, Zachary. 2021. Top percent policies and the return to postsecondary selectivity. Research and Occasional Paper Series no. CSHE.1.2021.

———. 2022. Affirmative action, mismatch, and economic mobility after California's Proposition 209. *Quarterly Journal of Economics* 137, no. 1:115–60.

Calonico, Sebastian, Matias D. Cattaneo, and Rocio Titiunik. 2014. Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica* 82, no. 6:2295–326.

Calvo-Armengol, Antoni, and Matthew O. Jackson. 2004. The effects of social networks on employment and inequality. *American Economic Review* 94, no. 3:426–54.

Canaan, Serena, and Pierre Mouganie. 2018. Returns to education quality for low-skilled students: Evidence from a discontinuity. *Journal of Labor Economics* 36, no. 2:395–436.

Carrell, Scott E., Mark Hoekstra, and James E. West. 2019. The impact of college diversity on behavior toward minorities. *American Economic Journal: Economic Policy* 11, no. 4:159–82.

Carrell, Scott E., Bruce I. Sacerdote, and James E. West. 2013. From natural variation to optimal policy? The importance of endogenous peer group formation. *Econometrica* 81, no. 3:855–82.

Chetty, Raj, John N. Friedman, Emmanuel Saez, Nicholas Turner, and Danny Yagan. 2020. Income segregation and intergenerational mobility across colleges in the United States. *Quarterly Journal of Economics* 135, no. 3:1567–633.

Coate, Stephen, and Glenn C. Loury. 1993. Will affirmative-action policies eliminate negative stereotypes? *American Economic Review* 83, no. 5:1220–40.

Cortes, Kalena E. 2010. Do bans on affirmative action hurt minority students? Evidence from the Texas Top 10% Plan. *Economics of Education Review* 29, no. 6:1110–24.

Dale, Stacy Berg, and Alan B. Krueger. 2002. Estimating the payoff to attending a more selective college: An application of selection on observables and unobservables. *Quarterly Journal of Economics* 117, no. 4:1491–527.

Daniel, Kermit, Dan A. Black, and Jeffrey Smith. 2001. Racial differences in the effects of college quality and student body diversity on wages. In *Diversity challenged: Evidence on the impact of affirmative action*, 221–231. Cambridge, MA: Harvard Publishing Group.

Dillon, Eleanor Wiske, and Jeffrey Andrew Smith. 2020. The consequences of academic match between students and colleges. *Journal of Human Resources* 55. no. 3:767–808.

Duflo, Esther, Pascaline Dupas, and Michael Kremer. 2011. Peer effects, teacher incentives, and the impact of tracking: Evidence from a randomized evaluation in Kenya. *American Economic Review* 101, no. 5:1739–74.

Durlauf, Steven N. 2008. Affirmative action, meritocracy, and efficiency. *Politics, Philosophy and Economics* 7, no. 2:131–58.

Estevan, Fernanda, Thomas Gall, and Louis-Philippe Morin. 2019. Redistribution without distortion: Evidence from an affirmative action programme at a large Brazilian university. *Economic Journal* 129, no. 619: 1182–220.

Ferman, Bruno, and Juliano Assunção. 2005. Affirmative action in university admissions and high school students' proficiency. XXVii Encontro Brasileiro de Econometria. Anais.

Francis, Andrew M., and Maria Tannuri-Pianto. 2012. Using Brazil's racial continuum to examine the short-term effects of affirmative action in higher education. *Journal of Human Resources* 47, no. 3:754–84.

Francis-Tan, Andrew, and Maria Tannuri-Pianto. 2018. Black movement: Using discontinuities in admissions to study the effects of college quality and affirmative action. *Journal of Development Economics* 135:97–116.

Hastings, Justine S., Christopher A. Neilson, and Seth D. Zimmerman. 2013. Are some degrees worth more than others? Evidence from college admission cutoffs in Chile. NBER Working Paper no. 19241, National Bureau of Economic Research, Cambridge, MA.

Hinrichs, Peter. 2011. The effects of attending a diverse college. *Economics of Education Review* 30, no. 2:332–41.

———. 2012. The effects of affirmative action bans on college enrollment, educational attainment, and the demographic composition of universities. *Review of Economics and Statistics* 94, no. 3:712–22.

Hoekstra, Mark. 2009. The effect of attending the flagship state university on earnings: A discontinuity-based approach. *Review of Economics and Statistics* 91, no. 4:717–24.

Hoxby, Caroline M. 2018. The productivity of U.S. postsecondary institutions. In *Productivity in higher education*, ed. Caroline M. Hoxby and Kevin Stange. Cambridge, MA: National Bureau of Economic Research.

Hoxby, Caroline M., and Christopher Avery. 2013. Missing one-offs: The hidden supply of high-achieving, low-income students. Brookings Papers on Economic Activity.

INEP (Instituto Nacional de Estudos e Pesquisas Educacionais Anísio Teixeira). 2019. Microdata for Higher Education Census (Censo da Educação Superior). INEP, Brasília. https://www.gov.br/inep/ (accessed June 2019).

———. 2022. Microdata for Enade exam (Exame Nacional de Desempenho dos Estudantes). INEP, Brasília. https://www.gov.br/inep/ (accessed May 2022).

Júnior, João Feres, and Verônica Toste Daflon. 2014. Políticas da igualdade racial no ensino superior. *Cadernos do Desenvolvimento Fluminense* 5:31–44.

Kapor, Adam. 2015. Distributional effects of race-blind affirmative action. Working paper.

Kirkebøen, Lars, Edwin Leuven, and Magne Mogstad. 2016. Field of study, earnings, and self-selection. *Quarterly Journal of Economics* 131, no. 3:1057–111.

Machado, Cecilia, and Christiane Szerman. 2021. Centralized college admissions and student composition. *Economics of Education Review* 85:102184.

MacLeod, W. Bentley, Evan Riehl, Juan E. Saavedra, and Miguel Urquiola. 2017. The big sort: College reputation and labor market outcomes. *American Economic Journal: Applied Economics* 9, no. 3:223–61.

MacLeod, W. Bentley, and Miguel Urquiola. 2015. Reputation and school competition. *American Economic Review* 105, no. 11:3471–88.

Marmaros, David, and Bruce Sacerdote. 2002. Peer and social networks in job search. *European Economic Review* 46, 4/5:870–79.

Mayer, Adalbert, and Steven L. Puller. 2008. The old boy (and girl) network: Social network formation on university campuses. *Journal of Public Economics* 92, no. 1/2:329–47.

McCrary, Justin. 2008. Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of Econometrics* 142, no. 2:698–714.

Mello, Ursula. 2022. Centralized admissions, affirmative action, and access of low-income students to higher education. *American Economic Journal: Economic Policy* 14, no. 3:16697.

Michelman, Valerie, Joseph Price, and Seth D. Zimmerman. 2022. Old boys's clubs and upward mobility among the educational elite. *Quarterly Journal of Economics* 137, no. 2:845–909.

Mocanu, Tatiana. 2022. Designing gender equity: Evidence from hiring practices and committees. Working paper.

Mountjoy, Jack, and Brent Hickman. 2020. The returns to college(s): Estimating value-added and match effects in higher education. Becker Friedman Institute for Economics working paper, University of Chicago.

Ng, Kevin, and Evan Riehl. 2024. The returns to STEM programs for less-prepared students. *American Economic Journal: Economic Policy* 16, no. 2:37–77.

Otero, Sebastián, Nano Barahona, and Cauê Dobbin. 2021. Affirmative action in centralized college admission systems: Evidence from Brazil. Working paper.

Pedrosa, Renato H. L., Eliana Amaral, and Marcello Knobel. 2013. Assessing higher education learning outcomes in Brazil. *Higher Education Management and Policy* 24, no. 2:55–71.

RAIS (Relação Anual de Informações Sociais). 2021. Ministério do Trabalho e Emprego, Brasília. http://www.rais.gov.br/ (accessed May 2021).

Reyes, Germán. 2023. Cognitive endurance, talent selection, and the labor market returns to human capital. arXiv, https://doi.org/10.48550/arXiv.2301.02575.

Ribeiro, Ana Carolina Trindade. 2016. Affirmative action outcomes: Evidence from a law school in Brazil. Department of Economics, FEA-USP Working Paper no. 2016-43.

Ribeiro, Ana Trindade, and Fernanda Estevan. 2021. Long-term outcomes of affirmative action: Evidence from a selective law school in Brazil. Working paper.

Riehl, Evan. 2024. Do less informative college admission exams reduce earnings inequality? Evidence from Colombia. *Journal of Labor Economics* 42, no. 4:1009–47.

Rivera, Lauren A. 2016. *Pedigree: how elite students get elite jobs*. Princeton, NJ: Princeton University Press.

Saavedra, Juan Esteban. 2009. The returns to college quality: A regression discontinuity analysis. Working paper, Harvard University.

Sekhri, Sheetal. 2020. Prestige matters: Wage premium and value addition in elite colleges. *American Economic Journal: Applied Economics* 12, no. 3:207–25.

Smith, Jonathan, Joshua Goodman, and Michael Hurwitz. 2020. The economic impact of access to public four-year colleges. NBER Working Paper no. 27177, National Bureau of Economic Research, Cambridge, MA.

UERJ (Universidade do Estado do Rio de Janeiro). 2020a. Applicants to UERJ. UERJ, Rio de Janeiro. https://www.uerj.br/ (accessed January 2020).

———. 2020b. Enrollees in UERJ. UERJ, Rio de Janeiro. https://www.uerj.br/ (accessed January 2020).

Vieira, Renato Schwambach, and Mary Arends-Kuenning. 2019. Affirmative action in Brazilian universities: Effects on the enrollment of targeted groups. *Economics of Education Review* 73:101931.

Weinstein, Russell. 2018. Employer screening costs, recruiting strategies, and labor market outcomes: An equilibrium analysis of on-campus recruiting. *Labour Economics* 55:282299.

———. 2022. Firm decisions and variation across universities in access to high-wage jobs: Evidence from employer recruiting. *Journal of Labor Economics* 40, no. 1:1–46.

Zhu, Maria. 2025. Beyond the "old boys' network": Social networks and job finding at community colleges. *Journal of Human Resources*. Forthcoming.

Zimmerman, Seth D. 2014. The returns to college admission for academically marginal students. *Journal of Labor Economics* 32, 4:711–54.
———. 2019. Elite colleges and upward mobility to top jobs and top incomes. *American Economic Review* 109, no. 1:1–47.
Zoninsein, Jonas, and João Feres Júnior. 2008. Ação afirmativa no ensino superior brasileiro. Editora UFMG.