AFFIRMATIVE ACTION IN CENTRALIZED COLLEGE ADMISSIONS SYSTEMS

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We study the consequences of affirmative action in centralized college admissions systems. We develop an empirical framework to examine the effects of a large-scale program in Brazil that required all federal institutions to reserve half their seats for socioeconomically and racially marginalized groups. By exploiting admissions cutoffs, we find that marginally benefited students are more likely to attend college and are enrolled at higher-quality degrees four years later. Meanwhile, there are no observed impacts for marginally displaced non-targeted students. To study the effects of larger changes in affirmative action, we estimate a joint model of school choices and potential outcomes. We find that the policy has impacts on college attendance and persistence that imply a virtually one-to-one income transfer from the non-targeted to the targeted group. These findings indicate that introducing affirmative action can increase equity without affecting efficiency.

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This draft: September 4, 2023. We are indebted to Pascaline Dupas, Liran Einav, Matthew Gentzkow, Caroline Hoxby, and Paulo Somaini for invaluable guidance and support on this project. We also thank Claudia Allende, Luis Armona, Zachary Bleemer, Nick Bloom, Christopher Campos, Arun Chandrasekhar, José Ignacio Cuesta, Michael Dinerstein, Marcel Fafchamps, Diego Jiménez, Patrick Kline, Tomás Larroucau, Neale Mahoney, Ursula Mello, Melanie Morten, Christopher Neilson, Christopher Walters, and seminar participants at Stanford University for valuable comments and suggestions. We are grateful to Anna Popova for her insights, patience, and dedication to making this article better. We gratefully acknowledge financial support from the Stanford King Center on Global Development, Stanford Center for Computational Social Sciences, Stanford Institute for Economic Policy Research (SIEPR), Haley-Shaw Dissertation Fellowship, and NAEd/Spencer Dissertation Fellowship Program.

1. INTRODUCTION

About 25% of countries worldwide use some form of affirmative action (AA) in an effort to increase the participation of underrepresented groups in higher education (Jenkins and Moses, 2017). These policies work by giving preferential treatment to applicants from marginalized backgrounds. However, prioritizing one group over another renders AA one of the most controversial regulations in education policy (Arcidiacono and Lovenheim, 2016).[1] The debate surrounding the value of AA centers on a potential trade-off between equity and efficiency. Equity arguments highlight the role of AA in reinforcing the equalizing role of colleges and promoting diversity as a pillar of a healthy and sustainable democracy (Singer, 2011, Alon, 2015, Chetty et al., 2020). Conversely, at the heart of the efficiency discussion is the fact that AA pushes targeted students into selective degree programs by displacing allegedly more deserving candidates.

There is contention on whether AA results in efficiency gains because economic theory fails to provide unambiguous predictions of its consequences. On the one hand, AA places low-performing targeted students in more selective programs and high-performing non-targeted students in less selective programs. Thus, in the presence of complementarities between college selectivity and academic preparedness, AA would reduce the allocative efficiency of the system (Ellison and Pathak, 2021). Moreover, AA policies may even harm targeted students by placing them in schools for which they are ill-prepared (also known as the "mismatch hypothesis") (Sander, 2004). On the other hand, AA could increase efficiency if displaced individuals have access to alternative high-quality private colleges not affordable for targeted students, or if test scores are statistically biased against the targeted group (Jacob and Rothstein, 2016). Quantifying AA's efficiency effects is thus an empirical question, but evidence on its impacts is limited.

This paper endeavors to fill this gap by estimating the effects of AA on the academic and implied labor market outcomes of winners and losers. To do this, we leverage a large-scale AA regulation implemented in Brazil's centralized college admissions system between 2013 and 2016—one of the largest and most radical AA programs ever implemented in the world (Sales and Moses, 2014). The regulation mandated federal universities to reserve 50% of seats in every degree program for students from public high schools ("targeted students"). The goal was to in-

[1] AA in the United States has been a hotly debated topic. Recently, the Supreme Court deemed race-conscious admissions policies at Harvard and the University of North Carolina unconstitutional. The ruling has sparked discussions on how to promote diversity and fairness in college admissions without compromising merit.

crease the representation of these groups closer to the 81% share of the population for which they account. Among public high school students, the policy heavily targeted those from low-income and marginalized racial groups. After the policy was fully implemented, the share of public high school students and public high school students of color admitted to federal degrees in the top decile of selectivity increased by 55% and 95%, respectively.

We examine whether the policy resulted in gains or losses for targeted students and compare these to those of displaced students to assess the impacts on overall educational output. To answer these questions, we use detailed administrative education data, including students' performance on the national university entrance exam, application portfolios to public universities, and admissions offers from these institutions. We combine these data with the Brazilian Higher Education Census to track students' progress and academic paths across the universe of degree programs (including private colleges not participating in the centralized system) over time. Finally, we use matched employer-employee records of pre-policy cohorts to estimate degree-specific labor market returns. This allows us to assess the implied impact of the AA policy on students' income. Overall, these datasets provide a rich set of outcomes and a comprehensive characterization of all individuals potentially affected by the AA policy.

Brazil is an ideal setting to study AA for two reasons. First, a major challenge in studying AA is that researchers seldom have access to detailed information on how college admissions decisions are made, let alone on who was benefited or displaced by AA. Brazil's AA policy allows us to overcome this difficulty because it is embedded in a centralized system, which produces transparent application data and clear admissions rules. Students are admitted to a given degree program if they score above the admissions cutoff for that degree and type of seat (reserved or open). Second, federal institutions are highly selective and segregated. Federal universities play a role similar to that of flagship state universities in the United States, as they are typically the most prominent, elite, and selective universities in their respective states. These institutions are both free and more prestigious than most of their private counterparts, which makes them attractive to high-performing students from both low and high socioeconomic backgrounds.

We begin by estimating the effects of AA on academic outcomes for marginally benefited and marginally displaced students. While several studies have examined college returns for students on the margin of admissions (e.g., Hoekstra, 2009, Zimmerman, 2014), we exploit the fact that reserved and open seats have different admissions cutoffs to construct different regression discontinuity estimates for individuals entering through each type of seat. Contrary to the predictions of the mismatch hypothesis, we find that gaining admission to a federal degree through a reserved seat substantially improves the academic outcomes of targeted students. Marginally benefited targeted students increase their college enrollment and the quality of the degree where they are enrolled 4 years later. In contrast, we calculate small and statistically insignificant losses for marginally displaced non-targeted students. This result is mainly explained by differential access to high-quality degrees outside the federal system (i.e., outside options) across AA groups.

While the reduced-form results provide credible evidence for marginally affected students, they do not fully capture the complete impact of the policy. Understanding the overall impacts of the AA policy also requires investigating the consequences for affected students who score further away from the admissions cutoffs. Furthermore, it also requires accounting for the equilibrium allocations and admissions cutoffs that arise from the overall reallocation of students across institutions and degree programs (i.e., knock-on effects). With this goal in mind, we develop a joint model of degree choices and potential outcomes that characterizes the impacts of AA in a centralized admissions system.

Bringing our model to the data presents two challenges. The first is determining the degree programs that students would enroll in with and without the policy. To do this, we use information on preferences contained in the centralized platform and summarize student preferences by fitting random utility models to the application behavior of students for each AA group. The second challenge is estimating the outcomes that would be realized under the counterfactual degree assignments. Estimating the treatment effects of attending a given degree is complicated due to selection bias; that is, potential outcomes could vary for students with different unobserved tastes for degrees in a way not captured by students' observables. To address this identification issue, we control for a rich set of covariates and also implement a selection correction approach using the multinomial logit control function estimator of Dubin and McFadden (1984) and Abdulkadiroglu et al. (2020). Our model accommodates a variety of unobserved selection schemes, including selection based on student- and degree-specific unobserved matching effects.

To identify our model, we rely on an exogenous score shifter that mimics random shocks to student test scores. The score shifter interacts with degree admissions thresholds to alter the degree programs available to otherwise identical individuals. The identification assumption is that while score shifters affect degree availability, they do not enter into the potential outcome equation. The score shifter exploits two exogenous sources of variation in test scores. The first source stems from random assignment to essay graders of varying strictness. The second source arises from plausibly exogenous assignment to multiple-choice examination booklets of varying difficulty. We use these to create a leave-one-out measure that characterizes the overall exam difficulty faced by a given student. We show that the score shifter is uncorrelated with student characteristics but is highly correlated with test scores. Moving from the 5th to the 95th percentile of the distribution of the score shifter increases test scores by 0.24 standard deviations. In addition, receiving a positive score shifter is strongly associated with a higher probability of attending a federal degree and with attending a higher-quality degree within the federal system.

Our goal is to estimate the overall impact of AA on students' lifetime earnings. Because we do not observe earnings for the impacted cohorts, we assess the effects of the AA policy on projected lifetime earnings, which we construct as an aggregator of intermediate academic outcomes (Angrist et al., 2022).[2] We use our model to show that the implied effects on projected earnings are equivalent to those resulting from realized earnings under a surrogacy and a comparability assumption (Athey et al., 2019). The surrogacy assumption states that realized earnings are independent of degree choice after conditioning on student covariates and academic trajectories post admissions. The comparability assumption presumes that potential outcomes are invariant to the policy. This would rule out, for instance, the fact that AA may affect the signaling value of education or the presence of peer effects in outcomes.

Our potential outcome model estimates imply that targeted individuals reap higher returns from attending federal degrees than do non-targeted individuals, especially among more selective degrees. This result is consistent with the fact that non-targeted students have access to better outside options than their targeted counterparts: Conditional on test scores, non-targeted students attend private institutions that are of higher quality and have higher tuition fees. We also find that the potential test score mismatch induced by the AA regulation is unlikely to affect outcomes.

We then use our model to estimate the consequences of a 50% AA schedule, resembling the Brazilian regulation. We simulate the allocation of students with and without the AA policy in the state of Minas Gerais and find that most of the change in degree composition occurs within the most selective degrees.[3] The AA policy differentially affects the probability of admission for targeted and non-targeted individuals by changing the admissions thresholds for reserved and

[2]Our admissions data start in 2016, and it is currently too early to observe the labor market outcomes for the individuals in our sample.

[3]We restrict the sample due to computational constraints. Minas Gerais is the largest state in terms of the number of applications.

open seats. Under the 50% AA policy, admissions thresholds for reserved seats are, on average, 35 points (or 0.47 standard deviations) lower than those for open seats.

The policy causes targeted individuals who benefit from the policy to experience large gains in academic outcomes that predict large gains in income. Meanwhile, it imposes a smaller cost on the average displaced non-targeted individuals with similar test scores. However, we also observe that the number of affected students that switch degrees as a result of the policy is 42% larger for non-targeted students than for targeted students. This difference highlights the importance of considering the knock-on effects when assessing the effects of large-scale AA.

Next, we integrate the gains and losses across all students, including those unaffected by the policy. We find that the average targeted student experiences gains of 1.02% in projected lifetime earnings relative to their baseline earnings without AA. In contrast, the average non-targeted student experiences losses equivalent to 1.34% of their baseline income. When we aggregate results across targeted and non-targeted groups, the policy prompts a negligible decrease in projected income of 0.014% for the average student. Taken together, we find that the AA policy had important distributional consequences, which resulted in almost one-to-one transfers from the non-targeted to the targeted group. These results indicate that AA can increase equity without affecting the overall efficiency of the education system.

Our paper is related to several strands of the literature. A large body of work examines whether AA creates an extreme mismatch between the academic preparedness of targeted students and the difficulty of the degrees they attend, such that it would harm them. Several papers study this "mismatch hypothesis" with varying results, depending on the construction of counterfactuals (e.g., Sander, 2004, Alon and Tienda, 2005, Rothstein and Yoon, 2008, Arcidiacono et al., 2011). Our findings are in line with recent studies that leverage quasi-experimental variation in access to college for targeted minorities in the context of public universities in California (Bleemer, 2021a); engineering colleges in India (Bertrand et al., 2010, Bagde et al., 2016); and one federal university in Brazil (Francis-Tan and Tannuri-Pianto, 2018). None of these studies find evidence of academic mismatch.

A closely connected literature examines AA's distributional and efficiency effects by studying its consequences on benefited and displaced individuals (Black et al., 2023, Bleemer, 2021a). These studies find that while benefited students increased college enrollment and earnings, marginally displaced students do not see declines in these outcomes. Similar to our paper, this finding highlights the role that the outside option plays for displaced students. We build on this research by assessing the effects of AA on all possibly affected students, taking into account the equilibrium knock-on effects of displaced students on other institutions or degree programs.

Several papers are conceptually close to ours in that they conduct structural policy analysis of college admissions (Arcidiacono, 2005, Howell, 2010, Bodoh-Creed and Hickman, 2019, Chetty et al., 2020, Kapor, 2020, Bleemer, 2021b, Chetty et al., 2023). While these papers study admissions policies in decentralized markets, we do this in a centralized system with well-defined admissions rules. The main advantage is that, unlike past research that had to rely on assumptions to infer students' AA status and explicitly model admissions, we can directly observe their status and simulate admissions based on the mechanism's rules. Two recent papers have also studied the effects of changes in centralized assignment mechanisms in college admissions on real outcomes beyond assignment (Kapor et al., 2022, Larroucau and Rios, 2022).

Finally, our work also relates to a large literature on the estimation of joint models of treatment selection and outcomes (Heckman, 1979, Dubin and McFadden, 1984, Bjorklund and Moffitt, 1987), and to literature that connects such models with IV estimators (Vytlacil, 2002, Kline and Walters, 2016, Brinch et al., 2017, Kline and Walters, 2019, Abdulkadiroglu et al., 2020). Our empirical approach is also related to recent literature that endeavors to extrapolate regression discontinuity treatment effects away from the discontinuity. Several of these studies are motivated by understanding the impacts of AA on students of varying skill levels (Rokkanen, 2015, Angrist and Rokkanen, 2015, Bertanha and Imbens, 2019). Relative to these papers, we use a selection model and identify the treatment effects for individuals away from the discontinuity by combining admissions discontinuities with a continuous instrument that shifts students' test scores.

The remainder of the paper is structured as follows. Section 2 introduces the setting and provides details of the AA policy, and Section 3 discusses the data and provides descriptive facts about Brazilian college admissions. Section 4 presents regression discontinuity evidence. Section 5 presents a model that characterizes the impacts of the AA policy. Section 6 introduces an empirical joint model of school choice and potential outcomes, and Section 7 presents parameter estimates. Section 8 estimates the effects of a 50% AA regulation, and Section 9 concludes.

2. INSTITUTIONAL BACKGROUND AND REGULATION

2.1. *Higher education in Brazil*

Brazil's higher education system is highly liberalized, and approximately 76% of incoming students enroll in private, fee-charging colleges. The remaining 24% attend public institutions.

These institutions charge no tuition fees and are associated with the federal, state, or municipal government, depending on the funding source. In 2016, there were 107 federal institutions that served nearly 1.2 million students and offered more than 5,700 different degrees. Federal institutions comprise 63 universities and 44 vocational institutions, with the former accounting for the vast majority of federal enrollment.

Federal universities are, in most cases, elite and highly selective and play a role similar to that of flagship state universities in the United States. They are typically the most prominent universities in their state and are, on average, of substantially higher quality than their private counterparts, as measured by student learning, infrastructure, and the quality of peers and faculty (Figure A.1 shows the distribution of degree quality as measured by the Ministry of Education). As a result of their prestige and tuition-free policy, federal institutions are highly oversubscribed.

As is common in many countries, prospective students apply in advance to a particular degree program: a specific major at a specific institution (i.e., Law at the University of Sao Paulo). Obtaining a postsecondary degree typically requires 3-6 years for bachelor's degrees, 4-5 years for teaching degrees, and 2-3 years for vocational degrees. In addition to selecting a degree, students must choose the instructional period ("shift" hereafter) in which to study for their degree: morning, evening, night, or full-time. Finally, it is important to note that dropout rates are high. In 2016, around 43% of students who enrolled in a federal university dropped out of that degree, and 22% dropped out of college after 4 years. Dropout rates are lower for bachelor's degrees and also for more selective degrees. As we show in Appendix Figure A.2, dropout rates converge at around 50% after an 8-year period following admission.

2.2. *College admissions: ENEM and SISU*

The national university entrance exam (ENEM) is a highly competitive standardized exam consisting of four multiple-choice tests—Math, Language, History, and Science—and one written essay. Students can use ENEM test scores to gain access to public institutions through a centralized admissions system (SISU).[4] In 2016, about 57% of the 4.8 million ENEM takers applied to a degree program in SISU.[5]

[4] In 2016, about 90% of all incoming students in federal institutions entered through the centralized system, and 95% of all federal institutions participated in it.

[5] The academic year runs from March to December. Students take the ENEM in December, apply to SISU in January, and begin their studies in March. Some degrees also open additional seats for the second round of admissions through

Students participating in SISU are matched to degree programs following an iterative deferred acceptance mechanism.[6] The system assigns students to degree programs based on their application scores, which is a weighted average of their test scores based on degree-specific weights for each ENEM component. In contrast to the standard deferred acceptance process, students are sequentially asked to submit up to two ranked program choices throughout several "trial" days. At the end of each day, the system produces a cutoff score that represents the lowest score necessary to be accepted into a specific program, and students are allowed to change their degree program preferences given the new information. The results of the last day are final and determine the admissions offers for every program. Individuals who are not offered admission to their top preference can opt-in to be waitlisted in that degree program.

2.3. *The affirmative action regulation*

In August 2012, the Brazilian federal government passed the *Law of Social Quotas* (Lei de Cotas, no. 12.711/2012), requiring all federal institutions to reserve half of their admission spots in each degree program for students coming from public high schools. This group represents 81% of all high school graduates. The regulation sought to reverse racial and income inequality in university access under the rationale that members of disadvantaged groups should not be underrepresented relative to their proportion in the population.[7] The law received strong endorsement in Congress, with just one dissenting vote, and is supported by both targeted and non-targeted populations in Brazil (Folha, 2022).

Under the regulation, only students coming from public high schools are eligible to compete for the AA vacancies, while the other half of the vacancies remain open for broad competition.[8] Reserved seats are then further distributed among students from low-income families and those

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SISU in June. Students taking the ENEM in a given year can only use their score to participate in the SISU process in the following year, on either the first or second round of applications.

[6]This is the same as the mechanism currently used in university admissions by the province of Inner Mongolia in China. See Bó and Hakimov (2022) for formal properties of the mechanism.

[7]In the 2000s, several federal and state Brazilian universities adopted socioeconomic- and race-based AA in admissions to address persistent disparities in college access. See, for example, Francis and Tannuri-Pianto (2012) and Estevan et al. (2018) for AA policies at the University of Brasilia and the University of Campinas. The Law of Social Quotas was an effort to unify these regulations.

[8]The regulation was implemented over 4 years. Starting in 2013, affected institutions were mandated to reserve a minimum of 12.5% of their vacancies for eligible students, with the minimum share increasing by 12.5 percentage points per year until it reached 50% in 2016. Compliance with the law was virtually perfect.

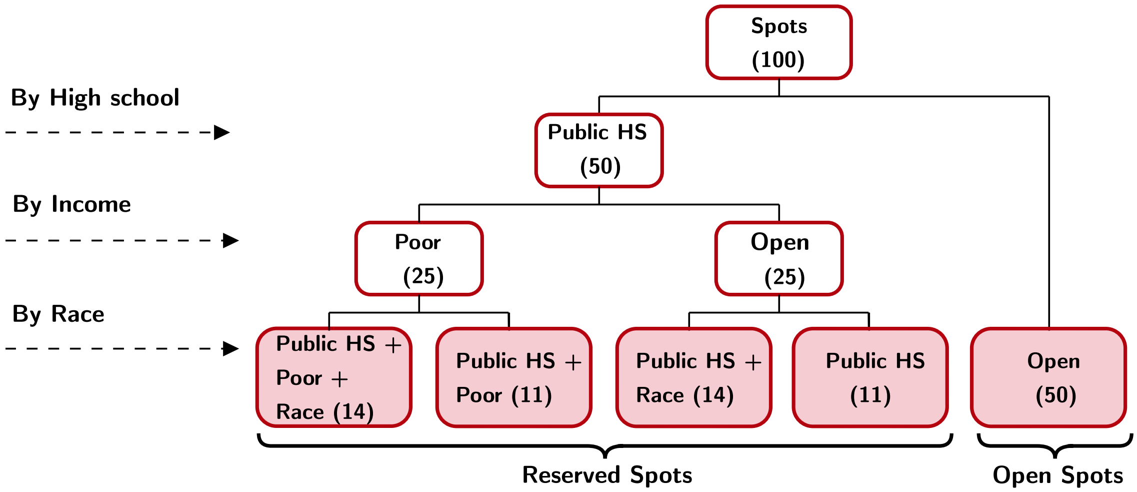


FIGURE 1.—Affirmative Action Regulation. *Notes:* Under the regulation, for every 100 spots, 50 are AA spots reserved for students from public high schools. Those AA spots are then divided equally by income, with 25 going to poor students and 25 to non-poor students. Finally, each group of 25 spots is distributed to reflect the proportion of black, brown, and indigenous individuals in the population of a given state, as reported by the Brazilian National Bureau of Statistics (IBGE). The proportions in this example correspond to Minas Gerais.

who identify as black, brown, or indigenous. Targeted students are eligible for either a reserved or an open seat, while non-targeted students can only apply for open seats. We refer to "targeted" and "non-targeted" students as the two different AA groups throughout the paper.

Figure 1 summarizes the distribution of seats under the policy by presenting the shares of AA students coming from different demographics. For every 100 university spots, 50 are AA spots for public high school graduates. Of those reserved spots, 25 go to poor students with monthly household income per capita below 1.5 times the minimum wage (about 1,500 BRL, equivalent to 310 USD), and 25 go to non-poor students. Finally, a percentage of the spots in both of the former categories is set aside for black, brown, and indigenous students in accordance with the racial makeup of each of Brazil's 27 states.[9] Overall, this results in four different AA admissions pools plus one open pool to which prospective students can apply. After applying, students are admitted to a given degree program if their application score is above the admissions cutoff specific to that degree program and admissions pool.

In the first semester of 2016, the centralized admissions system offered 200,788 spots across 4,900 degrees in 101 federal institutions. Across all admissions pools, selectivity was approxi-

[9]Brazil has a large black and mixed-race population. According to the 2010 Census, the population is 47.5% white, 43.4% brown, 7.5% black, 1.1% Asian, and 0.42% indigenous (IBGE, 2011).

mately 5%, which is comparable to many Ivy League colleges in the United States, as measured by the ratio of spots to applications.

3. DATA AND DESCRIPTIVE FACTS

3.1. *Data sources*

Test scores: We have access to test score data for all students taking the university entrance exam, ENEM. We observe these data for 2009-2015, which correspond to the 2010-2016 admission period. The data include scores on each of the components of the exam, as well as answers to a detailed socioeconomic background questionnaire.

Centralized admissions process: We complement test scores with records from the centralized admissions system, SISU. These data cover applications to federal and state institutions in 2016. We focus only on applications to federal institutions, since state institutions were not required to comply with the AA regulation. The dataset is at the application level and only includes records from the final rank-ordered list submitted by each student. For every application, we observe the preference list, the seat type requested by the applicant, and the student ranking. We also observe the students offered admission and the admissions cutoff for every degree program and admissions pool combination.

Higher education census: The third dataset we use in our analysis is the Brazilian Higher Education Census. It contains information on every student enrolled in any degree program, which allows us to observe the educational path of every student between 2009 and 2019. The data are at the degree-student level and include graduation, dropout, or current enrollment status at the end of the academic year. These data are of high quality since most institutions have systems that are integrated with the census in real time.

Matched employer-employee data: Finally, we combine the previous data sources with matched employer-employee annual administrative records (also known as RAIS) from the Ministry of Labor. This is considered to be a high-quality census of the Brazilian formal labor market. We use these data to produce earning profiles for every degree and student type. We use these estimates to develop a measure of predicted income for the 2016 cohort.

3.2. *Sample and descriptive statistics*

We concentrate our analysis on students applying to federal universities in SISU during the first semester of 2016. We exclude art programs that require additional exams beyond ENEM, as well as military programs that are not obligated to follow the law. To ensure consistency in the data, we limit our sample to open seats and to the four quotas mandated by the regulation and remove students who applied through other university-specific AA initiatives. Our final dataset consists of 2,185,817 students, which corresponds to 87% of all applicants to federal institutions. Appendix Table A.I presents summary statistics on the number of spots offered in the system and the number of applications by each admissions pool.

Table I shows mean characteristics for all SISU applicants (column 1), the subset of those students who were offered admission (column 2), and the subsample of students admitted by each admissions pool (columns 3-8). Admitted students are less likely to have attended a public high school, less likely to identify with a marginalized racial group, and more likely to come from affluent households than the applicant pool. This indicates that, despite the implementation of the AA regulation, the policy's intended beneficiaries were still underrepresented relative to their representation in the overall population. Also, female admitted students (45%) are substantially underrepresented relative to their applicant pool (57%).

The average ENEM score across all subjects is 535 points, and the standard deviation is 74.8 points. As expected, students admitted through open seats (column 3) score higher on ENEM than those admitted through reserved seats (column 4), with the latter group having an average of 45.5 fewer points (or a 0.61 standard deviation) in their SISU application scores. We do not observe significant differences in the number of ranked options between the two groups. Students admitted through reserved seats are 0.74 years older and are less likely to have just graduated from high school (35% compared with 42% for open seats), which likely contributes to their higher college enrollment the previous year.

The characteristics of applicants for reserved seats are consistent with the type of seat they apply for, with nearly all (98%) having attended a public high school. In addition, the vast majority of applicants using seats reserved for marginalized racial groups (columns 7 and 8) identify as people of color (95%), while those using income-restricted seats (columns 6 and 8) have substantially lower household incomes than applicants from other admissions pools.[10]

[10]Note that these variables are based on self-reported data from ENEM and are not binding for how reserved seats are assigned, so there may be some measurement error.

TABLE I

SUMMARY STATISTICS

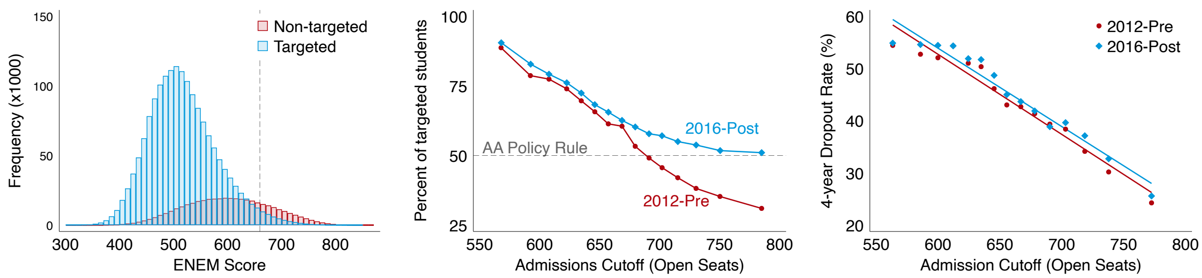
			All Admits					
					Reserved Seats			
	All Apps (1)	All Admits (2)	Open Seats (3)	Reserved Seats (4)	Public HS (5)	Income (6)	Race (7)	Income + Race (8)
Traits targeted by the policy								
Public HS	0.80	0.61	0.19	0.98	0.99	0.99	0.99	0.97
Brown, black, indig.	0.60	0.51	0.36	0.65	0.23	0.24	0.95	0.94
Household income	2,316	4,098	5,827	2,448	3,853	1,737	2,892	1,568
SISU-related variables								
# options ranked	1.78	1.78	1.78	1.78	1.77	1.79	1.79	1.78
Application scores	536.3	653.8	677.1	631.6	652.7	632.7	628.8	619.1
ENEM scores								
Natural science	497.3	594.7	618.8	571.8	592.2	572.4	569.4	559.6
Social science	578.1	656.7	668.8	645.2	657.5	645.1	644.6	637.3
Language	524.8	600.9	613.5	588.8	602.7	590.1	587.1	580.0
Math	493.6	650.4	688.0	614.5	653.6	618.6	608.8	590.5
Writing essay	579.7	741.0	769.6	713.7	730.0	711.5	712.1	705.7
Other student traits								
Female	0.57	0.45	0.46	0.44	0.42	0.46	0.42	0.45
Age	22.51	20.93	20.56	21.30	21.50	20.63	21.93	21.02
HS graduate in t-1	0.33	0.38	0.42	0.35	0.37	0.37	0.33	0.34
Married	0.11	0.07	0.06	0.08	0.08	0.06	0.10	0.06
Higher education in t-1								
Private	0.13	0.10	0.08	0.13	0.09	0.15	0.11	0.15
Federal	0.06	0.14	0.14	0.15	0.14	0.12	0.18	0.15
Any	0.21	0.26	0.23	0.29	0.25	0.28	0.31	0.32
Number of students	2,185,817	151,083	73,739	77,344	15,719	16,451	22,173	23,001

Note: Elaboration based on SISU microdata from applications in the first semester of 2016. We restrict the data to federal universities, as described in the main text. This table shows the demographic characteristics of applicants and admitted students for each of the different admissions pools. Admissions are defined as having received an admissions offer in the first round of application.

3.3. *Four key facts about test scores, college admissions, and dropout rates*

We highlight four empirical facts in the data.

Fact 1: Large test score differences by targeted status. A primary motivation behind the implementation of the AA regulation is that ENEM test scores are strongly correlated with socioeconomic status and high school demographics. Targeted individuals represent 80% of the 2.2 million SISU applicants in our sample. Panel (a) in Figure 2 shows the distribution of ENEM scores by targeted status. The average targeted student scored 517 points, while the average non-targeted student scored 600 points. This 83-point difference represents a 1.1 standard deviation difference in exam performance across targeted and non-targeted groups. As a result, targeted students represent only 29% of all ENEM takers above the median admissions cutoff for open seats in federal degrees (657 points and denoted by the gray dashed line).



(a) ENEM score distributions (b) Student body composition (c) Dropout rates

FIGURE 2.—ENEM scores, student body, and dropout rates. *Notes:* Panel (a) shows the histogram of test scores of targeted and non-targeted students among all 2016 SISU applicants in our sample. The average score is 535 points, and the standard deviation is 74.8 points. The gray line denotes the median admissions cutoff for open seats. Panel (b) presents the average share of incoming targeted students in federal institutions by degree selectivity, as defined by the admissions cutoff for open seats in 2016 SISU. The dashed line depicts the 50% AA policy rule mandated by the regulation. Panel (c) shows the average dropout rates by degree selectivity. In Panels (b) and (c), an observation is defined as a degree and shift tuple that we observe in our relevant sample. We weigh each observation by the size of the incoming cohort. We keep degree programs that existed in 2012 and 2016 and participated in the 2016 SISU.

Fact 2: Lower admissions cutoffs for targeted students. We document that the AA policy lowered admissions cutoffs for targeted students. Appendix Table A.I, columns (6)-(9) present the admissions cutoffs for open and reserved seats in different quantiles, and Appendix Figure A.3 plots the distribution of cutoffs as well as the differences in cutoffs between open and reserved seats across all degrees. The difference between cutoffs for open and reserved seats is 41.4 points on average, with wide heterogeneity across degree programs (we construct the average cutoff for reserved seats as the average cutoff across all four types of reserved seats weighted by the number of seats). The differences in admissions cutoffs helped to partially mitigate the differences in ENEM test scores between targeted and non-targeted students.

Fact 3: Large change in the student body composition but no change in dropout rates. The student body composition of federal institutions became more diverse after the implementation of the AA policy, driven by an increase in the representation of targeted students. Panel (b) in Figure 2 presents the average share of targeted students in federal institutions by degree selectivity. In 2012, one year before the AA regulation was introduced, public high school graduates represented only 58% of admissions to federal universities. In 2016, when the AA policy was fully in place, this share increased to 66%—closer to the 80% share of public high school graduates in our sample.[11] Most of the change was driven by an increase in the share of targeted students admitted to degrees above the 50th percentile of selectivity. The share of public high school graduates admitted to the top 10% most selective federal degree programs increased from 33% in 2012 to 51% in 2016, and the share of public high school graduates of color increased from 13% to 26%. Remarkably, despite the reduction in admissions cutoffs for targeted students and the resulting shift in the student body's composition, the implementation of the policy did not lead to an increase in dropout rates. Panel (c) in Figure 2 shows stable dropout rates based on degree selectivity before and after the law's enactment.

Fact 4: Non-targeted students have access to better outside options. In the Brazilian context, the choices students have outside the federal system are substantially different for targeted and non-targeted individuals. Students not attending federal degrees can study at another public institution (either state or municipal), enroll at a private institution, or not attend college. Around 42% of all non-targeted 2016 SISU applicants who do not enroll at a federal university enroll at a private institution. This share is only 28% for targeted students. Moreover, conditional on attending a private institution, non-targeted students attend degrees with 59% higher tuition fees and 0.15 standard deviations of higher quality (as measured by the Ministry of Education).

4. THE IMPACT OF AFFIRMATIVE ACTION ON MARGINAL STUDENTS

In this section, we use admissions discontinuities to estimate the impacts of AA on marginally benefited and marginally displaced students. We exploit the fact that reserved and open seats have different admissions cutoffs to construct credible instruments from discontinuities for individuals who enter through each of these channels.

As described in Section 2, when applying to a given degree program, students must declare the admissions pool for which they want to be considered. There are four admissions tracks for reserved seats and one for open seats. Thus, for every degree program, we observe five different

[11]These results are in line with those of Mello (2022), who exploits the progressive rollout of the AA policy to show that it increased the share of targeted students enrolled in federal institutions. Mello (2022) shows that full adoption of the AA regulation, from 0 to 50% of reserved seats, increased enrollments of targeted students by 9.9 percentage points for the average degree program. It is important to note that, in 2012, the average degree program was already admitting 22% of their students under reserved spots as a result of other institution-specific AA initiatives.

admissions pools. We define an admissions list as the interaction of a degree program and an admissions pool. A student is granted admission to a particular degree program if their application score exceeds the ex-post admissions cutoff for their chosen admissions list. For interpretability of the results, throughout this analysis, we focus on the top-ranked option submitted to the system.

We present the reduced-form evidence graphically in Figure 3. To produce this figure, we pool all admissions lists and center them around their admissions cutoffs. Then, we construct residualized outcomes by removing admissions list fixed effects and rescaling them by the outcome means.[12] For simpler presentation, we also pool individuals across all four types of reserved seats and present them as a single group.

To statistically estimate the effects of increasing one targeted seat at the expense of reducing one open seat, we employ admissions cutoffs to generate threshold-crossing instruments for each type of seat. Similar to Abdulkadiroglu et al. (2014), we pool all admissions lists to produce non-parametric RD estimates by restricting the sample to a 20-point bandwidth around the admissions cutoffs.[13] Specifically, we estimate the impact of gaining admission into the top-ranked degree program using the following 2SLS system of equations:

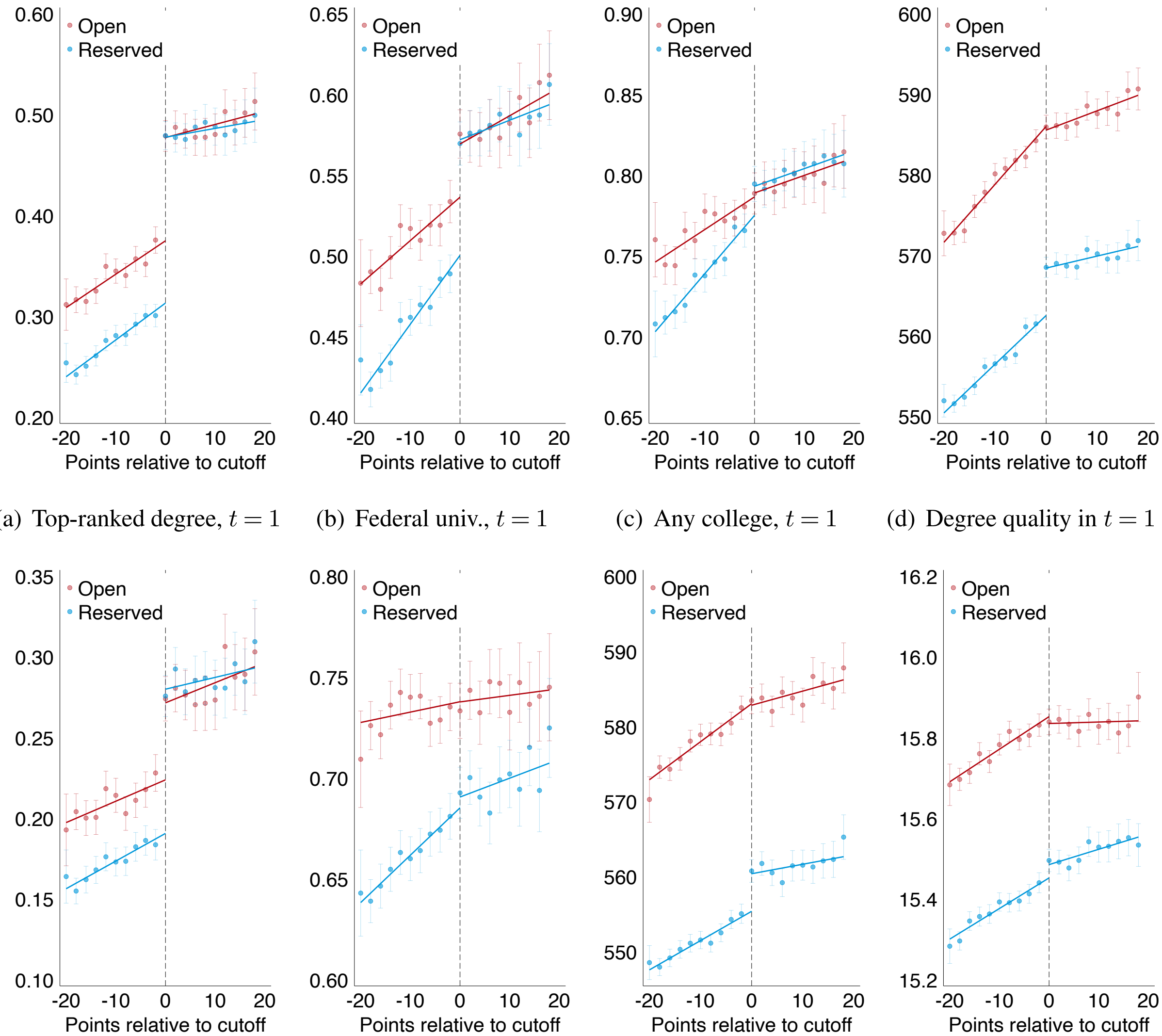
$$W_{ij} = \phi_{jk} + \pi_k D_{ij} + \delta_{0jk}(1 - D_{ij})r_{ij} + \delta_{1jk} D_{ij} r_{ij} + u_{ij},$$
$$Y_{ij} = \mu_{jk} + \rho_k W_{ij} + \gamma_{0jk}(1 - D_{ij})r_{ij} + \gamma_{1jk} D_{ij} r_{ij} + \epsilon_{ij},$$

where D_{ij} indicates an offer at degree program j, W_{ij} denotes degree attendance, and Y_{ij} is an outcome of interest for student i applying to degree program j. Parameters ϕ_{jk} and μ_{jk} control for admissions list fixed effects, as defined by the interaction of a given degree j and admissions pool k. The running variable is r_{ij} and is defined as the application score of student i centered at the cutoff of their relevant admissions list. We also control for linear regressions of the running variable $(\delta_{jk}, \gamma_{jk})$ at each side of the cutoffs. The first-stage coefficient is π_k, and the coefficient of interest is ρ_k.[14] Under this fully saturated regression, this estimate is a variance-weighted

[12]Formally, we regress the outcome on a constant, α, and an admissions list-specific fixed effect λ_{jk}, where j denotes the top-ranked degree, and k indicates one of the five admissions tracks. The residualized outcome is given by $\tilde{Y}_i = Y_i - (\hat{\alpha} + \hat{\lambda}_{jk}) + \bar{Y}$.

[13]Following Gelman and Imbens (2019), we avoid using a parametric RDD with high-order polynomials of the running variable, and we restrict our analysis to local linear regressions around admissions thresholds.

[14]For presentation purposes, we estimate two parameters for ρ_k—one for open seats and one for reserved seats—and pool all AA admissions pools together. In the estimation, we still allow all other relevant parameters to vary according to the specific admissions pool.



(a) Top-ranked degree, $t = 1$ (b) Federal univ., $t = 1$ (c) Any college, $t = 1$ (d) Degree quality in $t = 1$

(e) Top-ranked, $t = 4$ (f) Any college, $t = 4$ (g) Degree quality, $t = 4$ (h) Log(Lifetime earnings)

FIGURE 3.—Regression discontinuity plots. *Notes:* We pool all admissions lists and center them around the admissions cutoffs. We also pool individuals across all four types of reserved seats. The red and blue lines show the outcomes for students applying for open seats and reserved seats, respectively. The figure shows binned residualized mean outcomes for individuals in each type of seat for several educational outcomes. The error bars in a cell represent the 95% confidence interval for a given outcome mean. We include observations within a 20-point interval above and below the last admitted student and plot the fit of linear regressions on each side of the cutoff. Panels (a)-(d) present outcomes in the first year after admissions ($t = 1$). Panel (a) denotes enrollment in the degree program reported as the top preference, Panel (b) shows enrollment in any federal university, Panel (c) reports enrollment in any college, and Panel (d) presents the average quality of the degree enrolled (measured by the average test score of incoming students in a given degree in $t = 1$). Panels (e)-(g) repeat these outcomes four years after admissions ($t = 4$). Panel (h) reports the effects on the log of projected lifetime earnings as described in Section 6.4.

TABLE II

REDUCED-FORM AND 2SLS ESTIMATES

	Reduced-form			2SLS		
	Open	Reserved	Diff	Open	Reserved	Diff
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Higher education outcomes in $t = 1$						
Top-ranked degree enrollment	0.111	0.199	0.088	-	-	-
	(0.008)	(0.008)		. -	-	
Federal degree enrollment	0.030	0.101	0.071	0.275	0.510	0.235
	(0.008)	(0.008)		(0.057)	(0.028)	
Any college enrollment	-0.001	0.029	0.030	-0.010	0.146	0.156
	(0.007)	(0.006)		(0.060)	(0.030)	
Degree quality	-0.472	7.95	8.419	-4.269	40.01	44.28
	(0.801)	(0.680)		(7.39)	(2.955)	
Panel B: Higher education outcomes in $t = 4$						
Top-ranked degree enrollment	0.055	0.109	0.054	0.499	0.548	0.049
	(0.007)	(0.007)		(0.051)	(0.026)	
Federal degree enrollment	0.019	0.056	0.038	0.167	0.284	0.116
	(0.008)	(0.008)		(0.066)	(0.034)	
Any college enrollment	0.001	0.015	0.014	0.012	0.076	0.064
	(0.007)	(0.007)		(0.063)	(0.035)	
Degree quality	0.079	5.72	5.640	0.712	28.80	28.084
	(0.891)	(0.800)		(8.046)	(3.927)	
Log(Lifetime earnings)	-0.004	0.034	0.038	-0.032	0.182	0.214
	(0.015)	(0.018)		(0.148)	(0.093)	
Observations	70,784	88,494		70,784	88,494	

Note: This table shows the reduced-form and 2SLS estimates of gaining admission to a federal degree program. Each row presents estimates for different outcome variables. We pool all admissions lists, as defined by a degree and an admissions pool, and center them around their relevant admissions cutoffs. We also pool individuals across all four types of reserved seats. Panel A shows outcomes immediately after admission. The first row in Panel A is the first stage of the 2SLS system of equations presented in the main body of the text. The variable “Top-ranked degree” denotes enrollment in the degree program reported as the top preference in SISU. Panel B presents outcomes 4 years after admission. In Panel B, outcomes measure whether students are successfully enrolled in a degree program or have already graduated since the admission date. Standard errors are clustered at the degree level.

average of gaining an admission offer across all different degree programs. We present the results in Table II.

We start by describing the first-stage relationship between admissions offers and degree attendance. Table II shows that an offer increases first-year attendance by 11.1 and 19.9 percentage points for students in open and reserved seats, respectively. Figure 3(a) presents the relationship graphically. The take-up rate on the right-hand side of the discontinuity is 47% for open and reserved seats and masks substantial levels of heterogeneity. Highly selective degrees such as Medicine have an average take-up of 70%, while other less selective degrees at vocational institutions have substantially lower rates. Enrollment rates on the left-hand side of the discontinuity differ from zero because many students are admitted off the waitlist, and some programs offer a second round of applications in the second semester. As a result of lower take-up of waitlist offers and lower reapplication rates, the first-stage coefficient for AA students is almost twice as large. This difference highlights how admissions frictions can differentially affect students from diverse backgrounds.

We now describe the impacts on other relevant higher education outcomes during the first year after admission. We present these results in Figures 3(b)-3(d) and Panel A of Table II. Students offered admission to their top-ranked degree increase their overall access to a federal institution. This effect accounts for students who are admitted through their second option, reapply during the second semester, or were previously enrolled. Our data allow us to capture the effects in both the federal system and any higher education degree. For students applying through reserved seats, an admissions offer increases the likelihood of attending college by 3 percentage points and the quality of the degree they attend by 8 points.[15] In contrast, we do not observe any impacts for students who applied through open seats, which highlights the fact that these students can access equally good options outside the centralized system.

Next, in Figures 3(e)-3(g) and Panel B of Table II, we focus on what happens 4 years after admission. We construct several educational outcomes to measure whether students are successfully enrolled in a degree program or have already graduated from it. Even though dropout rates are high, at 43%, and admissions scores are much lower for targeted students, Figure 3(e) shows no differences in dropout rates by seat type. In Figure 3(g), we focus on the quality of the degree students attend. This outcome captures the net effect of gaining admission to a federal degree and individuals' academic paths after admission (be these inside or outside the federal system or dropping out). Our findings suggest that AA students just to the right of the admissions cutoff

[15] We measure degree quality by calculating the average test score of incoming students in a given degree. Since we have access to the universe of degrees and institutions in Brazil, we can compare the quality of the degrees inside and outside the federal system. We assume that everyone not attending college is implicitly attending the same option. Therefore, the degree quality measure for these students is given by the average test score of everyone who decided not to attend higher education.

attend degrees 5.7 points higher than comparable AA students who do not receive an offer. These results are in contrast to the prediction of the mismatch hypothesis, which states that targeted students would experience worse outcomes as a result of being assigned to a higher-quality degree. For the case of non-targeted students, similar to before, we do not find meaningful impacts on college access or on the quality of the degree they attend.

Finally, in Figure 3(h), we estimate the effect of gaining admission to a federal degree on the log of projected lifetime earnings. We do not use realized earnings because it is too early to follow the 2016 cohort in the labor market. Instead, we use individual-level microdata on older cohorts and construct a measure of predicted earnings that takes into account the academic trajectories of students together with their demographic characteristics and test scores. This is our primary outcome in the model-based analysis, and its definition is explained and justified in Section 6.4. While the marginally displaced individual does not see a decrease in their expected earnings, the 2SLS estimate for the marginally benefited targeted student shows an 18% gain in projected lifetime earnings. These results align with those of Francis-Tan and Tannuri-Pianto (2018) and Duryea et al. (2022), who exploit admissions discontinuities at two different Brazilian federal institutions and find that while low-income and racially marginalized applicants see increases in their realized earnings, high-income applicants experience small gains because they can still find similar opportunities in private education.

A critical assumption behind the regression discontinuity design is that marginal students on both sides of the admissions cutoffs are comparable. The main concern in our setting is that, given the iterative nature of the algorithm, students could learn the admissions cutoffs, self-select into the treatment option and break the comparability across both sides of the discontinuity. We do not believe this is a concern in our setting because students who are not eligible for admission to a given degree ex-ante still have incentives to apply, since they can be admitted from the waitlist (i.e., probabilities of admission to the first option are never zero ex-post). In Appendix Figure A.4, we assess the validity of our research design by investigating covariate balance around the admissions thresholds. Overall, we find no indication that applicants on different sides of the admission thresholds differ in terms of observables. Along the same line, we do not find evidence that students bunch at the margin of admissions.

5. A MODEL OF AFFIRMATIVE ACTION IN A CENTRALIZED SYSTEM

We are interested in estimating the consequences of changes in AA in a centralized system. For example, in the Brazilian case, we would like to estimate the effects of the 50% AA policy relative to a regime without AA. In this section, we propose a framework to characterize such effects, which we use to guide the empirical analysis.

Relative to the analysis of marginal students, this framework allows us to go further in two dimensions. First, it allows us to characterize the effects for individuals who score away from the admissions discontinuities. Second, it allows us to capture the equilibrium effects of AA. These effects are important even for marginal increases in the number of reserved seats. To build intuition, consider a case in which we increase AA for the most selective degree by a single reserved seat. In this example, AA will change the equilibrium allocation between students and degrees through two channels: (i) the *direct* effect generated by the marginally benefited and the marginally displaced individual in the selective degree, and (ii) the *indirect* (or knock-on) effects on all other degrees resulting from benefited and displaced individuals, which respectively make room for or push out other students from their preferred degrees.

We consider a centralized system in which students rank their preferred degree programs, and applicants are ranked using an application index comprised of standardized test scores. Similar to that of Dur et al. (2020), our framework embeds an AA regulation within the system by reserving admissions spots for the targeted group. Because the number of spots is fixed, AA operates by pulling in applicants from the targeted group at the cost of pushing out non-targeted students with higher test scores.

5.1. *Environment*

We consider a set of individuals indexed by $i \in \mathcal{I} = \{1, ..., n\}$ who apply to a finite set of selective college degree programs through a centralized platform. Let $\mathcal{J} = \{0, 1, ...J\}$ denote the set of degrees indexed by j offered across all public institutions, where $j = 0$ represents the outside option of either attending a private institution or not attending college.

Students have preferences over degree programs based on a strict ordering, $\succ_i$. Degree programs also have preferences over applicants based on students' application scores $s_i = \{s_{i1}, ..., s_{iJ}\}$. Scores are very fine, so no tie-breaker is needed. We allow for degree-specific scores, which may arise due to degrees assigning different weights to different components of the entrance exam (as in our empirical setting) or due to degree-specific exams.

There are different subgroups within the student population, defined by their AA status $t_i \in \mathcal{T}$. For ease of exposition, let $\mathcal{T} = \{\text{AA, NA}\}$, such that $t_i = \text{AA}$ represents targeted students, and $t_i = \text{NA}$ represents non-targeted students. The status t_i of each student is observable. A student is fully characterized by their type $\theta_i = (\succ_i, s_i, t_i)$—that is, the combination of an applicant's preferences, application scores across all degree programs, and AA status (Abdulkadiroglu et al., 2017). We denote the set of all student types by $\Theta = \bigcup_{i\in\mathcal{I}} \theta_i$.

Spots in degree programs are constrained by a strictly positive capacity vector, $q = \{q_0, ..., q_J\}$. An AA regulation is in place such that for every degree program, a share $\omega \in [0, 1]$ of spots is reserved for applicants from the targeted group. The remaining share is open to all individuals. As such, for any given degree j, ωq_j spots are reserved for targeted students, and $(1-\omega)q_j$ spots are open to all individuals. We assume $q_0 = \infty$, since the outside option of not attending a public institution is available to all students.

The centralized mechanism applies a student-proposing deferred acceptance algorithm to generate degree assignments. The inputs to the mechanism are student types Θ, school capacities q, and reservation shares ω. When reserved seats are processed, they are assigned to targeted students with the highest application scores. When open seats are processed, they are assigned to members of either group (targeted or non-targeted) in order of their application scores. When a student qualifies for both a reserved and an open spot, the admissions rules must specify which seats are processed first, i.e., the relative precedence of different admissions channels. In our empirical application, we assume that reserved spots are first assigned to targeted students based on application scores, and next, all open spots are assigned to the remaining individuals with the highest application scores.[16]

Let $\varphi(\Theta, q, \omega) = \mu$ denote the matching produced by mechanism φ for the problem (Θ, q, ω). The matching is a function $\mu : \Theta \to \mathcal{J}$, such that (i) $\mu(\theta_i) = j$ if student i is assigned to degree j, and (ii) no degree is assigned more students than its capacity. Because the mechanism implements

[16]The literature has established a difference between horizontal and vertical AA depending on which of the spots (reserved or open, respectively) are processed first. A horizontal reservation is a "minimum guarantee" in the sense that it is only binding when there are not enough targeted students who receive a spot on the basis of their test scores alone. A vertical reservation works on an "over-and-above" basis. This means that targeted individuals receiving spots on the basis of their application scores alone do not count toward vertically reserved spots (Sonmez and Yenmez, 2022). Our framework encompasses both types of AA.

a deferred acceptance algorithm, assignments μ between students and degrees are unique and stable (Gale and Shapley, 1962, Abdulkadiroglu, 2005). The stability property implies that each student enrolls in their most preferred degree among those for which they are eligible.

The matching μ has a unique representation in terms of a vector of market-clearing admissions cutoffs $c_j^t(\mu)$ for each degree program and AA status combination (Azevedo and Leshno, 2016).[17] A cutoff $c_j^t(\mu)$ is a minimum score s_{ij} required for admission to degree j for students with AA status t. Since targeted students can be admitted through reserved or open spots, admissions cutoffs for targeted students are always lower than for non-targeted students—i.e., $c_j^{\mathrm{NA}} \geq c_j^{\mathrm{AA}}$ for all $j \in \mathfrak{J}$. Because the outside option is always feasible, it has an admissions cutoff score of $-\infty$.

As in any strict-priority mechanism, the availability of options depends on the student's application score. Let $\Omega_i(\mu) = \{j \in \mathfrak{J} \mid s_{ij} \geq c_j^{t_i}(\mu)\}$ represent the "feasible choice set" for individual i, defined as those degrees to which they could have gained access based on their score and AA status under a given matching μ. Let the variable $D_i(\mu) = \{j \in \Omega_i \mid j \succeq k \text{ for all } k \in \Omega_i(\mu)\}$ denote the preferred option in the feasible choice set defined by $\Omega_i(\mu)$. In other words, $D_i(\mu)$ represents the preferred option among the degrees to which i could have been admitted. We refer to this option as the "preferred feasible degree." From the stability condition, we know that $D_i(\mu) = \mu(\theta_i)$.

The realized outcome for student i is given by $Y_i(\mu) = \sum_j \mathbb{1}\{D_i(\mu) = j\} \cdot Y_{ij}$, where $D_i(\mu)$ indicates the degree attended and Y_{ij} denotes the potential value of some outcome of interest for student i if they attend degree j.

5.2. *Gains and losses from AA*

Our goal is to define how much students would gain or lose as a result of an increased AA schedule ω'. To characterize such impacts, we leverage the assignment rules from the mechanism to compute a counterfactual matching function $\varphi(\Theta, q, \omega') = \mu'$. These new allocations are represented by a counterfactual vector of admissions thresholds $c_j^t(\mu')$, which translate into counterfactual feasible choice sets $\Omega_i(\mu')$, counterfactual preferred feasible degree options $D_i(\mu')$, and ultimately into counterfactual outcomes $Y_i(\mu')$. Thus, the conditional average treatment effect of

[17]Other mechanisms also have a unique representation in terms of admissions thresholds, as long as the matches between students and degrees are pairwise stable (Agarwal and Somaini, 2018).

increasing ω to ω' for individuals of type θ is

$$\tau(\theta) = \mathbb{E}[Y_i(\mu') - Y_i(\mu) | \, \theta_i = \theta]. \tag{5.1}$$

The aggregate effect of increasing AA is given by integrating over all student types. To aggregate these treatment effects, we assume equal weights for all individuals within a given AA group. Hence, the aggregate effects for each AA group t are

$$\Delta_t(\omega', \omega) = \sum_{i \in \mathcal{I}} \tau(\theta_i) \cdot \mathbb{1}\{t_i = t\}. \tag{5.2}$$

5.3. *A simple example*

Under this framework, the aggregate consequences of the AA policy are given by how much pulled-in students gain and how much pushed-out students lose in terms of a given outcome of interest (e.g., income). In Figure 4, we provide a stylized example to show how these gains and losses are characterized by how test scores, degree admissions, and outside options affect the outcome of interest. For expositional clarity, assume that $\mathcal{J} = \{0, 1\}$, such that there is only one selective degree available, or an outside option. For simplicity, we also assume that all students have strict preferences for the selective degree ($j = 1$) over the outside option ($j = 0$).

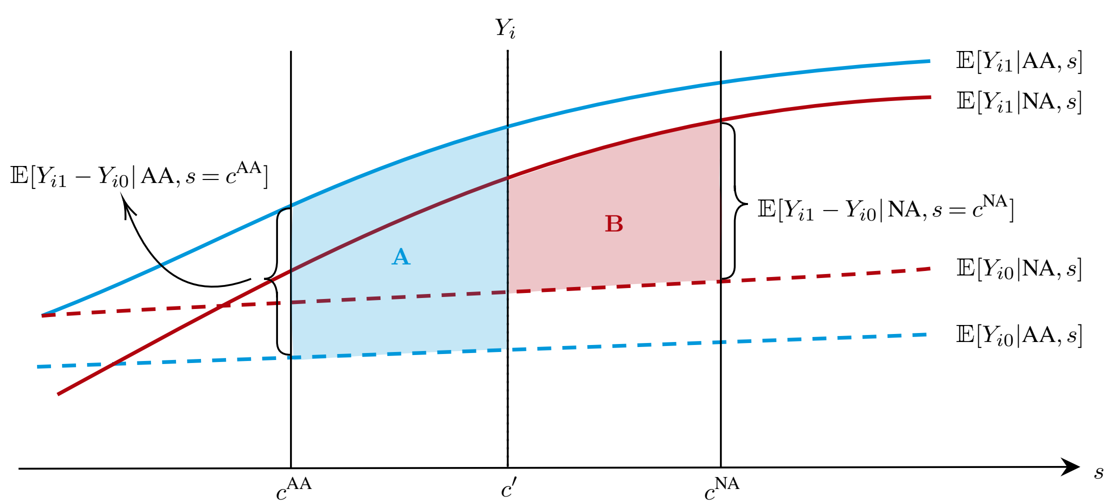


FIGURE 4.—Conceptual Framework. *Notes:* This figure presents the distributional impacts of AA. Each line denotes the mean potential outcome for targeted (blue) and non-targeted (red) individuals. The solid line presents the expected outcome of attending the selective degree, and the dashed one the expected outcome of attending the outside option.

The solid lines show the average potential outcome for students who attend the selective institution, conditional on their score and AA type, $\mathbb{E}[Y_{i1} | t_i, s_i]$. The dashed lines depict the average potential outcome from attending the outside option, $\mathbb{E}[Y_{i0} | t_i, s_i]$. In the absence of AA (i.e., $\omega = 0$), there is a single admissions cutoff c', for all students. By focusing on students who score around c', we can easily observe that implementing AA is efficient for the marginal student, as the returns to attending the selective institution are higher for targeted than non-targeted students.

Now assume that policymakers implement an AA policy such that targeted and non-targeted students face different cutoffs, represented by c^{AA} and c^{NA}, respectively. Targeted students with scores $s_i \in [c^{\text{AA}}, c']$ are now offered admission to the selective degree, while non-targeted students with scores $s_i \in [c', c^{\text{NA}}]$ are displaced from it. Area A depicts the aggregate gains for benefited individuals, and Area B depicts the costs incurred by displaced individuals. We highlight two observations. First, it is easy to observe that the gains and losses experienced by the marginally benefited and displaced students, as captured by $\mathbb{E}[Y_{i1} - Y_{i0} | t_i, s = c^{t_i}]$, do not capture the full effects of the policy. Second, the consequences of AA depend crucially on how heterogeneous the returns are for benefited and displaced individuals and how this difference in returns varies by test scores.

6. ECONOMETRIC MODEL

In our conceptual framework, we characterized the effect of increasing the AA schedule from ω to ω' for individuals of type θ. Using the stability condition, we rewrite Equation (5.1) as

$$\tau(\theta) = \mathbb{E}\left[\sum_j \mathbb{1}\{\mu'(\theta_i) = j\} \cdot Y_{ij} - \sum_j \mathbb{1}\{\mu(\theta_i) = j\} \cdot Y_{ij} \;\middle|\; \theta_i = \theta\right], \tag{6.1}$$

where μ and μ' represent the matching function for each AA schedule, and Y_{ij} denotes the potential value of some outcome of interest for individual i if they attend degree j. Accordingly, estimating $\tau(\theta)$ in the data requires estimating the matching functions for each AA schedule, together with the potential outcome of attending a given degree.[18] We start by specifying and parametrizing a joint model of school choice and outcomes. Next, we present the key identifying

[18] In the example described in Figure 4, this is equivalent to finding the admissions cutoffs with and without AA and estimating the shape of the potential outcome curves.

variation that allows us to estimate the effects of AA away from the admissions discontinuities. Finally, we end this section by introducing and defining our primary outcome variable.

6.1. *School choice model*

A natural way to compute counterfactual matching functions is to use the rules of the mechanism φ, together with its inputs (Θ, q, ω). In practice, however, researchers never observe the full set of student types Θ, because individuals do not rank the full list of degree preferences $\succ_i$. To recover student preferences, we introduce an empirical school choice model.

We summarize students' preferences by fitting a random utility model. Specifically, student i's indirect utility for attending degree j is

$$\begin{aligned} u_{ij} &= V_{ij} + \eta_{ij} \\ &= \delta_j^t + \gamma_j^t \cdot s_{ij} + \kappa^t \cdot d_{ij} + \sigma^t \cdot p_i + \eta_{ij}, \end{aligned} \tag{6.2}$$

where V_{ij} captures the part of the utility that varies according to the observed characteristics of students and degrees, and η_{ij} captures unobserved tastes for degrees.[19] We parametrize V_{ij} as a function of degree fixed effects δ_j^t, student test scores s_{ij}, and a dummy d_{ij} that indicates whether the student lives in the same commuting zone where the degree is offered.[20] We also include $p_i = p^t(X_i)$, which is a proxy for the underlying propensity of a student with characteristics X_i to choose any inside good over the outside option.[21] This variable allows us to parsimoniously estimate substitution patterns from the outside option toward inside goods based

[19] Implicit in Equation (6.2) is an assumption that preference parameters $(\delta_j^t, \gamma_j^t, \kappa^t, \sigma^t)$ can rationalize true utilities for degrees under any realization of the matching function. This assumption implies that preferences for degrees are independent of the observed allocation of students, ruling out preferences for peers. Although this assumption might sound restrictive, most empirical approaches abstract away from equilibrium sorting based on preferences for peers (Agarwal and Somaini, 2020).

[20] We use micro-regions as proxies for commuting zones to capture relocation needs based on distance from a specific degree. Brazil has 558 micro-regions defined by the IBGE as neighboring cities with shared social characteristics, geography, and spatial articulation.

[21] We estimate $p^t(X_i)$ in three steps. In the first, we residualized a dummy variable that indicates whether the student is assigned to an inside good on the number of degrees available for that student (i.e., $|\Omega_i|$). We also residualized a vector of student variables X_i on the same regressor. In the second step, we regress the residualized inside good dummy variable on the residualized vector X_i. This vector includes student gender, household income, municipality of residency income, marital status, race dummies, age, previous college attainment, year of high school graduation, and state of residency. In the third and final step, we make a prediction based on the second step. We implement this procedure for each AA group $t \in \{0, 1\}$.

on a high-dimensional vector of covariates X_i. We also allow flexible heterogeneity in tastes by estimating preference models separately for each AA type t. The unobserved taste η_{ij} is an iid error term from an Extreme Value Type I (logit) distribution, conditional on vectors s_i, $d_i = (d_{i1}, ..., d_{iJ})$, and p_i. The outside option aggregates all degrees offered by municipal, state, and private universities, as well as not enrolling at all. The outside option is available to everyone and has a deterministic utility V_{i0} normalized to zero.

To estimate preferences, we extract several preference relationships based on the properties of the mechanism and students' reported options. As described in Section 2.2, the Brazilian admissions system uses an iterative deferred acceptance mechanism, in which students report their top two degree choices over the course of several days. The last day's results are final and determine admissions offers. As of 2016, applicants may either be (i) accepted by their first choice, (ii) accepted by their second choice and waitlisted at their first choice, or (iii) rejected by both their first and second choices but waitlisted for their first choice.

Under reasonable assumptions, the Brazilian mechanism produces the student-optimal stable matching (Bó and Hakimov, 2022).[22] One concern is that the stability property, as an ex-post optimality condition, is not necessarily guaranteed when students are not fully informed about admissions cutoffs. In Appendix B, we show that in the Brazilian setting, students have little uncertainty over the final admissions cutoffs due to the iterative property of the mechanism.[23]

Let $\tilde{\mu}$ be a realized matching we observe in the data, and R_{ik} denote the final kth option submitted by student i. The stability property implies that by the last day of the application round, students are faced with the option of selecting a degree from an observable and personalized choice set $\Omega_i = \Omega_i(\tilde{\mu})$ defined by all degrees that are feasible to them (Fack et al., 2019). Accordingly, we can infer that the top-ranked option is preferred over all other feasible options in the system. Note that students may have incentives to list $R_{i1} \notin \Omega_i$ if they anticipate getting an admissions offer off the waitlist. When the top-ranked option is not feasible, we learn that the

[22]As discussed by Bó and Hakimov (2022), the Brazilian mechanism could be susceptible to manipulation via cutoffs. A group of high-scoring students could engage in sophisticated coordination to reduce the competition faced by another group of lower-scoring students. This strategy involves temporarily inflating the cutoffs for some degrees and then drastically lowering their cutoffs by changing reported options in the last step. This situation, although theoretically possible, is extremely unlikely. In Appendix B, we show that the dynamic trajectories of cutoffs indicate no evidence of manipulation via cutoffs.

[23]In experimental data, iterative versions of the deferred acceptance algorithm lead to stable allocations more frequently than the direct version (Klijn et al., 2019, Bó and Hakimov, 2019).

second-ranked option is preferred to all other feasible options.[24] If the student does not rank a second option and the first option is unfeasible, we assume the outside option is preferred over all other feasible options.

These preference relationships, together with the logit model, imply that the conditional likelihood of the rank list R_i is given by

$$\mathcal{L}(R_i|s_i, d_i, p^t(X_i)) = \left(\frac{\exp V_{iR_{i1}}}{\sum\limits_{k\in\Omega_i\cup R_{i1}} \exp V_{ik}}\right)\left(\frac{\exp V_{iR_{i2}}}{\sum\limits_{k\in\Omega_i} \exp V_{ik}}\right)^{p_i},$$

where $p_i = 1$ if R_{i1} is unfeasible and R_{i2} is feasible. We estimate $(\delta_j^t, \gamma_j^t, \kappa^t, \sigma^t)$ for $t \in \{0, 1\}$ using a maximum likelihood estimator.

6.2. *Potential outcomes model*

Next, we switch our focus to constructing selection-corrected estimates of a potential outcome model. Our main outcome of interest is projected lifetime earnings, which we describe in detail in Section 6.4 below. We follow a control function approach similar to that of Abdulkadiroglu et al. (2020), who link school choices to potential outcomes to estimate schools' value-added in New York City.

We define $\tilde{Y}_{ij}$ as the log of potential outcome Y_{ij} of student i at counterfactual degree j. We project $\tilde{Y}_{ij}$ on degree-specific fixed effects and student and degree characteristics:

$$\begin{aligned} Y_{ij} &= \exp \tilde{Y}_{ij} \\ \tilde{Y}_{ij} &= \alpha_j^t + \beta_j^t \cdot s_{ij} + X_{ij}'\Pi^t + \varepsilon_{ij}, \end{aligned} \tag{6.3}$$

where α_j^t, β_j^t, and Π^t are population parameters for AA group t, which implies $\mathbb{E}[\varepsilon_{ij}] = \mathbb{E}[s_{ij}\varepsilon_{ij}] = \mathbb{E}[X_{ij}\varepsilon_{ij}] = 0$. The variable X_{ij} is a vector of observed covariates, including a location dummy d_{ij} and the underlying propensity to choose an inside good over the outside option p_i, as described before. We also include a rich vector of student covariates to reduce the prediction variance.[25]

[24] When the top-ranked option is feasible, students are always assigned to that option and the information contained in the second option becomes irrelevant.

[25] We include the variable p_i both in levels and interacted with the outside option. The student vector of covariates includes student gender, household income, the income of the municipality of residency, marital status, race dummies,

The mean outcome $\tilde{Y}_i$ observed in the data for a given matching $\tilde{\mu}$ is given by

$$\mathbb{E}[\tilde{Y}_{ij}|s_{ij}, X_{ij}, D_i = j] = \alpha_j^t + \beta_j^t \cdot s_{ij} + X_{ij}'\Pi^t + \mathbb{E}[\varepsilon_{ij}|s_{ij}, X_{ij}, D_i = j],$$

where D_i denotes degree assignment. The OLS estimation of this equation would likely yield biased parameters due to selection into degrees based on unobservable preferences. To recover unbiased estimates of α_j^t and β_j^t, we would need to assume that $\mathbb{E}[\varepsilon_{ij}|s_{ij}, X_{ij}, D_i = j] = 0$, thus implying that degree choices and potential outcomes are not correlated after accounting for student and degree observed characteristics.

To account for selection on unobservables, we link the outcome equation to the school choice model by conditioning Equation (6.3) on the vector of unobserved tastes $\eta_i = (\eta_{i0}, \eta_{i1}, ..., \eta_{iJ})$:

$$\mathbb{E}[\tilde{Y}_{ij}|s_{ij}, X_{ij}, \eta_i] = \alpha_j^t + \beta_j^t \cdot s_{ij} + X_{ij}'\Pi^t + \mathbb{E}[\varepsilon_{ij}|s_{ij}, X_{ij}, \eta_i] \tag{6.4}$$

$$= \alpha_j^t + \beta_j^t \cdot s_{ij} + X_{ij}'\Pi^t + g^t(\eta_i). \tag{6.5}$$

This model allows expected potential outcomes to vary across students with different preferences for degrees in a way that is not captured by students' observables.[26] We estimate Equation (6.5) using the multinomial logit selection model of Dubin and McFadden (1984), which imposes a linear relationship between potential outcomes and the unobserved logit errors. Imposing such a parametric approximation on $g^t(\cdot)$ yields

$$\mathbb{E}[\tilde{Y}_{ij}|s_{ij}, X_{ij}, \eta_i] = \alpha_j^t + \beta_j^t \cdot s_{ij} + X_{ij}'\Pi^t + \sum_{k=0}^{J} \psi_k^t \cdot (\eta_{ik} - \bar{\eta}) + \rho^t \cdot (\eta_{ij} - \bar{\eta}), \tag{6.6}$$

where $\bar{\eta} \equiv \mathbb{E}[\eta_{ij}]$ is Euler's constant. As Abdulkadiroglu et al. (2020) point out, this parametric relationship allows for a wide range of selection on unobservables in the context of school choice. The parameter ψ_k^t captures the effect of the preference for degree k that is common across all potential outcomes. For example, students with high preferences for a given type of degree may have higher outcomes in all other degrees in a way that is not fully captured by student observables (e.g., students enrolling in medicine may also be high in motivation and thus do well in

age, previous college attainment, year of high school graduation, and gender interacted with age, race, and household income.

[26]From Equations (6.4) to (6.5), we impose a separability assumption that implies that the conditional expectation of ε_{ij} as a function of η_i does not depend on X_{ij}. This is a common assumption in applied work that uses instrumental variables to identify selection models (Kline and Walters, 2016, Brinch et al., 2017, Abdulkadiroglu et al., 2020).

any other degree). We refer to this term as selection on levels. The coefficient ρ^t represents the match effect of preferring degree j. This unobserved match component allows, for instance, for students to sort into degrees based on potential outcome gains. We refer to this type of selection as selection on gains (Roy, 1951).

By iterated expectations, the mean outcome observed in the data is

$$\mathbb{E}[\tilde{Y}_i|s_{ij}, X_{ij}, \Omega_i, D_i = j] = \alpha_j^t + \beta_j^t \cdot s_{ij} + X_{ij}'\Pi^t + \sum_{k=0}^{J} \psi_k^t \cdot \lambda_{ik}(\Omega_i) + \rho^t \cdot \lambda_{ij}(\Omega_i), \quad (6.7)$$

where $\lambda_{ik}(\Omega_i) \equiv \mathbb{E}[\eta_{ik} - \bar{\eta}|s_{ij}, X_{ij}, \Omega_i, D_i]$ is the expectation of the unobserved preference for a given degree, conditional on test scores s_{ij}, student's characteristics X_{ij}, their feasible choice set Ω_i, and their preferred feasible degree D_i. These objects serve as control functions to correct for selection on unobservables. The estimation proceeds in two steps. First, we compute $\hat{\lambda}_{ik}(\cdot)$ using the estimated preference parameters and the logit functional form. In Appendix C, we show the derivation of the control function expressions given the preference relationships that we exploit in our data. Next, we plug $\hat{\lambda}_{ik}(\cdot)$ into Equation (6.7), and estimate parameters $(\alpha_j^t, \beta_j^t, \Pi^t, \psi_j^t, \rho^t)$ using separate OLS regressions for each AA group t.

6.3. *Potential outcome model identification*

To identify the model, we need a variable that exogenously shifts students' allocation without directly affecting potential outcomes. With this aim, we rely on an exogenous score shifter z_i that mimics random shocks to student test scores. The shifter exploits two exogenous sources of variation in test scores. The first source stems from random assignment to graders of varying strictness. The second source arises from plausibly exogenous assignment to multiple-choice examination booklets of varying difficulty. We describe these below. In terms of our model, individuals with positive score shifters will see an exogenous increase in their personalized choice sets $\Omega_i(z_i)$ (i.e., they could be admitted to a larger set of degrees), while individuals with negative score shifters will experience the reverse.

We use the score shifter z_i, together with admission discontinuities, to identify the potential outcomes (α_j^t and β_j^t) and selection parameters (ψ_k^t and ρ^t) from Equation (6.7). Identification of parameters α_j^t and β_j^t comes from exogenous variation in choice sets $\Omega_i(z_i)$ available to otherwise identical individuals. To provide intuition, suppose there are two degrees, A and B, and two individuals, 1 and 2, who share an identical preference for degree B. Assume that both individuals have access to degree A, but individual 2 also has access to degree B as a result of receiving a positive score shifter $z_i = z^+$, which allowed them to cross the admissions threshold. The difference in outcomes for these individuals pins down the treatment effect of attending degree B over degree A.

Identification of selection parameters ψ_k^t and ρ^t comes from variation in the available choice sets for students who enroll in the same degree program. To provide intuition, suppose there are two additional individuals, 3 and 4, who are identical on observables but may have different unobserved preferences. Assume both individuals have access to degree A, but individual 4 also has access to degree B as a result of receiving a positive score shifter $z_i = z^+$. If both individuals choose degree A, we can use a revealed preference argument to learn that individual 4 has an unobserved taste for option A, which is higher in expectation than that revealed by individual 3. The selection parameters capture whether this expected difference in unobserved preferences is relevant to the potential outcome.

The identification of Equation (6.7) relies on the assumption that choice sets and score shifters are exogenous to unobserved tastes η_i and potential outcome errors $\varepsilon_i = (\varepsilon_{i0}, \varepsilon_{i1}, ..., \varepsilon_{iJ})$ after conditioning on s_{ij} and X_{ij}; that is, $(\varepsilon_i, \eta_i | s_{ij}, X_{ij}) \perp z_i, \Omega_i(z_i)$. The independence of z_i is satisfied because of the random assignment of graders and booklets. The independence of $\Omega_i(z_i)$ is a consequence of the fact that choice sets are a function of student observable characteristics and the score shifter $\Omega_i = f^t(s_{ij}, X_{ij}, z_i)$. Implicit in this assumption is the fact that students take admissions cutoffs as given and cannot manipulate them through their application behavior. Finally, we need an exclusion restriction that ensures that the shifters only affect outcomes through increased degree availability.

Identifying variation in the data: The ENEM exam consists of a written essay and four multiple-choice tests. We leverage features of its implementation to construct exogenous score shifters for each of these components. These shifters mimic random shocks that impact test scores but are uncorrelated with students' latent abilities. The shifter z_i exploits two exogenous sources of variation, which we discuss in turn.

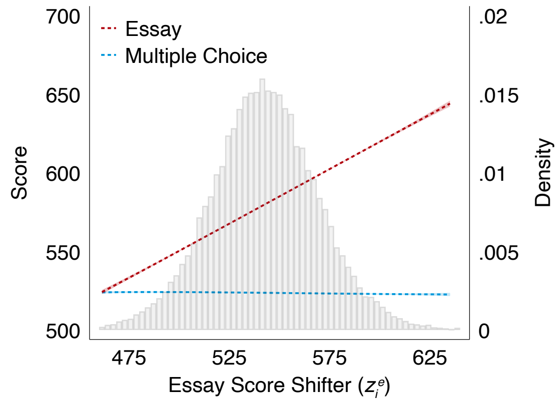
The first source stems from random assignment of the essay component to graders of varying strictness. Each essay is marked by two randomly assigned graders. Each year, over 10,000 graders are involved in the grading process, each of whom is assigned to an average of 1,067 exams. We use only graders assigned to more than 50 essays. Graders receive the exams via an online platform, which ensures that they are blind to students' identities and characteristics. The final grade is the average of the total points given by each of the graders.[27]

We construct a leave-one-out measure of grader leniency using the two randomly assigned graders. For a given student i, the leave-one-out score shifter of the essay z_i^e is the average score their graders gave to all other students they graded. Figure 5(a) presents the distribution of the essay score shifter for the 2016 SISU sample of interest. It shows wide dispersion, with a 5th-95th percentile range of 89 points. Given the large number of essays marked by each grader, in the absence of any leniency differences, the leave-one-out mean leniency for each grader should be concentrated around 580 points, the mean essay score. The dashed red line shows a local linear regression of the first-stage relationship between our essay score shifter and the essay score. The relationship is strong, positive, and mostly linear. The dashed blue line serves as a balance test and plots a local linear regression of the average multiple-choice score components of the test. Consistent with random assignment, this figure shows no correlation between the essay score shifter z_i^e and other measures of student ability, as captured by the average score in the multiple-choice part of the test. We present first-stage coefficients and additional balance tests in Appendix D.

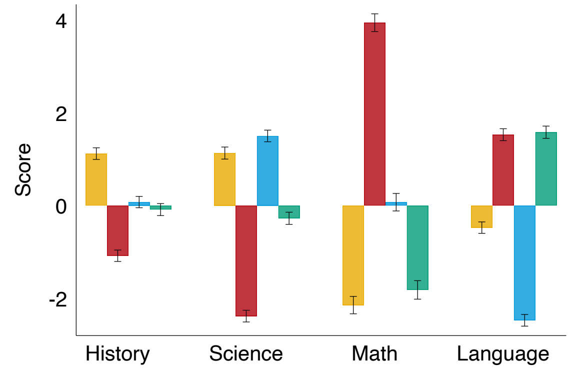
The second source of variation arises from the plausibly random assignment of students to examination booklets on the multiple-choice component of the test. To prevent cheating, students are assigned to one of four examination booklets. Each booklet type has a different color cover, and their contents vary only in the order in which questions are presented. In every examination location, students are organized alphabetically both across and within rooms, where they are assigned to one of the booklets. Accordingly, the booklet assignment is plausibly random.

We observe substantive differences in test scores across booklets, which we present in Figure 5(b). For instance, the average score for Math is 6 points higher for red booklets than for yellow booklets. In Appendix D, we show that booklet assignment is uncorrelated with students' characteristics. Despite containing the same questions, some booklets are more difficult than others because they present the easier questions later in the test. This means that students may run out of time or steam before they reach the easier questions (Kaur et al., 2023). For each component k in the multiple-choice part of the test, we construct a leave-one-out measure of booklet diffi-

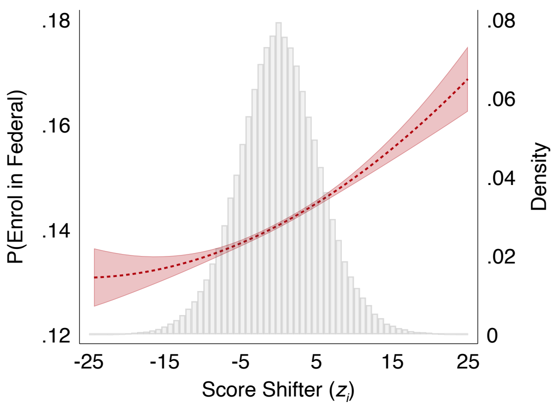
[27] The essay is marked based on five competencies, each scored on a scale from 0 to 200. If the score difference across graders is larger than 100 points or larger than 80 points for any of the individual competencies, that exam is graded by a third grader, and the final grade is then the average of the two closest scores.



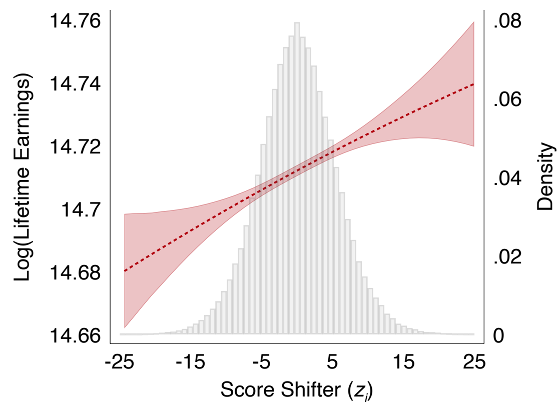
(a) Grader strictness



(b) Booklet difficulty



(c) Federal degree enrollment in $t = 1$



(d) Log(Lifetime earnings)

FIGURE 5.—Score shifters and reduced-form. *Notes:* Panel (a) shows the relationship between the essay score shifter and test scores. The red line shows a strong first-stage correlation between essay scores and the essay score shifter. The blue line serves as a balance test and shows no correlation between the essay score shifter and the average test score in the multiple-choice components of the test. Dashed lines present a local linear regression with a second-order polynomial. Solid areas represent a 95% confidence band. The histogram in the background reports the distribution of the essay shifter. Panel (b) reports the difference in average test scores across examination booklets for each of the different components of the ENEM. We define the score shifter $z_i \equiv \frac{1}{K}\sum_k (z_i^k - \bar{z}^k)$, where $K = 5$ is the number of components of the ENEM. Panel (c) shows the reduced-form between the score shifter and the probability of attending a federal degree in the year after taking the exam. Panel (d) reports the relationship between the score shifter and the log of projected lifetime earnings as described in Section 6.4.

culty z_i^k for student i, as defined by the average score of every other student assigned to the same examination booklet.

We combine these two sources of variation and create a corrected measure of student test scores by netting out the effect of the score shifters from the observed student test scores. Specifically, for a given component k in {Essay, Math, Language, History, Science} we impose the following parametric restriction:

$$\begin{aligned}\tilde{s}_i^k &= f^k(s_i^k, z_i^k) \\ &= s_i^k + \phi^k(z_i^k - \bar{z}^k),\end{aligned}$$

where s_i^k is the observed score, z_i^k is the score shifter, $\bar{z}^k$ is the average score shifter, and $\tilde{s}_i^k$ represents the corrected test score. An underlying assumption in this parametric model is that the score shifter z_i^k affects all students homogeneously—that is $\phi_i^k = \phi^k$ for all i.[28] We use these to construct corrected test scores that are relevant for the centralized system as $\tilde{s}_{ij} = \sum_k w_j^k \cdot \tilde{s}_i^k$, where w_j is the weight given by degree j to component k. We use corrected test scores $\tilde{s}_{ij}$ as the relevant score inputs included in the choice and potential outcome equations.

In Figure 5, we present the reduced-form relationship between our instrument and educational outcomes. Panel (c) shows that a student who received a score shifter in the top 5% of the distribution is 1.47 percentage points (or 11%) more likely to attend a federal institution the year after than one who got a score shifter in the bottom 5% of the distribution. In Panel (d), we show that the score shifter also meaningfully impacts students' projected lifetime income.

6.4. *Projected lifetime earnings as primary outcome*

We are interested in estimating the overall impact of AA on students' income. With that purpose, we establish our main outcome variable Y_i as the projected lifetime earnings, which is defined as the present value of future income flows. An important challenge for our analysis, however, is the unavailability of labor market income flows for the 2016 SISU sample, which is one of the inputs needed to construct the outcome measure Y_i. While we have access to a wide array of intermediate academic outcomes, labor market outcomes take several years to materialize. To address this problem, we follow Athey et al. (2019) and transform our intermediate outcomes into a "surrogate index" that captures the predicted value of income given intermediate outcomes and student covariates.

To estimate this index, we use microdata for cohorts entering college between 2010 and 2011, for which we observe intermediate outcomes and labor market income from matched employer-

[28]This assumption implies the monotonicity assumption, which is standard in the so-called "judge designs" that leverage similar sources of exogenous variation.

employee records in 2017. We use these old cohorts to estimate a model linking student demographics, test scores, and academic outcomes to income flows. We leverage our rich set of academic outcomes, including college enrollment and detailed academic progress across different degree programs over 4 years, to estimate a flexible income model. We use estimated parameters of the model to predict income flows 7 years after starting college for our sample of interest. Finally, we use this predicted measure to construct projected lifetime earnings by computing the present value of the predicted income flows. We provide details about the model and the present value computation in Appendix E.

To recover unbiased parameters of the outcome Equation (6.3) based on the log of projected lifetime earnings, two assumptions are necessary. These assumptions are formally discussed in Appendix F. The first one imposes a comparability condition and states that the conditional expectation functions of $\tilde{Y}_i$ are comparable for the 2010 and 2011 cohorts, and our estimating sample of 2016 SISU. That is, the mapping from individual characteristics, degree assignments, and intermediate outcomes to income flows is fixed and invariant to the sample. This assumption rules out, for instance, any general equilibrium effects as a result of AA, such as the presence of peer effects in the education production function. The second assumption is a "surrogacy" assumption and states that income is independent of degree assignment, conditional on the intermediate academic outcome. In our application, this assumption implies that unobserved selection can affect the quality and area of the degree from which students graduate but does not affect earnings after accounting for academic progression or graduation rates.

Using projected lifetime earnings as a primary outcome measure has two important advantages relative to using other academic measures, such as dropout rates. First, we use this projected earnings measure as a currency that allows us to compare individuals with different trajectories who would otherwise be unable to weigh up against each other.[29] Second, under the comparability surrogacy assumptions discussed above, we can interpret the projected lifetime earnings results as the long-term effects of the AA policy.

[29]Take, for example, the case of three individuals who enroll together in Nursing. Assume that after 4 years, the first individual remains enrolled in the same degree, the second individual drops out and switches to Medicine, and the third individual drops out of college. In this case, dropout rates would not be a good metric to compare the current academic status of the three students. High switching rates are commonplace in centralized systems. See Larroucau and Rios (2022) for evidence on switching rates into lower- and higher-quality degrees in Chile.

7. PARAMETER ESTIMATES

In this section, we present parameter estimates from our model. To estimate the model, we restrict our sample to every applicant who submitted an application to any federal institution in the state of Minas Gerais. We do this exclusively due to computational power constraints. This is the largest state in terms of the number of applications and represents 17% of all applicants in the system. In total, we have 17 institutions that offer 541 degrees (including the outside option). Our sample size consists of 327,633 students. We define a student as targeted if they graduated from a public high school, as reported in the ENEM data.

In Panel A of Table III, we present the parameter estimates from the school choice model. We find positive coefficients for the location dummy κ^t, which shows that students have preferences for studying close to where they live. We use these parameters to assess the in-sample model fit by comparing the admissions thresholds of degrees predicted by the model to those observed in the data. We discuss the construction of the predicted admissions thresholds in Appendix G, and present the scatterplot of simulated and observed admissions thresholds in Appendix Figure G.1. The correlation coefficient between simulated and observed admissions thresholds is virtually 1 for both open and reserved seats.

Next, we switch our attention to the outcome model estimates, which we report in Panel B. Parameters α_j^t and β_j^t of the potential outcome equation govern the structural relationship between lifetime earnings, degree attendance, and student characteristics. Our model estimation produces a vector of these estimates for each degree program and AA tuple. These estimates are unbiased but noisy measures of the underlying outcome parameters. To improve our estimates, we follow Abdulkadiroglu et al. (2020) and use an empirical Bayes shrinkage estimator to reduce sampling variance. This yields empirical Bayes posterior means of degree program-specific parameters, which we then use in our subsequent analyses (see Appendix H for more details on this procedure).

To visualize these parameters, we define the value-added of attending a given degree as the gains from attending that degree relative to those from attending the outside option; that is $Y_{ij} - Y_{i0}$. To calculate the value-added, we set the test score variable s_{ij} to be equal to the admissions cutoff score for open spots c_j^0, and we also set other covariates to the median value across all students, thus:

$$\mathrm{VA}_j^t = (\hat{\alpha}_j^t - \hat{\alpha}_0^t) + (\hat{\beta}_j^t - \hat{\beta}_0^t) \cdot c_j^0 + (\bar{X}_j - \bar{X}_0)'\hat{\Pi}^t,$$

TABLE III

MODEL PARAMETERS

	Targeted				Non-targeted			
	Mean	SD	P10	P90	Mean	SD	P10	P90
Panel A: School Choice Model								
Degree FE (δ_j^t)	73.160	24.327	41.590	102.53	121.945	40.940	74.623	175.16
Ability (γ_j^t)	-121.96	38.565	-171.00	-73.98	-185.04	56.245	-260.63	-119.69
Location (κ^t)	3.087	-	-	-	2.403	-	-	-
Substitution (σ^t)	7.180	-	-	-	8.156	-	-	-
Panel B: Potential Outcomes Model								
Degree FE (α_j^t)	16.396	0.444	15.926	16.847	16.878	0.231	16.642	17.138
Ability (β_j^t)	0.005	0.001	0.005	0.006	0.005	0.000	0.004	0.005
Selection in levels (ψ_j^t)	-0.010	0.008	-0.019	-0.000	-0.017	0.009	-0.028	-0.006
Selection in gains (ρ^t)	0.003	-	-	-	0.000	-	-	-
	(0.002)	-	-	-	(0.004)	-	-	-
Value-added (VA_j^t)	0.326	0.332	-0.033	0.732	0.307	0.260	0.005	0.647
Share of students	76.16%				23.84%			
Number of students	327,633							
Number of degrees	541							

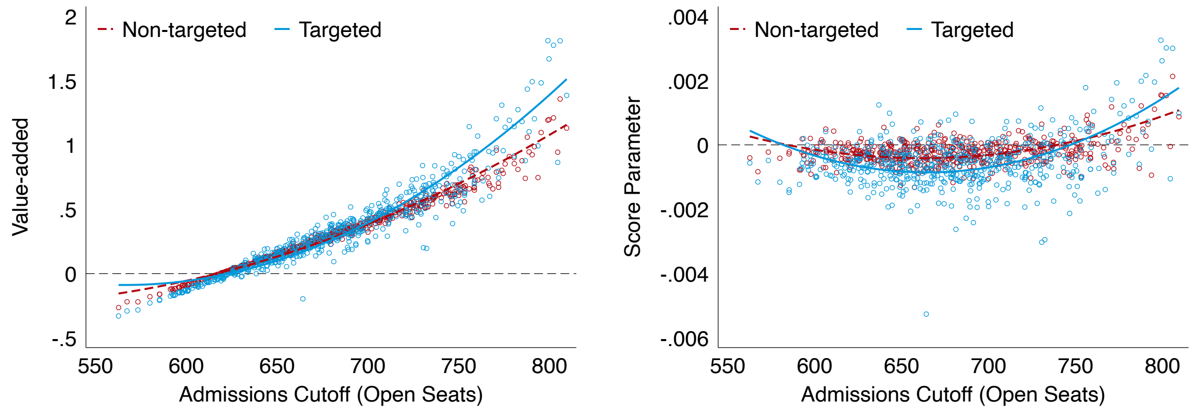
Note: This table summarizes parameter estimates from the school choice and the potential outcomes models. A student is considered targeted if, according to the ENEM data, they graduated from a public high school. Due to computational power constraints, we restrict our sample to every applicant who submitted an application to any federal institution in the state of Minas Gerais. Panel A presents school choice model coefficients from Equation (6.2). Panel B displays potential outcomes model parameters from Equation (6.7).

where $\bar{X}_j$ is the median value of X_{ij}.

In Figure 6(a), we plot the value-added of each degree against its selectivity level. We find that more selective degrees offer higher value-added, and that targeted students obtain higher value-added than their non-targeted counterparts. This is likely driven by non-targeted students having outside options with high returns, such as attending similar programs in private universities. In Figure 6(b), we plot the difference between the score parameter for every degree j and the outside option and order them by degree selectivity. We find that the relevance of test scores relative to the outside option does not increase with degree selectivity. In other words, test scores in federal degrees are as important as they are in the outside option for explaining lifetime earnings.

8. THE IMPACT OF AA ON WINNERS AND LOSERS

In this section, we use the parameter estimates from our model to compute student assignments, as well as their realized outcomes under different AA schedules. We then compare the overall



(a) Value-added (VA_j^t) (b) Score parameter ($\beta_j^t - \beta_0^t$)

FIGURE 6.—Parameters over degree selectivity. *Notes:* Panel (a) presents the degree's value-added for students with test scores evaluated at the open seats admissions cutoffs. The outcome variable is the log of projected lifetime earnings. Panel (b) presents the test score parameter of the potential outcome equation normalized relative to the test score parameter of the outside option. In both panels, the x-axis denotes degree selectivity as measured by the admissions cutoff of open seats c_j^0.

projected income gains and losses for targeted and non-targeted individuals and for the system as a whole.

8.1. *Estimating counterfactual assignments and outcomes*

We simulate assignments for a given AA schedule ω by leveraging the fact that we know the rules of the mechanism φ that generates the matching function μ between students and degree programs. As discussed in Section 5, the inputs of the mechanism, $\varphi(\Theta, q, \omega) = \mu$, are the set of student types Θ, degree capacities q, and the AA schedule ω. The vector of degree capacities, $q = \{q_0, ..., q_J\}$, is a policy choice that is decided together with the AA schedule. Since we are interested in learning about the consequences of AA, we keep q fixed and recover it from the observed data.

The first challenge is to recover the set of student types $\Theta = \bigcup_{i \in \mathcal{I}} \theta_i$, where $\theta_i = (\succ_i, s_i, t_i)$ is defined as a collection of a vector of student preferences $\succ_i$, test scores s_i and AA types t_i. Because we do not observe the full rank of student preferences, we recover $\hat{\succ}_i$ by evaluating indirect utilities from the school choice model introduced in Section 6.1 using preference parameter estimates from Section 7. To recover the remaining inputs to Θ, we assume no behavioral responses to the regulation, which allows us to recover inputs directly from the data. Specifically, we assume that the composition of applicants $\mathcal{I}$, application scores s_i, and AA status t_i, are fixed and invariant to changes in the AA schedule.[30]

The next step, after simulating student assignments, is to compute the realized outcomes associated with these assignments. We use selection and potential outcome parameter estimates from Equation (6.6) to simulate student outcomes for different AA schedules ω. We describe the simulation procedure in more detail in Appendix I.

This modeling assumes that potential outcomes are invariant to the AA regulation. There are two implications that we deem important to discuss. The first is that this assumption rules out, for instance, peer effects in the production function of degrees, as well as changes in the value of Y_{ij} that arise from stigmatization or from a reduction of the signaling value of degrees as a result of the AA regulation. The second implication of this assumption is that the value of the outside option is fixed. This assumption would be violated if non-targeted students displaced from public institutions were to crowd out other students from private institutions. This effect might create a crowding-out cascade extending throughout the whole higher education system. However, most private institutions, including those comparable to federal universities, are far from being capacity-constrained. Indeed, in our setting, only 1.9% of degree programs in the top 10% of selectivity are capacity-constrained; thus, crowding-out concerns are of second order.

8.2. *Impacts of the 50% AA policy*

We begin by contrasting simulated student outcomes between two AA schedules: one resembling the current policy ($\omega = 0.5$) and the other representing a laissez-faire scenario with no AA ($\omega = 0$). Under the latter scenario, all students compete for the same spots and thus face the same admissions thresholds. We present the distribution of admissions cutoffs in Appendix Figure A.5. A 50% AA counterfactual produces cutoff scores for open spots that are significantly higher than

[30] The assumption of no behavioral responses to test scores is consistent with findings by Francis and Tannuri-Pianto (2012) and Estevan et al. (2018) for public institutions in Brazil. They study AA regulations that increased the representation of disadvantaged groups at the University of Brasilia and the University of Campinas, respectively, and find no evidence of behavioral reactions regarding examination preparation efforts. The assumption of no behavioral responses to AA status is justified for two reasons. First, in our 2016 SISU sample, approximately 66% of all applicants had already completed high school before 2015. Considering the timing of the regulation—which requires students to have attended a public institution during the last three years of high school to qualify for AA—most applicants lacked any strategic incentives to switch schools. Second, recent evidence indicates that although some students opted to relocate in response to the quota, they predominantly moved from low-SES and low-performing private schools to even lower-SES and lower-performing public schools (Mello, 2023). Given the high selectivity of the federal institutions, these students are highly unlikely to receive an admissions offer.

those reserved for targeted students, which indicates that targeted students can gain admission to selective degrees with substantially lower scores. In the absence of AA, the distribution of cutoff scores becomes uniform across AA types and closer to the distribution of cutoff scores for open seats in the presence of AA. The overall results suggest that by removing the AA program, admission to selective degrees becomes much harder for targeted students but not substantially easier for non-targeted ones.

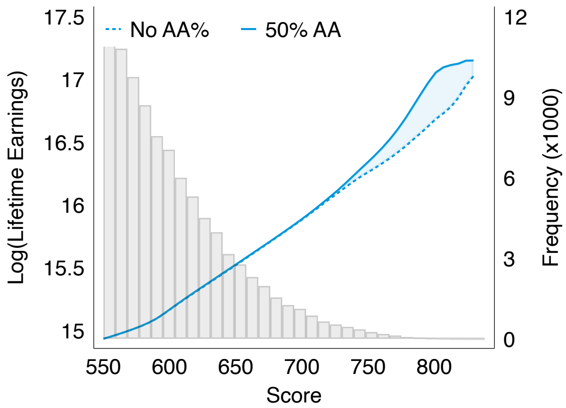
In Figure 7, we show the expected log of projected lifetime earnings as a function of student test scores under each of the counterfactuals. The dashed line denotes the scenario without AA, and the solid line indicates a counterfactual with 50% reserved seats. The gray bars in the background (and measured by the right-hand side axis) show the distribution of students over ENEM scores. In Panel (a), we demonstrate how the AA program generates significant gains for the targeted population, particularly for students with high test scores who can now enroll in more selective degrees with a higher value-added. However, as we show in Panel (b), targeted students' gains come at the expense of displacing non-targeted students from those selective degree programs.

Next, we examine the number of students affected by the policy. We observe large differences between the number of non-targeted and targeted students who undergo a change in their assigned degrees due to the policy. When the 50% AA policy is in place, a total of 10,080 targeted students switch degrees, out of which 9,320 come from the outside option. In contrast, a total of 14,300 non-targeted students are displaced from their degrees, out of which 9,320 are displaced to the outside option. This difference highlights the importance of taking into account the knock-on effects when evaluating the impacts of large-scale AA policy. Finally, among switchers, the average gains for targeted students are 67% larger than those of non-targeted students.

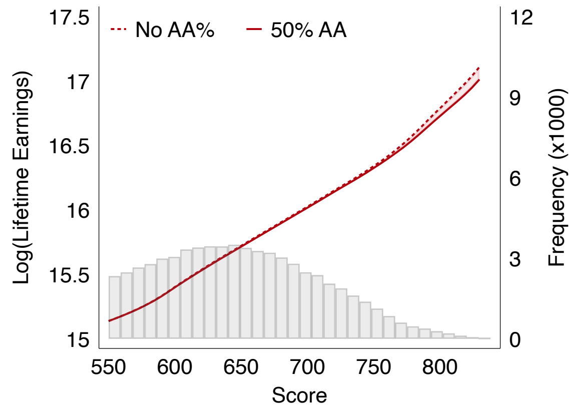
To take a stance on the efficiency impacts of the policy, we estimate the aggregate effects of AA on projected lifetime earnings across all students participating in the admissions process. Let $\Delta_t(\omega) = \Delta_t(\omega, 0)$ denote the aggregate gains for AA group t of moving from no AA to an ω AA schedule, as indicated by Equation (5.2). The overall aggregate gains over targeted and non-targeted individuals are defined as

$$\Delta(\omega, \lambda) = \Delta_0(\omega) + \lambda \Delta_1(\omega),$$

where λ denotes the welfare weight that captures society's concerns for fairness with respect to the targeted group.



(a) Targeted



(b) Non-targeted

FIGURE 7.—Projected lifetime earnings with and without AA. *Notes:* This figure shows the expected outcome for targeted and non-targeted students with and without AA across the score distribution. The blue line represents the expected outcome when $\omega = 0$ and the red line the expected outcome when $\omega = 0.5$. The gray bars in the background (and measured by the right-hand side axis) show the distribution of students over ENEM scores.

In Figure 8, we display the group-specific and overall changes in terms of projected lifetime earnings across various AA schedules, ranging from 0 to 50%. We normalize these changes in terms of the aggregated earnings for each AA group t when $\omega = 0$. The blue and red lines denote the gains and losses for targeted and non-targeted individuals, respectively. The gray line represents the normalized overall aggregate change using equal welfare weights across groups—that is, $\lambda = 1$.

Although the gains of the average benefited targeted student are greater than the losses of the average non-targeted displaced student, there are more displaced non-targeted students than benefited targeted students, as discussed above. Hence, integrating over all students in each group—including those unaffected by the policy—we find that the current AA schedule increases the projected earnings of targeted students by 1.02%, on average. In comparison, it reduces the projected earnings of non-targeted students by 1.34%, on average. Summing across both groups, the average student in the population experiences a 0.014% decrease in their projected income. Collectively, our findings indicate that the affirmative action policy led to nearly equivalent transfers from the non-targeted group to the targeted group without affecting the overall efficiency of the education system.

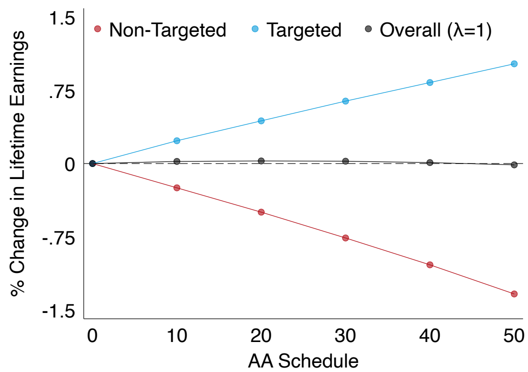


FIGURE 8.—Projected lifetime earnings under different AA schedules. *Notes:* This figure shows the overall gains and losses of AA in terms of projected lifetime earnings. We normalize the outcome with respect to the aggregate projected income in the absence of AA. We perform a separate normalization for each of the groups: targeted, non-targeted, and overall. The overall label denotes the outcome when individuals across both groups are given equal weights (i.e., $\lambda = 1$).

9. DISCUSSION AND CONCLUSION

In this paper, we study the distributional consequences of AA policies in centralized admissions systems. In addition to providing a controlled setting, analyzing AA in centralized systems is important due to its increasing adoption in over 45 countries for higher education admissions (Neilson, 2020). These systems rely heavily on standardized test scores to assign students to degrees. Because test scores are strongly correlated with socioeconomic status, reliance on such centralized systems will result in highly segregated student bodies. In this context, AA is a relevant policy lever for increasing diversity and the representation of minorities.

To understand the consequences of AA, we develop and estimate a model that links students' preferences for degree programs with the potential outcomes of attending each degree. AA increases targeted students' enrollment in selective degrees, positively impacting academic progress and implied lifetime earnings. Our findings indicate that the overall benefits for targeted students are similar in magnitude to the losses experienced by non-targeted students. These results suggest that introducing AA can increase overall equity without affecting the overall efficiency of the education system.

One key element we introduce in this paper is that targeted and non-targeted students may face different outside options. We highlight two features of the Brazilian context that shape these outside options for each group. First, there exists a sizable decentralized market in Brazil, not subject to AA, where non-targeted students can seek admission if they are displaced from the centralized system. This implies that non-targeted students facing displacement due to AA policies may not experience significant harm, as they have viable alternatives available in the private sector. Second, it is important to note that in our context, 15% of targeted students in the top quartile of ENEM performance have access to student loans that allow them to study in the private sector. This number is relatively large compared with the 4.3% of non-targeted students in the top quartile of ENEM performance who have access to loans. In the absence of student loans, targeted students would have different outside options, which would affect our efficiency estimates. We conclude that AA cannot be studied in isolation. Exploring this interaction is a direction for future research.

Finally, it is worth mentioning that this paper focuses on the first-order efficiency trade-off between targeted and non-targeted individuals. However, AA can also affect other education margins not considered in this paper. For example, universities can affect student outcomes through peer effects and influence intergroup attitudes by increasing diversity. In addition, a more diverse student body composition may change the production function of degrees by affecting students' academic outcomes, social behavior, and preferences. Understanding how these different margins interact with the direct distributional effects of AA is critical for understanding its overall role in shaping a higher education sector that fosters social mobility and promotes a healthy democracy.

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ONLINE APPENDIX FOR:

AFFIRMATIVE ACTION IN CENTRALIZED COLLEGE ADMISSIONS SYSTEMS

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APPENDIX A: ADDITIONAL TABLES AND FIGURES

TABLE A.I

2016 SISU APPLICANTS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Spots		Applications		Spots/	Admissions Cutoffs			
Admissions Pool	#	%	#	%	Apps	p10	p50	p90	p99
Open	73,739	48.81	1,657,328	46.70	4.45	586.7	657.0	737.0	795.4
Reserved, by Law									
Public HS	15,719	10.40	349,925	9.86	4.49	571.6	635.0	713.4	774.9
Public HS & Poor	16,451	10.89	405,783	11.44	4.05	560.1	619.6	683.8	746.9
Public HS & Race	22,173	14.68	395,115	11.13	5.62	555.9	612.5	681.1	742.3
Public HS & Race & Poor	23,001	15.22	740,422	20.83	3.11	552.9	604.1	663.9	728.0
Total	151,083	100	3,548,573	100					

Note: Elaboration based on SISU microdata of the first semester of 2016. We restrict the sample to federal institutions, as described in the main text. Columns (1)-(4) show the number of spots and applicants for each admissions pool. Column (5) shows degree selectivity as measured by the ratio of spots over the number of applications. Columns (6)-(9) show different percentiles of the distribution of admissions cutoff of degrees (weighted by the number of spots they offer).

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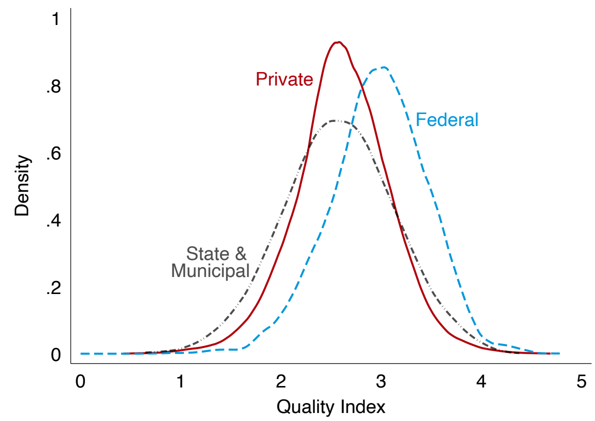


FIGURE A.1.—Quality distribution. *Notes:* This figure shows the distribution of quality as measured by an index from 1 to 5 prepared by the Ministry of Education to evaluate degrees between 2014 and 2016. An observation is a degree weighted by the number of students enrolled. State and municipal institutions are pooled together.

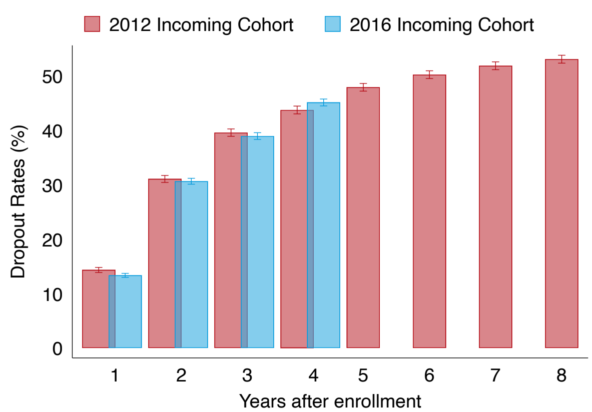
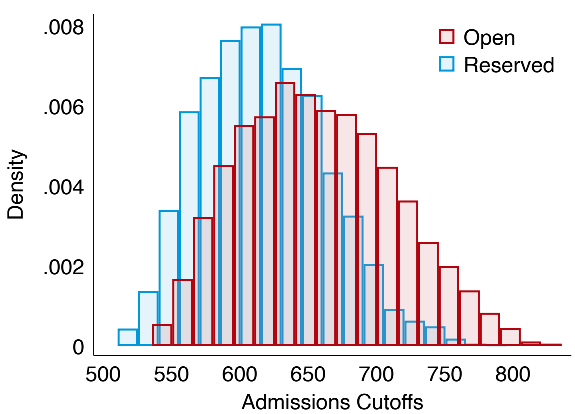
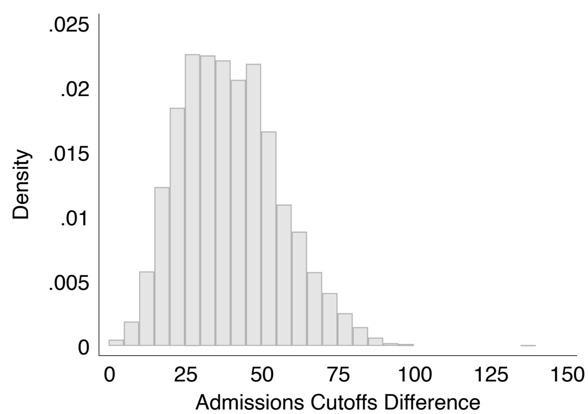


FIGURE A.2.—Dropout rates. *Notes:* This figure shows how dropout rates evolve after admission. As described in the main text, we restrict the sample to federal institutions participating in the 2016 SISU admissions process. We say that a student dropped out of a given degree if they are no longer successfully enrolled and have not yet graduated. The bars in red show the cumulative dropout rate over time for the 2012 incoming cohort. We observe that eight years after admission, around 50% of students have dropped out. The bars in blue report the cumulative dropout rates for the 2016 cohort, which is our sample of interest in all empirical exercises.

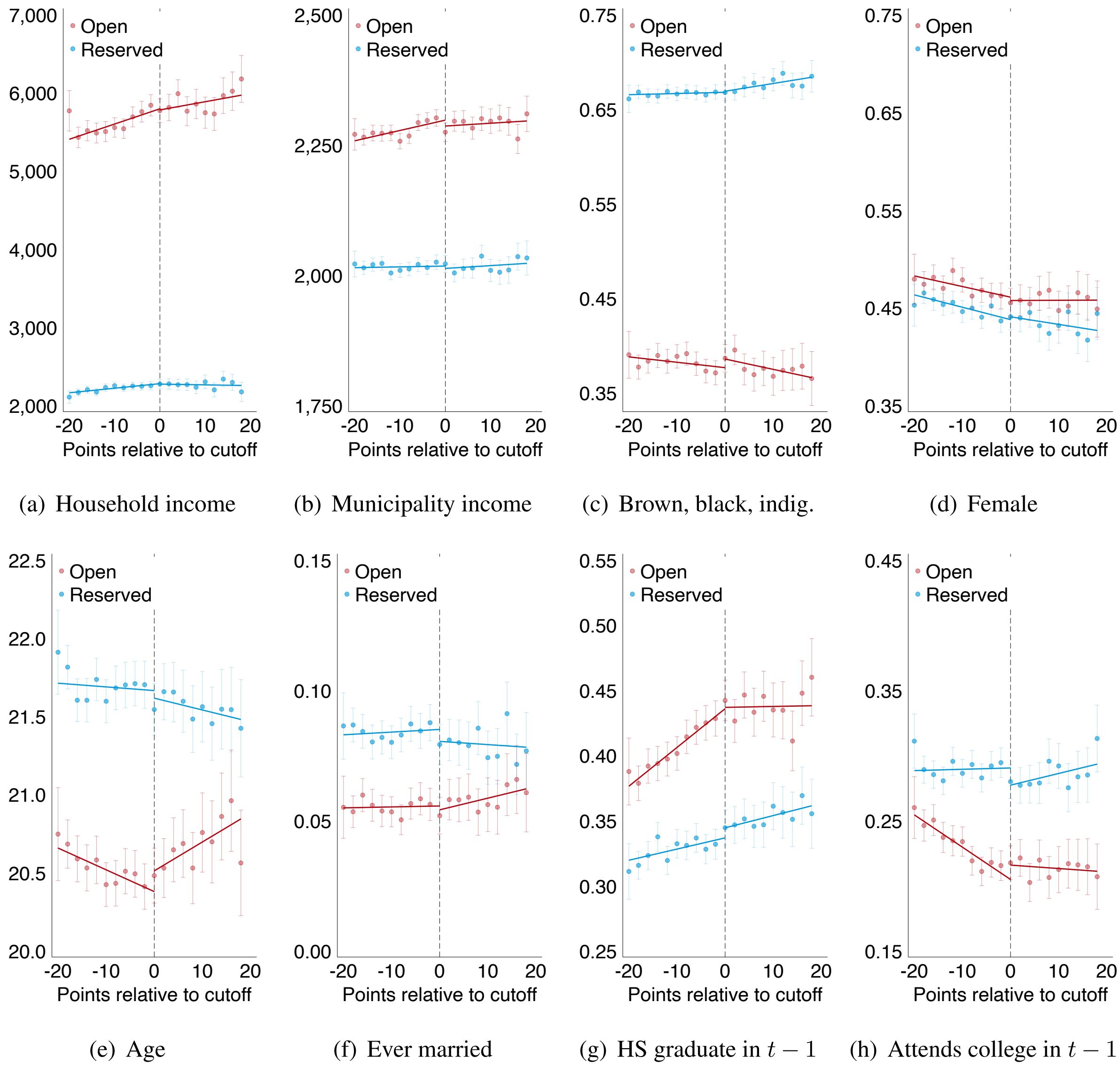


(a) Admissions Cutoffs



(b) Admission Cutoffs Differences

FIGURE A.3.—Distribution of admissions cutoffs. *Notes:* Panel (a) presents the distribution of admissions cutoffs for open and reserved seats for all degrees included in our sample. The cutoff for reserved seats is the average cutoff across all four types of reserved seats weighted by the number of seats. The average admissions threshold for open seats is 660 points, while the threshold for reserved seats is 619 points. Panel (b) shows the distribution of the difference between the admissions cutoffs for open and reserved seats. The mean of the difference is 41 points.



(a) Household income (b) Municipality income (c) Brown, black, indig. (d) Female

(e) Age (f) Ever married (g) HS graduate in $t-1$ (h) Attends college in $t-1$

FIGURE A.4.—Regression discontinuity estimates, balance tests. *Notes:* We pool all admissions lists and center them around the admissions cutoffs. We also pool individuals across all four types of reserved seats. The red and blue lines show the outcomes for students applying for open seats and reserved seats, respectively. The figure shows binned residualized mean outcomes for individuals in each type of seat for several educational outcomes. The error bars in a cell represent the 95% confidence interval for a given outcome mean. We include observations within a 20-point interval above and below the last admitted student and plot the fit of linear regressions on each side of the cutoff. The title of each panel denotes outcome variables. Period $t = -1$ represents the year 2015.

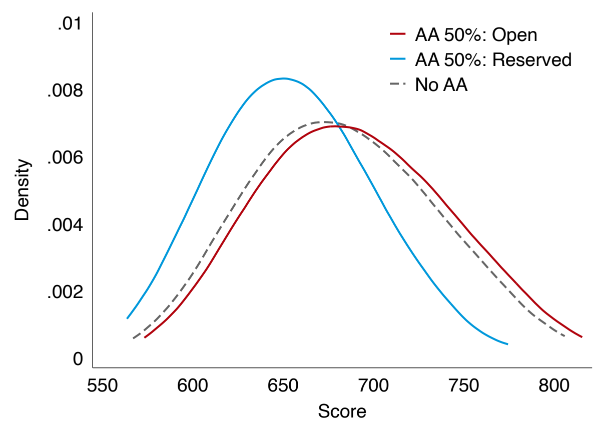
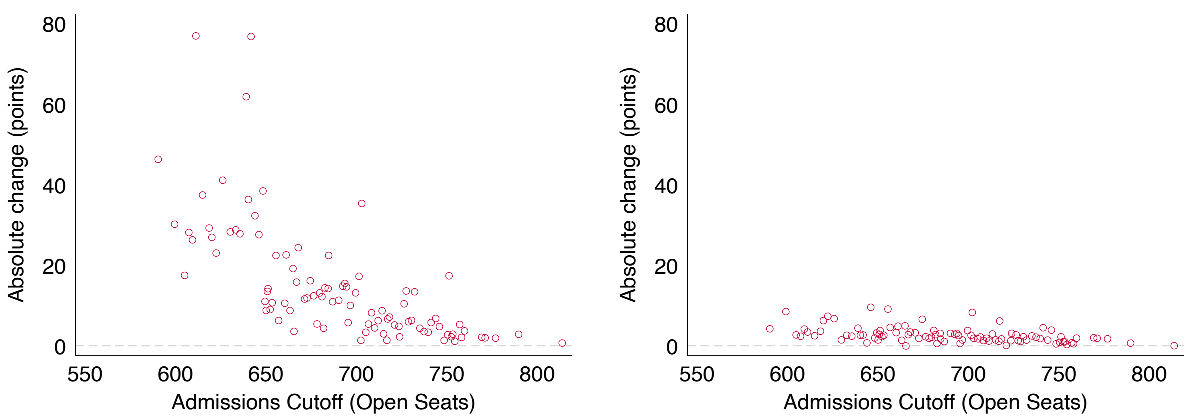


FIGURE A.5.—Counterfactual admissions cutoffs. *Notes:* This figure shows the distribution of simulated equilibrium cutoffs. The blue line shows the distribution of admissions cutoffs for open spots when $\omega = 0.5$, and the red line for reserved spots when $\omega = 0.5$. The dashed black line shows cutoffs faced by all students when $\omega = 0$.

APPENDIX B: ITERATIVE ADMISSION CUTOFFS

In an iterative deferred acceptance mechanism, students are sequentially asked to submit rank-ordered lists over the course of several "trial" days. At the end of each day, the system produces a cutoff grade that is the lowest grade necessary to be accepted by a specific program. Unfortunately, there are no administrative records of the admissions cutoffs reported by the system throughout the application period; the system only saves the final admissions cutoffs.

To circumvent this issue, we scraped online data on the degrees offered by the Federal University of Minas Gerais (UFMG) during the 2021 admissions period. In total, we observe 450 admissions cutoffs (90 degrees $\times$ 5 admissions tracks each) over the 9 days of the application process.



(a) Day 1 - Day T

(b) Day T-1 - Day T

FIGURE B.1.—Absolute change in admissions cutoffs *Notes:* This figure shows the absolute change in admissions cutoffs over degree selectivity, as defined by the degree's final admissions cutoffs of the open seats. Panel (a) displays the absolute difference between final admissions cutoffs and those reported on the first day of the system. Panel (b) displays the absolute difference between final admissions cutoffs and those reported on the penultimate day of the system.

Figure B.1 shows how the admissions cutoffs evolve over time. Panel (a) displays the absolute difference between the final admissions cutoffs (day T in the figure) and the one reported on the first day of the admissions period. We observe large differences in admissions cutoffs, especially for less selective degrees. Panel (b) displays the absolute difference between the final admissions cutoffs (day T in the figure) and the one reported on the last day of the admissions period. On the last day of the application period, most degrees converged or were very close to converging to the final admission cutoff. Overall, these data suggest that the ex-ante and ex-post eligibility for degrees are very similar.

These figures also allow us to address some of the concerns raised in Section 6.1 relative to the stability of the Brazilian mechanism. As discussed by Bó and Hakimov (2022), the “manipulation via cutoffs” involves a group of high-scoring students temporarily inflating the cutoffs for some degrees. This type of manipulation would manifest in the data as significant drops in cutoffs on the final day of application. Cutoffs between $T-1$ and T are similar, indicating that there is no evidence of manipulation via cutoffs.

APPENDIX C: CONTROL FUNCTION DERIVATION

This Appendix derives the control function expressions we use in Section 6.2. For a given individual i, a given degree k can fall into 4 cases:

- Case 1: $k = R_{i1}$
- Case 2: $k = R_{i2}$
- Case 3: $k \neq R_{i1}$, $k \neq R_{i2}$, and k is feasible
- Case 4: $k \neq R_{i1}$, $k \neq R_{i2}$, and k is not feasible,

where R_{ij} denotes the final jth option submitted by student i to the choice mechanism, where $j \in \{1, 2\}$. A degree k is feasible for individual i if $k \in \Omega_i$, as defined in Section 6.1. Note that k can represent the outside option, so we do not have to distinguish between students who list one or two options.

For ease of presentation, we define the following auxiliary variables:

$$\mathcal{P}_i(S'|S) = \sum_{j\in S'} \exp(V_{ij}) / \sum_{j\in S} \exp(V_{ij})$$

$$\mathcal{I}_i(S) = \overline{\eta} + \log \sum_{j\in S} \exp(V_{ij}).$$

Case 1: $k = R_{i1}$. We know that R_{i1} is preferred over any feasible alternative. This is similar to the chosen alternative in the standard approach of Dubin and McFadden (1984) or the highest-ranked alternative of Abdulkadiroglu et al. (2020). Then, $\lambda_{ik} = -\log\left[\mathcal{P}_i(k|\Omega_i \cup k)\right].$

Case 2: $k = R_{i2}$.

- **2.1.** *R_{i1} is feasible, and R_{i2} is not feasible.* Since we cannot learn anything about R_{i2}, then $\lambda_{ik} = 0$.
- **2.2.** *R_{i1} is feasible, and R_{i2} is feasible*. In this case, we can only learn that R_{i2} is worse than R_{i1}. So it is the same as the expression in Dubin and McFadden (1984) for the expected errors of alternatives that are not selected in the multinomial logit model:

$$\lambda_{ik} = \frac{\mathcal{P}_i(k|\Omega_i \cup R_{i1})}{1 - \mathcal{P}_i(k|\Omega_i \cup R_{i1})} \log \mathcal{P}_i(k|\Omega_i \cup R_{i1}).$$

- **2.3.** *R_{i1} is not feasible and R_{i2} is feasible.* In this case, we know that R_{i2} is less preferable than R_{i1}, and that R_{i2} is preferred over any other feasible option. So this is a special case of the formula for the not-highest-ranked alternatives in Abdulkadiroglu et al. (2020). The formula is:

$$\lambda_{ik} = -(V_{ik} + \overline{\eta}) + \frac{\mathcal{I}(\Omega_i) - \mathcal{P}_i(\Omega_i|\Omega_i \cup R_{i1}) \cdot \mathcal{I}(\Omega_i \cup R_{i1}).}{\mathcal{P}_i(R_{i1}|\Omega_i \cup R_{i1})}$$

To get this formula above, replace $j = 2$ in the last equation in Appendix B.1.1 in Abdulkadiroglu et al. (2020).

- **2.4.** *R_{i1} is not feasible and R_{i2} is not feasible.* In this case, we cannot learn anything about R_{i2} because the student will be assigned to the waitlist for R_{i1}, so their application to R_{i2} is meaningless. This implies $\lambda_{ik} = 0$.

Case 3: $k \neq R_{i1}, k \neq R_{i2}$, and k is feasible.

- **3.1.** *R_{i1} is feasible (and R_{i2} may be feasible or not).* In this case, the only information we have about k is that it is less preferred than R_{i1}. Thus, it is equivalent to the expression in Dubin and McFadden (1984) for the expected errors of alternatives that are not selected in the multinomial logit model:

$$\lambda_{ik} = \frac{\mathcal{P}_i(k|\Omega_i)}{1 - \mathcal{P}_i(k|\Omega_i)} \log \mathcal{P}_i(k|\Omega_i).$$

- **3.2.** *R_{i1} is not feasible and R_{i2} is feasible*. In this case, we know that both R_{i1} and R_{i2} are preferred over all feasible options that were not ranked. This is a special case of the formula for the unranked alternatives in Abdulkadiroglu et al. (2020). To get this formula, replace $l(i) = 2$ in the last equation in Appendix B.1.2 of Abdulkadiroglu et al. (2020). Then,

$$\lambda_{ik} = -(V_{ik} + \overline{\eta}) + \frac{[1 - \mathcal{P}_i(\Omega_i/k|\Omega_i \cup R_{i1}/k)]\mathcal{I}(k) - \mathcal{P}_i(k|\Omega_i) \cdot \mathcal{I}(\Omega_i) + \mathcal{P}_i(\Omega_i/k|\Omega_i \cup R_{i1}/k) \cdot \mathcal{P}_i(k|\Omega_i \cup R_{i1}) \cdot \mathcal{I}_i(\Omega_i \cup R_{i1})}{\mathcal{P}_i(R_{i2}|\Omega_i/k)^{-1} \cdot \mathcal{P}_i(R_{i1}|\Omega_i \cup R_{i1}) \cdot \mathcal{P}_i(R_{i2}|\Omega_i)}.$$

- **3.3.** *R_{i1} is not feasible and R_{i2} is not feasible.* This case is similar to case 3.1. described above, thus

$$\lambda_{ik} = \frac{\mathcal{P}_i(k|\Omega_i \cup R_{i1})}{1 - \mathcal{P}_i(k|\Omega_i \cup R_{i1})} \log \mathcal{P}_i(k|\Omega_i \cup R_{i1}).$$

Case 4: $k \neq R_{i1}, \neq R_{i2}$, and k is not feasible. We do not learn anything about k in this case, thus $\lambda_{ik} = 0$.

APPENDIX D: ADDITIONAL BALANCE CHECKS

D.1. *Grader Assignment*

In columns (1)-(4) of Table D.1, we present the OLS first-stage relationship between the average essay score shifter and essay test scores. The regression coefficient is 0.721 when using the full sample of ENEM takers. Although our sample is sufficiently large, this coefficient differs from 1 due to the introduction of the third grader when there is a disagreement between the two initially assigned graders. In column (2), we show regression estimates when we restrict the sample to individuals in our relevant sample in SISU 2016. The coefficient remains very similar to that of the full sample and drops slightly in magnitude to 0.693. Given that the allocation of graders is random, our score shifter measure should be uncorrelated with the student's performance on the other components of the test and any other important confounders. In column (3), we show that the first-stage coefficient is remarkably stable when adding a rich host of student-level controls. Finally, in column (4), we show the correlation relevant to the sample we use to estimate the model in Section 7.

In columns (5)-(8), we maintain the same set of specifications but change the dependent variable to the average score across the other four multiple-choice tests. We find a precise estimate of zero when we use the full sample of ENEM takers. When we restrict the sample to SISU applicants, we find a slight imbalance, but it is almost negligible in magnitude. In column (7), we see that the coefficient stays stable after including several control variables; this suggests that the minor imbalance is not correlated with any relevant student characteristic. Finally, in column (8), we show the balance test for our relevant sample and find a precise zero. Overall these results are consistent with a satisfactory randomized assignment of graders to students.

D.2. *Assignment of examination booklets*

Next, we assess the credibility of the randomness of the assignment of booklets to students. To reduce the dimensionality of our covariates and avoid multi-testing issues, we assess balance by checking the stability of the OLS coefficient when we include a comprehensive set of covariates. Specifically, we regress test scores on a given component of the test against booklet indicator dummies and present the results in Table D.2. To account for booklets being assigned within high schools, we include high school fixed effects in our main specification (odd columns). In a second specification, presented in even columns, we add a rich vector of student covariates,

TABLE D.1

FIRST-STAGE REGRESSIONS

Dependent Variable	Essay				Multiple Choice			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Essay score shifter (z_i)	0.721	0.693	0.695	0.712	0.002	-0.013	-0.012	-0.001
	(0.002)	(0.003)	(0.003)	(0.008)	(0.001)	(0.002)	(0.001)	(0.004)
Sample	All	SISU	SISU	Model	All	SISU	SISU	Model
Controls	No	No	Yes	Yes	No	No	Yes	Yes
Mean	543.8	580.6	576.9	602.7	504.4	524.1	524.1	545.8
Observations	4,790,888	2,185,800	2,185,800	326,781	4,790,888	2,185,800	2,185,800	326,781
R-squared	0.023	0.021	0.171	0.200	0.000	0.000	0.280	0.299

Note: The table presents the coefficients from OLS of ENEM scores on the score shifter z_i. Columns (1)-(4) use the score in the essay component as the main dependent variable, and columns (5)-(8) use the average score on the other four multiple-choice components of the test. Columns (3), (4), (7), and (8) include a host of student-level controls, including the type of high school attended, dummies for different races, the state of residency, gender, year of high school graduation, age, and marital status.

including age, gender, race dummies, parental education, household income, and answers on a comprehensive individual questionnaire from the ENEM. We find that coefficients are remarkably stable across specifications.

TABLE D.2

BOOKLET ASSIGNMENT

	History		Science		Math		Language	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Yellow	0.59	0.55	1.13	1.11	0.14	0.36	-1.53	-1.60
	(0.15)	(0.14)	(0.14)	(0.14)	(0.21)	(0.20)	(0.14)	(0.14)
Red	-0.34	-0.20	-1.64	-1.46	4.57	4.48	-0.68	-0.69
	(0.15)	(0.14)	(0.14)	(0.14)	(0.21)	(0.20)	(0.15)	(0.14)
Blue	0.32	0.39	2.19	2.36	2.76	2.93	-3.49	-3.53
	(0.15)	(0.14)	(0.14)	(0.14)	(0.21)	(0.20)	(0.15)	(0.14)
High school FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Note: This table shows estimates from a regression of test scores on booklets. The green booklet is the omitted category. Odd columns include only school fixed effects to account for booklets being assigned within schools. Even columns include a rich vector of student covariates, including age, gender, race dummies, parental education, household income, and high school fixed effects.

APPENDIX E: PROJECTED LIFETIME EARNINGS

Our main outcome of interest is the student's projected lifetime earnings. This Appendix describes the construction of such an outcome. The 2016 present value of lifetime earnings is defined by:

$$Y_i = \underbrace{\sum_{k=1}^{N_i} inc_i \left(\frac{1+g_i}{1+r}\right)^k}_{\text{Lifetime earnings}}.$$

This expression captures the lifetime earnings of student i, as defined by the present value of income flows observed seven years after starting college, inc_i. Unfortunately, our sample consists of 2016 SISU applicants—and many individuals are still enrolled in college, which means it is too early to find them in the labor market. Since we do not observe inc_i, we predict it using student intermediate outcomes, as described below. We assume an expected growth rate g_i equivalent to the growth rate observed for full-time employees in administrative matched employer-employee records between 2007 and 2017. We allow the g_i to vary by age, higher education attainment, and gender. To calculate the interest rate r, we use official interest rates reported by the Brazilian Central Bank, deflate them using monthly inflation rates, and compute the average real interest rate between 2007 and 2017.[1] Lastly, N_i represents the remaining years in the labor market, calculated by subtracting the student's age in 2016 from the official retirement age of 65 for men and 62 for women.

Next, we discuss the econometric procedure we use to predict inc_i based on students' academic trajectories between the 2016 and 2019 academic years. Table E.1 illustrates the problem. Assume 0 is not attending college, and 1 and 2 are degree programs in public and private institutions. For our sample, we observe trajectories represented by columns (1)-(4), but we do not observe income. Instead, for past cohorts, we observe both trajectories and students' income several years after taking the ENEM. Using these past cohorts, we create a mapping from trajectories to income and then apply this mapping to the 2016 SISU cohort to recover their predicted income.

Our sample of past cohorts consists of all ENEM takers between 2009 and 2010. We have one snapshot of income in 2017 from the administrative matched employer-employee records (RAIS).

[1] The interest rates can be downloaded here: https://www.bcb.gov.br/en/monetarypolicy/interestrates. Inflation rates can be accessed here: https://fred.stlouisfed.org/series/CPALTT01BRM659N.

TABLE E.1

EXAMPLES OF ACADEMIC TRAJECTORIES

	Degree in $t=1$ (1)	Degree in $t=2$ (2)	Degree in $t=3$ (3)	Degree in $t=4$ (4)	Income in $t=7$ (5)
Student A	1	1	1	1	400
Student B	1	0	0	0	100
Student C	1	2	2	2	300
Student D	0	0	0	2	150
Student E	2	2	2	2	350

Note: This table provides a simple example of the problem of predicting students' income. Columns (1)-(4) denote the academic trajectory of students for years 1 to 4. Column (5) shows the income observed for these students based on their trajectories. In this example, there are two different degree programs, 1 and 2, and one outside option denoted by 0.

Ideally, we would non-parametrically match individuals with identical academic trajectories to recover predicted income for the 2016 SISU sample. Although this exact matching is theoretically possible, it is unfeasible in our data due to the many possible combinations of degrees that define a trajectory.

We summarize the academic trajectory using degree attainment in year 1 and year 4 as an alternative approach. We also add a set of controls X_i that include gender and test scores in each of the five components of the ENEM and the traits targeted by the AA regulation (i.e., racial and high-school dummies). Specifically, we regress:

$$inc_i = \phi_t + X_i\pi + \delta_{J(i,t_1)} + \delta_{J(i,t_4)} + \epsilon_i,$$

where ϕ_t are dummies that indicate the year for the ENEM, degree fixed effects are captured by δ, and $J(i,t)$ is a function that indicates the degree student i attends in period t. We use these coefficients to predict the earnings of 2016 SISU applicants.

APPENDIX F: THE ROLE OF SURROGATES

In this Appendix, we discuss the econometric implications of using the log of projected lifetime earnings (which uses predicted income flows) instead of the log of observed discounted lifetime earnings (which uses observed income flows) as our outcome of interest. We start by rewriting our long-term outcome of interest $\tilde{Y}_{ij}$ as a function of an observed intermediate academic outcome P_{ij} (e.g., income flows as a function of college completion rates). We allow these variables to vary by degree-specific fixed effects and student test scores. For simplicity, we drop student covariates and assume that both outcomes and other regressors are residualized with respect to X_{ij} introduced in the article:

$$P_{ij} = \pi_j^t + \psi_j^t \cdot s_{ij} + \xi_{ij}/\zeta^t \tag{F.1}$$

$$\tilde{Y}_{ij} = \gamma_j^t + \rho_j^t \cdot s_{ij} + \zeta^t \cdot P_{ij} + \nu_{ij}, \tag{F.2}$$

where the term ξ_{ij}/ζ^t denotes the unobserved component of the intermediate outcome, and ν_{ij} represents the unobserved component of the long-term outcome equation after accounting for the intermediate outcome (we scale ξ_{ij} by ζ^t for expositional clarity).

We use Equations (F.1) and (F.2) and rewrite the long-term outcome $\tilde{Y}_{ij}$ as

$$\begin{aligned} \tilde{Y}_{ij} &= \alpha_j^t + \beta_j^t \cdot s_{ij} + \varepsilon_{ij} \\ &\equiv \alpha_j^t + \beta_j^t \cdot s_{ij} + \xi_{ij} + \nu_{ij}, \end{aligned}$$

where $\varepsilon_{ij} \equiv \xi_{ij} + \nu_{ij}$, and α_j^t and β_j^t are the same population parameters as in Equation (6.3). The conditional expectation of $\tilde{Y}_i$ given degree assignment D_i is

$$\begin{aligned} \mathbb{E}[\tilde{Y}_i|s_{ij}, D_i = j] &= \alpha_j^t + \beta_j^t \cdot s_{ij} + \mathbb{E}[\varepsilon_{ij}|s_{ij}, D_i = j] \\ &\equiv \alpha_j^t + \beta_j^t \cdot s_{ij} + \mathbb{E}[\xi_{ij}|s_{ij}, D_i = j] + \mathbb{E}[\nu_{ij}|s_{ij}, D_i = j]. \end{aligned}$$

Note that unobserved selection can arise from two sources in this context. The first is $\mathbb{E}[\xi_{ij}|s_{ij}, D_i = j]$, which implies that selection into degrees may correlate with intermediate outcomes in a way that is not fully captured by student observables. The second source is $\mathbb{E}[\nu_{ij}|s_{ij}, D_i = j]$, which encapsulates the effect of unobserved selection after accounting for the intermediate outcome.

The challenge is that we do not observe long-term outcomes $\tilde{Y}_i$ for our sample of interest. To overcome this issue, we follow Athey et al. (2019) and transform our intermediate outcomes into a "surrogate index"—that is, the predicted value of the long-term outcome given the intermediate outcomes and student covariates. The surrogate index for individuals in our sample of interest $\mathcal{N}$ is

$$Y^*_{ij} = \mathbb{E}[\tilde{Y}_i | s_{ij}, P_i, D_i = j, i \in \mathcal{N}].$$

To recover Y^*_{ij}, we proceed in two steps. First, we estimate the parameters of Equation (F.2) using older cohorts (which we denote by $\mathcal{O}$), for whom we observe both intermediate and long-term outcomes. Second, we use these coefficients to predict a measure of the long-term outcome Y^*_{ij} for our sample of interest $\mathcal{N}$. We describe the implementation of this procedure in Appendix E.

To recover unbiased parameters of the long-term outcome equation using the surrogate index, we establish two assumptions:

$$\begin{aligned} \text{A1 Surrogacy:} \quad & \mathbb{E}[\nu_{ij} | s_{ij}, P_{ij}, D_i = j] = 0 \\ \text{A2 Comparability:} \quad & \mathbb{E}[\tilde{Y}_i | s_{ij}, P_i, D_i = j, i \in \mathcal{N}] = \mathbb{E}[\tilde{Y}_i | s_{ij}, P_i, D_i = j, i \in \mathcal{O}]. \end{aligned}$$

Assumption A1 is usually referred to as the surrogacy assumption and states that the long-term outcome is independent of degree assignment, conditional on intermediate outcomes. In our application, this assumption implies that unobserved selection can affect the quality and area of the degree from which students graduate but does not affect earnings after accounting for academic trajectories and graduation rates. The second assumption ensures that the conditional expectation functions of long-term outcomes are comparable for samples $\mathcal{O}$ and $\mathcal{N}$. That is, the mapping from individual characteristics, degree assignments, and intermediate outcomes to long-term outcomes is fixed and invariant to the sample. This assumption rules out, for instance, any general equilibrium effects resulting from AA. Given these assumptions, we can rewrite the predicted long-term outcome Y^*_{ij} for individuals in sample $\mathcal{N}$ as

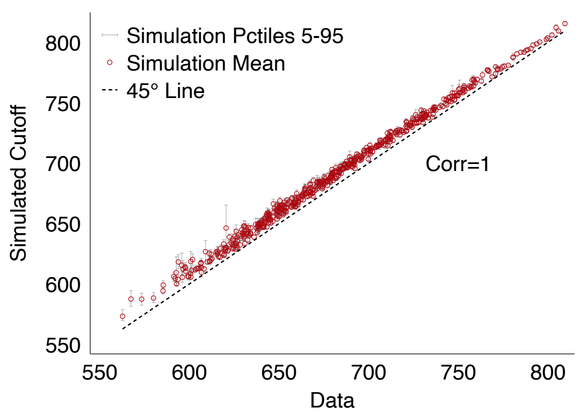
$$Y^*_{ij} = \alpha^t_j + \beta^t_j \cdot s_{ij} + \varepsilon_{ij}, \tag{F.3}$$

which recovers the same parameters of our main potential outcome model described by Equation (6.3).

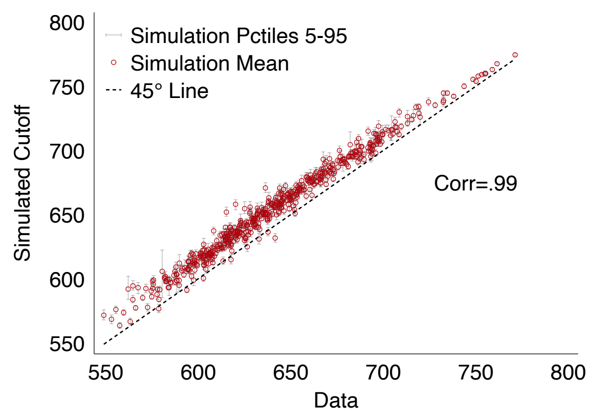
APPENDIX G: MODEL FIT

To verify that our parameters can recover the observed allocation, we simulate the admissions cutoffs predicted by the model and contrast them with those observed in the data. To recover admissions cutoffs, we follow steps 1 to 5 described in Appendix I and simulate 100 admissions cutoffs for each degree program.

Figure G shows the model fit for admissions cutoffs for open and reserved seats. On the horizontal axis, we plot the observed admissions cutoff scores; on the vertical axis, we plot the average simulated admission cutoff score across all simulations and the 95% coverage interval. It is important to note that in our data, we observe four admissions cutoffs for the reserved spots: one for each affirmative action type. To construct the observed cutoff, we take the average of the four admission cutoffs weighted by the number of seats offered in each AA track. We show that the admissions cutoffs predicted by our model align with those observed in the data. The correlation coefficient of the slope is virtually 1 for both open and reserved seats.



(a) Open



(b) Reserved

FIGURE G.1.—Model fit. *Notes:* This figure shows the in-sample model fit for the admissions cutoffs of open and reserved seats. On the horizontal axis, we plot the observed cutoff score; on the vertical axis, we plot the average cutoff score across 100 simulations and the 95% coverage interval.

APPENDIX H: EMPIRICAL BAYES SHRINKAGE

This Appendix describes the procedure for computing empirical Bayes posterior means for our parameters of interest. Parameters α_j^t, β_j^t, and ψ_j^t of the potential outcome equation govern the structural relationship between projected lifetime earnings, degree choices, and student characteristics. Our model estimation produces a vector of estimates for each degree program and AA tuple, $\hat{\varphi}_j^t = \left[\hat{\alpha}_j^t, \hat{\beta}_j^t, \hat{\psi}_j^t\right]'$. These coefficients are unbiased but noisy estimates of the underlying outcome parameters φ_j^t. We follow Abdulkadiroglu et al. (2020) and use a hierarchical Bayesian model to improve our parameter estimates. Specifically,

$$\hat{\varphi}_j^t | \varphi_j^t \sim N(\varphi_j^t, \Psi_j^t),$$
$$\varphi_j^t \sim N(\mu_{\varphi^t}, \Sigma_{\varphi^t}),$$

where Ψ_j^t is the sampling variance of the estimator $\hat{\varphi}_j^t$, and $\mu_{\varphi_j^t}$ and $\Sigma_{\varphi_j^t}$ are hyperparameters that govern a prior distribution for φ_j^t.

We estimate $\mu_{\varphi_j^t}$ and $\Sigma_{\varphi_j^t}$ via maximum likelihood. Conditioning on the hyperparameters, μ_{φ^t}, and Σ_{φ^t}, the likelihood function is

$$\mathcal{L}(\hat{\varphi}_j^t | \mu_{\varphi^t}, \Sigma_{\varphi^t}, \hat{\Psi}_j^t) = (2\pi)^{-\frac{K}{2}} |\hat{\Psi}_j^t + \Sigma_{\varphi^t}|^{-\frac{1}{2}} \exp\left(-\frac{1}{2}(\hat{\varphi}_j^t - \mu_{\varphi^t})'(\hat{\Psi}_j^t + \Sigma_{\varphi^t})^{-1}(\hat{\varphi}_j^t - \mu_{\varphi^t})\right),$$

where $K = dim(\hat{\varphi}_j^t) = 3$. The estimates of the hyperparameters are given by:

$$\left(\hat{\mu}_{\varphi^t}, \hat{\Sigma}_{\varphi^t}\right) = \arg \max_{\mu_{\varphi^t}, \Sigma_{\varphi^t}} \sum_j \log \mathcal{L}(\hat{\varphi}_j^t | \mu_{\varphi^t}, \Sigma_{\varphi^t}, \hat{\Psi}_j^t).$$

Finally, the empirical Bayes posterior mean for φ_j^t is given by

$$\hat{\varphi}_j^{t*} = \left((\hat{\Psi}_j^t)^{-1} + (\hat{\Sigma}_{\varphi^t})^{-1}\right)^{-1} \left((\hat{\Psi}_j^t)^{-1}\hat{\varphi}_j^t + (\hat{\Sigma}_{\varphi^t})^{-1}\hat{\mu}_{\varphi^t}\right),$$

where $\hat{\Psi}_j^t$, $\hat{\mu}_{\varphi^t}$, and $\hat{\Sigma}_{\varphi^t}$ are the estimates of Ψ_j^t, μ_{φ^t}, and Σ_{φ^t}, respectively.

APPENDIX I: SIMULATION PROCEDURE

We describe the procedure used to simulate the counterfactuals. Let $M = 100$ denote the number of Monte Carlo simulations. The m^{th} simulations for a given AA schedule ω and the associated realized outcomes, works as follows:

1. Simulate a vector of unobserved tastes $\eta_{ij}^m \sim EVT1$.
2. Compute preferences $\hat{\succ}_i^m$ that reflect indirect utilities $\hat{u}_{ij}^m$ using preference estimates $(\hat{\delta}_j^t, \hat{\gamma}_j^t, \hat{\kappa}^t, \sigma^t)$ together with η_{ij}^m.
3. Construct the set of student types as $\hat{\Theta}^m = \bigcup_i \hat{\theta}_i^m$, where $\hat{\theta}_i^m = (\hat{\succ}_i^m, s_i, t_i)$.
4. Compute the matching function $\varphi(\hat{\Theta}^m, q, \omega) = \hat{\mu}^m$ based on mechanism φ's rules.
5. Calculate cutoff scores $c_{jt}^m(\hat{\mu}^m)$ that are consistent with the equilibrium.
6. Use Equation (6.6) to compute the predicted log potential outcome as $\hat{\tilde{Y}}_{ij}^m = \hat{\alpha}_j^t + \hat{\beta}_j^t \cdot s_{ij} + X_{ij}' \hat{\Pi}^t + \sum_{k=1}^J \hat{\psi}_k^t \cdot (\hat{\eta}_{ik}^m - \bar{\eta}) + \hat{\rho}^t \cdot (\hat{\eta}_{ij}^m - \bar{\eta})$.
7. Transform outcome variable $\hat{\tilde{Y}}_{ij}^m$ from logs to levels $\hat{Y}_{ij}^m$ implementing the following steps:
   - 7.1 Compute the log potential outcome for the assignment observed in the data $\hat{\tilde{Y}}_i^m(\mu^{\text{data}}) = \sum_j \mathbb{1}\{D_i(\mu^{\text{data}}) = j\} \cdot \hat{\tilde{Y}}_{ij}^m$.
   - 7.2 Compute prediction errors: $\epsilon_i^m = \tilde{Y}_i - \hat{\tilde{Y}}_i^m(\mu^{\text{data}})$, with empirical distribution $\hat{F}(\epsilon^m)$.
   - 7.3 Compute the transformed outcome variable $\hat{Y}_{ij}^m$ by integrating over $\hat{F}(\epsilon^m)$

$$\hat{Y}_{ij}^m = \frac{1}{B} \sum_{b=1}^{B} \exp(\hat{\tilde{Y}}_{ij}^m + \epsilon_b^m),$$

     where ϵ_b^m is an error resampled from $\hat{F}(\epsilon^m)$, and B is the number of resamples.

8. Compute the realized transformed outcome for the corresponding matching function $\hat{Y}_i^m(\hat{\mu}^m) = \sum_j \mathbb{1}\{D_i(\hat{\mu}^m) = j\} \cdot \hat{Y}_{ij}^m$.

Thus, the expected realized outcome for individual i under affirmative action schedule ω is

$$\bar{\hat{Y}}_i = \frac{1}{M} \sum_m \hat{Y}_i^m(\hat{\mu}^m).$$