Affirmative Action in Centralized College Admission Systems*

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Abstract

This paper empirically studies the distributional consequences of affirmative action in selective college education in the context of a centralized admission system. We examine the effects of a law implemented in Brazil, mandating all federal public universities to increase the number of seats reserved for public high-school students to half of the total. We find that after the policy was put in place, the student body composition in public institutions became more similar to that of the applicant pool. To study the distributional consequences of the policy, we leverage admission discontinuities, and develop and estimate a model of school choice and educational outcomes. We use the rules of the centralized admission system to simulate counterfactual allocation of spots under different affirmative action regimes. We find that the policy creates income increases of around 1% for targeted students while imposing a similar cost on non-targeted individuals.

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1 Introduction

Affirmative action (AA) is widely used across the world to increase the presence of underrepresented groups in more selective colleges and universities. It works by placing preferential weight on applications by students from marginalized groups at the expense of displacing non-targeted students. The prioritization of one group over another elicits polarized reactions that makes this one of the most divisive regulations in higher education policy (Arcidiacono and Lovenheim, 2016).[1]

The discussion surrounding the value of AA policies centers around a hypothetical tradeoff between equity and efficiency. Equity arguments revolve around the role of AA in reinforcing the equalizing role of colleges and promoting diversity as a pillar of a sustainable and healthy democracy (Singer, 2011; Alon, 2015; Chetty et al., 2020). While most policy efforts have been motivated by the equity merits of AA, much of the debate has focused on its impacts on the efficiency of the higher education system.

At the heart of the efficiency discussion is the fact that AA pushes targeted students into selective degrees by discriminating against allegedly more qualified candidates. There is contention around whether this results in net positive efficiency benefits because theory fails to provide unambiguous predictions of the effects of AA on winners and losers. On the one hand, AA policies could reduce allocative efficiency by undermatching high-performing non-targeted students to colleges. In addition, it may even harm targeted students by placing them in schools for which they are ill-prepared (also known as the "mismatch theory") (Sander, 2004). On the other hand, AA could increase efficiency by improving the matching between students and colleges if it helps to correct for test scores that are statistically biased against targeted students. Similar efficiency arguments can be made if displaced individuals have access to alternative private colleges that are not affordable to targeted students. In spite of such conflicting predictions, quantitative evidence on the the distributional and welfare consequences of AA is limited.

This paper addresses these issues by leveraging a large-scale AA regulation in college education implemented in Brazil between 2013 and 2016. We provide new evidence on the impact of such policies on both targeted and non-targeted students in the context of a centralized admission system. The regulation mandated all federal public universities to increase the number of reserved seats for students from public high-schools to 50 percent of the total number of incoming students in every program. The policy heavily targeted low-income and marginalized racial groups –who account for most public high school enrollment– under the rationale that members of disadvantaged groups should not be underrepresented relative to their proportion in the community as a whole.

We combine several datasets to a characterize the population affected by the AA policy, and to determine its impact on several outcomes of interest. We have access to detailed administrative education data including students' performance on the national university entrance exam, appli-

[1] AA in the United States has been at the center of this dispute, with the Supreme Court having rejected a number of university admission procedures explicitly favoring disadvantaged groups (e.g., Regents of the University of California vs. Bakke, 1997).

cation portfolios to public universities, and admission offers from these institutions. We combine these administrative data with the Brazilian Higher Education Census to track students' progress and academic success in the universe of institutions and degree programs over time. Finally, we use matched employer-employee records comprising virtually all formal employment in Brazil, to assess the impact of the AA policy on expected earnings. Overall, these datasets provide a rich and comprehensive characterization of all individuals potentially affected by the AA policy.

A major challenge in studying AA is the general lack of transparency in college admissions. Brazil's AA policy allows us to overcome this difficulty because it is embedded in a centralized system. As such, it provides three big advantages relative to the literature. First, we can use admission discontinuities to estimate the returns to being admitted to a selective degree for each AA group. Since we observe students' AA status, we can estimate the effects of AA directly, rather than inferring these from the overall effects for students from a given demographic group. Second, we can leverage known admission rules to simulate variations in the AA schedule that change the allocation of students to degree programs. In addition, the centralized system also allows us to incorporate the knock-on effects of displaced students within the system. Third, because individuals are allocated based on their test scores, we have an observable and well-defined measure over which we can quantify the potential mismatch between students and degrees produced by AA.

Another feature that makes Brazil a compelling setting, is that public federal institutions are both free and substantially higher quality than their private counterparts. This makes them very attractive to high-performing students from low- and high-SES backgrounds alike. In practice, federal universities play a similar role to flagship state universities in the United States. They are typically the most prominent, elite, and selective universities in their specific states. In 2012, the average admission threshold across all degrees in public institutions was one standard deviations higher than the score of the average national exam taker.

As a result of their attractiveness and selectivity, federal institutions are highly segregated. In 2012, one year before the AA regulation was introduced, public high-school graduates represented 60 percent of admissions to all federal universities, and 43 percent of admissions to the 10 percent most selective federal degree programs. These proportions are much lower than the 82 percent share of overall national exam takers that these students represent. In 2016, when the AA policy was fully in place, the share of public high-school graduates admitted was substantially closer to their proportion in the population: now represented 66 percent of admissions to all federal universities, and 55 percent of admissions to the 10 percent most selective federal degree programs.[2]

We argue that the AA policy changed the composition of the student body by differentially

[2]This results are in line with Mello (2021). She exploits the progressive rollout of the AA policy to show that it increased the share of targeted students enrolled in federal institutions. Specifically, her study shows that the full adoption of the AA regulation, from 0 to 50 percent of reserved seats, increased enrollments of targeted students by 9.9 percentage points for the average degree program. It is important to notice 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. Appendix Figure I.2 shows the change in the distribution of reserved shares over time.

affecting the probability of admission of targeted and non-targeted individuals, through changes in the admission thresholds for reserved and open seats. In 2016, the average difference in the admission thresholds for reserved and open seats across all degrees was 0.4 standard deviations (SD) in test scores.

We are interested in the consequences of the AA policy, which are given by how much winners win and how much losers lose in terms of an outcome of interest. Accordingly, the gains and losses are characterized by how much changes in access to degrees impact students' realized outcomes. We employ two different but complementary econometric approaches to estimate these gains and losses in the data. In the first approach, we take advantage of degree admission cutoffs and construct regression discontinuity estimates by comparing individuals in similar waiting lists who were just above the threshold and offered admission, to those who were just below the threshold and thus not admitted. We exploit the fact that reserved and open seats have different admission cutoffs, to construct credible instruments from discontinuities for each of these groups. This research design allows us to causally estimate the cost-benefit ratio of marginally increasing the number of reserved seats in a given degree.

Our overall finding is that targeted students benefit from AA, and that their gains more than compensate for the losses experienced by marginally displaced students. We highlight two results supporting this claim. First, we examine the effects of gaining admission to a federal degree for the marginal targeted and non-targeted students in terms of the quality of the degree they attend four years later (as measured by average test scores). This outcome captures the net effect of gaining admission to a higher quality federal degree and individuals' academic paths after admission. The latter includes the results of their choices around enrollment (both inside and outside the federal system) and drop out. In contrast with the predictions of the mismatch hypothesis, we find that AA improves the quality of the degree attended by the marginal targeted student four years later. Moreover, we find that gaining admission to a federal degree increases degree quality by 52 percent more for marginal targeted students than for marginal non-targeted students.

Second, we estimate the effect of gaining admission into a federal degree on expected 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 of older cohorts and construct a measure that captures degree-specific value-added in earnings, together with the academic path of students. We use this measure to predict earnings for our sample four years after application. While the marginally displaced individual does not see a decrease in their expected earnings, the marginally benefited targeted student experiences an average gain of 270 BRL per month. This gain is equivalent to a 10% increase relative to the average expected salary of targeted students just below the admission threshold.[3]

In the second econometric approach, we study the effects of larger changes in AA by estimating a joint model of school choice and potential outcomes. We use the model to simulate counterfactual

[3] The 270 BRL monthly gain is equivalent to an annualized gain of 1,000 USD.

degree assignment and outcome realizations under different AA schedules. The first part of our model involves recovering student preferences over degrees. We use the 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. We allow students' indirect utility to include degree-specific fixed effects, parameters that vary with observed student characteristics, and an unobserved degree-specific taste. Following Fack et al. (2019), we restrict individuals to choose only among those degree programs available to them given their test scores, AA status and the degree's admission cutoffs. The identification of the model relies on a stability property of the deferred acceptance mechanism, which implies that students enroll in the most preferred program for which they are eligible.

Analysis using preference parameter estimates reveals that the preference for federal degrees relative to the outside option is higher for targeted students than for non-targeted students. The latter are twice as likely to switch to the outside option when their preferred degree is removed from their choice set. The outside option in our model comprises all degrees outside the federal system, as well as the option of not enrolling in college, or deferring for another year.

The second part of our model involves estimating the outcomes that would be realized under these 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 that is not captured by students' observables. The ideal solution to this problem –an experiment in which students are randomized to degrees, all else equal– is not possible. To circumvent this identification issue, we control for a rich set of covariates, including test scores in each of the components of the national exam. In addition, we use a selection correction approach by following the multinomial logit control function estimator of Dubin and McFadden (1984) and Abdulkadiroglu et al. (2020). Our model is flexible, as it can accommodate a variety of unobserved selection schemes, including selection based on student- and degree-specific unobserved matching effects.

To identify our outcome model, we rely on an exogenous score shifter. This variable aims to mimic random shocks to student test scores. The idea is that the score shifter combined with degree-specific admission thresholds, alters the degree programs that are available for otherwise identical individuals. The score shifter exploits two exogenous sources of variation in student test scores. The first source stems from random assignment of the written component of the national exam to graders of varying strictness. The second source of variation arises from plausibly random assignment of students to examination booklets in the multiple choice component of the test. While the only difference across booklets is the question order, some booklets are, on average, more difficult than others since they include easier questions later in the test.

We combine these two sources of variation and create a leave-one-out measure characterizing the relative grading strictness and booklet difficulty faced by a given student relative to the average 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 shifter increases average test scores by 0.17 standard deviations. In addition, we show that receiving a positive score shifter is strongly associated with a higher probability of attending a federal degree, with attending a higher quality degree within the federal system, and with higher expected earnings.

We use the score shifter to create a corrected measure of student test scores by netting out the effect of the score shifter from the observed student test scores. The identification assumption of the outcome model is that, while test scores affect the availability of degrees, only corrected test scores enter into the potential outcome equation. Intuitively, our identification argument compares two individuals with identical corrected test scores and unobserved tastes for degrees, but who fall on different sides the admission cutoff for a given degree as a result of having different score shifters. The differences in potential outcomes between these students can only be explained by differences in degree availability, and thus identifies the causal treatment effect of being offered admission into a degree. This strategy allows us to estimate treatment effects for individuals with corrected test scores away from the admission discontinuities, by focusing on individuals who randomly received large score shifters.

We find that targeted individuals reap higher value added of attending federal degrees than non-targeted individuals. This result is consistent with the fact that non-targeted students have access to a better outside option than their targeted counterpart: conditional on test scores, non-targeted students attend private institutions that are higher quality and have higher tuition. Interestingly, we also find that conditional on degree attendance student test scores do not explain higher earnings. This finding suggest that the potential mismatch in terms of test scores induced by the AA regulation is unlikely to affect realized outcomes.

We then use our model to estimate the distributional consequences of a 50 percent AA schedule. We simulate the allocation of students with and without the AA policy. We find that, under AA, targeted students increase their enrollment in selective degrees while displacing non-targeted students to less selective alternatives. Most of the change in the student body compositions occurs at the most selective degrees. We use our potential outcomes estimates to calculate the gains and losses for beneficiaries and displaced individuals in expected earnings. We find that the AA policy creates large income benefits for the targeted group while imposing a smaller cost on non-targeted individuals. These results suggest that introducing AA can increase the overall equity without affecting the overall efficiency of the education system.

Our paper is related to several strands of the literature. First, it contributes to the literature studying the effects of admission into selective colleges on educational and labor market outcomes. Several studies have used quasi-experimental research designs exploiting admission discontinuities to document significant benefits for the marginally admitted student (Hoekstra, 2009; Zimmerman, 2014; Canaan and Mouganie, 2017; Zimmerman, 2019; Sekhri, 2020; Jia and Li, 2021; Bleemer, 2021b).

These findings have been replicated in contexts in which an AA policy has effectively lowered the admission threshold for targeted students. Bagde et al. (2016) study an AA policy in India that reserved 20 percent of seats in engineering colleges for low-caste or female students. They find that the policy increased college graduation of targeted individuals. Closer to our setting, Francis-Tan and Tannuri-Pianto (2018) examines a race-based AA policy in a federal institution in Brasil that reserved 20 percent of seats for black students. They exploit a sharp discontinuity in the admissions and find positive academic and labor market impacts on targeted students.

Understanding the impact of AA on outcomes later in life has proven to be a difficult task in the U.S., owing to the lack of policy variation. A key concern that has drawn the attention of academics and policymakers is the possibility that AA may create a mismatch between the academic preparedness of targeted students and their peers' academic qualifications. Several papers address the mismatch hypothesis with varying results depending on the construction of the counterfactuals (Ayres and Brooks, 2004; Ho, 2004; Chambers et al., 2004; Sander, 2004; Alon and Tienda, 2005; Fischer and Massey, 2007; Rothstein and Yoon, 2008; Arcidiacono et al., 2011). One exception is Bleemer (2021a) who uses a differences-in-differences to evaluate the impacts of switching from race-sensitive to race-neutral admission policies at California public universities. He finds no evidence of mismatch. Specifically, he shows that after the AA removal, underrepresented minorities lowered their degree attainment and experienced wage declines.

One particularly relevant paper, in that –like our own– it estimates the effects of AA on both targeted and displaced students is Black et al. (2020), who study the trade-offs between winners and losers of the Top Ten Percent policy in Texas. They find that while benefited students increased college enrollment, graduation rates and earnings, students displaced by the policy do not see declines in these outcomes.

Another set of papers focus on conducting structural policy analysis of college admissions in the U.S. These papers emphasis different margins of the college admissions and simulate how AA could affect student enrollment and outcomes (Arcidiacono, 2005; Howell, 2010; Bodoh-Creed and Hickman, 2019; Chetty et al., 2020; Kapor, 2020; Bleemer, 2021b). We contribute to these papers by providing counterfactual analysis with actual policy variation in which the characteristics of targeted population and the college admissions rules are known.

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 a literature connecting 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 a recent literature that endeavors to extrapolates 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; Dong and Lewbel, 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 the admission discontinuities together with a continuous instrument that shifts students' test scores.

The structure of the article is as follows. The next section introduces the setting and provides extensive details of the policy and its implementation. Section 3 discusses the data and provides descriptive facts about the Brazilian college admissions. Section 4 presents a conceptual model that characterizes de impacts of AA in a centralized admission system. In Section 5 we present the regression discontinuity evidence together with the variation arising from the score shifters. We describe the school choice and potential outcome econometric model in Section 6, and present the parameters in Section 7. Section 8 examines the effect of alternative policy counterfactuals and Section 9 concludes.

2 Institutional background and regulation

In this section, we describe the higher education sector in Brazil and the admission process to federal institutions. We also document a strong correlation between college entrance exam scores and the traits targeted by the AA policy. Finally, we describe the details of the affirmative action regulation and its implementation over time.

2.1 Higher education in Brazil

The Brazilian post-secondary education system is a mixed system composed of public and private institutions. The system is highly liberalized and market-oriented; approximately 76 percent of students are enrolled in private fee-charging colleges.[4] The remaining 24 percent attend public institutions. These institutions charge virtually no tuition fees and are associated with the federal, state, or municipal government, depending on the source of funding. In 2016, there were a total of 107 federal institutions, which served nearly 1.25 million students, and offered more than 6,000 different degrees. Federal institutions comprise 63 universities and 44 vocational institutions, with the former accounting for the vast majority of federal enrollment. Table I.1 presents the number and share of students, institutions, and degrees, by sector.

Federal public universities in Brazil are, in most cases, elite and highly selective. These institutions play a similar role to flagship state universities in the United States. They are typically the most prominent and important universities in their specific states 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.[5] As a result of their differential quality and tuition-free policy, public institutions are highly over-subscribed. This contrasts with private universities that, on average, fill only 50 percent of their offered spots. The median state in Brazil has just one selective federal university.

[4] As a benchmark, in the United States, 3/4 of students attend public colleges, while only 1/4 attend a private institution.

[5] Appendix Figure I.1 shows the distribution of quality as measured by an index ranging from 1 to 5 prepared by the Ministry of Education to evaluate degrees.

Prospective students in Brazil, as is common in most countries, apply in advance to a particular degree program: a specific major at a specific institution (e.g. Law at the University of Sao Paulo). Obtaining a postsecondary degree normally 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 teaching shift in which to study their degree: morning, evening, night, or full-time.[6]

2.2 College admissions: ENEM and SISU

Several million students take the ENEM exam at the end of the academic year, with the aim of gaining access to higher education. The ENEM exam is an extremely competitive standardized exam, that consists of four multiple choice tests–on Math, Language, History, and Science–and one written essay. ENEM test scores can be used to gain admission to most public and some private institutions, and are the only merit-based criterion used by the Ministry of Education to assign financial aid to students attending private institutions.

Until 2010, college admissions worked in a fully decentralized way and each institution administered its own specific entrance exam. Since then, federal and state institutions can opt to participate in SISU, a centralized digital platform that matches students to degree programs according to their ENEM test scores, and degree preferences. By 2016, over 93% of the 204,633 incoming students in federal institutions entered through this centralized admission system, and 103 out of the 107 federal institutions participated in it. In the same year, about 57% of the 4.8 million ENEM takers applied to a degree program using the SISU platform. Note that students taking the ENEM in a given year can only use their score to participate in the SISU process in the following year. The Brazilian academic year runs from March to December. Students take the test in December, at the end of a given academic year; apply to higher education through the SISU in January; and begin their studies in March of the following academic year. In any given year, about 30% of ENEM takers are students graduating from high school that academic year. The remainder are individuals who have either already graduated, or are taking the ENEM as a means to high school certification.

Students participating in SISU can submit and rank program choices among the set of available programs in the system. A program is defined as a degree, institution, and shift tuple. Students are then matched following an iterative deferred acceptance mechanism based on ENEM scores.[7] As opposed to the standard deferred acceptance process, 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 score representing the lowest score necessary to be accepted into a specific program, and students are allowed to change their degree preferences given the newly available information. The results of the last day are final and determine the acceptances for every degree.[8]

[6]A student can only attend one shift and is not allowed to switch between them.

[7]Degrees may use different weights for each of the ENEM sub-parts.

[8]This is the same as the mechanism currently used by Inner Mongolia, China university admission systems. See Bó and Hakimov (2019) for formal properties of this mechanism.

2.3 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 for students coming from public high schools.[9] Targeted students represent 82 percent of all ENEM test takers. The regulation sought to reverse racial and income inequality in university access. Under the regulation, only students coming from public high schools are eligible to compete for the AA vacancies, while the remaining half of vacancies remain open for broad competition.[10] Reserved seats are then further distributed among students from low-income families, and who are of African or indigenous descent. Throughout the paper we refer to individuals who are eligible for an AA spot as targeted students.[11]

Figure 1 summarizes this distribution by presenting the shares of AA students coming from different demographics. For every 100 university spots, 50 are AA spots for public high-schools 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 360 USD), and 25 go to non-poor students. Finally, a percentage of the spots in both categories is set aside for black, brown and indigenous students, in accordance with the racial makeup of each of Brazil's 27 states.[12] Overall, this results in five different AA categories to which prospective students can apply.

The regulation was implemented in a staggered fashion over four years. Beginning in 2013, affected institutions were mandated to reserve a minimum of 12.5 percent of their vacancies for eligible students, with the minimum share increasing by 12.5 percentage points per year until it reached 50 percent in 2016. Institutions had the freedom to choose whether to adjust to the full regulation (50 percent) immediately or to adjust gradually, as long as they were complying with the mandated minimum share of AA spots in a given year.

Appendix Figure I.2 illustrates the staggered implementation of the policy between 2012 and 2016. In every year since the regulation was introduced, we observe increased bunching of quota admissions above the 50 percent threshold. In addition, in 2013, 2014, and 2015 there is substantial bunching above the minimum thresholds corresponding to those years, until the 50 percent quota is surpassed in 2016. Between 2013 and 2016, virtually all degree programs complied with the minimum quota mandated for the given period. This illustrates that federal institutions complied

[9] Students attending private high schools with full scholarships are also eligible to compete for AA vacancies.

[10] Such systems are also known as reservation policies. The design of these reservations has been studied in several real-life applications. These include school choice in Boston (Dur et al., 2018) and Chicago (Dur et al., 2020), college choice in Brazil (Aygun and Bó, 2021), allocation of H-1B visas in the United States (Pathak et al., 2020) and public sector jobs in India (Sonmez and Yenmez, 2021).

[11] In the United States, selective universities doing AA implicitly subsidize the application score of students who belong to a given minority. In the rest of the world, many countries use quotas and reserve a share of seats in their college admissions. Some examples include India (cast, gender), France (residence), South Africa (race), Malaysia (ethnic), Sri Lanka (residence), Nigeria (residence), Romania (ethnicity), China (ethnicity), New Zealand (ethnicity), and Chile (low-SES)

[12] According to the 2010 Census, the Brazilian population is 47.5 percent white, 43.4 percent brown, 7.5 percent black, 1.1 percent asian and 0.42 percent indigenous (IBGE 2011).

with the regulation.

3 Data and descriptive evidence

3.1 Data sources

We combine several datasets to a characterize the population affected by the affirmative action policy and to determine its impact along several margins and outcomes of interest. We have access to detailed administrative education data including students' performance on the national university entrance exam, application portfolios to public universities, and admission offers from these institutions. We combine these administrative data with the Brazilian Higher Education Census to track students' over time. Finally, we link these datasets to matched employer-employee records comprising virtually the universe of formal employment in Brazil.

Test scores: The first dataset we employ contains test score data for the universe of students taking the university entrance exam, ENEM. We observe these data for the period 2009-2015, which corresponds to the 2010-2016 admission period. The number of exam takers has increased from 2.4 million individuals in 2009 to 4.8 million in 2015. These data include individual-level test scores on each of the components of the test, as well as answers to a detailed survey including questions on socioeconomic background and student perceptions.

Centralized admissions process: We complement test score data with records from the centralized admission system, SISU. These data cover applications to federal and state institutions. We focus our attention only on applications to federal institutions, as state institutions were not mandated to comply with the affirmative action 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 ranking of the degree in the student preference list, the affirmative action group of the applicant, and the student ranking among all applicants from a given degree and affirmative action group tuple. We also observe the students who were offered admission, and the admission cutoff for every degree and affirmative action group tuple. We observe these data for the 2016 admissions period.

Higher education census: The third dataset we use for our analysis is the Brazilian Higher Education Census. This contains information on every student enrolled at any higher education institution in Brazil, allowing us to observe the educational path of every student in any degree program between 2009 and 2019. In a given year, the unit of observation is at the degree-student level, and includes a variable indicating if the individual graduated, dropped out, or is successfully enrolled at the end of the academic year. This data is of very high-quality as most institutions have their own systems integrated with the census in real time. In addition to the student data, this database also includes administrative information at the level of degrees and institutions, allowing us to characterize both over 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. This dataset includes variables at the firm and worker levels, such as payroll, contracted hours, hiring and firing dates, and occupation. We use these data to produce earning profiles for every degree and student type. We observe these data for 2017.

3.2 Brazilian college admissions

One of the primary motivations behind the implementation of the AA regulation in Brazil is that ENEM test scores are strongly correlated with socioeconomic status and high school demographics. As such, selective admissions to public institutions are highly segregated in favor of students from wealthier backgrounds. We provide descriptive evidence for the 2016 admission year to show large differences in test performance by targeted status. In addition, we show that the effect of the difference in performance on access to federal institution is partly mitigated by lower admission thresholds for targeted individuals.

Test participation and performance by targeted status: In 2016, targeted individuals represented nearly 85% of the 4.8 million ENEM takers. Figure 2 shows the distribution of the average ENEM score by targeted status. We observe that the average targeted student scored 494 points, while the average non-targeted student scored 560 points. This 66 points difference represents a 1 SD difference in performance across targeted and non-targeted individuals.

Applications and spots by targeted status: In the first semester of 2016, the centralized admission system offered 200,877 spots across 4,900 degrees in 101 federal institutions. Table 1 presents summary statistics on the number of spots and student applicants to open and reserved spots. Nearly 46% of spots at federal institutions were open for anyone to apply, 48% were reserved for targeted individuals as mandated by the regulation, and the remaining 6% were reserved for institution-specific quotas (e.g. place-based affirmative action). Notably, across all admission pools, selectivity–as reflected in the ratio of spots to applications–was approximately 5%, comparable to many Ivy League colleges in the U.S.

By normalizing test score admission cutoffs in terms of the average admission cutoff for open seats, we observe that admission cutoffs for targeted students range between 0.43 and 0.88 SDs below that of non-targeted students, depending on the specific admission pool. These differences in admission cutoffs somewhat compensate for the difference in ENEM test scores between targeted and non-targeted individuals.

In 2016, when the policy was fully implemented, targeted students in the top quintile of the score distribution were 20 percentage points more likely to attend a public institution than non-targeted students, after conditioning on test scores. This difference represents a striking 300 percent increase relative to the 5 percentage point difference observed in 2012. The coverage of the SISU centralized admission system grew substantively between 2012 and 2016, hence averages in each time period represent different populations of federal institutions. However, the result remains unchanged when using a balanced panel of states, in which the largest federal institution was part of SISU both in 2012 and 2016.

Composition of the student body: Next, we show that the student body composition in federal institutions became more diverse after the implementation of the AA policy, driven by an increase in the representation of targeted students. Figure 3 presents the average share of targeted students in federal institutions by degree selectivity. We observe a large increase in the share of targeted students for degrees above the 50th percentile of selectivity. In Figure I.3 we show that the relationship between the share of targeted students and degree selectivity across state institutions maintained a similar pattern before and after the law was implemented.

3.3 Educational outcomes by targeted status

We explore the educational landscape faced both by targeted and non-targeted students in 2016, the year when the AA regulation was fully implemented. We present a series of educational outcomes conditioning on student ENEM scores. In Figure I.4 we present college attendance outcomes. We find that targeted individuals are substantially more likely to enroll at a federal institution relative to non-targeted individuals with similar test scores. This result is expected as a direct consequence of the AA policy lowering the admission threshold for targeted students. The differences are larger for students in the upper part of the score distribution, where the regulation is more effective. In turn, we observe that non-targeted individuals are more likely to enroll at a private institution.

In Figure I.5, we focus on what happens to those students who enroll in federal institutions. We observe that, conditional on test scores, targeted students are: (i) more likely to enroll in better degrees, (ii) more likely to be enrolled at a better degrees 4 years after, (iii) and less likely to dropout or switch to another degree. In Figure I.6, we show that these patterns are reversed for targeted students enrolling in the private sector. In addition, targeted students attend cheaper degrees than their non-targeted counterparts. It is important to note that targeted students are substantially more likely to receive financial aid, either through discounted prices, student loans, or grants. This last result helps mitigate the differences in access to more selective degrees in the private sector, thus improving the value of the outside option for targeted students.

3.4 Expected earnings

Next, we ask how to aggregate educational outcomes. The ideal outcome would aggregate both the extensive and intensive margins. The extensive margin captures whether the student attended and/or graduated from a higher education institution. The intensive margin, on the other hand, captures individuals' academic trajectories, alongside the quality and field of study of their attended degrees. The ideal variable to combine these two margins is labor market income. Unfortunately, the law was only fully implemented in 2016 so it is too early to observe the labor market outcomes of affected individuals.

Instead, we use predicted income as a currency to aggregate academic progress. For the 2016 sample, we observe college enrollment and detailed academic progress over four years, from 2016 to 2019. For older cohorts, we observe academic progress and income. We use individual-level microdata for cohorts entering college between 2010 and 2012, and their observed labor market income in 2017, to estimate a model linking academic progress to income. We then use these estimates to create predicted income measures for the 2016 sample. We discuss the econometric implementation of this procedure in Appendix C.

4 A conceptual model

We propose a framework that characterizes the effects of AA in selective federal institutions in the context of a centralized college admission system. We consider a system in which students rank their preferred degree programs, and institutions rank applicants using a priority index comprised of standardized test scores. Similar to Dur et al. (2020), our framework embeds an AA regulation within such admission systems by reserving admission spots for the group targeted with preferential treatment. Because the number of spots is fixed, the AA policy operates by pulling in applicants from the targeted group at the cost of pushing out non-targeted students with higher academic scores.

4.1 Environment

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

Students have preferences over degrees based on a strict ordering, $\succ_i$. Degree programs also have preferences over applicants based on students priority scores $s_i = \{s_{i1}, ..., s_{iJ}\}$. Scores are very fine so that no tie-breaker is needed. We allow degree-specific scores which may arise due to degree-specific exams or to degrees assigning different weights for different components of a single entrance exam.[13]

There are different subgroups within the student population, defined by their AA status $t_i \in \mathcal{T}$. For convenience of the presentation, let $\mathcal{T} = \{0, 1\}$, such that $t_i = 0$ represent the non-targeted students and $t_i = 1$ 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

[13]For example, the ENEM exam, have different examinations for math, language, social science, natural science and writing. While STEM degrees tend to place higher weight on math and natural science, more humanities oriented degrees give more importance to language and social science.

preferences, priority scores across all degrees, and AA status.[14] We denote the set of all student types by $\Theta = \bigcup_{i \in \mathcal{I}} \theta_i$.

Spots at degrees are constrained by a strictly positive capacity vector, $q = \{q_0, ..., q_J\}$. An AA regulation is in place such that for every degree, a share $\omega \in [0, 1]$ of the 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 federal 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, targeted students with the highest priority score receive them. When open seats are processed, members of any group (targeted or otherwise) are admitted in order of their priority score. When a student qualifies for both a reserved and an open spot, the set of admission rules must specify the relative precedence of different admissions channels. In our empirical application, we assume that spots are horizontally reserved. That is, reserved spots are allocated first to targeted students based on priority scores, and next all open spots are allocated to the remaining individuals with the highest priority scores.[15]

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 a deferred acceptance algorithm, assignments μ between students and degrees are unique and stable (Gale and Shapley, 1962; Abdulkadiroglu, 2005).[16] The stability property of the mechanism implies that each student enrolls in their most preferred program for which they are eligible.

The matching μ has a unique representation in terms of a vector of market clearing admission cutoffs $c_{tj}(\mu)$ for each degree program and AA status combination (Azevedo and Leshno, 2016).[17] A cutoff $c_{tj}(\mu)$ is a minimal score s_{ij} required for admission at degree j for students with AA status t. Since targeted students can be admitted through the reserved or the open spots, admission cutoffs for targeted students are always lower than for non-targeted individuals, i.e. $c_{0j} \geq c_{1j}$ for all $j \in \mathcal{J}$. Because the outside option is always feasible, it has an admission cutoff score of $-\infty$.

[14] The definition of a student type as a combination of preferences, priorities, and status is similar to that of Abdulkadiroglu et al. (2017).

[15] 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 only binds when there are not enough targeted students who receive a spot on the basis of their test-score alone. A vertical reservation works under a "over-and-above" basis. This means that targeted individuals receiving a spot solely on the basis of their priority score does not count towards a vertically reserved position (Sonmez and Yenmez, 2021). Our framework encompasses both types of AA.

[16] A matching μ is stable if there is no student-degree pair (i, j) where: (i) student i prefers degree j to their assignment, and (ii) student i has higher priority than some other student who is assigned to j

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

As in any strict-priority mechanism, the availability of options will be different for students according to their priority score. Let $\Omega_i(\mu) = \{j \in \mathcal{J} \,|\, s_{ij} \geq c_{t_i j}(\mu)\}$ 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 $D_i(\mu) = \{j \in \Omega_i | j \succeq k \text{ for all } k \in \Omega_i(\mu)\}$ denote the preferred option in the feasible choice set defined by $\Omega_i(\mu)$. In other words, $D_i(\mu)$ represents the highest option ranked by individual i among the degrees to which they 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.

4.2 Counterfactual admission cutoffs, choice sets, and preferred degrees

One of the advantages of studying AA in the context of a centralized system is that it follows systematic and transparent admission rules. The idea is that by leveraging the assignment rules from the mechanism, we can characterize student allocations in a regime with a different AA schedule ω'. For ease of exposition, we think of ω' as an increase in the shares reserved to AA students.

The new schedule, together with the students types and degree capacities, result in a counterfactual matching function $\varphi(\Theta, q, \omega') = \mu'$. In turn, this allocation can be represented by a new vector of of admission thresholds $c_{tj}(\mu')$.[18] In this scenario, the feasible choice sets, preferred feasible degree, and ultimately the realized outcomes, would also change as a result of the changing admission cutoffs. These objects are represented by $\Omega_i(\mu')$, $D_i(\mu')$ and $Y_i(\mu')$, respectively. Combining these elements we can characterize the set of individuals who were effectively benefited and displaced by the policy, together with their respective gains and losses.

4.3 Gains and losses of AA

Our goal is to define how much a student would gain or lose if they were pulled into or pushed out of their preferred feasible degree as a result of an increase in the AA policy. The conditional average treatment effect of increasing ω to ω' for individuals of type θ is:

$$\tau(\theta) = \mathbb{E}[Y_i(\mu') - Y_i(\mu) |\, \theta_i = \theta]. \tag{1}$$

The aggregate effect of increasing AA are given by integrating over all student types. To aggregate these treatment effects, we assume an equal weight for all individuals within similar AA

[18]In the extreme case of $\omega = 0$, applications from targeted and non-targeted students are processed together and spots are allocated solely based on priority scores, and thus every student would face the same admission threshold irrespective of their AA status.

groups. Thus, the aggregate effects for each AA group t are given by:

$$\Delta_t(\omega', \omega) \quad = \quad \sum_{i \in \mathcal{I}} \tau(\theta_i) \cdot \mathbb{1}\{t_i = t\}. \tag{2}$$

We can use the status-quo and counterfactual admission thresholds, $c_{tj}(\mu)$, and $c_{tj}(\mu')$, to identify the individuals who gain or lose access to any given degree as a result of the policy. For the case of targeted students, individuals with $D_i(\mu') = j$ and priority score $s_{ij} \in [c_{1j}(\mu'), c_{1j}(\mu))$ will benefit and gain admission into degree j when the AA policy is intensified. Conversely, non-targeted individuals with priority scores $s_{ij} \in [c_{0j}(\mu), c_{0j}(\mu'))$, will be displaced from degree j by the regulation.

4.4 A simple example

Under the lense of this framework, the aggregate consequences of the 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. For instance, the policy maker could be interested in how much earnings benefited individuals gain, and how much earnings displaced individuals lose as a result of the policy. These gains and losses are characterized by how admission scores, degree attendance, and the outside options affect the outcome of interest.

In Figure 4, we describe one case in which the earnings gains for targeted individuals outweigh the losses experienced by displaced individuals. 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 ($j = 0$).

The solid lines show the average potential earnings for students attending 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 earnings 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 admission cutoff c', for all students. By focusing on students scoring around c', we can easily observe that implementing AA is efficient for the marginal student. The expected earnings of attending the selective institution are higher for targeted than for non-targeted students. In addition, non-targeted students have a better outside option than targeted individuals. As a result, the returns of attending the selective institution are larger for targeted than for non-targeted students.

Now assume the planner implements an AA policy such that targeted and non-targeted students face different cutoffs, represented by $c_{t=1}$ and $c_{t=0}$ respectively. Targeted students with scores $s_i \in [c_{t=1}, c']$ are now offered admission, while non-targeted students with scores $s_i \in [c', c_{t=0}]$ are displaced from the selective institution. Area A depicts the aggregate gain for benefited individuals, while the aggregate cost incurred by displaced individuals is represented by area B. In the figure, we observe that the consequences of AA depend crucially on heterogenous returns for benefited and displaced individuals, and also on how this difference varies with the admission cutoff.

5 Exogenous variation in access to federal degrees

In this section, we describe two sources of exogenous variation that we use to estimate the returns of gaining admission into a federal degree. First, we use admission discontinuities to estimate the effects of AA for both targeted and non-targeted groups. This research design allows us to causally estimate the effects of marginally increasing the number of reserved seats in a given degree. Second, we introduce an exogenous score shifter that arises from the random assignment of students to graders and examination booklets of varying strictness and difficulty. In the next section of the paper, we use the admission discontinuities together with the score shifter to estimate to understand how individuals with different test scores could be differentially benefited or harmed by larger changes in the scope of the AA policy.

5.1 Regression discontinuity evidence

In our setting, many students apply to federal degree programs while simultaneously deciding whether or not to attend the private sector, or to not study at all. Thus, some admitted students decline their admissions offer, which opens slots for additional offers farther down the admissions waiting list.[19] We compare individuals in similar waiting lists who were just above the admission threshold and offered admission, to those who were just below the threshold and thus not admitted.

We exploit the fact that reserved and open seats have different admission cutoffs, to construct credible instruments from discontinuities for each of these groups. As described in Section 2, there are four different admission tracks for reserved seats, and one for open seats. Thus, for every degree program we observe five different waitlists. We pool all admission cutoffs and normalize the data so that zero on the x-axis represents the position of the last individual admitted in a given waitlist.[20] Because the last admitted student in the waitlist always accepts the offer, applicants getting and not getting an offer are not statistically comparable. We follow de Chaisemartin and Behaghel (2020), and drop the last admitted student in every waitlist to restore comparability across groups. For presentational purposes, we also pool all individuals in reserved seats, and present their estimates as a single group.

[19] Individuals in our setting have to report two options to the system. They may be (i) either accepted at their first choice, or (ii) accepted at their second choice and waitlisted at their first choice. Initially admitted students are offered an admission through the centralized platform. After an acceptance deadline, the Ministry of Education forwards the list of waitlisted students to the participating institutions, with the objective of filling the remaining vacancies. Thus, some students are offered spots immediately, whereas others may be offered seats closer to the beginning of the academic year.

[20] One natural alternative was to pool the data using the score difference between applicant test scores and the admission cutoff (Kirkeboen et al., 2016). Instead, we use the relative position in the waitlist. We do this because degrees have very different levels of test scores density. For example, while Medicine has hundreds of applicants within a 20 points bandwidth around the admission threshold, other degrees have only a handful. Using waitlists as the index variable determining admissions allows us to have the same number of students across degrees, and below and above the admission threshold.

We provide graphical and statistical evidence. To produce the figures, we first construct residualized outcomes, removing waitlist fixed effects. For readability, we rescale the residualized outcomes by the outcome means.[21] Seat types are indexed by $t \in \{0,1\}$, were 0 denote open seats, and 1 represent reserved seats. In Figure 5, we plot residualized means in bins for individuals in each type of seat.[22] The standard errors in a given cell represent the standard error of the outcome mean. We also include estimated linear regression lines on each side of the cutoff. To make groups comparable, we include observations within a small interval above and below the last admit.[23]

Now we focus our attention on describing the main results in Figure 5. We begin by estimating the first stage relationship between admission offers and enrollment in student's preferred degree during the first academic year. Panel (a) shows that the first stage coefficient for open seats is 32 percent. As a result of lower a take-up and lower reapplication rates from targeted students, the first stage coefficient for reserved seats is 6 p.p. lower. This difference highlights how frictions in the admission process can differentially affect students from different backgrounds.[24] Panel (b) presents enrollment rates in the preferred degree four years after gaining admission. Interestingly, even though dropout rates are high, at 43 percent, we find no differences in drop out rates by AA status. In Panel (c), we observe enrollment at any federal degree four years after admissions. This figure reveals that non-targeted students are substantially more likely to re-apply to a degree in the federal system.

Next, we compute the threshold crossing effects on the quality of the degree attended. We compute this measure 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 are able to compare the quality of the degrees attended inside and outside of the federal system.[25] In Panel (d), we look at what happens during the first year. From this figure we learn three insights. First, students in open seats attend degrees of substantially higher quality. This is a result of both having access to better degrees outside the system, and a higher take-up rate of admission at federal degrees. Second, gaining access into a federal degree substantially increase the quality of the degree attended. Third, even though the RDD estimate is equivalent across groups, the implied IV estimate indicates that the impact of assigning a seat in a federal degree on the quality of the degree attended is 20% larger for targeted students.

In Panel (e), we focus on the quality of the degree students attend four years after enrollment.

[21] Formally, we regress the outcome on a constant, α, and a waitlist-specific fixed effect λ_{jq}, where j denotes the preferred degree j and q indicates the admission track. The residualized outcome is given by $\tilde{Y}_i = Y_i - (\alpha + \lambda_{jq}) + \bar{Y}$.

[22] As a result of pooling all targeted groups, reserved seats bins have 4 individuals per degree, while open seats bins only 1 individual per degree.

[23] Note that since our running variable is discrete in nature, we do not follow the existing literature in calculating the optimal bandwidth, as those methods are developed for assignment variables with density $g(x)$, where $g(.)$ is continuous and bounded away from zero (Calonico et al., 2014).

[24] Non-targeted students on the left hand side of the admission discontinuity have higher admission rates than targeted students as a result of a larger participation in the application process during the second semester of the academic year.

[25] We assume that everyone not attending college is implicitly attending the same option. The degree quality measure for these students is given by the average test score of everyone who decided not to study.

This outcome captures the net effect of gaining admission to a higher quality federal degree and individuals' academic paths after admission (both inside and outside the federal system) and drop out. The "mismatch hypothesis" implies that targeted students would experience worse outcomes as a result of being assigned to a higher quality degree. In contrast with these predictions, we find that AA improves the quality of the degree attended by targeted students four years later. Moreover, the IV estimate implies that gaining admission to a federal degree increases degree quality by 64 percent more for targeted students than for non-targeted students.

[Paragrah on Panel (f) + main conclusion from the section]

In Figure 6, we assess the validity of our research design by investigating covariate balance around the admission thresholds. We consider several individual characteristics: application score, race, gender, age, and household income, and a dummy indicating if the student is a recent high school graduate. Overall, we find no indication that applicants on different sides of the application boundaries are different in term of observables. In Appendix Table H.1 we report results from several specification checks, all of which support our main findings.

5.2 Test scores and score shifters

The ENEM exam consists of a written essay and four multiple choice tests –on Math, Language, History, and Science.[26] We leverage features of the test implementation to construct exogenous score shifters for each of these components. These shifters mimic random shocks that impact test scores but that are uncorrelated with students' latent ability. The score shifter exploits two exogenous sources of variation. We discuss them in turn.

The first source stems from random assignment of the written component of the national exam to graders of varying strictness. Each essay is marked by two randomly assigned graders. Each year, over 10,000 individuals are involved in the grading process, each of whom is assigned to an average of 1,100 exams.[27] Graders receive the exams via an online platform which ensures that graders are blind to students' identity and characteristics. The only restriction imposed in the assignment process is that graders must be from different states to their assigned students. The essay is marked based on five different competencies, each of them with a score ranging from 0 to 200. The final grade is the simple average of the total points given by each of the graders. If the score difference across graders is large for any exam, that exam is graded by a third individual, and the final grade is then the average of the two closest scores.[28]

We construct a leave-one-out measure of grader leniency for every student using the first two

[26] The essay usually has a 20% weight for most degrees. This holds true across different fields of study.

[27] In Figure I.7 we show the distribution of the number of exams assigned to each grader

[28] A third grader intervenes in two situations: (i) If the difference between scores across graders is larger than 100 points, (ii) if the difference in one or more of the competencies is larger than 80 points. The grading rubric is available from the following link:
http://download.inep.gov.br/educacao_basica/enem/guia_participante/2018/manual_de_redacao_do_enem_2018.pdf

randomly assigned graders. Specifically, we define the essay score shifter z_i^e as:

$$z_i^e \quad = \quad \frac{1}{2}\left(\underbrace{\frac{1}{|N_{g_{i1}}-1|}\sum_{m\in\left(N_{g_{i1}}\setminus\{i\}\right)} s_m^e}_{\text{strictness of grader 1}} + \underbrace{\frac{1}{|N_{g_{i2}}-1|}\sum_{m\in\left(N_{g_{i2}}\setminus\{i\}\right)} s_m^e}_{\text{strictness of grader 2}}\right),$$

where g_{ik} denotes kth grader assigned to individual i, s_m^e represents the essay score of student m, and N_g denotes the set of exams received by grader g. In words, for a given student i, the leave-one-out score shifter is the average score their graders gave to all other students they graded.

Panel (a) in Figure 7 presents the distribution of the score shifter for the SISU sample. It shows a wide dispersion with a range of 165 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 576 points, the mean essay score. The dashed red line shows a local linear regression of the first-stage relationship between our 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 score in the multiple choice components of the test. Consistent with a satisfactory randomized assignment, this figure shows no correlation between the score shifter z_i^e and other measures of student ability, as captured by the average score in the multiple choice components of the test. In Appendix H we show the first stage coefficients and additional balance tests.

The second source of variation arises from plausibly random assignment of students to examination booklets in the multiple choice component of the test. To prevent cheating, students are assigned to one of the four different examination booklets, with different color covers, that only vary 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. We argue that within examination locations, booklet assignment is plausibly random.[29]

We observe substantive differences in scores across examination booklets, which we present in Panel (b) of Figure 7. For instance, the average score in the Math component of the test is 6 points larger for the red booklet and for the yellow booklet. In Appendix H, we show that the assignment of booklets are uncorrelated with students characteristics. In Barahona et al. (2021), we show that some booklets exhibit greater difficulty because they have the easier questions later in the test. For each component k in {Math, Language, History, Science} in the multiple choice part of the test, we construct the following leave-one-out measure of booklet difficulty:

$$z_i^k \quad = \quad \frac{1}{|N_{b_{ik}}-1|}\sum_{m\in\left(N_{b_{ik}}\setminus\{i\}\right)} s_m^k,$$

[29] Examination locations receive a tag for each participant, with their name, ID number, examination room and desk number. Students can only take the exam if the tag matches the ID attached to the student's desk

where b_{ik} denotes the booklet assigned to student i, s_m^k is the score that student m gets in component k, and N_{b_k} is the set of students assigned to booklet b in such component k. For a given student i, the leave-one-out score shifter z_i^k captures 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.[30] We use these to construct corrected test scores that are relevant for the centralized system relevant 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.

In Figure 8, we present the reduced-form relationship between our instrument and educational outcomes. Panel (a) shows that a student that got a score shifter in the top 5% is 1.55 percentage points (or 12%) more likely to attend a federal institution the year after than one who got a grader in the bottom 5%. In Panel (b) we show that the score shifter also meaningfully impacts the predicted income of students based on their degree attendance 4 years after taking the test.

6 An 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 can rewrite Equation (1) as:

$$\begin{aligned} \tau(\theta) &= \mathbb{E}[Y_i(\mu') - Y_i(\mu) \,|\, \theta_i = \theta] \\ &= \mathbb{E}\left[\sum_j \mathbb{1}\{\mu'(\theta_i) = j\} \cdot Y_{ij} - \sum_j \mathbb{1}\{\mu(\theta_i) = j\} \cdot Y_{ij} \;\middle|\; \theta_i = \theta\right] \end{aligned} \tag{3}$$

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 $\tau(\theta)$ requires estimating the matching functions for each of the AA schedules, together with the potential outcomes. The estimation of each of these objects presents its specific challenges, which we address in turn. First, to estimate the matching function, we

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

leverage the rules of the mechanism φ, together with its inputs (Θ, q, ω). The main difficulty emerges from the fact that, in practice, researchers never observe the full set of student types Θ, as individuals rarely exhaustively rank the full list of preferences $\succ_i$. As such, in Section 6.1, we introduce an empirical school choice model to estimate student preferences for degrees.

Second, we ought to estimate the potential outcomes for every student and degree combination Y_{ij}. One possible direction is to impose a linear projection of Y_{ij} on student and degree characteristics and estimate the conditional expectation of Y_{ij} using OLS. However, identifying these parameters is difficult because of the standard problem of selection into degrees. To overcome this problem, in Section 6.2, we follow Abdulkadiroglu et al. (2020) and construct selection-corrected estimates of the parameters using a control function approach. This approach jointly models the choice of degrees (selection equation) together with the potential outcomes (outcome equation). The identification strategy leverages the fact that the score shifter z_i impacts the availability of degrees in student choice sets while being excluded from their potential outcomes.

6.1 School choice model

To recover student preferences, we summarize the observed choices in the centralized platform by fitting a random utility model. Specifically, student i's indirect utility for attending degree j is:

$$\begin{aligned} u_{ij} &= V_{ij} + \eta_{ij} \\ &= \delta_j^t + \gamma_j^t \cdot a_{ij} + \kappa^t \cdot d_{ij} + \eta_{ij} \end{aligned} \tag{4}$$

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. We parametrize V_{ij} as a function of degree fixed effects δ_j^t, the student's ability a_{ij}, and d_{ij} indicating whether the student lives in the same commuting zone where the degree is offered.[31]. We allow flexible heterogeneity in tastes by estimating preference models separately for each AA type t. We model the unobserved taste η_{ij} as following an extreme value type I distribution, conditional on vectors s_i and $d_i = (d_{i1}, ..., d_{iJ})$. The outside option aggregates all degrees offered by municipal, state, and private universities, or not enrolling at all. This option is available to everyone and has a deterministic utility V_{i0} normalized to zero.

Implicit in Equation (4) is an assumption that V_{ij} is independent of the matching $\tilde{\mu}$. As such, preference parameters $(\delta_j^t, \gamma_j^t, \kappa^t)$ 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 existing empirical approaches abstract away from equilibrium sorting based on preferences for peers (Agarwal and Somaini, 2020).[32]

[31] We use micro regions geographic division as commuting zones to capture whether individuals need to move to a new residency as a result of geographical distance. There are a total of 558 micro regions in Brazil, and they are defined by IBGE as bordering cities with "common social characteristics, geography and spatial articulation."

[32] Allende (2021) and Cox et al. (2021) are notable exceptions.

Let $\tilde{\mu}$ be a realized matching that we observe in the data. The mechanism's stability property implies a discrete choice model with observable and personalized choice sets $\Omega_i = \Omega_i(\tilde{\mu})$ (Fack et al., 2019). Also, it implies that the degree to which the student is assigned is also their preferred feasible option ex-post, that is $D_i = D_i(\tilde{\mu}) = \tilde{\mu}(\theta_i) = \arg\max_{j \in \Omega_i} u_{ij}$. One of the main advantages of final stable allocations is that researchers can exploit revealed preference relations based only on assignment data instead on to relying on the information reported on rank-ordered lists.[33] As such, this approach can be implemented in any centralized system using strict priorities with stable assignments. The stability property, as an ex-post optimality condition is not necessarily guaranteed when students do not have complete information about the admission cutoffs. In Appendix B, we show that in the Brazilian setting students have little uncertainty over the final admission cutoffs due to the iterative property of the mechanism.

Following Fack et al. (2019), we restrict the choice set Ω_i to all degrees available to student i based on their score and on the admission cutoff specific to their degree and AA type. The logit model implies that the probability that individual i selects degree j is given by:

$$Pr(D_i = j) = \frac{\exp V_{ij}}{\sum_{k \in \Omega_i} \exp V_{ik}}.$$

We estimate $(\delta_j^t, \gamma_j^t, \kappa^t)$ for $t \in \{0, 1\}$ using a maximum likelihood estimator.

6.2 Potential outcome equation

That is, the decision to attend a given degree is likely correlated with unobservables of the potential outcome, thus biasing the parameter estimates. For example, students who choose to attend the most selective degrees are commonly also the best prepared to perform well in these, be it due to relevant prior training, a comfort with the environment, useful socio-emotional traits, or other unobservables.

Next, we switch our focus to constructing selection-corrected estimates of a potential outcome model using a control function approach. We follow a similar approach to that of Abdulkadiroglu et al. (2020), who link school choices to potential outcomes to estimate schools' value-added in New York City. We use our estimated parameters to predict the potential outcomes for attending a given counterfactual preferred degree.

We project the potential outcome of student i at counterfactual degree j on degree fixed effects

[33] An alternative route to estimating preferences would be to rely on the ranked ordered lists submitted to the system. The DA mechanism is strategy-proof which implies that it is in the best interest of students to rank schools truthfully (Abdulkadiroglu and Sonmez, 2003). In consequence, a large body of researcher has used submitted ranked order lists to infer preferences over degree programs. We take a different approach for two reasons. First, in the SISU system students can only rank up to two options which destroys the strategy proofness of the mechanism (Haeringer and Klijn, 2009; Calsamiglia et al., 2010). Second, as pointed out by Fack et al. (2019), in strict priority mechanisms where individuals are ranked by test scores, students face limited uncertainty about their admission outcomes and may "skip the impossible" and choose not to apply to degree programs that are out of their reach.

and student and degree characteristics:

$$Y_{ij} = \alpha_j^t + X_{ij}'\beta_j^t + \varepsilon_{ij} \tag{5}$$

where α_j^t and β_j^t are population parameters for AA group t, implying $\mathbb{E}[\varepsilon_{ij}] = \mathbb{E}[X_{ij}\varepsilon_{ij}] = 0$. The variable X_{ij} is a vector of observed covariates, including student corrected test scores $\tilde{s}_{ij}$, and d_{ij} which indicates whether student's i lives in the same commuting zone where the degree is offered. Our goal is to recover the parameters of the potential outcome equations defined above.

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

$$\mathbb{E}[Y_{ij}|X_{ij}, D_i = j] \quad = \quad \alpha_j^t + X_{ij}'\beta_j^t + \mathbb{E}[\varepsilon_{ij}|X_{ij}, D_i = j] \tag{6}$$

The OLS estimation of this equation would likely yield biased parameters due to selection into degrees based on unobservable preferences. To recover unbiased estimates we would need to assume that $\mathbb{E}[\varepsilon_{ij}|X_{ij}, D_i = j] = 0$, 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 (5) on the vector of unobserved tastes $\eta_i = (\eta_{i0}, \eta_{i1}, ..., \eta_{iJ})$:

$$\begin{aligned} \mathbb{E}[Y_{ij}|X_{ij}, \eta_i] &= \alpha_j^t + X_{ij}'\beta_j^t + \mathbb{E}[\varepsilon_{ij}|X_{ij}, \eta_i] && (7) \\ &= \alpha_j^t + X_{ij}'\beta_j^t + g^t(\eta_i) && (8) \end{aligned}$$

From Equation (7) to (8) 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).

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. To estimate Equation (8) we use 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:

$$\mathbb{E}[Y_{ij}|X_{ij}, \eta_i] = \alpha_j^t + X_{ij}'\beta_j^t + \sum_{k=0}^{J} \psi_k^t \cdot (\eta_{ik} - \bar{\eta}) + \rho^t \cdot (\eta_{ij} - \bar{\eta}) \tag{9}$$

where $\bar{\eta} \equiv \mathbb{E}[\eta_{ij}]$ is Euler's constant. As pointed out by Abdulkadiroglu et al. (2020), this parametric relationship allows for a wide range of selection on unobservables in the context of school choice. The parameter ψ_k 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 be also high in motivation and thus do well in any other degree). We refer to this term as selection on levels. The coefficient ρ 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 for a given assignment $\tilde{\mu}$ is:

$$\mathbb{E}[Y_i|X_{ij}, z_i, \Omega_i, D_i = j] \quad = \quad \alpha_j^t + X_{ij}'\beta_j^t + \sum_{k=0}^{J} \psi_k^t \cdot \lambda_{ik}(\Omega_i(z_i)) + \rho^t \cdot \lambda_{ij}(\Omega_i(z_i)) \tag{10}$$

where $\lambda_{ik}(\Omega_i(z_i)) \equiv \mathbb{E}[\eta_{ik} - \bar{\eta}|X_{ij}, z_i, \Omega_i, D_i]$ is the expectation of the unobserved preference conditional on the student's characteristics, the feasible choice set given an score shifter $\Omega(z_i)$, and degree of choice. These functions have a closed-formed solution and can be computed using the logit functional form. These objects serve as control functions to correct for selection on unobservables.

Identification of the selection parameters ψ_k^t and ρ^t come from the variation in available choice sets for students who enroll at the same degree program. To provide intuition, suppose there are two available degree choices, A and B, and two individuals, 1 and 2. Individuals are identical in observables but have access to different choice sets as a result of a different score shifter z_i. Assume that individual 1 was assigned to a strict grader, while individual 2 was assigned to a lenient one. Let's further assume that as a result of the grader assignment, individual 1 can only choose option A, while individual 2 can pick between options A and B. If both individuals choose degree A, we can use a revealed preferences argument to learn that individual 2 has an unobserved taste for option A that is higher in expectation than that revealed by individual 1. The selection parameters capture whether this expected difference in unobserved preferences is relevant for the potential outcome. In Appendix F we provide a formal identification proof.

The identification of Equation (10) 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 X_{ij}; that is $(\varepsilon_i, \eta_i|X_{ij}) \perp z_i, \Omega(z_i)$. While independence of z_i is satisfied because of the random assignment of graders to students, the independence of $\Omega_i(z_i)$ is implied by the model, as the personalized choice sets are a function of student observable characteristics and the score shifter $\Omega_i = f_t(X_{ij}, z_i)$. Implicit in this assumption is the fact that students take admission cutoffs as given and cannot manipulate them with their own specific application behavior. Finally, we need an exclusion restriction that ensures that the score shifter only affects outcomes through changing the availability of choice sets. This final assumption is implied by the independence assumption, together with the projection of potential outcomes in Equation (5).

The estimation of the outcome model proceeds in two steps. First, we compute $\hat{\lambda}_{ik}(\cdot)$ using the estimated preference parameters and the logit functional form. Next, we plug $\hat{\lambda}_{ik}(\cdot)$ into Equation (10), and estimate parameters $(\alpha_j^t, \beta_j^t, \psi_j^t, \rho^t)$ using separate OLS regressions for each AA group t.

Labor market earnings are the most natural outcome of interest to assess the effects of the AA policy. Unfortunately, our admissions data starts in 2016; given that degree programs take between 4-6 years to complete, it is too soon to observe labor market outcomes for the individuals in our sample. We address this challenge by analyzing the effects of the AA policy on expected earnings, which is a function of intermediate academic outcomes. In Appendix J, we show that interpretation of the outcome equation coefficients when using expected earnings are equivalent to those resulting from realized earnings under a surrogacy assumption (Prentice, 1989; Athey et al., 2019). In other words, this assumption states that realized earnings are independent of unobserved tastes for degrees conditional on the intermediate outcomes incorporated in the expected earnings.

7 Parameter estimates

In this section, we present the parameter estimates. We first discuss the preference parameters that arise from the estimation of the school choice model. To estimate the model, we focus on the round of applications to higher education institutions, which involved taking the ENEM in 2015 and using the SISU platform in 2016. We restrict the sample to every applicant submitting an application to any institution in the state of Minas Gerais. We do this exclusively for computational power constraints. In total, we have 11 institutions offering 502 degrees (including the outside option). Our sample size consists of 329,621 students, of which 56% are targeted and 44% are non-targeted. We then discuss the estimates from the potential outcome model.

7.1 School choice model

In Panel A of Table 2 we present the parameters estimates from the school choice model. We find positive coefficients for the distance parameter, indicating that students have strong preferences for studying close to where they reside. The distance coefficient is 2.49 for targeted individuals, and slightly smaller for non-targeted students, with a magnitude of 2.17.

Parameters related to degree fixed effects (δ_j) or those associated with student test scores (γ_j) are very high-dimensional, as we have one different estimated coefficient per degree and affirmative action status tuple. To visualize these parameters, we estimate the average valuation for a given degree $\bar{V}_j^t$. To make estimates comparable across targeted and non-targeted groups, we fix the test score variable to equal the admission cutoff score for open spots for that degree, c_{0j}, and also fix the distance variable equal to 0 (i.e., the student lives in the same municipality where the degree is offered). Specifically,

$$\bar{V}_j^t \quad = \quad \hat{\delta}_j^t + \hat{\gamma}_j^t c_{0j}$$

The value of $\bar{V}_j^t$ indicates the valuation of a given degree relative to the outside option. The larger $\bar{V}_j^t$, the more likely it is that the student would choose degree j over the outside option if only these two degrees were offered to them.

In Figure **??**, we show how these valuations vary by degree selectivity and affirmative action status. First, we note that, relative to the outside option, degrees have a low valuation. This is not surprising as the outside option pools a collection of different alternatives (e.g., working, attending a private institution, waiting another year to go to college) and its value is the maximum value of all these alternatives. Second, we find that targeted students put a higher value on the outside option relative to attending a federal degree when compared to their non-targeted counterpart. If targeted students are in greater need of working, for example, they will be more likely to choose the outside option and to enter the labor market, rather than enrolling in any given degree. Third, there is a very strong correlation between degree selectivity and average degree valuation. Fourth, there are a handful of degrees at the top of the selectivity distribution that are remarkably more valued than other degrees. These degrees mostly consist of medicine and engineering-related programs.

We use these parameters to assess the in-sample model fit by comparing the admission thresholds of degrees predicted by the model to those observed in the data. We discuss the construction of the predicted admission thresholds in Appendix D. In Figure D.1 we show the scatterplot of simulated and observed admission thresholds. The correlation coefficient between simulated and observed data is virtually 1 for the admission thresholds of both open and reserved seats.

7.2 Potential outcome equation

The potential outcome equation establishes a structural relationship between predicted income, degree attendance, and student characteristics. We define the value added as the gains of attending a given degree relative to the outside option. To calculate the value added, we use the population parameters α_j and β_j, and fix the test score variable to equal the admission cutoff score for open spots c_{0j}, and also fix the location dummy equal to 0.

$$\mathrm{VA}_j = (\hat{\alpha}^t_j - \hat{\alpha}^t_0) + (\hat{\beta}^t_j - \hat{\beta}^t_0) \cdot c_{0j}$$

In Figure **??**, we plot the value-added of each degree against its selectivity level. We find that more selective degrees offer higher value added. In contrast to the degree valuations from Figure **??**, we find than the average degree offers higher value than the outside option (i.e., positive value added). Moreover, targeted students obtain higher value added than their non-targeted counterpart. This is likely driven by non-targeted students having outside options with high returns, such as attending similar programs in private universities.

8 Counterfactual estimation

In this section, we use the parameter estimates from our model to compute student allocations, as well as their respective potential outcomes under different affirmative action schedules. We then compare the overall predicted income gains and losses for targeted and non-targeted individuals, and for the system as a whole.

8.1 Estimating counterfactual allocations

The goal is to simulate student allocations for any given affirmative action schedule ω by leveraging the rules of the mechanism. We start by recovering the inputs of the matching function $\varphi(\Theta, q, \omega) = \mu$, namely student types, Θ, and degree capacities, q.

The first step is to compute the set of student types $\Theta = \bigcup_{i \in \mathcal{I}} \theta_i$, where $\theta_i = (\succ_i, s_i, t_i)$. We recover preferences, $\hat{\succ}_i$, 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. This allows us to recover inputs directly from the data. Specifically, we assume that the composition of applicants, $\mathcal{I}$, priority scores, s_i, and affirmative action status, t_i, are fixed and invariant to changes in the affirmative action schedule.[34] The main concern related to ruling out behavioral responses is whether the policy effects are well predicted by ignoring them. In Appendix A, we introduce an empirical design to test whether these assumptions are supported by the data.

The second step is to recover degree capacities, q. This variable, as opposed to the inputs of Θ, is a policy choice that is decided together with the affirmative action schedule. Since we are interested in learning about the affirmative action consequences we keep this variable fixed and recover it from the observed data. After recovering, $\hat{\Theta}$ and q, we simulate the matching function as $\varphi(\hat{\Theta}, q, \omega) = \hat{\mu}$.

8.2 Estimating counterfactual outcomes

The next step after estimating the matching function, is to compute the realized outcomes associated with such assignment. We define our object of interest for individual of type θ_i as $\mathbb{E}[Y_i(\hat{\mu}(\theta_i)) \mid \theta_i]$, where $\hat{\mu}$ denotes the matching function estimated above. Using Equations (3), (**??**), and (9) we parametrize this object as:

$$\begin{aligned}
\mathbb{E}[Y_i(\hat{\mu}(\theta_i)) \mid \theta_i] &= \sum_j \mathbb{1}\{\hat{\mu}(\theta_i) = j\} \cdot \mathbb{E}[Y_{ij} \mid \theta_i] \\
&= \sum_j \mathbb{1}\{\hat{\mu}(\theta_i) = j\} \cdot \left(\alpha_j^t + X_{ij}'\beta_j^t + \mathbb{E}[\varepsilon_{ij} \mid \theta_i]\right) \\
&= \sum_j \mathbb{1}\{\hat{\mu}(\theta_i) = j\} \cdot \left(\alpha_j^t + X_{ij}'\beta_j^t + \sum_{k=0}^{J} \psi_k^t(\eta_{ik} - \bar{\eta}) + \rho^t(\eta_{ij} - \bar{\eta})\right) \qquad (11)
\end{aligned}$$

This parametrization assumes that potential outcomes are invariant to the affirmative action regulation. There are two implications that we deem important to discuss. The first one is that this assumption rules out, for instance, peer effects in the production function of degrees, as well

[34] The assumption of no behavioral responses on priority scores is consistent with finding by Francis and Tannuri-Pianto (2012) and (Estevan et al., 2018) in 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 effort.

as changes in the value of Y_{ij} coming from stigmatization or reduction of the signaling value of degrees as a result of the AA regulation. Second, it implies 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 could potentially create a crowding-out cascade extending throughout the whole system. However, in Appendix E we show that most private institutions, including those comparable to federal universities, are far from being capacity constrained. Thus the crowding-out concerns are of second order in our specific setting.

We use Equation (11) together with the selection, and selection-corrected potential outcome parameter estimates, to simulate student allocations for a given schedule ω. In Appendix G we describe the simulation procedure in detail.

8.3 Counterfactual admission thresholds

Next, we perform counterfactual exercises where we vary the share of reserved seats ω from 0 to 100%. For each affirmative action schedule ω, we compute students' allocations and estimate their expected potential outcome in terms of predicted income as described in Section 3.4. We proceed by computing the admission thresholds associated with counterfactual allocations.

For expositional purposes, in Figure 10 we show the distribution of admission cutoff scores across degrees for reserved and open spots under two different counterfactual scenarios. The first one is a counterfactual similar to the current policy that reserves 50% of the seats to targeted students ($\omega = 0.5$). The second one corresponds to a laissez-faire situation in which there is no affirmative action at all ($\omega = 0$). Under this scenario, all students compete over the same spots and thus face the same admission thresholds.

We find that under the affirmative action counterfactual, where $\omega = 0.5$, cutoffs for open spots students are substantially higher than those reserved for targeted ones. This implies that targeted students can get admitted into selective degrees with much lower scores. In the absence of affirmative action, when $\omega = 0$, the distribution of cutoff scores (dashed black line) becomes uniform across affirmative action types and closer to the distribution of cutoff scores for open seats in the presence of affirmative action. Figures I.9 and I.10 present quantile-quantile plots that show how admission thresholds change for open and reserved spots under different affirmative action schedules ranging from 0 to 100%. The overall results suggest that, by removing the affirmative action program, admission into selective degrees becomes much harder for targeted students but not substantially easier for non-targeted ones.

8.4 Counterfactual outcomes

In Figure 11 we show the expected predicted income for targeted and non-targeted students under each of the counterfactuals. The red dashed line denote a counterfactual scenario without AA, while the blue solid line indicates a counterfactual with 50% reserved seats. The grey line in the background (and measured by the right-hand side axis) shows the distribution of students over

ENEM scores. In Figure 11(a) we show that the affirmative action program induces large gains on targeted individuals, especially on those students with high scores that now can access more selective degrees with higher value-added. As expected, targeted individuals at the very top of the score distribution are not affected by the AA regulation as all degrees are within reach even in the absence of the regulation.

These gains for targeted students, however, come at the cost of displacing non-targeted students from those very selective degrees. In Figure 11(b) we show that, even though non-targeted students are worse off under the affirmative action policy, their losses are small relative to the gains of targeted individuals. This is mostly explained by the fact that non-targeted individuals have lower chances of ending up in the outside option (see Figure **??**) and that their outside option has a relatively higher value-added (see Figure **??**).

Next, we estimate the aggregate effects of AA on predicted income. Let $\Delta_t(\omega) = \Delta_t(\omega, 0)$ denote the aggregate gains for affirmative action group t of moving from no AA to an ω AA schedule, as indicated by Equation (2). The overall aggregate gains over targeted and non-targeted individuals are defined as:

$$\Delta(\omega, \lambda) \quad = \quad \Delta_0(\omega) + \lambda\Delta_1(\omega) \tag{12}$$

where λ denote the welfare weights capturing society's concerns for fairness with respect to the targeted group.

In Figure 12(a) we present the group-specific, as well as the overall aggregate gains in terms of predicted income. We normalize these gains in terms of the aggregated predicted income for affirmative action group t when $\omega = 0$. The blue and red lines denote the gains and losses for targeted and non-targeted individuals, respectively. The grey line denote the normalized overall aggregate change using equal welfare weights across groups, that is $\lambda = 1$. We find that, at the current affirmative action schedule of $\omega = 0.5$, the average targeted individual sees an increase of 2.1% in their predicted income, while the average non-targeted students faces a drop of 3.8%. The predicted income of the average student across affirmative action groups reduces by 0.96%. These results are consistent with those presented in Figure 11. Although the gains for targeted students are greater than the losses experienced by non-targeted individuals, these gains are added over fewer targeted individuals and thus do not compensate for the overall losses of non-targeted individuals.

To take a stance on the efficiency of the policy, in Figure 12(b) we present the overall aggregate gains, $\Delta(\omega, \lambda)$, under different affirmative action schedules for different values of λ an. For a given λ, we normalize the efficiency gains relative to the overall aggregate predicted income in the absence of AA. The darker areas denote higher predicted income. The solid black line denotes the values of λ and ω such that $\Delta(\omega, \lambda) = 0$. The area to the left of the black solid line are net positive efficiency gains, while the area to the right reflects net losses. The black dashed line indicates the

AA schedule ω that is associated to highest $\Delta(\omega, \lambda)$ for a given λ. We find that a welfare weight of $\lambda = 2.2$ rationalizes the current policy of $\omega = 0.5$ as the optimal AA schedule.

9 Discussion and Conclusion

9.1 Discussion

i) what parameters are driving these results,

ii) how these results connect to the initial motivation of the paper

9.2 Conclusion

In this paper, we study the distributional consequences of affirmative action policies in centralized admission systems. We develop and estimate a model where that link preferences over degrees together with the potential outcomes from attending each of them. We find that the affirmative action policy increases enrollment of targeted students into more selective degrees. In terms of outcomes, we focus on the impact of affirmative action policies on academic progress and the expected income of attending selective degrees. Our results suggest that these policies create large benefits for targeted students while imposing a smaller cost to non-targeted individuals. However, the effects of the policy cascade through the system affecting a larger number of non-targeted individuals than the number of targeted students that it benefits. Taken altogether we find that the efficiency impacts on the system largely depends on the welfare weight given to targeted students.

This paper focuses on the first order welfare trade-off between targeted and non-targeted individuals. However, these type of policies can also be affect other margins of the educational market not considered in this paper. By increasing diversity, universities can affect student outcomes through peer effects and influencing inter-group attitudes. A more diverse composition of the student body may change the production function of degrees, by affecting student's academic outcomes, social behavior and preferences. Understanding how these different margins interact with the direct distributional effects of affirmative action policy is key to understand its overall role in shaping a higher education sector that fosters social mobility and promotes healthy democracies.

As pointed out by Chetty et al. (2020) only equalizing application, admission, and matriculation rates conditional on test scores would have little impact on the fraction of low-income students at selective colleges because there are relatively few students from low-income families with sufficiently high test scores.

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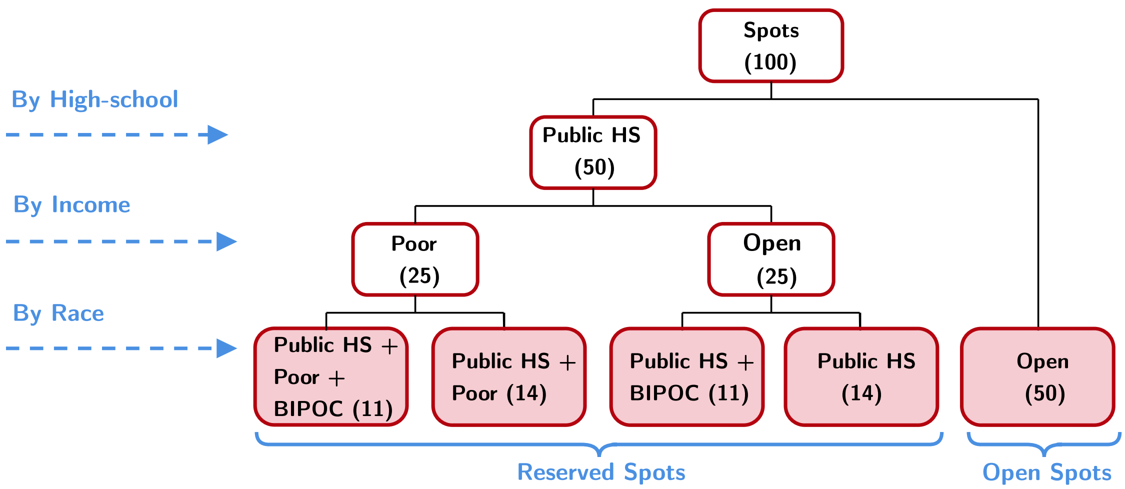
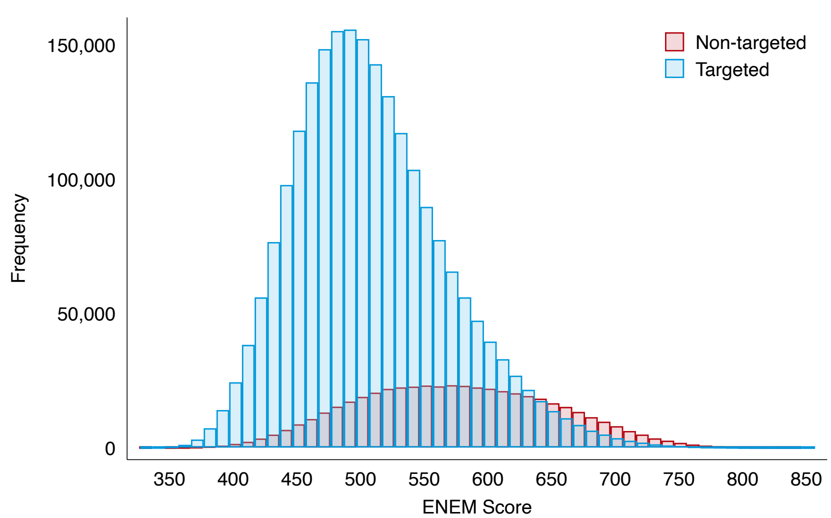
10 Figures

Figure 1: Affirmative Action Regulation



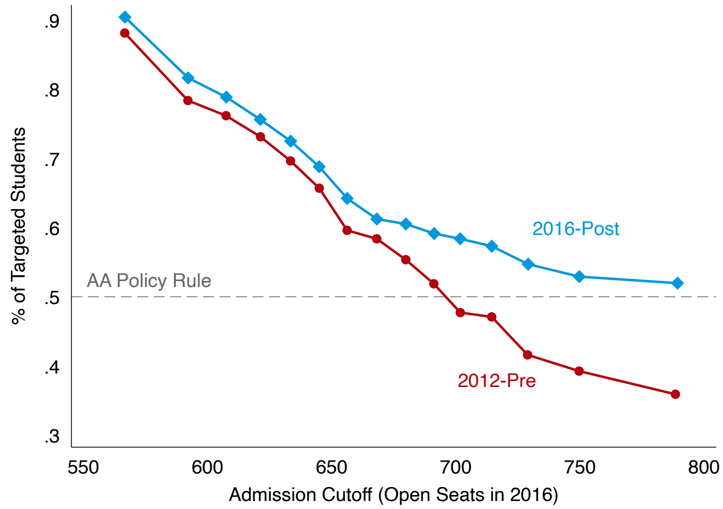
Notes: This figure describes the affirmative action policy. Under the regulation, for every 100 university spots, 50 are affirmative action spots for students who attended public high schools. Those affirmative actions spots are then divided equally by income, with 25 going to poor students and 25 to non-poor students. Finally, each group of 25 spots is distributed to reflect the proportion of non-white individuals in the population of a given state. This example uses a proportion of 54%, which is the combined share of black, brown, and indigenous people in the state of Minas Gerais, as reported by the Brazilian National Bureau of Statistics (IBGE) in 2012.

Figure 2: ENEM score distributions by targeted status



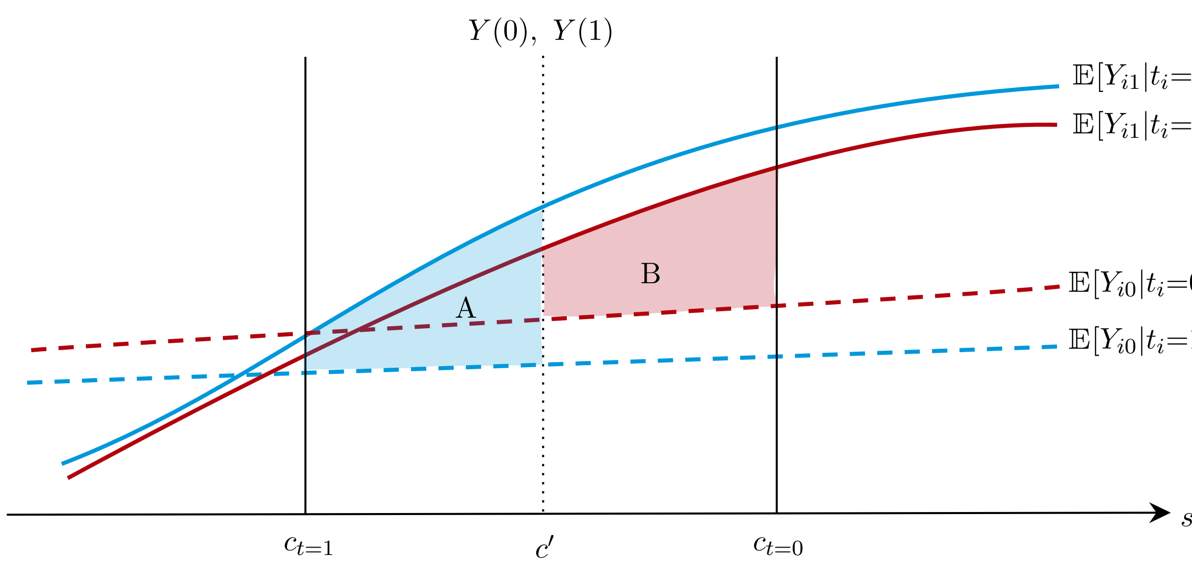
Notes: This figure shows kernel density plots of the average ENEM score distribution of targeted and non-targeted individuals. The sample is the universe of ENEM takers who had positive test scores in the 2015 ENEM test. The average test score include math, language, natural science, and social science. The average score is 504 and the standard deviation is 66 points. Targeted students are defined as those who are eligible for any of the affirmative action vacancies.

Figure 3: Student body composition in federal institutions



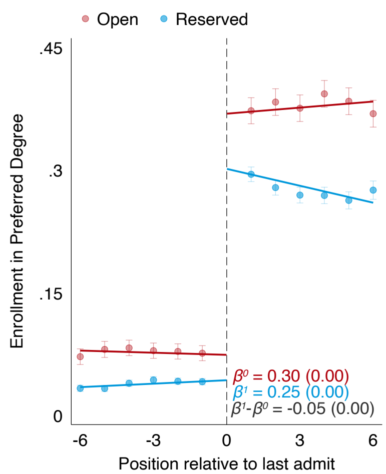
Notes: This Figure presents the average share of incoming targeted students in federal institutions by degree selectivity as defined as the admission cutoff of open spots in 2016 SISU. We consider individuals starting a degree program in the first semester of each year. An observation is a degree and shift tuple. We weight each observation by the size of the incoming cohort. We keep degree programs that exist both in 2012 and 2016 and that participated in 2016 SISU. Our sample covers 94% of the total enrollment in federal institutions in 2012 and 2016. The dashed line depicts the 50% AA policy rule mandated by the regulation.

Figure 4: Conceptual Framework

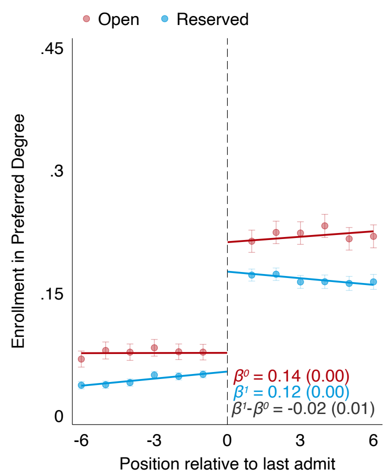


Notes: This figure presents the distributional impacts of an affirmative action policy in a centralized mechanisms. Each of the lines denote the mean potential outcome for targeted (blue line) and non-targeted (yellow line) individuals. While the solid line present the expected outcome of attending the selective degree, the dashed line presents the expected outcome of attending the outside option.

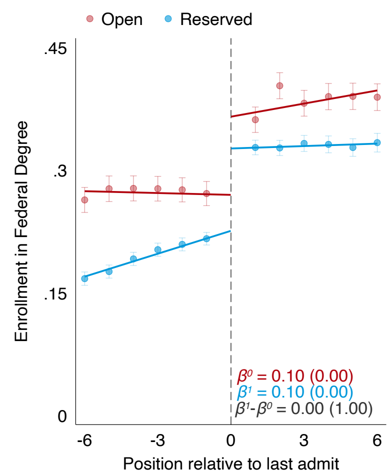
Figure 5: Regression discontinuity estimates



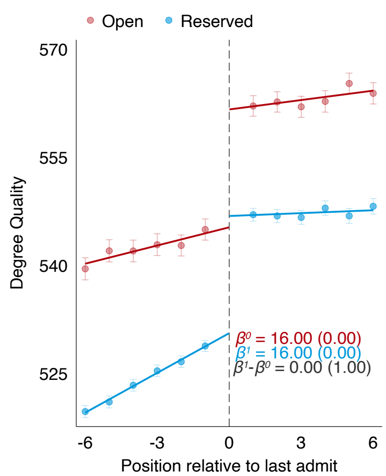
(a) Enrolled in preferred degree, $t = 1$



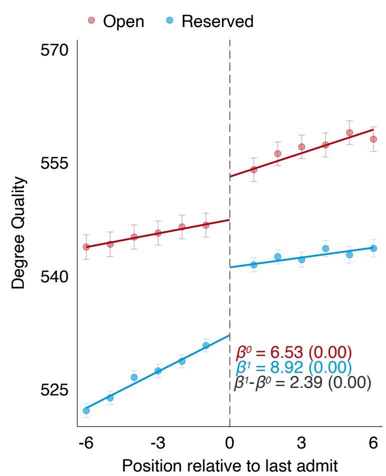
(b) Enrolled in preferred degree, $t = 4$



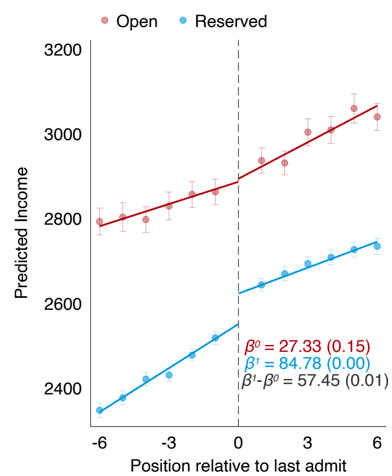
(c) Enrolled in federal degree, $t = 4$



(d) Degree quality in $t = 1$



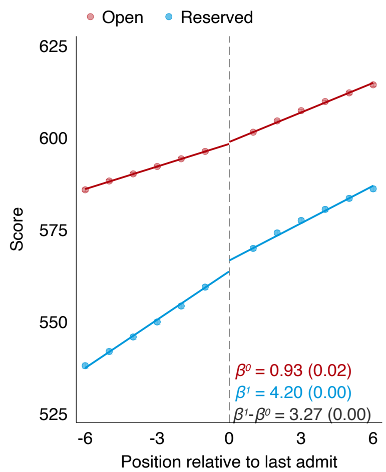
(e) Degree quality in $t = 4$



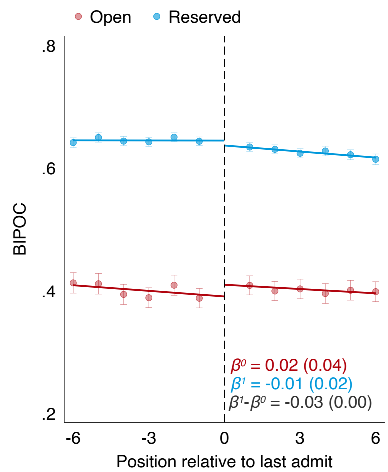
(f) Expected Earnings

Notes: This figure shows the regression discontinuity estimates. We pool all admission degrees and center them around the last admitted individual. The x-axis represents de position of a given student relative to the last admitted student. The red and blue lines show the outcomes for students applying for an open seat and a reserved seat respectively. The standard errors in a given cell represent the standard error of the outcome mean. The coefficients at the bottom of the figure indicate the treatment effect for each of the groups, as well as the difference between these coefficients. The p-value of the estimates is in parenthesis. The standard errors of the estimates are clustered at the waitlist level.

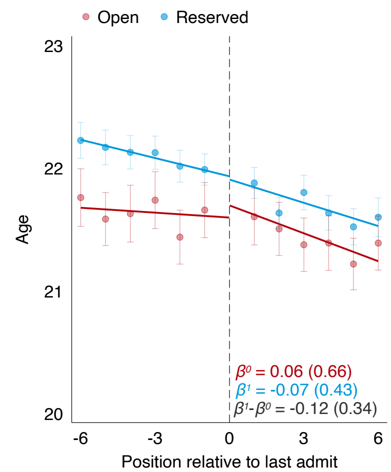
Figure 6: Regression discontinuity estimates, balance tests



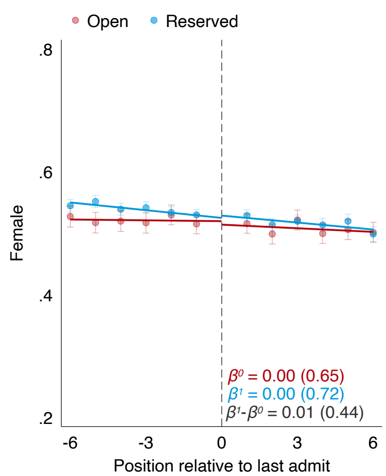
(a) Priority Score



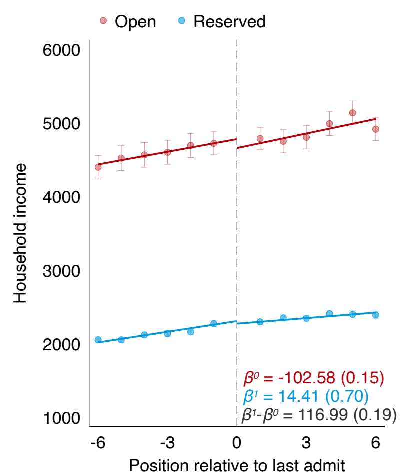
(b) BIPOC



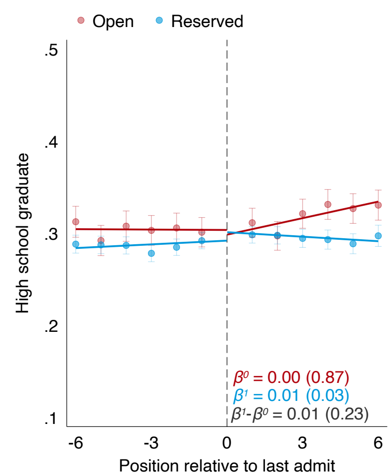
(c) Age



(d) Gender



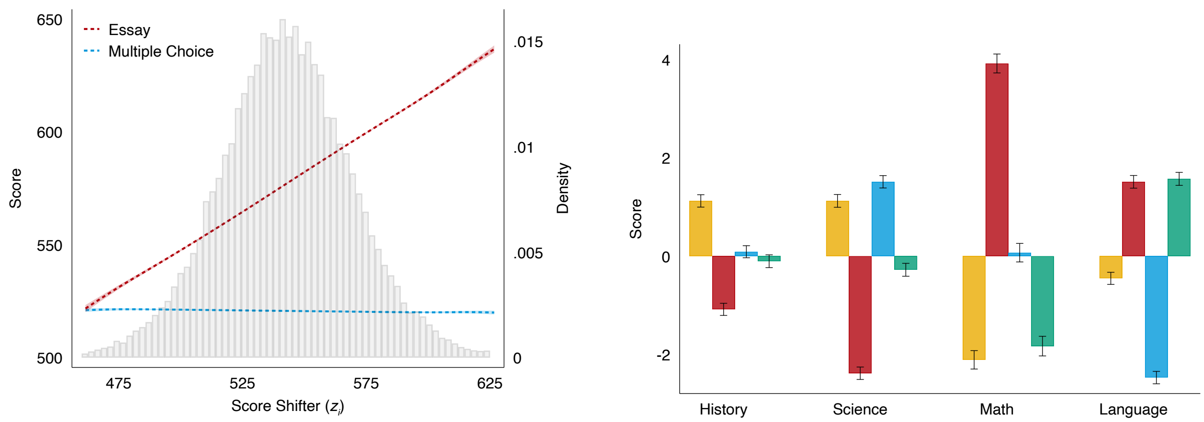
(e) Household income



(f) High school graduate

Notes: This figure shows the regression discontinuity estimates. We pool all admission degrees and center them around the last admitted individual. The x-axis represents de position of a given student relative to the last admitted student. The red and blue lines show the outcomes for students applying for an open seat and a reserved seat respectively. The standard errors in a given cell represent the standard error of the outcome mean. The coefficients at the bottom of the figure indicate the treatment effect for each of the groups, as well as the difference between these coefficients. The p-value of the estimates is in parenthesis. The standard errors of the estimates are clustered at the waitlist level.

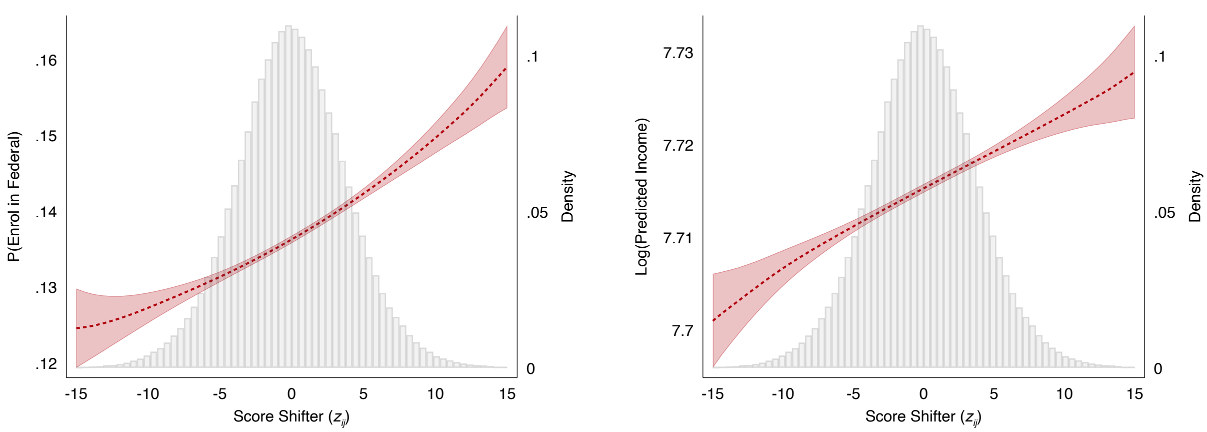
Figure 7: Score shifters



(a) Grader strictness (b) Booklet difficulty

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. The dashed line presents a local linear regression with a second order polynomial. The solid lines represent a 95% confidence bands. The histogram in the background reports the distribution of the shifter. Panel (b) reports the difference in average test scores across examination booklets for each of the different components of the ENEM national exam.

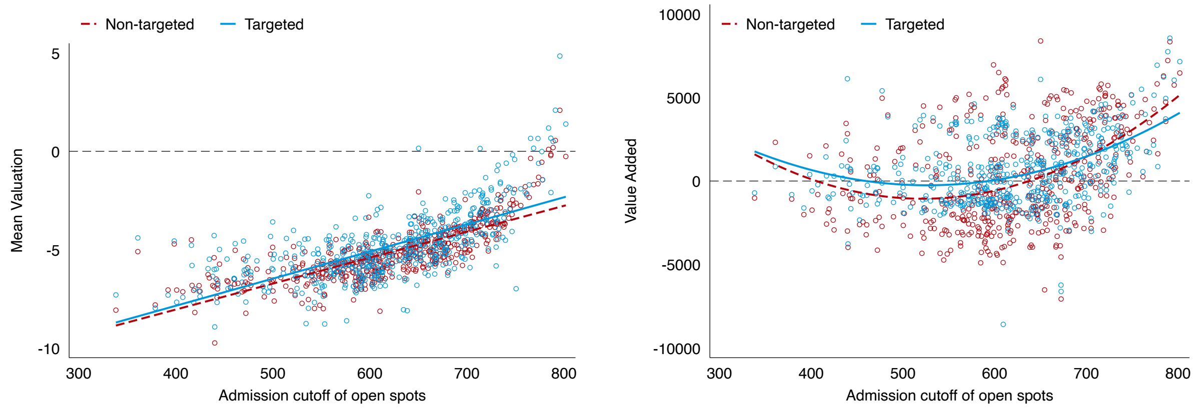
Figure 8: Score shifter, reduced form



(a) Enrollment in Federal Institution in 2016 (b) Log(Predicted Income)

Notes: The histogram in the background reports the distribution of the shifter z_i. Panel (a) shows the reduced-form between the score shifter leniency and the probability of attending a federal institution during the year after taking the exam. Panel (b) reports the relationship between the score shifter and log of predicted income based on college attainment in 2019. The dashed line presents a local linear regression with a second order polynomial. The solid lines represent a 95% confidence bands.

Figure 9: Degree valuations and Value Added



(a) Mean valuation and degree selectivity

(b) Value Added and degree selectivity

Notes: This figure present the mean valuation and value added of degrees based on the parameter estimates. Panel (a) shows the relationship between degree selectivity and students' mean valuation for them. Panel (b) shows the relationship between degree selectivity and degree's value added. In Panels (a) and (c), the x-axis denotes degree selectivity as measured by the admission cutoff of open seats c_{0j}. Panel (b) and (d) show the distribution of degree valuation and value added for each affirmative action type.

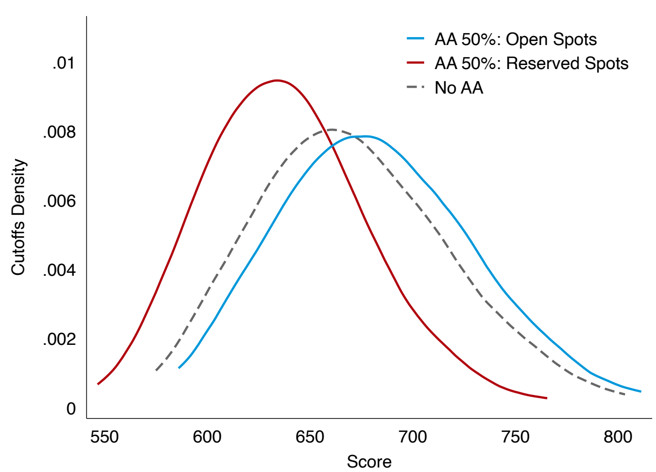
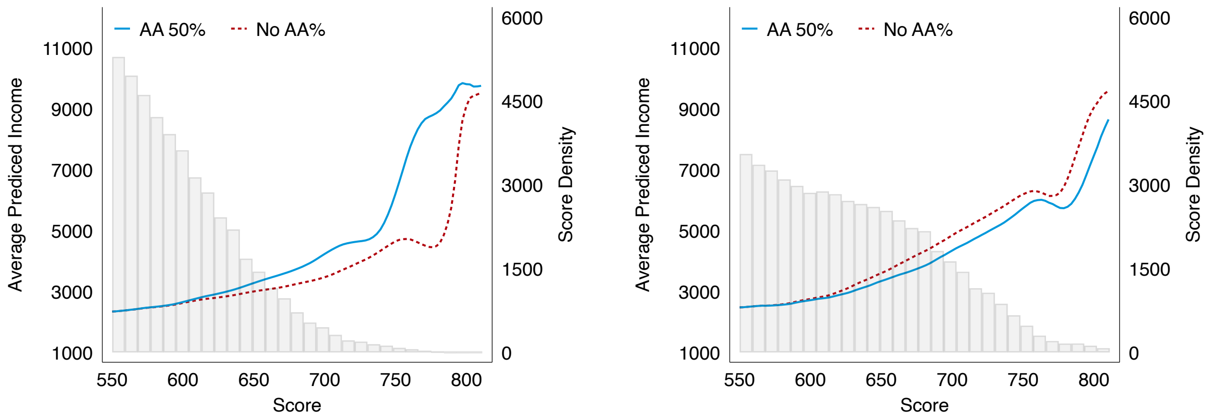


Figure 10: Distribution of cutoff scores

Notes: This figure shows the distribution of equilibrium cutoffs for open and reserved spots under different affirmative action schedules. The blue line shows the distribution of admission cutoffs for open spots when $\omega = 0.5$. The red line shows the distribution of admission cutoffs for reserved spots when $\omega = 0.5$. The dashed black line shows the distribution of admission cutoffs faced by all students when $\omega = 0$.

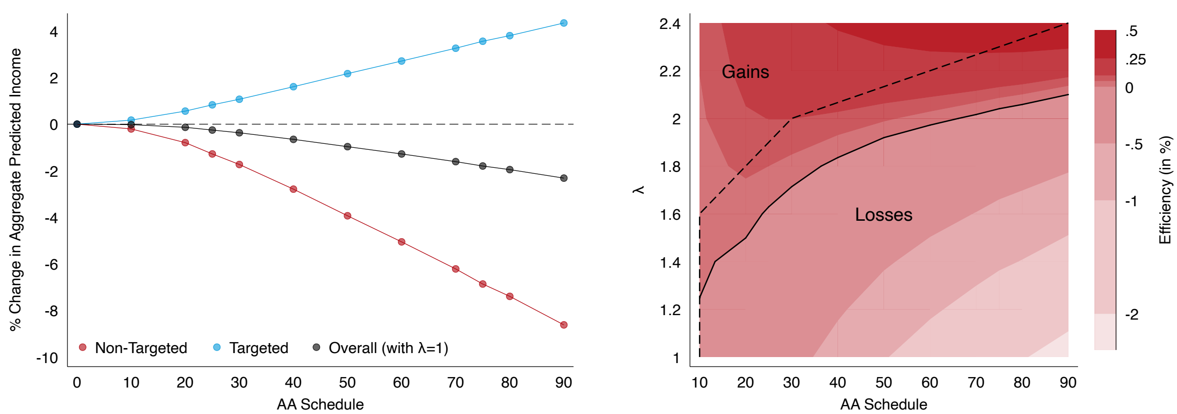


(a) Targeted students

(b) Non-targeted students

Figure 11: Students' income with and without affirmative action

Notes: This figure shows the expected outcome for targeted and non-targeted students with and without affirmative action across the score distribution. The blue line represents the expected outcome when $\omega = 0$, and the red line represents the outcome when $\omega = 0.5$.



(a) Predicted income

(b) By welfare weights

Figure 12: Predicted income under different affirmative action schedules

Notes: This figure shows the overall gains and losses of affirmative action in terms of predicted income. In Panel (a), we normalize the outcome with respect to the aggregate predicted 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. Panel (b) shows a heat map of the overall efficiency gains when we vary the affirmative action schedule and the welfare parameter λ. We normalize the outcome for a given λ, relative to a counterfactual scenario without AA. The solid black line denotes the values of λ and ω, such that there are zero efficiency gains. The dashed black line indicates the values of λ and ω, such that the efficiency gains are maximized.

Table 1: SISU Applicants

Admission Pool	Spots #	Spots %	Applications #	Applications %	# Spots to Apps (%)	Cutoff in SD
Open	92,409	46.0	1,978,077	44.0	4.7	0.00
Reserved, by Law						
Public HS	18,850	9.4	412,892	9.2	4.6	-0.43
Public HS & Income	19,768	9.8	482,761	10.7	4.1	-0.69
Public HS & Race	28,475	14.2	522,994	11.6	5.4	-0.76
Public HS & Race & Income	29,597	14.7	964,483	21.5	3.1	-0.88
Reserved, Other	11,778	5.9	135,491	3.0	8.7	-0.75
Total	200,877	100	4,496,698	100		

Notes: Own elaboration based on SISU microdata from 2016. This table shows the breakdown of the number of applicants and spots for each of the different admission pools. The "Other" admission channel includes other affirmative action initiatives that are not mandated by the federal regulation.

Table 2: Preference Parameters

	Targeted				Non-targeted			
	Mean	SD	P10	P90	Mean	SD	P10	P90
Panel A: School Choice Model								
Degree FE (δ_j)	11.85	11.34	-0.06	25.97	29.83	17.55	12.34	51.06
Ability (γ_j)	-0.02	0.02	-0.05	-0.01	-0.05	0.02	-0.07	-0.02
Location (κ)	- 2.73	-	-	-	-2.36	-	-	-
	(0.35)	-	-	-	(0.15)	-	-	-
Average Valuation (V_j)	-4.47	1.38	-5.90	-2.97	-3.43	1.15	-4.56	-2.07
Panel B: Potential Outcomes Model								
Degree FE (α_j)	-278	13,330	-16,974	11,362	1,177	39,553	-49,216	31,888
Ability (β_j^a)	0.96	20.58	-17.73	26.59	-3.97	56.83	-50.27	70.21
Selection in gains (ρ)	72.28	-	-	-	572	-	-	-
	(10.78)	-	-	-	(25.37)	-	-	-
Value Added (VA_j)	695	1,687	-687	3,011	-665	2,230	-2,736	3,161
Share of students	54.8%				44.7%			
Number of students	329,621							
Number of degrees	502							

Notes: This table summarizes the parameters estimates. AA denote the targeted students, while NA refers to non-targeted students. Panel A presents coefficients from the school choice model. Panel B display parameters from the potential outcomes model.

Appendix for:

Affirmative Action in Centralized College Admission Systems

Sebastián Otero Nano Barahona Cauê Dobbin

October 14, 2021

A Behavioral Responses [In Progress]

In this Appendix we explore the behavioral responses to the affirmative action regulation. We explore behavioral responses on three different margins. First, students might have responded by changing their pre-college human capital accumulation. Second, some students may have switched from private to public high-schools to gain eligibility as affirmative action students. Third, the affirmative action regulation might have changed the composition of applicants. We study each of these responses in turn.

A.1 Empirical Design

To study the causal impacts of increasing the number of reserved seats on behavioral responses we use a shift-share design that leverage differential exposure to the policy at the municipality level. Our master sample of municipalities is the universe of 5,571 municipalities in Brazil.

We create a measure of exposure to the AA policy based on the exposure to reserved seats in federal universities. Specifically, let r_{jt} denote the share of reserved seats at institution j in year t, and let s_{mt} be the share of ENEM takers that reside in municipality m and enroll at federal institution j. We use these variables to create an exposure measure at the municipality level, which is our treatment variable:

$$x_{mt} = \sum_{j \in \mathcal{J}} s_{mjt} \cdot r_{jt}$$

Of interest is a causal effect or structural parameter β, relating treatment to outcomes by

$$\begin{aligned} x_{mt} &= \sum_{k} \gamma_k \cdot z_{mtk} + \delta_m + \delta_t + \nu_{mt} \\ y_{mt} &= \beta x_{mt} + \delta_m + \delta_t + \epsilon_{mt} \end{aligned}$$

where ϵ_{mt} is an unobserved residual. We create an instrument for x_{mt} leveraging the difference between the share of pre-policy reserved spots, and the 50% share mandated by the regulation. The source of this variation is very salient in Figure I.2. In 2012, the year before the policy was implemented, 37% of degrees in federal institutions did not offered reserved spots, while another sizable 28% of the degrees offered more than 50% of their spots through reserved seats. Let g_j denote the difference between pre-policy and mandated reserved shares at institution j:

$$g_j \quad = \quad 2 \cdot \max\left(0, \left[0.5 - \frac{\text{reserved spots}_{j,2012}}{\text{total spots}_{j,2012}}\right]\right)$$

Institutions with $g_j = 0$ had 50% or more reserved seats in the pre-policy period and thus had little exposure to the AA regulation. In contrast, institutions with $g_j = 1$ did not offer any reserved spots and were highly exposed to the policy. We combine this variation with municipality-level exposure share weights based on the share of students in that municipality that enroll at institution j in 2012. We then interact those averages with year-specific dummies. Accordingly, our instrument z_{mt} is given by:

$$z_{mtk} = \mathbb{1}\{k = t\} \cdot \sum_{j \in \mathcal{J}} s_{mj,2012} \cdot g_j$$

In Figure **??** we show the coefficients of a first-stage regression between the institution level affirmative action shares r_j and the variation arising due to the policy g_j

A.2 ENEM composition

First, in terms of compositional responses, we find no evidence of the affirmative action regulation inducing changes in the number of ENEM takers or the share of targeted students who take the test.

A.3 ENEM test scores

Second, we examine behavioral responses in terms of student test scores. Based on theory, affirmative action can have heterogeneous effects on exam preparation effort depending on a student's position in the ability distribution and their affirmative action status (Bodoh-Creed and Hickman, 2018).[1] In Appendix **??**, we show that we find no evidence of changes in preparation effort for either group at any point in the test score distribution. This is in line with the results of Estevan et al. (2018), who examine an affirmative action policy in a large state university in Brazil and find no evidence of behavioral reactions concerning exam preparation effort.[2]

[1] Following Cotton et al. (2020)'s comparative static predictions, targeted individuals in the upper part of the ability distribution may reduce their effort profiting from lower admission test scores in their preferred degrees. On the other hand, the AA regulation may place within reach degrees which would otherwise be unattainable for targeted individuals, thus incentivizing them to increase their preparation effort. The predictions are reversed for the case of non-targeted students. For empirical evidence in other contexts, see Akhtari et al. (2020) and Cotton et al. (2021).

[2] These null effects on test scores are consistent with effort cost functions being sufficiently convex. The highly competitive nature of the exam might have already induced students to be at a sufficiently convex point of the effort cost function where the elasticity of effort to changes in admission thresholds is low.

A.4 High school movers

Third, we assess whether the regulation induces strategic responses from individuals gaming their affirmative action eligibility. As explained in Section 2, the AA regulation targeted students who had attended public high school from grades 10 to 12. In Appendix A.4, we evaluate the impact of the regulation on high school choice. Similar to Mello (2020), we find supportive evidence of students switching from private to public schools between grades 9 and 10 to obtain AA eligibility. Nonetheless, we neglect this margin of response from the analysis for two reasons. The first one is conceptual. We are interested in learning the implications of affirmative action regulation which targets fixed demographics, such as race or socioeconomic background. As such, we do not deem it important to include eligibility manipulation as a potential margin of response in the model. The second reason is empirical; ignoring this margin of response is very unlikely to bias our estimates. We focus our analysis on the 2016 SISU sample, where roughly 70% of all applicants had graduated from high school before 2015. Consequently, given the timing of the regulation, most applicants did not have any strategic incentives to switch schools.[3] Our reduced form estimates, imply that only 0.05% of all 2016 SISU applicants had switched schools in response to the AA regulation. Moreover, in Appendix A.4 we show that most strategic school-switchers come from low-performing private schools, suggesting that these movers are very unlikely to affect the final allocations of the mechanism.

To be eligible for the affirmative action admission tracks, students need to have completed grades 10 to 12 in a public high-school. In this appendix we study whether the affirmative action regulation impacted the switching rates from private to public high schools at grade 10. We use micro data from the school census in Brazil. This dataset is at the student leveI and allows us to follow the universe of students over time and across schools between 2009 and 2017.

We start by analyzing the switching behavior of 9th graders who were enrolled in regular private schools.[4] The private sector represents about 20% of the total enrollment in a given year. In 2012, around 250,000 students enrolled in 10th grade after completing 9th grade in a private school the year before. From this sample, 14% enrolled in a public institution and the remaining stayed in the private sector. It is important to note that between grades 9 and 10, students transition from middle school to high school. Since students are forced to change schools, switching rates from the private to the public sector are higher than in any other grade.

In Figure **??** we show how the share of movers from private to public institutions in 10th grade changes over time. The x-axis represents the year in which students attend 10th grade. We observe a big increase in the share of movers after the law was introduced in 2013. The rate of switchers revolves around 14% before the policy and stabilizes around 17.5% in 2015. This change in the trend of the switching rate is consistent with changes due the affirmative action policy but it could

[3]The regulation was announced in 2012 and implemented in 2013. Thus, students attending grades 10th or higher in private schools in 2012 (i.e. graduating before 2015) could have not switched to a public school and become eligible for the reserved spots.

[4]We restrict our sample to students between 13 and 17 years of age.

also mask other contemporaneous shocks affecting the decision to switch from the private to the public sector (e.g. improvements in the public sector infrastructure).

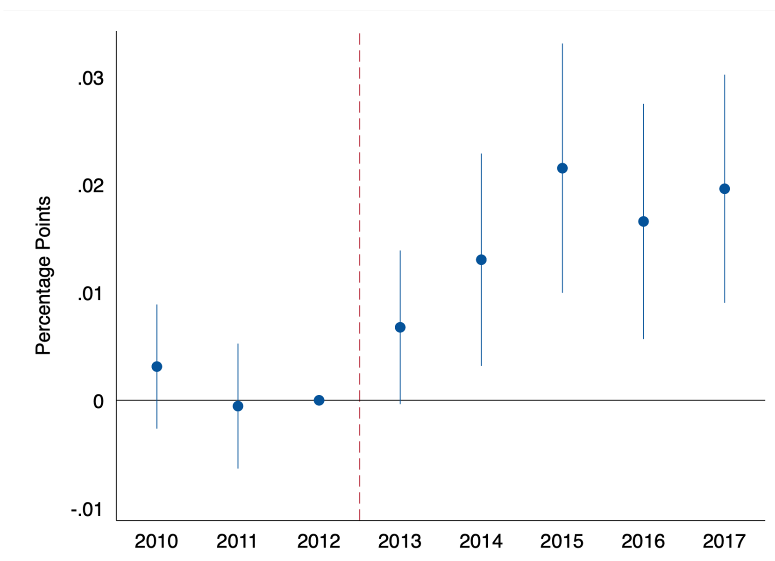
In order to have a comparison group that helps isolate the effect of the the policy from other confounding factors we use students switching to the public sector between grades 10 and 11. In contrast to individuals who switch between grades 9 and 10, these students cannot gain affirmative action status as a result of switching to a public institution. As such, it is unlikely that these movers are motivated by strategic reasons related to the affirmative action policy.

We estimate the following differences in differences model:

$$switch_{it} = \sum_{j} \beta_j \cdot Treat_i \cdot 1\{t = j\} + \alpha \cdot Treat_i + \delta_{d(i)t} + \varepsilon_{it} \tag{13}$$

where $d(i)$ and t stand for school district and year, respectively, and $Treat_i$ is an indicator variable taking the value 1 if student i is in 9th grade and 0 otherwise. We make the normalization $\beta_{2012} = 0$, so that all β_j coefficients represent differences in outcomes relative to the year before the policy was implemented. The estimates are presented in Figure A.1. We observe that before the affirmative action regulation is passed there are no differences in the pre-trend between switching rates of 9th graders relative to 10th graders. The coefficient grows between 2013 and 2015, and then stabilizes around 2 percentage points.

Figure A.1: Dynamic differences-in-differences estimates



Notes: This figure shows the differences in differences β_j estimates from Equation 13. The coefficient are year-specific coefficient for individuals in 9th grade relative to individuals in 10th grade.

In 2016, roughly 70% of all applicants to SISU had graduated from high school before 2015. Thus, this subset of applicants did not have any strategic incentives to switch schools. The remaining 30% graduated from high school in 2015, out of which 27% came from a private high schools.

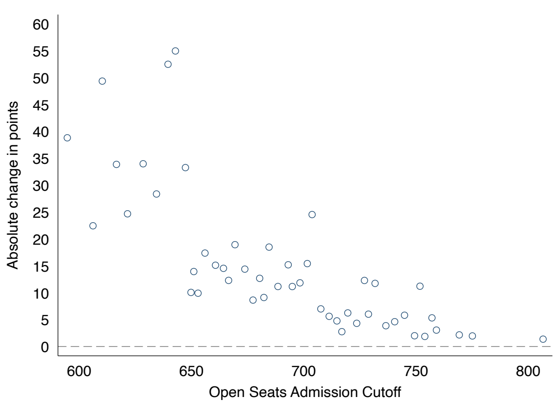
Taking the estimates from Equation 13 at face value, implies that the total share of students from private high schools would have been 2 percentage points higher 0.3×0.27

B Iterative Admission Cutoffs

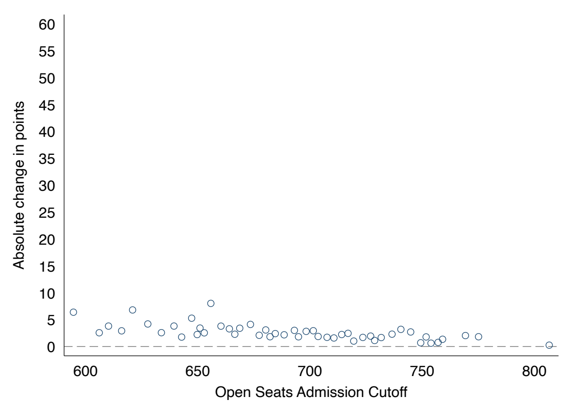
The centralized admission system uses an iterative deferred acceptance mechanism. Under this system, students are sequentially asked to submit rank ordered lists over the course of several "trial" day. At the end of each day, the system produces a cutoff grade representing the lowest grade necessary to be accepted at a specific program. In this Appendix we explore the change in admission cutoffs over the course of the application period.

Unfortunately, there are no administrative records of the admission cutoffs reported by the system throughout the application period. The system only saves the final admission cutoffs. To circumvent this issue, we scrapped online data of the degrees offered by the Federal University of Minas Gerais (UFMG) during the 2021 admission period. In total we observe 450 admission cutoffs (90 degrees with 5 admission tracks each) over the 9 days of the application process. In Figure B.1 we show how the admission cutoffs evolve over time. Panel (a) displays the absolute difference between the final admission cutoffs (day T in the figure) and the one reported in the first day of the admission period. We observe large differences in admission cutoffs, especially for less selective degrees. Panel (b) displays the absolute difference between the final admission cutoffs (day T in the figure) and the one reported in last day of the admission period. The pattern in the data shows that at the last day of the application period, most degrees have converged or are very close to converging to the final admission cutoff.

Figure B.1: Absolute Change in admission cutoffs



(a) Day 1 - Day T



(b) Day T-1 - Day T

Notes: This figure shows the absolute change in admission cutoffs over degree selectivity, as defined by the degree's final admission cutoffs of the open seats. Panel (a) display the difference between the absolute difference between the final admission cutoffs and those reported in the first day of the system. Panel (b) displays the difference between the absolute difference between the final admission cutoffs in those reported in the penultimate day of the system.

Overall, these data suggest that the ex-ante and ex-post eligibility into degrees are very similar.

C Predicted income

In this Appendix we discuss the econometric procedure that we use to predict student's income based on their academic trajectories. Ideally, we would like to use labor market outcomes of students as the main outcome of interest for the analysis. Unfortunately, our sample consist of 2015 ENEM takers, and as such several individuals are still enrolled in college which makes it too early to find them in the labor market. Instead, we create a measure of predicted income that takes into account the academic trajectories between the 2016 and 2019 academic years. We argue that using the predicted income has two important advantages. First, we use this income as a currency that allow us to compare individuals with different trajectories that would be otherwise impossible weigh up against each other. Second, we can use trajectories as surrogates, and under some assumptions interpret these results as the long term effects of the policy (Athey et al., 2019).

Table 3 illustrates the problem. Assume 0 is the outside option, and 1 and 2 are degree programs in public and private institutions respectively. For the 2015 ENEM cohort we observe trajectories represented by columns (1)-(4), but we do not observe income. Instead for past cohorts we observe both trajectories together with student's income several years after taking ENEM. We use past cohorts to create a mapping from trajectories to income, and then apply this mapping to the 2015 ENEM cohort to recover their predicted income.

Table 3: Examples of Academic Trajectories

	Degree in $t=1$	Degree in $t=2$	Degree in $t=3$	Degree in $t=4$	Income in $t=8$
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

Our working sample consists of all ENEM takers between years 2009-2012. We have one snapshot of income in 2017 from the administrative matched employer employee records (RAIS). Ideally, we would not parametrically match individuals with identical academic trajectories as a way to recover predicted income for the 2015 ENEM sample. Although this exact matching is hypothetically possible, it is unfeasible in our data due to the large number of degrees and the many possible combinations of degrees that define a trajectory.

As an alternative approach, we summarize the academic trajectory using degree attainment in year 1 and year 4. We provide two different specification which differ on the set of individual level controls that we add together with the trajectory fixed effects. In specification (1) we add a set of controls X_i^1 which include gender, and the test scores in each of the 5 components of the ENEM test. In specification (2) we include the same set of covariates but also add variables accounting for the traits targeted by the AA regulation (i.e. racial and high-school dummies). Specifically, these equations are:

$$
\begin{aligned}
\text{Specification 1:} \quad y_{i,T} &= \phi_t^1 + X_i^1 \pi^1 + \delta^1_{J(i,t+1)} + \delta^1_{J(i,t+4)} + \epsilon_i^1 \\
\text{Specification 2:} \quad y_{i,T} &= \phi_t^2 + X_i^2 \pi^2 + \delta^2_{J(i,t+1)} + \delta^2_{J(i,t+4)} + \epsilon_i^2,
\end{aligned}
$$

where α_t are dummies indicating the year of ENEM, T denotes the year in which we observe income. Degree fixed effects are captured by δ, and $J(i,t)$ is a function indicating the degree that student i attends in period t. We use these coefficients to predict earnings of 2015 ENEM takers (SISU 2016).

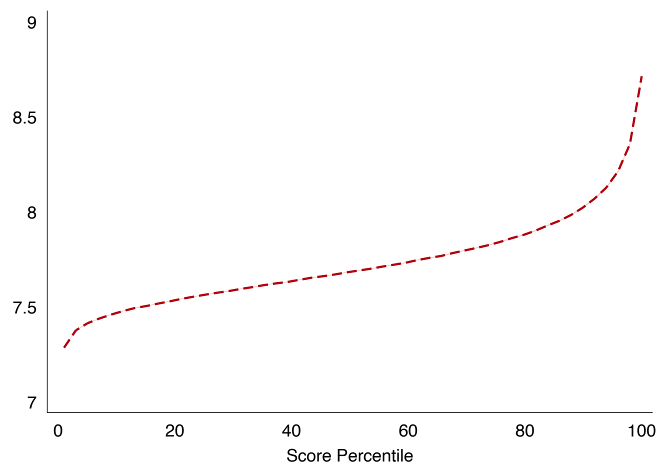


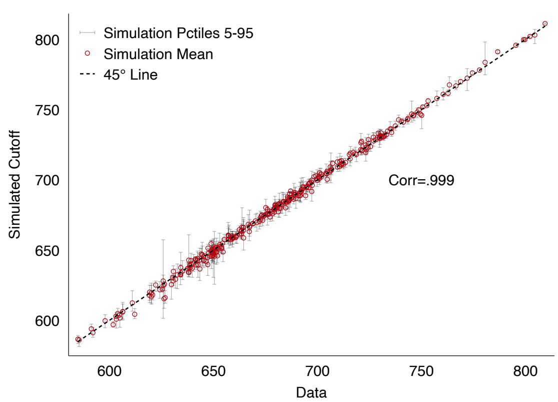
Figure C.1: Log (Predicted Income)

D Model Fit

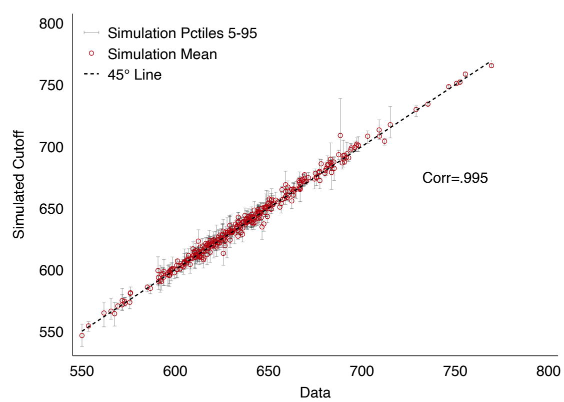
To verify that our parameters can recover the observed allocation, we simulate the admission cutoffs predicted by the model and contrast them against those observed in the data. To recover the admission cutoffs we follow steps 1 to 5 described in Appendix G, and simulate 20 different admission cutoffs for each of the different degree programs.

Panel (a) and (b) in Figure D show the model fit for the admission cutoffs of open and reserved seats, respectively. In the horizontal axis, we plot the observed admission cutoff scores, while in 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 different admission cutoffs for the reserved spots: one for each affirmative action type. To construct the observed cutoff we take a weighted average of the four admission cutoffs where the weights correspond to the share of students of each affirmative action type applying on SISU. We observe that admission cutoffs predicted by our model are very similar to those observed in the data. The correlation coefficient of the slope is virtually 1 for both open and reserved seats.

Figure D.1: Model fit



(a) Open



(b) Reserved

Notes: This figure shows the in-sample model fit for the admission cutoffs of open and reserved seats. In the horizontal axis, we plot the observed cutoff score, while in the vertical axis, we plot the average cutoff score across all simulations and the 95% coverage interval.

E Capacity constraints in the private sector [In progress]

F Identification Proof [In Progress]

We show that in the extreme case in which our score shifter has a variance of infinite (i.e., test scores are randomly assign), we can identify effects at any point of the observed test score distribution. In contrast, when the shifter has a variance of zero, the identification strictly arises from the discontinuities and cannot be extrapolated away from the marginally affected student. extrapolation away from it is driven by functional form assumptions.

F.1 Setting

Assume there are $J = 4$ degrees $\mathcal{J} = \{A, B, C, D\}$. For simplicity we order them terms of selectivity such that the admission thresholds of A, B and C are given by $c_A > c_B > c_C$, and D is the outside option with unlimited capacity.

Students are characterized by their preferences $\succ_i$ and score s_i. All degrees give the same weight to all tests such that score does not depend on j. Note that everything can be generalize to having X_i, where s_i is just a component of X_i. In addition, every student-degree combination has a potential outcome given by:

$$
\begin{aligned}
Y_{ij} &= \mathbb{E}[Y_{ij}|s_i] + \varepsilon_{ij} \\
&= Y_j(s_i) + \varepsilon_{ij}
\end{aligned}
$$

In the data we observe choice sets Ω_i, choices probabilities $\pi_j(\Omega_i, s_i) = Pr(D_i = j \,|\, \Omega_i, s_i)$ and outcomes. The non-parametric form of the mean observed outcome in the data is:

$$
E[Y_i|D_i = j, \Omega_i, s_i] = Y_j(s_i) + E[\varepsilon_{ij}|D_i = j, \Omega_i, s_i]
$$

Our goal is to recover $Y_j(s_i)$. We explore two cases:

1. **Random allocation of choice sets**: By assigning choice sets at random, we have the following combinations of choice sets:

$$
\Omega_i \in \{(A, B, C, D), (A, B, D), (A, C, D), (A, D), (B, C, D), (B, D), (C, D), (D)\}
$$

2. **Deferred acceptance algorithm**: Under this mechanism, we would observe the following choice sets in the data:

$$
\Omega_i \in \{(A, B, C, D), (B, C, D), (C, D), (D)\}
$$

F.2 Random Choice Sets

When choice sets are random, we can flexibly condition on any given score $s_i = s$. Let Δ_j^{m-n} denote the difference in mean outcomes of individuals attending j between individuals with choice set Ω^m and Ω^n. Note that Δ_j^{m-n} is observed. We prove identification of $Y_j(s_i)$ for $j = A$. A similar argument applies for the rest of degrees.

From the definition of Δ_j^{m-n} we know that:

$$\begin{aligned}
\Delta_A^{(A,B,C,D)-(A,B,D)} &= E[Y_i|D_i = A, \Omega_i = (A,B,C,D), s_i = s] - E[Y_i|D_i = A, \Omega_i = (A,B,D), s_i = s] \\
\Delta_A^{(A,B,C,D)-(A,D)} &= E[Y_i|D_i = A, \Omega_i = (A,B,C,D), s_i = s] - E[Y_i|D_i = A, \Omega_i = (A,D), s_i = s] \\
\Delta_A^{(A,C,D)-(A,D)} &= E[Y_i|D_i = A, \Omega_i = (A,C,D), s_i = s] - E[Y_i|D_i = A, \Omega_i = (A,D), s_i = s] \\
\Delta_A^{(A,B,D)-(A,D)} &= E[Y_i|D_i = A, \Omega_i = (A,B,D), s_i = s] - E[Y_i|D_i = A, \Omega_i = (A,D), s_i = s]
\end{aligned}$$

Using Equation (14) we can rewrite the previous system of equations as:

$$\begin{aligned}
\Delta_A^{(A,B,C,D)-(A,B,D)} &= E[\varepsilon_{iA}|D_i = A, \Omega_i = (A,B,C,D), s_i = s] - E[\varepsilon_{iA}|D_i = A, \Omega_i = (A,B,D), s_i = s] \\
\Delta_A^{(A,B,C,D)-(A,D)} &= E[\varepsilon_{iA}|D_i = A, \Omega_i = (A,B,C,D), s_i = s] - E[\varepsilon_{iA}|D_i = A, \Omega_i = (A,D), s_i = s] \\
\Delta_A^{(A,C,D)-(A,D)} &= E[\varepsilon_{iA}|D_i = A, \Omega_i = (A,C,D), s_i = s] - E[\varepsilon_{iA}|D_i = A, \Omega_i = (A,D), s_i = s] \\
\Delta_A^{(A,B,D)-(A,D)} &= E[\varepsilon_{iA}|D_i = A, \Omega_i = (A,B,D), s_i = s] - E[\varepsilon_{iA}|D_i = A, \Omega_i = (A,D), s_i = s]
\end{aligned}$$

which leads to a system of 4 equations and 4 unknowns. This means that the unobserved component of equation (14) is identified. Since we know $E[\varepsilon_{ij}|D_i, \Omega_i, s_i]$, and we observe $E[Y_i|D_i, \Omega_i, s_i]$, we use Equation (14) to recover $Y_j(s_i)$.

F.3 Deferred acceptance mechanism

When individuals are assigned using a deferred acceptance mechanism, we can only estimate parameters for individuals in the neighborhood of the admission threshold scores $s_i = c_A$, $s_i = c_B$, and $s_i = c_C$.

Let Δ_j^s denote the difference in mean outcomes of individuals attending j between individuals with score $s^+ = s + \epsilon$ and score $s^- = s - \epsilon$ (with $\epsilon \to 0$). Then:

$$\begin{aligned}
\Delta_B^{c_A} &= E[Y_i|D_i = B, \Omega_i = (A,B,C,D), s_i = c_A^+] - E[Y_i|D_i = B, \Omega_i = (B,C,D), s_i = c_A^-] \\
\Delta_C^{c_A} &= E[Y_i|D_i = C, \Omega_i = (A,B,C,D), s_i = c_A^+] - E[Y_i|D_i = C, \Omega_i = (B,C,D), s_i = c_A^-] \\
\Delta_D^{c_A} &= E[Y_i|D_i = D, \Omega_i = (A,B,C,D), s_i = c_A^+] - E[Y_i|D_i = D, \Omega_i = (B,C,D), s_i = c_A^-] \\
\Delta_C^{c_B} &= E[Y_i|D_i = C, \Omega_i = (B,C,D), s_i = c_B^+] - E[Y_i|D_i = C, \Omega_i = (C,D), s_i = c_B^-] \\
\Delta_D^{c_B} &= E[Y_i|D_i = D, \Omega_i = (B,C,D), s_i = c_B^+] - E[Y_i|D_i = D, \Omega_i = (C,D), s_i = c_C^-] \\
\Delta_D^{c_C} &= E[Y_i|D_i = D, \Omega_i = (C,D), s_i = c_C^+] - E[Y_i|D_i = D, \Omega_i = (D), s_i = c_C^-]
\end{aligned}$$

Using Equation (14) we can rewrite the previous system of equations as:

$$
\begin{aligned}
\Delta_B^{c_A} &= E[\varepsilon_{iB}|D_i = B, \Omega_i = (A,B,C,D), s_i = c_A^+] - E[\varepsilon_{iB}|D_i = B, \Omega_i = (B,C,D), s_i = c_A^-] \\
\Delta_C^{c_A} &= E[\varepsilon_{iC}|D_i = C, \Omega_i = (A,B,C,D), s_i = c_A^+] - E[\varepsilon_{iC}|D_i = C, \Omega_i = (B,C,D), s_i = c_A^-] \\
\Delta_D^{c_A} &= E[\varepsilon_{iD}|D_i = D, \Omega_i = (A,B,C,D), s_i = c_A^+] - E[\varepsilon_{iD}|D_i = D, \Omega_i = (B,C,D), s_i = c_A^-] \\
\Delta_C^{c_B} &= E[\varepsilon_{iC}|D_i = C, \Omega_i = (B,C,D), s_i = c_B^+] - E[\varepsilon_{iC}|D_i = C, \Omega_i = (C,D), s_i = c_B^-] \\
\Delta_D^{c_B} &= E[\varepsilon_{iD}|D_i = D, \Omega_i = (B,C,D), s_i = c_B^+] - E[\varepsilon_{iD}|D_i = D, \Omega_i = (C,D), s_i = c_B^-] \\
\Delta_D^{c_C} &= E[\varepsilon_{iD}|D_i = D, \Omega_i = (C,D), s_i = c_C^+] - E[\varepsilon_{iD}|D_i = D, \Omega_i = (D), s_i = c_C^-]
\end{aligned}
$$

This leads to a system of 6 equations and 12 unknowns, and therefore is not identified. We need to make additional assumptions.

We define $\lambda_k(\cdot)$ as a function that maps from students unobservables to choice probabilities. We can choose any functional form of λ as long as [We need to check if this is true]

$$\lambda_k(D_i, \Omega_i, s_i) > \lambda_h(D_i, \Omega_i, s_i) \Leftrightarrow Pr(D_i = k|\Omega_i, s_i, \succ_i) > Pr(D_i = h|\Omega_i, s_i, \succ_i)$$

Given the functional form that we choose, $\lambda_k(\cdot)$ is observed.

A potential assumption is to restrict the way in which ε_i relates to choices. Following Dubin and McFadden (1984), we impose a linear relationship between potential outcomes and the unobserved error component

$$
\begin{aligned}
E[\varepsilon_{ij}|D_i, \Omega_i, s_i] &= \psi_A(s_i)\lambda_A(D_i, \Omega_i, s_i) + \psi_B(s_i)\lambda_B(D_i, \Omega_i, s_i) + \psi_C(s_i)\lambda_C(D_i, \Omega_i, s_i) \\
&\quad + \psi_D(s_i)\lambda_D(D_i, \Omega_i, s_i) + \rho(s_i)\lambda_j(D_i, \Omega_i, s_i)
\end{aligned}
$$

With this restriction in place, we can rewrite our identifying equations:

$$
\begin{aligned}
\Delta_B^{c_A} \;=\;& \psi_A(c_A)\left[\lambda_A(D_i = B, \Omega_i = (A,B,C,D), s_i = c_A) - \lambda_A(D_i = B, \Omega_i = (B,C,D), s_i = c_A)\right] \\
& + \psi_B(c_A)\left[\lambda_B(D_i = B, \Omega_i = (A,B,C,D), s_i = c_A) - \lambda_B(D_i = B, \Omega_i = (B,C,D), s_i = c_A)\right] \\
& + \psi_C(c_A)\left[\lambda_C(D_i = B, \Omega_i = (A,B,C,D), s_i = c_A) - \lambda_C(D_i = B, \Omega_i = (B,C,D), s_i = c_A)\right] \\
& + \psi_D(c_A)\left[\lambda_D(D_i = B, \Omega_i = (A,B,C,D), s_i = c_A) - \lambda_D(D_i = B, \Omega_i = (B,C,D), s_i = c_A)\right] \\
& + \rho(c_A)\left[\lambda_B(D_i = B, \Omega_i = (A,B,C,D), s_i = c_A) - \lambda_B(D_i = B, \Omega_i = (B,C,D), s_i = c_A)\right] \\
\Delta_C^{c_A} \;=\;& \psi_A(c_A)\left[\lambda_A(D_i = C, \Omega_i = (A,B,C,D), s_i = c_A) - \lambda_A(D_i = C, \Omega_i = (B,C,D), s_i = c_A)\right] \\
& + \psi_B(c_A)\left[\lambda_B(D_i = C, \Omega_i = (A,B,C,D), s_i = c_A) - \lambda_B(D_i = C, \Omega_i = (B,C,D), s_i = c_A)\right] \\
& + \psi_C(c_A)\left[\lambda_C(D_i = C, \Omega_i = (A,B,C,D), s_i = c_A) - \lambda_C(D_i = C, \Omega_i = (B,C,D), s_i = c_A)\right] \\
& + \psi_D(c_A)\left[\lambda_D(D_i = C, \Omega_i = (A,B,C,D), s_i = c_A) - \lambda_D(D_i = C, \Omega_i = (B,C,D), s_i = c_A)\right] \\
& + \rho(c_A)\left[\lambda_C(D_i = C, \Omega_i = (A,B,C,D), s_i = c_A) - \lambda_C(D_i = C, \Omega_i = (B,C,D), s_i = c_A)\right] \\
\Delta_D^{c_A} \;=\;& \psi_A(c_A)\left[\lambda_A(D_i = D, \Omega_i = (A,B,C,D), s_i = c_A) - \lambda_A(D_i = D, \Omega_i = (B,C,D), s_i = c_A)\right] \\
& + \psi_B(c_A)\left[\lambda_B(D_i = D, \Omega_i = (A,B,C,D), s_i = c_A) - \lambda_B(D_i = D, \Omega_i = (B,C,D), s_i = c_A)\right] \\
& + \psi_C(c_A)\left[\lambda_C(D_i = D, \Omega_i = (A,B,C,D), s_i = c_A) - \lambda_C(D_i = D, \Omega_i = (B,C,D), s_i = c_A)\right] \\
& + \psi_D(c_A)\left[\lambda_D(D_i = D, \Omega_i = (A,B,C,D), s_i = c_A) - \lambda_D(D_i = D, \Omega_i = (B,C,D), s_i = c_A)\right] \\
& + \rho(c_A)\left[\lambda_D(D_i = D, \Omega_i = (A,B,C,D), s_i = c_A) - \lambda_D(D_i = D, \Omega_i = (B,C,D), s_i = c_A)\right]
\end{aligned}
$$

and so on for the other thresholds...

Note that here we have only 3 equations and 5 parameters ($\psi_A(c_A)$,$\psi_B(c_A)\psi_C(c_A)$,$\psi_D(c_A)$,$\rho(c_A)$) and therefore the model is not identified. We need an additional restriction.

By imposing that $\psi_j(s) = \psi_k$ and that $\rho(s) = \rho$, I will show now that the model is identified.

We can rewrite our previous conditions:

$$
\begin{aligned}
\Delta_B^{c_A} &= \psi_A \left[\lambda_A(D_i = B, \Omega_i = (A,B,C,D), s_i = c_A) - \lambda_A(D_i = B, \Omega_i = (B,C,D), s_i = c_A)\right] \\
&\quad + \psi_B \left[\lambda_B(D_i = B, \Omega_i = (A,B,C,D), s_i = c_A) - \lambda_B(D_i = B, \Omega_i = (B,C,D), s_i = c_A)\right] \\
&\quad + \psi_C \left[\lambda_C(D_i = B, \Omega_i = (A,B,C,D), s_i = c_A) - \lambda_C(D_i = B, \Omega_i = (B,C,D), s_i = c_A)\right] \\
&\quad + \psi_D \left[\lambda_D(D_i = B, \Omega_i = (A,B,C,D), s_i = c_A) - \lambda_D(D_i = B, \Omega_i = (B,C,D), s_i = c_A)\right] \\
&\quad + \rho \left[\lambda_B(D_i = B, \Omega_i = (A,B,C,D), s_i = c_A) - \lambda_B(D_i = B, \Omega_i = (B,C,D), s_i = c_A)\right] \\
\Delta_C^{c_A} &= \psi_A \left[\lambda_A(D_i = C, \Omega_i = (A,B,C,D), s_i = c_A) - \lambda_A(D_i = C, \Omega_i = (B,C,D), s_i = c_A)\right] \\
&\quad + \psi_B \left[\lambda_B(D_i = C, \Omega_i = (A,B,C,D), s_i = c_A) - \lambda_B(D_i = C, \Omega_i = (B,C,D), s_i = c_A)\right] \\
&\quad + \psi_C \left[\lambda_C(D_i = C, \Omega_i = (A,B,C,D), s_i = c_A) - \lambda_C(D_i = C, \Omega_i = (B,C,D), s_i = c_A)\right] \\
&\quad + \psi_D \left[\lambda_D(D_i = C, \Omega_i = (A,B,C,D), s_i = c_A) - \lambda_D(D_i = C, \Omega_i = (B,C,D), s_i = c_A)\right] \\
&\quad + \rho \left[\lambda_C(D_i = C, \Omega_i = (A,B,C,D), s_i = c_A) - \lambda_C(D_i = C, \Omega_i = (B,C,D), s_i = c_A)\right] \\
\Delta_D^{c_A} &= \psi_A \left[\lambda_A(D_i = D, \Omega_i = (A,B,C,D), s_i = c_A) - \lambda_A(D_i = D, \Omega_i = (B,C,D), s_i = c_A)\right] \\
&\quad + \psi_B \left[\lambda_B(D_i = D, \Omega_i = (A,B,C,D), s_i = c_A) - \lambda_B(D_i = D, \Omega_i = (B,C,D), s_i = c_A)\right] \\
&\quad + \psi_C \left[\lambda_C(D_i = D, \Omega_i = (A,B,C,D), s_i = c_A) - \lambda_C(D_i = D, \Omega_i = (B,C,D), s_i = c_A)\right] \\
&\quad + \psi_D \left[\lambda_D(D_i = D, \Omega_i = (A,B,C,D), s_i = c_A) - \lambda_D(D_i = D, \Omega_i = (B,C,D), s_i = c_A)\right] \\
&\quad + \rho \left[\lambda_D(D_i = D, \Omega_i = (A,B,C,D), s_i = c_A) - \lambda_D(D_i = D, \Omega_i = (B,C,D), s_i = c_A)\right] \\
\Delta_C^{c_B} &= \psi_A \left[\lambda_A(D_i = C, \Omega_i = (B,C,D), s_i = c_B) - \lambda_A(D_i = C, \Omega_i = (C,D), s_i = c_B)\right] \\
&\quad + \psi_B \left[\lambda_B(D_i = C, \Omega_i = (B,C,D), s_i = c_B) - \lambda_B(D_i = C, \Omega_i = (C,D), s_i = c_B)\right] \\
&\quad + \psi_C \left[\lambda_C(D_i = C, \Omega_i = (B,C,D), s_i = c_B) - \lambda_C(D_i = C, \Omega_i = (C,D), s_i = c_B)\right] \\
&\quad + \psi_D \left[\lambda_D(D_i = C, \Omega_i = (B,C,D), s_i = c_B) - \lambda_D(D_i = C, \Omega_i = (C,D), s_i = c_B)\right] \\
&\quad + \rho \left[\lambda_C(D_i = C, \Omega_i = (B,C,D), s_i = c_B) - \lambda_C(D_i = C, \Omega_i = (C,D), s_i = c_B)\right] \\
\Delta_D^{c_B} &= \psi_A \left[\lambda_A(D_i = D, \Omega_i = (B,C,D), s_i = c_B) - \lambda_A(D_i = D, \Omega_i = (C,D), s_i = c_B)\right] \\
&\quad + \psi_B \left[\lambda_B(D_i = D, \Omega_i = (B,C,D), s_i = c_B) - \lambda_B(D_i = D, \Omega_i = (C,D), s_i = c_B)\right] \\
&\quad + \psi_C \left[\lambda_C(D_i = D, \Omega_i = (B,C,D), s_i = c_B) - \lambda_C(D_i = D, \Omega_i = (C,D), s_i = c_B)\right] \\
&\quad + \psi_D \left[\lambda_D(D_i = D, \Omega_i = (B,C,D), s_i = c_B) - \lambda_D(D_i = D, \Omega_i = (C,D), s_i = c_B)\right] \\
&\quad + \rho \left[\lambda_D(D_i = D, \Omega_i = (B,C,D), s_i = c_B) - \lambda_C(D_i = D, \Omega_i = (C,D), s_i = c_B)\right]
\end{aligned}
$$

$$
\begin{aligned}
\Delta_D^{c_C} \quad = \quad & \psi_A \left[\lambda_A(D_i = D, \Omega_i = (C, D), s_i = c_C) - \lambda_A(D_i = D, \Omega_i = (D), s_i = c_C)\right] \\
& + \psi_B \left[\lambda_B(D_i = D, \Omega_i = (C, D), s_i = c_C) - \lambda_B(D_i = D, \Omega_i = (D), s_i = c_C)\right] \\
& + \psi_C \left[\lambda_C(D_i = D, \Omega_i = (C, D), s_i = c_C) - \lambda_C(D_i = D, \Omega_i = (D), s_i = c_C)\right] \\
& + \psi_D \left[\lambda_D(D_i = D, \Omega_i = (C, D), s_i = c_C) - \lambda_D(D_i = D, \Omega_i = (D), s_i = c_C)\right] \\
& + \rho \left[\lambda_D(D_i = D, \Omega_i = (C, D), s_i = c_C) - \lambda_C(D_i = D, \Omega_i = (D), s_i = c_C)\right]
\end{aligned}
$$

Note that now we have 6 equations and 5 unknown, and therefore the parameters $(\psi_A, \psi_B, \psi_C, \psi_D, \rho)$ are identified. Once those parameters are identified, we can plug them in in equation (14) and recover $Y_j(s_i)$.

G Simulation procedure

In this Appendix we describe the procedure used to simulate the counterfactuals. Let M denote the number of Monte Carlo simulations. The m^{th} simulations works as follows:

1. Simulate a vector of unobserved tastes $\eta_{ij}^m \sim EVT1$
2. Compute preferences $\hat{\succ}_i^m$ reflecting indirect utilities $\hat{u}_{ij}^m$ using preferences estimates $(\hat{\delta}_j^t, \hat{\gamma}_j^t, \hat{\kappa}^t)$ 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 allocation rules.
5. Calculate the cutoff scores $c_{jt}^m(\hat{\mu}^m)$ that are consistent with the equilibrium
6. Use Equation (9) to compute the predicted potential outcome as $\hat{Y}_{ij} = \hat{\alpha}_j + X_{ij}'\hat{\beta}_j + \sum_{k=1}^J \hat{\psi}_k \cdot (\hat{\eta}_{ik}^m - \mu_\eta) + \hat{\rho} \cdot (\hat{\eta}_{ij}^m - \mu_\eta)$
7. Compute the expect potential outcome for the corresponding matching function $\hat{Y}_i(\hat{\mu}^m) = \sum_j \mathbb{1}\{\hat{\mu}^m(\theta_i) = j\} \cdot \hat{Y}_{ij}$

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

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

H Additional balance checks

H.1 Regression discontinuity estimates

We use the variation in the data to estimate a regression discontinuity model. Students applying for a seat of type q in preferred degree j, receive and offer if their priority score, s_{ij}, is higher than the admission cutoff c_{qj}. Let $D_i = j$ denote such offer, where $D_i = 0$ indicates that the student did not receive any offer. We estimate the following model:

$$Y_i \quad = \quad \alpha^t + X'_{ij}\beta^t + \tau \cdot \mathbb{1}\{D_{ij} \neq 0\} + f^t(w_i) + \lambda^t_{jq} + \upsilon_i, \tag{14}$$

where Y_i is our outcome of interest, $f(w_i)$ is a flexible function controlling for the position in the waitlist relative to the last admitted student, λ_{jq} is a wailitst-specific fixed effect (i.e. interaction of degree program j and admission track q), and υ is an unobservable term. Coefficient τ is our parameter of interest, and measures the effect of receiving an offer from the preferred degree. We cluster the standard errors at the waitlist level. Our baseline specification uses a linear functional form for $f(w_i)$ allowing for different slopes at each side of the threshold. We include observations within a bandwidth of 6 places above or below the last admit.[5] Finally, to reduce residual variance we also add controls for student test scores, gender, cohort and age at application, which are predetermined and contained in X_{ij}. In Appendix Table H.1 we report results from several specification checks, all of which support our main findings.

Table H.1: Regression discontinuity estimates, robustness checks

complete

Notes: Complete.

H.2 Grader Assignment

In columns (1)-(3) of Table H.2 we present the OLS first-stage relationship between the average score shifter and test scores across all subjects. The regression coefficient is of 0.721 when using the full sample of ENEM takers. Although our sample is sufficiently large, this coefficient is different than 1 because of the introduction of the third grader when there is disagreement among the two initially assigned graders. In column (2), we show the regression estimates when we restrict the sample to individuals participating in SISU 2016, our relevant sample. The coefficient remains very similar to that of the full sample, and drops slightly in magnitude to 0.692.

Given that the allocation of graders is random, our score shifter measure should be uncorrelated with student's performance in the other components of the test and any other important confounders. In column (3) we show that the first stage coefficient is remarkably stable to adding

[5] Note that since our running variable is discrete in nature, we do not follow the existing literature in calculating the optimal bandwidth, as those methods are developed for assignment variables with density $g(x)$, where $g(.)$ is continuous and bounded away from zero (Calonico et al., 2014).

a rich host of student level controls.[6] Consistent with a satisfactory randomized assignment, this specification suggests no correlation between the score shifter z_i and any relevant student characteristics. In columns (4)-(6) we maintain the same set of specifications, but change the dependent variable to the average in score across the other four multiple choice tests. We find a precise estimate of 0 when we use the full sample of ENEM takers. When we restrict the sample to SISU applicants we find a slight imbalance, but almost negligible in magnitude. In column (6) we see that the coefficient stays stable after including several control variables suggesting the minor imbalance is not correlated with any relevant student characteristic.

Table H.2: First Stage Regressions

Dependent Variable	Essay			Multiple Choice		
	(1)	(2)	(3)	(4)	(5)	(6)
Score shifter (z_i)	0.721	0.693	0.696	0.001	-0.012	-0.010
	(0.002)	(0.003)	(0.003)	(0.001)	(0.002)	(0.001)
Sample	All	SISU	SISU	All	SISU	SISU
Controls	No	No	Yes	No	No	Yes
Mean	542.9	576.9	576.9	503.9	520.7	520.7
Observations	4,742,468	2,704,388	2,704,367	4,742,468	2,704,388	2,704,367
R-squared	0.023	0.021	0.170	0.000	0.000	0.282

Notes: The table presents the coefficients from OLS of ENEM scores on the score shifter z_i. Columns (1)-(3) use the test in the essay as main dependent variable, and columns (4)-(6) use the average score in the other four multiple choice components of the test. Specifications (1) and (4) use the full sample of ENEM takers. All other specifications use a restricted sample of ENEM takers who apply to the centralized system SISU. Columns (3) and (6) 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.

H.3 Assignment of examination booklets

In this section, however, we provide a supplementary analysis to assess the credibility of the randomness of the assignment. To ensure that books are effectively randomized we perform a balance test for our set covariates. In order to reduce the dimensionality of our covariates and to avoid multi-testing issues we compute the propensity score for each examination book assignment for a given vector of covariates. Specifically, we estimate a logit regression to compute the propensity score of being assigned to booklet j in day k

$$e_{ij} = Pr(D_{ij}^k = 1|\mathbf{X}_i = \mathbf{x})$$

where D_{ij}^k are the the assignment dummies described above and $\mathbf{X}$ is a comprehensive set of covariates. Control variables include demographics (age, gender, race, nationality, parents education and income), high-school fixed effects, and 50 answers from an individual questionnaire taken from ENEM that characterize student's SES and educational plans.

[6] These controls include the type of high-school attended, dummies for different races, the state of residency, gender, year of high school graduation, age and marital status.

In Figure **??** we show the propensity scores for each of the possible assignments in day 2, $\mathbf{B}_i^2$, in 2014. Alternatively, we could have presented the results for day 1, $\mathbf{B}_i^1$, or for the 16 combination of the two assignments, $\mathbf{Z}_i$. Also we could have chosen a different year, or have pooled them altogether. Results look similar in any of these fashions. As expected, and confirming that randomization worked well, the distributions are tightly centered in 25% and are identical for all the possible assignments.

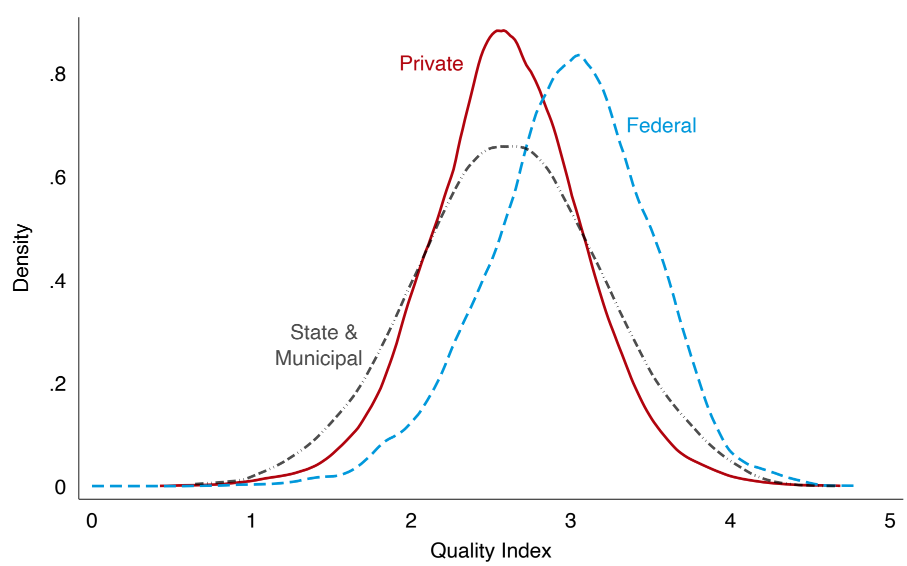
I Additional tables and figures

Table I.1: Administrative sector and type categories (as of 2016)

	Enrollment (# in 1,000)	Enrollment (share %)	Institutions	Degrees
Panel A: by sector, all students				
Federal	1,249	15.7	107	6,361
State	623	7.8	123	3,541
Municipal	47	0.6	45	264
Private	6,058	75.9	2,110	22,827
Panel B: by institution type, only federal enrollment				
University	1,084	86.7	63	5,005
Vocational	165	13.3	44	1,356

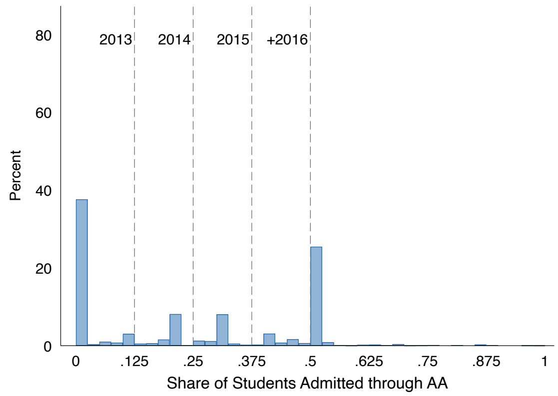
Notes: Calculations based on the Brazilian Higher Education micro data of 2016. Panel A in this table shows the breakdown of the number and share of students enrolled by sector. Panel B shows the enrollment number and share by institutions type among students enrolled in federal institutions. The total number of students is in 1,000s. Sector refers to whether the higher education institution is public (Federal, State, or Municipal administered) or private. Type refers to whether the institution is a university, or a vocational institution.

Figure I.1: Quality distribution

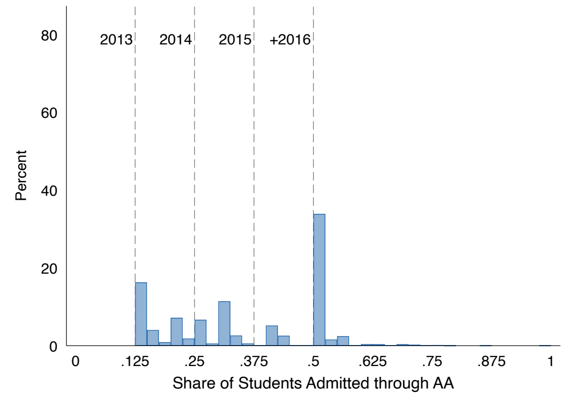


Notes: This figure shows the distribution of quality as measured by an index ranging from 1 to 5 prepared by the Ministry of Education of Brazil to evaluate degrees between years 2014 and 2016. An observation is a degree, and each observation is weighted by the number of students enrolled in the degree. Degrees from state and municipal institutions are pooled together.

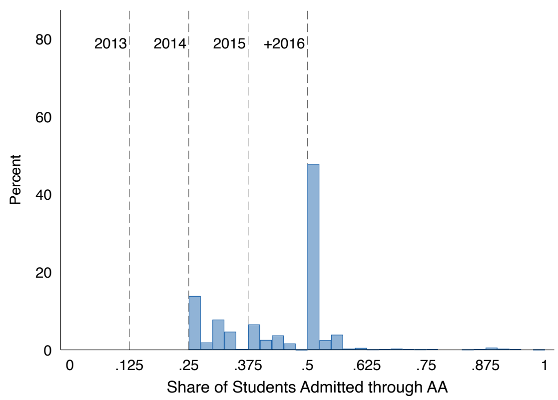
Figure I.2: Staggered Implementation of the policy



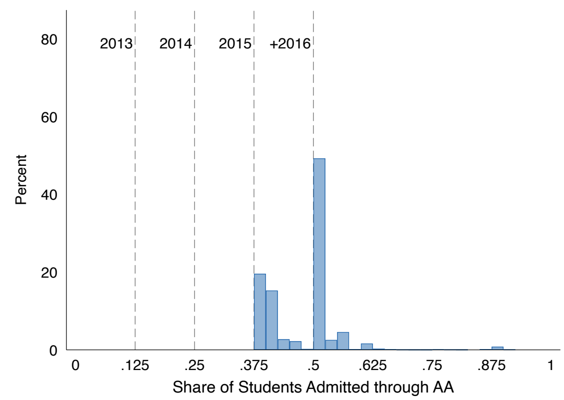
(a) 2012



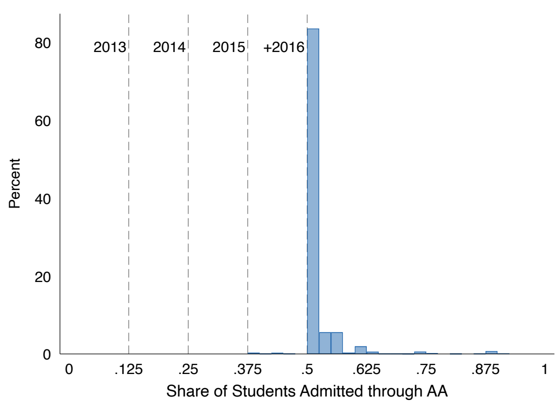
(b) 2013



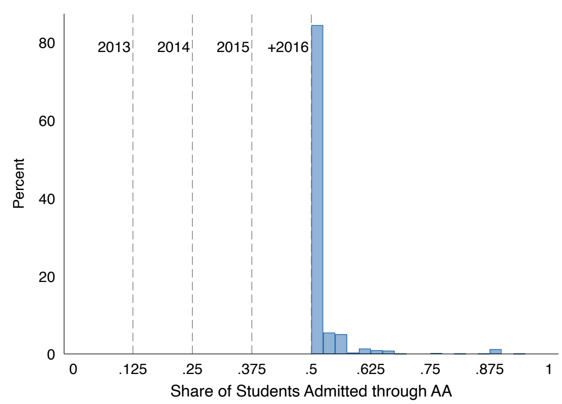
(c) 2014



(d) 2015



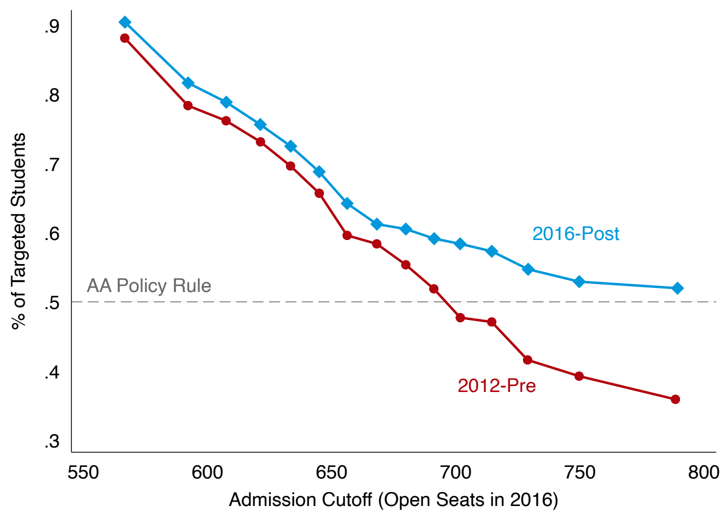
(e) 2016



(f) 2017

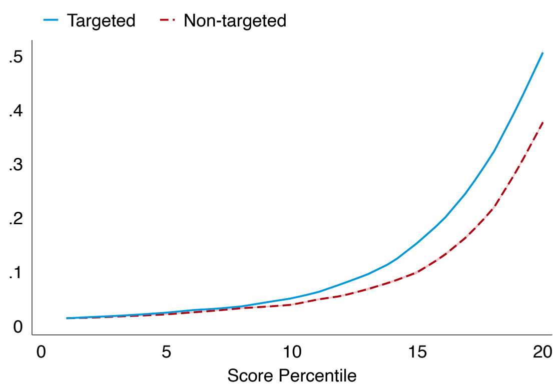
Notes: These figures describe the implementation of the affirmative action regulation. An observation is a degree program, and the y-axis measures the share of student that got admitted through the affirmative action admission track in each of the years. The dashed lines represents 12.5%, 25%, 30% and 50%. This figure only uses data from institutions participating from the SISU process.

Figure I.3: Student body composition in state institutions

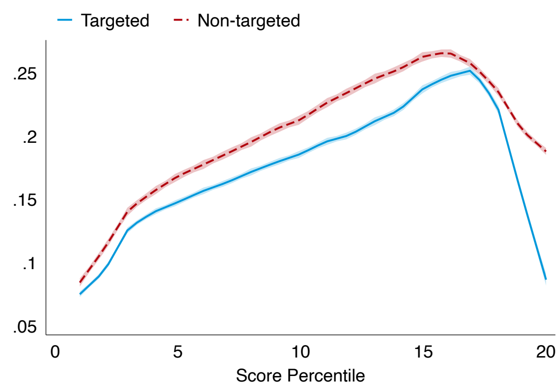


Notes: This Figure presents the average share of incoming targeted students in state institutions by degree selectivity as defined as the admission cutoff of open spots in 2016 SISU. The circles denote 2016 and the diamonds represent year 2012. We consider individuals starting a degree program in the first semester of each year. An observation is a degree and shift tuple. We weight each observation by the size of the incoming cohort. We keep degree programs that exist both in 2012 and 2016 and that participated in 2016 SISU. Our sample covers 38% of the total enrollment in state institutions in 2012 and 2016.

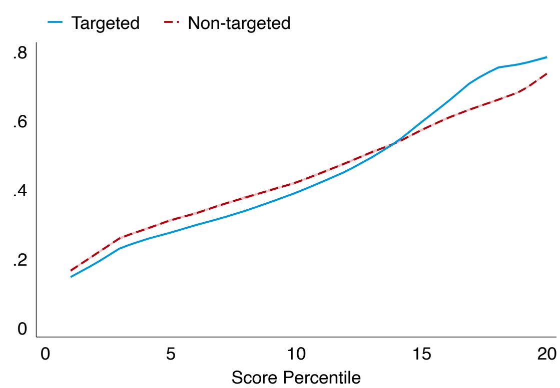
Figure I.4: College Attendance in 2016



(a) Enroll Federal



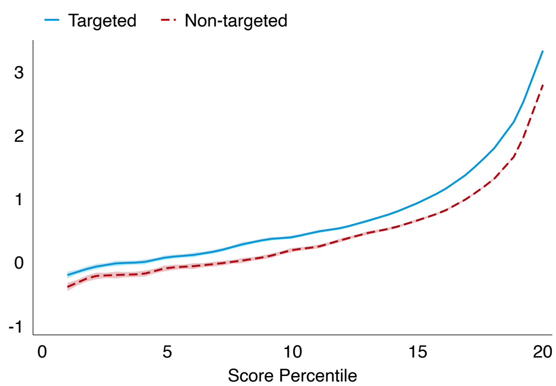
(b) Enroll Private



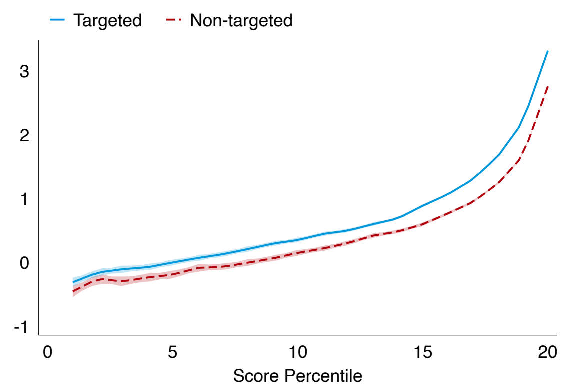
(c) Attend Any

Notes: These figures describe...

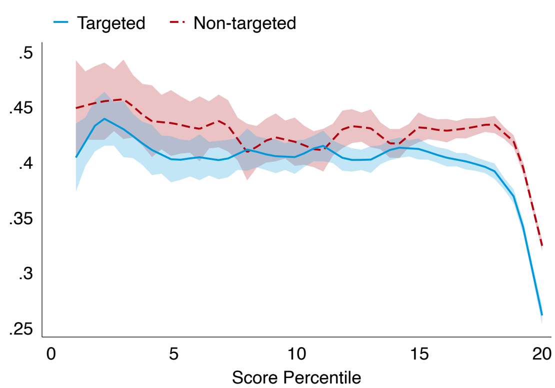
Figure I.5: College Outcomes — Enrolling Federal in 2016



(a) Quality 2016



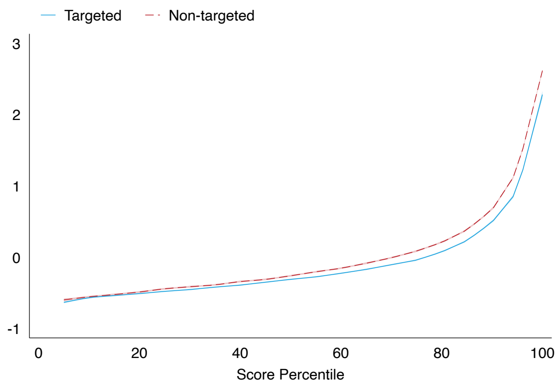
(b) Quality 2019



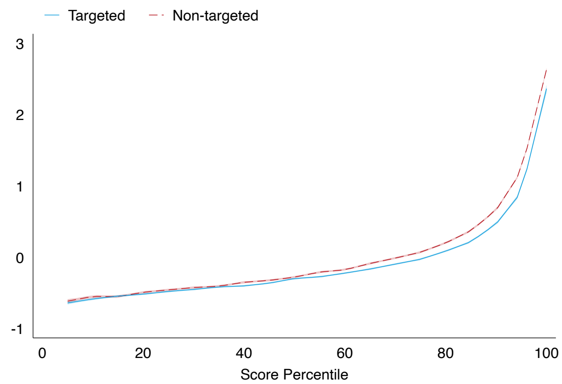
(c) 4-year Dropout

Notes: These figures describe...

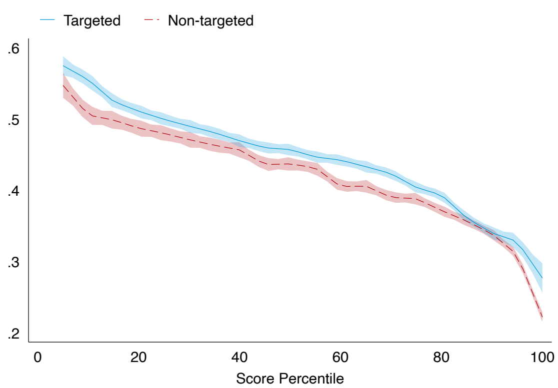
Figure I.6: College Outcomes — Enrolling Private in 2016



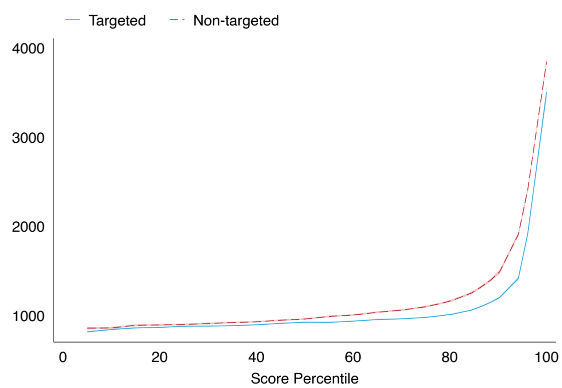
(a) Quality 2016



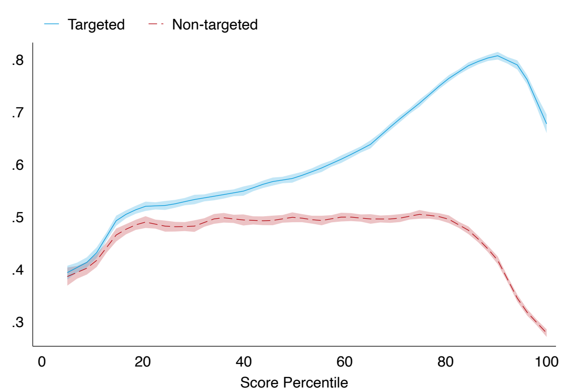
(b) Quality 2019



(c) 4-year Dropout



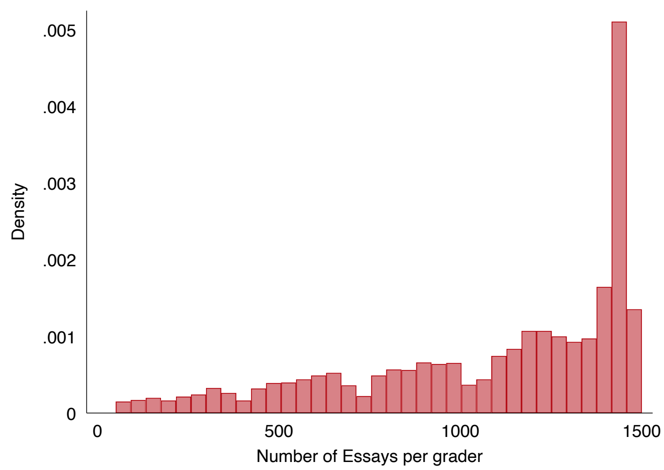
(d) Degree Price



(e) Financial Aid

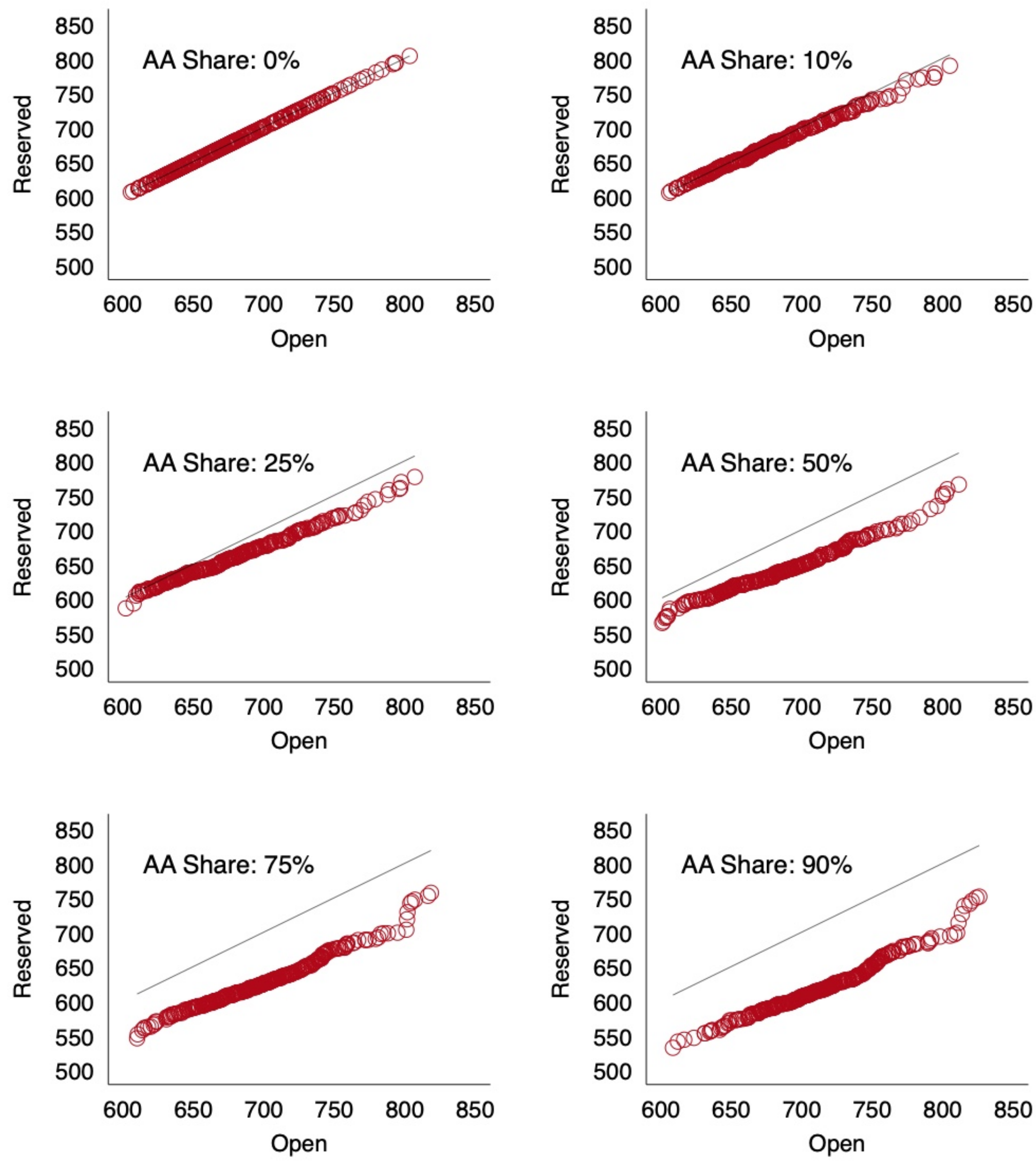
Notes: These figures describe...

Figure I.7: Number of exams per grader



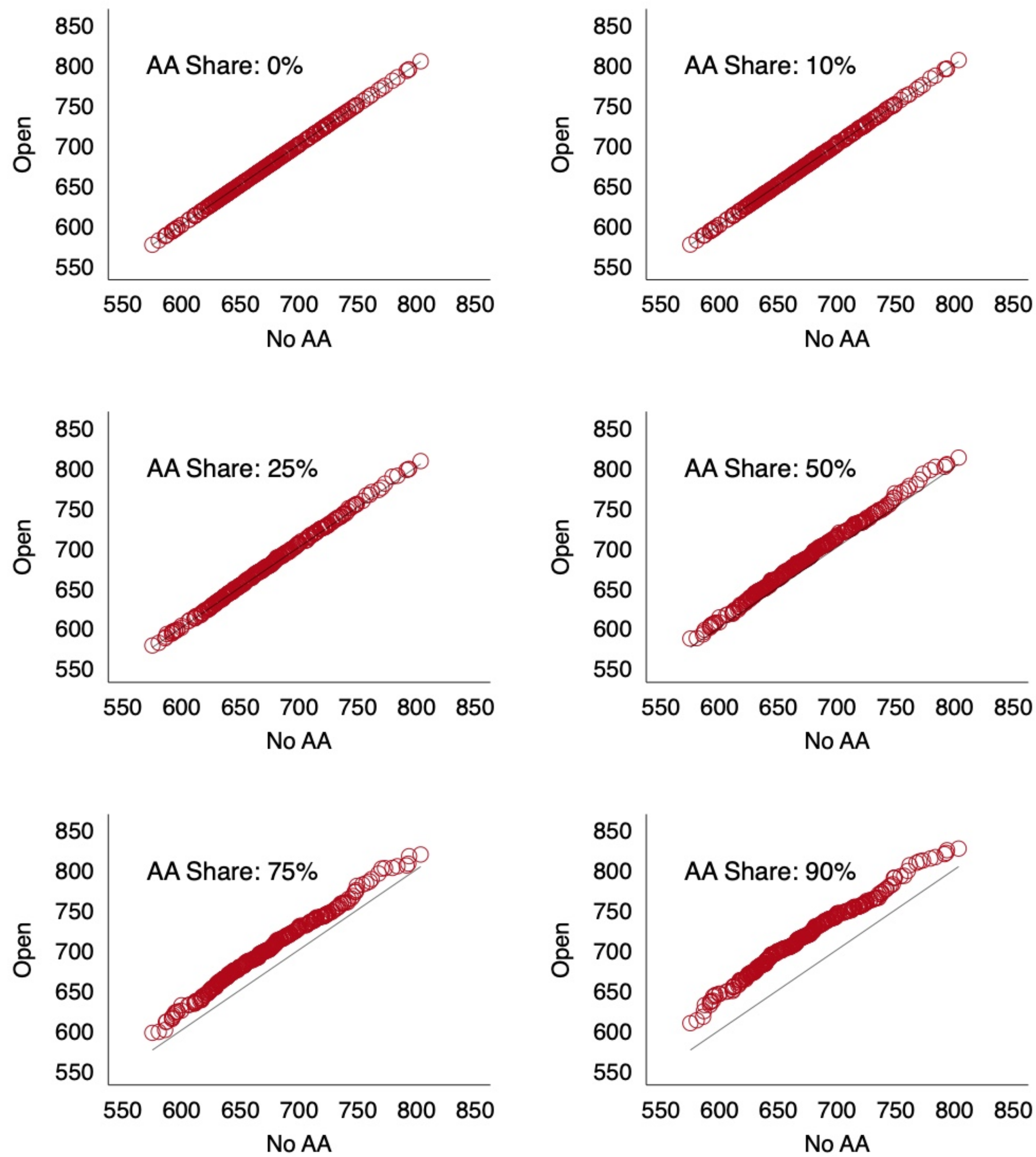
Notes: This figure shows the distribution of the number of essays assigned to each grader. We use only graders assigned to more than 50 essays. There are a total of 10,356 graders, and the average grader receives 1,067 exams to grade.

Figure I.8: Quantile-quantile admission cutoffs, Open vs Reserved



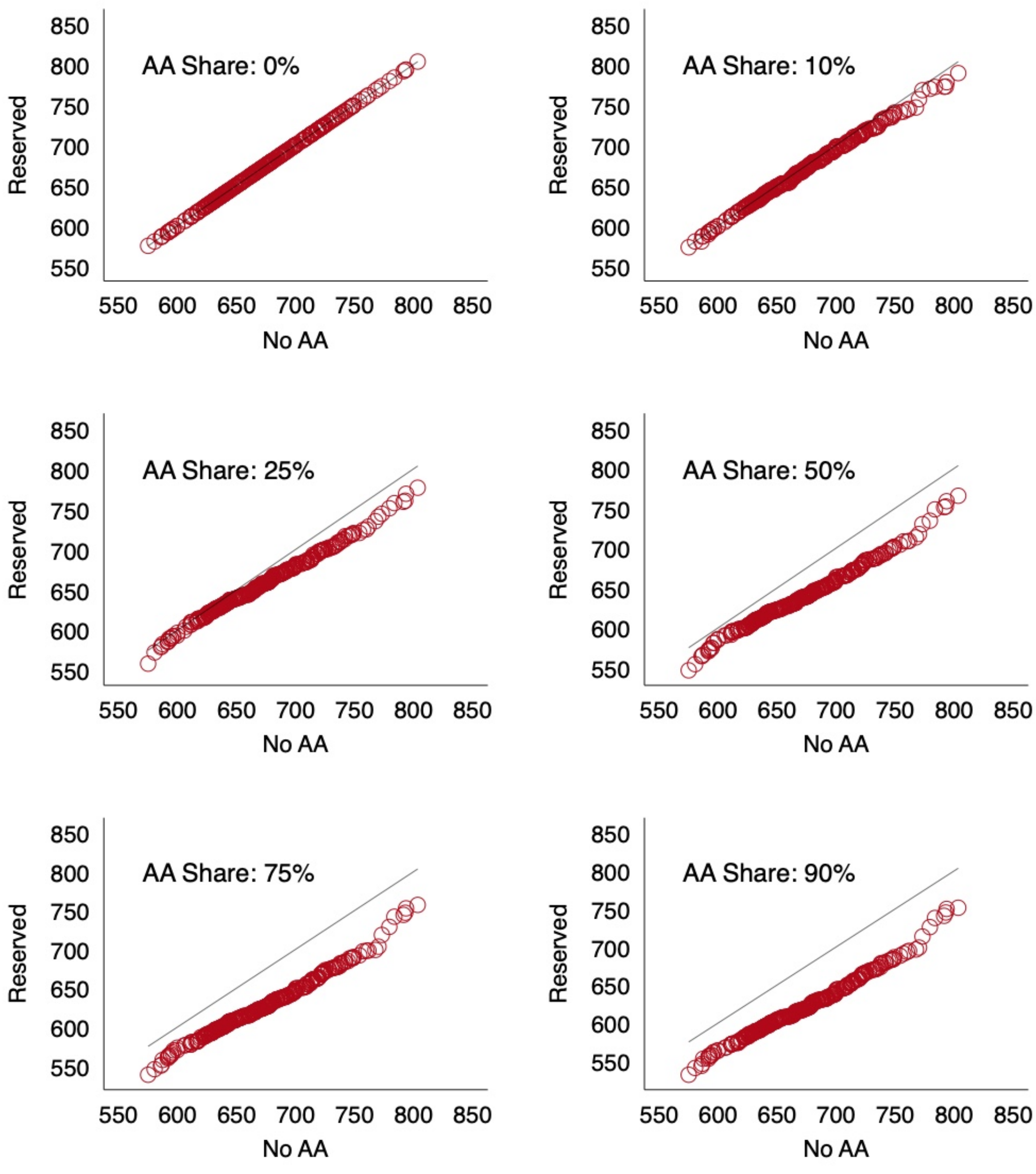
Notes: This figure shows the quantile-quantile plot of admission cutoffs of open and reserved spots, under different affirmative action schedules. The solid line is a 45 degree line. The x-axis and the y-axis represent the admission cutoff of open and reserved spots, respectively. The red circles denote the quantile associated to each of the admission cutoff value.

Figure I.9: Quantile-quantile plots, No AA vs Open



Notes: This figure shows the quantile-quantile plot of admission cutoffs of open spots. The solid line is a 45 degree line. The x-axis represent the admission cutoff of spots in the absence of the AA policy (i.e. $\omega = 0$). The y-axis represent the admission cutoff of open spots under different affirmative action schedules. The red circles denote the quantile associated to each of the admission cutoff values.

Figure I.10: Quantile-quantile plots, No AA vs Reserved



Notes: This figure shows the quantile-quantile plot of admission cutoffs of reserved spots. The solid line is a 45 degree line. The x-axis represent the admission cutoff of spots in the absence of the AA policy (i.e. $\omega = 0$). The y-axis represent the admission cutoff of reserved spots under different affirmative action schedules. The red circles denote the quantile associated to each of the admission cutoff values.

J The role of surrogates [In Progress]

The ideal metric to comprehensively study the distributional impacts of AA would be to use labor market earnings. Unfortunately, our admissions data starts in 2016 and given that degree programs take between 4-6 years to complete, it is still too soon to observe the labor market outcomes of the individuals in our sample. The fact that the outcomes of interests are observed with long delays greatly limits the ability to evaluate and improve these type of policies in higher frequency.

We address this challenge by analyzing the effects of the AA policy on intermediate proxy variables, commonly known as "statistical surrogates" (Prentice, 1989; Athey et al., 2019). The idea is that we can use an auxiliary sample for which surrogates and outcomes are observed, and estimate the effect of AA by predicting the value of the long-term outcome given the intermediate outcomes. To incorporate the surrogates into our framework, we rewrite the potential outcome equation in terms of two structural equations:

$$\begin{aligned} S_{ij} &= \pi_j + X_{ij}\psi_j + \nu_{ij} && (15) \\ Y_{ij} &= \gamma_j + \theta S_{ij} + X_{ij}\omega_j + \xi_{ij} && (16) \end{aligned}$$

where Y_{ij} is the potential outcome of interest, and S_{ij} is the surrogate. Note that by plugging in Equation (15) into Equation (16), we can recover the same potential outcome model described in Equation (8):

$$\begin{aligned} Y_{ij} &= \gamma_j + \theta\pi_j + X_{ij}(\omega_j + \theta_j\psi_j) + \theta\nu_{ij} + \xi_{ij} \\ &\equiv \alpha_j + X_{ij}\beta_j + \underbrace{\theta\nu_{ij} + \xi_{ij}}_{\varepsilon_{ij}} && (17) \end{aligned}$$

where $\alpha_j \equiv \gamma_j + \theta\pi_j$, $\beta_j \equiv \omega_j + \theta_j\psi_j$, and $\varepsilon_{ij} \equiv \theta\nu_{ij} + \xi_{ij}$. Under this framework, the unobserved component of the potential outcome ε_{ij} arises from two different sources: (i) ν_{ij} captures the unobserved term that comes from the surrogate equation, and (ii) ξ_{ij} reflects the residual term that arises after accounting for the intermediate proxy variable S_{ij}. We use Equation (17) to rewrite Equation (7) as a function of two unobserved components:

$$\begin{aligned} \mathbb{E}[Y_{ij}|X_{ij}, \eta_i] &= \alpha_j + X_{ij}\beta_j + \theta\mathbb{E}[\nu_{ij}|X_{ij}, \eta_i] + \mathbb{E}[\xi_{ij}|X_{ij}, \eta_i] \\ &= \alpha_j + X_{ij}\beta_j + \underbrace{f(\eta_i) + h(\eta_i)}_{g(\eta_i)} && (18) \end{aligned}$$

Let $\mathcal{N}$ denote our relevant sample for which we observe S_{ij}, and $\mathcal{O}$ denote the sample of previous cohorts for which we observe S_{ij} and Y_{ij}. Similar to Athey et al. (2019), we need make two assumptions that allow us to obtain the causal interpretation of the effect of degree attendance on long-term outcomes Y_{ij}. These are:

$$\begin{array}{rl} \text{A1 Surrogacy:} & \mathbb{E}[\xi_{ij}|\nu_{ij}, \eta_i, X_{ij}] = 0 \\ \text{A2 Comparability:} & \mathbb{E}[Y_i|X_i, D_i = j, S_i, i \in \mathcal{O}] = \mathbb{E}[Y_i|X_i, D_i = j, S_i, i \in \mathcal{N}] \end{array}$$

Assumption A1 is usually referred as the "surrogacy" assumption and states that the unobserved component ξ_{ij} of the potential outcome Y_{ij} is independent of degree preferences after conditioning on the surrogate. The second assumption ensures that the conditional expectation functions of potential outcomes given the degree assignment D_i and surrogates S_i are comparable. That is, the mapping from individual characteristics, degree assignment, and surrogates to potential outcomes, is fixed and invariant to the sample. This assumption rules out for instance any general equilibrium effects as a result of AA.

We use sample $\mathcal{O}$ to estimate Equation (15). Using Assumption A1 and the law of iterated expectations we show that:

$$\mathbb{E}[Y_i|X_i, D_i = j, S_{ij}] = \gamma_j + \theta S_{ij} + X_i\omega$$

We estimate θ and ω using OLS. Next, we use these parameters together with sample $\mathcal{N}$ to create a measure of expected earnings income for individuals in our sample of interest. Specifically,

$$\bar{Y}_{ij} = \hat{\gamma}_j + \hat{\theta} S_{ij} + X_i\hat{\omega}$$

Using the predicted income, $\bar{Y}_i$ for the $\mathcal{N}$ sample, we can estimate the parameters using a control function

$$\begin{aligned} \bar{Y}_{ij} &= \hat{\gamma}_j + \hat{\theta} S_{ij} + X_i\hat{\omega} && (19) \\ &= \hat{\gamma}_j + \hat{\theta}(\pi_j + X_i\psi + \nu_{ij}) + X_i\hat{\omega} && (20) \\ &= \alpha_j + X_i\beta + \hat{\theta}\nu_{ij} && (21) \\ \mathbb{E}[\bar{Y}_{ij}|X_i, \eta_i] &= \alpha_j + X_i\beta + \theta\mathbb{E}[\nu_{ij}|X_i, \eta_i] && (22) \\ &= \alpha_j + X_i\beta + f_j(\eta_i) && (23) \\ \mathbb{E}[\bar{Y}_i|X_i, z_i, D_i = j] &= \alpha_j + X_i\beta + \mathbb{E}[f_j(\eta_i)|X_i, z_i] && (24) \end{aligned}$$

Relative to when income is available, we need to assume that $\mathbb{E}[h_j(\eta_i)|X_i, z_i] = 0$ (implied by A2)