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On the road to social mobility? Affirmative action and major choice ☆

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ABSTRACT

Students from disadvantaged backgrounds remain underrepresented in prestigious and high-paying fields of study, such as STEM. While affirmative action (AA) policies have been shown to increase the representation of minority students in selective universities, they may also affect students' choice of majors, with potential implications for social mobility. We study a policy implemented by a highly selective Brazilian university that expanded the range of majors accessible to lower-SES applicants, using it as a natural experiment. The policy led targeted students to apply to and enroll in more prestigious, higher-paying STEM majors and attenuated the influence of socioeconomic background on major choices. Our findings suggest that in contexts where applicants select their majors before university entry and these choices are influenced by strategic considerations, AA policies can be particularly effective in promoting social mobility.

1. Introduction

There is substantial heterogeneity in labor market returns across post-secondary fields of study.[1] Recent research further indicates that the chosen field of study tends to have a greater impact on future earnings than institutional quality (e.g., Altonji et al., 2016; Kirkeboen et al., 2016). Additionally, low-income students are typically underrepresented in lucrative and prestigious fields, notably STEM majors.[2] The considerable variation in major returns and the strong relationship between parental socioeconomic status and major selection likely contribute to limited intergenerational social mobility. Nevertheless, the extent to which public policies can influence disadvantaged students' choice of majors remains uncertain.

In this paper, we investigate whether an affirmative action (AA) policy at a selective Brazilian university, *Universidade Estadual de Campinas* (UNICAMP), increased the representation of students from disadvantaged socioeconomic backgrounds in prestigious university majors, even though the policy was not explicitly designed to influence major choice. The policy granted bonus points in the admission exam to public high school students, a group that typically has low socioeconomic status (SES) and is underrepresented in public universities. This AA policy thereby expanded their major choice sets without altering applicants' perception of their ability.

Our previous work (Estevan et al., 2019b) provides a background for this study. We found that UNICAMP's AA policy increased low SES applicants' admission probabilities without significantly changing the

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[1] For instance, in the U.S., it is frequently documented that returns to engineering majors substantially exceed those associated with education (e.g., Altonji et al., 2012, 2016; Oreopoulos and Petronijevic, 2013). Similar evidence is found internationally: see Estevan et al. (2024) for Brazil, Hastings et al. (2013) for Chile, and Kirkeboen et al. (2016) for Norway.

[2] This issue has garnered considerable attention in both the academic literature and popular media worldwide. In the UK, approximately 80% of medical students have parents employed in higher managerial or professional occupations (Carrell, 2016), and applicants from lower socioeconomic status (SES) occupational groups represent only 2.3% to 8.4%, depending on the medical school (Steven et al., 2016). Similar discussions persist in the U.S., highlighting the underrepresentation of low-income and minority students in STEM fields (Camera, 2017; NSF, 2019). In Brazil, the fraction of public school graduates – a proxy for low SES students – admitted into highly competitive majors such as Law and STEM in flagship state universities was near zero in the early 2000s (Cavalcanti et al., 2010).

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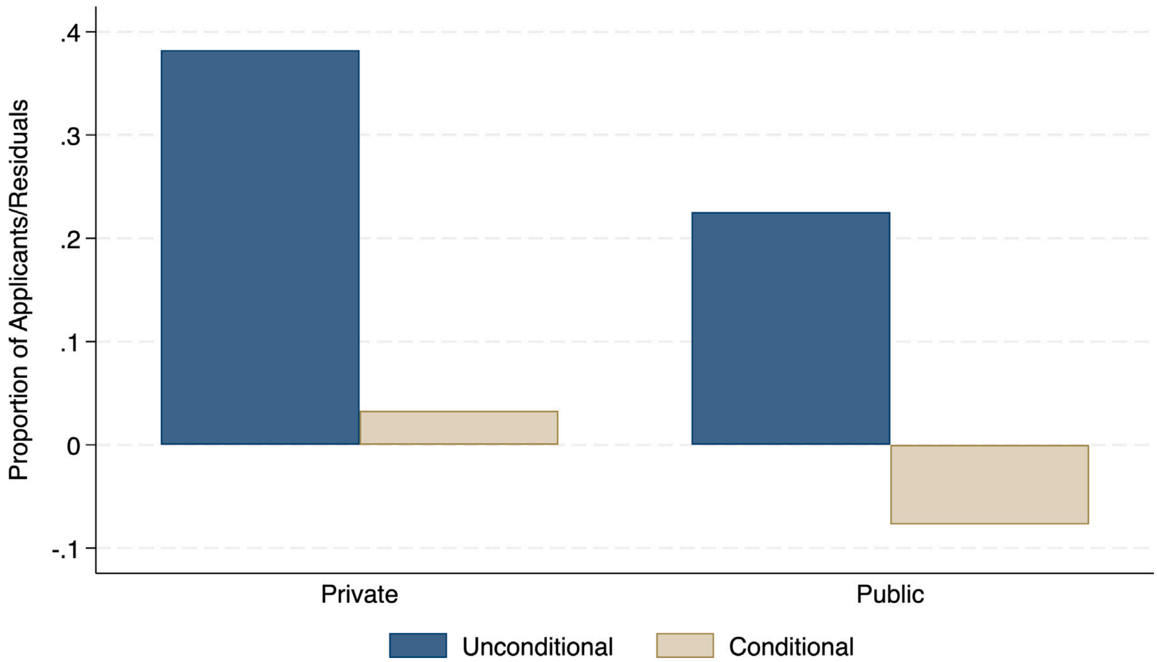


Fig. 1. Proportion of applicants choosing a top five major.
Notes: The figure shows the proportion of UNICAMP's 2004 applicants who chose one of the top five most competitive UNICAMP majors based on the 2003 P_2 cutoffs (i.e., Medicine, Computer Engineering-daytime, Control and Automation Engineering-evening, Electrical Engineering-daytime, Electrical Engineering-evening), by the type of high school the applicants attended (public or private). 'Unconditional' refers to the actual values observed, while 'Conditional' refers to the residuals of an OLS regression of application to a top five major on individuals' ENEM scores.

applicants' pool or their exam preparation in 2005. However, beyond the initial year of implementation, the policy may have more substantial, enduring impacts by influencing major choices, considering the diverse returns to majors. Indeed, one's major choice seems an essential driver of social mobility beyond academic achievement.

As in the majority of OECD countries (Kirkeboen et al., 2016), Brazilian universities have "college-major-specific admission rules" (Bordon and Fu, 2015). Applicants choose a major when they register to take UNICAMP's entrance exam and receive information on the previous year's exam, which gives useful predictors of the current year's minimum exam score necessary to be admitted (hereafter, cutoff scores). As shown in Fig. 1, public high school students are underrepresented in selective majors compared to their private counterparts, even conditional on their national end-of-high-school exam, *Exame Nacional do Ensino Médio* (ENEM), scores. UNICAMP's cutoff scores differ substantially based on the chosen major and are notably higher for well-paying jobs, as illustrated in Panel (a) of Fig. 2. Although some of this correlation may be due to differing ability levels among individuals, there appears to be a positive association between major cutoffs and salary. In addition, individuals selecting majors with relatively larger cutoff scores have, on average, higher parental income, as shown in Panel (b) of Fig. 2.[3]

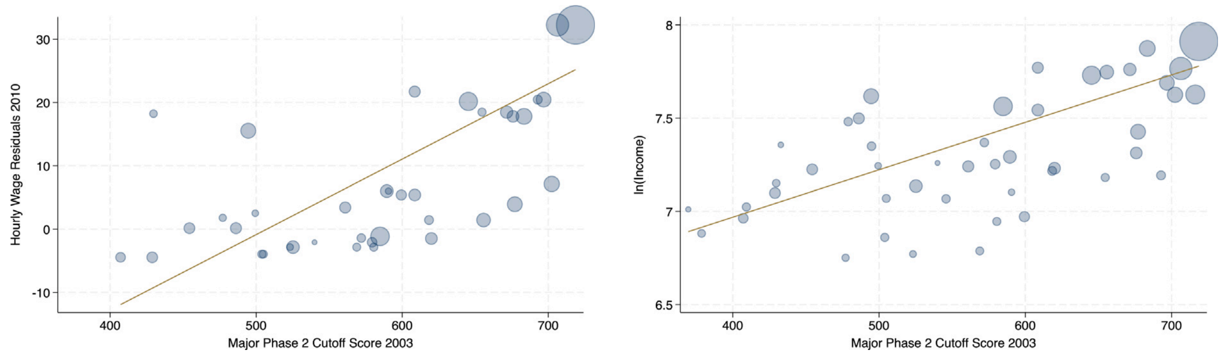
These patterns could simply reflect the intergenerational correlation of occupational preferences (e.g., van de Werfhorst and Luijkx, 2010). However, they could also stem from factual constraints in the major choice of applicants since UNICAMP, like most Brazilian universities at that time, used a version of the well-known Boston mechanism (Carvalho et al., 2019).[4] This allocation algorithm strongly incentivizes applicants to be strategic and list as their first choice their most preferred major *among those they believe they are likely to be accepted in*. This incentive automatically generates some correlation between majors applied to and expected entrance exam results, which in turn correlate with the educational (and other) resources applicants had access to, i.e., their socioeconomic status.

A bonus-point policy allows for a relatively straightforward theoretical identification of the policy's effects on application behavior. We use a simple educational choice model in Section 3 for illustration: students are characterized by their end-of-high-school grade and type of high school attended, which are sufficient statistics for the expected entrance exam score. Some recipients of the bonus will apply for more competitive majors if their preferred major was not in their choice set initially, akin to consuming more expensive goods after a budget increase in consumer choice. This behavioral shift leads to a general increase in cutoffs, not unlike price inflation. This general equilibrium effect may induce private high school students to select majors that require a lower admission score. In sum, we expect public high school students to become more likely to apply for selective majors than private high school students. The model also predicts that the effects should arise at the top of the ability distribution.

Using a difference-in-difference approach, we find that targeted applicants were more likely to choose and gain admission to selective majors relative to non-beneficiaries following the AA policy implementation. In line with our theoretical predictions, the impacts on major selection are especially pronounced among students at the top of the ENEM score distribution. We show that the policy reduces the socioeconomic gradient in major choice (i.e., the reciprocal of the

[3] Relatedly, Campbell et al. (2022) and Murphy and Silva (2024) show that disadvantaged students in the UK and Portugal choose lower-quality degrees across the achievement distribution. Similarly, Bleemer and Mehta (2024) find that higher grade requirements in popular U.S. majors push underprivileged students toward less lucrative fields.

[4] Boston-type mechanisms used to be common in school choice (Abdulkadiroglu and Sonmez, 2003) and also in university admission, for instance in China (Chen and Kesten, 2017). Moreover, similar incentives also occur when applicants face constraints on preference lists, e.g., they can apply to a single program choice, as in Japan and South Korea (Avery et al., 2014; Che and Koh, 2016), and in direct-entry tracks for competitive majors in the U.S. and Canada (Davies and Hammack, 2005).



(a) Wages

(b) Parental income

Fig. 2. Correlation between admission exam cutoffs, wages, and parental income.
Notes: Each circle corresponds to a UNICAMP major. A major's circle size is proportional to the number of applicants who listed that major as their first choice. In the two panels, the variable on the horizontal axis represents the major's P_2 cutoff in 2003. We present cutoffs for each major in Online Appendix Table O.1, which we calculate as the lowest P_2 (i.e., final) score of an applicant admitted to the major who selected it as a first choice in 2003. We use wage data from the Brazilian Population Census (IBGE, 2010), which includes information on the major completed by individuals. In Panel (a), the variable on the vertical axis is the residual of the major-specific hourly wage, derived from a regression of wages by major completed from the 2010 Brazilian Population Census on a quartic polynomial of age. In Panel (b), the variable on the vertical axis represents the logarithmic transformation of the parental income from candidates who took the 2004 UNICAMP admission exam.

slope parameter found in Panel (b) of Fig. 2) by close to one-fifth, contributing to greater equity in access to prestigious fields of study.

We contribute to a growing literature on how socioeconomic background affects major choice and whether affirmative action policies could alter its impact.[5] Because students in most U.S. universities choose their majors after admission, the U.S. literature has primarily focused on whether affirmative action policies might induce underrepresented students to select less demanding or less prestigious fields once admitted to more selective institutions. The evidence on this question is mixed. Arcidiacono et al. (2016) finds that affirmative action can reduce minority representation in STEM degrees, whereas Bleemer (2024) reports little to no effect of such policies on major choice. Our paper shows that when strategic incentives shape applicants' major choices, affirmative action (AA) can increase the enrollment of disadvantaged students in high-paying fields, beyond the purely mechanical effect of raising their admission probabilities to more selective programs. In contexts where students apply directly to specific majors (as is common outside the U.S.), AA policies may thus have stronger effects on the allocation of students across fields of study than in systems where majors are chosen after university entry. Taken together, these results suggest that AA policies are likely to be more effective at promoting social mobility in environments where they offset strategic distortions in major choice. This is particularly relevant in Latin America, where evidence indicates that disadvantaged students experience especially high returns to enrolling in STEM programs, despite lower completion rates (Ng and Riehl, 2024).

Differing from the U.S. setting, our study analyzes a context where applicants must declare their intended majors prior to learning their admission outcomes, thus allowing us to isolate a more direct influence of AA policies on students' initial major selection decisions. Building on our work (Estevan et al., 2019a), Melo (2025) shows that an AA policy at the *Universidade Federal do Espírito Santo* (UFES), which allocated quotas at the major level, also increased the application and enrollment of eligible applicants into more selective majors. However, causal identification of quota-based policy effects on behavior is comparatively challenging, as computing individual optimal behavior requires very complex reasoning (it is, e.g., difficult to assess whether one would have gotten in last year because of the quota, as it requires knowledge about the full ranking of applicants, not merely the cutoff), unless one wishes to invoke strong assumptions on individuals' rationality and common knowledge.

A related literature shows that positive exam signal shocks can affect college application portfolios, major choice, and college choice (e.g., Papay et al., 2016; Avery et al., 2018; Bond et al., 2018). However, unlike these settings, UNICAMP's bonus points do not provide new information about the applicant's ability and, therefore, are not expected to influence applicants' beliefs about their likelihood of succeeding in a program. Nevertheless, we observe significant effects of the policy. Similar to Bond et al. (2018), our findings reveal that students with higher abilities respond more to the affirmative action policy.

We organize the paper as follows. Section 2 describes UNICAMP's admission system and the AA policy. We provide some theoretical pointers to explain the potential impact of the affirmative action policy on major choice in Section 3. We present our data and identification strategy in Sections 4 and 5. We show the main results, explore some heterogeneous effects, and present robustness checks in Section 6. In Section 7, we present event study results. Finally, we present our findings on the effect of the AA policy on the parental-income gradient when it comes to major choice in Section 8, and conclude in Section 9.

2. UNICAMP's admission and affirmative action policy

To select its students, UNICAMP organizes an annual admission exam to allocate around 2,950 seats in nearly 60 majors.[6] Every year, around 50,000 applicants register in September for UNICAMP's admission exam, which consists of Phase 1 (P_1) and, for those who

[5] Patnaik et al. (2021) reviews the literature analyzing other factors determining the choice of field of study. Individuals sort themselves based on comparative advantage (Kirkeboen et al., 2016), expected earnings (Arcidiacono et al., 2012; Montmarquette et al., 2002; Boudarbat and Montmarquette, 2009; Wiswall and Zafar, 2015; Baker et al., 2018; Abramitzky et al., 2024), nonpecuniary factors (Beffy et al., 2012), and their (mis)perceptions about their ability to do well in a given program (Zafar, 2011; Stinebrickner and Stinebrickner, 2014). There are also some recent studies investigating whether peers (Anelli and Peri, 2015), role models (DellaVigna, 2010; Ferrando and Gille, 2025), or exposure to major (Fricke et al., 2018) can alter the choice of major.

[6] Each major is defined by subject and schedule (day or evening). Ten are offered in both schedules and 40 in only one. We treat each subject-schedule as a separate major, since cutoffs are schedule-specific.

pass P_1, Phase 2 (P_2). Both P_1 and P_2 cover high school subjects, such as chemistry, mathematics, and Portuguese, among others. When registering for the admission exam, applicants can authorize UNICAMP to obtain their ENEM scores from the Ministry of Education—a choice made by nearly 90% of applicants. UNICAMP then calculates each applicant's P_1 score both with and without the ENEM (weighted at 20%) and uses the higher value, giving applicants a clear incentive to authorize its use, since it can only improve their P_1 score.

Upon admission exam registration, applicants must select up to three majors, which they rank first, second, and third. The choice of the major is crucial for many reasons. First, students gain admission to a specific major, not just the university. Once accepted, it is difficult for a student to change major, and it typically involves retaking the admission exam. Second, the applicant's major choice determines the pool of competitors and, consequently, the minimum P_1 score required for advancing to P_2 and the P_2 cutoff scores for being admitted. More specifically, the P_1 cutoff grade is initially set at 50%. However, it is adjusted upward or downward to ensure between three and eight applicants in P_2 per major slot.

Most majors are quite competitive, as UNICAMP is a prestigious, tuition-free university. Each year, P_1 eliminates about 70 percent of applicants, and about 10 percent gain admission to UNICAMP. Still, there are stark differences in admission rates (and cutoff scores) across majors (see Online Appendix Table O.1). Finally, UNICAMP's admission process uses a version of the well-known Boston mechanism (Abdulkadiroglu and Sonmez, 2003) to assign students to majors. Under this assignment mechanism, students have an incentive to apply to majors they are likely to get into. For instance, a student who aspires to study medicine might choose nursing, requiring a lower admission score, to secure a place in university.

Applicants who choose a given major and fulfill minimal grade requirements are ranked based on their admission exam score (NPO, for *nota padronizada de opção* in Portuguese), which is a weighted average of P_1 and P_2 scores.[7] Importantly, the NPO ranking initially considers only applicants who choose a given major as their first choice.[8] Between 2001 and 2008, nearly two-thirds of majors were filled in the first round of offers, meaning only first-choice applicants were considered for admission. Only 8 of the 60 majors had vacancies after the first round in every admission cycle during this period. In majors with seats available after the first round, second- or third-choice applicants who meet the minimum grade requirements receive offers based on their NPO ranking. If vacancies remain, grade thresholds are lowered, though first-choice applicants retain priority for admission. Indeed, 85 percent of admitted applicants obtained their first choice in 2001–2008. Thus, selecting a very competitive major as the first choice, such as medicine, may prevent an applicant from being admitted to nursing even if her NPO score was way above the cutoff for nursing.

In 2004, UNICAMP implemented an affirmative action program granting a 30-point bonus on the NPO to applicants who completed their entire high school in a public school (and passed P_1). The bonus was sizeable, around 30% of a standard deviation. Public high school graduates who also self-identified as visible minorities (Black, mulatto, or Indigenous) received an additional 10 points, for a total bonus of 40 points. Visible minorities from private high schools were not eligible. We do not focus on the racial dimension of the policy for two reasons. First, visible minority applicants comprised a small share of the sample (about 13% overall and only 9% among 2004 private school applicants), making precise estimation of race-specific effects a challenge. Second, because race is self-reported, the policy may have created incentives for some students to change their reported race, rendering it potentially endogenous and thus a lousy control variable.[9]

We analyze the impact of the affirmative action policy on students' major choices using data from admission exams between 2001 and 2008, excluding the year 2005. Although the policy was announced a few months before the registration for the 2005 UNICAMP admission exam, most applicants learned about it only during registration (September 2004), at which point they simultaneously selected their majors. Therefore, it is unlikely that the affirmative action policy influenced major selection in 2005. Nevertheless, we conduct event-study analyses in Section 7 to examine the policy's impact year-by-year, including 2005.

3. An education choice model

Before specifying our empirical approach, we illustrate how a bonus policy, such as the one implemented by UNICAMP, could theoretically influence applicants' major choices. The model also highlights potential heterogeneity in the policy's impact across the academic ability distribution, which we later examine empirically.

The model follows the spirit of the literature on modeling major choice based on updating potentially biased expectations (e.g., Arcidiacono et al., 2012; Stinebrickner and Stinebrickner, 2014; Wiswall and Zafar, 2015; Bond et al., 2018; Kapor et al., 2020), but taking into account that the affirmative action policy generates a deterministic point bonus on the exam score, instead of a noisy signal on ability.

The economy is populated by a continuum I of students. Individuals are characterized by their innate academic ability θ_i and have disutility from exerting effort e_i at a strictly convex utility cost $c_i(e_i)$. When young, they (or possibly their parents) choose a high school s_i from a discrete set S (we abstract from high-school capacity constraints) and an effort level e_i. Ability, effort, and the school determine individual academic human capital at the end of high school, given by function $h(\theta_i, e_i, s_i)$, which strictly increases in θ_i and e_i. Academic human capital captures an individual's skill at sitting exams. An individual's end-of-high-school score g_i is the realization of a random variable $\tilde{g}_i$ whose mean is determined by the academic human capital:

$$\tilde{g}_i = h(\theta_i, e_i, s_i) + \epsilon_i, \tag{1}$$

where ϵ_i is a random variable with mean zero, distributed according to a distribution function F_ϵ. This assumption implies that the realized end-of-high-school score g_i is an unbiased estimator of individual academic human capital $h(\theta_i, e_i, s_i)$, i.e. a signal of academic ability or of the ability to obtain good marks. This academic ability signal is likely an aggregate of true ability, past effort, and educational investment.

Suppose a student chooses from a discrete set M of university majors. Denote the major choice of student i by $m_i \in M$.[10] A student

[7] P_2 covers eight high school subjects. Applicants are automatically eliminated if they score zero in any subject and must also meet minimum grade requirements in priority subjects, which vary by major. See Estevan et al. (2019b) for further details on UNICAMP's admission process.

[8] There are four groups of majors for which first and second options are considered simultaneously: Electrical Engineering (day and evening), Chemical Engineering (day and evening), Medicine, and Nursing.

[9] As robustness, we interact race with our key regressors, restricting the analysis to 2003–2008 (when race data are available). Online Appendix Tables O.2 and O.3 show negligible effects on our main estimates, with race-related coefficients generally small and insignificant.

[10] The model assumes that all students choose exactly one major. This is because, in the allocation mechanism used by UNICAMP, the first choice matters disproportionately (see Section 2). For models allowing for choices of a portfolio of educational options, see, e.g., Epple et al. (2006), Chade et al. (2014), and Fu (2014). We also explicitly omit outside options, i.e., other universities, because this would not qualitatively change our results as long as outside options are not affected by the policy, which seems plausible in our setting. This is because the outside option can be modeled as majors m_j, unaffected by affirmative action bonuses, and whose admission thresholds do not change due to the policy. Even if most UNICAMP applicants apply to more than one university before and after the policy change, other comparable universities did not change their admission policy at the same time as UNICAMP.

i derives utility $u_i(m)$ from enrolling in a major $m \in M$. Suppose that $u_i(m) > 0$ for some m for all students and that application is costless. Different students may have different preferences over majors, so that $u_i(m) > u_i(m')$ does not imply that $u_j(m) > u_j(m')$ for $i \neq j$ and $m \neq m'$, but suppose that each individual's ranking is strict, i.e. $u_i(m) = u_i(m')$ if and only if $m = m'$. Given θ_i, s_i, e_i and the realized scores g_i and g_j for all other $j \neq i \in I$, an individual i chooses a major $m_i \in M$ to apply to. In this formulation, individual preferences over majors do not depend on effort e_i or high school s_i.

An exam governs university admission. The exam result $\tilde{t}_i$ is a random variable whose mean depends on academic human capital $h(.)$, as well as the realized end-of-high-school score g_i (since our studied admission exam uses ENEM as a potential input):

$$\tilde{t}_i = \alpha h(\theta_i, e_i, s_i) + (1-\alpha) g_i + v_i, \tag{2}$$

where $\alpha \in (0,1)$ and v_i is a random variable with mean 0, distributed according to a distribution function F_v.

We do not model exam preparation effort, mainly because our earlier work (Estevan et al., 2019b) suggests that students have not significantly changed their effort provision and application decision in response to the policy, at least in the short term.[11] Importantly, modeling effort choice would not qualitatively change our results with strictly convex effort cost and a continuum of agents. Still, quantities would be affected, such as admission cutoffs under the policy and the measure of applicants who effectively gain access to a major they would not have had access to without the policy.

Each major m has capacity $k(m)$, yielding an admission cutoff $\bar{t}(m)$ such that the measure of individuals who choose major m equals $k(m)$. Note that $\bar{t}(m)$ is endogenous and determined by application choices, and thus cutoffs may react to changes in admission policy if they, in turn, induce changes in application behavior, generating a general equilibrium effect. We assume that all majors are oversubscribed (which is guaranteed if the mass of applicants is large enough and every major is preferred to the outside option for a sufficient measure of students).

To summarize, the timeline is:

- Individuals are born with innate ability θ_i,
- Individuals choose school s_i, and then effort level e_i at school s_i, which yields academic human capital $h(\theta_i, e_i, s_i)$,
- The end-of-high-school final exam (ENEM) yields grade $\tilde{g}_i$, a random variable with mean $h(\theta_i, e_i, s_i)$,
- Individuals choose a major m_i and then take the university admission exam, yielding test score $\tilde{t}_i$, a random variable with mean $\alpha h(\theta_i, e_i, s_i) + (1-\alpha) g_i$.
- Given test scores $(t_i)_{i \in I}$ that yield major specific admission cutoffs $\bar{t}(m)$, an applicant i is admitted to the major applied for m_i if $t_i + B_i \geq \bar{t}(m_i)$, where B_i denotes the score bonus through affirmative action available to individual i.

Turn now to individual decisions. To choose which major to apply for, an individual solves the following:

$$\max_{m_i \in M} Prob(t_i \geq \bar{t}(m_i) - B_i) u_i(m_i),$$

where $Prob(.)$ denotes the probability of being accepted into major m_i, and B_i reflects the policy (i.e. $B_i > 0$ if individual i is entitled to a bonus and $B_i = 0$ otherwise). We use rational expectations to model expectations, which seems adequate in our context since applicants can predict the minimum scores required for each major well in advance of preparing for entrance exams based on the information published in the applicant's registration manual. With a bonus policy, rational expectations do not seem like a particularly demanding assumption, especially in contrast with quota-based policies that induce more strategic complexity, as the bonus can be simply added to expected entrance exam scores. With a continuum of applicants and a discrete set of majors, invoking a law of large numbers for each $m \in M$, it must hold that $\bar{t}(m) = \bar{h}(m)$, where:

$$\bar{h}(m): \int I(\alpha h(\theta_j, e_j, s_j) + (1-\alpha) g_j \geq \bar{h}(m) - B_j) \cdot I(m_j = m) dj = k(m), \tag{3}$$

where $I(.)$ denotes an indicator function, $k(m)$ is the capacity of major m, and the integral is with respect to the Lebesgue measure. The probability of being accepted into major m is then:

$$Prob(t_i \geq \bar{t}(m) - B_i) = Prob(v \geq \bar{h}(m) - B_i - (\alpha h(\theta_i, e_i, s_i) + (1-\alpha) g_i)).$$

Therefore an individual will prefer major m over another major m' if:

$$Prob(v_i \geq \bar{h}(m) - B_i - \alpha h(\theta_i, e_i, s_i) - (1-\alpha) g_i) u_i(m)$$
$$> Prob(v_i \geq \bar{h}(m') - B_i - \alpha h(\theta_i, e_i, s_i) - (1-\alpha) g_i) u_i(m').$$

This immediately implies two useful revealed preference properties.

Fact 1. *Given θ_i, e_i, s_i and $(t_j)_{j \in I}$, if an individual i chooses m then $u_i(m) > u_i(m')$ for all m' with $\bar{t}(m') \leq \bar{t}(m)$.*

That is, any major with a lower equilibrium cutoff than the one chosen will yield lower utility than the chosen one. Similarly, when the AA policy is active (i.e., the year is 2006), applicants who are eligible for the AA policy will choose the same major or a major with a higher equilibrium cutoff, which they must prefer but have not been able to get into.

Fact 2. *Given θ_i, e_i, s_i and $(t_j)_{j \in I}$, if an individual i chooses m when $B_i = 0$ and m' when $B_i > 0$, then $u_i(m') > u_i(m)$ and $\bar{t}(m) \leq \bar{t}(m')$.*

Here $\bar{t}(.)$ denotes the equilibrium threshold without the AA policy. Equilibrium thresholds with the AA policy, denoted by $\bar{t}^A(.)$, may differ from those without the policy. Indeed, applicants not eligible for the policy will choose the same major or a major with either a lower equilibrium cutoff under the policy or a major with a relatively lower equilibrium threshold, i.e., one that increased less in equilibrium cutoff due to the policy.

Fact 3. *Given θ_i, e_i, s_i and $(t_j)_{j \in I}$, if an individual i with $B_i = 0$ chooses m when $AA_j = 0$ for all j, but m' when $B_j > 0$ for some j, then either $u_i(m') < u_i(m)$ and $\bar{t}^A(m) > \bar{t}^A(m')$, or $u_i(m') > u_i(m)$ and $\bar{t}^A(m') - \bar{t}^A(m) < \bar{t}(m') - \bar{t}(m)$.*

With Eq. (3), all thresholds must weakly increase when an AA policy is active, if application choices remain constant. Considering optimal major choice, for the highest threshold major, denoted by $\overline{m}$, clearly $\bar{t}^A(\overline{m}) \geq \bar{t}(\overline{m})$. Individuals with $B_i = 0$ will still apply if $\bar{t}^A(\overline{m}) = \bar{t}(\overline{m})$, but additionally, there is a non-negative measure of applicants j with $B_j > 0$ who have $u_j(\overline{m}) > u(m')$ for some m' and who now find applying to $\overline{m}$ preferable. Hence, $\bar{t}^A(\overline{m}) \geq \bar{t}(\overline{m})$. If the inequality is strict, the measure of individuals with $B_j > 0$ who switch from a major m' to $\overline{m}$ is the same as that of those with $B_i = 0$ who switch from $\overline{m}$ to some m''. Without adding further structure on preferences, some thresholds may, in fact, decline, as outflows may dominate inflows. Assuming independence of preference and affirmative action status, however, would ensure that inflows from more selective majors (in the form of $B_i = 0$ individuals) will exactly match outflows of $B_j > 0$ individuals to these same more selective majors and thus allow a monotonicity result (this is a version of the argument in Proposition 1 in Estevan et al., 2020).

Fact 4. *For the most selective major $\overline{m}$, $\bar{t}^A(\overline{m}) \geq \bar{t}(\overline{m})$. If individuals' policy bonus B_i is independent of preference over majors, $\bar{t}^A(m) \geq \bar{t}(m)$ holds for all majors m.*

[11] After applicants have had time to adjust their effort decisions, we cannot exclude the effects of the policy in that dimension, as noted in previous studies (e.g., Assuncao and Ferman, 2015; Bodoh-Creed and Hickman, 2017; Grau, 2018; Tincani et al., 2023; Akhtari et al., 2024).

Finally, under our assumptions, major choices are determined by preferences alone, given academic human capital and (observable) high school exit grades.

Fact 5. *If $m_i = m$ and $m_j = m'$ for $h(\theta_i, e_i, s_i) = h(\theta_j, e_j, s_j)$ and $g_i = g_j$, then $u_i(m) > u_i(m')$ and $u_j(m) < u_j(m')$.*

That is, under our assumptions, any school effects on the admission exam performance are present in the high school exit grade and individual human capital, and different major choices for individuals with the same academic human capital (and thus similar high school exit grades) must be due to different preferences. Thus, our assumptions exclude that high schools directly affect preferences over majors, which may appear restrictive, so we will control for high school fixed effects in our empirical approach.

Discussion

Effect sizes

To gain some idea about which applicants would be affected the most by a bonus policy, recall that an individual receiving a bonus B_i will switch from major m to another major m' (with $u_i(m) > u_i(m')$ and $\overline{h}(m) < \overline{h}(m')$) if both:

$$\frac{Prob(\nu \geq \overline{h}(m) - \alpha h(\theta_i, e_i, s_i) - (1-\alpha) g_i)}{Prob(\nu \geq \overline{h}(m') - \alpha h(\theta_i, e_i, s_i) - (1-\alpha) g_i)} > \frac{u_i(m')}{u_i(m)},$$

and

$$\frac{u_i(m')}{u_i(m)} > \frac{Prob(\nu \geq \overline{h}(m) - B_i - \alpha h(\theta_i, e_i, s_i) - (1-\alpha) g_i)}{Prob(\nu \geq \overline{h}(m') - B_i - \alpha h(\theta_i, e_i, s_i) - (1-\alpha) g_i)}.$$

Denoting by $x_i = E[\tilde{t}_i]$ the predicted admission exam grade, the condition implies that:

$$\frac{Prob(\nu \geq \overline{h}(m) - x_i)}{Prob(\nu \geq \overline{h}(m') - x_i)} - \frac{Prob(\nu \geq \overline{h}(m) - x_i - B_i)}{Prob(\nu \geq \overline{h}(m') - x_i - B_i)} > 0. \tag{4}$$

Hence, the larger the difference on the LHS of condition (4), the more likely it is that an applicant i will switch major choice for a bonus of B_i because the set of preferences that will induce switching increases. Since $Prob(\nu \geq \overline{h}(m) - x_i) = 1 - F_\nu(\overline{h}(m) - x_i)$, the probability of switching major choice after receiving a bonus B_i is (weakly) monotone in:

$$\frac{1 - F_\nu(\overline{t}(m) - x_i)}{1 - F_\nu(\overline{t}(m') - x_i)} - \frac{1 - F_\nu(\overline{t}(m) - x_i - B_i)}{1 - F(\overline{t}(m') - x_i - B_i)}.$$

For small B_i, this difference is well approximated by the differential:

$$\frac{\partial \frac{1-F_\nu(\overline{h}(m) - x_i)}{1-F_\nu(\overline{h}(m') - x_i)}}{\partial x_i} = \frac{(1 - F_\nu(\overline{h}(m) - x_i)) f_\nu(\overline{h}(m') - x_i) - (1 - F_\nu(\overline{h}(m') - x_i)) f_\nu(\overline{h}(m) - x_i)}{(1 - F_\nu(\overline{h}(m') - x_i))^2}$$
$$= \left(\frac{f_\nu(\overline{h}(m') - x_i)}{1 - F_\nu(\overline{h}(m') - x_i)} - \frac{f_\nu(\overline{h}(m) - x_i)}{1 - F_\nu(\overline{h}(m) - x_i)} \right) \frac{1 - F_\nu(\overline{h}(m) - x_i)}{1 - F_\nu(\overline{h}(m') - x_i)}.$$

This expression is guaranteed to be greater (less) than zero if F_ν has an increasing (decreasing) hazard rate on $[\overline{h}(m) - x_i, \overline{h}(m') - x_i]$. The normal distribution has the property that its hazard rate is strictly increasing and convex, which implies the following statement.

Fact 6. *If the error term ν follows a normal distribution, the set of preferences for which a switch to a more competitive major is preferred under the policy increases in x_i.*

The fact states that if the error term is distributed normally, the set of payoff functions such that an individual will find it profitable to upgrade their major choice expands in the expected admission exam grade. This means that applicants eligible for a bonus become more likely to apply for a major with a higher cutoff, the higher their expected entrance exam score. Thus, expected policy effects on the eligible applicants are heterogeneous and stronger for those with higher ENEM, as, with Eqs. (1) and (2), both ENEM and the entrance exam score have the same expected value.

Theoretical identification: Summary

To summarize, the above reasoning allows us to state some predictions on the effects of introducing the policy on individual major choices. Our arguments relied, in essence, on the following set of assumptions:

Assumption 1.

(1) $\overline{h}(m) = \overline{t}(m)$ is observable,
(2) the relation of $u_i(m)$ to observable individual characteristics (such as SES, origin, school, and academic capital captured by ENEM) is stationary and
(3) the end-of-high-school exam score g_i and school s_i are sufficient statistics for $h(\theta_i, e_i, s_i)$.

These assumptions imply the following set of predictions:

Prediction 1. *Conditional on ENEM grades and individual characteristics,*

(1) applicants' major choices will remain the same in the absence of an affirmative action bonus policy,
(2) on average, the cutoff of major choices will increase for applicants who are eligible for a bonus when an affirmative action bonus policy is active and
(3) applications for the most selective major from applicants eligible for the bonus will increase, and those from applicants who are not eligible will decrease.
(4) If the error term ν follows a normal distribution, eligible applicants are more likely to apply to a higher cutoff major the higher their ENEM score.

Prediction (1) corresponds to common trends. Prediction (2) predicts a positive effect of the intention to treat: recipients of the bonus are expected to apply to majors with higher entry cutoffs, i.e., more prestigious and rewarding majors, when controlling for individual covariates. Given Fact 6 and additional structure on the error terms, the policy effects are expected to be heterogeneous in individual ability, with higher-ability applicants more likely to be affected. Prediction (4) thus states that any policy effect should be more pronounced for applicants with higher ENEM scores. Prediction (3) states that applicants not eligible for the bonus will become less likely to apply for the most competitive major. More general predictions on the ineligible applicants would require putting more structure on preferences, which may not necessarily adequately reflect our case.

Given these predictions, we now focus on the data and devote the remainder of this paper to an empirical examination of UNICAMP's affirmative action policy.

4. Data

We use administrative individual-level data from the 2001 to 2008 UNICAMP admission exams (*vestibular*) obtained from COMVEST.[12]

Our dataset covers all applicants to the admission exam and includes socioeconomic characteristics such as age, gender, race (since 2003), ENEM scores, high school type, household income (in intervals corresponding to multiples of the minimum wage),[13] and parents' education

[12] *Comissão Permanente para os Vestibulares* (COMVEST) is UNICAMP's admission office (https://www.comvest.unicamp.br/).

[13] For each year, we compute the household income midpoint from survey intervals, deflate it to 2001 Brazilian *reais* using the CPI, and take logs.

and occupations, providing a rich set of controls. We also observe applicants' three major choices and all admission exam grades.

Applicants have access to detailed admission statistics from previous years in the registration manual, helping them gauge the competitiveness of each major. As illustrated in Online Appendix Figure O.1, the manual reports, for each major: the number of seats (*vagas*), applicants (*inscritos*), applicant-to-seat ratios in P_1 and P_2 (*rel. C/V*), the number and share of P_1 passers (*aprovados*), the P_1 cutoff for P_2 (*pontuação do último convocado*), and the final P_2 score of the last enrolled student (*nota padronizada do último matriculado*).

We examine the impact of UNICAMP's affirmative action policy on major choice along three dimensions: (1) the probability of selecting one of the top five most competitive majors based on 2003 P_2 cutoffs; (2) the 2003 P_2 cutoff of the first major choice; and (3) its 2003 admission rate. The first outcome captures preferences for highly selective programs linked to prestigious careers. A major is classified as "top five" if it ranked among the five highest P_2 cutoffs in 2003, which includes medicine and four engineering majors.[14] The second and third outcomes capture more nuanced shifts in preferences, such as choosing slightly more competitive majors. While these two last measures are negatively correlated, the relationship is imperfect. For instance, Online Appendix Figure O.1 shows that daytime Biology had over twice as many applicants per seat as daytime Economics (43.1 vs. 21.5), yet a much lower cutoff (584.81 vs. 683.38). To isolate shifts in applicant behavior rather than changes in annual admissions dynamics, we use 2003 values, which were publicly available to the last pre-AA cohort of applicants, as the reference point for all three outcomes. Finally, since application behavior matters most when it leads to actual admissions, we also examine the joint probability of applying to and being admitted to a top-five major (hereafter "admitted"). We focus on the joint probability, as estimating the conditional admission probability consistently is challenging if the policy affects application behavior.

To concentrate on our population of interest, we restrict our sample to individuals aged 17 to 36 who took the exam for immediate admission (i.e., not as a practice test) and completed their high school education in Brazil.[15] We exclude applicants to majors requiring an aptitude test, which evaluates specific skills.[16] Finally, we discard applicants who applied to programs created after 2003, our reference year for our prestige/competitiveness measures.[17] As noted in Section 2, it is unclear whether 2005 applicants selected majors before or after learning of the policy change. We exclude 2005 from the regressions but retain it in the event-study.

Out of our sample of interest, we first discard individuals with missing high-school or socioeconomic information (6.9 percent of the sample of interest) and those without ENEM scores (11.2 percent). Finally, because we cluster standard errors at the high school level, we drop 4.3% of singleton observations (schools with only one applicant) to avoid overstating the statistical significance of our coefficients of interest (Correia, 2015, 2016). The final sample has 130,421 and 89,951 applicants in 2001–2004 and 2006–2008, respectively.

Table 1 shows descriptive statistics by school type and period (pre- vs. post-AA). Between 2001 and 2008, the top five majors attracted roughly 34% of all applications. Private high school students – over 70% of our sample – appear to have avoided competitive majors: their application rate to top-five majors fell by 3.1 percentage points (7.8%) after the AA policy. While this decline aligns with our theoretical predictions (Section 3), it may partly reflect broader trends, as public school applicants also showed a 1.7 p.p. decline. Nevertheless, the unconditional difference-in-differences estimate ($\Delta\Delta$) indicates that public school students narrowed the gap by 1.4 p.p., or 6% of their pre-AA baseline.

Following the AA policy, private school applicants applied to less selective majors, as reflected in a 0.4 p.p. increase in the average admission rate of chosen majors (3.6% of the pre-AA baseline). Surprisingly, the difference-in-difference for the threshold of chosen majors is negative but economically negligible – 0.26% of the baseline, or 1.7% of a standard deviation – a magnitude detectable only given our large sample size. As expected, the share of private high school applicants admitted to a top-five major declined, while that of public school applicants rose. The difference-in-difference in top-five major admission rates is 0.7 p.p., a 77.8% increase relative to the pre-AA public school baseline. These patterns suggest shifts in major choice after the policy, which we examine more formally in the next section.

Some applicant characteristics evolved over the period, so we will explicitly control for them. ENEM scores declined for both private and public applicants – more for the latter – but the magnitude is small: the difference-in-difference is 1.1% of the pre-AA public-school baseline (4.6% of a standard deviation). Because ENEM is a key determinant of major choice, we include a flexible quartic in ENEM with time-varying coefficients in some of our specifications.[18] Real household income declined substantially between 2001 and 2008 for both groups, driven by high inflation.[19] Parental education – especially mothers' – rose, with fewer parents lacking a high school diploma, reflecting cohort trends (Estevan et al., 2019b). Changes in parental occupation show no consistent pattern. Because these shifts in parental income, education, and occupation may influence major choice, we include them as controls in our regressions.[20]

5. Empirical strategy

Given the plausibly exogenous nature of UNICAMP's affirmative action policy, we estimate its effects on major choice with a straightforward difference-in-difference model. Formally, for all our outcomes of interest, our regression equation takes the following form:

$$Z_{i,s,t} = \alpha P_i + \beta(P_i \times AA_t) + \boldsymbol{X_i}\boldsymbol{\Gamma} + \sum_{j=1}^{4} \phi_{j,t} ENEM_i^j + \eta_s + \tau_t + \varepsilon_{i,s,t}, \quad (5)$$

where $Z_{i,s,t}$ is one of our major-choice measures (described above) for applicant i who attended high school s and was observed in admission year t. P_i is equal to one if the applicant went to a public high school, zero otherwise; AA_t is equal to one if the applicant applied during UNICAMP's affirmative-action years (2006–2008), zero otherwise. $\boldsymbol{X_i}$ is a vector of controls for the applicant's characteristics (i.e., gender,

[14] Medicine, Computer Engineering (daytime), Control and Automation Engineering (evening), Electrical Engineering (daytime), and Electrical Engineering (evening) (see Online Appendix Table O.1).

[15] We obtain an age range of 17–36 by trimming the top and bottom 1% of applicants by age. Each year, roughly four percent of exam takers write it as a practice test, and about 0.3 percent of applicants completed their high school abroad.

[16] Over our period of interest, 5.6 percent were required to pass an aptitude test. We keep 'Dentistry' since its aptitude test only evaluates applicants' psychomotor coordination.

[17] In 2004, four new majors were created. By 2008, Pharmacy and Social Communication had cutoffs in the top quartile, while the two technology majors ranked in the bottom quartile.

[18] Accordingly, our parameter estimates remain identical across variants of the "raw" ENEM measure (e.g., normalized by year within our sample or restricted to São Paulo); only the estimates for the alternative ENEM measures differ.

[19] Nominal household income increased by 0.19 and 0.13 log points for private and public school applicants.

[20] Online Appendix Table O.4 shows that controlling for these variables explains nearly all of the unconditional difference in differences in ENEM reported in Table 1; the difference-in-difference estimate in Column (2) of Online Appendix Table O.4 is small and statistically insignificant. Adding high school fixed effects slightly overcompensates for changes in individual and parental characteristics as the difference-in-difference estimate in Column (3) becomes positive. However, the estimate is very small as it equals 0.6% of the pre-AA public school baseline and is significant only at the 10% level.

Table 1
Descriptive statistics.

		Private			Public			
	Full Sample	2001–2004	2006–2008	Difference	2001–2004	2006–2008	Difference	$\Delta\Delta$
Applied to Top Five Major	0.341	0.396	0.365	−0.031***	0.243	0.226	−0.017***	0.014***
2003 Phase 2 Cutoff	633.751	644.934	649.285	4.351***	600.111	602.874	2.763***	−1.588*
	(91.641)	(88.053)	(81.818)		(100.266)	(94.684)		
2003 Admission Rate	0.094	0.085	0.090	0.005***	0.112	0.113	0.001*	−0.004***
	(0.067)	(0.061)	(0.063)		(0.074)	(0.075)		
Admitted to Top Five Major	0.017	0.020	0.017	−0.003***	0.009	0.014	0.004***	0.007***
Public High School	0.285							
ENEM Score	6.774	7.027	6.986	−0.041***	6.227	6.116	−0.111***	−0.069***
	(1.504)	(1.477)	(1.258)		(1.683)	(1.489)		
Age	18.699	18.343	18.370	0.027***	19.498	19.659	0.161***	0.134***
	(2.019)	(1.515)	(1.605)		(2.493)	(2.939)		
Real ln(Household Income)	7.631	7.918	7.846	−0.072***	7.033	6.908	−0.125***	−0.053***
	(0.882)	(0.797)	(0.774)		(0.783)	(0.729)		
Female	0.493	0.493	0.478	−0.015***	0.512	0.504	−0.007*	0.007
Mother without Formal Instruction	0.006	0.004	0.002	−0.002***	0.016	0.014	−0.001	0.001
Mother without HS Degree	0.225	0.146	0.104	−0.042***	0.489	0.435	−0.054***	−0.011***
Mother with HS Degree	0.317	0.317	0.298	−0.019***	0.319	0.363	0.044***	0.063***
Mother with Univ. Degree	0.451	0.533	0.596	0.063***	0.177	0.187	0.011***	−0.052***
Father without Formal Instruction	0.006	0.002	0.002	0.000	0.015	0.019	0.004***	0.004***
Father without HS Degree	0.221	0.140	0.110	−0.030***	0.468	0.437	−0.032***	−0.002
Father with HS Degree	0.279	0.256	0.268	0.011***	0.308	0.350	0.042***	0.030***
Father with Univ. Degree	0.493	0.602	0.620	0.018***	0.209	0.194	−0.015***	−0.033***
Father with Manual Occ.	0.125	0.053	0.049	−0.004***	0.309	0.311	0.002	0.006**
Father with Mid-Top Occ.	0.355	0.364	0.282	−0.083***	0.461	0.351	−0.110***	−0.027***
Father with Top Occ.	0.468	0.566	0.609	0.043***	0.182	0.169	−0.013***	−0.057***
Father with Other Occ.	0.052	0.016	0.060	0.043***	0.048	0.169	0.121***	0.078***
Mother with Manual Occ.	0.060	0.030	0.027	−0.003***	0.132	0.149	0.017***	0.020***
Mother with Mid-Top Occ.	0.315	0.358	0.283	−0.075***	0.302	0.254	−0.047***	0.028***
Mother with Top Occ.	0.263	0.287	0.402	0.115***	0.076	0.095	0.018***	−0.097***
Mother with Other Occ.	0.360	0.325	0.281	−0.044***	0.490	0.497	0.007*	0.051***
Observations	220,372	93,244	64,324	157,568	37,177	25,627	62,804	220,372

Notes: Standard deviations are in parentheses. The columns "Difference" correspond to the 2006–2008 average minus that of 2001–2004. Each number below $\Delta\Delta$ is a difference in differences. * significant at 10%; ** significant at 5%; *** significant at 1%.

age fixed effects, parental educational attainment and occupation, and the log of household income, as defined in Table 1). In some of our specifications, we control for a quartic function of $ENEM_i$ and allow each of the quartic-function parameters, $\phi_{j,t}$, to differ across years. η_s and τ_t are (last attended) high school and year fixed effects, respectively. Note that including year-fixed effects absorbs the AA_t dummy. To account for potential serial correlation in the error terms and school-specific shocks, our standard errors will be cluster-robust at the high school level in all our regressions.

The parameter of interest is β, which measures how much the AA policy changed the gap in the outcome between private and public high-school applicants.[21] Our regression framework controls for individual ENEM and a host of individual characteristics. According to our theoretical framework, the coefficient will identify the policy effects on the bonus recipients under Assumption 1.

Whether to control for ENEM or not is debatable. On the one hand, we expect that academic ability, or its signal received by the applicant, could affect her major choice (and be correlated with our regressors of interest), and we would, therefore, want to control for it. On the other hand, some studies have found that affirmative action could affect student effort and performance on exams (Akhtari et al., 2024) and, therefore, affect the link between student ability and ENEM. So, one could be concerned that $ENEM_i$ is a 'bad' control. In our case, we see ENEM as an ability signal (potentially composed of 'true' ability, effort, and exam difficulty) received by the student. We want to control for this signal, and not the true ability per se. If applicants make decisions based on the *signal* of ability and not the ability itself, then the potential risk of dealing with a bad control is significantly lessened. Two considerations suggest that whether or not to control for ENEM should not be a major cause for concern. First, Estevan et al. (2019b) did not find evidence of public–private differential performance changes on the admission exam following the introduction of UNICAMP's affirmative action. Second, and more importantly, Online Appendix Table O.4 shows that, once we control for personal and parental characteristics, the public–private ENEM gap did not change significantly following the AA policy. Therefore, we should not expect significant differences in β estimates whether we control for ENEM or not, given the lack of partial correlation between ENEM and $P_i \times AA_t$, once we control for personal and parental characteristics. In any case, our main regression results are presented with and without ENEM controls.

6. Main results

We begin by examining whether UNICAMP's affirmative action policy led public high school students to apply to, and gain admission to, more selective majors. We then assess where in the ENEM distribution the effects arise and whether they are stronger among students likely to have more information about the admission process.

Table 2 reports the estimated effects of the AA policy on our outcomes of interest. Columns (1)–(3) sequentially add controls for

[21] As mentioned in the Introduction, attending a public high school is a proxy for low SES. We use public high-school status rather than opting for the AA policy since eligible applicants could, in principle, not opt into the bonus. Not surprisingly, the vast majority (94%) of eligible applicants opt for the AA policy.

Table 2
Affirmative action, application and admission.

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Application to a Top-Five Major						
Public High School	−0.152***	−0.064***		−0.106***	−0.045***	
	(0.011)	(0.010)		(0.008)	(0.009)	
Public HS × AA	0.013*	0.023***	0.026***	0.022***	0.025***	0.025***
	(0.008)	(0.007)	(0.006)	(0.007)	(0.007)	(0.007)
Mean of Dep. Var. (2004, Public)	0.225	0.225	0.225	0.225	0.225	0.225
Panel B: 2003 Major Cutoff						
Public High School	−44.616***	−21.806***		−31.255***	−16.124***	
	(2.523)	(2.119)		(1.677)	(1.707)	
Public HS × AA	−1.661	1.010	1.963	1.895	2.953**	2.692**
	(1.703)	(1.467)	(1.378)	(1.520)	(1.412)	(1.348)
Mean of Dep. Var. (2004, Public)	595.392	595.392	595.392	595.392	595.392	595.392
Panel C: 2003 Admission Rate						
Public High School	0.026***	0.017***		0.022***	0.015***	
	(0.001)	(0.001)		(0.001)	(0.001)	
Public HS × AA	−0.004***	−0.005***	−0.005***	−0.004***	−0.006***	−0.005***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Mean of Dep. Var. (2004, Public)	0.115	0.115	0.115	0.115	0.115	0.115
Panel D: Admission to a Top-Five Major						
Public High School	−0.011***	−0.002		−0.001	0.003***	
	(0.002)	(0.002)		(0.001)	(0.001)	
Public HS × AA	0.007***	0.008***	0.011***	0.009***	0.009***	0.011***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Mean of Dep. Var. (2004, Public)	0.009	0.009	0.009	0.009	0.009	0.009
Observations	220,372	220,372	220,372	220,372	220,372	220,372
High School Clusters	10,278	10,278	10,278	10,278	10,278	10,278
Controls:						
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
$\phi_{j,t} ENEM_i^j, j \in \{1,2,3,4\}$	No	No	No	Yes	Yes	Yes
Personal Characteristics	No	Yes	Yes	No	Yes	Yes
Parental Characteristics	No	Yes	Yes	No	Yes	Yes
High School Fixed Effects	No	No	Yes	No	No	Yes

Notes: The sample includes applicants to UNICAMP admission exam between 2001 and 2008, excluding 2005. The dependent variables are: Panel A: a binary variable equal to one if the applicant applied to one of the top five most competitive UNICAMP majors (i.e., Medicine, Computer Engineering-daytime, Control and Automation Engineering-evening, Electrical Engineering-daytime, Electrical Engineering-evening) based on the 2003 Phase 2 cutoffs, and zero otherwise; Panel B: the 2003 Phase 2 cutoff of the major chosen by the applicant as the first choice; Panel C: the 2003 admission rate of the major chosen by the applicant as the first choice; Panel D: a binary variable equal to one if the applicant applied and was admitted to one of the five majors mentioned above and zero otherwise. 'Public High School (HS)' is a binary variable equal to one if the applicant attended a public high school and zero otherwise, and 'AA' is a binary variable equal to one if the candidate applied during UNICAMP's affirmative action years (i.e., 2006–2008), and zero otherwise. Personal characteristics consist of age fixed effects and gender. Parental characteristics consist of the log of household income, and education and occupation categorical variables (for each parent), both containing the four levels presented in Table 1. We control for a quartic function of ENEM for which the parameters can change over time in columns (4) to (6). Although we control for 'Public High School' in all specifications, we do not present their coefficient estimates when we include high school fixed effects (columns (3) and (6)), as they are estimated using only a few students who switched between public and private schools during their secondary education. Singletons (observations in high schools that we only observe once) are dropped. Cluster-robust standard errors (at the high school level) are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

personal and parental characteristics and, in Column (3), fixed effects for the applicant's last-attended high school.[22] Columns (4)–(6) additionally include a time-varying quartic function of ENEM scores, allowing us to test whether, conditional on the same academic ability signal, public school applicants' propensity to apply to competitive majors changed relative to their private school counterparts after the policy.

Panel A indicates that the AA policy increased the likelihood that public high school students applied to a top-five major relative to their private school counterparts. Estimates are similar with or without ENEM controls once we control for personal characteristics. In our preferred specification (6), the 'Public HS × AA' coefficient estimate is

[22] In specifications with school fixed effects ((3) and (6)), the 'Public High School' (P_i) coefficient is identified only from a few students who switched school types during high school. As this atypical group is not our focus, we include P_i as a control but omit its estimates to avoid confusion when comparing specifications.

Table 3
Affirmative action, application and admission - heterogeneity.

	(1)	(2)	(3)	(4)
Panel A: Application to a Top-Five Major				
Public HS × AA	0.020***	0.017***	0.013	0.017*
	(0.006)	(0.006)	(0.009)	(0.009)
Public HS × AA × Previous Attempt	0.016	0.020*		
	(0.012)	(0.012)		
Public HS × AA × At Least One Parent with HE			0.020*	0.012
			(0.011)	(0.011)
Mean of Dep. Var. (2004, Public)	0.225	0.225	0.225	0.225
Panel B: 2003 Major Cutoff				
Public HS × AA	1.461	1.990	−1.025	0.647
	(1.286)	(1.348)	(1.741)	(1.670)
Public HS × AA × Previous Attempt	1.584	1.741		
	(2.226)	(2.119)		
Public HS × AA × At Least One Parent with HE			5.286**	3.452
			(2.401)	(2.288)
Mean of Dep. Var. (2004, Public)	595.392	595.392	595.392	595.392
Panel C: 2003 Admission Rate				
Public HS × AA	−0.006***	−0.005***	−0.003**	−0.003***
	(0.001)	(0.001)	(0.001)	(0.001)
Public HS × AA × Previous Attempt	0.001	−0.000		
	(0.002)	(0.002)		
Public HS × AA × At Least One Parent with HE			−0.001	−0.000
			(0.002)	(0.002)
Mean of Dep. Var. (2004, Public)	0.115	0.115	0.115	0.115
Panel D: Admission to a Top-Five Major				
Public HS × AA	0.009***	0.008***	0.006***	0.008***
	(0.001)	(0.002)	(0.002)	(0.002)
Public HS × AA × Previous Attempt	0.006	0.006*		
	(0.004)	(0.004)		
Public HS × AA × At Least One Parent with HE			0.011***	0.008**
			(0.003)	(0.003)
Mean of Dep. Var. (2004, Public)	0.009	0.009	0.009	0.009
Observations	220,372	220,372	220,372	220,372
High School Clusters	10,278	10,278	10,278	10,278
Controls:				
Year Fixed Effects	Yes	Yes	Yes	Yes
$\phi_{j,t} ENEM_i^j, j \in \{1,2,3,4\}$	No	Yes	No	Yes
Personal Characteristics	Yes	Yes	Yes	Yes
Parental Characteristics	Yes	Yes	Yes	Yes
High School Fixed Effects	Yes	Yes	Yes	Yes

Notes: The sample includes applicants to UNICAMP admission exam between 2001 and 2008, excluding 2005. The dependent variables are: Panel A: a binary variable equal to one if the applicant applied to one of the top five most competitive UNICAMP majors (i.e., Medicine, Computer Engineering-daytime, Control and Automation Engineering-evening, Electrical Engineering-daytime, Electrical Engineering-evening) based on the 2003 Phase 2 cutoffs, and zero otherwise; Panel B: the 2003 Phase 2 cutoff of the major chosen by the applicant as the first choice; Panel C: the 2003 admission rate of the major chosen by the applicant as the first choice; Panel D: a binary variable equal to one if the applicant applied and was admitted to one of the five majors mentioned above and zero otherwise. 'Public High School (HS)' is a binary variable equal to one if the applicant attended a public high school and zero otherwise, and 'AA' is a binary variable equal to one if the candidate applied during UNICAMP's affirmative action years (i.e., 2006–2008), and zero otherwise. 'Previous Attempt' is a binary variable equal to one if the candidate has previously applied to UNICAMP and zero otherwise. Personal characteristics consist of age fixed effects and gender. 'At Least One Parent with HE' is a binary variable equal to one if at least one of the candidate's parents has a higher education degree and zero otherwise. Parental characteristics consist of the log of household income, and education and occupation categorical variables (for each parent), both containing the four levels presented in Table 1. We control for a quartic function of ENEM for which the parameters can change over time in columns (4) to (6). 'At Least One Parent with HE' and 'Previous Attempt' are interacted with all control variables (including high school fixed effects, when included). Although we control for 'Public High School' in all specifications, we do not present their coefficient estimates when we include high school fixed effects (columns (3) and (6)), as they are estimated using only a few students who switched between public and private schools during their secondary education. Singletons (observations in high schools that we only observe once) are dropped. Cluster-robust standard errors (at the high school level) are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

2.5 p.p., statistically significant at the 1% level, and corresponds to an 11% increase over the 2004 share of public school applicants choosing these majors.

Panel B investigates whether our findings hold more generally by considering applications for all majors and using their 2003 P_2 cutoff scores as a measure of selectivity. The estimates are larger and statistically significant once ENEM is included. Column (6) shows that the affirmative action policy increases the average cutoff of majors chosen by public school students by 2.7 points. While small relative to their 2004 average cutoff, this represents 16.7% of the 16.1-point private–public gap in Column (5). Panel C shows results using the 2003 admission rate as the dependent variable. We find that public school students select majors with lower admission rates after the policy, regardless of ENEM controls. The effect is sizeable, corresponding to one-third of the 0.015-point private–public gap (Column (5)).

Finally, Panel D in Table 2 suggests that public high school applicants increased their likelihood of applying and being admitted to a top-five major by between 0.7 and 1.1 percentage points following affirmative action.[23] The magnitude of the effect is large: specification (6) suggests a 122 percent increase when compared to the proportion of public high school applicants who applied and gained admission to a top-five major in 2004 (0.9 percent).

Several factors may explain the large magnitude of our joint probability estimates. First, the probability of admission conditional on applying may have increased 'mechanically' for public high school applicants due to the 30-point bonus. Moreover, top-five majors differ from typical, less competitive majors: they attract applicants with higher ENEM scores and, crucially, more homogeneous P_2 scores. This homogeneity amplifies the relative impact of the 30-point bonus within the P_2 distribution for public school applicants. Second, public school applicants have increased their probability of applying to a top-five major, as shown in Panel A of Table 2. This rise in applications further magnifies the effect of the increased conditional admission probability on the joint probability of applying and being admitted.

Taken together, the results in Table 2 suggest that the affirmative action policy had a sizeable positive effect on public high school students' major choices and their representation in selective programs.

Heterogeneity

We next examine whether the AA policy's effect on major choice varies across the ENEM distribution. Motivated by Fact 6 from our theoretical framework and by Bond et al. (2018), who find that higher-ability students respond more to SAT shocks than lower-ability students, we test whether high-ENEM applicants reacted differently from others. While SAT shocks in their setting affect both admission probabilities and perceived ability, our treatment (bonus points) should influence only the former. Nonetheless, a similar pattern could emerge under our affirmative action policy.

We focus on major cutoffs for this exercise, as they align most closely with our theoretical framework. In this exercise, we use yearly ENEM percentiles by group (public/private high school) rather than raw scores to get more precise estimates at the bottom and top of the distribution (since each percentile contains the same number of observations) and to abstract from any overall ENEM shifts across time affecting all students equally (e.g., changes in test difficulty). We also restrict the analysis to 2004 and 2006 since using multiple-year fixed effects pre- and post-AA in our 2001–2008 analysis makes it impossible to identify the level effect of the AA policy (i.e., the common effect of AA on private and public school applicants' major cutoff). We first estimate an augmented version of Eq. (5) that allows the policy's impact to vary with ENEM percentiles by interacting a quartic function of ENEM percentiles with 'Public HS × AA.' We then predict the cutoff of the chosen major as a function of ENEM percentile, holding all other regressors constant.

Fig. 3 plots predicted major cutoffs for public and private high school students in 2004 and 2006. In 2004, public school applicants tended to choose majors with lower cutoffs. Comparing 2004 to 2006 shows little response to the AA among private school students and public school students in the bottom half of the ENEM distribution. In contrast, public school applicants above the 60th percentile respond increasingly with ENEM score; by the 80th percentile, their predicted cutoffs rise by about 10 points, matching those of private school counterparts. These patterns suggest that the modest average effect in Table 2 (2.7 points) masks substantial heterogeneity, consistent with our theoretical framework and Bond et al. (2018).

Finally, we examine whether the AA policy's effects vary with applicants' potential information levels. Kapor et al. (2020) highlight the importance of subjective beliefs in school choice and show that information provision can shift application behavior and admission outcomes. We focus on two groups likely to have more information about the admission process: those who had previously taken the UNICAMP exam (possibly as practice), labeled 'Previous Attempt,' and those with at least one parent holding a higher education degree (labeled 'At Least One Parent with HE'). In these regressions, we interact 'Previous Attempt' and 'At Least One Parent with HE' with all control variables (including high school fixed effects, when controlled for). Table 3 reports the results. While not all estimates are statistically significant, there is suggestive evidence that both groups were more likely to adjust their choices toward top-five majors and higher-threshold programs. They also appear to have experienced greater increases in admission probabilities to top-five majors, indicating that they may have benefited disproportionately from the expanded access to selective programs under the policy.

Robustness checks

In our main estimations, we drop singleton observations (high schools with only one applicant), reducing the sample by 4.3%. This could bias results if excluded applicants responded differently to the AA policy, for instance, if information spread more easily in schools where UNICAMP is a common choice. To assess this, we re-estimate the regressions including singletons. While this likely understates standard errors (Correia, 2015, 2016), the results in Online Appendix Table O.6 show that adding nearly 10,000 observations yields parameter estimates and standard errors virtually identical to those in Table 2.

We also retain applicants up to age 36 in the main analysis. The AA policy could have encouraged older public school graduates to reapply, and Table 1 shows a small but significant increase in both mean age and its standard deviation. As a robustness check, we re-estimate restricting the sample to ages 17–23, which covers most applicants and reduces the sample by 10%. Online Appendix Table O.7 shows that results remain very similar to those in Table 2.

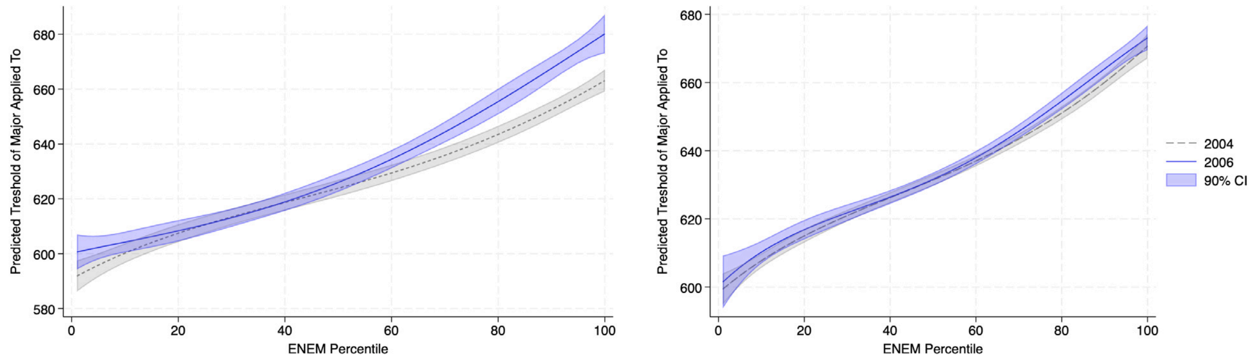
7. Event study results

The central assumption underlying the use of a difference-in-difference estimator is that, in the absence of the affirmative action policy, the application behavior of private and public high school applicants would have followed the same trend. To shed light on the validity of this assumption, we adapt Eq. (5) to estimate an event-study model using data from 2001 to 2008:[24]

$$Z_{i,s,t} = \alpha P_i + \sum_{j=2001}^{2003} \gamma_j \mathbb{1}[j=t] P_i + \sum_{j=2005}^{2008} \beta_j \mathbb{1}[j=t] P_i + \boldsymbol{X}_{\boldsymbol{i}} \boldsymbol{\Gamma}$$

[23] Online Appendix Table O.5 shows a similar increase in the joint probability of applying and *enrolling* in a top-five major.

[24] We present the results for regressions where we control for ENEM. However, the results for which we exclude ENEM as controls lead to the same conclusions.



(a) Public high school (b) Private high school

Fig. 3. Predicted threshold of first major choice by ENEM percentile—2004 and 2006.
Notes: This figure shows the predicted P_2 major cutoffs in the UNICAMP admission exam according by ENEM score percentile, application year, and high school type (public or private). The shaded regions represent the 90% confidence intervals. We compute the predicted values of P_2 major cutoffs using an OLS regression of P_2 major cutoffs on a quartic function of the ENEM score percentile interacted with AA_t and $P_i \times AA_t$ (i.e., a 2006 year dummy variable, a public high school dummy variable, and their interaction). The regression also includes controls for high school fixed effects, and personal and parental characteristics. Standard errors are clustered at the high school level.

$$+\sum_{j=1}^{4} \phi_{j,t} ENEM_i^j + \eta_s + \tau_t + \varepsilon_{i,s,t}, \tag{6}$$

where $\mathbb{1}[j = t] = 1$ if $j = t$, and 0 otherwise. The γ_j's and β_j's are meant to capture the public–private outcome gaps (for each year) before and after the AA policy, respectively. Note that we include 2005, the first year the AA policy took effect, for transparency reasons. However, many students may not have reacted to the policy, as they learned about it when registering for the admission exam. The event study will provide suggestive evidence on the validity of the parallel-trend assumption and shed light on the effect of UNICAMP's affirmative action policy after 2006. Results for 2008 should also be interpreted cautiously, as the *Universidade de São Paulo* (USP), UNICAMP's main alternative, introduced its AA policy in 2007.

Fig. 4 shows the γ_j and β_j estimates (along with their 90% confidence intervals) for applying to a top-five major. These coefficient estimates capture how the public–private gap in applying to a top-five major changed from year to year. Fig. 4 shows that all coefficients for the years leading to affirmative action are close to zero and statistically insignificant, supporting the parallel-trends assumption. We can also notice that the coefficient for 2005 (presented in the striped shaded area) is close to being statistically significant at a 10% level, suggesting that some applicants may have reacted upon learning about the policy. Finally, the coefficients for 2006–2008 are all statistically significant at a 1% significance level and increasing, suggesting that the impact of affirmative action does not dissipate over time.

Fig. 5 presents the results from an identical exercise for applying and being admitted to a top-five major. The main difference between the two figures is that the impact of affirmative action on applying and being admitted is immediate, which is not surprising since the bonus points awarded to public high school applicants increased their admission rate in 2005. As in the case of applying to a top-five major, all coefficients for the years leading to the affirmative action are close to zero and statistically insignificant, and lag parameters are all statistically significant at 1%.

Even though the leading parameter estimates (the $\hat{\gamma}_j s$) presented in Figs. 4 are statistically insignificant, recent research suggests that standard pre-trend tests could lack power (Freyaldenhoven et al., 2019; Roth, 2022; Rambachan and Roth, 2023). Also, a small but increasing secular trend between 2001 and 2004 in Fig. 5 could align with a gradual catch-up from public high school applicants. We therefore follow Rambachan and Roth (2023) to investigate whether our findings are sensitive to allowing for the difference in trends to be nonlinear (i.e., to have a non-constant slope). Rambachan and Roth (2023) propose a methodology for constructing robust confidence intervals for the lag parameters that allow the trend differential's slope to change by as much as M between consecutive periods. $M = 0$ imposes the difference in trends to be linear. The choice of M is not trivial. Rambachan and Roth (2023) suggest picking a potential confounding factor for which the magnitude can be easily interpreted. In our case, we use the estimates for gender differences in our outcomes of interest as the base for our choice of M. In our event-study regressions, we find statistically significant and economically important gender differences, even after controlling for ENEM and our other control variables. For example, females are 5.1 percentage points (p.p.) less likely to apply to a top-five major (when roughly 30% of all applicants apply to a top-five major).[25] We, therefore, pick M to represent omitting a confounding that has the same effect on the outcome of interest as increasing, each year, the share of females by five p.p. (equivalent to 10%), which is sizeable. Accordingly, we set M to 0.0025 for applying to top-five majors (0.05×0.051) and 0.0004 for applying and being admitted to a top-five major.

Online Appendix Figures O.2 and O.3 present the 90% confidence intervals for the average of our lag parameters of interest, β_{2006} to β_{2008}, so we can compare them to our difference-in-difference estimates. In Online Appendix Figure O.2, we can see that allowing for a perfectly linear trend differential ($M = 0$) suggests that, if anything, our difference-in-difference estimate for applying to a top five major is downward biased since the middle value of the confidence interval for the average of the lag parameters lies close to 0.05 and the interval excludes 0.025, our original difference-in-difference estimate. Such a result is expected as Fig. 4's pre-trend slightly decreases while the post-trend is upward. Increasing M (i.e., gradually relaxing the linearity assumption in the trend differential) slowly widens the confidence interval. The "breakdown" M value (the value for which we fail to reject the null hypothesis at a 10% significance level) is larger than 0.0024, which would be equivalent to allowing for an omitted confounding factor to change the slope of the trend differential by the equivalent of increasing, each year, the share of female applicants by five p.p. In the case of applying and being admitted to a top-five major (Online Appendix Figure O.3), we can see that the middle points of the

[25] Similarly, gender differences are 0.008 p.p. for being admitted to a top five major, 15.7 points for the major 2003 cutoff score, and 1.7 p.p. for the major 2003 admission rate, respectively, always suggesting that, conditional on ENEM, males apply to more competitive programs.

Table 4
Affirmative Action, Application and Admission - SES.

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Application to a Top-Five Major						
Real ln(Household Income)	0.101***	0.098***	0.078***			
	(0.008)	(0.011)	(0.010)			
ln(Household Income) × AA	−0.010**	−0.011***	−0.014***			
	(0.004)	(0.004)	(0.004)			
Financed by Family				0.166***	0.168***	0.116***
				(0.017)	(0.025)	(0.025)
Financed × AA				−0.014	−0.020*	−0.032***
				(0.011)	(0.012)	(0.008)
Panel B: 2003 Major Cutoff						
Real ln(Household Income)	26.150***	24.457***	20.632***			
	(0.834)	(0.610)	(0.679)			
ln(Household Income) × AA	−1.556*	−1.987***	−2.657***			
	(0.823)	(0.711)	(0.747)			
Financed by Family				49.608***	46.971***	32.220***
				(2.205)	(2.196)	(2.611)
Financed × AA				3.630	1.273	−1.099
				(3.234)	(3.329)	(2.220)
Panel C: 2003 Admission Rate						
Real ln(Household Income)	−0.014***	−0.014***	−0.013***			
	(0.001)	(0.000)	(0.000)			
ln(Household Income) × AA	0.003***	0.003***	0.004***			
	(0.001)	(0.001)	(0.001)			
Financed by Family				−0.033***	−0.033***	−0.024***
				(0.002)	(0.001)	(0.001)
Financed × AA				0.005*	0.006*	0.007***
				(0.003)	(0.003)	(0.003)
Panel D: Admission to a Top-Five Major						
Real ln(Household Income)	0.008***	0.007***	0.006***			
	(0.001)	(0.001)	(0.001)			
ln(Household Income) × AA	−0.000	−0.000	−0.001*			
	(0.000)	(0.000)	(0.000)			
Financed by Family				0.015***	0.015***	0.013***
				(0.001)	(0.001)	(0.001)
Financed × AA				−0.002**	−0.003***	−0.004***
				(0.001)	(0.001)	(0.001)
Observations	216,052	216,052	216,052	216,052	216,052	216,052
Municipality Clusters	924	924	924	924	924	924
Controls:						
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Age and Gender	No	Yes	Yes	No	Yes	Yes
Other Parental Characteristics	No	No	No	No	No	No
Municipality Fixed Effects	No	No	Yes	No	No	Yes
$\phi_{j,t} ENEM_i^j, j \in \{1,2,3,4\}$	No	No	No	No	No	No

Notes: The sample includes applicants to UNICAMP admission exam between 2001 and 2008, excluding 2005. The dependent variables are: Panel A: a binary variable equal to one if the applicant applied to one of the top five most competitive UNICAMP majors (i.e., Medicine, Computer Engineering-daytime, Control and Automation Engineering-evening, Electrical Engineering-daytime, Electrical Engineering-evening) based on the 2003 Phase 2 cutoffs, and zero otherwise; Panel B: the 2003 Phase 2 cutoff of the major chosen by the applicant as the first choice; Panel C: the 2003 admission rate of the major chosen by the applicant as the first choice; Panel D: a binary variable equal to one if the applicant applied and was admitted to one of the five majors mentioned above and zero otherwise. 'AA' is a binary variable equal to one if the candidate applied during UNICAMP's affirmative action years (i.e., 2006–2008), and zero otherwise. 'Financed by Family' is a binary variable equal to one if the candidate's education is financed by their family. Personal characteristics consist of age fixed effects and gender.. Singletons (observations in municipalities that we only observe once) are dropped. Cluster-robust standard errors (at the municipality level) are shown in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

confidence intervals are slightly smaller than our original difference-in-difference estimate (0.011). However, the intervals always contain it, and the average of the lag parameters remains statistically significant even for values of M corresponding to increasing the share of female applicants by five p.p.

While the pre-trends for applying to and for applying and being admitted to a top-five major are relatively flat, this is not the case for the cutoff and the admission rate of the major applied to. Online Appendix Figures O.4 and O.5 show that there was a downward (upward) sloping trend for the 2003 cutoff (admission rate) applied to. In both cases, we reject the null hypothesis of parallel trends. However, these pre-trends should bias our estimates towards zero in both cases. Online Appendix Figures O.6 and O.7 show that allowing for nonlinear linear secular pre-trend differentials leads to parameter estimates that

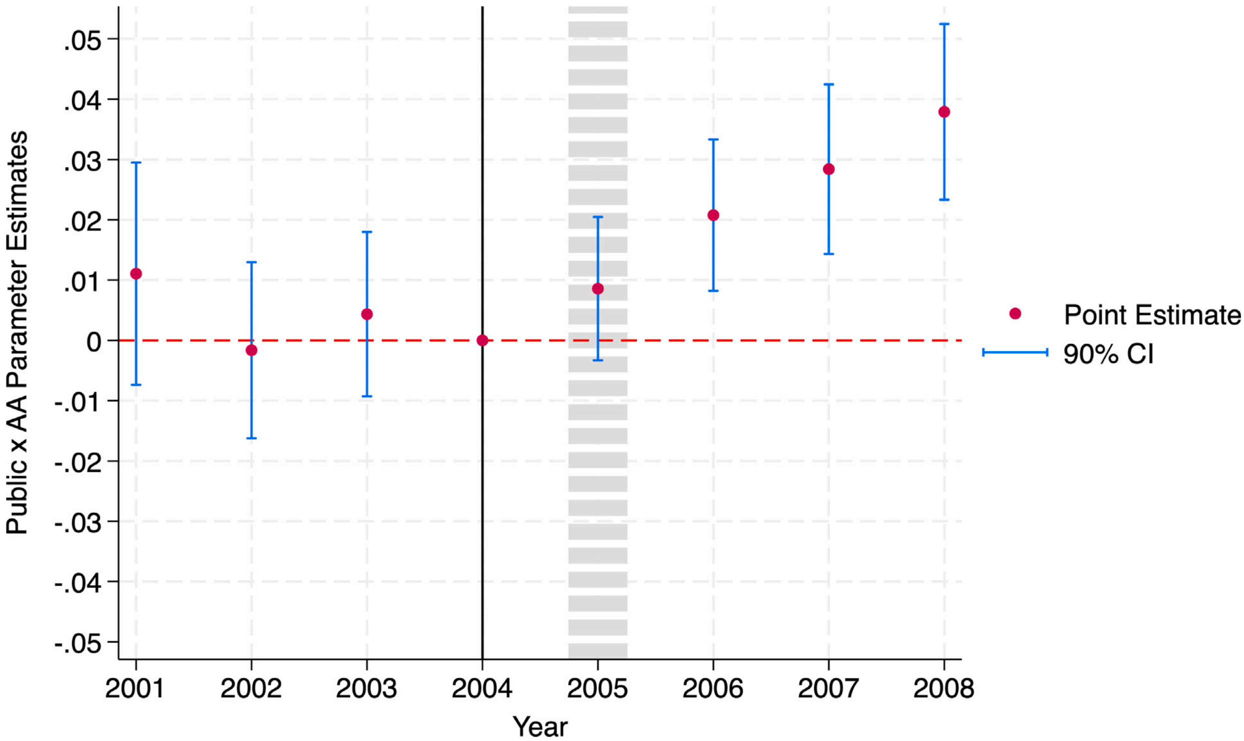


Fig. 4. Event study results: Application to a top-five major.
Notes: This graph displays the event-study coefficient estimates for 'Application to a Top-Five Major.' 'Application to a Top Five Major' is a binary variable equal to one if the applicant applied to one of the top five most competitive UNICAMP majors (i.e., Medicine, Computer Engineering-daytime, Control and Automation Engineering-evening, Electrical Engineering-daytime, Electrical Engineering-evening) based on the 2003 P_2 cutoffs, and zero otherwise. We control for a quartic function of ENEM, for which the parameters can change over time. The regression also includes controls for high school fixed effects, and personal and parental characteristics. The blue lines represent the 90% confidence intervals of the point estimates. Note that 2005 (included in the figure for transparency reasons) is shaded in gray since it is the first year the affirmative action policy took effect, leaving little time for applicants to react. Standard errors are clustered at the high school level.

are larger in magnitude than those presented in our original difference-in-difference analysis (especially in the case of 2003 cutoffs) and that these estimates remain statistically significant at 10% for large M values—the breakdown value is equal or larger than the one equivalent to increasing the share of female applicants by five p.p.

Thus, our parameter estimates for the 2006–2008 average effect of the AA policy are robust to allowing for secular pre-trend differentials between public and private high school applicants that can deviate from linearity significantly for all our outcomes of interest. Figs. 4, 5, O.4, and O.5 also suggest that the effect of the affirmative does not decrease over time: the estimates for applying and being admitted to a top five major exhibit a flat (if not slightly increasing) trend around 0.009 while those for the other three outcomes suggest an increasing effect of the affirmative action policy.[26] Overall, if anything, our event-study results suggest that our difference-in-difference estimates are conservative ones.

8. Affirmative action and the parental-income gradient

By modifying applicants' major choice and their probability of being admitted to more selective majors, the affirmative action policy could have impacted the intergenerational mobility of low SES individuals. We now investigate whether the affirmative action policy changed the link between parental income and major selection/admission. To investigate this question, we slightly modify our regression model presented in Eq. (5). Essentially, we replace our public-school indicator variable by one of two measures of parental income: (1) the log of the parental income, or (2) an indicator variable equal to one if the student is financed by their family. While almost all (98%) private high school applicants are financed by their family, significantly fewer public high school applicants are (81%). The parameter estimates for being financed by their family will therefore be driven by applicants at the bottom of the income distribution (i.e., those not financed by their family). For these regressions, we do not control for high-school fixed effects as they are (probably) determined by parental income. Instead, we use municipality fixed effects in Columns (3) and (6).

Panel A of Table 4 suggests that the link between parental income and applying to a top five major weakened following the affirmative action.[27] Concentrating on the specification (3), we see that before the affirmative action, a 10-percent increase in parental income increased the likelihood of applying to a top-five major by 0.8 percentage points. This gradient decreased by 0.14 percentage points (or 18%) following the affirmative action. We get similar results when we focus on the link between being financed by their family and applying to a top-five major.

[26] We present the 90% confidence interval for the average of the lag parameters including β_{2005} for our outcomes of interest, allowing for nonlinear linear secular pre-trend differentials in Online Appendix Figures O.8 to O.11. As in the case of the β_{2006}–β_{2008} average, the average including β_{2005} remains statistically significant at a 10% significance level even for substantial deviations from linearity.

[27] We present results for when we do not control for ENEM. As in our main regressions, whether one should control for ENEM in this SES exercise is debatable. We present the results when controlling for ENEM in Online Appendix Table O.8. If anything, the results are slightly stronger. Note that we lose about 4000 observations because of municipality singletons.

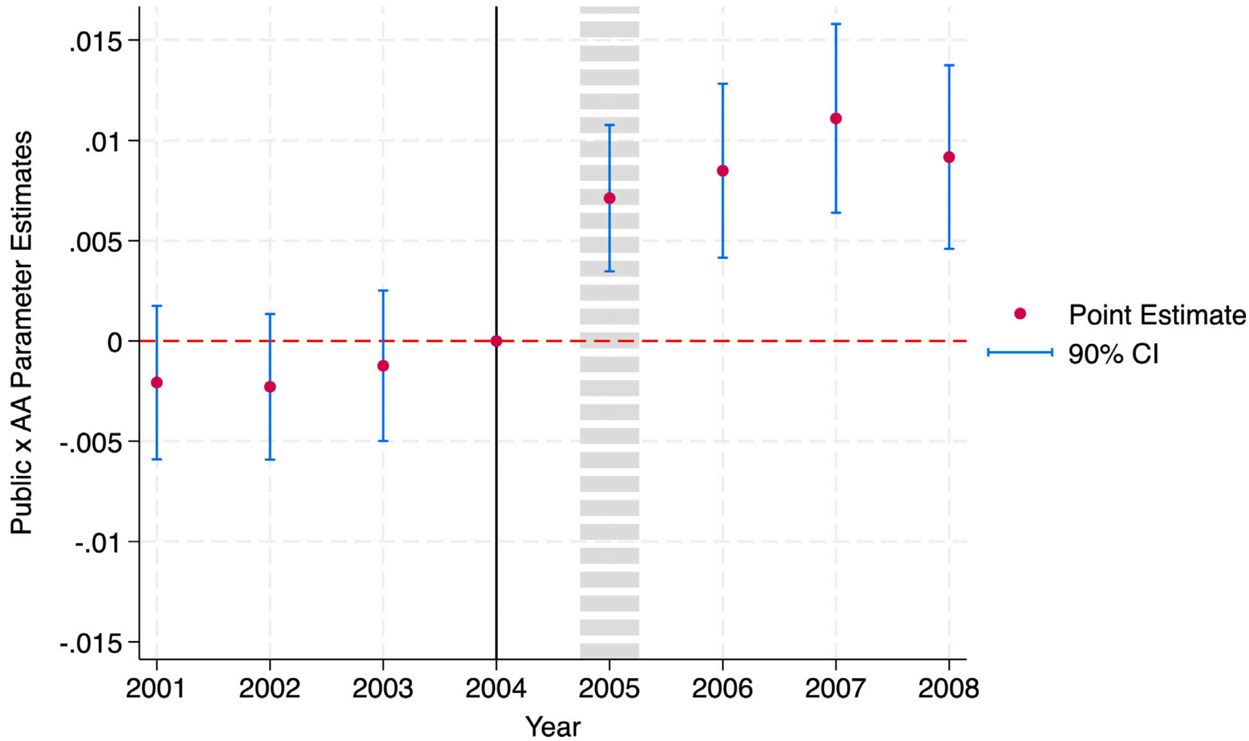


Fig. 5. Event study results: Application and admission to a top-five major.
Notes: This graph displays the event-study coefficient estimates for 'Application and Admission to a Top-Five Major.' 'Application and Admission to a Top-Five Major' is a binary variable equal to one if the applicant applied and was admitted to one of the top five most competitive UNICAMP majors (i.e., Medicine, Computer Engineering-daytime, Control and Automation Engineering-evening, Electrical Engineering-daytime, Electrical Engineering-evening) based on the 2003 Phase 2 cutoffs, and zero otherwise. We control for a quartic function of ENEM, for which the parameters can change over time. The regression also includes controls for high school fixed effects, and personal and parental characteristics. The blue lines represent the 90% confidence intervals of the point estimates. Note that 2005 (included in the figure for transparency reasons) is shaded in gray since it is the first year the affirmative action policy took effect, leaving little time for applicants to react. Standard errors are clustered at the high school level.

Overall, Panels B and C also suggest that the magnitude of the parental-income gradient for the majors' cutoff scores and admission rates students apply to decreased significantly following the affirmative action. However, there is an exception. Students financed by their family apply to majors with significantly larger cutoff scores than those who are not, and the difference remained the same following the affirmative action policy.

Finally, Panel D shows that the link between being admitted to a top-five major and parental income decreased by a sixth once we control for municipality fixed effects. The gradient for being financed by your family also decreased by 31%, suggesting more students from the bottom of the parental-income distribution benefited largely from the policy for gaining access to top majors.

9. Conclusion

This paper examines the impact of an affirmative action policy introduced by a Brazilian selective university in 2005 on beneficiaries' major selection and admission probabilities relative to non-beneficiaries. The policy targeted students from public high schools, who generally have lower socioeconomic status than students from private high schools in Brazil, by granting them bonus points in the admission exam. Additionally, we analyze whether the affirmative action affected the link between parental income and major choice.

The affirmative action policy effectively expanded the range of options available to the targeted applicants, and they responded by choosing more competitive majors. Following the implementation of the policy, applicants from public high schools were approximately ten percent more likely to select one of the top five majors. This behavior change and the bonus points were linked to a substantial increase of about 122% in the joint probability of applying to and being accepted into a top-five major. As anticipated by the theoretical framework, beneficiaries with higher abilities were more responsive to the change in admission probabilities resulting from the affirmative action policy. This provides evidence that applicants indeed behaved strategically, i.e. the preferences over majors stated in applications are not necessarily students' true preferences.

Our findings hold substantial policy implications. They clearly suggest that application choices are not solely driven by applicants' preferences but also by their socioeconomic background and access to educational and other resources. This underscores the need for well-designed public policies to address the unequal access to high-paying majors and promote social mobility.

CRediT authorship contribution statement

Fernanda Estevan: Writing – review & editing, Writing – original draft, Supervision, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Thomas Gall:** Writing – review & editing, Writing – original draft, Investigation, Formal analysis, Conceptualization. **Louis-Philippe Morin:** Writing – review & editing, Writing – original draft, Methodology, Investigation, Funding acquisition, Formal analysis, Conceptualization.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.labeco.2025.102821.

Data availability

The data that has been used is confidential.

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