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IZA DP No. 15633

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ABSTRACT

The Effect of Preferential Admissions on the College Participation of Disadvantaged Students: The Role of Pre-College Choices*

Exploiting the randomized expansion of preferential college admissions in Chile, we show they increased admission and enrollment of disadvantaged students by 32%. But the intended beneficiaries were nearly three times as many, and of higher average ability, than those induced to be admitted. The evidence points to students making pre-college choices that caused this divergence. Using linked survey-administrative data, we present evidence consistent with students being averse to preferential enrollment, misperceiving their abilities, and having social preferences towards their friends (although social preferences did not mediate the admission impacts). Simulations from an estimated structural model suggest that aversion to the preferential channel more than halved the enrollment impacts, by inducing some to forgo preferential admission eligibility, and that students' misperceptions worsened the ability-composition of college entrants, by distorting pre-college investments into admission qualifications. The results demonstrate the importance of understanding high school students' preferences and beliefs when designing preferential admissions.

JEL Classification: I2, D8

Keywords: preferential college admission, experimental policy evaluation, subjective beliefs

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1 Introduction

Preferential admissions are widely used to increase enrollment of the socioeconomically disadvantaged into selective colleges (Black, Denning, and Rothstein, 2020; Kapor, 2020; Bleemer, 2021). Three features define preferential admissions. They offer students admission partly on the grounds of their socioeconomic circumstances (*social targeting*). They offer admissions only to the most academically talented in the group (*skill targeting*). And by reserving opportunities for some students in the group, they necessarily exclude others (*exclusion*). Each of these features could interfere with their effectiveness by shaping the pre-college choices that qualify students for an admission. Students may feel uncomfortable with the salience of their socioeconomic status in the admission offers, they may not know the level of effort required to meet the skill targets, and they may have social preferences towards the friends who are denied the college-going opportunities. While these features characterize all preferential admission policies for disadvantaged students, their implications for policy effectiveness are unknown. This paper is a first step towards closing this knowledge gap.

We study a Chilean policy called PACE (*Programa de Acompañamiento y Acceso Efectivo a La Educación Superior*), a percent plan. A popular alternative to race-based affirmative action (Horn and Flores, 2003), percent plans offer college admission to students from disadvantaged schools who graduate at the top of their high school. In 2016, the Chilean Government identified a set of high schools serving students from disadvantaged backgrounds and randomly assigned a subset to be in the PACE program. The experimental cohort was about to start eleventh grade (Figure A1). All students obtaining grades in the top 15% of treated high schools and taking the college entrance exam were offered guaranteed college admission.[1] We use data on the randomized assignment of PACE to high schools to evaluate its impacts, relaxing the identifying assumptions imposed by evaluation methods based on quasi-experiments or structural modeling.

PACE is a state-of-the-art college access policy that combines tools known to increase the college admission and enrollment of the disadvantaged. As in the pioneering Texas Top 10 percent plan, orientation classes are offered to all students in PACE schools, and nearly all students have a family income that qualifies them for a full tuition fee waiver. And indeed, in 2018, our sample year, PACE increased admissions of targeted disadvantaged students by 3.7 percentage points (p.p.) and enrollments by 2.7 p.p., increases of 32% compared with the control group. The admission effect was fairly stable across the ability distribution, with students from the top quintile

[1] Throughout the paper, with the term "college" we refer to colleges participating in the Chilean centralized admission system (SUA), which we describe in section 2.1.

of the baseline test score distribution experiencing similar admission effects as those from the bottom quintile.

But in the control group, students graduating in the top 15% of their school and not already being admitted to college, the intended beneficiaries, were nearly three times as many, and had, on average, higher baseline test scores, than those induced to be admitted by the policy.[2] Data from the pre-admission stage show that 10% of students graduating in the top 15% of treated schools did not become eligible for an admission under PACE because they did not take the entrance exam, and that pre-college effort (towards the college entrance exam and schoolwork) and achievement worsened as an effect of PACE, as did the ability composition of those graduating in the top 15% of their school (measured by their baseline test scores). Therefore, while PACE provided new opportunities for college access, it was students' pre-college choices that determined how many and which students it brought to college.

Individual preferences and different kinds of barriers to college access could have driven students' pre-college choices, limiting the policy's impacts on admissions. While we cannot explore all possible mechanisms behind the students' choices, our data allowed us to test for three that are plausible in light of the three features of preferential admissions: social targeting, skill targeting, and exclusion.

The first mechanism we consider is aversion to social targeting. Researchers have long recognized that participation in social programs can be hindered by "feelings of lack of self-respect" and stigma (Moffitt, 1983; Besley and Coate, 1992). Students may interpret a preferential admission as a lesser achievement, or take it as a signal of a low future standing in college. If preferential admissions carry a disutility, some students may choose to forgo them by not becoming eligible for them (in the PACE context, by not taking the entrance exam).

To identify such disutility, we exploited a unique feature of PACE: students could be admitted through the regular and the preferential channels simultaneously. We studied the enrollment choices of those who were admitted through both channels and enrolled, and found they were 40 p.p. less likely to accept a preferential admission over a regular admission to an equivalent program (i.e., that was similarly close to their high school, in the same subject, and of the same selectivity). This effect cannot be explained by lack of knowledge about the program or application costs and complexity, common reasons for the low take-up of social programs (Moffitt, 1992), nor by low preference for or barriers to college, because all students in this sample applied for a preferential admission and enrolled in college. Since turned-

[2] The term "beneficiaries" refers to the fact that these students can gain college admission through the policy (i.e., they can go from not being admitted to being admitted as an effect of the policy). It does not refer to a benefit in a welfare/utilitarian sense.

down preferential seats remain vacant, prosocial behavior cannot explain this effect either. Instead, we interpret it as evidence of aversion to being socially targeted, which could include fear of (public or internalized) stigma (Goffman, 1963), social image or belonging concerns (Bursztyn and Jensen, 2017; Walton and Cohen, 2011), and misperceptions about relative standing in college or about graduation chances. If such aversion is present not only among those admitted through both channels, but also among all targeted students, it could limit the effectiveness of preferential admissions in bringing disadvantaged students to college.

The second mechanism we consider is students' lack of knowledge about their skills, which may be particularly salient among disadvantaged students (e.g., Falk et al., 2020). Skill targeting implies that preferential admissions are offered only to students whose GPA exceeds a school-level cutoff. Since GPA is not a fixed trait, but rather an outcome that responds to study effort, not knowing one's distance from the cutoff can result in unintended consequences for effort.

To study this mechanism, we surveyed over 6,000 students in the experimental schools, elicited their beliefs, and linked their survey answers to administrative records. We found that students were, on average, overoptimistic about their entrance exam score and within-school rank. We also found that overestimating one's lead over the school-level cutoff was associated with lower admission effects, especially among the top-performing students, consistent with overoptimistic beliefs leading them to underinvest in effort. This can help explain why it was especially the stronger students who appeared not to take up the PACE opportunities. Rather than the outcome of a deliberate coices, this could have been a mistake. When students are misinformed about what effort investments are required to meet or maintain new skill targets, skill targeting could have unintended consequences for their pre-college effort investments, and for the resulting ability composition of admitted students.

The third mechanism we consider is social preferences. Percent plans exclude some students in favor of higher-ranked ones in the same school. Research on work in teams has shown that relative-reward schemes can reduce effort, as prosocial workers avoid imposing negative externalities on co-workers (Ashraf and Bandiera, 2018). Therefore, social preferences could have caused the negative effort effects of PACE.

To study this mechanism, we developed a simple tournament model with social incentives that builds on the seminal Lazear and Rosen (1981) and Bandiera, Barankay, and Rasul (2005) models. Two students in a school differ in terms of ability, and can be admitted to college through the regular channel, where there is no interdependence in payoffs. If they are in the treatment group, they can also be admitted through a rank tournament that awards admission to the student with the highest

GPA. Students choose how much effort to invest towards their GPA, and have social preferences, that is, their utility includes the payoff of their schoolmate.

A model's core result is that social preferences lead students in the treatment group who have a high likelihood of being admitted through the regular channel to attempt to lose the tournament, so as to leave space for college admission to their schoolmates who would not be admitted otherwise (through the regular channel). Therefore, in the treatment group we should observe a higher likelihood of being admitted to college through the regular channel among those who rank in the bottom 85% than in the control group. But this implication is not borne out in the data, regardless of whether we test it using the true or the perceived likelihood of a regular admission. Therefore, the evidence suggests that social preferences did not drive the students' response.

The model's first-order conditions provide an intuition for the result. For social preferences to drive behavior, students must believe their behavior can impose an externality on their peers by affecting their likelihood of admission. In a tournament, this occurs only when students are sufficiently close to the cutoff, i.e., when they are marginal for winning the tournament. But our survey data showed that most students did not believe they were marginal: they reported believing that bringing their GPA (up or down) to the within-school cutoff would require substantial and sustained changes in study habits. This can help explain why social preferences did not appear to be a widespread driver of behavior in our sample.

But social preferences could have driven behavior among friends, where they could be stronger (e.g. Bandiera, Barankay, and Rasul, 2005). We found that PACE induced high-ability students to lower their perceived GPA rank below that of their self-reported best friends, consistent with social preferences among friends. But the effect lacks statistical significance, consistent with the marginality argument: only marginal students whose best friends are also sufficiently close to the cutoff can impose an externality on them. The evidence, then, suggests that externalities that are localized, as in rank tournaments, can safeguard against widespread adverse effects of social preferences on effort. But they could still affect many people in tournaments that are perceived to be tight, unlike PACE. Therefore, for some scheme designs, social preferences could shape the impacts of preferential admissions in unintended ways.

To close the analysis, we developed a dynamic structural model that incorporates the channels for which we found evidence: aversion to social targeting and biased beliefs relevant to skill targeting. The model allows for preferences for and barriers to college as an additional, residual explanation; it does not posit that the two channels under study are the only possible ones. We estimated the model and separately identified the parameters governing the channels, using the novel longitudinal dataset we constructed, which links survey and administrative data and includes all model choices and outcomes, beliefs, student characteristics, and repeated skill measures.

The counterfactual simulations offer two insights. If students valued identical regular and preferential admissions in an identical way, more students would take the entrance exam, and the policy impacts on admissions would nearly double, and on enrollment, more than double. If the policy combined offering preferential seats with correcting biases in pre-college beliefs, it would improve the ability composition of those the policy induces to enter college by 0.12 standard deviations, by avoiding the distortions in pre-college effort investments that lead higher-ability students to miss out on admissions by mistake. Therefore, the channels driving pre-college choices may have non-negligible impacts on the effectiveness of preferential admissions.

The paper contributes to the literature on preferential admissions by showing that pre-college choices are endogenous to these policies, that they can determine how many and which students the policies bring to college, and that they are not always driven by pure preferences. A small, growing literature has studied the causal effects of preferential admissions on pre-college choices (Akhtari, Bau, and Laliberte (2020), Bodoh-Creed and Hickman (2019) and Golightly (2019) are among the first to study them in the United States). We add to it in two ways. First, we have constructed a new dataset that allows us to contribute new experimental evidence and to identify new plausible mechanisms behind the effects. The dataset combines a randomized experiment with rich longitudinal information on students around the transition from high school to college, including repeated skill measures and new survey measures. Second, we provide a structural model quantification that measures the importance of these mechanisms in driving a percent plan's impacts on the college participation of disadvantaged students. A novel finding is that information frictions and social concerns could drive pre-college choices and shape the success of preferential admissions in bringing disadvantaged talent to college. This result opens up promising new areas for future research and policy interventions that have not yet been studied systematically in the context of preferential admissions.

Our study findings are consistent with recent studies of financial aid for college attendance, which show that some disadvantaged students do not take aid up for reasons other than pure preferences (e.g. Bettinger et al., 2012; Hoxby and Turner, 2013; Dynarski et al., 2021). We show that a similar phenomenon could also operate in the different context of preferential admissions. The results, therefore, demonstrate the importance of understanding the preferences, social concerns and beliefs of disadvantaged high school students when designing these policies.

Findings from the literature on information frictions in education strongly suggest that the misperceptions we measured in our data can be corrected by providing information (Azmat and Iriberri, 2010; Bandiera, Larcinese, and Rasul, 2015; Wiswall and Zafar, 2015; Azmat et al., 2019; Owen, 2020; Larroucau et al., 2021; Arteaga et al., 2022). Bobba and Frisancho (2019), for example, show that providing information to middle-school students about their abilities affected their beliefs and high-stake education choices, improving their match to educational opportunities. Therefore, our model simulations provide a reasonable benchmark.

Much less is known, however, about interventions that could mitigate the effects of social concerns. A long tradition in social psychology, dating back to at least Goffman (1963), highlights the central role of stigma in shaping people's choices. More recently, Leslie, Mayer, and Kravitz (2014) showed it could worsen the college performance of affirmative action recipients. The economics literature has documented the related phenomenon of low take-up of social programs in various domains (Currie, 2003; Bhargava and Manoli, 2015; Deshpande and Li, 2019; Finkelstein and Notowidigdo, 2019; Bettinger et al., 2012). And social image concerns and stereotypes are known to affect economic behavior (Bursztyn and Jensen, 2017; Carlana, 2019). We add to this body of research by providing evidence of disadvantaged students behaving as if they dislike being treated preferentially, and quantifying the likely impacts of such behavior on the effectiveness of an education access policy. We do not yet know what mechanisms could be causing such disutility, what interventions could mitigate its effects, and their welfare implications. But given its potentially central role in limiting the impacts of preferential admissions, we make suggestions for future research.

The paper is organized as follows. Section 2 describes the Chilean college admission system and PACE policy, the randomization design, and the data. Section 3 presents the treatment effects on admissions, enrollments, and pre-college outcomes. Section 4 examines plausible mechanisms. Section 5 develops the model incorporating the empirically relevant mechanisms. Section 6 presents model estimates and counterfactuals. Section 7 concludes.

2 Context, Randomization and Data

2.1 Context and PACE Policy

Regular channel admissions. Selective colleges in Chile participate in a centralized admission system (*Sistema Único de Admisión*).[3] Students wishing to go to

[3] These colleges offer five-year (and longer) programs. They include the 23 public and private not-for-profit colleges that are part of the Council of Rectors of Chilean Universities (CRUCH) and 14 additional private colleges. Higher-education institutions outside this system do not have minimum admission requirements and provide vocational and shorter degrees. There is no enrollment gradient by socioeconomic status for these institutions.

college must take the PSU (*Prueba de Selección Universitaria*) standardized college admission exam. After observing their scores, they decide whether to submit an application to the system. Higher scores increase the likelihood of admission.

Introduction of PACE. In line with global statistics, college enrollment in Chile is unequal across socioeconomic lines. Students from families in the top income quintile are over three times more likely to enroll than students from families in the bottom income quintile (Figure G1 of the supplementary material). PACE was introduced to increase college admissions among disadvantaged students. The Government selected the schools to be targeted by PACE using the school-level vulnerability index (*Indice de Vulnerabilidad Escolar*), based on students' socioeconomic characteristics. Students in targeted schools are underprivileged: their 10^{th} grade standardized test scores are 0.62 standard deviations below the national average and their family income is half that of the average student (Table A1).

Admission rules under PACE. Students in high schools participating in PACE can apply to college through the regular channel. Moreover, they receive a guaranteed college admission if they satisfy three conditions. First, the grade point average in grades 9 to 12 must be in the top 15% of the high-school cohort.[4] Second, like in the Texas and California percent plans (Horn and Flores, 2003), the student must take the entrance exam, even though the score does not affect the likelihood of obtaining a PACE admission. When students decide whether to take the exam, they have not yet been told whether they have graduated in the top 15% of their school. Third, the student must attend the PACE high school continuously for the last two high-school years (eleventh and twelfth grade).

Other features of PACE. Optional tutoring sessions in college are available to those who enroll via PACE. As with the Texas percent plan, light-touch orientation classes (two hours per month on average) are offered to all students in PACE high schools. The classes cover the college application process and study techniques and often replace orientation classes (MinEduc, 2018).

PACE college seats are supernumerary: they do not replace regular seats but are offered in addition to them. Therefore, the introduction of PACE did not make it mechanically harder to obtain a regular admission. PACE seats span the same majors as regular seats and are of similar quality, as measured by the average entrance exam score of regular entrants into each college-major pair (Figure A2). A student can

[4] The central testing authority computes the score used to rank students, called *Puntaje Ranking de Notas* (PRN), by adjusting the raw four-year grade point average to account for the school context. The Pearson's correlation coefficient between the unadjusted four-year grade point average and the PRN is 97.44%. Details of how the score is calculated can be found at: https://demre.cl/psu/ /proceso-admision/factores-seleccion/puntaje-ranking.

obtain both a PACE and a regular admission. If a student does not accept a PACE admission, that PACE seat remains vacant.

2.2 Randomization and Balancing Tests

Randomization. The Government introduced the PACE program in 69 disadvantaged high schools in 2014 and later expanded it to more schools. In 2015, it identified 221 high schools that were not yet PACE schools, but that met the eligibility criteria for entering PACE in 2016, per students' socioeconomic status. Using a randomization code written by PNUD Chile (United Nations Development Program), it randomly selected 64 of the 221 eligible schools to receive the PACE treatment. The randomization was unstratified.

When a school first enters PACE, only the cohort of eleventh graders is entered into the program. The randomized expansion concerned the cohort who started eleventh grade in March 2016. Before starting the school year, students who were enrolled in schools randomly selected to be treated were informed their school was in the PACE program. This announcement was made after the school enrollment deadline; thus, we did not observe strategic selection into high schools (Appendix D). The control schools were not entered into the PACE program; they were not promised participation. Figure A1 illustrates the timeline. Grades in the first two high-school years (9 and 10) were already determined when students in treated schools were informed they were in a PACE school. But students who wished to affect their four-year GPA average had two school years to do so.

Sample and balancing tests. We collected data on the experimental cohort. We sampled all 64 schools the Government randomly allocated to treatment. For budget reasons, we randomly selected 64 of the 157 schools the Government randomly allocated to control. Table 1 presents the balancing tests for the 128 sampled schools using background information collected when the cohort was in the tenth grade. The students in treated and control schools did not differ significantly at baseline on gender, age, socioeconomic status (SES), academic performance or type of high school track attended (academic or vocational). Given the low SES, nearly all students in the sample, across treatment groups, were eligible for a full tuition fee waiver.

2.3 Data and Descriptive Analysis

Construction of linked administrative-survey data. Table 2 lists the administrative and primary data sources. We linked them through unique student, classroom

Table 1: Sample Balance Across Treatment and Control Groups

	Control (1)	Difference between Treatment and Control (2)	p-Value (Difference equals zero) (3)	N (4)
Female	0.476	0.001	0.988	9,006
		(0.054)		
Age (years)	17.541	0.031	0.561	9,006
		(0.052)		
Very-low-SES student	0.602	0.014	0.489	9,006
		(0.020)		
Mother's education (years)	9.553	0.081	0.631	6,000
		(0.168)		
Father's education (years)	9.320	0.115	0.517	5,722
		(0.178)		
Family income (1,000 CLP)	283.950	14.335	0.265	6,018
		(12.794)		
SIMCE score (points)	221.355	7.600	0.151	8,944
		(5.256)		
Never failed a year	0.970	-0.010	0.101	8,944
		(0.006)		
Santiago resident	0.140	0.051	0.482	9,006
		(0.073)		
Academic high-school track	0.229	0.055	0.451	9,006
		(0.073)		

Note.– Standard errors clustered at the school level are shown in parentheses. Very-low-SES student is a student that the Government classified as very socioeconomically vulnerable (*Prioritario*). SIMCE is a standardized achievement test taken in 10^{th} grade.

and school identifiers and built a longitudinal dataset that follows $9,006$ students for five years, from ninth grade to one year after leaving high school.

For all $9,006$ students enrolled in the 128 sampled schools, we obtained administrative information on baseline socioeconomic characteristics, baseline standardized test scores, school grades in high school (years 9 to 12), grade progression, college entrance exam scores, regular and PACE channel admissions and enrollments.

To complement the administrative data, we collected primary data in all 128 sampled schools between September and November 2017, when students were completing 12^{th} grade (Appendix C describes the fieldwork). Our primary data contain three main pieces of information. First, we measured pre-college achievement. As standardized achievement tests are not administered universally at the end of high school, we administered a 20-minute mathematics achievement test to all students (see Behrman et al. (2015) for a similar approach), developed for us by professional testing agencies. Without this skill measure, it would be difficult to estimate policy impacts on pre-college achievement: using the scores on the entrance exam could introduce selective attrition bias, because the decision to take the exam could be affected by the policy, and using GPA could give misleading results, because GPA is not comparable across schools. Second, we elicited study effort through the survey

Table 2: Overview of Data

Dataset	Variables	Collected	Source
1. *SIMCE*	Achievement test scores, background characteristics	Grade 10	Admin
2. *SEP*	Very-low-SES classification (*Prioritario* student)	Grade 10	Admin
3. School records 1	High-school enrollment	Grades 9-12	Admin
4. Student survey	Study effort, beliefs about self and others	Grade 12	Primary
5. Achievement	Achievement test scores	Grade 12	Primary
6. School records 2	GPA (overall and by subject), grade progression	Grades 9-12	Admin
7. Higher education records	Entrance exam (PSU) scores, applications, admissions, enrollments via regular channel, seat characteristics	After grade 12	Admin
8. PACE program records	Allocation of PACE seats, applications, admissions, enrollments via PACE channel, seat characteristics	After grade 12	Admin

Note. – SIMCE: *Sistema Nacional de Evaluación de Resultados de Aprendizaje*, SEP: *Subvención Escolar Preferencial.* Seat characteristics include the location, field of study, and selectivity of the seats to which each student is admitted.

instruments used in Mexican high schools by Behrman et al. (2015) and Todd and Wolpin (2018), complemented with questions on entrance exam preparation. Third, we elicited subjective beliefs about future outcomes and returns to effort.[5]

We surveyed 6,094 students, approximately 70% of those enrolled in the 128 sample schools. Attrition was not selective across the treatment and control groups (Appendix D). Our response rate compares favorably with that of ministerial surveys (MinEduc (2015, 2017)), and it reflects dropout in the last weeks of the last high school year (schooling is compulsory until then). We account for survey attrition in two ways. For the regression analyses, we built inverse probability weights using baseline administrative data. For the estimation of the structural model, we let the distribution of unobservable characteristics depend on whether a student was surveyed, to allow for survey-non-response based on unobservables.

Descriptive analysis. We describe the path to college of students in control schools to shed light on the college participation of the disadvantaged students targeted by PACE, in the absence of preferential admissions.

Around two thirds of students take the college entrance exam, which aligns nicely with our survey data, where a similar fraction reports preparing for it (first two rows

[5] We also surveyed mathematics and Spanish teachers and school principals. Tincani, Kosse, and Miglino (2021) describes the information we collected from them.

Table 3: Description of Choices and Outcomes in the Control Group

	Mean (1)	Std. Deviation (2)	N (3)
Took college entrance exam	0.655	0.475	4,231
Reports having prepared for college entrance exam	0.626	0.484	2,936
College entrance exam score | took exam (σ)	-0.602	0.611	2,773
Applied to college	0.210	0.407	4,231
College entrance exam score | applied to college (σ)	-0.171	0.595	887
Admitted to college	0.114	0.318	4,231
Not admitted to college and graduated in top 15% of school	0.102	0.303	3,909
Enrolled in college	0.085	0.279	4,231

Note. – Sample of students enrolled in the 64 control schools. σ is the standard deviation of PSU college entrance exam scores among all exam-takers in the country.

of Table 3). Even students with very low admission likelihoods prepare for and take the entrance exam (Figure A4). But, as the third row of the table shows, exam scores are well below the national average (-0.6 standard deviations). It is unsurprising, then, that upon observing their exam scores only 21.0% apply, those with higher scores (fourth and fifth rows). 11.4% of students are admitted and 8.5% enroll.

Around 10% of the sample were intended beneficiaries of PACE: they were not admitted to college but graduated in the top 15% of their school (second-last row of Table 3). We expect the average policy effect on admissions to be approximately 10 percentage points (p.p.) if all these students are admitted to college under PACE (i.e., nobody forgoes the possibility of a preferential admission by not taking the entrance exam) and if nobody who is admitted in the absence of PACE is not admitted in its presence (i.e., no negative admission effects). We call this the *mechanical admission effect.*

3 Experimental Evidence on Policy Impacts

To identify the policy impacts, we exploit the randomized assignment of schools to PACE, and estimate the following Probit model for the binary outcomes:

$$Pr(Y_{is} = 1|T_s, X_i) = \Phi\left(\tilde{\alpha} + \tilde{\beta}T_s + \tilde{\lambda}X_i\right), \tag{1}$$

and the following linear regression for the continuous outcomes:

$$Y_{is} = \alpha + \beta T_s + \lambda X_i + \eta_{is}, \tag{2}$$

where Y_{is} is the outcome of student i in school s, T_s is the treatment status of school s, and X_i is a vector of student i's baseline characteristics. The parameters of interest are the average marginal effects of treatment calculated from the Probit model estimates

$(\int_{X_i} Pr(Y_{is} = 1|T_s = 1, X_i) - Pr(Y_{is} = 1|T_s = 0, X_i)dF(X_i))$ and β from the linear models.[6] The standard errors are clustered at the school level.

Admissions and enrollments. Table 4 shows that students in schools randomly assigned to the treatment are 3.7 p.p. more likely to be admitted to college and 2.7 p.p more likely to enroll than students in control schools.[7] The effects are a 32% increase relative to the control group.

Table 4: Effect of Percent Plan on Admissions and Enrollments

	Admissions	Enrollments
	(1)	(2)
Treatment	0.037***	0.027**
	(0.012)	(0.011)
Observations	8,944	8,944
Pseudo-R^2	0.243	0.235

Note.– Average marginal effects from probit models. The delta-method standard errors are clustered at the school level. Controls: gender, age, indicator for very-low-SES student, baseline SIMCE test score, never failed a grade, and high school track (academic or vocational). *Treatment* is a dummy variable indicating whether a student is in a school that was randomly assigned to be in the PACE percent plan program. *p < 0.10; **p < 0.05; ***p < 0.01.

The admission effect is 64% below the mechanical one of 10.2 p.p. (defined in section 2.3). As shown in Figure 1, the admission effect is fairly stable across baseline ability: it increases with it only initially. The mechanical one, theoretically, could have increased or decreased with ability: higher-ability students are more likely to already be admitted to college without the policy (lowering the mechanical effect), but they are also more likely to graduate in the top 15% of their school (increasing the mechanical effect). Empirically, we find that it increases with ability. As a result, the gap between the mechanical and the actual admission effect widens with ability.

By construction, the gap is generated by treated students not taking the entrance exam, and by the distribution of pre-college achievements (GPAs and entrance exam scores) changing across treatment groups, leading to changes in the ability composition of those graduating in the top 15% and not being admitted to college through the regular channel. We study data from the pre-college stage next.

Pre-college effort, achievement and entrance-exam taking. Table 5 shows that students in treated schools perform 10% of a standard deviation worse than students in control schools on the standardized achievement test we administered. The result is robust to using item response theory to calculate the achievement score (Table G2 of the supplementary material), and to using Lee (2009) bounds (Appendix D).

[6] Estimating linear models for the binary outcomes gives treatment impacts similar to those estimated via Probit models.

[7] See also the companion policy reports (Cooper et al., 2022, 2019).

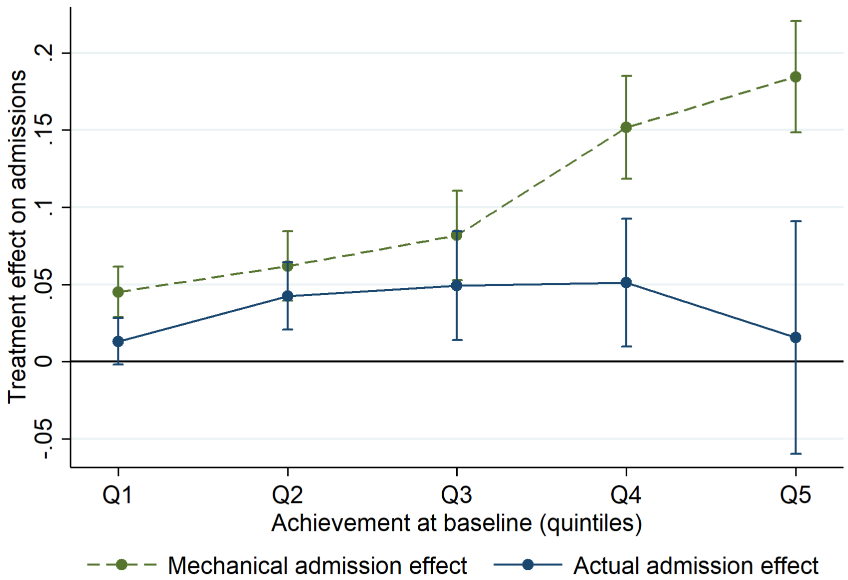

Figure 1: Admission effects gap. The figure compares the treatment effect on college admissions with the mechanical admission effect defined in section 2.3 at different points of the baseline ability distribution. Estimates of the actual treatment effect are average marginal effects from probit models with the standard set of controls estimated on different quintiles of the baseline SIMCE test score distribution. Mechanical admission effects trace the distribution of intended beneficiaries in the control group. They are obtained as the fraction of control students in each baseline SIMCE quintile who were not admitted to college and who graduated in the top 15% of their school. 95% confidence intervals are obtained from standard errors clustered at the school level.

Columns (3) and (4) show that the treatment had a negative average effect on study effort of 9% of a standard deviation, also robust to using Lee (2009) bounds (Appendix D). The effect is driven by a reduction in study effort towards schoolwork inside and outside the classroom and in entrance exam preparation (Table A2).[8] Column (5) shows that the policy had no significant effect on the proportion of students taking the entrance exam. This is surprising, because while the monetary and non-monetary costs of taking the exam did not change across treatment groups, its benefits were higher in the treatment group, where it could potentially lead to a preferential admission. In fact, of those graduating in the top 15% in treated schools, around 10% did not take it, forgoing the possibility of a preferential admission.

To complement the analysis, we collected grade information and found that students in treated schools achieve lower grades on the subjects tested on the PSU entrance exam, and equal grades on those not tested (Table A3). Therefore, in response

[8]Tincani, Kosse, and Miglino (2021) validates the achievement and effort measures, showing they can independently predict high-stake outcomes such as admissions, enrollments and persistence in college.

Table 5: Effect of Percent Plan on Pre-College Choices and Outcomes

	Achievement Score		Study Effort Score		Entrance-exam taking
	(1)	(2)	(3)	(4)	(5)
Treatment	-0.104^{**}	-0.099^{**}	-0.086^{**}	-0.088^{**}	-0.036
	(0.050)	(0.050)	(0.039)	(0.038)	(0.027)
Inverse Probability Weights	No	Yes	No	Yes	No
Observations	6,054	6,054	5,631	5,631	8,944
R^2	0.260	0.259	0.047	0.047	0.094

Note.– The coefficients are OLS estimates in columns (1) to (4) and the average marginal effect from a probit model in column (5). Standard errors were clustered at the school level; delta-method used in column (5). The standard set of controls (see notes in Table 4) were used. Field-worker fixed effects were used for columns (1) to (4). Pseudo-R^2 shown in column (5). *Treatment* is a dummy variable indicating whether a student is in a school randomly assigned to be in the PACE program. The outcome variable in columns (1) and (2) is the number of correct answers on the achievement test, standardized. The outcome variable in columns (3) and (4) is the standardized score predicted from the principal component analysis of the eight survey instruments reported in Table A2 of the Appendix. *p $<$ 0.10; **p $<$ 0.05; ***p $<$ 0.01.

to the treatment, students reduced study effort on PSU exam preparation and exam subjects. But they did not reallocate effort toward other subjects.[9]

Finally, since the admission effects gap increases with baseline ability, we examine whether the treatment lowered the ability composition of those graduating in the top 15%, which could help explain the gap's shape. Such a change can occur when pre-college investments respond endogenously to the policy, as they did. Table A4 in Appendix A shows that, indeed, students with higher baseline test scores are less likely to graduate in the top 15% in treated compared to control schools.

Discussion. The evidence on admission effects and pre-college outcomes is consistent with students making pre-college choices that compressed the admission effect, which was 64% lower than the mechanical one, and that worsened the ability composition of the beneficiaries.[10] A policy-relevant question, one that we seek to answer next, is what drove such choices.

4 Mechanisms

4.1 Aversion to Social Targeting

Researchers have long recognized that participation in social programs can be hindered by "feelings of lack of self-respect and negative self-characterizations" (Moffitt, 1983). Students may perceive a preferential admission as a lesser achievement, and choose

[9]Course selection in high school is not a possible margin of policy response because students could not select courses.

[10]We focus on students' choices because survey and administrative data described in the supplementary material and in Tincani, Kosse, and Miglino (2021) rule out that teachers' effort, focus of instruction, grading and support classes played a major role. See Tables G4, G5, G6, G7 and section G3 in the supplementary material.

to forgo it by not taking the entrance exam. We refer to this as aversion to social targeting, a disutility from the preferential nature of an admission offer. Our data do not allow us to identify the exact mechanisms underpinning such disutility. For example, we cannot separate fear of stigma from misperceptions about the graduation likelihood or social image concerns. But we can identify the reduced-form disutility separately from several other reasons for rejecting preferential seats.

Disutility from participating in a social program is difficult to disentangle from lack of knowledge about the program and from application costs and complexity, since they all lead to low take-up (Moffitt, 1992). To avoid such confounders, we focus on students admitted to college through both the preferential and the regular channel, who eventually enroll. Since they all applied for a preferential admission, such confounders cannot explain low acceptance of preferential seats. Since they all enrolled, we can also exclude low preference for college and barriers to college attendance. And since turned-down preferential seats remain vacant, prosociality cannot explain this choice either.

But how often these students choose the regular over the preferential enrollment could give a misleading picture of aversion to social targeting if regular seats are different from preferential seats.[11] Therefore, we collected the universe of administrative records on the programs (college-major pairs) to which they were admitted. For each program, we recorded the location, field of study and quality, measured by its selectivity (calculated as the lowest entrance exam score among all regular entrants into that program in 2018). We find that programs across admission channels are in similar locations and fields of study, but they differ substantially on selectivity.[12]

Figure 2 plots the choice of enrollment channel of the students in this sample against the selectivity of the preferential and regular seats they were offered.[13] The figure looks very similar if we restrict the sample to those admitted to the same major or field of study across channels, or if we restrict it to those admitted to programs in the same province or region across channels. The figure shows that most students enroll through the regular channel (see also Table A6), even though on average they are admitted to significantly more selective programs through the preferential channel (Table A6 shows that the difference in selectivity is 16.3 points on the entrance exam ($p < 0.01$), or 0.36 standard deviations of the distribution of selectivity in the full

[11] The full tuition fee waiver applies to both the regular and the preferential channel, therefore, differences in tuition fees across channels are not candidate explanations.

[12] Regular and preferential admissions are equally likely to be from the same province (51%) and from the same region (75%) in which the student went to school. 79% (63%) of admissions are in the same field of study (major) across channels.

[13] We exclude from the sample those admitted to the same program through both channels, because in this case the enrollment channel was chosen by administrators. Therefore, it cannot be used to infer student preferences.

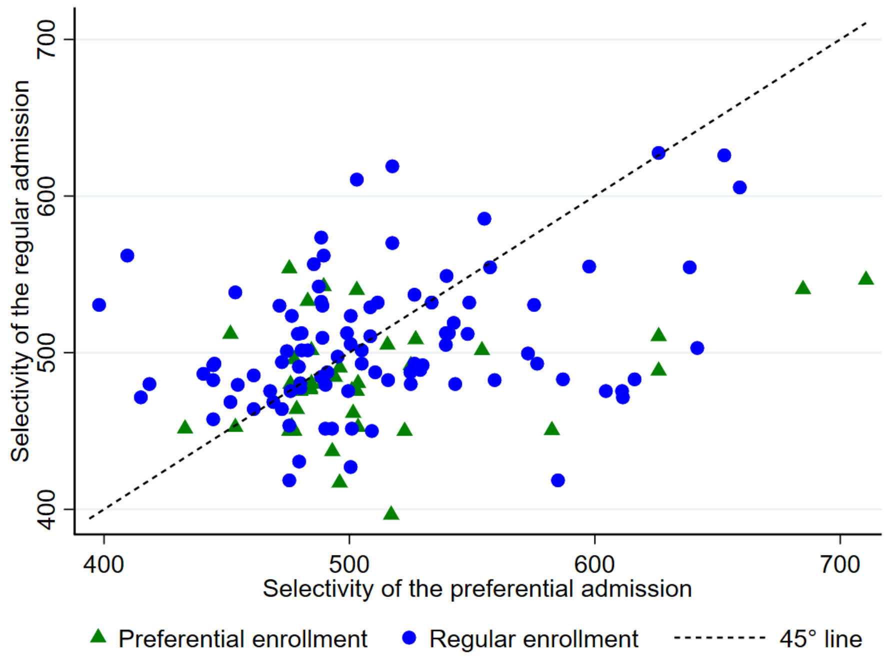

Figure 2: Choice of enrollment channel among students admitted through both channels. The selectivity of a program is measured as the lowest entrance-exam score among all regular entrants into that program.

sample). The figure also shows that students choose the regular seat more often not only when it is of higher quality than the preferential seat (above the 45-degree line), but also when it is of lower quality (below the 45-degree line).[14]

We next perform a regression analysis that allows us to quantify how much less likely students are to accept the preferential over the regular admission, when the characteristics of the programs to which they are admitted are kept constant across channels. We build a dataset where each observation is a student and admission pair (i.e., the regular and the preferential admission of student i are two separate observations), and estimate the following regression model:

$$Y_{ic} = \alpha + \delta P_{ic} + \lambda W_{ic} + \epsilon_{ic} \tag{3}$$

where Y_{ic} is equal to 1 if student i accepts the offer received through channel c, and 0 if he or she accepts the offer received through the other channel, P_{ic} is a dummy variable equal to 1 if the observation refers to an offer through channel c, and 0 otherwise, and W_{ic} is a vector of offer characteristics (quality measured by the lowest entrance exam score among regular entrants, and distance proxied by whether the university campus is in the same province as the student's high school). To control for field of study, we

[14] 84 percent choose the regular over the preferential seat above the 45-degree line; 60 percent choose the regular over the preferential seat below the 45-degree line.

estimate the regression in a restricted sample in which the major is identical across admission channels. The parameter of interest is δ, which captures, in percentage points, how much more likely a student is to accept the preferential over the regular offer, keeping fixed program characteristics. The results are reported in Table 6 and show that, keeping the location, quality and field of study of the programs constant, students are 40 p.p. less likely to accept a preferential admission over a regular one (column (4)).

Table 6: Effect of the Preferential Nature of an Admission offer on the Likelihood to Accept It

	Accept (1)	Accept (2)	Accept (3)	Accept (4)
Preferential	-0.324***	-0.399***	-0.405***	-0.399**
	(0.116)	(0.113)	(0.113)	(0.148)
Selectivity	No	Yes	Yes	Yes
Location	No	No	Yes	Yes
Major	No	No	No	Yes
Observations	290	274	273	168
R^2	0.105	0.159	0.163	0.160

Note. – Sample of students admitted to college through both channels, and who enroll. Estimates of parameter δ in equation (3). Each observation is a student and admission pair (e.g., the regular and the preferential admissions of student i are two separate observations). Therefore, the outcome variable identifies the choice to accept that admission (e.g., if student i chooses the regular channel, the outcome variable is 0 for the preferential admission of student i and 1 for the regular admission of student i). The rows labelled "Selectivity" and "Location" indicate whether the admission selectivity (i.e., the minimum entrance exam score among all regular entrants) and admission location (i.e., whether it is in the same province as the high school) were added as controls. Selectivity is missing for eight students. The row labelled"Major" indicates whether we restrict the sample to students who are admitted to the same major across channels. Standard errors clustered at the school level are in parenthesis. *p $<$ 0.10; **p $<$ 0.05; ***p $<$ 0.01.

Therefore, students do not shy away from selective seats; they shy away from preferential ones. *Ceteris paribus*, they value a preferential admission less than a regular one, and are willing to give up on program quality so as to avoid enrolling preferentially. We interpret these findings as evidence of an aversion to social targeting. If such aversion characterizes not only students admitted through both channels, but more broadly students in PACE schools, it could be a reason for the lower-than-expected admission effects.

4.2 Subjective Beliefs Relevant to Skill Targeting

Preferential admissions introduce new skill targets based on pre-college achievement. Since achievement responds to study effort, not knowing how one's skills compare to the targets for an admission can result in unintended consequences for effort and, ultimately, admissions. Biased self-assessment is common in many environments (Burks et al., 2013). As it has been documented among disadvantaged students (e.g., Stinebrickner and Stinebrickner, 2012; Falk, Kosse, Schildberg-Hörisch, and Zimmermann, 2020), it might be especially relevant in the context of preferential admissions.[15]

To understand the effort response to the policy, we first examine effect heterogeneity along baseline within-school rank and baseline ability. We split the sample into quintiles of baseline ability and baseline within-school rank, and estimate the regression from equation (2), used to estimate the average effects of the policy, on each sub-sample. The results are reported in Figure 3. We do not find statistically significant evidence of encouragement effects, that is, of positive effects on pre-college effort or achievement. If students had rational expectations, we would have expected evidence of positive impacts among low ability students and those close to the within-school cutoff. For them, the policy put a previously unattainable admission within reach, increasing the incentives to invest in pre-college effort. A potential reason for not finding effects expected under rational expectations is that beliefs are systematically biased. Therefore, we examine students' beliefs next.

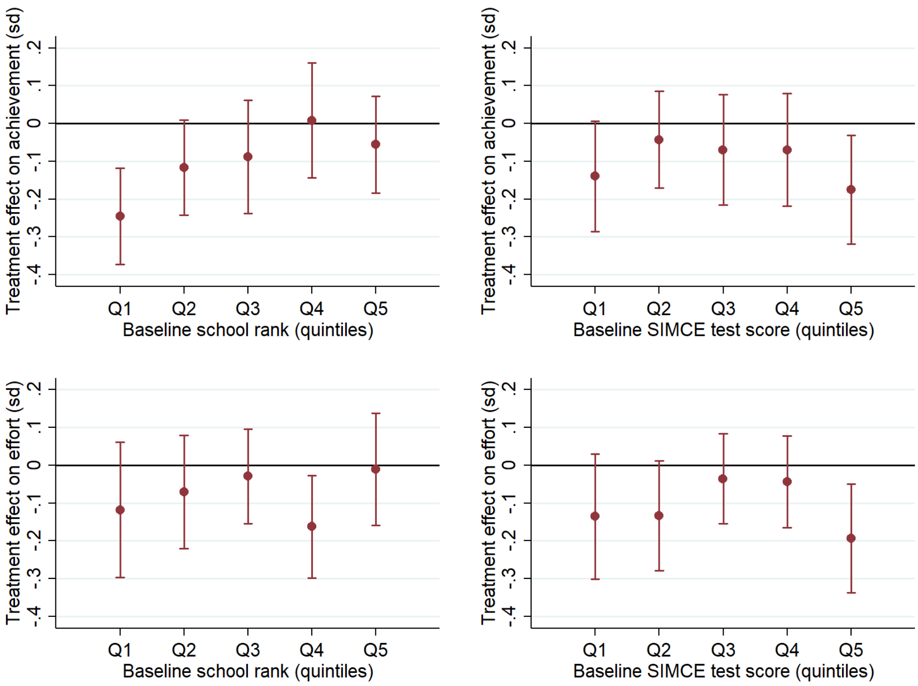

Figure 3: Heterogeneity of policy effects on pre-college effort and achievement. Notes: Each dot is the coefficient on *Treatment* from an OLS regression where: *Treatment* is a dummy variable indicating whether a student is in a school that was randomly assigned to be in the PACE program, the controls are the standard set of controls (see Table 4), Inverse Probability Weights and field-worker fixed effects are used, the estimation samples are quintiles in the within-school rank based on 10^{th} grade GPA (left panel) and quintiles in the distribution of 10^{th} grade standardized test scores (right panel). The units of measurement of the treatment effects are standard deviations. The bars are 95% confidence intervals built using standard errors clustered at the school level.

[15]Falk, Kosse, Schildberg-Hörisch, and Zimmermann (2020) show that overconfident self-assessment is pronounced for students with lower SES. They experience less intense interactions in their social environment, receive less feedback, and, therefore, learn less about themselves.

Description of students' beliefs. The belief data confirm that students have biased beliefs about relevant skill targets. Table 7 shows that students display large overoptimism over their PSU entrance exam score (first two lines), on average expecting a score that is 0.6 standard deviations above the score they actually obtain. Students also display large overoptimism about their within-school rank, with over 40% believing that their GPA is in the top 15%. Such relative-rank bias is due to misperceptions about others: students hold accurate beliefs about their own GPA (GPA is measured on a scale from 1 to 7 and on average the GPA students expect differs from the one they obtain by less than 0.1 GPA points), but, as they are never given relative feedback, they have a small belief bias about the 85^{th} GPA percentile in their school, of less than half GPA point (fourth row of the Table). The small belief bias in absolute terms translates into a large belief bias in relative terms because of strong grade compression (see Figures G2 and G3 of the supplementary material).[16]

Figure A5 shows that students of all ability levels are overoptimistic; table A5 shows that belief biases do not vary systematically by socioeconomic background in our homogeneously disadvantaged sample. The findings align with existing evidence that overoptimism is widespread in many contexts, including education (Stinebrickner and Stinebrickner, 2014).[17]

Table 7: Description of Subjective Beliefs

	Mean (1)	Std. Deviation (2)	N (3)
Believed entrance exam score (σ)	-0.033	0.920	2,413
Believed minus actual entrance exam score | took exam (σ)	0.591	0.916	1,853
Believed minus actual 12^{th} grade GPA (GPA points)	-0.075	0.552	2,558
Actual minus believed top 15% cutoff in school (GPA points)	0.401	0.854	3,326
Believes is in top 15% of school	0.431	0.495	2,469

Note. – Sample of students enrolled in the 64 control schools. This table is based on linked survey-administrative data: we elicited students' beliefs and linked their survey answers to actual outcomes. σ is the standard deviation of PSU entrance exam scores among the population of exam-takers. GPA is a number between 1.0 and 7.0. We define a student as believing she is in the top 15% of her school if her believed GPA is above her believed top 15% cutoff. Appendix Figure A3 contains an English translation of the survey instruments we used to elicit the beliefs reported in this Table.

Students on average believe they are high ability and high rank, which is exactly the student type for whom we would expect effort reductions under rational expectations. Believing you are high ability can lead you to perceive a regular admission as within reach, and study for the entrance exam when the policy is not in place (something most students in the control sample do, as per Table 3). Additionally believing

[16] Tincani, Kosse, and Miglino (2021) shows the predictive validity of the belief measures using high-stake outcomes up to 18 months after the survey.

[17] We have also collected beliefs about returns to effort, which we describe in section 5.2. As actual returns to effort are not directly observed in the data, we do not include them in this section, which describes errors in beliefs.

you are high rank can lead you to perceive a preferential admission as guaranteed, and reduce effort when the policy is introduced. Therefore, the data are consistent with students choosing effort based on their beliefs. But if beliefs affect the accumulation of the skills targeted by the admission rules, we should observe that admission effects vary with students' beliefs. We test this hypothesis next.

Testing for belief biases as a channel behind policy impacts. Our data allow us to explore the role of biased beliefs for admission effects only to a limited extent. We cannot estimate admission effects as a function of both belief biases (about the within-school GPA rank and the entrance exam), because the belief bias about the entrance exam is only available for students who took the exam, which could introduce selective-attrition bias.[18] Instead, we estimate how admission effects vary with the belief bias about within-school rank only, which is measured for everyone.

A second limitation is that there is no baseline measure of belief bias. Instead, we use the difference between the within-school 85^{th} GPA percentile at end-line and the expectation about this value (measured shortly before the end of high school). Treatment has no effect on this measure of belief bias ($p = 0.558$). Positive values indicate that someone thinks that the cutoff is easier to reach than it actually is (overoptimism). Negative values indicate that someone thinks that the cutoff is harder to reach than it actually is (overpessimism). Zero indicates an accurate belief about the cutoff (realism). As noted, on average, students are overoptimistic as they think the threshold is 0.401 grade points lower than it is ($p < 0.01$, row four of Table 7).

With these caveats in mind, we now test whether admission impacts vary with students' rank belief biases. If such biases drove choices, we would expect the treatment impacts to vary both with the belief bias and with the student's actual baseline rank, since the same belief bias translates into different over- or under-estimations of one's distance from the admission cutoff for students with different baseline ranks. We would also expect the treatment impacts to vary non-linearly with a student's perceived distance from the admission cutoff, since perceiving a small distance can induce encouragement effects that perceiving a large distance (in either direction) cannot. To capture both of these effects, we use the following Probit model:

$$Pr\left(Y_{is} = 1 | T_s, G_i, B_i\right) = \Phi\left(\sum_{b=0}^{\bar{B}} \sum_{g=0}^{\bar{G}} \beta_{bg} B_i^b G_i^g + \sum_{b=0}^{\bar{B}} \sum_{g=0}^{\bar{G}} \delta_{bg} B_i^b G_i^g T_s\right), \quad (4)$$

[18] Nevertheless, descriptive evidence from the control group is consistent with beliefs about the entrance exam affecting regular admissions. For high-ability students, who are near the admission cutoff, overoptimism is associated with *lower* admissions, consistent with it leading them to perceive an admission as easier to obtain than it is, and under-investing. For low-ability students, who are further below the admission cutoff, overoptimism is associated with *higher* admissions, consistent with it leading them to perceive an admission as more within reach than it is, and over-investing.

where Y_{is} is the admission of student i in school s, B_i is i's belief bias about the within-school rank, G_i is i's baseline within-school rank, T_s is the treatment status of school s, and $\bar{B}$ and $\bar{G}$ are the orders of the polynomials used to capture the non-linearities, which we choose through an information criterion.[19]

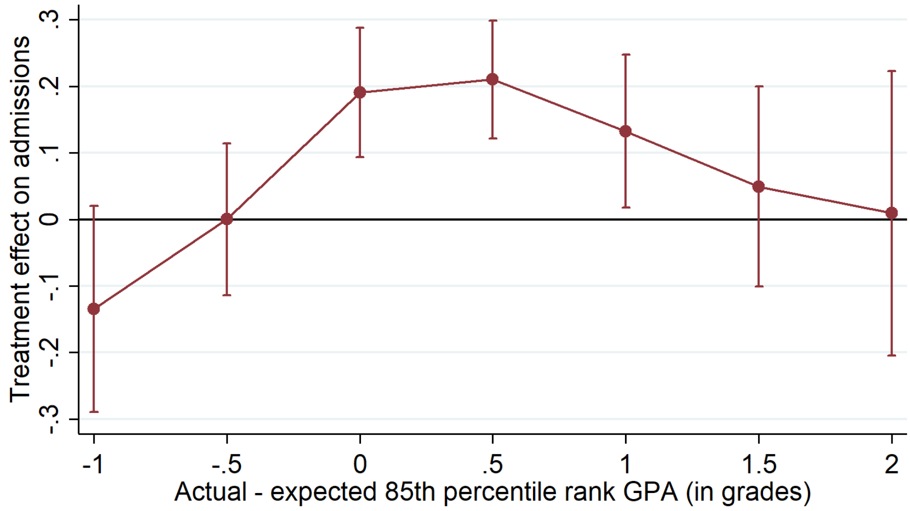

Figure 4: Treatment effect on admissions by belief bias. The figure shows treatment effects on admissions at different points of the belief bias distribution. Treatment effects are predicted for those at the 85^{th} GPA percentile at baseline. Positive values on the x-axis indicate that a student overestimates his or her lead over the cutoff (overoptimism); negative values indicate that he or she does not realize how close he or she is to the cutoff (overpessimism). Error bars are 95% confidence intervals (based on standard errors clustered at school level). Treatment effect estimates are marginal effects computed from the estimates of the Probit model in equation (4), with $\bar{B} = 4$ and $\bar{G} = 3$ (the orders of the polynomials are chosen using Akaike's information criterion).

Figure 4 shows results from the estimation of equation (4). It plots the (average) effect of treatment on admissions as a function of the rank belief bias, for students who, at baseline, are at the admission cutoff (the 85^{th} GPA percentile in their school). If they are overoptimistic about the cutoff, they are overestimating their lead over it (positive values on the x-axis). If they are overpessimistic about the cutoff, they are overestimating how far below it they are (negative values on the x-axis).

If beliefs did not drive behavior, we would expect the relation to be flat. If, instead, beliefs mattered, then we would expect, first, overpessimistic students to be admitted at a lower rate because, overestimating how far below the cutoff they are, they would take the entrance exam at a lower rate than realistic or moderately optimistic students. This is exactly what we find: a significantly lower treatment effect among overpessimistic students compared to the realistic or moderately optimistic ones (belief bias of -1.0 vs 0.5: $p = 0.000$). Second, we would expect overoptimistic students to be admitted at a lower rate because, overestimating their lead over the cutoff, they would not invest as much as realistic students to keep it. This is exactly what we

[19]The OLS regression model gives similar results to those from the Probit model.

find: a significantly lower treatment effect among very overoptimistic students compared to more realistic ones (belief bias of 2 vs. 0.5: $p = 0.072$). Finally, as noted, we would expect these effects to vary with a student's baseline rank. For those well above the cutoff, overoptimism should not lower the admission impacts, except for extreme overoptimism leading to extreme effort reductions. Similarly, overpessimism should not decrease their admission impacts, as we do not expect it to lead them to believe an admission is out of reach. For those well below the cutoff, we do not expect belief biases in any direction to shape admission impacts. These are the patterns we find in the data, as Figure A6 shows.

These findings can help explain why the admission effects gap was larger for those with higher baseline test scores. While belief biases are widespread, especially in the form of overoptimism, we have found them to be especially consequential for those towards the top of the baseline within-school GPA distribution. As those with larger baseline test scores are more likely than lower-ability students to belong to this category, they are more likely to miss out on admissions by mistake. The evidence in this section, therefore, is consistent with belief biases being a channel behind the impacts of PACE.

4.3 Social Preferences and the Exclusion Feature

PACE has an exclusion feature: if a student gets into the top 15% of the school in terms of GPA, another one must come out. Research shows that when people are rewarded for how their output relates to that of their peers, social preferences can lead them to lower their effort if it reduces the payoffs of others (Bandiera, Barankay, and Rasul, 2005; Ashraf and Bandiera, 2018).[20] While the negative effort effects of PACE were partly driven by reductions in entrance exam preparation, which cannot be explained by social preferences, they were also partly driven by reductions in effort towards schoolwork (Table A2), which could be explained by social preferences because PACE introduced a GPA-based interdependence between payoffs that is not present in the control group. We now study the role of social preferences in shaping PACE's impacts.

We develop a simple model that builds on the seminal Lazear and Rosen (1981) tournament model and allows for social preferences, as in the seminal Bandiera, Barankay, and Rasul (2005) social incentives model. While simple, the model offers powerful insights that allow us to test for the social preferences hypothesis. Two students in a school can be admitted to college through the regular channel, where

[20] With social preferences we mean the reduced-form representation of preferences-regarding-others in the utility function. We do not distinguish between collusive and altruistic motives.

there is no interdependence in payoffs: the admission likelihood depends on a student's ability. For simplicity, we assume that student 1 is high ability, and always admitted, and student 2 is low ability, and never admitted. If they are in the treatment group, they can also be admitted through a rank tournament that awards an admission to the student with the highest GPA, which, like in Lazear and Rosen (1981), is a stochastic function of effort. Students choose how much effort e_i to invest into improving their GPA: $y_i = e_i + \epsilon_i$, $i = 1, 2$, and face different costs of such effort, $c_i(e_i)$. Under standard regularity assumptions on the distribution $G(\cdot)$ of the shock $\epsilon_1 - \epsilon_2$, i's likelihood of winning the tournament is $G(e_i - e_j)$. Letting P_i denote i's likelihood of being admitted to college through at least one channel (regular or preferential) and $W_i > 0$ i's valuation of college, the utility function is

$$u_i = P_i \cdot W_i + \alpha e_i - c_i(e_i) + \pi(P_j \cdot W_j + \alpha e_j - c_j(e_j)) \quad \text{with } i \neq j, \tag{5}$$

where αe_i, $\alpha > 0$, is the utility from effort e_i (students value human capital, accumulated through effort). The parameter π captures a social preference. Whenever i's effort affects j's admission likelihood, student i takes this externality into account when choosing effort if $\pi \neq 0$, as can be seen from the model's first order condition for student 1, whose effort can reduce the likelihood that the lower-ability schoolmate is admitted to college:

$$\alpha - \frac{e_1}{c_1} - \pi W_2 g(e_2 - e_1) = 0. \tag{6}$$

In Proposition 2 in Appendix E.1 we provide conditions on the shocks' distribution that are sufficient for the existence and uniqueness of the equilibrium. In Proposition 1 in Appendix E.1 we derive a key testable implication. Specifically, social preferences lead students in the treatment group who have a high likelihood of being admitted through the regular channel (student 1 in the model) to attempt to lose the tournament, so as to leave space to their lower-ability schoolmates (student 2 in the model). Therefore, in the treatment group we should observe more students being admitted to college through the regular channel among those who graduate below the school-cutoff (that qualifies for preferential admissions in treated schools) than in the control group. To test this model prediction, we compute, from the treatment and control groups, the fractions being admitted to college through the regular channel among those who graduate in the bottom 85% of the school. Columns (1) and (2) of Table 8 report estimates for their difference. The point estimates are very small, and they are either insignificant or significantly *negative*, not positive, rejecting the model prediction. We reach a similar conclusion if we use the perceived instead of the actual likelihood of a regular admission (columns (3) and (4)), suggesting that social preferences did not operate jointly with belief biases either.[21]

Table 8: Testing Proposition 1: Across-groups Difference in Admissions among the Low Ranking

	Admissions	Admissions	Perceived Admissions	Perceived Admissions
	(1)	(2)	(3)	(4)
Treatment	0.014	-0.017^{*}	-0.030^{*}	-0.042^{***}
	(0.039)	(0.010)	(0.016)	(0.013)
Controls	No	Yes	No	Yes
Observations	7,008	6,970	4,716	4,691
Pseudo-R^2	0.001	0.224	0.003	0.062

Note.– Sample restricted to students who graduate in the bottom 85% of their school. The outcome variable in columns (1) and (2) is the regular admission dummy, in columns (3) and (4) it is the perceived likelihood of a regular admission, elicited through the question: "How sure are you that, if you take the entrance exam, your score will be sufficiently high to be admitted to a selective college (450 or more)?", with five possible answers ranging from "entirely sure that it will not" to "entirely sure that it will", to which we assigned numerical values between 0 and 1. Columns (1) and (2) report average marginal effects from probit models. Columns (3) and (4) report OLS estimates. "Controls" refers to the standard set of controls (see notes to Table 4). *Treatment* is a dummy variable indicating whether a student is in a school that was randomly assigned to be in the PACE percent plan program. Standard errors clustered at the school level are in parenthesis. *p < 0.10; **p < 0.05; ***p < 0.01.

The model and survey data provide a plausible explanation for why we fail to find the evidence we would expect if social preferences drove behavior. The first-order condition in equation (6) shows that social preferences influence behavior only when students can affect the admission likelihood of others (the term $g(e_2 - e_1)$ multiplies the social preference parameter π). In the model, this requires that the shock to the GPA has a sufficiently large support (the derivation of this condition from the first-order conditions is in Appendix E.1). The large support assumption means that students of any ability can affect the admission likelihood of others, even those who are not marginal for a preferential admission. But if this strong assumption is violated, students who are not marginal for a preferential admission, for example, because they are well above or well below the cutoff, cannot impose an externality on others. Even if they had social preferences, such preferences would not influence their behavior.

Our survey data reveal that most students believe that they are far from the cutoff. They believe that bringing their expected GPA (up or down) to the preferential admission cutoff requires changing their study habits by 9.3 hours per week on average,

[21] On the other hand, the evidence from columns (3) and (4) is consistent with belief biases driving choices and outcomes on their own. Many students perceive a preferential admission as easier to obtain than it is, and many lower their effort towards the entrance exam. It is not surprising, then, that students in the treatment group expect a lower subjective probability of a regular admission. That the estimates in columns (3) and (4) are lower than those in columns (1) and (2) suggests that students overestimate the true reduction in regular admission chances. This occurs if students over-estimate the returns to effort in securing a regular admission, something for which, indeed, we find evidence in section 6.1.

over twice the average weekly study hours in the sample; 90% of students believe it requires a change of at least one hour per week, over a quarter of average weekly study hours.[22] Such substantial and persistent changes in study habits suggest that most students do not think they are marginal for a preferential admission. The survey evidence, therefore, helps rationalize why a key implication of the social preferences hypothesis is not borne out in the data.

Nonetheless, social preferences may have driven behavior among friends. Research shows that people internalize externalities more when the externality hurts or benefits their friend rather than other peers (e.g. Bandiera, Barankay, and Rasul, 2005). We use data on self-reported friendships to examine whether PACE changed students' perceived rank relative to their best friend.[23] Although we do not observe the baseline ability or the admission outcome of the best friend, under sufficiently strong social preferences we expect that high-ability students, who have a high likelihood of progressing through the regular channel, are more likely to rank below their best friends in the treatment than in the control group. The left part of Figure 5 indeed suggests that PACE induced high-ability students to lower their perceived GPA rank below that of their best friends, consistent with strong social preferences among friends.[24] The estimates in the right part of Figure 5, however, indicate that the effects lack statistical significance, consistent with the marginality argument: only marginal students, whose best friends are also sufficiently close to the cutoff, can impose an externality on them.

We interpret the evidence as suggesting that while students may have social preferences towards their friends, social preferences are unlikely to have mediated the impacts of PACE, because the externalities embedded in the tournament were perceived to be localized (that is, PACE was not perceived to be a tight tournament). This may have safeguarded against widespread adverse effects of social preferences on effort. We caution, however, that localized externalities are a feature only of rank-based schemes, and that even localized externalities could affect many people in tournaments that, unlike PACE, are perceived to be tight. Therefore, the evidence of social preferences among friends suggests that social preferences could shape the

[22]These calculations are based on perceived returns to effort, which we describe in detail in section 5.2. They are based on the survey questions described in section 5.2 and Table A7.

[23]We combine two survey question to compute the perceived rank with respect to the best friend. One question elicits the belief about own GPA (it is reported in Figure A3 in Appendix B). The other translates into English as: *Thinking of the schoolmate with whom you meet the most to play sports or for other recreational activities, what do you think his/her GPA is this year?* A student is considered as believing he/she ranks above the best friend if he/she expects a GPA above the GPA he/she expects for the best friend.

[24]A limitation of this test is that we do not know the identity or baseline characteristics of a respondent's best friend. If the treatment had a direct effect on friendship formation, the differences across treatment groups in Figure 5 would reflect the combined effect of friendship and rank changes.

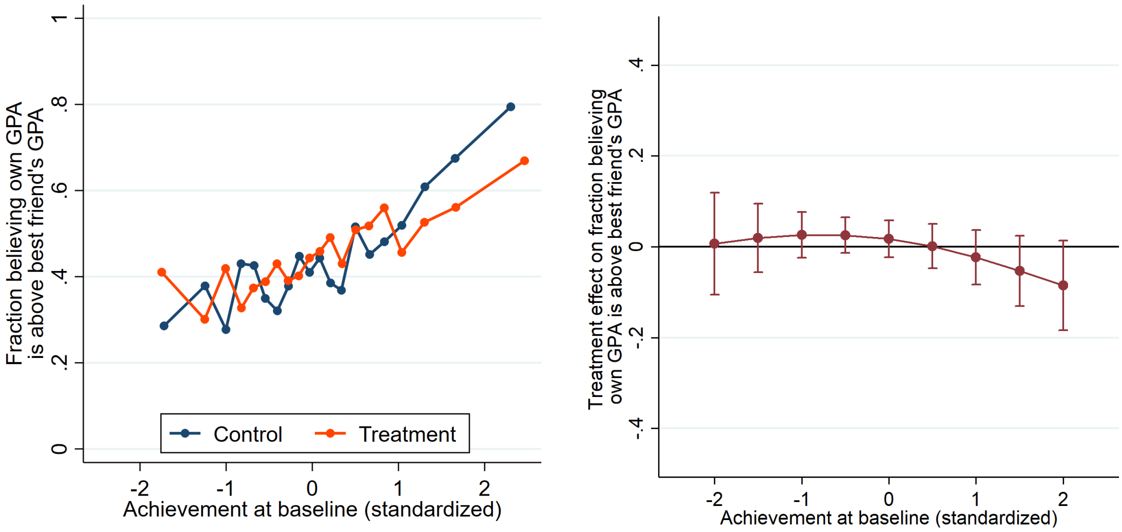

Figure 5: Perceived rank relative to best friend by baseline ability. The outcome variable is built by comparing the GPA that a student believes she has achieved by the end of high school to the GPA that she believes her best friend has achieved. The **left part** of the figure shows a binned scatter plot of the relation between the fraction believing their GPA is above their best friend's GPA and baseline ability. The figure shows 20 equally sized bins for each group, i.e. each dot shows the mean of 5% of the respective group. The **right part** shows treatment effects on the fraction believing own GPA is above best friend's GPA at different points of the baseline ability distribution. Errors bars are 95% confidence intervals (based on clustered standard errors). Estimates (marginal effects from Probit estimates) stem from regressing a dummy for believing own GPA is above best-friend's on a treatment dummy, a second order polynomial of baseline ability and an interaction of the treatment dummy and the polynomial (the order of the polynomial is chosen using Akaike's information criterion), using fieldworker fixed effects and inverse probability weights.

impacts of preferential admissions in unintended ways when the preferential admission schemes allow for non-localized externalities, or when they allow for localized externalities that affect many people.

5 Dynamic Model

The reduced-form tests identified aversion to social targeting and biases in beliefs about skills as empirically relevant mechanisms behind the students' pre-college choices. We now develop a structural model that incorporates them. The model allows us to quantify the relative importance of the mechanisms in shaping students' choices and, ultimately, the policy's impacts on admissions and enrollments, and it helps us to understand their implications for the design of preferential admissions.[25]

The model has two key features. First, it is dynamic. This allows us to quantify the consequences for admissions and enrollments of the pre-college choices shaped by

[25]Other studies interpreting experiments through structural models include Alfonsi et al. (2020); Allende, Gallego, and Neilson (2019); Attanasio, Meghir, and Santiago (2011); Kaboski and Townsend (2011); Todd and Wolpin (2006).

misperceptions and aversion to social targeting. Second, it allows for preferences for college (broadly defined to include also barriers to college attendance) as a residual channel behind policy impacts. Therefore, it does not posit that the channels at the core of this study are the only possible ones.

Model summary. In the model, students form beliefs about the returns to effort in securing an admission, and choose study effort. They then decide whether to take the entrance exam. Based on entrance exam scores, school ranks, and entrance-exam-taking, admissions are realized. Therefore, belief biases can shape policy impacts by distorting effort and the choice to take the exam and, consequently, admission sets. Given the admission sets, students choose whether to enroll and through which channel. *Ceteris paribus*, preferential admissions and enrollments carry a utility cost compared to regular ones. Therefore, aversion to social targeting can shape policy impacts on enrollments and admissions by affecting enrollment choices, and by affecting the pre-college choices that qualify for an admission (pre-college effort and the decision to take the entrance exam) of forward-looking students.

5.1 Model Set-up

Heterogeneous students. Each student i is characterized by vectors x_i and y_{it-1} of baseline characteristics and baseline achievement measures, respectively, and by $k_i \in \{1, 2, ..., K\}$, a time-constant type unobserved by the econometrician but observed by the student (Heckman and Singer (1984); Keane and Wolpin (1994, 1997)).[26] The number of types, K, is known to the econometrician. We let parameters that govern the preference for college, achievement and subjective beliefs depend on a student's type, to capture potential correlation between ability, preferences and beliefs that is not explained by observables. Not allowing for such correlation could lead to biased parameter estimates that mischaracterize the role of beliefs in choice (Wiswall and Zafar, 2015; Bobba and Frisancho, 2019).

Timing. Figure 6 shows the model timeline. Before the first choice period, students form beliefs about the top 15% cutoff in their high school and about how study effort maps into a GPA and an entrance exam score. These determine the *subjective* probabilities of a regular and preferential admission as a function of pre-college effort (represented in Figure 6 as $PrR(e)$ and $Pr15(e)$). Based on these beliefs, in period 1 students choose study effort so as to maximize its perceived present value. In period

[26]Vector x_i, measured in 10^{th} grade, includes age, gender, dummy for whether the Government classified the student as low-SES, dummy for whether the student repeated a year and dummy for high-school track (vocational or academic). Vector y_{it-1} comprises a standardized test score in 10^{th} grade (SIMCE), GPA in 10^{th} grade and the average of 9^{th} and 10^{th} grade GPA.

2, students decide whether to take the PSU entrance exam. As in the real world, students do not yet know their entrance exam score or whether they are in the top 15% of their school, and must base their choices on beliefs about these outcomes. In period 3, admissions are realized according to *objective* admission chances, which depend on the entrance-exam-taking decision and on the entrance exam score and GPA rank actually achieved. In period 4, students make enrollment decisions given their admissions.

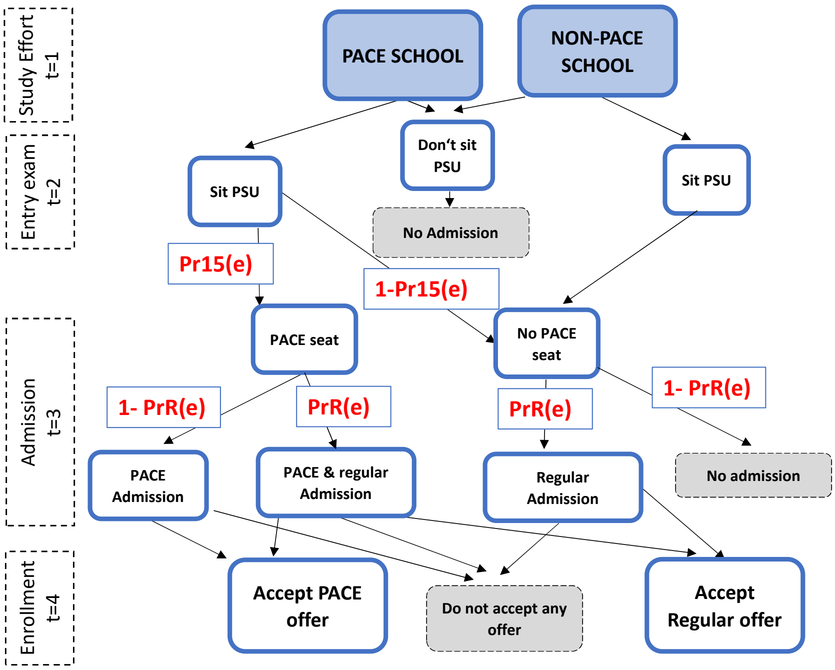

Figure 6: Model timeline.

Parameterization. Below we show how preferences and the objective and subjective production functions and admission probabilities enter the model. In Appendix E.2 we show how we parameterize them when we estimate the model.

Objective and subjective admission probabilities. The entrance exam score is produced through effort e_i: $PSU_i = PSU(e_i, y^{(1)}_{i,t-1}; \beta^P) + \epsilon^P_i$, where $y^{(1)}_{i,t-1}$ is a baseline standardized test score and ϵ^P_i is a normally distributed idyosincratic shock. Letting A^R_i be equal to 1 if student i obtains a regular admission and to 0 otherwise, and letting S_i be equal to 1 if student i takes the entrance exam and to 0 otherwise, the objective probability of a regular admission for those who take the entrance exam depends on the entrance exam score, and can be written as: $Pr(A^R_i = 1|PSU_i, S_i = 1; \gamma)$. But students base their pre-admission choices on beliefs about the PSU production function: $PSU^b_i = PSU^b(e_i, y^{(1)}_{i,t-1}, k_i; \beta^{Pb}) + \epsilon^{Pb}_i$, where normally distributed ϵ^{Pb}_i captures belief uncertainty around the expected score, and on beliefs about how the entrance exam score translates into a regular-admission chance (captured by the parameters γ^b): $Pr^b(A_i^R = 1|\overline{PSU}_i^b, S_i = 1; \gamma^b)$, where $\overline{PSU}_i^b$ is the expected score.

Similarly, GPA is produced through effort: $GPA_i = GPA(e_i, y_{i,t-1}^{(2)}; \beta^G) + \epsilon_i^G$, where $y_{i,t-1}^{(2)}$ is baseline GPA and ϵ_i^G is a normally distributed idiosyncratic shock, potentially correlated with the PSU production shock. The objective probability of a preferential admission is determined by the joint distribution of the shocks in the school; preferential admissions are assigned to students in treated schools who take the entrance exam and whose GPA is in the top 15% of their school. But students base their pre-admission choices on beliefs about the GPA production function: $GPA_i^b = GPA^b(e_i, y_{i,t-1}^{(2)}, k_i; \beta^{Gb}) + \epsilon_i^{Gb}$, where normally distributed ϵ_i^{Gb} captures belief uncertainty around the expected GPA, and on beliefs about how the GPA translates into a preferential admission chance (captured by the parameters ξ^b): $Pr^b(A_i^P|\overline{GPA}_i^b, c\bar{1}5_i^b; \xi^b)$, where $\overline{GPA}_i^b$ and $c\bar{1}5_i^b$ are the expected GPA and school cut-off and where A_i^P is equal to 1 if student i obtains a preferential admission and to 0 otherwise.

Per-period utilities. In the first period, students derive utility from achievement, produced through effort, and face a cost of exerting effort, such that the per-period utility associated with each effort choice $e_i \in \{0, 1, ..., E\}$ is $u_{i1}(e_i) = y(e_i, x_i, y_{i,t-1}^{(1)}, k_i; \alpha) - c(e_i; \xi)$, where the cost function is assumed to be quadratic: $c(e_i; \xi) = \xi_1 e_i + \xi_s e_i^2$, with a constant normalized to zero because only the difference in utilities is identified. In period 2, students decide whether to take the entrance exam. The per-period utility from taking the exam is the sum of the cost of taking the exam (capturing monetary and non-monetary costs), and a standard logistic shock: $u^{S_i=1} = -c^S + \eta_i$.[27] The per-period utility from not taking the exam is normalized to 0 because only the difference in utilities is identified. In time period 3, admissions are realized, and students who receive a preferential admission pay a utility cost $\delta^A \geq 0$ (disutility from preferential admission).[28] In time period 4, when making enrollment decisions, students derive the following utilities from a regular and a preferential enrollment, respectively:

$$u_i^{ER} = \lambda_{0k_i} + \lambda_1 SES_i + \lambda_2 a_i + \lambda_3 q^R(PSU_i) + \nu_i^R, \quad (7)$$

$$u_i^{EP} = \lambda_{0k_i} + \lambda_1 SES_i + \lambda_2 a_i + \lambda_3 q^P(GPA_i) - \delta^E + \nu_i^P. \quad (8)$$

The utility from not enrolling is normalized to 0. We let the enrollment utilities depend on: the type k_i; the socioeconomic status and ability (SES_i, a_i); the selectivity

[27]The fee is approximately USD 30; most students in the sample can apply for a fee waiver. But research shows that disadvantaged students can face non-monetary barriers to taking entrance exams (Dynarski et al., 2022).

[28]When students who are averse to social targeting receive pressure from parents to accept a preferential admission, the admission itself can carry a utility cost.

of the degree-program to which they are admitted (defined like in section 4.1 as the lowest entrance exam score among all regular entrants), which, approximating the allocation mechanisms, depends on the PSU score in the regular channel and on the GPA in the preferential channel, $q^R(PSU_i), q^P(GPA_i)$; and a standard-logistic utility shock.[29] When making pre-admission choices, students use their expected PSU and GPA to form beliefs about the quality of the degree-programs to which they will gain admissions, but realized qualities depend on the objective PSU score and GPA achieved. Enrolling through the preferential channel carries a utility cost $\delta^E \geq 0$ (aversion to enrolling preferentially). The enrollment preferences, which are relative to the outside option, capture tastes, barriers and outside options that vary by unobserved student characteristics (k_i) and by background and ability (SES_i, a_i).

Solution. Students construct a *subjective* value function using their beliefs, which we indicate with a b superscript:

$$V_t^b(\Omega_{it}) = \max_{d_{it} \in D_{it}} \left\{ u(d_{it}, \Omega_{it}) + E^b \left[V_{t+1}(\Omega_{it+1} | \Omega_{it}, d_{it}) \right] \right\} \quad (9)$$

where Ω_{it} is the state vector, which evolves from the initial condition according to *objective* production functions and admission probabilities, and d_{it} is the period choice.[30] We solve the problem by backward induction and find the value of the subjective value function in all decision periods and at all possible state space values. We compute the exact analytical solution, a sequence of optimal, non-randomized decision rules $\{d_{it}^*(\Omega_{it})\}$ that are deterministic functions of the state space Ω_{it}.[31]

5.2 Identification

We now discuss key measures we use, and how we identify the parameters that govern the channels of subjective beliefs and aversion to social targeting. In Appendix E.3 we discuss permanent unobserved heterogeneity, modelled following Heckman and Singer (1984), Keane and Wolpin (1994, 1997), and Wooldridge (2005).

[29] SES_i is an indicator for whether the student is identified as with very-low SES by the government; a_i is an indicator for whether a student is above or below median ability at baseline. We let the utilities depend on the selectivity to be able to identify the aversion from enrolling preferentially, δ^E. As section 4.1 showed, students are admitted, across channels, to programs that are in similar locations and fields of study, but that differ substantially on selectivity. Not letting the enrollment utility depend on selectivity would bias the estimate of δ^E, because it would capture differences in selectivity across channels as well as the disutility from enrolling preferentially.

[30] The vector of initial conditions is $\Omega_{i1} = [x_i, k_i, y_{it-1}, c\bar{1}5_i^b, T_{j(i)}]$, where $T_{j(i)}$ is a dummy equal to 1 if a student is in a school randomly allocated to the PACE treatment.

[31] The model presumes that college admission is one of the motives behind effort provision in high school, but 9.7% of students report, at baseline, that they do not think they will stay in education beyond high-school, and 97.3% of them do not enroll in college. We assume these students solve a static decision problem in period 1 (effort decision). We allow for treatment to have a direct effect on their cost of study effort (see footnote 37 in Tincani, Kosse, and Miglino (2021) for details).

Pre-college achievement and effort. Pre-college achievement enters the utility of students in the first model period. We assume that the score on the standardized test that we administered, y_i^o, is a noisy measure of pre-college achievement: $y_i^o = y_i + \epsilon_i^{m.e.y.}$, where $\epsilon_i^{m.e.y.} \sim N(0, \sigma^2_{m.e.y.})$ is a classical measurement error.[32] Pre-college effort is a choice of students in the first model period. We assume that reported hours of study per week over a semester are a noisy measure of pre-college effort: $e_i^o = e_i + \epsilon_i^{m.e.e.}$, where $\epsilon_i^{m.e.e.} \sim N(0, \sigma^2_{m.e.e.})$ is a classical measurement error. Using reported hours of study to measure effort allows us to use a common scale to estimate the objective and perceived returns to effort in the production of entrance exam scores and GPA.

Subjective beliefs. We separately identify subjective beliefs from unobserved ability and preferences using the belief data we collected (Manski, 2004). The subjective probabilities of a regular and a preferential admission, conditional on taking the entrance exam ($S_i = 1$), are a function of effort e_i, and depend on the expected believed PSU score, $E[PSU^b_{k_i}(e_i, x_i)]$, the expected believed GPA, $E[GPA^b_{k_i}(e_i, x_i)]$, and the believed top 15% cutoff in the school, $c\bar{1}5^b_i$, as shown in the following equations and, in more detail, in equations (29) and (30) in the Appendix:

$$Pr^b(A_i^R = 1 | e_{it}, x_i, k_i, S_i = 1) = \Phi\left(\gamma_0^b + \gamma_1^b E[PSU^b_{k_i}(e_i, x_i)]\right), \tag{10}$$

$$Pr^b(A_i^P = 1 | e_i, x_i, k_i, S_i = 1) = \Phi\left(\xi_0^b + \xi_1^b (E[GPA^b_{k_i}(e_i, x_i)] - c\bar{1}5^b_i)\right), \tag{11}$$

where x_i are baseline student characteristics and k_i is the student's type.

First, we follow a standard approach from the behavioral game theory literature, and assume that students in treated schools best-respond to the perceived cutoff that we have elicited, without imposing equilibrium behavior (Stahl and Wilson, 1995; Costa-Gomes and Zauner, 2003; Camerer, Ho, and Chong, 2004; Costa-Gomes and Crawford, 2006; Crawford and Iriberri, 2007). Therefore, this argument of the function in (11) is observed.

Second, to identify the perceived returns to effort in the subjective production functions, in the right-hand side of (10) and (11), we do not rely on the cross-sectional relationship between expected outcomes and effort, because it cannot necessarily be interpreted as causal. Instead, we measured perceived returns with our survey. We elicited beliefs about the PSU score and the GPA that students expect to obtain under

[32] Tincani, Kosse, and Miglino (2021) present the predictive validity of the achievement score as an achievement measure. It can predict high-stake outcomes including admission, enrollment and persistence in college, even conditional on baseline achievement and student and school characteristics.

the actual and hypothetical effort levels. For example, for entrance exam scores, we asked:

Thinking of yourself, how many hours per week do you think you need to study, between August and December, to obtain...

... 600 or more on the PSU

... 450 or more on the PSU

... 350 or more on the PSU.

The answers are hypothetical hours of study, which we assume are affected by measurement error: $h_i^{oj} = h_i^j + \epsilon_i^{m.e.e.}$, where $j = 600, 450, 350$ and $\epsilon_i^{m.e.e.} \sim N(0, \sigma^2_{m.e.e.})$. We convert the answers into the expected increase in PSU score per additional hour of study per week, i.e., the perceived returns to effort in PSU score production. To improve precision of our measure, we combine the answers to the hypothetical questions with those to the questions on how much they studied and what PSU score they expect. Let $e_i^o = e_i + \epsilon_i^{m.e.e.}$ denote the hours of study they report, and let $PSU_i^b|_{e_i^o}$ denote the PSU score they expect given those hours. We measure the perceived returns to effort as

$$\sum_{j\in\{350,450,600\}} \frac{1}{3} \cdot \frac{j - PSU_i^b|_{e_i^o}}{h_i^{oj} - e_i^o}, \quad \text{if } h_i^{oj} \neq e_i^o. \tag{12}$$

Figure 7 shows the distribution of returns to effort in our sample (Table A7 summarizes the survey answers used to construct the returns). In estimation, we match moments of these distributions using their model counterparts.[33] To simulate perceived returns, we simulate the expected PSU score and GPA for each student at various values of hours of study. For example, consider distinct effort levels h_i^z and h_i^j and let $\widehat{PSU_i^b}(h_i)$ be the expected PSU score predicted by the model at effort level h_i. The simulated returns to effort are:

$$\frac{\widehat{PSU_i^b}(h_i^z) - \widehat{PSU_i^b}(h_i^j)}{h_i^{oz} - h_i^{oj}}, \quad \text{where } h_i^{oz} = h_i^z + \epsilon_i^{m.e.e.} \text{ and } h_i^{oj} = h_i^j + \epsilon_i^{m.e.e.}. \tag{13}$$

Having identified the parameters governing perceived returns to effort, we match the distributions of expected PSU scores and GPA to identify the remaining parameters of the perceived production functions. Then, all arguments of the subjective admission probabilities in (10) and (11) are either observed or identified. The relation between choices and these arguments identify the parameters of the subjective

[33]Naively matching them would introduce sample-selection bias because perceived returns are not observed among students who were not surveyed. To mitigate the issue we let parameters that govern the perceived returns depend on the unobserved student type, and we let the type distribution vary across students who were and were not surveyed. We then simulate the distributions of perceived returns conditional on being surveyed to build the model counterparts to the empirical moments.

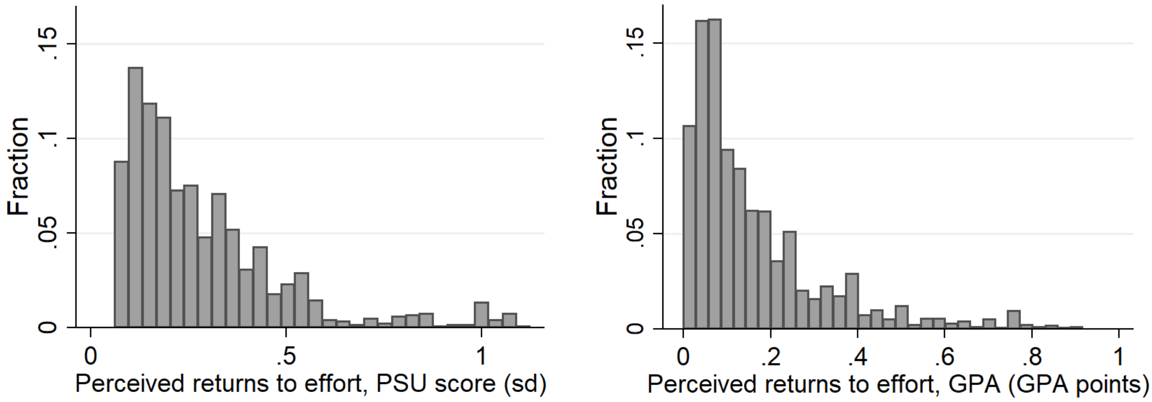

Figure 7: Distribution of perceived returns to effort, measured as the perceived impact of an additional hour of study per week in the semester (top 1% trimmed).

probabilities $(\gamma_0^b, \gamma_1^b, \xi_0^b, \xi_1^b)$. Appendix E.3 details how we mitigate potential endogeneity of these arguments by imposing additional exclusion restrictions, exploiting the experimental data variation wherever possible.

Aversion to social targeting. We identify the disutility from preferential enrollment (δ^E) from the enrollment choices of students who are admitted through both channels (regular and preferential), accounting for the differences in the qualities of the programs to which they are admitted. The data variation is visualized in Figure 2. We assume the disutility from enrolling preferentially is the same for all students. Without this assumption, we would not be able to extrapolate outside of the sample of students admitted through both channels and we would not be able to quantify the role of this disutility in determining policy impacts on admissions, one of the model's objectives. Since the students in this sample have applied for a preferential seat, it is reasonable to expect that they have a lower disutility from preferential seats than those who are not in this sample. Therefore, our simulations likely yield lower bounds on the role of this disutility. We identify the disutility from being admitted preferentially (δ^A) from the (null) treatment effect on the decision to take the entrance exam. Without the admission disutility, it is difficult to justify the null impacts of PACE on entrance-exam-taking, since PACE provides new admission opportunities to those who take the entrance exam, increasing the value of taking the exam without increasing its cost.

5.3 Estimation

Aside from the parameters of the regular admission probability (equation (26)) and of the selectivity of an admission (equations (31) and (32)), whose estimates we report in Table A8, all parameters are estimated within the model. They pertain to the production technologies $(\alpha, \beta^P, \beta^G)$, subjective beliefs $(\beta^{Pb}, \beta^{Gb}, \gamma^b, \xi^b)$, preferences, including the disutility from the preferential channel $(\xi, c^S, \lambda, \delta)$, and the distribution of model shocks, measurement errors, and unobserved types $(\Sigma, \sigma^2_{m.e.y.}, \sigma^2_{m.e.e.}, \omega)$. We assume that there are two unobserved types ($K = 2$) that follow a logit distribution that depends on the ninth and tenth grade GPA average ($y^{(3)}_{it-1}$) and on an indicator for whether a student was surveyed in our data collection, D^s_i, to correct for survey attrition based on unobservables. Since the treatment was randomized, we can assume that types are identically distributed across treatment groups (i.e., balanced unobservables). Letting $X_i = [1, y^{(3)}_{it-1}, D^s_i]$:

$$Pr(k_i = \tau | X_i) = \frac{e^{X_i'\omega}}{1 + e^{X_i'\omega}}. \tag{14}$$

Estimation is by generalized indirect inference (Bruins et al., 2018), as in Altonji, Smith Jr, and Vidangos (2013). In a first step, we estimate a set of auxiliary models that summarize the experimental findings and data patterns to be targeted in the structural estimation. In a second step, an outer loop searches over the parameter space, while an inner loop solves the dynamic model at each candidate parameter value and forms the criterion function. The latter is the distance between the auxiliary model estimates from the data and their counterparts from the simulated data. Appendix E.4 lists the auxiliary models and moment conditions.

At each parameter iteration θ, we simulate S datasets, where each simulation is a draw for the model shocks and the student type.[34] Let $\bar{\beta}$ denote the vector of auxiliary model parameters and moments computed from the data, and let $\hat{\beta}^s(\theta)$ denote the corresponding values obtained from the s^{th} dataset predicted by the model at the value θ of the structural parameters. Let $\hat{\beta}(\theta) = \frac{1}{S}\sum_{s=1}^{S} \hat{\beta}^s(\theta)$. The structural parameter estimator is obtained as the solution to:

$$\hat{\theta} = \arg\min_{\theta} \left[\hat{\beta}(\theta) - \bar{\beta}\right]' W \left[\hat{\beta}(\theta) - \bar{\beta}\right] \tag{15}$$

where W is a positive definite weighting matrix. Generalized indirect inference, developed for dynamic discrete choice models like ours, ensures that the criterion function is differentiable and allows us to rely on a fast derivative-based optimization method to solve (15).[35]

[34] Following the results in Eisenhauer, Heckman, and Mosso (2015), we set $S = 20$.

[35] Following Altonji, Smith Jr, and Vidangos (2013), we use the smoothing function $\frac{exp(\frac{u_i}{\lambda})}{1+exp(\frac{u_i}{\lambda})}$, where u_i is the latent utility, with smoothing parameter $\lambda = 0.05$. We use Knitro to solve the optimization problem (Byrd, Nocedal, and Waltz (2006)).

6 Model Results

6.1 Estimation Results

Estimates of the model parameters are in Table A9. *Ceteris paribus*, students value preferential admissions and enrollments less than regular ones (δ^E and δ^A are positive). Comparing the perceived and objective production functions shows that students hold overoptimistic beliefs about the returns to effort. In the objective production function of entrance exam score (GPA), the coefficient on effort is 0.161 standard deviations (0.037 GPA points, or 0.065 standard deviations). But students, depending on their type (as defined in section 5.1), believe it is larger, between 0.262 and 0.331 standard deviations (0.148 and 0.353 GPA points, or 0.260 and 0.619 standard deviations). Therefore, both student types are overoptimistic. Those of the more optimistic type also have higher unobserved ability and preference for college. Therefore, ability, preferences and beliefs correlate with each other.

Model fit Table A10 shows that the model captures the key features of the data, including the policy impacts.

6.2 Counterfactual Simulations

The reduced-form analysis showed the effect of the aversion to social targeting on the enrollment channel of those admitted both regularly and preferentially, but it could not quantify how it shapes admissions and enrollments at the extensive margin. It showed that the policy impacts varied with belief bias intensity, but it could not quantify the effects of combining preferential admissions with eliminating belief biases for all students. We use the model to go beyond the reduced-form results, and quantify the importance of belief biases and of the aversion to social targeting in shaping the policy impacts. Since different channels have different policy implications, the simulation results are helpful to inform policy design.

Simulating the counterfactuals. To simulate eliminating belief biases, we assume students use objective rather than subjective production and admission likelihood functions. We then solve for the rational-expectations equilibrium of the tournament game that takes place in each school to award the preferential admissions, a high-dimensional fixed-point problem (Appendix E.5 describes the algorithm we use).[36]

[36]Previous studies have simplified the problem by assuming that there is a continuum of individuals and that they differ only along one dimension (Hopkins and Kornienko (2004); Bodoh-Creed and Hickman (2018, 2019); Cotton, Hickman, and Price (2020)). But these simplifications are inappropriate in our setting: i) our populations are schools, which are limited in size, and ii) individuals differ in more than one dimension. Therefore we relax these assumptions. To lower the dimension-

To simulate eliminating the aversion to social targeting, we set the disutility from the preferential channel to zero. We must then make assumptions to predict the student beliefs at the counterfactual distribution of effort. We consider the two, extreme cases of no and full information acquisition. In one case, we assume that students do not acquire more information about the cutoff than in the baseline scenario: their belief bias over the rational-expectations cutoff is assumed to be the same as at baseline. But since the preferential channel is more desirable when students are not averse to social targeting, they may endogenously acquire more information. Therefore, in the other case we assume that students acquire full information: they are assumed to have rational expectations. The two cases give us bounds for the likely impacts of eliminating the aversion to social targeting. The full-information-acquisition case can also be interpreted as combining interventions that eliminate the disutility from the preferential channel with interventions that eliminate belief biases.

In all simulations, to avoid confounding effects from the (insignificant) imbalances in baseline covariates across treatment and control samples, we simulate outcomes from the initial conditions of the control group. We simulate the counterafactuals in the simulated treatment group only, keeping the simulated control group at the baseline scenario.

Average policy impacts on admissions and enrollments. Table 9 shows that the baseline simulations (first row) replicate the effects of the current policy well. Correcting beliefs does not substantially change admission and enrollment effects on average (second row). In contrast, eliminating the aversion to social targeting substantially increases the policy impacts on admissions (from 0.039 to between 0.057 and 0.071) and enrollments (from 0.020 to between 0.050 and 0.060). Therefore, eliminating the aversion has a stronger average effect on admissions and enrollments than eliminating belief biases.

Table 9: Counterfactual Simulation Analysis: Average Effects

	Policy Effect on:	
Scenario:	Admissions	Enrollments
Baseline	0.039	0.020
Debias Beliefs	0.035	0.019
Eliminate Aversion to Social Targeting (No Info Acquisition)	0.071	0.060
Eliminate Aversion to Social Targeting (Full Info Acquisition)	0.057	0.050

Note. – This Table shows the simulated impacts of the preferential admission policy in the baseline scenario and under three counterfactual ones. Simulations in the absence of the policy (simulated control group) are always in the baseline scenario. Model shock draws are kept constant across treatment groups (policy vs. no policy) and scenarios.

ality of the fixed point, we solve for an approximated equilibrium instead. The intuition is that the strategies of others affect own payoffs only through the probability of a preferential admission. We posit a parametric approximation for this probability and solve for a fixed point in its parameters.

Policy impacts on admissions by baseline ability. The gap between the mechanical and actual admission effects and how it varies with ability is central to this study (see Figure 1 in section 3). Figure 8 shows that the model successfully replicates the gap, and the fact that it widens with ability.[37] Eliminating the aversion to social targeting increases admission effects for students of all ability levels in the no-information-acquisition case ("Eliminate Aversion" in Figure 8), and for medium- and high-ability students in the full-information-acquisition case ("Eliminate Aversion and Debias Beliefs" in Figure 8). It closes between 19% and 36% of the admission effects gap, depending on the the assumptions we make on endogenous information acquisition (the gap is 8.8 p.p. at baseline and 7.1 p.p. or 5.6 p.p. in the absence of the aversion; $\frac{(8.8-7.1)}{8.8} = 19\%$ and $\frac{(8.8-5.6)}{8.8} = 36\%$). In contrast, debiasing beliefs does not close the gap on average, but it decreases admission effects for low- and medium-ability students, and it increases them for high-ability students, improving the ability composition of those induced to be admitted.

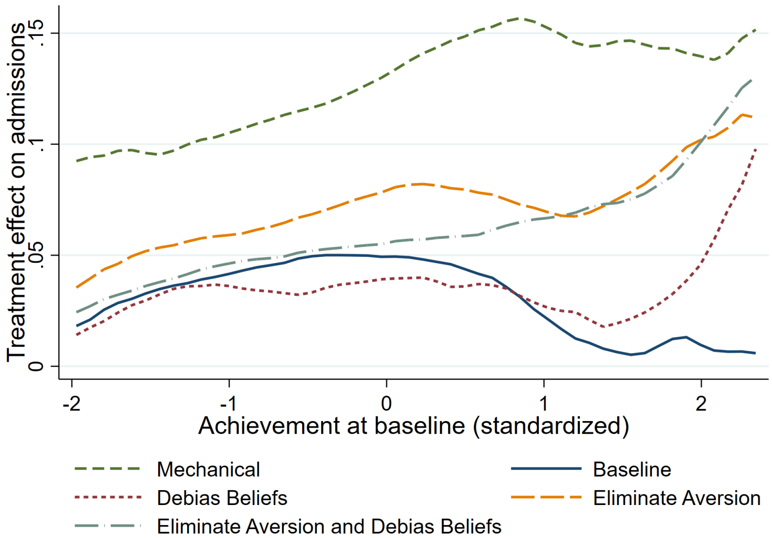

Figure 8: Model simulations: Heterogeneous admission effects of baseline and counterfactual policies. The figure compares the simulated admission effects of the current policy (baseline) to those of counterfactual policies that combine offering preferential admissions with eliminating the aversion to social targeting, eliminating belief biases, or eliminating both. We report the simulated mechanical admission effect (section 2.3) for reference. Top and bottom 1% of baseline achievements are trimmed.

There remains a sizeable gap between mechanical and actual admission effects that cannot be closed by any of the interventions we consider (Figure 8). Some potential beneficiaries do not take up the opportunity of a preferential admission because they may face financial or other barriers to college, or simply prefer to enter the labor market or attend vocational higher education.

[37]The simulated mechanical admission effect, not in Table 9, is 0.127, and the simulated actual one at baseline, first row of Table 9, is 0.039.

Understanding the mechanisms: the role of pre-college choices. To understand why the different interventions have very different impacts on the admission effects, and why they vary along baseline ability, we must understand how they affect the pre-college choices of different students. Figure 9 shows the effects of eliminating the aversion to social targeting (in the no-information-acquisition scenario, to isolate the role of the aversion) and of debiasing beliefs on the pre-college choices and resulting admissions of treated students. We focus on treated students because the counterfactuals are implemented in the treatment group (i.e., in the presence of preferential admissions). Therefore, the effects on admissions, in the right-most column of Figure 9, correspond to the differences between the counterfactual and baseline admission effects from Figure 8.

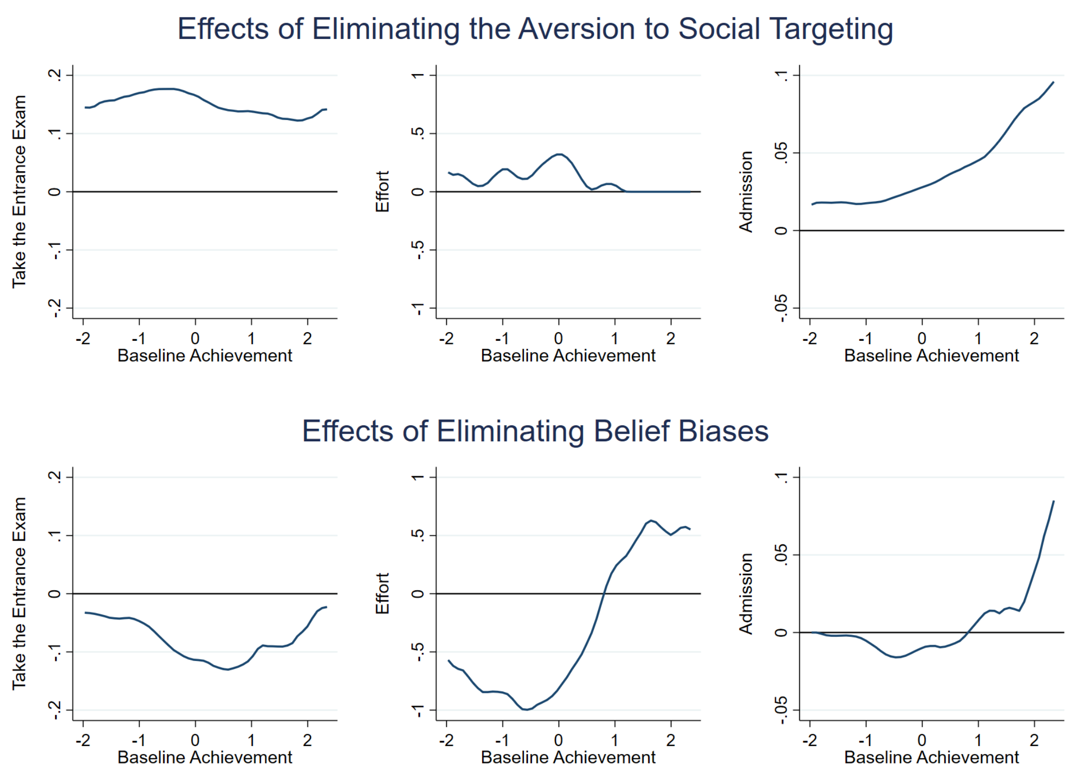

Figure 9: Model simulations: effects of eliminating the aversion to social targeting and of eliminating belief biases on entrance-exam-taking, effort and admissions, at different values of baseline ability (top and bottom 1% trimmed), in the presence of the preferential admission policy. Effort is measured in hours of study per week in the semester. Admissions are through any channel.

Unsurprisingly, eliminating the aversion to social targeting (top row of Figure 9) increases the rate at which students of all ability levels take the entrance exam (first column); fewer forgo the possibility of a preferential admission when preferential admissions are more desirable. It also slightly increases the effort of the low ability (second column). As a result of higher entrance-exam-taking at all ability levels, admissions increase at all ability levels. They increase relatively more for the high ability, who are more likely to qualify for an admission (third column).

Unsurprisingly, eliminating belief biases (bottom row of Figure 9) decreases the rate at which students of all ability levels take the entrance exam (first column), because the overoptimistic belief biases, found at all ability levels in our sample (Figure A5), lead students to take the exam more often than they should, so that eliminating them reduces exam taking. But eliminating overoptimism has opposite effects on effort depending on ability (second column). Overoptimism leads high-ability students to incorrectly perceive an admission as guaranteed and under-provide effort, and low-ability students to incorrectly perceive it as within reach and over-provide effort. Therefore, eliminating it increases the effort of high-ability students and decreases that of low-ability ones. Since effort affects the likelihood of qualifying for an admission, effort under- (over-)provision results in under- (over-)admissions, so that eliminating overoptimism increases the admissions of the high ability and decreases those of the low ability (third column). As a result of a better selection of admitted students, the ability composition of the students induced to attend college by the policy improves by 0.116 standard deviations of baseline test scores when belief biases are eliminated compared to when they are not.

Summary of results and discussion. Aversion to social targeting may have compressed the admission effects of PACE, by between 32% and 45%, because some students did not wish to be admitted and enroll preferentially. It compressed entrance-exam-taking, which was required for an admission. Extrapolating outside of the PACE context, we expect such aversion, if present, to lower take-up of preferential admissions. In contrast, biases in pre-college beliefs about the likelihood of being admitted to college did not compress the admission impacts of PACE, but they affected the composition of college entrants by distorting pre-college effort investments. In the context of PACE, where beliefs are on average overoptimistic, they worsened the ability composition of those the policy brought to college by 0.116 standard deviations. Extrapolating outside of the PACE context, we expect pre-college belief biases to distort the pre-college choices that qualify students for a preferential admission. How such distortions shape the composition of college entrants depends on the context-specific magnitude of the biases and their correlation with student characteristics. Regardless, we expect some degree of misallocation of preferential seats when students are misinformed about their admission chances.

The structural model results are necessarily based on more assumptions than the reduced-form findings. But they allow us to quantify the role of pre-college choices in shaping policy impacts and, further, to quantify the importance of the preferences and beliefs affecting such choices. They suggest that aversion to social targeting and misperceptions relevant to skill targeting can play different but both important roles in shaping the impacts of preferential admission policies. Therefore, the findings imply that understanding the beliefs and social concerns of disadvantaged high-school students could help improve the design of preferential admissions and have large returns for social mobility.

7 Conclusions

This paper provides experimental evidence on the impacts of a preferential college admission policy in Chile, showing that it significantly increased admissions, but 64% less than expected based on the number of intended beneficiaries. It further provides evidence on: (i) students' aversion to enrolling preferentially (when given a choice, students were 40 p.p. less likely to accept a preferential admission over an identical regular one); (ii) students' biased beliefs about the skills that qualify them for an admission (newly collected belief data documented such beliefs and showed that the admission impacts varied with students' beliefs); (iii) students' social preferences towards their friends (the rank-order tournament led high-ability students to attempt to rank below their self-reported best friends to avoid imposing a negative externality, although these effects are not expected to drive the admission impacts of such schemes when the externalities are localized). Our structural model estimates indicate that the aversion to social targeting may have shrunk the enrollment impacts of the policy by up to 55%. They further indicate that students' biased beliefs about their skills may have worsened the ability composition of those the policy brought to college by 0.12 standard deviations of baseline ability, by inducing mistakes in the investments students made towards qualifying for an admission. These results are among the first to show that the pre-college choices of students targeted by preferential admissions can determine the impacts of this widespread policy on the college participation of disadvantaged groups, and could be driven by misperceptions and by social mechanisms that are not yet well understood.

Mounting evidence shows that students' biases in beliefs can be affected by informational interventions. Our estimates on the role of belief errors, then, can be interpreted as the likely impacts of best-case-scenario informational interventions. Much less is known, however, about the mechanisms that can generate aversion to social targeting. Our data do not allow us to identify such mechanisms. From our discussions with the policy coordinators, we conjecture that several mechanisms could be at play, including social image concerns and fear of stigma, misperceptions about the likelihood of graduating from a program to which one is admitted through social targeting, pressure from parents. Understanding each of these mechanisms, their welfare implications, and whether they can be shaped or influenced through new interventions, is an important avenue for future research. Examples of interventions to test include hiding the enrollment channel (to the targeted students, college peers, or both), and aiming the orientation classes at improving self-image.[38]

The magnitudes of the effects we estimated may be context dependent. Belief biases and aversion to social targeting may have had large impacts because the students in our sample were not given regular feedback on the skills they needed to qualify for an admission, and because college entrants through this relatively new policy are still a small minority. Social preferences may have had negligible impacts because the rank-order tournament was not perceived to be tight. But the findings show that to the extent that students have some misinformation or social concerns, their pre-college choices may steer the impacts of preferential admissions away from the intended direction. Therefore, our study demonstrates that understanding the social concerns and the beliefs of the disadvantaged students targeted by preferential college admissions is key for the design of these policies, because they can interact with the same features that define them.

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[38] If any of these interventions increases the number of students who are admitted to selective universities through the centralized admission system and then choose off-platform options, Kapor, Karnani, and Neilson (2022) show that there is a potential for negative externalities on others who are on the platform's waiting list. Such phenomenon should be monitored.

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Online Appendix

The Effect of Preferential Admissions on the College Participation of Disadvantaged Students: The Role of Pre-College Choices

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October 6, 2022

A Additional Tables

Table A1: Baseline characteristics of all students and of those targeted by the PACE policy

	All students	Targeted students
	(1)	(2)
Very low SES	0.40	0.61
Mother's education (years)	11.49	9.60
Father's education (years)	11.43	9.38
Family income (1,000 CLP)	600.10	291.66
SIMCE score (standardized)	0.00	-0.62
Rural resident	0.03	0.03
Santiago resident	0.30	0.17

Source.– SIMCE and SEP administrative data on 10^{th} graders in 2015. Note.– Very low SES indicates a student that the Government classified as socioeconomically vulnerable (*Pioritario*). SIMCE is a standardized achievement test taken in 10^{th} grade. Sample restriction in column (2): students in the 128 experimental schools. All characteristics were collected before the start of the intervention.

Table A2: Average Treatment Effect on Pre-College Study Effort - Items

Panel A: At home	Study hours	Study days test	Assignm on time
Treatment	-0.081**	0.003	-0.086***
	(0.040)	(0.043)	(0.033)
R-W adjusted p	0.089	0.947	0.027

Panel B: In class	Take notes	Participate	Pay attention	Ask questions
Treatment	-0.089**	-0.008	-0.061	-0.018
	(0.039)	(0.013)	(0.037)	(0.042)
R-W adjusted p	0.083	0.864	0.269	0.864

Panel C: PSU entrance exam preparation	Prepare for PSU
Treatment	-0.042**
	(0.017)

Note.– Panels A and B report OLS estimates, panel C reports the average marginal effect from a probit model. Standard errors are clustered at the school level (for panel C, the delta method is used). We use the standard set of controls (see Table 4), field-worker fixed effects and Inverse Probability Weights. *Treatment* is a dummy variable indicating whether a student is in a school that was randomly assigned to be in the PACE program. The family of survey instruments in Panel A asked students the number of hours of study per week outside of class time, how many days before a test they start preparing, and how often they hand in homework on time. The family of survey instruments in Panel B asked students how often, when in class, they take notes, actively participate, pay attention, and ask questions. We report Romano-Wolf adjusted p-values calculated within family (as per the pre-analysis plan). The dependent variable in Panel C is a dummy indicating whether the student does at least one of the following PSU exam preparation activities: attending a PSU preparation course (*Preuniversitario*) for a fee, attending a free *Preuniversitario*, using an online *Preuniversitario* for a fee, using an online free *Preuniversitario*, preparing on his/her own. *p $<$ 0.10; **p $<$ 0.05; ***p $<$ 0.01.

Table A3: Average Treatment Effect on GPA by Subject Type

	12^{th} grade GPA (standardized)	
	Subjects tested in PSU	Subjects not tested in PSU
	(1)	(2)
Treatment	-0.152*	-0.006
	(0.087)	(0.132)
Observations	6,046	4,288
R^2	0.220	0.109

Note.– The coefficients are OLS estimates. Standard errors are clustered at the school level. Standard set of controls (see notes under Table 4). Inverse Probability Weights used. Sample of surveyed students. *Treatment* is a dummy variable indicating whether a student is in a school that was randomly assigned to be in the PACE program. The subjects tested on the PSU are core subjects such as mathematics and Spanish. Those not tested are specific to the high-school track and include subjects such as accounting and industrial mechanics. *p $<$ 0.10; **p $<$ 0.05; ***p $<$ 0.01.

Table A4: Treatment Effect on the Likelihood of Graduating in the top 15% by Baseline Test Score

	Graduate in top 15%
Treatment	-0.022
	(0.039)
Treatment × SIMCE score (standardized)	-0.033^{**}
	(0.015)
Observations	8,289
R^2	0.123
Predicted effect when SIMCE score = -1	0.011
	(0.019)
Predicted effect when SIMCE score = +1	-0.055^{**}
	(0.024)

Note.– Estimates stem from an ordinary least squares regression. SIMCE is a baseline standardized test score, and it is included as regressor also not interacted. Standard set of controls used (see notes to Table 4). *Treatment* is a dummy variable indicating whether a student is in a school that was randomly assigned to be in the PACE percent plan program. Standard errors clustered at the school level are in parenthesis. *p < 0.10; **p < 0.05; ***p < 0.01.

Table A5: Socioeconomic correlates of belief biases

	Rank belief bias	PSU belief bias
	(1)	(2)
Very low SES	0.014	-0.033
	(0.022)	(0.022)
Household log-income	-0.024	0.007
	(0.023)	(0.017)
Mother education (years)	0.003	0.018***
	(0.005)	(0.005)
Father education (years)	-0.009**	0.016***
	(0.004)	(0.004)
Observations	4,570	3,769

Note.– Estimates stem from ordinary least square regressions. Very low SES is a dummy variable identifying students the Government classified as particularly vulnerable based on socioeconomic status. Rank belief bias is the difference between actual and expected 85^{th} GPA percentile in the school, it is measured in GPA points (GPA ranges from 1 to 7). Positive values indicate overoptimism. PSU belief bias is the difference between expected and actual PSU entrance exam score, it is measured in standard deviations. Positive values indicate overoptimism. Standard errors in parenthesis clustered at the school level. Inverse Probability Weights used. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Enrollment Choices and Selectivity of Admissions

	Preferential enrollment	Regular enrollment	Difference	Preferential selectivity	Regular selectivity	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
Mean	0.283	0.555	-0.272***	513.370	497.067	16.303***
	(0.034)	(0.038)	(0.067)	(4.414)	(3.291)	(4.758)
Observations	173	173	173	165	165	165

Note.– Sample of students admitted to university through both channels. Selectivity is measured in entrance exam points. Standard errors are in parentheses. Significance levels in columns (3) and (6) are based on the p-value of a t-test where the null-hypothesis is that the difference in means equals zero. Selectivity of the preferential admission is missing for eight students. *p < 0.10; **p < 0.05; ***p < 0.01.

Table A7: Survey answers to hypothetical effort questions

Survey question	Observations	Mean	Standard Deviation
Hours of study per week in the semester to obtain:			
at least 600 on the PSU	5,469	10.106	4.748
at least 450 on the PSU	5,442	7.668	4.390
at least 350 on the PSU	5,344	5.506	4.536
a GPA in the top 15% of the school	5,443	8.105	4.330
a GPA of at least 5.5	5,451	7.077	4.335

Note.– This table describes the answers to the survey questions used to build the perceived returns to effort in the production of a PSU score and of GPA. For the second-last row, the survey asked the student to think of how many hours they believe they needed to study to obtain a GPA above the cutoff that they perceived as the 85^{th} percentile according to a previous survey answer. In constructing perceived returns, we eliminated answers that delivered infinite or negative returns.

Table A8: Parameters estimated outside of the model

Symbol	Description	Estimate	Standard Error
γ_0	Constant, regular adm. prob.	-0.306***	0.061
γ_1	Coefficient of PSU, regular adm. prob.	2.481***	0.199
λ_0^R	Constant, regular selectivity	467.603***	1.334
λ_1^R	Coefficient of PSU, regular selectivity	43.861***	3.491
λ_0^P	Constant, PACE selectivity	295.740***	60.013
λ_1^P	Coefficient of GPA, PACE selectivity	32.295***	9.708

Note.– First two estimates from Probit regression, remaining estimates from OLS regressions. Standard errors clustered at school level. *p < 0.10; **p < 0.05; ***p < 0.01.

Table A9: Parameter Estimates

Symbol	Description	Estimate	Standard Error
	A. Preferences		
ξ_1	Linear term, effort cost	-0.141^{***}	0.0045
ξ_2	Quadratic term, effort cost	-0.029^{***}	0.0054
ξ_3	Coefficient on treatment in effort cost for those w/ no intention to enroll	-0.020^{**}	0.0081
$\tilde{\alpha}$	Time preference	1.384^{***}	0.0079
c^S	Cost of taking PSU exam	0.467^{***}	0.0021
λ_{01}	Constant in utility from college enrollment, type 1	0.802^{***}	0.0065
λ_{02}	Constant in utility from college enrollment, type 2	0.607^{***}	0.0066
λ_1	Very-low-SES in utility from college enrollment	-0.500^{***}	0.0027
λ_2	Above median ability in utility from college enrollment	0.052^{***}	0.0041
λ_3	Program selectivity in utility from college enrollment	0.001	0.0007
δ^E	Stigma: disutility from PACE enrollment	0.999^{***}	0.0074
δ^A	Stigma: disutility from PACE admission	0.498^{***}	0.0067
	B. Technology		
α_{01}	Constant in achievement, type 1	-0.001	0.0089
α_{02}	Constant in achievement, type 2	-1.132^{***}	0.0045
α_{11}	Age in achievement	0.132^{***}	0.0026
α_{12}	Female in achievement	-0.238^{***}	0.0035
α_{13}	Very-low-SES in achievement	-0.093^{***}	0.0050
α_{14}	Never failed a year in achievement	-0.169^{***}	0.0068
α_{15}	Academic track in achievement	0.116^{***}	0.0038
α_2	Effort in achievement	0.281^{***}	0.0074
α_3	Lagged test score in achievement	0.619^{***}	0.0070
β_0^G	Constant in GPA	2.125^{***}	0.0020
β_1^G	Effort in GPA	0.037^{***}	0.0014
β_2^G	Lagged GPA in GPA	0.619^{***}	0.0052
β_0^P	Constant in PSU entrance exam score	-1.399^{***}	0.0038
β_1^P	Effort in PSU entrance exam score	0.161^{***}	0.0070
β_2^P	Lagged test score in PSU entrance exam score	0.602^{***}	0.0057
	C. Subjective Beliefs		
β_{01}^{Pb}	Constant in believed PSU entrance exam score, type 1	-1.393^{***}	0.0076
β_{02}^{Pb}	Constant in believed PSU entrance exam score, type 2	-1.696^{***}	0.0025
β_{11}^{Pb}	Effort in believed PSU entrance exam score, type 1	0.331^{***}	0.0047
β_{12}^{Pb}	Effort in believed PSU entrance exam score, type 2	0.262^{***}	0.0049
β_2^{Pb}	Lagged test score in believed PSU entrance exam score	0.952^{***}	0.0052
β_0^{Gb}	Constant in believed GPA	-2.201^{***}	0.0038
β_{11}^{Gb}	Effort in believed GPA, type 1	0.353^{***}	0.0026
β_{12}^{Gb}	Effort in believed GPA, type 2	0.148^{***}	0.0069
β_2^{Gb}	Lagged GPA in believed GPA	1.208^{***}	0.0047
γ_0^b	Constant in subj. prob. regular admission	0.408^{***}	0.0071
γ_1^b	Believed entrance exam score in subj. prob. regular admission	0.910^{***}	0.0054
ξ_0^b	Constant in subj. prob. PACE admission	1.064^{***}	0.0051
ξ_1^b	Perceived distance from cutoff in subj. prob. PACE admission	0.182^{***}	0.0054
	D. Unobserved Heterogeneity and Shocks		
ω_0	Constant in prob. type 1	-1.501^{***}	0.0011
ω_1	Missing survey in prob. type 1	-1.498^{***}	0.0004
ω_2	Lagged GPA in prob type 1	0.498^{***}	0.0039
$\sigma^{m.e.y.}$	St. dev. of measurement error on achievement	0.775^{***}	0.0034
$\sigma^{m.e.e.}$	St. dev. of measurement error on hours of study	2.720	0.0023
σ_G	St. dev. GPA shock	0.553^{***}	0.0030
σ_P	St. dev. PSU entrance exam shock	0.401^{***}	0.0060
ρ	Correlation coefficient of GPA and PSU shocks	0.873^{***}	0.0025

Note. – Standard Errors bootstrapped using 50 bootstrap samples. $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A10: Model Fit

	Sample	Data	Simulations
A. Beliefs			
Believed minus actual PSU score | sat PSU (σ)	Control	0.591	0.609
Believed minus actual 12^{th} grade GPA (GPA points)	Control	-0.075	-0.060
Believes is in top 15% of school	Control	0.431	0.376
Perceived returns to effort, GPA	All	0.177	0.123
Perceived returns to effort, PSU	All	0.299	0.188
B. Disutility from Preferential Channel			
Enrolls regular	Admitted through both	0.555	0.610
Enrolls preferential	Admitted through both	0.283	0.279
Regular selectivity	Admitted through both	497.1	489.3
Preferential selectivity	Admitted through both	513.4	502.7
C. Treatment Effects			
Achievement score	All	-0.121	-0.072
Study hours	All	-0.258	-0.371
Admissions	All	0.037	0.045
Enrollments	All	0.027	0.020
Entrance-exam taking	All	-0.036	-0.013

Note. – Perceived returns to effort are the expected change in outcome for an additional hour of study per week in the semester. Expected PSU is measured in standard deviations; expected GPA is measured on a scale from 1 to 7. Study hours are measured in reported study hours per week in the semester. The treatment effects are obtained from the same regressions and probit models used to estimate the treatment effects in section 3, except that here we do not use fieldworker fixed effects, because their influence is not modelled and, therefore, they are not part of the simulated data.

B Additional Figures

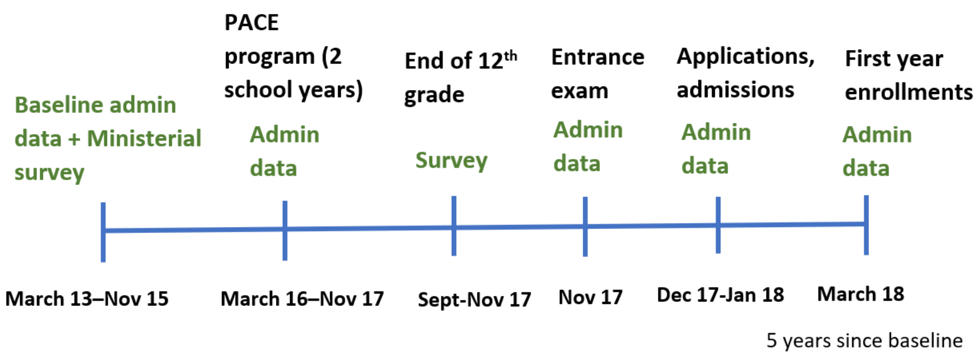

Figure A1: Timeline. Two-digit numbers refer to years (e.g. 13 means year 2013).

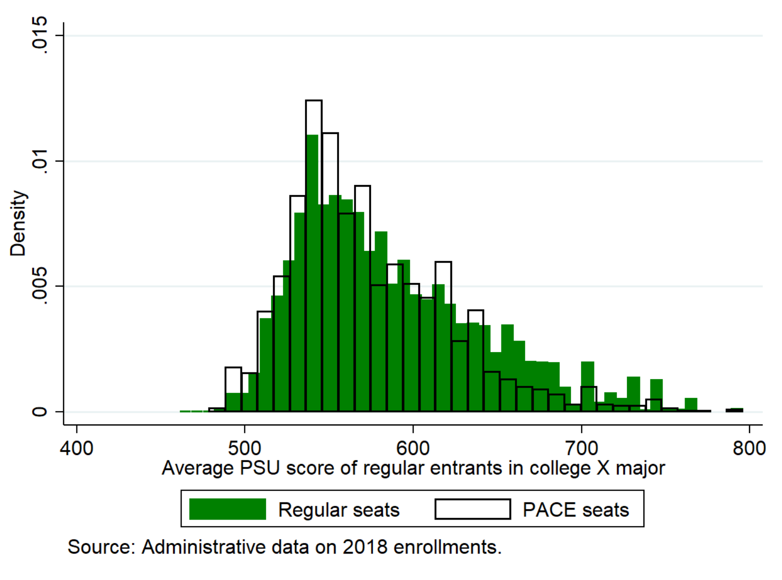

Figure A2: Quality distribution of PACE and regular college seats.

Belief over:	**Question:**	**Possible answers:**
Score on the PSU entry exam.	*Suppose that you will sit the PSU entry exam this year. What do you think your PSU score will be?*	• *700-850 (excellent)* • *600-700 (very good)* • *450-600 (good)* • *350-450 (modest)* • *250-350 (unsatisfactory)* • *150-250 (very unsatisfactory)* • *I don't know*
Own GPA.	*Thinking of yourself, what do you think your grade point average (GPA) will be at the end of high-school? (Introduce a number between 1.0 and 7.0)*	*Free format*
Percentiles of the GPA distribution in the school.	*Suppose that, in your school, there are 40 students in 12th grade. Think of the student with the **highest** grade point average (GPA) among the 40 students. (GPA is a number between 1.0 and 7.0).* *What do you think is the GPA that he/she has?* *Now think of the student with the 6th highest grade point average (GPA) among the 40 students. His/her GPA is in the **top 15%**.* *What do you think is the GPA that he/she has?* [This set of questions further elicits beliefs about the 12th student (top 30%) and the 30th student (bottom 25%)]	*Free format*

Figure A3: Selected survey questions.

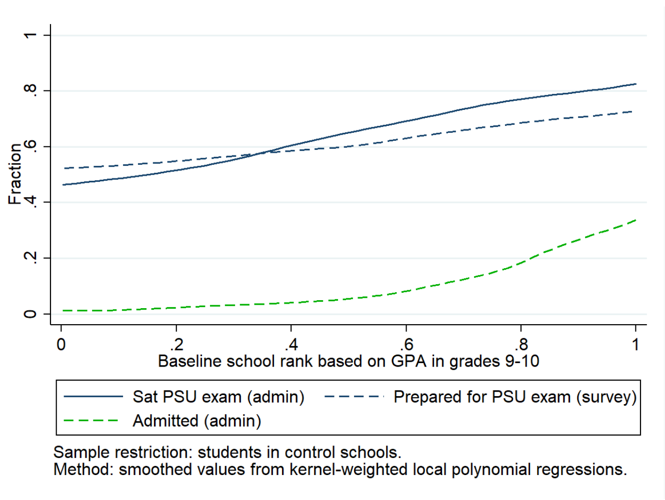

Figure A4: Decision to take and prepare for PSU entrance exam and objective admission likelihood.

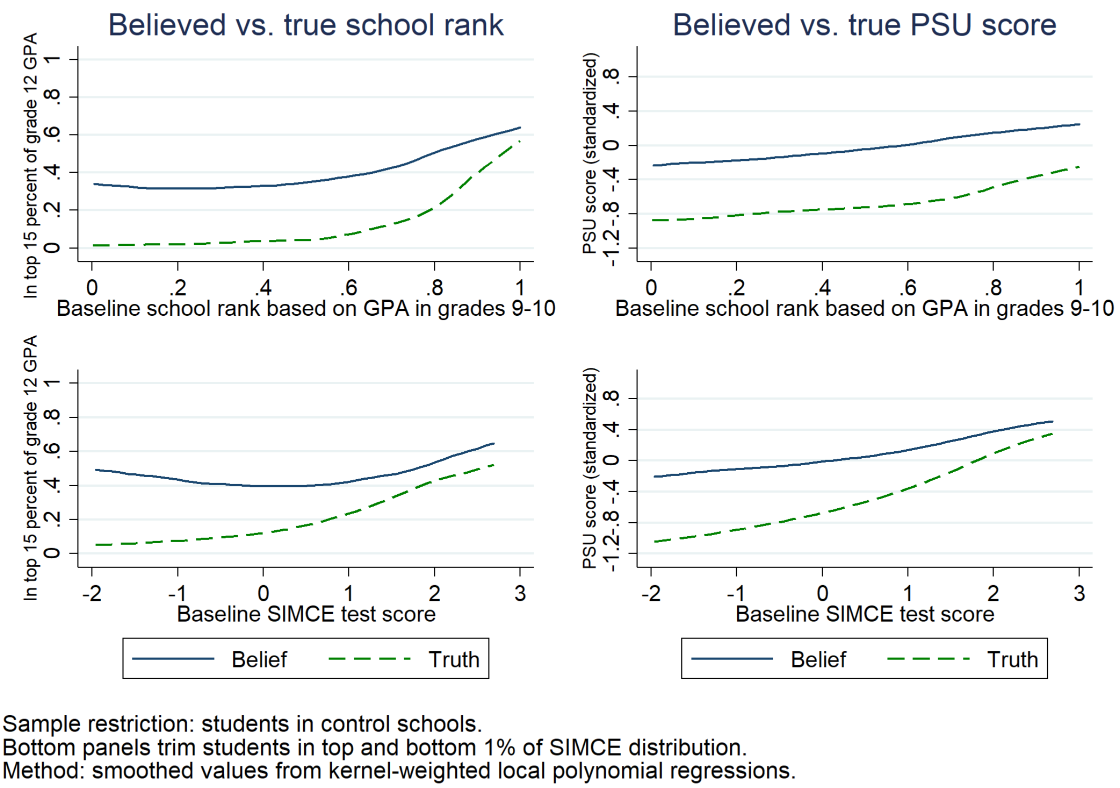

Figure A5: Heterogeneity of subjective beliefs by baseline within-school rank and by baseline test scores.

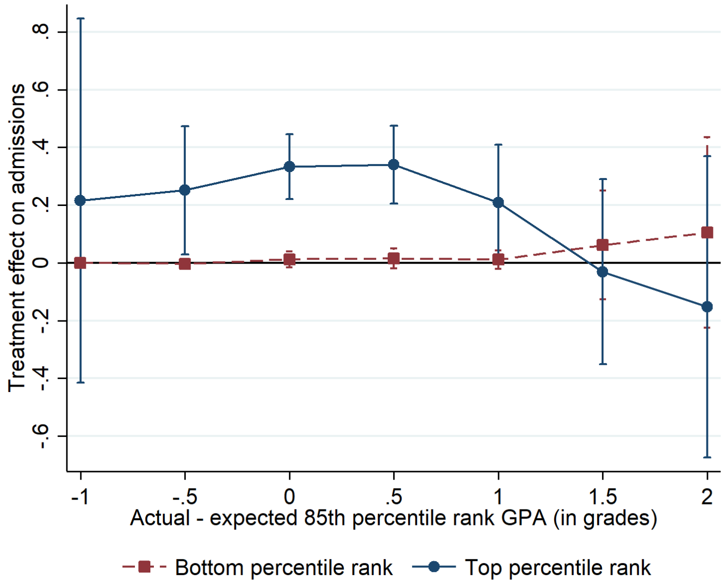

Figure A6: Treatment effect on admissions by belief bias. The figure shows treatment effects on admissions at different points of the belief bias distribution. Treatment effects are shown for those at the bottom and top GPA percentile at baseline. Positive values on the x-axis indicate that a student thinks that the cutoff is easier to reach than in actually is (overoptimism); negative values indicate that a student thinks that the cutoff is harder to reach than it actually is (overpessimism). Error bars are 95% confidence intervals (based on standard errors clustered at school level). Treatment effect estimates are marginal effects computed from the estimates of the Probit model in equation (4), with $\bar{B} = 4$ and $\bar{G} = 3$ (the orders of the polynomials are chosen using Akaike's information criterion).

C Fieldwork Information

All the sampled schools agreed to participate in our study, also thanks to the Ministry of Education, who encouraged school principals to participate. Our fieldworkers visited the schools several times and were able to survey all students who were present.

Students filled out paper questionnaires. Schools allowed us to administer our survey during class time. Our survey displaced one lecture. It took students approximately 50 minutes to fill out the questionnaire. At the start of the data collection, fieldworkers explained that they take an achievement test for the first 20 minutes, and that they would be entered into a lottery to win an iPad, with the number of lottery tickets determined by the number of correct answers.[39] At the 20 minute mark, fieldworkers told students to stop working on the achievement test and to proceed to the survey part of the questionnaire. If a student completed the achievement test before the 20 minutes were up, she was allowed to proceed to the survey.

[39]The professional testing agencies Aptus Chile and Puntaje Nacional developed the test and we extensively piloted it.

To limit the influence of the fieldworkers, the instructions were printed on the first page of the survey and the fieldworkers enunciated them. To further harmonize the data collection across fieldworkers, they had to submit check-lists to their supervisors. During the first 20 minutes, the fieldworkers acted as invigilators. To further avoid cheating, we produced 6 versions of the achievement test. Versions differed in the question order. To ensure that all students faced questions of increasing difficulty, we assigned questions to three different difficulty categories (based on the difficulty index provided by the testing agencies and on extensive piloting on our target population), and we randomized the order of the questions within each category. Students were told, at the start of the test, that they would not all have identical tests.

The questionnaires did not show logos of any Ministry or public agency.

D Robustness analysis

D.1 Strategic high-school enrollment

There is no evidence of strategic high-school enrollment (where advantaged students enroll in disadvantaged schools to benefit from the top 15% rule) because parents were informed a school was treated only after the deadline for school enrollment. They did not have an incentive to change their school selection at a later time because a requirement to benefit from the percent rule is continuous attendance for the last two high-school years (Section 2).[40] Nonetheless, three sets of evidence point to a lack of strategic school enrollment. First, the treated and control students are balanced on baseline student characteristics. Second, the expected impact of strategic enrollment is to produce higher pre-college achievement in treated schools (where advantaged students move to) than control schools. This is the opposite of what we observe. Third, we collected administrative data on school transitions into and out of the treated schools around the start of the experiment. The results are reported in Table G3 of the supplementary material: transitions into or out of the treated schools do not depend on a student's background. We interpret these results as evidence that the policy impacts we estimate cannot be attributed to strategic high-school enrollment.

D.2 Survey attrition

The response rate in our survey data is 69.4% percent in the control group, and it is not statistically significantly different in the treatment group, suggesting the absence

[40] Even so, we restrict the sample to students enrolled in the same school for the last two high-school years, which has a negligible impact on the sample and estimates.

of selective attrition. Table A11 presents Lee (2009) bounds for the treatment effects, confirming that the estimated treatment effects are not due to selective attrition.

Table A11: Lee bounds for average treatment effects

Treatment effect on	Lower bound (1)	Upper bound (1)
Standardized achievement score (res)	-0.209	-0.024
Standardized study effort (res)	-0.285	-0.012
Standardized achievement score	-0.163	-0.013
Standardized study effort	-0.268	0.005

Note.– This table presents Lee (2009) bounds on the average treatment effect of being in a PACE school on pre-college achievement and effort. In the first and second rows we use residuals from a regression of the outcomes on baseline test scores as the dependent variable. In the third and fourth rows we use the raw outcome variables. In all rows we scale the outcomes as in Table 5, to keep our analysis of bounds analogous to the main average treatment effects.

E Technical Appendix

E.1 A theoretical framework for social preferences

We develop a model of student behavior that allows us to derive testable implications of social preferences. Because PACE is a rank-tournament scheme, to describe behavior in the treatment group we develop a tournament model.

E.1.1 Model setup

We build on the seminal Lazear and Rosen (1981) tournament model. There are two students in a school: $i \in \{1, 2\}$. GPA is the sum of a student's effort and a mean-zero shock: $y_i = e_i + \epsilon_i$, $i = 1, 2$. A student obtains a preferential admission if her GPA is above her competitor's. Let $\psi = \epsilon_1 - \epsilon_2$ have cdf $G(\cdot)$ and mean $E(\psi) = 0$. Assume the shock ψ follows a symmetric distribution around zero, with pdf $g(\psi)$ with full support on the real line, achieving a maximum at zero and decreasing for $\psi > 0$. For example ϵ_1 and ϵ_2 are Normal, so that ψ is Normal. Then, the probability that student i obtains a preferential admission is $Prob(\psi < e_i - e_j) = G(e_i - e_j)$.

Our first departure from the model in Lazear and Rosen (1981) is to allow for heterogeneous agents, because students in our data have different baseline abilities and grades, which affect regular and preferential admission likelihoods. We assume that student 1 (2) is high-ability (low-ability): she can (cannot) access college through the regular channel for any effort level. Students are heterogeneous in their cost of producing GPA: $c_i(e_i) = \frac{e_i^2}{2c_i}$, $c_i > 0$, $i = 1, 2$. For example, a student with a higher lagged GPA may be able to produce GPA at a lower cost.

Our second departure from the model in Lazear and Rosen (1981) is to allow for social preferences. We model them following the seminal Bandiera, Barankay, and Rasul (2005) model of social incentives. Letting P_i denote i's likelihood of being admitted to college through at least one channel and $W_i > 0$ i's valuation of college, the utility function is $u_i = P_i \cdot W_i + \alpha e_i - c_i(e_i) + \pi (P_j \cdot W_j + \alpha e_j - c_j(e_j))$, with $i \neq j$. The term αe_i, $\alpha > 0$, captures utility from effort, for example, students value human capital, which is accumulated through effort. The parameter π captures a social preference. Whenever student i's effort affects student j's admission likelihood, student i takes this externality into account when choosing effort if $\pi \neq 0$.

First, Proposition 2 in section E.1.2 provides sufficient conditions for an interior pure-strategy equilibrium of the tournament game to exist and to be unique. Existence and uniqueness of the equilibrium are ensured if performance (GPA) is a sufficiently noisy function of effort, which is a commonly invoked condition to prove equilibrium existence and uniqueness in rank-order tournaments (Lazear and Rosen, 1981; Nalebuff and Stiglitz, 1983).

We can now derive implications for students' effort in the control and treatment groups. When PACE is absent, students do not impose externalities, because i's effort cannot affect j's admission probability $\forall i \neq j$. Therefore, utilities in the control group reduce to: $u_{iC} = P_i W_i + \alpha e_i - \frac{1}{2}\frac{e_i^2}{c_i}$, where $P_1 = 1$ and $P_2 = 0$. The utility-maximizing effort choices are $e^*_{iC} = c_i \alpha$ and the resulting GPAs are $y_{iC} = e^*_{iC} + \epsilon_i$, $i = 1, 2$.

In the treatment group, student 1 imposes an externality on student 2 because her effort affects 2's admission likelihood, which is equal to: $P_2 = Prob(e_2 + \epsilon_2 > e_1 + \epsilon_1) = Prob(\psi < e_2 - e_1) = G(e_2 - e_1)$.[41] Players choose effort to maximize their payoff. Assuming interior solutions, this implies that the equilibrium effort choices satisfy the first order conditions

$$\alpha - \frac{e_1}{c_1} - \pi W_2 g(e_2 - e_1) = 0 \tag{16}$$

$$\alpha - \frac{e_2}{c_2} + W_2 g(e_2 - e_1) = 0 \tag{17}$$

for students 1 and 2 respectively. From equation (16) it is clear that social preferences affect behavior at all effort and parameter levels only when the shock distribution $g(\cdot)$ has full support. If $g(\cdot)$ was equal to zero for some values of $e_2 - e_1$, social preferences would not affect behavior at those values because the parameter π would be multiplied by zero. Intuitively, the full support assumption captures the idea that students of any ability, even those who are not marginal for a preferential admission (for example, because they are considerably more or less able than their peers) can affect the admission likelihood of their peers.[42]

A model implication is that when $\pi \geq 0$, the treatment increases the likelihood that the high-ability student ranks below the low-ability student in the school's GPA distribution. In particular, under social preferences, the high-ability student lowers her effort when treated to ease the admission of the low-ability student through the preferential channel. Therefore, the admission likelihood of the lower-ranking student

[41] Conversely, student 2 affects student 1's preferential admission probability. But since student 1 is always admitted through the regular channel, student 2 does not affect student 1's likelihood of obtaining at least one admission. Therefore, student 2's effort does not affect 1's payoff.

[42] In this simple model being marginal is necessary for the policy to affect effort also in the absence of social preferences (the term $g(e_2 - e_1)$ enters also the first order condition of student 2, equation (17), even though student 2 does not impose an externality on student 1). In a richer model, marginality remains necessary only under social preferences. The simple model ignores cases such as that in which a student is uncertain about a regular admission but certain about a preferential one. In this case, and in all cases in which the policy changes the returns to effort in the admission likelihood, self-interested students change their effort in response to the policy even if they are not marginal. But for social preferences to generate a policy response, marginality is necessary.

should be higher in the treatment than in the control group. We formally derive these predictions below.

Proposition 1 (***Across-groups Difference in Admissions among the Low Ranking.***)**.** *Let e^*_{iT} be the effort of treated student i at an interior Nash Equilibrium, y_{iT} be the resulting GPA, and $P_{iT}(e^*_{iT})$ the resulting admission likelihood, $i = 1, 2$. Let e^*_{iC} be the utility maximising effort of control student i, y_{iC} be the resulting GPA, and $P_{iC}(e^*_{iC})$ be the resulting admission likelihood, $i = 1, 2$. Define the low-ranking student as the student with the lowest GPA at an interior Nash Equilibrium. Let $P_{LR,T}$ and $P_{LR,C}$ denote the probabilities that the low-ranking student is admitted to college in the treatment and control group, respectively.*

- *If $\pi > 0$, then the policy increases the likelihood that the high-ability student, 1, ranks below the low-ability student, 2, in terms of GPA. (Formally, $e^*_{1T} < e^*_{1C}$ and $e^*_{2T} > e^*_{2C}$, so that $prob(y_{1T} < y_{2T}) > prob(y_{1C} < y_{2C})$). Then, the admission likelihood of the low-ranking student is larger in the treatment than in the control group. (Formally, $P_{LR,T} > P_{LR,C}$).*
- *If $\pi = 0$, then the policy increases the likelihood that the high-ability student, 1, ranks below the low-ability student, 2, in terms of GPA (Formally, $e^*_{1T} < e^*_{1C}$ and $e^*_{2T} > e^*_{2C}$, so that $prob(y_{1T} < y_{2T}) > prob(y_{1C} < y_{2C})$). Then, the admission likelihood of the low-ranking student is larger in the treatment than in the control group. (Formally, $P_{LR,T} > P_{LR,C}$).*[43]
- *If $\pi < 0$, then the sign of the policy impact on the probability that 1 ranks below 2 is ambiguous. Then, it is ambiguous whether the admission likelihood of the low-ranking student is larger or lower in the treatment than in the control group.*

Proof. The probability that the low-ranking student is admitted is equal to:

$$P_{LR,J} = prob(y_{1J} < y_{2J}) * 1 + prob(y_{1J} > y_{2J}) * 0 = prob(y_{1J} < y_{2J}), \quad J \in \{T, C\},$$

i.e., it is the probability that the low-ranking student is student 1 times the probability that student 1 is admitted, which is equal to 1, plus the probability that the low-raking student is student 2 times the probability that student 2 is admitted when she is low ranking, which is equal to 0. The across-treatment-groups difference in the admission

[43] In this simple model, the low-ability student (2) increases effort in response to the treatment, leading to this (probabilistic) rank reversal even when students do not have social preferences and, therefore, even when the high-ability student (1) does not lower her effort to rank below student 2. In a richer model where, for example, student 2 does not respond to the treatment when she is so far from the preferential admission cutoff that the treatment does not increase her returns to effort, the implication $P_{LR,T} > P_{LR,C}$ would not necessarily hold under $\pi = 0$, but it would still hold under $\pi > 0$, because it would be driven by the effort reduction of the high-ability student.

likelihood of the low-ranking student is $P_{LR,T} - P_{LR,C} = prob(y_{1T} < y_{2T}) - prob(y_{1C} < y_{2C})$. To prove the result, we now study the sign of $prob(y_{1T} < y_{2T}) - prob(y_{1C} < y_{2C})$.

From the first order condition in (16), $g(e_2 - e_1) = \frac{\left(\alpha - \frac{e_1}{c_1}\right)}{\pi W_2}$, from the first order condition in (17), $g(e_2 - e_1) = \frac{\left(\frac{e_2}{c_2} - \alpha\right)}{W_2}$. Setting the two RHS (right-hand side) expressions equal to each other and rearranging, we obtain e_2 as a function of e_1: $e_2 = \frac{c_2}{\pi}\left[\alpha(1+\pi) - \frac{e_1}{c_1}\right]$. Plugging the expression for e_2 into student 1's first order condition in (16), we obtain that in a Nash Equilibrium e_1^{*T} must satisfy the following equation:

$$e_1^{*T} = \alpha c_1 - \pi W_2 c_1 g\Big(\frac{c_2}{\pi}\big[\alpha(1+\pi) - \frac{e_1^{*T}}{c_1}\big] - e_1^{*T}\Big). \tag{18}$$

The first term in the summation on the RHS coincides with student 1's effort in the control group, e_1^{*C}. Therefore, when $\pi = 0$, $e_1^{*T} = e_1^{*C}$; when $\pi > 0$, $e_1^{*T} < e_1^{*C}$, because $W_2 > 0, c_1 > 0$ and $g(\cdot) > 0$; when $\pi < 0$, $e_1^{*T} > e_1^{*C}$.

By the same manipulation of the first order conditions, we can write e_1 as a function of e_2: $e_1 = c_1\left[\alpha(1+\pi) - \frac{\pi e_2}{c_2}\right]$. Plugging the expression for e_1 into student 2's first order condition in (17), we obtain that in a Nash Equilibrium e_2^{*T} must satisfy the following equation:

$$e_2^{*T} = \alpha c_2 + W_2 c_2 g\Big(e_2^{*T} - c_1\big[\alpha(1+\pi) - \frac{\pi e_2^{*T}}{c_2}\big]\Big). \tag{19}$$

The first term in the summation on the RHS coincides with student 2's effort in the control group, e_2^{*C}. Therefore, $e_2^{*T} > e_2^{*C}$.

GPA is the sum of effort and shock ϵ_i. Therefore, $prob(y_{1C} > y_{2C}) = G(e_{1C}^* - e_{2C}^*)$ and $prob(y_{1T} > y_{2T}) = G(e_{1T}^* - e_{2T}^*)$. But then, $prob(y_{1C} > y_{2C}) > prob(y_{1T} > y_{2T})|_{\pi=0} > prob(y_{1T} > y_{2T})|_{\pi>0}$ because $e_2^{*T} > e_2^{*C}$ and $e_1^{*T}|_{\pi>0} < e_1^{*T}|_{\pi=0} = e_1^{*C}$. In particular, under social preferences ($\pi > 0$) the policy has an unambiguously negative impact on the probability that agent 1's GPA ranks above agent 2's GPA within the school. On the other hand, $prob(y_{1T} > y_{2T})|_{\pi<0}$ can be larger or smaller than $prob(y_{1C} > y_{2C})$, because $e_2^{*T} > e_2^{*C}$ but $e_1^{*T} > e_1^{*C}$, so that the sign of such policy impact is ambiguous when $\pi < 0$.

□

E.1.2 Equilibrium Existence and Uniqueness

Proposition 2 (***Equilibrium Existence and Uniqueness.***)**.** *If* $g(\alpha c_2(1+1/\pi)) < \frac{\alpha}{\pi W_2}$ *and* $\max_x g'(x) < \frac{1}{W_2(\pi c_1 + c_2)}$, *an interior pure strategy equilibrium exists and is unique.*

Proof. Let $L_1(e_1)$ and $R_1(e_1)$ be the left and right-hand side of equilibrium equation (18) and let $L_2(e_2)$ and $R_2(e_2)$ be the left and right-hand side of equilibrium equation (19).

Taking limits of the equilibrium equation (18) we obtain $\lim_{e_1\to 0} L_1(e_1) = 0$, $\lim_{e_1\to 0} R_1(e_1) = \alpha c_1 - \pi W_2 c_1 g(\alpha c_2(1 + 1/\pi))$, $\lim_{e_1\to\infty} L_1(e_1) = \infty$ and $\lim_{e_1\to\infty} R_1(e_1) = \alpha c_1$. By continuity of $L_1(e_1)$ and $R_1(e_1)$ in e_1, a sufficient condition for the existence of an interior solution $e_1 > 0$ is:

$$g(\alpha c_2(1 + 1/\pi)) < \frac{\alpha}{\pi W_2}. \tag{20}$$

Taking limits of the equilibrium equation (19) we obtain $\lim_{e_2\to 0} L_2(e_2) = 0$, $\lim_{e_2\to 0} R_2(e_2) = \alpha c_2 + \pi W_2 c_1 g(-\alpha c_1(1 + \pi))$, $\lim_{e_2\to\infty} L_2(e_2) = \infty$ and $\lim_{e_2\to\infty} R_2(e_2) = \alpha c_2$. By continuity of $L_2(e_2)$ and $R_2(e_2)$ in e_2, there exists at least one $e_2 > 0$ that solves the equilibrium equation for e_2. Therefore, at least one interior equilibrium exists.

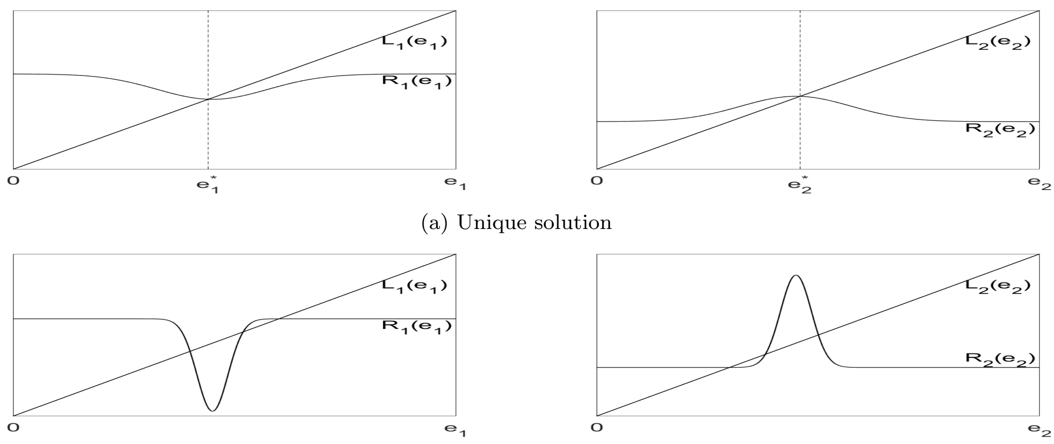

Figure A7: Equilibrium existence and uniqueness

To find conditions for equilibrium uniqueness, it helps to plot the right-hand-side and the left-hand-side of the two equilibrium equations. From Figure A7, we can see that sufficient conditions for uniqueness are that $R_1'(e_1) < L_1'(e_1)$ for all levels of e_1 and $R_2'(e_2) < L_2'(e_2)$ for all levels of e_2. This is implied by a single sufficient condition:

$$\max_x g'(x) < \frac{1}{W_2(\pi c_1 + c_2)}. \tag{21}$$

To understand the intuition behind the sufficient conditions for existence (20) and uniqueness (21), assume that $\varepsilon_1 \sim N(0, \sigma_1^2)$ and $\varepsilon_2 \sim N(0, \sigma_2^2)$, $\epsilon_1 \perp \epsilon_2$, so that $\psi = \varepsilon_1 - \varepsilon_2 \sim N(0, \sigma^2)$, where $\sigma^2 = \sigma_1^2 + \sigma_2^2$. As $g(0) > g(\alpha c_2(1 + 1/\pi))$, condition (20) is implied by $\frac{1}{\sqrt{2\pi\sigma^2}} = g(0) < \frac{\alpha}{\pi W_2}$. Rearranging we obtain a lower bound on the variance of the shocks:

$$\sigma^2 > \frac{\pi W_2^2}{2\alpha^2}. \tag{22}$$

The maximum of the derivative of the Normal $g(\cdot)$ is reached at the first inflection point: $\max_x g'(x) = g'(-\sigma) = \frac{1}{\sigma^2\sqrt{2\pi e}}$. Then, condition (21) imposes another lower bound on the variance:

$$\sigma^2 > \frac{W_2(\pi c_1 + c_2)}{\sqrt{2\pi e}}. \tag{23}$$

□

E.2 Structural model parameterizations

This section describes the functional form assumptions we make in estimating the structural model.

The production functions of the PSU score and of GPA are as follows:

$$PSU_i = \beta_0^P + \beta_1^P e_i + \beta_2^P y_{i,t-1}^{(1)} + \epsilon_i^P, \tag{24}$$

$$GPA_i = \beta_0^G + \beta_1^g e_i + \beta_2^G y_{i,t-1}^{(2)} + \epsilon_i^G, \tag{25}$$

where $y_{i,t-1}^{(1)}$ is a baseline standardized test score and $y_{i,t-1}^{(2)}$ is the baseline GPA (we restrict GPA_i to be between 1 and 7). We assume that the technology shocks $\epsilon_i = [\epsilon_i^P, \epsilon_i^G]$ are distributed as bivariate normal: $\epsilon_{it} \sim N(0, \Sigma)$, with $\Sigma = \begin{bmatrix} \sigma_P^2 & \rho\sigma_P\sigma_G \\ \rho\sigma_P\sigma_G & \sigma_G^2 \end{bmatrix}$. Given a PSU score, the probability of a regular admission is

$$Pr(A_i^R = 1 | PSU_i, S_i = 1; \gamma) = \Phi(\gamma_0 + \gamma_1 PSU_i). \tag{26}$$

The subjective production functions of the PSU score and of GPA are as follows:

$$PSU_{it}^b = \beta_{0k_i}^{Pb} + \beta_{1k_i}^{Pb} e_{it} + \beta_2^{Pb} y_{it-1}^{(1)} + \epsilon_{it}^{PSU^b}, \quad \epsilon_{it}^{PSU^b} \sim N(0, \sigma_{PSU^b}^2) \tag{27}$$

$$GPA_{it}^b = \beta_0^{Gb} + \beta_{1k_i}^{Gb} e_{it} + \beta_2^{Gb} y_{it-1}^{(2)} + \epsilon_{it}^{GPA^b}, \quad \epsilon_{it}^{GPA^b} \sim N(0, \sigma_{GPA^b}^2) \tag{28}$$

where the shocks $(\epsilon_{it}^{PSU^b}, \epsilon_{it}^{GPA^b})$ are i.i.d. normal and capture belief uncertainty. Observationally identical students hold heterogeneous beliefs about the production function: parameters $\beta_{0k_i}^{Pb}, \beta_{1k_i}^{Pb}, \beta_{1k_i}^{Gb}$ vary with the student's unobserved type. The believed outcomes vary also with baseline characteristics and effort.

The subjective probability of a regular admission, conditional on taking the PSU entrance exam ($S_i = 1$), is equal to the subjective probability that a student's believed score will be above the believed admission cutoff. Students form a subjective probability distribution for the admission cutoff: $c_i^{Rb} \sim N(\bar{c}^{Rb}, \sigma^2_{c^{Rb}})$. Letting $\overline{PSU}_{it}^b = \beta_{0k_i}^{Pb} + \beta_{1k_i}^{Pb} e_{it} + \beta_2^{Pb} y_{it-1}^{(1)}$ denote the expected PSU score, $\epsilon_i^{c^{Rb}}$ the mean-zero additive belief shock around the expected cutoff, and A_i^R a dummy for a regular admission, the subjective probability of a regular admission is:

$$\begin{aligned} Pr^b(A_i^R = 1 | e_{it}, y_{it-1}^{(1)}, k_i, S_i = 1) &= Pr\left(\overline{PSU}_{it}^b + \epsilon_i^{PSU^b} \geq \bar{c}^{Rb} + \epsilon_i^{c^{Rb}}\right) \qquad (29) \\ &= \Phi\left(\frac{\overline{PSU}_{it}^b - \bar{c}^{Rb}}{\sqrt{\sigma^2_{PSU^b} + \sigma^2_{c^{Rb}}}}\right) \\ &= \Phi\left(\gamma_0^b + \gamma_1^b \overline{PSU}_{it}^b\right), \end{aligned}$$

where $\gamma_0^b = \frac{-\bar{c}^{Rb}}{\sqrt{\sigma^2_{PSU^b} + \sigma^2_{c^{Rb}}}}$ and $\gamma_1^b = \frac{1}{\sqrt{\sigma^2_{PSU^b} + \sigma^2_{c^{Rb}}}}$ and $\Phi(\cdot)$ is the standard Normal cumulative distribution function. Given an expected PSU score, uncertainty is generated by uncertainty around own score ($\sigma^2_{PSU^b}$) and around the admission cutoff ($\sigma^2_{c^{Rb}}$), which are absorbed by the parameters γ_0^b and γ_1^b. As it is standard to impose functional form restrictions on subjective probabilities (e.g. Delavande and Zafar (2019); Kapor, Neilson, and Zimmerman (2020)), we impose normality.

Letting $\overline{GPA}_{it}^b = \beta_0^{Gb} + \beta_{1k_i}^{Gb} e_{it} + \beta_2^{Gb} y_{it-1}^{(2)}$ denote the expected GPA, ϵ_i^{c15b} the mean-zero belief shock around the expected school cutoff[44], and A_i^P a dummy for a preferential admission, the subjective probability of a preferential admission, conditional on taking the entrance exam ($S_i = 1$), for students in treated schools is:

$$\begin{aligned} Pr^b(A_i^P = 1 | e_{it}, y_{it-1}^{(2)}, k_i, S_i = 1) &= Pr\left(\overline{GPA}_{it}^b + \epsilon_i^{GPA^b} \geq c_0 + c\bar{1}5_i^b + \epsilon_i^{c15b}\right) \qquad (30) \\ &= \Phi\left(\frac{\overline{GPA}_{it}^b - c_0 - c\bar{1}5_i^b}{\sqrt{\sigma^2_{GPA^b} + \sigma^2_{c15^b}}}\right) \\ &= \Phi\left(\xi_0^b + \xi_1^b(\overline{GPA}_{it}^b - c\bar{1}5_i^b)\right), \end{aligned}$$

[44]Students form a subjective probability distribution for the cutoff in their school: $c15_i^b \sim N(c\bar{1}5_i^b, \sigma^2_{c15^b})$, characterized by a heterogeneous expected cutoff, $c\bar{1}5_i^b$, with uncertainty around it, $\sigma^2_{c15^b}$. We assume our survey instrument measured the expected cutoff $c\bar{1}5_i^b$ for each student i. The elicited $c\bar{1}5_i^b$ is missing for less than 20% of students. We assume these students correctly predict the cutoff; thus, results provide a lower bound to the role that biased rank beliefs play in policy response.

where $\xi_0^b = \frac{-c_0}{\sqrt{\sigma^2_{GPA^b} + \sigma^2_{c15^b}}}$ and $\xi_1^b = \frac{1}{\sqrt{\sigma^2_{GPA^b} + \sigma^2_{c15^b}}}$.[45] Given an expected GPA and an expected cutoff, uncertainty is generated by the uncertainty around own GPA ($\sigma^2_{GPA^b}$) and around the school cutoff ($\sigma^2_{c15^b}$), which are absorbed by the parameters ξ_0^b and ξ_1^b. As before, we assume normality.

In the first period, the per-period utility from effort depends on how effort affects achievement. We assume achievement is produced as follows: $y_i = \alpha_{0k_i} + \alpha_1 x_i + \alpha_2 e_{it} + \alpha_3$. We assume that our survey measures study effort with additive noise: $e_i^o = e_i + \epsilon_i^{m.e.e.}$, where $\epsilon^{m.e.e.} \sim N(0, \sigma^2_{m.e.e.})$ is a classical measurement error. We assume that our standardized test score measures achievement with additive noise: $y_i^o = y_i + \epsilon_i^{m.e.y.}$, with $\epsilon_i^{m.e.y.} \sim N(0, \sigma^2_{m.e.y.})$.

As in the real-world admission system, the selectivity of an admission depends on a student's PSU (for regular admissions) and GPA (for preferential admissions). We assume the following functional forms:

$$q^R(PSU_i) = \lambda_0^R + \lambda_1^R PSU_i + \epsilon_i^{qR} \tag{31}$$

$$q^P(GPA_i) = \lambda_0^P + \lambda_1^P GPA_i + \epsilon_i^{qP}. \tag{32}$$

E.3 Additional identification details

First, we discuss the identification of unobserved heterogeneity. Unobserved types affect parameters of the perceived production functions, the utility from enrolling in college, and achievement. We discuss these sets of parameters separately.

Type-dependent heterogeneity in beliefs. Unobserved heterogeneity and measurement error on the survey answers used to elicit returns to effort generate variation across observationally identical students in perceived PSU scores, GPA, and returns to effort. We assume that the measurement error on the survey answers regarding hours of study under alternative hypothetical outcome scenarios, used to construct beliefs, is identically distributed to the measurement error on the reported actual hours of study. Therefore, variation in reported actual hours of study that is not explained by observed baseline characteristics identifies the variance of the measurement error. Having identified this parameter separately, we can use variation in beliefs between observationally identical students to pin down the unobserved heterogeneity in beliefs.

Type-dependent heterogeneity in the utility from enrolling in college. Observationally identical students who face identical admission sets can make different

[45] Parameter c_0 is a net adjustment to the GPA and the cutoff to capture the fact that the top 15% rule is based on adjusted GPA.

enrollment decisions because of idiosyncratic preference shocks and because of permanent unobserved heterogeneity. To separately identify them we exploit the longitudinal aspect of our data. We observe student's preference-revealing choices at both the exam-taking decision stage and the enrollment stage. Unlike temporary preference shocks, permanent unobserved heterogeneity induces correlations in behavior over time, which allow us to pin down unobserved heterogeneity in the preference for college.

Type-dependent heterogeneity in achievement. Observationally identical students can obtain different scores on the achievement test because of different type-dependent unobserved ability and different realizations of the measurement error. To separately identify them, first, we assume that the type is discrete and the measurement error is continuous. Therefore, the observed modes in the part of the achievement score not explained by observed characteristics are informative about type-specific ability. Second, we exploit the longitudinal aspect of our data. Students of different types obtain different achievement scores, exert different levels of effort, and make different educational choices. Unlike measurement error, permanent unobserved heterogeneity induces correlations between achievement, effort and later outcomes that are not explained by baseline characteristics and, therefore, are informative about unobserved heterogeneity.

Second, we discuss how we mitigate potential endogeneity of the arguments of the subjective probability functions. For the subjective probability of a preferential admission, we use variation that comes from the experiment. The treatment makes this subjective probability salient: differences in choices across treatment groups are informative about the parameters of this subjective probability, because it governs pre-college behavior in the treatment group but not in the control group. For the subjective probability of a regular admission, we assume that there is a continuous characteristics (lagged achievement test score) that affects the expected entrance exam score but not the type distribution. Therefore, conditional on the variables that enter the type distribution (which include lagged GPA), variation in this lagged achievement score is exogenous. The intuition is that this variation captures idyosincratic, test-day shocks that are uncorrelated with a student's true ability or preferences.

E.4 Auxiliary Regressions and Moments

In this section we list the parameters of the auxiliary models and the additional moments we match in estimation. The standard set of controls in the regressions is: age, gender, very-low-SES index (*alumno prioritario*), dummy for whether the student ever failed a grade, school-track type, baseline SIMCE score.

1. *Treatment Effect Regressions:*

   - All parameters, including the constant, of a regression of achievement on treatment, the standard controls, and average GPA in 9^{th} and 10^{th} grade (9).
   - Coefficient on treatment of a regression of hours of study on treatment and the standard controls (1).
   - Coefficient on treatment of a regression of hours of study on treatment and the standard controls for the sample of students who report, at baseline, no intention to attend college (1).
   - Coefficient on treatment of a regression of college enrollment on treatment and the standard controls (1).
   - Coefficient on treatment of a regression of taking the entrance exam on treatment and the standard controls (1).

2. *Descriptive Regressions:*

   - Constant and coefficient of regression of hours of study on dummy for whether student has no intention to stay in school beyond high school (2).
   - Coefficient on 10^{th} grade GPA of regression of 12^{th} grade GPA on 10^{th} grade GPA (1).
   - Coefficient on baseline SIMCE score of regression of entrance exam score on baseline SIMCE score (1).
   - Coefficients on whether the student participated in the survey and on the average between 9^{th} and 10^{th} grade GPA in a regression of whether a student takes the entrance exam on these variables and on the standard controls (2).
   - Coefficient on the average between 9^{th} and 10^{th} grade GPA in a regression of study hours on this variable and on the standard controls (1).

3. *Descriptive Statistics:*

   - Mean and variance of hours of study (2).
   - Fraction of students admitted to college by treatment group and baseline achievement, i.e., above or below median SIMCE score (4).
   - Correlation between regular admissions and PACE admissions for treated students (1).
   - Fraction taking entrance exam by treatment group (2).
   - Mean and variance of entrance exam score by treatment group (4).
   - Fraction of students who enroll in college by treatment group and baseline achievement, i.e., above or below median SIMCE score (4).

- Fraction of students enrolled in college by very-low-SES status, i.e., *alumno prioritario* categorization (2).
- Mean and variance of GPA in the control group (2).
- All pairwise correlations between the expected score on the PSU, enrollment, and the actual score on the PSU (3).
- Mean and variance of perceived returns to effort in GPA production and in PSU production (4).
- Correlation between taking the entrance exam and enrollment in the control group (1).
- Correlation between study hours and enrollment in the control group (1).
- Correlation between study hours and admissions in the control group (1).
- Correlation between taking the entrance exam and perceived distance from the within-school cutoff in the treatment group (1).
- Correlation between taking the entrance exam and expected PSU score in the control group (1).
- Unexplained variation in achievement and GPA after controlling for all initial conditions in the model affecting these outcomes. Specifically, variance of the residuals from regressions of achievement and of GPA on treatment, GPA in 9^{th} grade and average GPA between 9^{th} and 10^{th} grade, a dummy for whether a student reported at baseline to not being interested in attending college, perceived within-school cutoff, and the standard controls (2).
- Fractions enrolling through the regular and through the PACE channel for those admitted through both channels (2).
- Selectivity of the regular and of the PACE admissions for those admitted through both channels (2).
- Mean and variance of expected GPA and PSU score (4).

E.5 Equilibrium of the Tournament Game in the Counterfactuals

In the counterfactuals that debias students' beliefs, we must solve for the Bayesian Nash equilibrium of the tournament game that awards preferential seats. We start by defining the Bayesian Nash Equilibrium (BNE) of the simultaneous effort game in each treated school in the first time period, under the assumption that students have rational expectations. When making effort decisions in time period 1, students observe their type k_i, private information. The joint distribution of types in the school, $F(k_1, k_2, ..., k_n)$, is common knowledge. There are no other shocks privately observed by students in the first time period. The distribution of all other model shocks, which are realized in later periods, is common knowledge. Model shocks include preference $(\eta_{it}, \eta_{it}^R, \eta_{it}^P)$ and technological shocks $(\epsilon_{it}^P, \epsilon_{it}^G)$. Objective production functions are common knowledge. Types make this a game of incomplete information.

$e_i(\cdot)$ is a function mapping $\{1, 2, ..., K\}$ into $\{0, 1, 2, ..., E\}$, the set of effort choices. This is the strategy for student i. Given a profile of pure strategies for all students in the school, $(e_1(k_1), e_2(k_2), ..., e_n(k_n))$, the expected payoff of student i is

$$\tilde{u}_i(e_i(k_i), k_i, e_{-i}(\cdot)) = E_{k_{-i}}[u_i(e_1(k_1), e_2(k_2), ..., e_n(k_n), k_i)],$$

where u_i is the sum of the first period utility and the expected value functions calculated using objective admission likelihoods. Let I denote the set of students in the school and E_i denote the pure strategy set of student i.

Definition 1. Rational Expectations Equilibrium. *A (pure strategy) Bayesian Nash equilibrium for the Bayesian game* $[I, \{E_i\}, \{\tilde{u}_i(\cdot)\}]$ *is a profile of decision rules* $(e_1^*(k_1), e_2^*(k_2), ..., e_n^*(k_n))$ *that are such that, for every* $i = 1, 2, ..., n$ *and for every realization of the type* k_i*,*

$$\tilde{u}_i(e_i^*(\cdot), k_i, e_{-i}^*(\cdot)) \geq \tilde{u}_i(e_i^{'}(\cdot), k_i, e_{-i}^*(\cdot))$$

for all $e_i^{'} \in \{0, 1, 2, ..., E\}$.

Intuition for approximation. Solving for the rational expectations equilibrium requires solving for a multi-dimensional fixed point in the vector of decision rules in each school. To reduce the dimensionality of the problem, we find an approximation to the rational expectations equilibrium.[46] Given an equilibrium profile of strategies for students $-i$, $e_{-i}^*(\cdot)$, each effort choice of student i maps into the expected probability of a preferential admission for student i: $P_i^{15}(e_i, e_{-i}^*(\cdot))$, where the expectation is taken with respect to others' types. It is only through this probability that the strategies of others enter own payoffs. We posit a parametric approximation to this probability, $\check{P}^{15}(e_i, \gamma)$, where γ captures the strategy profiles of students $-i$. Let $\check{u}_i(e_i(\cdot), k_i, \check{P}^{15}(e_i, \gamma))$ denote i's approximated expected payoff.

Definition 2. Approximated Rational Expectations Equilibrium. *An approximation to the (pure strategy) Bayesian Nash equilibrium for the Bayesian game* $[I, \{E_i\}, \{\tilde{u}_i(\cdot)\}]$ *is a* γ^* *that is such that:*

[46] We thank Nikita Roketskiy for suggesting this approximation. All errors are our own.

- *given γ^*, each i and k_i chooses a decision rule $\check{e}_i(k_i)$ that maximizes his/her approximated expected payoff:*

$$\check{u}_i(\check{e}_i(k_i), k_i, \check{P}^{15}(\check{e}_i, \gamma^*)) \geq \check{u}_i(e_i^{'}(\cdot), k_i, \check{P}^{15}(e_i^{'}, \gamma^*))$$

for every $i = 1, 2, ..., n$, $k_i = 1, 2, ...K$ and for all $e_i^{'} \in \{0, 1, 2, ..., E\}$.

- *given the profile of decision rules $(\check{e}_1(k_1), \check{e}_2(k_2), ..., \check{e}_n(k_n))$, the approximated admission probability is close to the true admission probability for all i: $P_i^{15}(\check{e}_i, \check{e}_{-i}(\cdot)) \approx P^{15}(\check{e}_i, \gamma^*)$ $\forall i = 1, ..., n$.*

Algorithm. Solving for the approximated rational expectations equilibrium requires solving for a fixed point problem of the dimension of γ^*. We use a linear probability approximation: $\check{P}^{15}(e_i, \gamma) = \gamma_0 + \gamma_1 GPA_{it}(e_i; \epsilon_{it}^G) + \gamma_2 X_i + \gamma_3 Z_j$, where GPA_{it} is own GPA, X_i are baseline student characteristics and Z_j are baseline school characteristics, and use the following algorithm:

1. Draw types and shocks for all students and fix these draws across iterations.
2. From the data, estimate a linear probability model of the likelihood of a preferential admission as a function of own GPA and of baseline characteristics of the student (X_i) and of the school (Z_j) selected through LASSO:

$$Prob_i(Adm^P = 1 | GPA_{it}, X_i, Z_j) = \gamma_0 + \gamma_1 GPA_{it} + \gamma_2 X_i + \gamma_3 Z_j + \epsilon_{ij}$$

   Let the estimates $\hat{\gamma}_0, \hat{\gamma}_2, \hat{\gamma}_3$ be fixed across iterations, let the estimate $\hat{\gamma}_1$ be our first guess in all schools: $\gamma_{1j}^{(s=0)}$. The goal is to find a fixed point in γ_{1j}.
3. At the current iteration s, let students believe that

$$\begin{aligned} P_i^{15(s)}(e_i, \check{e}_{-i}(\cdot)) &= P_i^{(s)} = \\ &= \hat{\gamma}_0 + \gamma_{1j}^{(s)} GPA_{it}(e_i; \epsilon_{it}^G) + \hat{\gamma}_2 X_i + \hat{\gamma}_3 Z_j. \end{aligned}$$

4. Given these beliefs, find the best reply of each student. Let $e_{it}^{(s)}$ be the utility maximizing effort that each student exerts.
5. Calculate $GPA_{it}^{(s)} = GPA(e_{it}^{(s)}; \epsilon_{it}^G)$. Assign PACE slots to those with a GPA in the top 15 percent of their school and who took the entrance exam.
6. From the simulated data on PACE slot allocations and $GPA(e_{it}^{(s)}; \epsilon_{it}^G)$, compute $\gamma_{1j}^{(s+1)}$ by OLS.
7. If $\gamma_{1j}^{(s+1)}$ is sufficiently different from $\gamma_{1j}^{(s)}$, go back to point 3, otherwise stop.

We checked for uniqueness by plotting the $\gamma_{1j}^{(s+1)}$ against the $\gamma_{1j}^{(s)}$ and found that there is a unique fixed point in all schools.