Subjective Beliefs and Inclusion Policies: Evidence from College Admissions *

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Abstract

Many countries have introduced preferential admissions to provide new opportunities for talented but disadvantaged students to attend college. Their effectiveness critically depends on students' perceptions of their incentive schemes. This paper studies this issue by exploiting a randomized preferential admission policy and linked survey-administrative data for 6,054 high-school students in Chile. We document that these students hold overly optimistic beliefs about their admission credentials. We then estimate policy effects on student behavior and outcomes. We find that pre-college effort and achievement fall by 0.1 standard deviations in response to the policy. We develop and structurally estimate a dynamic model of effort, entrance-exam-taking, admissions and enrollments incorporating subjective beliefs. We show that, by distorting effort, belief biases lead overconfident but underprepared students to enter college in response to the policy. We discuss how preferential admission policies can be redesigned to mitigate such distortions and draw closer to achieving their intended objective.

Keywords: Preferential college admissions, experimental policy evaluation, subjective beliefs, dynamic choice model, tournament model.
JEL Classification: I2, D8.

1 Introduction

Inclusion policies, such as quotas, affirmative action and preferential admissions, are common in major sectors such as employment and education (OECD (2020)). It is well known that they can change the incentives to invest in qualifying for certain positions. Misperceptions of these incentives can induce over- and under-investments that distort the match between candidates and positions. This raises the question of whether and, if so, how misperceptions shape the effects of inclusion policies. Addressing this question is important because a country's economic success depends on how it allocates talent to opportunity.

This paper addresses this open question in the context of preferential college admissions for disadvantaged students in Chile. This context is particularly useful because higher education is starkly unequal across socioeconomic lines globally (UNESCO (2017)). While the Chilean system shares many similarities with other industrialized regions of the world, its centralized admission process and detailed administrative records make it more straightforward to study.[1] We ask two questions: i) What are the impacts of preferential admissions on various outcomes of disadvantaged students, including investments made before college? ii) How do subjective beliefs shape these impacts? To answer them, we exploit a randomized preferential admission policy and linked administrative-survey data.

Our analysis comprises three steps. First, we document the pre-college beliefs of students targeted by preferential admissions. To do so, we surveyed over $6,000$ students enrolled in disadvantaged Chilean high schools identified by the Government as eligible for the PACE percent plan program, which grants college admission to graduates with a grade-point average (GPA) within the top 15% of their school.[2] We linked the survey answers to longitudinal administrative records spanning six years around the transition from high school to college. Our dataset has three key features. It represents a population of direct policy interest. It links beliefs about uncertain outcomes to their realized counterparts, allowing us to measure systematic biases in beliefs. It links our belief measures to future high-stake choices, allowing us to assess the predictive validity of our measures. Datasets that combine all three features are rare, as we show in Table 1.

[1] Inequality figures in Chile align with those from other industrialized countries. For example, children from families where at least one parent has attained higher education are 5.8 times more likely to have a higher education degree than children from families where neither parent has attained secondary education. This figure is 5.7 in Germany, 7.8 in the United States, and 8.1 in Italy (OECD.Stat, 2018).

[2] Percent plans are becoming a popular alternative to race-based affirmative action in college admissions. Texas, California and Florida have state-wide percent plans (see, e.g., Kapor (2020) and Black, Denning, and Rothstein (2020)).

Table 1: Overview of Selected Datasets on Subjective Beliefs in the Education Setting

Paper	Sample (Country)	Sample Size and Data Source: N Survey - N Admin - Are Survey and Admin data linked?	Are Believed and Actual Outcomes Linked?	Beliefs as Predictors of Later Choices or Outcomes?
This paper	Students in disadvantaged high schools eligible for preferential admission policy (Chile)	6,054 - 8,944 - Yes	Yes	Yes, for all respondents, administrative information 18 months later.
Arcidiacono, Hotz, and Kang (2012)	Male undergraduate students at Duke University (United States)	173 - 346 - No	No	No
Arcidiacono, Hotz, Maurel, and Romano (2020)	Male undergraduate students at Duke University (United States)	173 - 0 - No	Yes	Yes, for 156 respondents, self-reported or internet-searched information 6 years later.
Azmat, Bagues, Cabrales, and Iriberri (2019)	Undergraduate students at University Carlos III (Spain)	154 - 977 - Yes	Yes	No
Bobba and Frisancho (2019)	Middle-school students (Mexico)	2,825 - 2,825 - Yes	Yes	Yes, for all respondents, administrative information 3 years later.
Boneva and Rauh (2020)	High-school students (England)	2,540 - 0 - No	No	Yes, for 319 respondents, self-reported information 2 months later.
Delavande and Zafar (2019)	Male undergraduate students in three universities and four madrassas (Pakistan)	2,149 - 0 - No	No	No
Giustinelli (2016)	High-school students (Italy)	998 - 0 - No	No	No
Hastings, Neilson, Ramirez, and Zimmerman (2016)	College entrance exam takers and undergraduate students (Chile)	39,154 - 11,014 - Yes	Yes	Yes for 3,292 to 4,042 respondents, administrative information 1 year later.
Kapor, Neilson, and Zimmerman (2020)	Ninth grade applicants in New Haven, Connecticut (United States)	417 - 3,189 - Yes	Yes	No[a]
Stinebrickner and Stinebrickner (2012, 2014a,b)	Undergraduate students at Berea College (United States)	325 to 655 - 325 to 655 - Yes	Yes	Yes, for all respondents, administrative information 1 to 4 years later.
Wiswall and Zafar (2015)	Undergraduate students at New York University (United States)	488 - 0 - No	No[b]	No
Zafar (2011)	Undergraduate students at Northwestern University (United States)	161 - 0 - No	Yes	Yes, for 117 respondents, self-reported information 15 months later.

Note.– This table shows a list of datasets from studies that are closest to ours in terms of setting and type of belief data. The list is not exhaustive.
[a] But for 186 respondents, administrative information on outcomes collected before the survey is used to validate the belief data.
[b] But the students were asked about population-level earnings, so the authors could build a measure of belief errors by comparing their answers to true aggregate earnings.

We find that students are, on average, overconfident about their admission credentials through regular and preferential admission channels. They expect a college entrance exam score, used to determine regular admissions, that is on average 0.59 standard deviations above the score they obtain. They have accurate beliefs about their GPA, but underestimate the top 15% school cutoff, which results in over 40% of them expecting to graduate among the top 15%. Looking at belief heterogeneity, we find evidence of optimistic belief biases along the entire distribution of baseline test scores and within-school ranks.

We also find that our pre-college belief measures independently predict high-stake choices up to the second college year. While we do not interpret these as causal effects, this finding suggests that subjective beliefs are relevant in choice and that our survey recovers credible measures of these beliefs.

The second step of the analysis provides the first experimental evaluation of a preferential admission policy. To identify the impacts of admission policies, a large empirical literature has relied on quasi-experimental variation in these policies or on the structural modeling of student sorting and simulation of admission rule impacts. We relax the identifying assumptions those methods necessarily impose by exploiting the randomized expansion of the PACE percent plan. Researchers have designed field experiments that approximate admission policies (e.g. Cotton, Hickman, and Price (2020)). But Government-backed experiments that randomize admission policies are rare. Yet, they teach valuable lessons about the effects of these policies in the real world, where the choices students make affect high-stake outcomes with potentially long-term consequences, a difficult feature to recreate in a field or lab experiment.[3]

In 2016, the Chilean Government identified a set of high schools across the country that met the eligibility criteria for entering the PACE percent plan program, based on the students' low socioeconomic status, and randomly assigned a subgroup of them to be in the program.[4] The cohort entered in the experiment was about to start 11^{th} grade (see the timeline in Appendix Figure A3). In treated high schools, students who graduated with a GPA in the top 15% of their high school, two school years after the start of the experiment, were guaranteed college admission. This option was not available in the control schools.

[3]To approximate college admissions, Cotton, Hickman, and Price (2020) promised cash prizes to middle-school students according to their relative performance on a mathematics exam and tracked them over a few weeks. See also Calsamiglia, Franke, and Rey-Biel (2013). In contrast, we do not need to approximate the admission prize and we tracked the study subjects for several years.

[4]We collaborated with the Government. The Ministry of Education decided to randomize following consultations with its research group (*Centro de Estudios*), Orazio Attanasio (Yale, IFS), our project consultant Ranjita Rajan (Karta Initiative) and paper co-author Michela Tincani (UCL, IFS). The randomization code was written by the PNUD Chile (United Nations Development Program).

The experimental data analysis shows that the percent plan increased college admissions and enrollments by a third. However, relative to college entrants from the control group, those from the treatment group have a higher likelihood of dropping out. Regarding pre-college outcomes, the percent plan reduced pre-college study effort and achievement by approximately 10% of a standard deviation. These negative impacts are spread across the baseline within-school rank and test-score distributions. They are hard to rationalize with standard models of incentive response under rational expectations (e.g., Bodoh-Creed and Hickman (2019)). But they are compatible with students responding rationally to the policy, given the optimistic belief biases we measure.

We also find evidence suggesting the choices students made *before* college contributed to policy impacts on admissions. We cannot reject negative admission effects for students with baseline test scores in the top quintile of the sample. These are (ex-ante) impossible under rational expectations,[5] but are possible under biased beliefs: if students who would be admitted without the policy lower their effort in the incorrect anticipation of a guaranteed admission, they could miss out on an admission altogether. This raises the question of whether pre-college belief distortions, even if short-lived, can shape policy impacts on the allocation of college seats by affecting students' investments in acquiring admission credentials.

The third step of the analysis addresses this question. Answering it requires knowing what the policy impacts would have been had students' beliefs been correct. This is a counterfactual not seen in the data and must be simulated. To do so, we go beyond the experimental results and build a dynamic structural model of pre-college effort, entrance-exam-taking, admissions and enrollments that lets us characterize how biases in beliefs distort choices and outcomes. Moreover, the model makes it possible to perform counterfactuals to explore the impacts of combining preferential admissions with best-case informational interventions.[6]

The evidence guides the modeling. Given the clear evidence that beliefs about admissions are inaccurate, we relax the standard assumption of rational expectations and assume that subjective beliefs are one of the determinants of behavior.[7] But we do not assume that the predictive validity of our belief measures is causal; we allow

[5]Negative effects on effort are possible under rational expectations, but not to the extent of reducing a student's admission likelihood.

[6]This part of our study is related to a small but growing literature stream that shows how departing from rational expectations affects the predictions of dynamic choice models. In industrial organization, see the review in Aguirregabiria and Jeon (2020). In labor, see, for example, d'Haultfoeuille, Gaillac, and Maurel (2018).

[7]Structural models of preferential admissions typically assume rational expectations. See, for example, the seminal Arcidiacono (2005), and the models in Grau (2018) and Bodoh-Creed and Hickman (2019), where pre-college effort is endogenous, as in this paper.

for flexible correlation between beliefs and unobserved preferences and ability. This is in contrast to a common approach of assuming that elicited beliefs do not correlate with structural model shocks (e.g. Stinebrickner and Stinebrickner (2014b); Kapor, Neilson, and Zimmerman (2020)), which is often taken for tractability reasons.[8] We apply to our non-traditional data the finite mixture technique developed for estimating models with permanent unobserved heterogeneity from traditional data on choices and outcomes (Heckman and Singer (1984); Keane and Wolpin (1994, 1997)). This approach is of great practical value because elicited beliefs can correlate with unobserved determinants of behavior in many settings. Ignoring this correlation in estimation can lead to biased parameter estimates that mischaracterize the role of beliefs in choice (Wiswall and Zafar (2015)). To separately identify ability, beliefs and preferences, we leverage elicited belief data and experimental data variation. The estimation strategy builds on the identification argument using generalized indirect inference (Bruins, Duffy, Keane, and Smith Jr (2018)).

Using the model, we simulate each student's choices and outcomes under biased beliefs and a counterfactual of rational expectations. We define the wedge between them as distortions that occur as a consequence of belief biases.[9] We find that while there are distortions in the allocation of college seats even pre-intervention, preferential admissions exacerbate them. Regardless of the admission regime, pre-college belief biases lower the ability and increase the dropout rate of college entrants. This is because they induce 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. This generates under-admission and under-enrollment distortions among high-ability students and over-admission and over-enrollment distortions among low-ability students. Preferential admissions exacerbate these distortions because the relaxed admission requirements lead to more frequent over-enrollments of low-ability students who, despite the effort reduction, are still over-providing effort; and because they lead to more frequent under-enrollments of high-ability students who, because of the effort reduction, under-provide effort by more.

Our findings imply that belief biases lead overconfident but underprepared students to enter college in response to preferential admissions. Motivated by this, we examine whether policy can mitigate these effects. We simulate combining preferential

[8]Notable exceptions are Wiswall and Zafar (2015) and Arcidiacono, Hotz, Maurel, and Romano (2020), who combine multiple individual-level observations on beliefs and fixed-effect techniques to make their results robust to correlation between preferences and beliefs.

[9]To simulate rational expectations, we use the estimated preference and technology parameters and replace all subjective beliefs with their objective counterparts. We then simulate choices and outcomes. This requires solving for the Bayesian Nash equilibrium of the tournament game that takes place in each school under the preferential admission regime, where students' payoffs depend on rank and, therefore, on the effort of others.

admissions with belief correction. We find that the impacts of such interventions depend on what dimension of belief bias they correct. A policymaker wanting to correct all belief biases faces a trade-off: doing so when introducing preferential admissions can avoid adverse distortions in the academic preparedness of college entrants, but it can also induce a substantial reduction in the pre-college achievement of those who do not go to college.

This paper contributes to our understanding of inclusion policies. A large literature has shown that they affect many outcomes. However, how subjective beliefs shape their impacts has received less attention. In the context of preferential college admissions, some papers estimate policy impacts on pre-college outcomes, demonstrating that the response to the incentives introduced by these policies is an empirically relevant margin (e.g. Akhtari, Bau, and Laliberte (2019); Bodoh-Creed and Hickman (2019); Golightly (2019)). Others estimate policy impacts on admissions and enrollments (e.g. Howell (2010); Hinrichs (2012); Kapor (2020)) and earnings (Arcidiacono (2005); Arcidiacono, Lovenheim, and Zhu (2015)). In contrast, this paper shows that biases in pre-college beliefs can distort policy impacts on the allocation of college seats. This is important because it suggests that admission policies can be redesigned to draw closer to achieving their intended objective of allowing the most talented students from disadvantaged backgrounds access to college education.

This paper also contributes to the literature on information frictions in education. A growing body of literature has studied subjective beliefs as determinants of, for example, the choice of college (Delavande and Zafar (2019)), what to study in college (Stinebrickner and Stinebrickner (2014b); Wiswall and Zafar (2015)), what high school to apply to ((Bobba and Frisancho (2019); Kapor, Neilson, and Zimmerman (2020)) and what occupation to enter after college (Arcidiacono, Hotz, Maurel, and Romano (2020)). In contrast, we study how misinformation interacts with education policy. This is particularly important in the context of policies aimed at helping the disadvantaged, who face substantial information frictions (e.g. Stinebrickner and Stinebrickner (2014b); Hastings, Neilson, Ramirez, and Zimmerman (2016)). We provide evidence of a channel through which the disadvantaged may stay disadvantaged: they can remain mismatched to college opportunities, despite programs aimed at helping them.[10]

The remainder of this paper is organized as follows. Section 2 describes the study context and the randomized experiment. Section 3 describes the data construction and introduces the descriptive analysis, including the predictive validity of our belief

[10] We complement Hoxby and Avery (2013) and Hoxby and Turner (2013), who explore how misinformation about college-going opportunities, for example, due to lack of counseling, can prevent talented, disadvantaged students from applying to selective colleges.

measures. Section 4 presents the experimental results, robustness analyses, a discussion of mechanisms and a summary of the model-free evidence. Sections 5 and 6 introduce the structural model and the estimation and identification strategies. The model results are presented in Section 7, and Section 8 concludes.

2 Context, Policy and Randomization

This section describes the Chilean college admission system, the PACE policy, the randomized controlled trial, the sampling design, sample characteristics and balancing tests.

2.1 College Admissions and Enrollments in Chile

Selective colleges in Chile participate in a centralized admission system (*Sistema Único de Admisión*).[11] Students wishing to go to 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.

2.2 The PACE Preferential Admission Policy

In line with global statistics (Appendix Figure A1), 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 (Supplementary Figure G1). The PACE policy (*Programa de Acompañamiento y Acceso Efectivo a La Educación Superior*) was introduced to increase college admissions among disadvantaged students. The Government selected the schools to be targeted by PACE using the *Indice de Vulnerabilidad Escolar* school-level vulnerability index, based on students' socioeconomic characteristics. Students in targeted schools are underprivileged: for example, 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 (Appendix Table A1).

Admission rules under PACE. Students in high schools participating in PACE can apply to college through the regular channel described in Section 2.1. Moreover,

[11] 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 typically provide vocational and shorter degrees. There is no enrollment gradient by socioeconomic status for these institutions (see Supplementary Figure G1). In the remainder of this paper, the term college refers to a selective college.

they receive a guaranteed college admission if they satisfy the following three conditions. First, the student's average grades in years 9 to 12 must be in the top 15% of her high-school cohort.[12] Second, the student must take the PSU entrance exam, even though the score obtained does not affect the likelihood of obtaining a PACE admission.[13] Third, the student must attend the PACE high school continuously for the last two high-school years (11^{th} and 12^{th} grades).

Other policy features. Optional tutoring sessions in college are available to those who enroll via PACE, and light-touch orientation classes (two hours per month on average) are offered to all students in PACE high schools. These classes cover the college application process and study techniques and often replace orientation classes (MinEduc (2018)).[14]

PACE college seats are supernumerary: they do not replace regular seats but are offered in addition to them.[15] Therefore, the introduction of the PACE policy 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 PSU entrance exam score of regular entrants into each college-major pair (Appendix Figure A2). A student can obtain both a PACE and a regular admission. If a student does not accept a PACE admission, that PACE seat remains vacant.

2.3 Policy Randomization and Balancing Tests

2.3.1 Timing

The Government introduced the PACE program in 69 disadvantaged high schools in 2014 and later expanded it to more schools. This study uses data from the randomized expansion of the policy. In 2015, the Government 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. The Government 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 11^{th} graders is entered into the program. The randomized expansion concerned the cohort who started 11^{th} grade

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

[13] The Texas Top 10 and the Californian Eligibility in Local Context percent plans share this feature (Horn and Flores (2003)).

[14] The Texas Top Ten percent plan shares this feature.

[15] Colleges did not reach capacity constraints because PACE remains relatively small: fewer than 1% of college students are currently enrolled through PACE.

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 in high schools (Section 4.3). The control schools were not entered into the PACE program; they were not promised participation. Figure A3 of the Appendix 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. However, students who wished to affect their four-year GPA average had two school years to do so.

2.3.2 Sample and Balancing Tests

Data were collected from the experimental cohort. We sampled all 64 schools the Government randomly allocated to treatment. For budget reasons, we randomly selected 64 of 157 eligible schools the Government randomly allocated to control. Table 2 presents the balancing tests for the 128 sampled schools using background information collected when the cohort under study was in the 10^{th} 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).

Table 2: 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)		
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. Low-SES student indicates a student that the Government classified as very socioeconomically vulnerable (*Prioritario*). SIMCE is a standardized achievement test taken in 10^{th} grade.

3 Data and Descriptive Analysis

This section describes the construction of the linked administrative-survey data, including information on the data collection; presents descriptive statistics on choices, outcomes and subjective beliefs; and demonstrates the predictive validity of the belief data.

3.1 Construction of Linked Administrative and Primary Survey Data

Table 3 lists the administrative and primary data sources employed. We linked all data sources through unique student, classroom and school identifiers and built a longitudinal dataset that follows approximately $9,000$ students for six years, from 9^{th} grade to two years after leaving high school (see Figure A3 for the timeline).

Table 3: Overview of Data

Dataset	Variables	Time Collected	Source
1. *Sistema Nacional de Evaluación de Resultados de Aprendizaje*	Achievement test scores, background characteristics	Grade 10	Administrative
2. *Subvención Escolar Preferencial*	Low-SES classification (*Prioritario* student)	Grade 10	Administrative
3. School records 1	High-school enrollment	Grades 9-12	Administrative
4. Student survey	Study effort, beliefs	Grade 12	Primary
5. Teacher survey	Effort, focus of instruction, characteristics	Grade 12	Primary
6. School-principal survey	Support classes, assessment methods	Grade 12	Primary
7. Achievement	Achievement test scores	Grade 12	Primary
8. School records 2	GPA (overall and by subject), grade progression	Grades 9-12	Administrative
9. Higher education records	Entrance exam (PSU) scores, applications, admissions, enrollments and retentions via regular channel	After grade 12	Administrative
10. PACE program records	Allocation of PACE seats, applications, admissions, enrollments and retention via PACE channel	After grade 12	Administrative

For all $9,006$ students enrolled in the 128 sample schools, we obtained rich 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 applications, and admissions and enrollment up until the second year in college.

Further, to complement the administrative data, we collected primary data from all 128 sample schools between September and November 2017, when students were completing 12^{th} grade (Appendix F describes our fieldwork). Our primary data contain four key pieces of information. First, we measured pre-college achievement. In Chile, as in other countries (e.g. the U.S.), standardized achievement tests are not administered universally at the end of high school. Administrative information is limited to the GPA, available for all students but not comparable across schools, and entrance exam scores, standardized nationally but available only for a selected sample (i.e. those who take the entrance exam, a decision that can be affected by a preferential admission policy). To overcome this data limitation, we administered a 20-minute mathematics achievement test to all students (see Behrman, Parker, Todd, and Wolpin (2015) for a similar approach).[16] Second, we elicited study effort through the survey instruments used in Mexican high schools by Behrman, Parker, Todd, and Wolpin (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. Fourth, we surveyed mathematics, Spanish teachers and school principals (Supplementary Appendix G3 describes the information we collected from them).

We surveyed 6,094 students, approximately 70% of those enrolled in the 128 sample schools. Our response rate compares favorably with ministerial surveys (MinEduc (2015, 2017)), and it reflects a natural dropout that occurs in the last weeks of the last high-school year (schooling is compulsory until then).[17] We account for survey attrition in two ways. For the regression analysis, 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. Importantly, attrition was not selective across the treatment and control groups (Section 4.3).

3.2 Descriptive Analysis

As noted, students in the sample are underprivileged compared to country-level averages (Table A1). This section describes their path to college and beliefs. We focus on students in control schools to shed light on college participation in the absence of a preferential admission policy.

[16] The professional testing agencies, Aptus Chile and Puntaje Nacional, developed the test and we extensively piloted it. Appendix F details how we implemented the test during the fieldwork.

[17] The survey responses from one school were lost in the mail. For this school, the survey data are limited to school principals and teachers, whose responses were collected via tablets.

Table 4: Description of Outcomes and Subjective Beliefs

	Mean (1)	Standard Deviation (2)	N (3)
		A. Outcomes	
Sat PSU entrance exam	0.655	0.475	4,231
Reports having prepared for PSU entrance exam	0.626	0.484	2,936
PSU score | sat PSU (σ)	-0.602	0.611	2,773
Applied	0.210	0.407	4,231
PSU score | applied (σ)	-0.171	0.595	887
Admitted	0.112	0.315	4,231
Graduated in bottom 85% of school | admitted	0.522	0.500	471
Not admitted | graduated in top 15% of school	0.641	0.480	627
Enrolled in 1^{st} year	0.081	0.273	4,231
Enrolled in 2^{nd} year	0.065	0.246	4,231
		B. Subjective Beliefs	
Believed PSU score (σ)	-0.033	0.920	2,413
Believed minus actual PSU score | sat PSU (σ)	0.591	0.916	1,853
Believes admission probability ≥ 0.50	0.840	0.367	2,798
Believed minus actual 12^{th} grade GPA (GPA points)	-0.075	0.552	2,558
Believed minus actual 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. – Survey and administrative data from the sample of students enrolled in the 64 control schools. σ is the standard deviation of PSU 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.

3.2.1 Path to College

Around two-thirds of students take the college entrance exam per administrative data, which aligns nicely with our survey data, where a similar fraction report having prepared for the entrance exam (first two rows of Table 4). Even students with very low admission likelihoods prepared for and took the entrance exam (Figure A5 of the Appendix). But, as the third row of the table shows, exam scores are well below the national average (-0.6 standard deviations). Therefore, it is not surprising that upon observing their exam scores, less than a third of those who took the entrance exam apply, and they are those with the higher scores (fourth and fifth rows). 11.2% of students are admitted, 8.1% enroll and 6.5% remain enrolled in the second year.

While on average fewer than 15% of students are admitted to college, in some relatively high-performing schools more than 15% are. As a result, over half of those admitted to college graduated in the bottom 85% of their school (seventh row in Table 4). For these students, a decrease in entrance exam preparation in response to the policy could harm admission outcomes. Conversely, among all those who graduated in the top 15% of their school, 64% were not admitted to college (eighth row of Table 4). If all these students were admitted to college under the percent plan and nobody experienced negative admission effects when the percent plan was introduced, we would expect the policy effect on admissions to be approximately 10 percentage points (p.p.) $(0.15 * 0.641 = 0.096)$. We refer to this as the mechanical admission effect. We return to it later.

3.2.2 Subjective Beliefs and Belief Biases

We collected two sets of pre-college beliefs: about future outcomes and about returns to effort.[18] This section summarizes beliefs about outcomes. Figure A4 of the Appendix shows an English translation of the survey questions. We elicited beliefs about two types of outcomes: own and of other students in the school. We interpret the answers as believed outcomes at the level of study effort that the respondent or her school peers *actually exerted*. We measure systematic biases in beliefs by comparing believed outcomes to the actual ones from the linked administrative data.

Overoptimism around regular admissions. Panel B of Table 4 shows that before taking the exam, students display large overconfidence over their PSU entrance exam score. On average, they expect a PSU score of 0.59 standard deviations above their score (second row). They report high subjective probabilities of a regular admission: while only 11.2% of students in control schools are admitted through the regular channel, over 80% believe they are at least 50% likely to be admitted (third row).[19] These subjective beliefs fit well with the results from the administrative data described in Section 3.2.1, which are consistent with overoptimism.

These misperceptions may arise from limited knowledge of the distinction between a selective college and a vocational higher-education institution, as documented by Hoxby and Turner (2015) among disadvantaged students in the U.S.. Moreover, they may reflect overconfidence about ability, which previous research has shown to be pronounced among disadvantaged students (Stinebrickner and Stinebrickner (2012); Falk, Kosse, Schildberg-Hörisch, and Zimmermann (2020)).[20]

[18] Additionally, we collected beliefs about pecuniary returns to college, which we use to discuss mechanisms behind our findings in Section 4.4.

[19] We elicit this subjective probability via a Likert scale. The question is as follows: "*How certain are you that if you take the PSU entrance exam this year, your score will be sufficiently high to be admitted to a selective university (450 or more)?*". The possible answers are as follows: "Totally certain that it will not," "It is more likely that it will not," "It is equally likely that it will or will not," "It is more likely that it will" and "Totally certain that it will." Conditional on sitting the entrance exam, the objective admission likelihood is 17.6%.

[20] Biased self-assessment in the form of overconfidence is a widespread phenomenon documented in many contexts (see DellaVigna (2009) and Burks, Carpenter, Goette, and Rustichini (2013) for overviews and Stinebrickner and Stinebrickner (2014b) and Bobba and Frisancho (2019) for other examples in the education setting). Several studies find that especially low-skilled individuals are overconfident, as described by the unskilled and unaware hypothesis (for example, Park and Santos-Pinto (2010)). Zimmermann (2020) shows that asymmetric processing of positive and negative feedback sustains optimistic beliefs: the impact of negative feedback on beliefs drastically diminishes over time, but this is not the case for positive feedback.

Overoptimism around school rank. Students hold accurate beliefs about their GPA, which is measured on a scale from 1.0 to 7.0: the average belief bias is below a tenth of a GPA point (fourth row in panel B). But they have a small belief bias about the top 15% school cutoff (−0.40 GPA points, second-last row in Table 4). These findings are not surprising: students are informed of their own grades, but they are never given feedback on the GPA distribution in their school. Given the high degree of grade compression (Supplementary Figures G2 and G3), the small absolute belief bias over the cutoff translates into a large relative belief bias: over 40% of students believe that their GPA is in the top 15% of their school (last row in Table 4).

Belief heterogeneity. The overoptimistic beliefs we described are widespread: they are found across the entire distribution of baseline test scores and within-school ranks (Figure A6 of the Appendix).

3.3 Predictive Validity of Belief Data

This section shows that pre-college beliefs predict outcomes and high-stake choices taken up to 18 months *after* belief elicitation.

Believed PSU entrance exam score. We elicited a student's believed PSU score before the decisions to take the PSU entrance exam (which occurred up to two months after the date of our data collection), apply to college (one to three months after), enroll in college (four to six months after) and remain enrolled in college in the second year (16 to 18 months after).

Panel A in Table A3 of the Appendix shows a sizable and statistically significant relationship between a student's believed PSU score and later choices. Controlling for baseline characteristics and test scores, an increase in the believed PSU score of one standard deviation of the PSU distribution increases the probability that a student takes the entrance exam by 4.8 p.p. or 6% of the sample mean, applies to college by 8.6 p.p. or 32% of the sample mean, enrolls by 6.0 p.p. or 56% of the sample mean, and remains enrolled in the second year by 4.8 p.p. or 53% of the sample mean. In Panel B, we control for the actual PSU score, such that variation in the believed PSU score reflects variation in overoptimism among those who sat the exam. The believed PSU score remains a significant independent predictor of applications and enrollment.

That believed PSU predicts a student's decision to take the exam suggests that the perceived likelihood of college admission is relevant for the decision to take the exam. But since students observe their PSU score before they apply, the fact that overoptimism about the PSU score predicts application and later choices among those who sat the exam suggests that this pre-college belief correlates with unobservable determinants of application, enrollment and persistence in college, such as the preference for college and unobserved ability. For example, individuals more overoptimistic about their entrance exam scores may have a stronger college preference.

Believed school rank. Whether a student graduates in the top 15% of her school does not affect admission chances in the policy's absence. Therefore, we do not expect that the belief about school rank independently predicts whether students in control schools take the entrance exam, apply, enroll and stay in college. In fact, in the control group, the belief of graduating in the top 15% of the school significantly predicts only a subset of later choices but only when we do not control for the believed PSU score (Panel C of Table A3). When we do, it does not predict any choices (Panel D). Therefore, the school rank belief correlates positively with the belief about the PSU entrance exam score, suggesting that relative and absolute performance beliefs correlate. But, as expected, the school rank belief does not independently predict entrance-exam-taking and college-going in the control group.

But for students targeted by the policy, whether their GPA is above the school cutoff determines whether they are awarded a preferential admission. Since sitting the PSU entrance exam is a requirement to obtain the preferential seat (Section 2.2) and students must take the decision to sit it before they are informed of whether they graduate in the top 15%, we expect that, if our survey recovered credible measures of believed GPA and school cutoff, the difference between them should predict exam-taking in treated schools but not in control schools, which proved to be the reality (see columns (1) and (2) in Table A4 of the Appendix).[21]

We interpret the findings in this section as follows. First, subjective beliefs are important in choice. Second, our survey recovers credible measures of these beliefs. Third, subjective beliefs likely correlate with the preference for college and unobserved ability.

4 Experimental Evidence on Policy Impacts

This section presents the experimental evaluation of the PACE policy. It shows impacts on admissions, enrollments, pre-college achievement and pre-college effort. It presents robustness analyses and discusses the likely mechanisms. It concludes with a summary and discussion of the model-free evidence.

[21] Columns 3 to 5 in Table A4 of the Appendix additionally show that the believed school cutoff correlates significantly with the actual school cutoff, even after controlling for students' characteristics: a GPA point increase in the actual cutoff is associated with an increase of approximately 0.4 GPA points in the believed cutoff. Therefore, elicited beliefs about the school cutoff are meaningful guesses that trace the outcome they represent.

4.1 Treatment Effects on College Admissions and Enrollments

Students in schools randomly assigned to the treatment are 3.8 p.p. more likely to be admitted to college, 2.9 p.p more likely to enroll and 2.1 p.p. more likely to still be enrolled in the second year than students in control schools (Table 5). These effects correspond to a 34%, 36% and 32% increase relative to the control group, respectively. Relative to college entrants from the control group, those from the treatment group have a higher likelihood of dropping out.

Table 5: Impact of Percent Plan on Admissions and Enrollments

	Admissions	Enrollments 1^{st} year	Enrollments 2^{nd} year
	(1)	(2)	(3)
Treatment	0.038***	0.029**	0.021**
	(0.012)	(0.012)	(0.010)
Observations	8,944	8,944	8.944
Pseudo-R^2	0.245	0.237	0.217

Note.– Average marginal effects from probit models. The delta-method standard errors are clustered at the school level. Controls: gender, age, indicator for low-SES student, baseline SIMCE test score, never failed a grade and 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 and enrollment gains are concentrated among students with baseline school grades at the top of their school. However, they are spread between the 20^{th} and 80^{th} percentiles of the baseline test score distribution in the whole sample (Appendix Figure A7). This reflects the fact that top-performing students from different schools have different absolute ability, because student ability varies across schools. Students with baseline test scores in the top quintile of the sample experience no admission or enrollment gains at the point estimate.

The treatment effect on admissions is 60% lower than the mechanical admission effect of 9.6 p.p. (Section 3.2.1) for two reasons. First, for students with baseline test scores in the top quintile of the sample, we cannot reject negative admission effects of up to 6 p.p. or 16% of the control mean.[22] Second, 12% of students who graduated in the top 15% of a treated school renounced a preferential admission by not taking the entrance exam.[23]

[22] The 95% confidence interval for the treatment effect on admissions for this group is $[-0.061, 0.893]$ (see Figure A7 of the Appendix).

[23] Had all of these students taken the exam, the treatment effect on admissions would have been 8.3 p.p., substantially closer to the mechanical admission effect, but still below it, suggesting that this channel alone does not explain the discrepancy between realized and mechanical admission effects. This result is obtained from a probit regression where a student who did not take the entrance exam and graduated in the top 15% is categorized as admitted.

4.2 Treatment Effects on Pre-College Achievement and Study Effort

Table 6 shows that students in treated schools perform 10% of a standard deviation worse than students in control schools in the standardized achievement test we administered. This result is robust to using item response theory to calculate the achievement score (Supplementary Table G2). Columns (3) and (4) show that the treatment had a negative average effect on study effort of 9% of a standard deviation. This effect is driven by a reduction in study effort inside and outside the classroom and in PSU entrance exam preparation (Table A5 of the Appendix).

Table 6: Impact of Percent Plan on Pre-College Achievement and Effort Scores

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

Note.– The coefficients are OLS estimates. Standard errors were clustered at the school level. The standard set of controls (see notes in Table 5) and field-worker fixed effects were used. *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 A5 of the Appendix. *p < 0.10; **p < 0.05; ***p < 0.01.

To complement this analysis, we obtained administrative grade data and found that students in treated schools obtain lower (equal) grades on the subjects tested (not tested) on the PSU (Table A6 of the Appendix). Therefore, in response to the treatment, students reduced their study effort on PSU exam preparation and PSU exam subjects. But they did not reallocate effort toward other subjects.[24]

Figure A7 of the Appendix shows that the negative impacts are spread across the baseline within-school rank and baseline test score distributions. In particular, students with baseline test scores in the top quintile of the sample experience large and statistically significant negative impacts on achievement and effort. We find no evidence of an encouragement effect for students around the top 15% cutoff.

4.3 Robustness

Attrition. The response rate in our primary data is equal to 69.4% percent in the control group, and it is not statistically significantly different in the treatment

[24] Course selection is not a possible margin of policy response because students cannot select courses. They can choose a high-school track before starting 9^{th} grade. In the experimental cohort, students chose the track two years before the start of the experiment.

group, suggesting the absence of selective attrition. Nonetheless, in Table A7 of the Appendix, we present Lee (2009) bounds for the treatment effects, confirming that the estimated treatment effects are not due to selective attrition.

Validating achievement and effort data. Appendix and Supplementary Tables A2 and G1 validate the achievement and study effort measures. Their relationships with high-stake outcomes are sizable, significant and persist for up to 18 months after the data collection. For example, Table A2 shows that, controlling for student characteristics and lagged test scores, a standard deviation increase in the achievement test score is associated with an increase in the probability that a student is enrolled in the second year in college of 3.8 p.p. (p=0.000) or 49% of the sample mean. The relationship remains significant even after controlling for the PSU entrance exam score (1.3 p.p. or 12% of the sample mean, p=0.024). The study effort measure displays similar relationships with the outcomes. Finally, the fact that our findings on pre-college achievement are replicated using administrative grade data generates additional confidence in the validity of our results.

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 2.2).[25] Nonetheless, we present three sets of evidence, each of which points 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 Supplementary Table G3: 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 school composition changes.

4.4 Discussion of Mechanisms

The widespread negative treatment effects on pre-college effort and achievement are hard to rationalize with an incentive response under rational expectations. As shown

[25] 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.

in Bodoh-Creed and Hickman (2019), we would expect negative impacts to be concentrated among students near regular admission cutoffs (high ability) and well above the preferential admission cutoff (high within-school rank). For them, regular admission is within reach if they exert effort, but a preferential one is guaranteed even at lower effort. Conversely, we would expect positive impacts among students far from the regular admission cutoff (low ability) and near the top 15% school cutoff because, for them, the percent plan put a previously unattainable admission within reach, thereby increasing the returns to effort. But this is not what we found. This section discusses possible mechanisms underlying our findings.

Incentive response under biased beliefs. The data evidence is compatible with students responding rationally to the policy, given their biased beliefs. The widespread overoptimism about a regular admission (Section 3.2.2) suggests that many perceive a regular admission as within reach, leading them to exert effort, and in particular effort toward the entrance exam, when not treated. The widespread overoptimism around school rank (Section 3.2.2) suggests that many perceive a preferential admission as guaranteed, leading them to lower their entrance exam preparation when treated. Additional evidence points to this channel: the treatment effects by *perceived* rank are compatible with a response to incentives. The treatment effect on pre-college achievement is positive (large and negative) for students whose baseline GPA is around (well above) the perceived cutoff (Figure A8 of the Appendix). Therefore, the negative impacts are driven by those who perceive a preferential admission is guaranteed.[26]

This is not direct evidence that students responded to perceived incentives; but it is consistent with that notion. Direct evidence requires knowing what students believe the returns to effort to be in securing a college admission and how the policy affected these perceived returns. We return to this point in Sections 6.2 and 7.1.2, where we show how, by combining the model structure with additional measures of beliefs about the productivity of effort in achievement, we can estimate these perceived returns to effort.

Teachers and school principals. Teachers could respond to a percent plan by changing their effort, focusing their instruction on a subset of students, or changing their grading. Principals may reallocate teachers across classrooms or change other

[26]While the large negative effects among those who believe they are top-performing in their school are statistically significant, the positive ones among those who believe they are around the top 15% school cutoff are not. This is due to power issues: only a few students satisfy the conditions for positive impacts; that is, they believe they are near the 15% school cutoff *and* far from the regular admission cutoff. First, the widespread overconfidence in school rank suggests that few believe they are near the top 15 percent cutoff. Second, the widespread optimism about regular admission chances and the fact that most students in the control group already prepare for and take the entrance exam (Sections 3.2.1 and 3.2.2) suggest that few believe they are far from the regular admission cutoff. Therefore, even fewer students simultaneously satisfy both conditions.

school-level resources. In turn, such changes may directly affect student achievement or induce a response in students' effort. This section analyzes data from our teacher and principal surveys and matched grade-test score data.

First, we collected rich measures of teacher characteristics, their focus of instruction and effort (Supplementary Appendix G3 describes our survey instruments and variable construction). Adding these variables as controls to the regression models for pre-college effort and achievement does not bring the coefficient on the treatment dummy closer to zero (Supplementary Table G4).[27] While we do not interpret this as definitive evidence that these teacher factors do not contribute to treatment effects, it suggests that they are unlikely to be the main drivers of the negative impacts on pre-college effort and achievement. Second, we consider changes in grading behavior. If grade manipulation weakens the relationship between achievement and grades in treated schools, students in those schools have a lower incentive to study to improve their grades. But the experimental evidence indicates that pre-college effort reductions resulted in grade reductions (Section 4.2). Accordingly, the estimated mapping between standardized achievement and grades does not differ between treated and control schools (Supplementary Table G6), suggesting that grading did not respond to the treatment.[28] Finally, using our survey of school principals, we find that treated schools do not differ from control schools regarding the support offered to students (PSU entrance exam preparation support or remedial classes), as shown in Supplementary Table G7. The curriculum is not a possible margin of policy response because the Ministry of Education sets it.

Expected returns to college. If the light-touch orientation classes negatively affected students' beliefs about the net returns to college, they could have generated the negative response of pre-college study effort. The data suggest that this did not occur. The policy had no impact on students' beliefs about the monetary returns to a college degree (Supplementary Appendix G4) or their awareness of financial aid (83.6% of surveyed students are aware they are eligible for a tuition fee waiver, and there is no statistically significant difference between the treatment and control groups (p=0.618)). Therefore, the treatment did not affect students' perceived costs of and returns to college education, which are large (200% of age 30 earnings).[29]

[27]Depending on the outcome, adding these controls either leaves the coefficient on treatment practically unchanged, or makes it slightly more negative, which suggests that teacher factors, if anything, play a remedial role. We reach the same conclusion if we add controls for students' perceptions of teachers' behaviors (see Supplementary Table G5).

[28]Consistent with this result, school principals report similar grading practices across treatment groups (Supplementary Table G7).

[29]This figure is consistent with survey data collected among other samples of Chilean students of the same age (Hastings, Neilson, Ramirez, and Zimmerman (2016)).

4.5 Summary of Model-Free Evidence and Conclusions

First, we study the pre-college beliefs of disadvantaged students targeted by a preferential admission policy in Chile. A unique feature of our dataset is that it links beliefs to true outcomes for a large sample of students, representative of a population of direct policy interest. We document that pre-college beliefs about admission credentials independently predict choices up to 18 months after our data collection (Section 3.3) and belief biases are widespread (Section 3.2.2).

Second, exploiting the randomized expansion of the PACE percent plan to identify causal effects, we show that it increased by a third college admissions and enrollments of disadvantaged students up to the second college year and it increased dropout among college entrants (Section 4.1). But admissions increased 60% less than the mechanical effect: we cannot rule out that negative admission effects occurred for a subgroup of students and that positive admission effects failed to materialize among students who chose not to sit the entrance exam. This indicates that choices students made *before* the admission stage contributed to policy impacts.

Looking at pre-college choices and outcomes, we found substantial and widespread reductions in pre-college effort and achievement (Section 4.2). While these effects are hard to rationalize with a response to incentives under rational expectations, they are consistent with a response to incentives under the belief biases we measured (Section 4.4). This finding can help rationalize why we cannot rule out negative admission effects for some students: if students who would be admitted without the policy lower their effort in the incorrect anticipation of a guaranteed admission, they could miss out on an admission altogether. This would be (ex-ante) impossible under rational expectations, where students would not reduce effort to the extent of reducing their admission likelihood, and suggests that biases in pre-college beliefs, even if short-lived, could contribute to policy impacts on admissions, enrollments and persistence in college.

From our model-free evidence, we conclude that preferential admissions can improve diversity in college by increasing participation from disadvantaged groups. But in the presence of belief distortions, they can have unintended consequences on admission credentials which may distort the allocation of college seats. This is a novel contribution, resting on a combination of new linked survey-administrative data and a real-world randomized policy experiment on preferential admissions.

5 Dynamic Model with Subjective Beliefs

This section introduces the structural model and discusses its features.

5.1 From Experimental Evidence to a Model

We develop a dynamic structural model with subjective beliefs that serves several purposes. First, it offers an interpretation of the estimated treatment effects.[30] Without a model, it would be difficult to understand how the response of pre-college effort contributes to policy impacts on the allocation of college seats. Second, the model makes explicit the assumptions needed to recover the parameters that govern the choices of students targeted by preferential admissions. Third, it allows us to quantify how distortions in student choices due to belief biases shape policy impacts on college seat allocation. Fourth, the model makes it possible to perform counterfactuals to explore the impacts of combining preferential admissions with best-case informational interventions.[31]

Our key model assumption is that subjective beliefs are determinants of behavior, which is weaker than it may appear. First, the evidence clearly indicates that beliefs about the admission process are inaccurate, suggesting that an appropriate model of student behavior should relax the standard assumption of rational expectations. Second, we do not assume that the predictive power of our pre-college belief measures for later outcomes (Section 3.3) is causal. This contrasts a common approach of assuming that elicited beliefs do not correlate with structural model shocks. Instead, we allow for flexible correlation between subjective beliefs, unobserved preferences and ability. To do so, we apply to our non-traditional data, which include elicited beliefs, the finite mixture technique developed for estimating, from traditional data on choices and outcomes, duration and dynamic programming models with permanent unobserved heterogeneity (Heckman and Singer (1984); Keane and Wolpin (1994, 1997)). This approach is of great practical value, because previous studies have shown that elicited beliefs can correlate with unobserved behavior determinants. Ignoring this correlation can lead to biased model parameters that mischaracterize the role of beliefs in choice (Wiswall and Zafar (2015)). Third, we do not assume that pre-college beliefs about admission credentials are the only channel of policy impacts.[32] Finally, we validate key model restrictions by matching some of our belief data by design and leaving some out of the estimation sample for validation purposes.

[30]Several recent studies interpret experiments through structural models (e.g. Todd and Wolpin (2006); Attanasio, Meghir, and Santiago (2011); Kaboski and Townsend (2011); Allende, Gallego, and Neilson (2019)).

[31]It is common for studies that incorporate incorrect or incomplete information into structural models to simulate counterfactuals where information is correct or complete to evaluate whether informational interventions can be *promising*, acknowledging that interventions in practice may not always reach this benchmark (Arcidiacono, Aucejo, Maurel, and Ransom (2016); Kapor, Neilson, and Zimmerman (2020); Kapor (2020)).

[32]We can identify other channels from the set of students who at baseline report no intention to pursue college education: impacts on these students cannot be due to a response to incentives related to attending college.

A key feature of the model is that it is dynamic. This lets us quantify the impacts of pre-college beliefs on later outcomes. The relevant pre-college choices we model are effort and the decision to take the entrance exam, since these choices are made before school rank information and entrance exam scores are revealed and must be based on beliefs.

5.2 Primitives

5.2.1 Students and Schools

Subscript i indicates a student, j indicates a school and $j(i)$ the high school in which student i is enrolled. High schools are either treated ($T_{j(i)} = 1$) or control ($T_{j(i)} = 0$). Each student 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)).[33] The number of types, K, is known to the econometrician. We use types to capture permanent unobserved heterogeneity in ability, beliefs and preferences.

5.2.2 Timeline

Figure 1 sketches the timeline. Before the model starts, 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 beliefs are instrumental in forming *subjective* probabilities of a regular and preferential admission as a function of pre-college effort (represented in Figure 1 as $PrR(e)$ and $Pr15(e)$, respectively). In model period 1, students choose study effort. Effort affects GPA and the score on the PSU entrance exam (if they take it) according to objective production functions, which in turn affect their *objective* admission probabilities. In model period 2, students decide whether to sit the PSU entrance exam. As in the real world, students do not yet know their entrance exam score and whether they are in the top 15% of their school, and must form beliefs about these outcomes. In model period 3, admissions are realized according to objective admission chances. They depend on the choices students made in periods 1 and 2. Finally, in model period 4, students make enrollment decisions given their admissions. Even though all uncertainty is resolved at this stage, the

[33] 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.

choices made in periods 1 and 2, based on pre-college beliefs, affect the choice set in period 4.

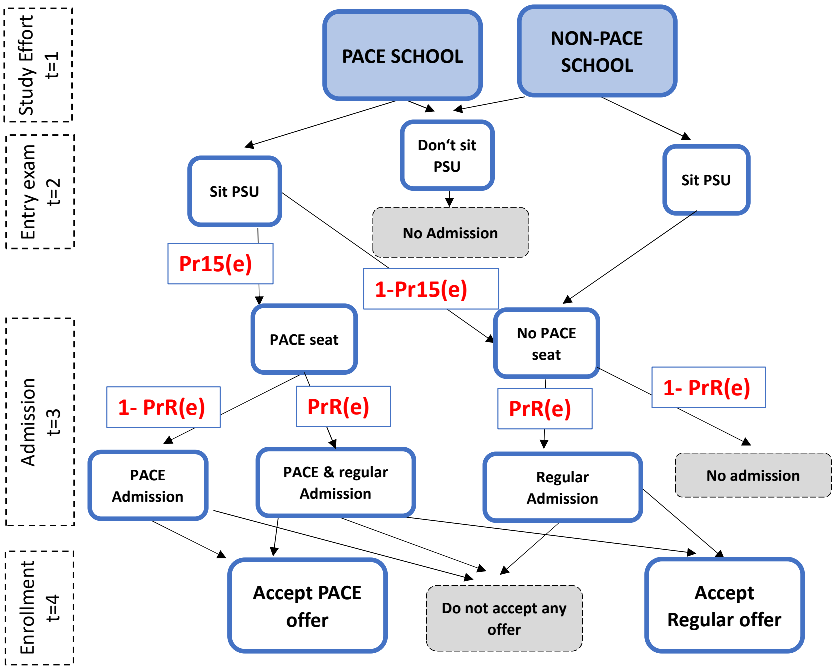

Figure 1: Model timeline.

5.2.3 Objective Achievement Production Functions

The achievement of student i at the end of high school depends on study effort during high school, e_{it}, on characteristics observed and unobserved by the researcher, (x_i, k_i), and on the baseline standardized test score, $y^{(1)}_{i,t-1}$. The score y_{it} on the test that we administered measures achievement with noise $\epsilon^{m.e.y.} \sim N(0, \sigma^2_{m.e.y.})$:

$$\begin{aligned} y_{it} &= \alpha_{0k_i} + \alpha_1 x_i + \alpha_2 e_{it} + \alpha_3 y^{(1)}_{i,t-1} + \epsilon^{m.e.y.}_{it} \\ &= y(e_{it}, x_i, y^{(1)}_{i,t-1}, k_i; \alpha) + \epsilon^{m.e.y.}_{it}. \end{aligned} \quad (1)$$

The achievement function in equation (1) is a variant of the commonly used value-added specification, which includes lagged test score as a determinant of the current test score (Hanushek (1996, 2003); Krueger (2000)). To introduce permanent unobserved ability into the model, we let the constant depend on unobserved type k_i.

We do not model separate effort decisions for producing PSU and GPA. Instead, we assume the same effort that affects the achievement test score enters as an input in the PSU and GPA production. This modeling choice is motivated by the evidence that students do not substitute strategically between studying toward the entrance exam and toward GPA (Section 4.2). The production functions are:

$$PSU_{it} = \beta_0^P + \beta_1^P e_{it} + \beta_2^P y_{it-1}^{(1)} + \epsilon_{it}^P, \tag{2}$$

$$GPA_{it} = \beta_0^G + \beta_1^G e_{it} + \beta_2^G y_{i,t-1}^{(2)} + \epsilon_{it}^G, \tag{3}$$

where we include the baseline standardized score ($y_{it-1}^{(1)}$) in equation (2) and the baseline 10^{th} grade GPA ($y_{i,t-1}^{(2)}$) in equation (3) as lagged achievement measures.[34] We assume that the technological shocks $\epsilon_{it} = [\epsilon_{it}^P, \epsilon_{it}^G]$ are distributed as bivariate normal: $\epsilon_{it} \sim N(0, \Sigma)$, with $\Sigma = \begin{bmatrix} \sigma_P^2 & \rho\sigma_P\sigma_G \\ \rho\sigma_P\sigma_G & \sigma_G^2 \end{bmatrix}$, where we allow for correlation among these achievement measures not explained by measured baseline ability and effort.

We assume that our survey measures study effort with additive noise: $e_i^o = e_i + \epsilon_i^{m.e.e.}$, where $\epsilon^{m.e.e.} \sim N(0, \sigma_{m.e.e.}^2)$ is a classical measurement error. Therefore, OLS estimation of equations (1), (2) and (3) using the observed effort as an explanatory variable would suffer from attenuation bias. Instead, we jointly estimate the production function parameters and measurement error variance within the model.

5.2.4 Preferences

All students derive utility from achievement and face a cost of exerting study effort. A fraction $\lambda < 1$ of students also derive utility from enrolling in college. These students solve a dynamic utility maximization problem because their choices of effort and whether to sit the entrance exam affect future payoffs by affecting their admission chances. We describe the per-period utilities of these students first.

In time period 1, the per-period utility associated with each choice of effort $e_i \in \{0, 1, ..., E\}$ is:

$$u_{it}(e_{it}) = y(e_{it}, x_i, y_{it-1}^{(1)}, k_i, x_i; \alpha) - c(e_{it}; \xi) \tag{4}$$

where $c(e_{it}; \xi)$ is a quadratic cost of effort parameterized as follows:

$$c(e_{it}; \xi) = \xi_1 e_{it} + \xi_2 e_{it}^2. \tag{5}$$

The constant term in (5) is normalized to zero because only the difference in utilities across discrete choices is identified. As the scale of utilities is not identified, we normalize the coefficient of achievement $y(\cdot)$ to 1 in equation (4).

In time period 2, students decide whether to sit the PSU entrance exam. The per-period utilities associated with sitting ($S_i = 1$) and not sitting ($S_i = 0$) the exam are:

$$\begin{aligned} u_{it}^{S_i=0} &= 0 \\ u_{it}^{S_i=1} &= -c^S + \eta_{it} \end{aligned} \tag{6}$$

[34] We restrict GPA_{it} to be between 1 and 7.

where c^S is the cost of sitting the exam, capturing time, money and psychic costs, and η_{it} is a standard logistic shock.[35] The utility from not sitting the exam is normalized to zero because only the utility difference is identified.

In time period 4, depending on their admission sets, students choose whether to not enroll ($E_i = 0$), accept a regular channel admission offer ($E_i = R$) or accept a preferential admission offer ($E_i = P$). The per-period utilities associated with each choice are:

$$\begin{aligned} u_{it}^{E_i=0} &= 0 \\ u_{it}^{E_i=R} &= \lambda_{0k_i} + \eta_{it}^R \\ u_{it}^{E_i=P} &= \lambda_{0k_i} - \delta + \eta_{it}^P. \end{aligned} \tag{7}$$

The utilities depend on standard logistic preference shocks, η_{it}^R and η_{it}^P, and on type k_i to capture the fact that observationally identical students make different enrollment choices because of transitory and permanent shocks. The utility from enrolling via the preferential channel can be affected by stigma (δ).

The fraction $1-\lambda$ of students who derive no utility from college enrollment solve a static decision problem in model period 1 (effort decision). These students are those who, at baseline, report that they do not think they will stay in education beyond high school.[36] Their utility is $\tilde{\alpha} u_{i,t=1}$, where $u_{i,t=1}$ is defined in equation (4).[37] The treatment can have a direct effect ξ_3 on their cost of study effort: $c(e_{it};\xi) = \xi_1 e_{it} + \xi_2 e_{it}^2 + \xi_3$. Therefore, the model allows for a channel of policy impacts not mediated by the response to incentives.

5.2.5 Heterogeneous Subjective Beliefs

Subjective beliefs vary with individuals' observed and unobserved characteristics. We allow for correlation between beliefs, preferences and ability by introducing perma-

[35] The fee is approximately USD 30, but poor students can apply for a fee waiver.

[36] The translation of the survey question is "*Thinking about your future, what is the highest level of education you think you can complete?*." We assume that anyone who answers secondary education or less derives no utility from college enrollment. 9.7% of students fall in this category (thus, $\lambda = 90.3\%$), and 97.3% of them do not enroll in college.

[37] Parameter $\tilde{\alpha}$ captures the relative importance of present versus future time periods. It is identified by comparing the effort of those who do not intend to go to college with that of those who do. To see why, notice that at an interior optimum,

$$\tilde{\alpha}\frac{\partial u_0}{\partial e}|_{e^{*NoUni}} = \frac{\partial u_0}{\partial e}|_{e^{*Uni}} + \frac{\partial u_1}{\partial e}|_{e^{*Uni}} = 0, \tag{8}$$

where u_0 and u_1 represent the current and future utilities, respectively; e^{*NoUni} and e^{*Uni} are the effort choices of those who do not and those who do want to attend college, and $\tilde{\alpha}$ is the weight given to the current utility by those who solve a static problem. Larger values of $\tilde{\alpha}$ that do not violate equation (8) necessarily require larger values of e^{*NoUni} when u_0 is concave in effort. Therefore, the difference in effort choices ($e^{*Uni} - e^{*NoUni}$) identifies $\tilde{\alpha}$ because lower values of this difference imply a lower value of $\tilde{\alpha}$.

nent unobserved heterogeneity in the form of unobserved types (Heckman and Singer (1984); Keane and Wolpin (1994, 1997)) and letting key parameters of beliefs, preferences and ability vary across types. In particular, we let beliefs about educational production functions vary with type.

Beliefs about the school cutoff. The school cutoff is determined by the simultaneous effort choices of all students in the school. We do not assume that students play a rational expectations equilibrium. They 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.[38] We take this subjective belief as given and assume each student best-responds to it. Estimating the model does not require imposing assumptions on the beliefs that students hold about others' behaviors, nor does it require solving for the equilibrium of an effort game in each school at each parameter iteration, saving computing time in estimation.

Beliefs about the production functions. We assume that students hold the following beliefs about the PSU entrance exam score and GPA production functions:

$$PSU_{it}^b = \beta_{0k_i}^{Pb} + \beta_{1k_i}^{Pb} e_{it} + \beta_2^{Pb} y_{it-1}^{(1)} + \epsilon_{it}^{PSU^b}, \quad \epsilon_{it}^{PSU^b} \sim N(0, \sigma^2_{PSU^b}) \tag{9}$$

$$GPA_{it}^b = \beta_0^{Gb} + \beta_{1k_i}^{Gb} e_{it} + \beta_2^{Gb} y_{it-1}^{(2)} + \epsilon_{it}^{GPA^b}, \quad \epsilon_{it}^{GPA^b} \sim N(0, \sigma^2_{GPA^b}) \tag{10}$$

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. This captures heterogeneous beliefs about unmeasured factors such as ability. Additionally, the believed outcome varies with baseline characteristics and effort.

Subjective probability of a regular admission. 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:

[38] 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.

$$
\begin{aligned}
Pr^b(A_i^R = 1|e_{it}, y_{it-1}^{(1)}, k_i, S_i = 1) &= Pr\left(\overline{PSU}_{it}^b + \epsilon_i^{PSU^b} \geq \bar{c}^{Rb} + \epsilon_i^{c^{Rb}}\right) \qquad (11)\\
&= \Phi\left(\frac{\overline{PSU}_{it}^b - \bar{c}^{Rb}}{\sqrt{\sigma_{PSU^b}^2 + \sigma_{c^{Rb}}^2}}\right)\\
&= \Phi\left(\gamma_0^b + \gamma_1^b \overline{PSU}_{it}^b\right),
\end{aligned}
$$

where $\gamma_0^b = \frac{-\bar{c}^{Rb}}{\sqrt{\sigma_{PSU^b}^2 + \sigma_{c^{Rb}}^2}}$ and $\gamma_1^b = \frac{1}{\sqrt{\sigma_{PSU^b}^2 + \sigma_{c^{Rb}}^2}}$ and $\Phi(\cdot)$ is the standard Normal cumulative distribution function. Given an expected PSU score, uncertainty is generated by uncertainty around own score ($\sigma_{PSU^b}^2$) and around the admission cutoff ($\sigma_{c^{Rb}}^2$), which are absorbed by the parameters γ_0^b and γ_1^b. In the literature, 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.

Subjective probability of a preferential admission. 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, 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:

$$
\begin{aligned}
Pr^b(A_i^P = 1|e_{it}, y_{it-1}^{(2)}, k_i, S_i = 1) &= Pr\left(\overline{GPA}_{it}^b + \epsilon_i^{GPA^b} \geq c_0 + c\bar{1}5_i^b + \epsilon_i^{c15b}\right) \qquad (12)\\
&= \Phi\left(\frac{\overline{GPA}_{it}^b - c_0 - c\bar{1}5_i^b}{\sqrt{\sigma_{GPA^b}^2 + \sigma_{c15^b}^2}}\right)\\
&= \Phi\left(\xi_0^b + \xi_1^b(\overline{GPA}_{it}^b - c\bar{1}5_i^b)\right),
\end{aligned}
$$

where $\xi_0^b = \frac{-c_0}{\sqrt{\sigma_{GPA^b}^2 + \sigma_{c15^b}^2}}$ and $\xi_1^b = \frac{1}{\sqrt{\sigma_{GPA^b}^2 + \sigma_{c15^b}^2}}$.[39] Given an expected GPA and an expected cutoff, uncertainty is generated by the uncertainty around own GPA ($\sigma_{GPA^b}^2$) and around the school cutoff ($\sigma_{c15^b}^2$), which are absorbed by the parameters ξ_0^b and ξ_1^b. As before, we assume normality.

5.2.6 Objective Admission Process

Regular and preferential admissions are realized according to objective admission probabilities. The probability of regular admission for those who take the entrance exam is:[40]

[39] 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 an adjusted GPA score (see footnote 12).

[40] Admissions are a stochastic function of the PSU score because, first, colleges apply adjustments to students' raw entrance scores, implying that two students with identical PSU scores may have different college-major-specific entrance scores. Second, students with identical PSU scores may apply to colleges and majors with different selectivities.

$$Pr(A_i^R = 1 | PSU_i, S_i = 1) = \Phi(\gamma_0 + \gamma_1 PSU_i). \tag{13}$$

Preferential admissions are assigned to students in treated schools who sit the PSU entrance exam and whose GPA is in the top 15% of their school.

5.2.7 Permanent Unobserved Heterogeneity

In estimation, we assume that there are two unobserved types ($K = 2$) which follow a logit distribution that depends on the 9^{th} and 10^{th} grade GPA average ($y_{it-1}^{(3)}$) and an indicator for whether a student was surveyed in our data collection, D_i^s, to correct for survey attrition based on unobservables. Letting $X_i = [1, y_{it-1}^{(3)}, D_i^s]$:

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

Equation (14) determines the joint distribution of unobserved preferences, beliefs and abilities: parameters that govern the preference for college (λ_{0k_i}), pre-college beliefs ($\beta_{0k_i}^{Pb}, \beta_{1k_i}^{Pb}, \beta_{1k_i}^{Gb}$) and achievement ($\alpha_{0k_i}$) are type-dependent.

By virtue of the randomization, types are identically distributed across treatment groups. Therefore, the treatment dummy $T_{j(i)}$ does not enter the type probability in (14). This is useful for identification, which exploits the experimental variation.

5.3 Model Solution

A central model feature is the discrepancy between students' beliefs about their college admission credentials and probabilities and the objective values. Students construct a *subjective* value function using their beliefs, which we indicate with a b superscript:

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

where Ω_{it} is the state vector, which evolves from the initial condition $\Omega_{i1} = [x_i, k_i, y_{it-1}, c\bar{1}5_i^b, T_{j(i)}]$ according to *objective* production functions and admission probabilities, and d_{it} is the period choice. 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. Thanks to the assumption that the preference shocks have generalized extreme value distributions, we can compute the exact analytical solution, which is a sequence of optimal, non-randomized decision rules $\{d_{it}^*(\Omega_{it})\}$ for $t = 1, 2, 4$, which are deterministic functions of the state space Ω_{it}.

5.4 Discussion of Model Features

Beliefs about outcomes that depend on effort. Since effort is a choice, subjective beliefs about outcomes, such as GPA, depend not only on belief parameters, but also on the preference and ability parameters that affect the effort choice. This is fully incorporated in our model, which determines the effort choice endogenously.

Peer effects. At least three kinds of peer effects could be at play. First, the students' payoff in treated schools depends on GPA rank. This type of peer effect, a rank concern (Tincani (2018)), is fully incorporated into our model: students in treated schools form a belief about others' GPA and best respond to it. Second, teachers could best-respond to students (Todd and Wolpin (2018)), generating spillovers between students. The data evidence suggests that teachers' choices are not substantial drivers of policy impacts (Section 4.4), so we do not include them in the model. Finally, complementarities in the achievement production could be such that a change in effort among students whose incentives are affected by the policy directly affects others' effort. We do not explicitly model this peer effect, but we include it in a reduced-form way through the direct policy impact on the effort cost of students who do not intend to attend college (parameter ξ_3).

6 Estimation and Identification

This section presents the strategy and algorithm for estimating the structural model parameters and it describes the data measures and variation used to identify them.

6.1 Estimation

Aside from the parameters of the regular admission process in equation (13) (estimates are reported in Appendix 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 $(\xi, c^S, \lambda, \delta, \tilde{\alpha})$ and the distribution of unobserved types, model shocks and measurement errors $(\omega, \Sigma, \sigma^2_{m.e.y.}, \sigma^2_{m.e.e.})$. Estimation is by generalized indirect inference (Bruins, Duffy, Keane, and Smith Jr (2018)). In a first step, we estimate from the data a set of auxiliary models that summarize the experimental findings and data patterns to be targeted for 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 actual data and their counterparts from the data simulated using the structural model. We target the following auxiliary models and additional moment conditions (see the full list in Appendix D):

1. *Treatment Effects.* Regressions of achievement, study effort, admissions and enrollments on the treatment dummy and baseline characteristics.

2. *Descriptive Regressions.* Regressions of choices and outcomes on baseline characteristics.

3. *Descriptives and Correlations.* Mean and variances of outcomes, choices and beliefs in various sample subsets. Correlations between beliefs, behaviors and outcomes. Correlations in choices over time.

Estimation algorithm. At each parameter iteration θ, we simulate S datasets, where each simulation is a draw for the shocks $\epsilon_{it}^{Ps}, \epsilon_{it}^{Gs}, \eta_{it}^{s}, \eta_{it}^{Ps}, \eta_{it}^{Rs}$ and for the student type k_i^s.[41] 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. The generalized indirect inference method, developed for dynamic discrete choice models, ensures that the objective function is differentiable and allows us to rely on a fast derivative-based optimization method to solve (15).[42]

6.2 Identification

A central identification challenge in models that relax rational expectations is to separately identify unobserved ability, beliefs and preferences (Manski (2004)). To mitigate this problem, we leverage elicited belief data and experimental data variation. This section discusses the identification of belief parameters. The identification of the other parameters follows standard arguments for dynamic programming discrete choice models (Rust (1994); Keane and Wolpin (1997)).[43]

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

[42] Whenever the dependent variable of the auxiliary model is discrete, the dependent variable in the model-generated auxiliary model is obtained as a smoothed function of the latent utilities. Letting u_i indicate the latent utility associated with one of the choices (the utility from the other choice is normalized to zero), we follow Altonji, Smith Jr, and Vidangos (2013) and choose the smoothing function $\frac{exp(\frac{u_i}{\lambda})}{1+exp(\frac{u_i}{\lambda})}$ with smoothing parameter $\lambda = 0.05$. We use Knitro to solve the optimization problem (Byrd, Nocedal, and Waltz (2006)).

[43] In particular, permanent unobserved heterogeneity is identified from correlation in outcomes over time not explained by observed characteristics and from the specification of the unobserved types probabilities (Wooldridge (2005)). The parameters of the objective educational production

Beliefs about the production function of PSU and GPA ($\beta^{Pb}_{0k_i}, \beta^{Pb}_{1k_i}, \beta^{Pb}_2, \beta^{Gb}_0, \beta^{Gb}_{1k_i}, \beta^{Gb}_2$). To identify the coefficients of effort ($\beta^{Pb}_{1k_i}$, $\beta^{Gb}_{1k_i}$), we elicited beliefs about the PSU score and the GPA that students expect to obtain under the actual and alternative hypothetical effort levels. To facilitate understanding, we phrased the questions in terms of effort needed to achieve hypothetical levels of PSU and GPA.[44] For example, for PSU 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. We assume they are affected by additive measurement error: $h_i^{oj} = h_i^{*j} + \epsilon_i^{m.e.e.}$, where $j = 600, 450, 350$ and where $\epsilon_i^{m.e.e.} \sim N(0, \sigma^2_{m.e.e.})$ is drawn from the same distribution of the measurement error on reported actual hours of study per week. We convert the answers into an expected increase in PSU per additional reported hour of study per week. Let the reported actual effort be $e_i^o = e_i + \epsilon_i^{m.e.e.}$. Denote the elicited expected PSU score at the effort actually exerted by $PSU_i^b(e_i)$. We measure believed returns to effort in PSU production as

$$\sum_j \frac{1}{3} \frac{j - PSU_i^b(e_i)}{h_i^{oj} - e_i^o}, \quad \text{if } h_i^{oj} - e_i^o \neq 0 \quad \forall j, \text{ where } j = 350, 450, 600.$$

We eliminate answers that deliver negative or infinite returns. We follow the same procedure to measure the believed returns in GPA production. Appendix Figure A10 shows the distribution of measured returns to effort in the data.

We match the distribution of measured returns to effort using their model counterpart, obtained by simulating expected PSU and GPA scores at various values of reported hours of study. For example, for PSU, consider distinct effort levels h_i^z and h_i^j and denote by $\widehat{PSU_i^b}(h_i)$ the expected PSU score predicted by the model. The predicted returns per reported hour are:

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

We do not estimate a belief parameter for each student, but for each student type. This allows us to use the model to simulate the beliefs of students not sur-

functions are identified from repeated longitudinal measures of achievement and the value-added specification (Hanushek (1996, 2003); Krueger (2000); Todd and Wolpin (2003)).

[44]We thank Chilean teacher Yorka Álamos who suggested this wording in the piloting phase.

veyed, without the need to assume that survey non-respondents have the same belief distribution as respondents, because we estimated how the distribution of student types varies across respondents and non-respondents (equation (14)). To identify the remaining parameters of the believed production functions, we use the distribution of believed PSU scores and GPA at the effort levels actually exerted, and regressions of believed PSU and GPA on the lagged achievement measures that enter the production functions.

Subjective admission probabilities ($\xi_0^b, \xi_1^b, \gamma_0^b, \gamma_1^b$). To identify the parameters of the subjective probability of a preferential admission, we use differences in behavior across treatment groups: the randomized treatment acted as a shock to the saliency of this belief that kept unobserved factors constant. We formalize this intuition in Appendix E using a simplified version of the model. To identify the parameters of the subjective probability of a regular admission, we rely on a model restriction that we validate by comparing the subjective probability implied by the estimated model to the subjective probability that we elicited but did not target in estimation. Appendix E provides further details.

7 Model Results

This section describes the results from the estimation (Section 7.1) and from counterfactual simulations that i) quantify how beliefs shape the impacts of preferential admissions (Section 7.2) and ii) perform ex-ante evaluations of interventions that combine preferential admissions with belief correction (Section 7.3).

7.1 Estimation Results

7.1.1 Parameter Estimates and Model Fit

Parameters. Estimates of the model parameters and standard errors are reported in Appendix Table A9. A comparison of the perceived and objective production functions shows that students hold optimistic beliefs about returns to effort. The true coefficient of effort is 0.161 (0.038) in PSU (GPA) production. But students, depending on their type, believe it is between 0.262 and 0.331 (0.149 and 0.350).

Both student types are optimistic, but type 1 students, with higher unobserved ability and preference for college, are more optimistic. Therefore, there is a correlation between ability, preferences and beliefs.

Model fit. The model fits the key features of the data (Appendix Table A10). It fits the treatment effects reasonably well, although the treatment effect on enrollments is larger than its data counterpart by 4.2 percentage points. It successfully predicts that a high proportion of students take the entrance exam (55.21%), but a much lower proportion is admitted (7.70%). It captures optimism in PSU exam scores (the predicted belief bias is 0.653 standard deviations, relative to 0.591 in the data) and in school rank (38.5% are predicted to believe they graduate in the top 15%, relative to 43.1% in the data), and it closely matches perceived returns to effort. The model fits correlations between beliefs and behaviors (e.g., the correlation between the believed PSU score and enrollment is 23.9% in the data and 32.8% in the simulations), and correlations in outcomes over time (e.g., the correlation between the PSU score and enrollment is 52.07% in the data and 51.51% in the simulations).

The model can simultaneously fit beliefs, choices and outcomes, which is typically difficult to achieve because observable characteristics that predict choices and outcomes often explain only a small part of the variation in beliefs (Delavande and Zafar (2019); Bobba and Frisancho (2019)). We tackled this well-known challenge by including permanent unobserved heterogeneity in our model.

7.1.2 The Mechanism of Perceived Incentives

We now describe a central mechanism. We use the estimated model to calculate each student's perceived marginal productivity of effort in the likelihood of a college admission. In the absence of preferential admissions, this is the derivative of the perceived likelihood of a regular admission with respect to effort. In the presence of preferential admissions, it is the derivative of the perceived likelihood of obtaining either a preferential admission or a regular admission or both. Since this derivative varies with effort, we average it across effort levels. Letting $e = 0, 1, 2, ..., 10$ denote the possible levels of hours of study per week (effort) and $Pr^b(A_i = 1|e, \Omega_{i1})$ the perceived probability of an admission (conditional on sitting the entrance exam) for a student who exerts effort e and has a vector of initial conditions Ω_{i1}, the average perceived marginal productivity of effort for student i is approximated by the numerical derivative:

$$\frac{\partial Pr^b(A_i = 1|e, \Omega_{i1})}{\partial e} \approx \frac{1}{10} \sum_{e=0}^{9} \frac{Pr^b(A_i = 1|e + \Delta e, \Omega_{i1}) - Pr^b(A_i = 1|e, \Omega_{i1})}{\Delta e},$$

where $\Delta e = 1$. Using the distribution of initial conditions, we average this derivative across students, in the treatment and control groups.

Without the preferential admission policy, students believe that one additional hour of study per week in the last school semester increases the likelihood of college admission by 6.9 p.p., on average. With the policy, this figure falls to 1.5 p.p.: a negative effect of 5.4 p.p. Students perceive that the policy undercut their incentive to exert effort.

The consequences of perceived incentives for admission effects of preferential admissions. The regression analysis could not rule out that some students experienced negative admission effects (Section 4.1). We simulate within-individual treatment effects and identify the students who experience negative admission effects. We find that they are 5.7% of those who are admitted in the absence of preferential admissions, and that they have baseline test scores that are 0.81 standard deviations above the sample average. Therefore, being targeted by preferential admissions but not being awarded a preferential seat can have negative admission effects, by reducing the perceived incentive to prepare for the entrance exam. When students hold distorted beliefs, preferential admissions can stifle the upward mobility of some underprivileged high-achieving students.

7.2 Simulation Results: How Belief Biases Shape Choices and Policy Impacts

This section compares outcomes under biased beliefs to counterfactual ones that would have occurred if beliefs were correct. Students' beliefs are biased in multiple dimensions. We consider the benchmark of rational expectations, where all belief errors are corrected. Simulating rational expectations (RE) poses two challenges. First, we must simulate objective production functions and admission likelihoods. Thanks to the richness of the dataset, we can estimate objective processes from actual outcome data. Second, we must solve for the equilibrium of the tournament game that takes place in each school under the preferential admission regime. Students play a simultaneous effort game, because whether they obtain a preferential admission depends on the effort of all students in the school, which determines their school rank in equilibrium. Solving for the Bayesian Nash equilibrium is challenging, because it requires solving a high-dimensional fixed-point problem, as formalized in Appendix C.

Previous studies have simplified the problem by assuming, first, that there is a continuum of individuals and, second, that they differ only along one dimension (Hopkins and Kornienko (2004); Bodoh-Creed and Hickman (2018, 2019); Cotton, Hickman, and Price (2020)). But these two simplifications are inappropriate in our setting: i) our populations are schools, which are limited in size, and ii) we observe that individuals differ in more than one dimension. Therefore, we relax these assumptions and, to lower the dimensionality of the fixed point, we solve for an approximated equilibrium. In Appendix C, we define the equilibrium and provide the solution algorithm.[45] We find that the RE equilibrium is unique in all schools.

7.2.1 Pre-College Choices

Figure 2 reports the simulation results. For each student, we simulate the choices of effort and sitting the entrance exam with and without the preferential admission policy, under belief biases (the baseline scenario) and in the RE counterfactual. The within-student differences between choices under belief biases and RE measure choice distortions due to biases in beliefs. We find that, regardless of the admission regime, the optimistic belief biases lead students across the baseline test score distribution to over-sit the entrance exam (right panel), but they have heterogeneous effects on pre-college effort (left panel): they induce higher-ability students to incorrectly perceive an admission as guaranteed, and *under*-provide effort, and lower-ability students to incorrectly perceive it as within reach, and *over*-provide effort.

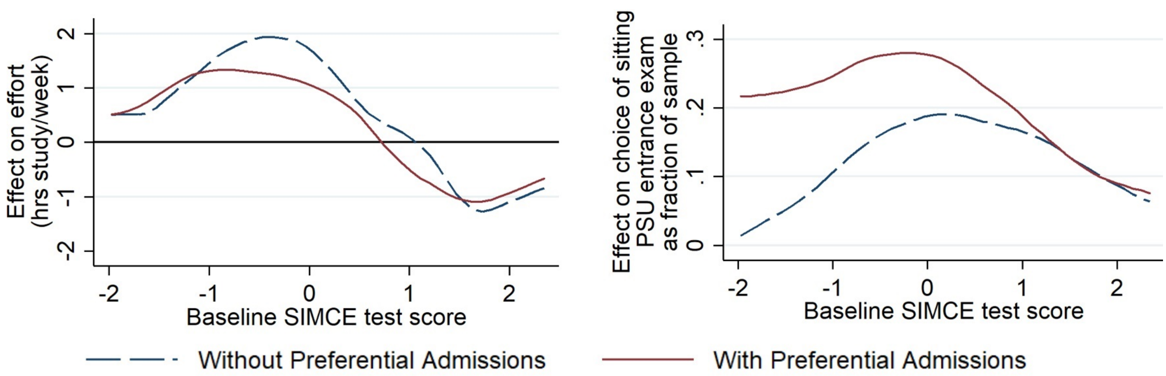

Figure 2: Heterogeneous effects of belief biases on pre-college effort and entrance-exam-sitting, with and without preferential admissions. Notes: For the sample of students in control schools in the data, we simulate these choices with and without the preferential admission policy, at baseline and in the rational expectations (RE) counterfactual. To simulate RE, we replace the subjective beliefs about the PSU production in (9) and the likelihood of a regular admission in (11) with their objective counterparts in (2) and (13); we replace the subjective belief about the GPA production in (10) with its objective counterpart in (3), and solve for the Bayesian Nash equilibrium using the algorithm in Appendix C to find the objective likelihood of a preferential admission, which replaces the subjective probability in (12). We take the within-student difference between choices in the baseline scenario and under RE, and plot them against the baseline SIMCE test score. Realizations of model shocks are kept constant across all scenarios.

7.2.2 Allocation of College Seats

Admissions and enrollments. The left panel of Figure 3 shows that, as a result of effort under-provision, high-ability students are under-admitted. Low-ability students

[45] We exploit the fact 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.

are over-admitted, more so under the preferential admissions regime. This may appear surprising at first: the left panel of Figure 2 showed that these students over-provide effort by less with preferential admissions than without, because of the negative effect of the policy on effort. But the relaxation of admission requirements means that, despite this effort reduction, low-ability students still get over-admitted at higher rates under preferential admissions.

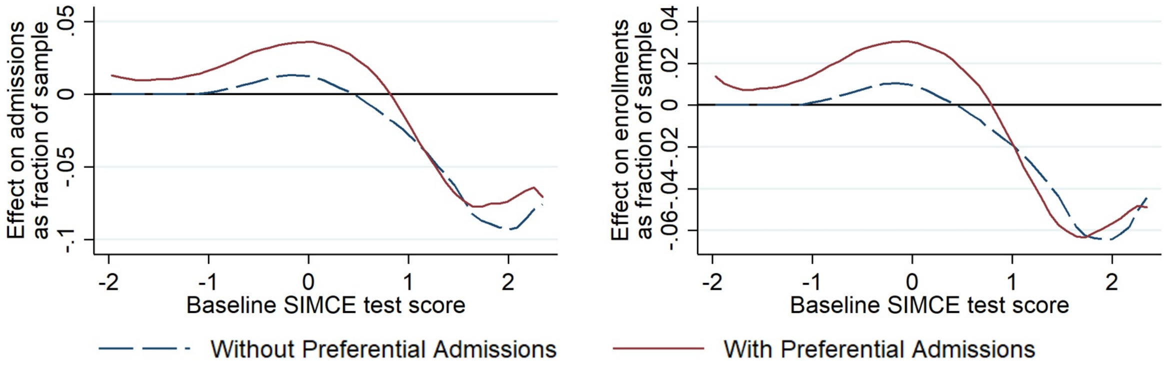

Figure 3: Heterogeneous effects of belief biases on admissions and enrollments with and without preferential admissions. Notes: We followed the simulation procedure described in the Notes to Figure 2, with the appropriate change in outcome variables.

The right panel of Figure 3 shows that the distortions in enrollments mirror those in admissions. First, admission is necessary for enrollment; therefore, under-admissions result in under-enrollments. Second, those who are over-admitted because of their optimistic belief biases tend to be those with a higher preference for college, because preferences correlate with beliefs (Section 7.1); therefore, they are likely to accept the admission offer.

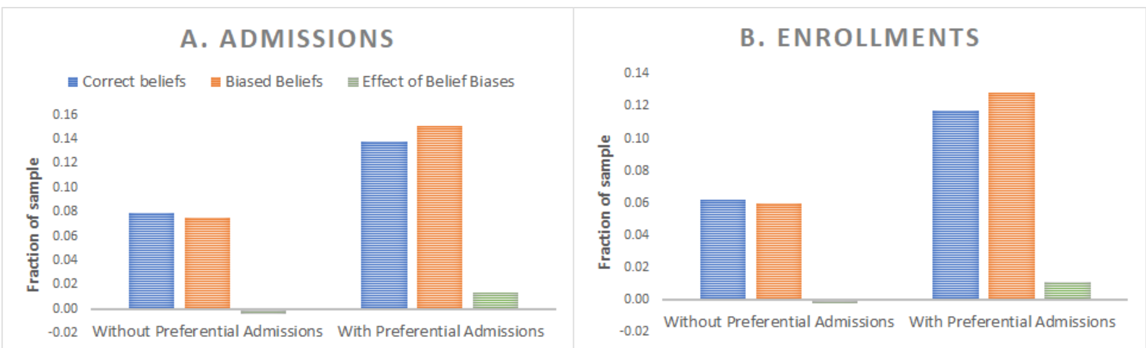

Figure 4: Average effects of belief biases on admission and enrollment rates with and without preferential admissions. Notes: We show average outcomes under RE ("Correct Beliefs") and in the baseline scenario ("Biased Beliefs"), and the average difference between them ("Effect of Belief Biases"), with and without preferential admissions.

Figure 4 reports the average effects of belief biases on admissions and enrollments. On average, without preferential admissions, belief biases have negligible impacts on admission and enrollment rates. With preferential admissions, they have a small positive effect of around 1 p.p. As a result, belief biases increase the admission and enrollment effects of the preferential admission policy above their rational expectations level. Therefore, belief biases are not the reason admission effects are considerably below their mechanical level on average (Section 4.1).[46] But as we have seen, the policy does have negative admission effects for some high-ability students who reduce their effort in response to perceived incentives (Section 7.1.2).

Composition of college entrants. Because they have heterogeneous effects on enrollments (Figure 3), belief biases distort the composition of college entrants. Panel A in Figure 5 shows that belief biases lower the mean ability of college entrants, measured by the baseline SIMCE test score, by 0.11 (0.18) standard deviations without (with) preferential admissions. Panel B shows that they also worsen the entrance exam scores of college entrants by approximately 0.20 standard deviations with and without preferential admissions.[47]

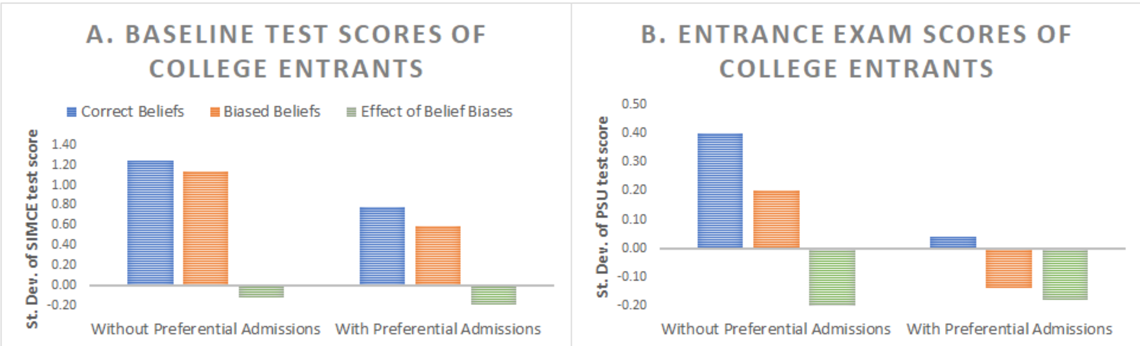

Figure 5: Average effects of belief biases on the composition and pre-college achievement of college entrants with and without preferential admissions. Notes: SIMCE test scores (Panel A) are standardized in the population of 10^{th} graders. PSU entrance exam scores (Panel B) are standardized in the population of test-takers. See the Notes to Figure 4.

Do the negative effects of belief biases on the composition of college entrants affect college dropout? To answer this question, we extend the model to incorporate a college dropout decision and estimate the extended model using additional data on dropout. We do not impose that lower SIMCE and entrance exam scores necessarily

[46]Preferences keep the value of attending college low for some students who graduate in the top 15% of treated schools. They renounce a preferential admission by not sitting the PSU exam.

[47]Because belief biases lead some students to over-provide effort in high-school (Section 7.2.1), their overall effect on the entrance exam scores of college entrants was ex-ante ambiguous. Empirically, belief biases lower the pre-college effort of college entrants on average. With preferential admissions, their pre-college effort is, on average, 3.40 hours/week under biased beliefs and 3.82 hours/week under rational expectations; without preferential admissions, it is 3.66 hours/week with biased beliefs and 4.39 hours/week under rational expectations.

lead to shorter persistence in college. Instead, we let the dropout decision depend on pre-college effort and a student's type, capturing ability and preferences.[48]

We find that belief biases cause higher dropout, and this effect is stronger under preferential admissions. In the absence of preferential admissions, belief biases increase dropout from 31 to 34 percent of college entrants, a 3 p.p increase, or 9.7%. In the presence of preferential admissions, they increase dropout from 31 to 36 percent of college entrants, a 5 p.p. increase, or 16.1%.

To summarize, belief biases lower the baseline ability and pre-college achievement of college entrants, resulting in higher dropout, especially under preferential admissions. We interpret these findings as evidence that belief biases lower the academic preparedness of college entrants, more so under preferential admissions. Distortions are stronger under preferential admissions because i) under-admission and under-enrollment distortions among high-ability students are slightly more common, due to the effort reduction, resulting in negative admission effects of the policy among some high-achievers; ii) over-admission and over-enrollment distortions among low-ability students are considerably more common, due to the relaxed admission requirements. Channel ii) is quantitatively more important (Appendix Table A11).

7.2.3 Summary & Interpretation of Findings on the Role of Belief Biases

The results in this section show that pre-college belief biases distort pre-college effort, generating distortions in the allocation of college seats that are more severe under preferential admissions. As a result, belief biases inflate the admission and enrollment effects of preferential admissions and exacerbate their negative effect on the academic preparedness of college entrants from disadvantaged groups. Conversely, they can also induce some high-ability students to miss out on college admission in response to preferential admissions. Moreover, we have shown that pre-college belief biases can determine college dropout.[49]

Preferential admissions are highly controversial, because critics claim they induce underprepared disadvantaged students to displace better-prepared advantaged ones from college seats. Our results speak to this central criticism and show that, when

[48] We replace the per-period enrollment utilities in equation (7) with $u_{it}^{E_{it}=R} = \tilde{\lambda}_{0k_i} + \lambda_1 e_{i,t=1} + \eta_{it}^R$ and $u_{it}^{E_{it}=P} = \tilde{\lambda}_{0k_i} + \lambda_1 e_{i,t=1} - \delta + \eta_{it}^P$, where k_i is the student type and $e_{i,t=1}$ is pre-college effort. There are two enrollment choices, in the first and second college year, corresponding to model periods $t = 4, 5$. We estimate the new parameters $\tilde{\lambda}_{0k_i}$ and λ_1, and fix all other parameters at their values estimated from the main model. Parameter estimates are $\hat{\tilde{\lambda}}_{01} = 0.8632, \hat{\tilde{\lambda}}_{02} = 0.4342, \hat{\lambda}_1 = 0.1004$.

[49] In general, reducing dropout is not necessarily welfare improving (Stinebrickner and Stinebrickner (2014a); Arcidiacono, Aucejo, Maurel, and Ransom (2016); Larroucau and Rios (2020)). A detailed welfare analysis is beyond the scope of this paper.

beliefs are biased, it is in principle possible to redesign these policies so as to improve the academic preparedness of college entrants from the disadvantaged group.

7.3 Simulation Results: Ex-Ante Policy Evaluations

Motivated by the previous findings, we examine the likely impacts of combining preferential admissions with informational interventions in high schools that eliminate belief biases. Students hold different dimensions of belief biases; we consider eliminating either only those concerning within-school rank and the likelihood of a preferential admission, or all of them.[50]

First, Panel A in Figure 6 shows that introducing preferential admissions without belief correction, the scenario implemented in Chile, negatively affects the academic preparedness of college entrants as captured by their entrance exam scores (first bar), because of the relaxed admission requirements. Second, adding the partial belief correction mitigates this negative effect, and full belief correction provides the strongest mitigation (second and third bars).[51] This is for two reasons: eliminating biases in beliefs improves the selection of college entrants in terms of baseline ability, and it increases their pre-college effort.

Panel B in Figure 6 shows the simulated effects on the average pre-college achievement of all students in targeted schools, not only of those who enroll in college. First, the baseline scenario (first bar) shows that preferential admissions reduce pre-college achievement by approximately 0.10 standard deviations on average, because a preferential admission is perceived as easier to obtain than a regular admission, as we discussed. Second, eliminating only belief biases about the preferential admission channel avoids most of this reduction (second bar), because most students realize that a preferential admission is harder to obtain than they perceived. Third, eliminating all belief biases would lead to a large average negative effect on pre-college achievement (third bar). This is because, in the absence of such belief corrections, students who incorrectly perceive a regular admission as within reach exert pre-college effort to obtain one. But realizing that a regular admission is out of reach leads them to lower their pre-college effort. This reduction in effort more than offsets the increase in effort among college entrants and results in average detrimental effects on achievement.

[50]To simulate outcomes when beliefs are biased, we use the estimated model. To simulate outcomes when all beliefs are corrected, we use the RE counterfactual. To simulate outcomes when only belief biases about the preferential channel are corrected, we start from the estimated model and replace the subjective production function of GPA in (10) with its objective counterpart in (3). We then solve for the Bayesian Nash equilibrium to determine the objective likelihood of a preferential admission, which replaces its subjective counterpart in (12).

[51]These results are unaffected if we use baseline test scores of college entrants or their dropout rates to measure academic preparedness instead.

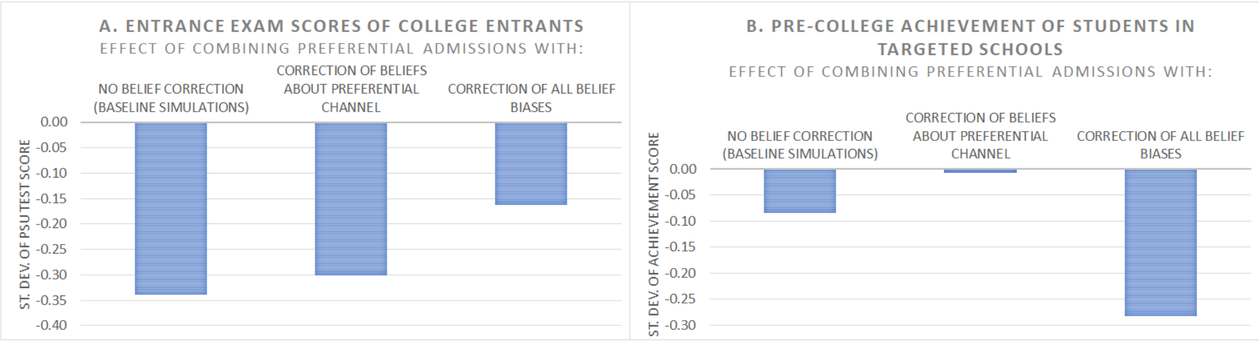

Figure 6: Average effects of preferential admissions with belief correction interventions. Notes: For the sample of students who are in control schools in the data, we simulate the average impacts of three interventions on the entrance exam scores of college entrants (Panel A) and on the pre-college achievement of students in targeted schools (Panel B).

We take this as evidence that the impacts of information interventions depend on what dimension of belief bias they correct. A policymaker wanting to correct all belief biases faces a trade-off: combining preferential admissions with full belief correction can increase college enrollment and mitigate negative effects on the academic preparedness of disadvantaged college entrants. However, it can induce a substantial reduction in the average pre-college achievement of students in targeted schools, negatively affecting the outcomes of those who do not go to college.

8 Conclusions

Inclusion policies are among the most prevalent and fastest-growing policy tools to expand the opportunities of disadvantaged groups in society (OECD (2020)). Now more than ever it is important to understand what drives their impacts. It is common for existing empirical and theoretical models to either treat investment decisions as exogenous (e.g. Chetty, Friedman, Saez, Turner, and Yagan (2020)) or assume individuals respond rationally to the investment incentives introduced by these policies (e.g., Coate and Loury (1993); see also the survey in Fang and Moro (2011)). Our findings demonstrate that understanding how individuals respond to perceived incentives is essential to understand the impacts of inclusion policies on the allocation of talent to opportunity, and ultimately on diversity and social mobility.

The current study does not seek to understand the origins of belief errors, but our results suggest that their consequences are more widespread than previously thought. Therefore, this is an important area for future research.

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Online Appendix

A Additional Tables

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

	All students (1)	Targeted students (2)
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.– 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: Validating Achievement and Effort Measures

	Sit PSU (1)	Apply (2)	Admitted (3)	Enroll year 1 (4)	Enroll year 2 (5)
	A. Achievement				
Achievement Score	0.060***	0.074***	0.057***	0.047***	0.038***
	(0.009)	(0.008)	(0.008)	(0.007)	(0.007)
PSU score	No	No	No	No	No
Dep. var. mean	0.725	0.241	0.131	0.094	0.078
Observations	2,922	2,922	2,922	2,922	2,922
Pseudo-R^2	0.100	0.169	0.283	0.296	0.262
		B. Achievement, controlling for PSU score			
Achievement Score		0.037**	0.017**	0.016**	0.013**
		(0.012)	(0.006)	(0.006)	(0.007)
PSU score		Yes	Yes	Yes	Yes
Dep. var. mean		0.333	0.180	0.130	0.107
Observations		2,122	2,122	2,122	2,122
Pseudo-R^2		0.238	0.552	0.507	0.421
	C. Study effort				
Study Effort Score	0.056***	0.069***	0.045***	0.034***	0.031***
	(0.010)	(0.009)	(0.007)	(0.006)	(0.006)
PSU score	No	No	No	No	No
Dep. var. mean	0.731	0.244	0.133	0.095	0.081
Observations	2,746	2,746	2,746	2,746	2,746
Pseudo-R^2	0.096	0.163	0.257	0.262	0.237
		D. Study effort, controlling for PSU score			
Study Effort Score		0.055***	0.018**	0.014*	0.017**
		(0.010)	(0.007)	(0.007)	(0.007)
PSU score		Yes	Yes	Yes	Yes
Dep. var. mean		0.334	0.182	0.130	0.110
Observations		2,010	2,010	2,010	2,010
Pseudo-R^2		0.241	0.546	0.501	0.421

Note.–: The outcome variables, listed at the top of the Table, are the same across Panels. The Panels differ in the measure (of achievement or of effort) used as an explanatory variable, high-lighted in the title of each Panel, and in some of the controls, high-lighted in the left-most column. All regressions use the standard set of controls (see notes under Table 5) and Inverse Probability Weights. Sample restriction: students in control schools. Average marginal effects from probit models reported. Delta-method standard errors clustered at school level in parenthesis. The study effort score is the standardized score predicted from the principal component analysis of the eight survey instruments reported in Appendix Table A5. *p < 0.10; **p < 0.05; ***p < 0.01.

Table A3: Validating Belief Data I

	Sit PSU	Apply	Enroll year 1	Enroll year 2
	(1)	(2)	(3)	(4)
	A. Believed PSU score			
Believed PSU score	0.048***	0.086***	0.060***	0.048***
	(0.010)	(0.010)	(0.007)	(0.008)
PSU score	No	No	No	No
Dep. var. mean	0.768	0.272	0.107	0.090
Observations	2,401	2,401	2,401	2,401
Pseudo-R^2	0.089	0.161	0.284	0.249
	B. Believed PSU score, controlling for PSU score			
Believed PSU score		0.065***	0.029***	0.024**
		(0.012)	(0.006)	(0.007)
PSU score		Yes	Yes	Yes
Dep. var. mean		0.354	0.139	0.117
Observations		1,848	1,848	1,848
Pseudo-R^2		0.239	0.503	0.417
	C. Believed school rank			
Believes is in top 15%	0.028	0.038*	0.020	0.026*
	(0.024)	(0.022)	(0.015)	(0.015)
Believed PSU score	No	No	No	No
Dep. var. mean	0.786	0.310	0.137	0.111
Observations	1,708	1,708	1,708	1,708
Pseudo-R^2	0.066	0.122	0.213	0.175
	D. Believed school rank, controlling for believed PSU score			
Believes is in top 15%	0.001	0.014	-0.002	0.010
	(0.023)	(0.021)	(0.015)	(0.014)
Believed PSU score	Yes	Yes	Yes	Yes
Dep. var. mean	0.804	0.327	0.146	0.120
Observations	1,541	1,541	1,541	1,541
Pseudo-R^2	0.072	0.150	0.261	0.209

Note.–: The outcome variables, listed at the top of the Table, are the same across Panels. The Panels differ in the subjective belief used as an explanatory variable, high-lighted in the title of each Panel, and in some of the controls, high-lighted in the left-most column. All regressions use the standard set of controls (see notes under Table 5) and Inverse Probability Weights. Sample restriction: students in control schools. Average marginal effects from probit models. Delta-method standard errors clustered at school level. The believed PSU score is standardized using the distribution of PSU scores among all exam-takers in the country. 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. *p < 0.10; **p < 0.05; ***p < 0.01.

Table A4: Validating Belief Data II

	Sit PSU		Believed top 15% cutoff		
	(1)	(2)	(3)	(4)	(5)
Believed GPA minus believed cutoff	0.023**	0.002			
	(0.010)	(0.009)			
Actual top 15% cutoff			0.390***	0.369***	0.357***
			(0.102)	(0.077)	(0.081)
Sample	Treatment	Control	Control	Control	Control
Standard controls	Yes	Yes	No	Yes	Yes
Fieldworker fixed effects	No	No	No	No	Yes
Dep. var. mean	0.714	0.753	5.823	5.826	5.825
Observations	2,595	2,460	3,326	3,307	3,307
Pseudo-R^2	0.131	0.078	0.012	0.038	0.053

Note.– The standard set of controls is described in the notes under Table 5. Inverse Probability Weights used. Columns 1-2: Average marginal effects from probit models; delta-method standard errors clustered at school level. Columns 3-5: Coefficients are OLS estimates; standard errors clustered at the school level. Actual and believed cutoff are measured in GPA-points. *p < 0.10; **p < 0.05; ***p < 0.01.

Table A5: 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 5), 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 A6: AVERAGE TREATMENT EFFECT ON GPA BY SUBJECT TYPE

	12^{th} grade GPA (standardized)	
	Subjects tested in PSU	Subjects not tested in PSU
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 5). 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 A7: LEE BOUNDS FOR AVERAGE TREATMENT EFFECTS

Treatment effect on	Lower bound	Upper bound
	(1)	(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 precollege 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 6, to keep our analysis of bounds analogous to the main average treatment effects.

Table A8: PARAMETERS ESTIMATED OUTSIDE OF THE MODEL

Symbol	Description	Estimate	Standard Error
(1)	(2)	(3)	(4)
γ_0	Constant	-0.329***	0.060
γ_1	Coefficient of PSU	2.469***	0.197

NOTE.– Estimates from Probit regression. Standard errors clustered at school level. *p < 0.10; **p < 0.05; ***p < 0.01.

Table A9: Parameter Estimates

Symbol (1)	Description (2)	Estimate (3)	Standard Error (4)
	A. Preferences		
ξ_1	Linear term, effort cost	-0.144^{***}	0.0041
ξ_2	Quadratic term, effort cost	-0.029^{***}	0.0015
ξ_3	Coefficient on treatment in effort cost for those w/ no intention to enroll	-0.018^{**}	0.0076
$\tilde{\alpha}$	Preference for current time	1.384^{***}	0.0039
c^S	Cost of sitting PSU exam	0.466^{***}	0.0032
λ_{01}	Utility from enrollment, type 1	1.709^{***}	0.0025
λ_{02}	Utility from enrollment, type 2	0.867^{***}	0.0068
δ	Dis-utility from PACE enrollment	0.082^{***}	0.0040
	B. Technology		
α_{01}	Constant in achievement, type 1	0.001	0.0055
α_{02}	Constant in achievement, type 2	-1.130^{***}	0.0055
α_{11}	Age in achievement	0.134^{***}	0.0078
α_{12}	Female in achievement	-0.238^{***}	0.0058
α_{13}	Low-SES in achievement	-0.092^{***}	0.0038
α_{14}	Never failed a year in achievement	-0.167^{***}	0.0061
α_{15}	Academic track in achievement	0.116^{***}	0.0049
α_2	Effort in achievement	0.277^{***}	0.0040
α_3	Lagged test score in achievement	0.621^{***}	0.0051
β_0^G	Constant in GPA	2.125^{***}	0.0025
β_1^G	Effort in GPA	0.038^{***}	0.0055
β_2^G	Lagged GPA in GPA	0.625^{***}	0.0064
β_0^P	Constant in PSU entrance exam score	-1.395^{***}	0.0065
β_1^P	Effort in PSU entrance exam score	0.161^{***}	0.0068
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	-1.394^{***}	0.0028
β_{02}^{Pb}	Constant in believed PSU entrance exam score, type 2	-1.695^{***}	0.0046
β_{11}^{Pb}	Effort in believed PSU entrance exam score, type 1	0.331^{***}	0.0042
β_{12}^{Pb}	Effort in believed PSU entrance exam score, type 2	0.262^{***}	0.0057
β_2^{Pb}	Lagged test score in believed PSU entrance exam score	0.955^{***}	0.0051
β_0^{Gb}	Constant in believed GPA	-2.201^{***}	0.0026
β_{11}^{Gb}	Effort in believed GPA, type 1	0.350^{***}	0.0080
β_{12}^{Gb}	Effort in believed GPA, type 2	0.149^{***}	0.0056
β_2^{Gb}	Lagged GPA in believed GPA	1.209^{***}	0.0070
γ_0^b	Constant in subj. prob. regular admission	0.408^{***}	0.0049
γ_1^b	Believed entrance exam score in subj. prob. regular admission	0.911^{***}	0.0051
ξ_0^b	Constant in subj. prob. PACE admission	1.067^{***}	0.0075
ξ_1^b	Perceived distance from cutoff in subj. prob. PACE admission	0.185^{***}	0.0046
	D. Unobserved Heterogeneity and Shocks		
ω_0	Constant in prob. type 1	-1.500^{***}	0.0034
ω_1	Missing survey in prob. type 1	-1.497^{***}	0.0060
ω_2	Lagged GPA in prob type 1	0.503^{***}	0.0044
$\sigma^{m.e.y.}$	St. dev. of measurement error on achievement score	0.776^{***}	0.0069
$\sigma^{m.e.e.}$	St. dev. of measurement error on hours of study	2.720^{***}	0.0019
σ_G	St. dev. GPA shock	0.553^{***}	0.0048
σ_P	St. dev. PSU entrance exam shock	0.399^{***}	0.0051
ρ	Correlation coefficient of GPA and PSU shocks	0.757^{***}	0.0043

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

Table A10: Model Fit

	Sample (1)	Data (2)	Simulations (3)
		A. Choices (means)	
Study hours per week	All	4.11	3.82
Admitted (%)	Control	11.17	7.70
Pace admissions (%)	Treated	11.46	11.31
Corr (admitted regular, admitted PACE) (%)	Treated	30.85	31.26
Sit PSU entrance exam (%)	Control	65.64	55.21
PSU score (σ)	Control	-0.601	-0.729
GPA (GPA points)	Control	5.691	5.651
Enrolled (%)	Control	8.08	6.25
Corr (sit PSU, enroll) (%)	Control	21.454	23.263
Corr (PSU, enroll) (%)	All	52.071	51.511
		B. Beliefs (means)	
Believed PSU score (σ)	Control	-0.033	-0.187
Believed minus actual PSU score | sit exam (σ)	Control	0.591	0.653
Believed minus actual GPA (GPA points)	Control	-0.075	-0.164
Believes is in top 15% of school	Control	0.431	0.385
Corr (believed PSU, enroll) (%)	All	23.9	32.8
Perceived mg. returns to study hours, GPA (GPA points)	All	0.177	0.165
Perceived mg. returns to study hours, PSU (σ)	All	0.299	0.316
		C. Treatment Effects	
Achievement score (σ)	All	-0.111	-0.072
Study hours per week	All	-0.257	-0.342
Enrollment (%)	All	3.234	7.435

Note.– Panel C reports OLS estimates from regressions that do not use field-worker fixed effects.

Table A11: Enrollment Distortions due to Belief Biases

	With preferential admissions		Without preferential admissions	
	Mistakenly In (1)	Mistakenly Out (2)	Mistakenly In (3)	Mistakenly Out (4)
Baseline SIMCE score (standardized)	0.063	1.145	0.471	1.101
	(0.741)	(0.549)	(0.732)	(0.587)
Observations (as frac. of college entrants w/ biased beliefs)	0.196	0.109	0.184	0.224
Observations	106	59	46	56

Note.– Each column represents a sample of students with a specific combination of baseline and counterfactual outcomes: we distinguish between those who enroll under biased beliefs but would not have enrolled under rational expectations ("Mistakenly In", or over-enrollments), and those who do not enroll under biased beliefs but would have enrolled under rational expectations ("Mistakenly Out", or under-enrollments). To simulate outcomes, we start from the sample of students who, in the data, are in control schools, and for each student we simulate choices and outcomes with and without the preferential admission policy, in the baseline and RE scenarios. Realizations of model shocks are kept constant across scenarios.

B Additional Figures

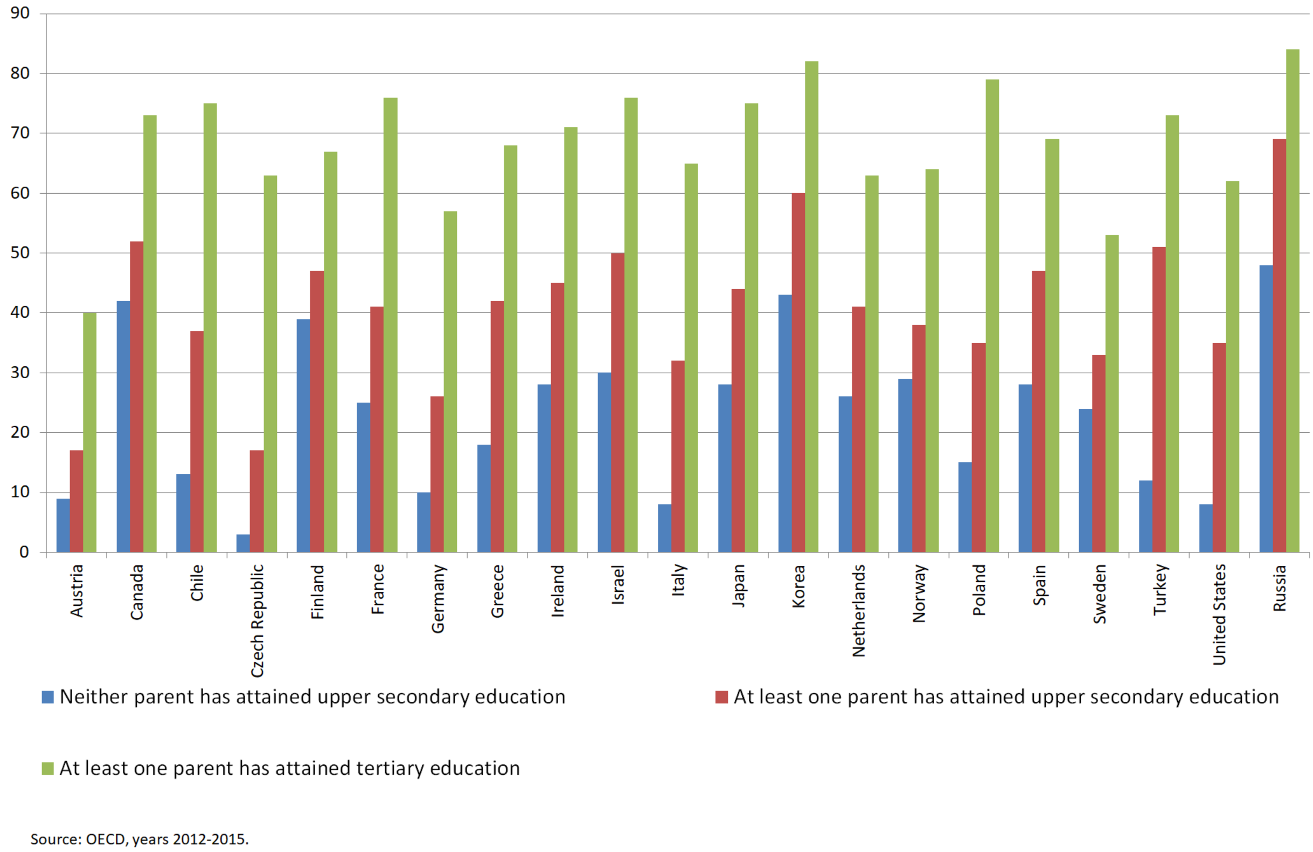

Figure A1: Percentage of 24-45 year-old with tertiary education by parental education, OECD countries.

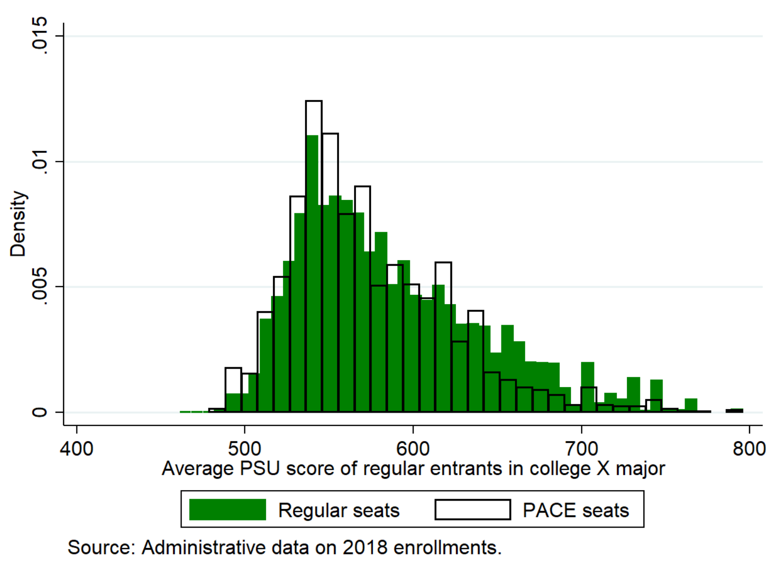

Figure A2: Quality distribution of PACE and regular college seats.

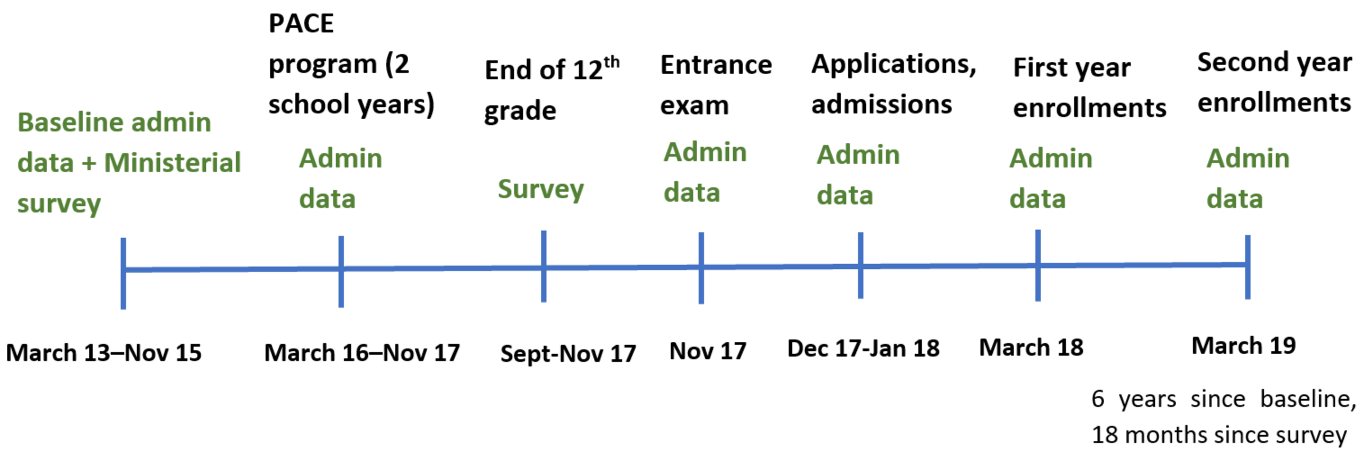

Figure A3: Timeline.

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 12^{th} 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 6^{th} 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 12^{th} student (top 30%) and the 30^{th} student (bottom 25%)]	*Free format*

Figure A4: Selected survey questions.

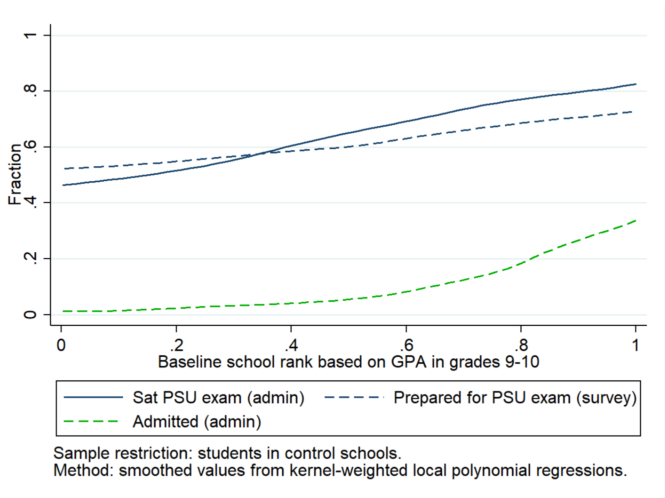

Figure A5: Decision to sit and prepare for PSU entrance exam and objective admission likelihood.

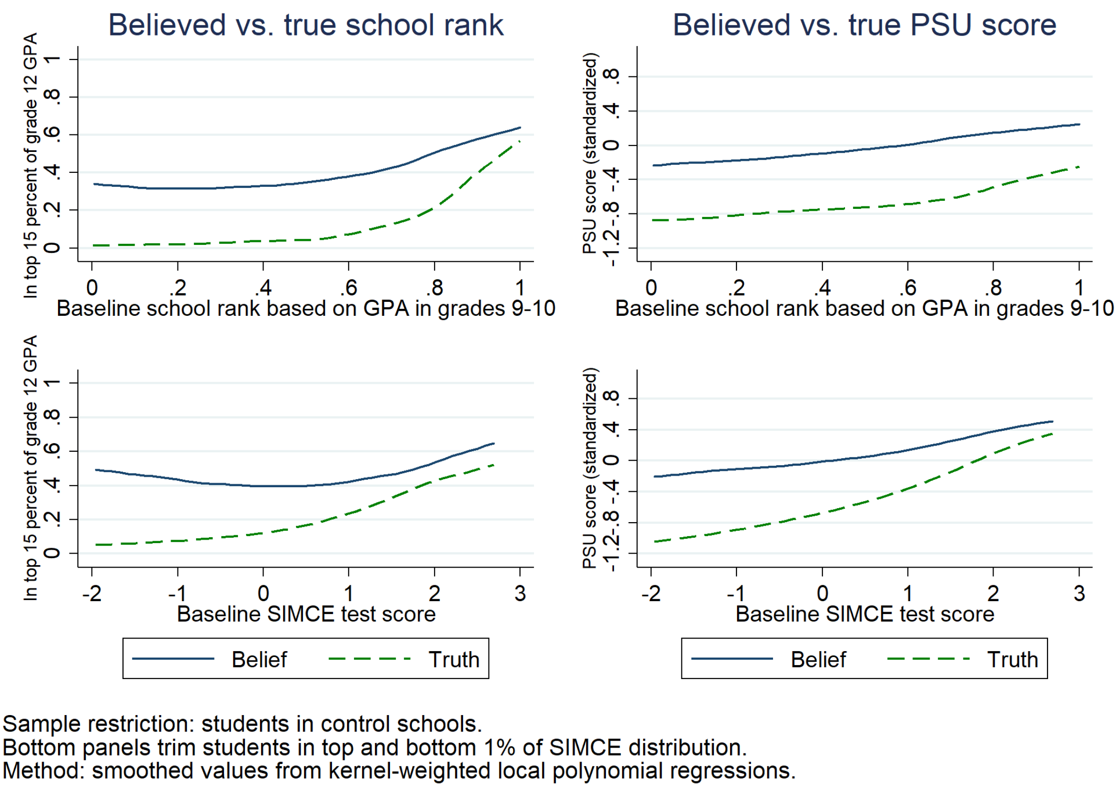

Figure A6: Heterogeneity of subjective beliefs by baseline within-school rank and by baseline test scores.

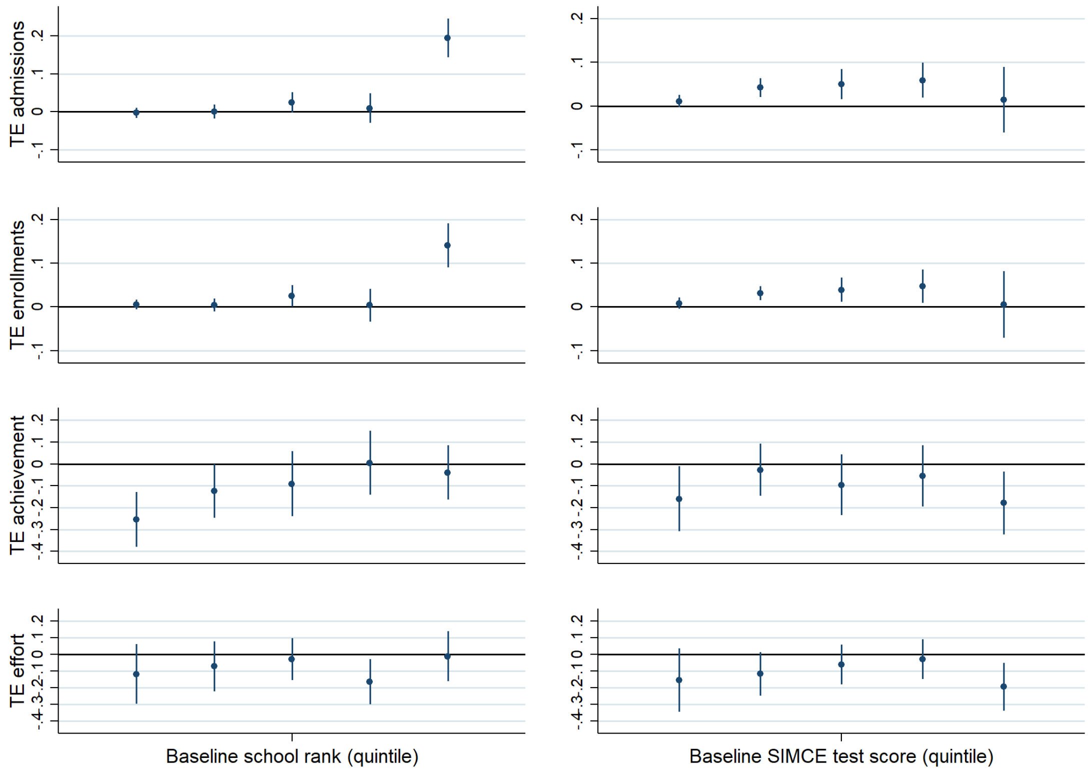

Figure A7: Treatment effect heterogeneity. Notes: Each dot is the coefficient on *Treatment* from an OLS regression where: *Treatment* is a dummy variable indicating whether a student is in a school that was randomly assigned to be in the PACE program, the controls are the standard set of controls (see Table 5), Inverse Probability Weights and field-worker fixed effects are used for the survey outcomes effort and achievement, the estimation samples are quintiles in the school rank based on 10^{th} grade GPA (left panel) and quintiles in the distribution of 10^{th} grade standardized test scores (SIMCE) in the entire sample (right panel). The units of measurement of the treatment effects are: standard deviations for achievement and effort, percentage points for admissions and enrollments. The bars are 95% confidence intervals built using standard errors clustered at the school level.

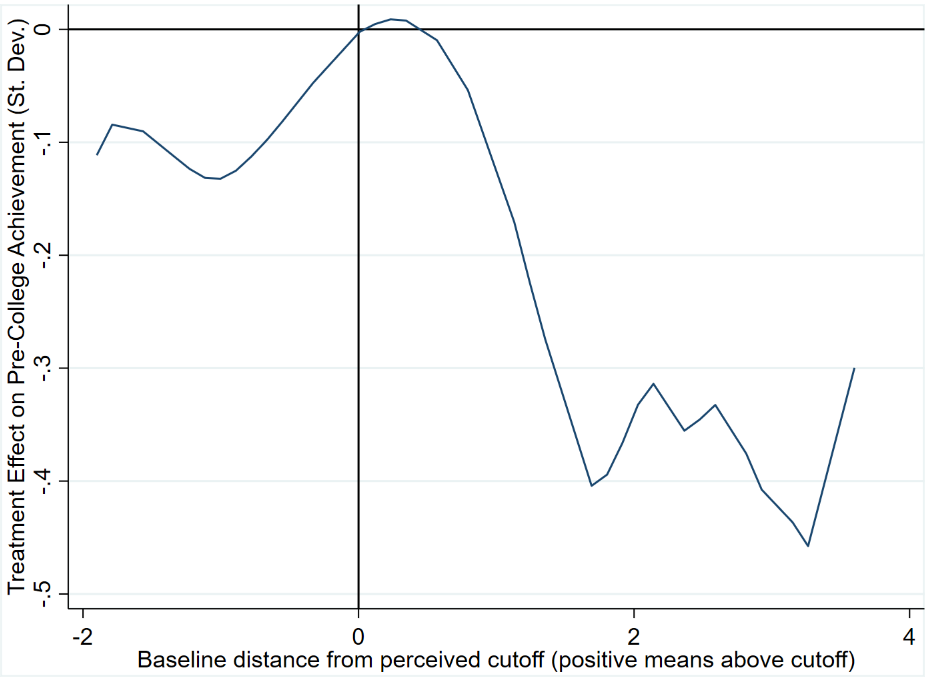

Figure A8: Treatment effect on achievement score by distance from perceived school cutoff. Notes: The treatment effect is obtained as the difference of smoothed values from kernel-weighted local polynomial regressions estimated in treated and control schools. Dependent variable: Achievement score, standardized. Independent variable: difference between GPA in 10^{th} grade and the perceived school cutoff.

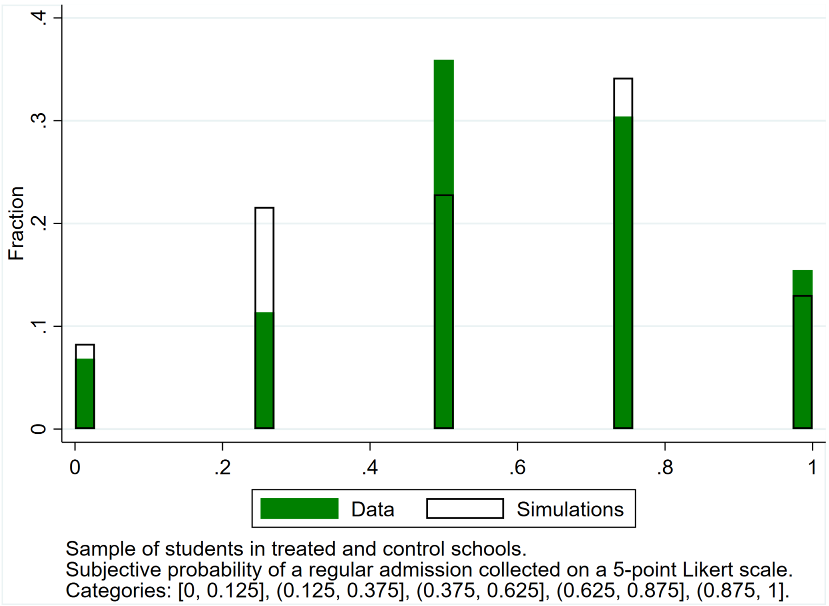

Figure A9: Validation: Matching elicited subjective probability data not used in estimation.

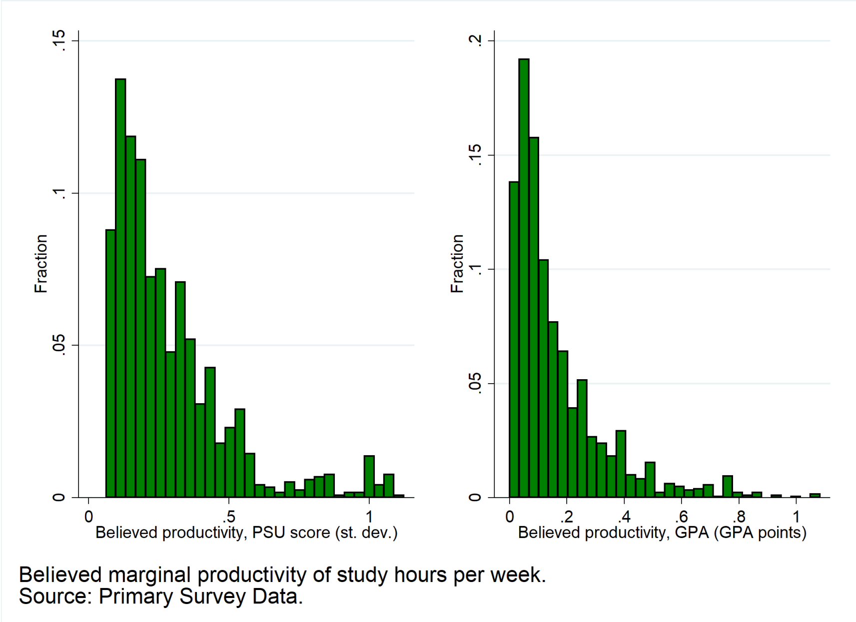

Figure A10: Distribution of believed marginal productivity of effort.

C Rational Expectations Equilibrium: Definition and Algorithm

First, we define 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, which is 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, realized in later periods, is common knowledge. Model shocks include preference $(\eta_{it}, \eta_{it}^R, \eta_{it}^P)$ and technological $(\epsilon_{it}^P, \epsilon_{it}^G)$ shocks. Objective production functions (equations (1), (2) and (3)) are common knowledge. The existence of types makes this a game of incomplete information.

$e_i(\cdot)$ is a function mapping $\{1, 2, ..., K\}$ into $\{0, 1, 2, ..., E\}$, which is the set of effort choices. It 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 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 multidimensional 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.[52] 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 the approximated expected payoff of i calculated using this probability approximation.

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:*

- *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 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

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

GPA, X_i are baseline student characteristics and Z_j are baseline school characteristics. We 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:

$$Prob_i(Adm^P = 1|GPA_{it}, X_i, Z_j) = \gamma_0 + \gamma_1 GPA_{it} + \gamma_2 X_i + \gamma_3 Z_j + \epsilon_{ij} \quad (16)$$

   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% of their school.

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 $\gamma_{1j}^{(s+1)}$ against $\gamma_{1j}^{(s)}$ and found that there is a unique fixed point in all schools.

D Auxiliary Regressions and Moments Used in Estimation

1. *Treatment Effect Regressions*:

- All parameters, including the constant, of a regression of achievement score on treatment, age, female, low-SES index (*alumno prioritario*), dummy for whether ever failed a year, school type, baseline SIMCE score, average GPA in 9^{th} and 10^{th} grade (9).

- Coefficient on treatment of a regression of hours of study (i.e., our noisy measure of effort) on treatment, age, female, low-SES index (*alumno prioritario*), dummy for whether ever failed a year, school type, baseline SIMCE score (1).
- For the sample of students who have no intention to stay in school beyond high-school, coefficient on treatment of a regression of hours of study on treatment, age, female, low-SES index (*alumno prioritario*), dummy for whether ever failed a year, school type, baseline SIMCE score (1).
- Coefficient on treatment of a regression of admissions through the regular channel on treatment, age, female, low-SES index (*alumno prioritario*), dummy for whether ever failed a year, school type, baseline SIMCE score (1).
- Coefficient on treatment of a regression of first-year enrollment on treatment, age, female, low-SES index (*alumno prioritario*), dummy for whether ever failed a year, school type, baseline SIMCE score (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 in regression of 12^{th} grade GPA on 10^{th} grade GPA (1).
- Coefficient in regression of PSU entrance exam score on baseline SIMCE score (1).
- Coefficients on dummy for whether the student was surveyed and on average GPA in 9^{th} and 10^{th} grades in a regression of a dummy for whether the student sat the PSU entrance exam score on these two variables and on age, female, low-SES index (*alumno prioritario*), dummy for whether ever failed a year, school type, baseline SIMCE score (2).
- Coefficient on average GPA in 9^{th} and 10^{th} grades in a regression of hours of study on this variable and on age, female, low-SES index (*alumno prioritario*), dummy for whether ever failed a year, school type, baseline SIMCE score (1).
- Variance of the residuals from regressions of the achievement score and of 12^{th} grade GPA on all the observed initial conditions in the model (treatment, GPA in 10^{th} grade, average between 9^{th} and 10^{th} grade GPA, intention to continue studying beyond high-school, believed cutoff, baseline SIMCE score, gender, low-SES indicator (*alumno prioritario*, dummy for whether student ever failed a year, school type, age) (2).

3. *Descriptives and Correlations*:

- Mean and variance of hours of study (2).

- Fraction admitted through the regular channel in the control group (1).
- Fraction admitted through the PACE channel in the treatment group (1).
- Correlation between regular and PACE admission in the treatment group (1).
- Fraction sitting the PSU entrance exam in the control group (1).
- Mean and variance of PSU entrance exam score among those who sit the exam in the control group (2).
- Fraction who enrolls in college in the control group (1).
- Average and mean 12^{th} grade GPA in the control group (2).
- For the sample of students who are admitted both through the PACE and the regular channel in the treatment group, fraction who enrolls through PACE (1).
- Correlation between believed PSU entrance exam score and enrollment (1).
- Correlation between PSU entrance exam score and enrollment (1).
- Correlation between believed and actual PSU entrance exam score (1).
- Mean and variance of believed marginal productivity of study hours in production of GPA (2).
- Mean and variance of believed marginal productivity of study hours in production of PSU entrance exam (2).
- Mean and variance of believed PSU entrance exam score (2).
- Mean and variance of believed GPA (2).
- For the sample of students in control schools: correlation between choice of sitting entrance exam and enrollment, correlation between hours of study and enrollment, correlation between hours of study and admission (3).
- For the sample of students in treated schools, correlation between the perceived distance from the cutoff (believed GPA minus believed cutoff) and the choice of sitting the entrance exam (1).
- For the sample of students in control schools, correlation between the believed PSU entrance exam score and the choice of sitting the entrance exam (1).

E Additional Identification Details

Subjective probability of a preferential admission (ξ_0^b, ξ_1^b). All arguments of this function (see equation (12)) are observed or governed by parameters that are separately identified: we elicited the perceived school cutoff, and, as discussed, we can separately identify the parameters of the perceived GPA production. But these arguments could correlate with unobservable determinants of choices, which would prevent identification of ξ_0^b and ξ_1^b. To mitigate this issue, we exploit the experiment. While students in both control and treated schools can form a belief about graduating in the top 15%, this belief coincides with the subjective probability of a preferential admission only for students in treated schools. In the control group, any correlation between the arguments of this subjective probability and behavior must be spurious. In the treatment group, it is both spurious and due to the subjective probability of preferential admission. *Differences* across treatment groups in how behavior varies with the arguments of this subjective probability help identify ξ_0^b and ξ_1^b. The treatment acts as a shock to the saliency of this belief that keeps preferences and ability constant.

We provide an intuition for why treatment effects identify the parameters of the subjective probability of a preferential admission using a simplified static version of the model. Students in control schools choose effort e_i to maximize the following utility function:

$$\underbrace{\alpha_0 + \alpha_{1k} e_i}_{\text{Achievement}} - \underbrace{\xi_1 e_i^2}_{\text{Effort cost}} + \underbrace{\gamma^R(z_i;\theta^R)e_i}_{\text{Subj pr regular adm}}$$

where the utility from college is set to 1, z_i is the argument of the subjective probability of a regular admissions, and the subscript k indicates a student type, which captures unobserved factors that can correlate with z_i.[53] From the first-order condition, the utility-maximizing effort choice is:

$$e_i^* = \frac{\alpha_{1k} + \gamma^R(z_i;\theta^R)}{2\xi_1} = \tilde{\alpha_{1k}} + \tilde{\gamma_k^R}(z_i;\theta^R)$$

where the tilde indicates division by $2\xi_1$, which we assume is identified.

Students in treated schools choose effort to maximize the following utility function:

$$\underbrace{\alpha_0 + \alpha_{1k} e_i}_{\text{Achievement}} - \underbrace{\xi_1 e_i^2}_{\text{Effort cost}} + \underbrace{\gamma_k^R(z_i;\theta^R)e_i}_{\text{Subj pr regular adm}} + \underbrace{\gamma^P(z_i;\theta^P)e_i}_{\text{Subj pr pref adm}}$$

where for simplicity we are assuming that a regular and a preferential admission are perceived as independent by students. From the first-order condition, the utility-

[53]In this simplified model, the type affects the coefficient of effort in the achievement function rather than the constant, as in equation (1). Thus, in this simplified model the chosen effort level depends on the type, like in the main model, which can give rise to spurious relationships between behavior and characteristics.

maximizing effort choice is:

$$e_i^* = \frac{\alpha_{1k} + \gamma^R(z_i;\theta^R) + \gamma^P(z_i;\theta^P)}{2\xi_1} = \tilde{\alpha_{1k}} + \tilde{\gamma^R}(z_i;\theta^R) + \tilde{\gamma^P}(z_i;\theta^P).$$

Consider two levels of z_i: z_i^H and z_i^L. Then, within each treatment group we have that:

$$E[e_i^*|z_H, T_i = 0] - E[e_i^*|z_L, T_i = 0] = \underbrace{\Delta E[\tilde{\alpha_{1k}}|T_i = 0]}_{\text{Spurious}} + \underbrace{\Delta\tilde{\gamma^R}(\theta^R)}_{\text{Governed by } \theta^R} \tag{17}$$

$$E[e_i^*|z_H, T_i = 1] - E[e_i^*|z_L, T_i = 1] = \underbrace{\Delta E[\tilde{\alpha_{1k}}|T_i = 1]}_{\text{Spurious}} + \underbrace{\Delta\tilde{\gamma^R}(\theta^R)}_{\text{Governed by } \theta^R} + \underbrace{\Delta\tilde{\gamma^P}(\theta^P)}_{\text{Governed by } \theta^P} \tag{18}$$

where Δ takes differences across z_i levels. By virtue of the randomization, $\Delta E[\tilde{\alpha_{1k}}|T_i = 0] = \Delta E[\tilde{\alpha_{1k}}|T_i = 1]$. Hence, the difference between (17) and (18), which is obtained from the data, is equal to $\Delta\tilde{\gamma^P}(\theta^P)$: it is only governed by the parameter of interest, because the spurious relationship cancels out.

Subjective probability of a regular admission (γ_0^b, γ_1^b). The argument of this probability is the expected entrance exam score (see equation (11)), and, as discussed, it is governed by parameters that are separately identified. Parameters γ_0^b and γ_1^b govern the choice of sitting the entrance exam as a function of the expected entrance exam score and can be identified by varying this expected score while keeping unobservables fixed. To do so, we rely on an exclusion restriction: we require a variable that affects the expected entrance exam score but not the type probability. The lagged test score does not directly enter the type probability (but it can correlate with unobservables through its correlation to the lagged grades that enter the type probability in equation (14)). Variation in lagged test scores conditional on lagged grades generates variation in expected PSU that is uncorrelated with unobservables and that identifies the parameters γ_0^b, γ_1^b. To validate this restriction, we compare the subjective probability of a regular admission implied by the estimated model with the subjective probability we elicited through a Likert scale, which we did not target in estimation. The model predicts that, on average, students perceive a regular admission as 56% likely, this probability is 59% in the data. Appendix Figure A9 shows that the model fits the entire distribution of this subjective probability reasonably well, giving us confidence in this restriction.

F Fieldwork Information

All the sampled schools agreed to participate in our study, thanks to the full support we received from the Chilean Ministry of Education, who called and sent letters to school principals encouraging them to participate. Our fieldworkers visited the schools several times and could survey all students who were present.

Students filled out questionnaires on paper, and teachers and school principals filled out questionnaires on tablets. The schools allowed us to administer our survey during class time. Our survey displaced one lecture. It took students approximately 50 minutes to complete the questionnaire. At the start of the data collection, fieldworkers explained to the students that for the first 20 minutes, they would be taking an achievement test, 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. At the 20-minute mark, fieldworkers told students to stop working on the achievement test and move on to the survey part of the questionnaire. If a student completed the achievement test before the 20 minutes were up, he or she was allowed to move on to the survey part of the questionnaire.

To limit the influence of any fieldworker, the instructions were printed on the first page of the survey, and we asked the fieldworkers to read them aloud. To further harmonize the data collection process across fieldworkers, we required them to fill out check-lists they had to submit to their supervisors. During the first 20 minutes, the fieldworkers acted as invigilators to ensure that students did not copy each other. To further avoid cheating behavior, we produced six different versions of the achievement test, where the tests differed in the question order. Moreover, to ensure that all students faced questions of increasing difficulty, questions were first assigned to three different difficulty categories (based on the difficulty index provided to us by the testing agencies and on extensive piloting of the questions on our target population), and the order of the questions was randomized within each category. At the start of the test, students were informed they would not have identical achievement tests.

Although we conducted our survey with the full support of the Ministry of Education, the data collection was not financed by the Ministry or any other public agency. Our surveys did not show any logo for any Government Ministry of Agency. Students were informed the data collection was being carried out by University College London, the Karta Initiative, and the Focus Data Collection Agency. The logos of these three institutions were printed on each page of the questionnaire.