College Access When Preparedness Matters: New Evidence from Large Advantages in College Admissions*

Michela M. Tincani[1], Fabian Kosse[2], and Enrico Miglino[3]

[1]University College London, IFS, CEPR & LEAP
[2]University of Würzburg & Institute on Behavior and Inequality (briq)
[3]University College London

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Abstract

Exploiting a randomized control trial and a dynamic structural model, we provide evidence on the impact of preferential college admissions in Chile on education outcomes. The college admission policy (PACE) targeted disadvantaged students scoring 1.5 standard deviations below regular college entrants on high school tests. We constructed a 9-year-long longitudinal dataset on 9,006 students linking detailed administrative records to survey data. We show that PACE increased first-year college enrollment by 3.1 percentage points and the effect shrank to 1.1 in the fifth year. The policy decreased the pre-college effort of students, likely due to belief biases about their absolute and relative ability. Using simulations from a dynamic structural model, we show that eliminating the pre-college belief biases would improve the college preparedness of college entrants. Our results demonstrate that expanding admission advantages to very disadvantaged populations can improve their college attainment, but college preparedness matters and it responds to incentives shortly before college.

1 Introduction

Young adults from better-off families are much more likely to attend college than those from worse-off families.[1] One policy response to this intergenerational inequality is to provide college admission advantages to students from disadvantaged contexts. Context-based admissions are gaining increasing attention, especially as admissions based on race or ethnicity are proving contentious (Arcidiacono and Lovenheim, 2016).

Most evidence on context-based admissions comes from programs designed to improve the opportunities of disadvantaged students to attend more selective colleges (Long, Saenz, and Tienda, 2010; Niu and Tienda, 2010; Kapor, 2020; Bleemer, 2021; Black, Denning, and Rothstein, 2023). Many of the disadvantaged students they target have sufficient academic preparedness to be admitted to some college.[2] Prior studies have concluded that context-based admissions improve the enrollment in selective colleges of such relatively well-prepared students and that the effects for the most part persist until graduation. An entirely open question is what impacts more extreme forms of admission advantages would have on the college enrollment and persistence of students further down the academic preparedness distribution. Answering it is necessary to build policy recommendations that extrapolate beyond the populations studied so far. Understanding how far context-based admissions can go while generating persistent educational gains for disadvantaged students is also the starting point for discussions about their optimal design.[3]

This paper answers this question in the context of Chile, which offers three advantages: a policy, called PACE (*Programa de Acompañamiento y Acceso Efectivo a la Educación Superior*), that provided unusually large advantages in admission, detailed longitudinal data from a transparent centralized admission system linkable to survey data, and successive governments willing to collaborate to experimentally evaluate the admission policy. PACE targets disadvantaged schools, and it offers students who graduate in the top 15 percent of their high school guaranteed admissions to colleges participating in the centralized admission system, eliminating the entrance exam score requirement.[4] These colleges offer five-year (and longer) programs of an academic nature, and the PACE admission offerings are guaranteed by an official agreement

[1]For example, in the United States children from families where at least one parent has attained higher education are 37 percentage points more likely to have a college degree than children from families where neither has. In the United Kingdom the figure is 40, in Chile and Australia 35, in Germany 26 (OECD.Stat).

[2]The same has been argued for race-based preferences in undergraduate admissions (e.g. Arcidiacono, Aucejo, Coate, and Hotz, 2014; Hinrichs, 2014; Machado, Reyes, and Riehl, 2023).

[3]Critics of preferential college admissions claim that they can induce disadvantaged students to pursue educational opportunities for which they are not academically prepared, leading to a larger dropout from higher education than would have occurred absent such policies, and harming the labor market prospects of these students. This is sometimes dubbed the "mismatch hypothesis" (Sander, 2004; Arcidiacono et al., 2011). This paper examines the persistence of the impacts on higher education enrollment but does not aim to test for the mismatch.

[4]Throughout the paper we describe the PACE policy as it was for our sample. Some changes to the PACE rules were recently introduced, but they did not affect the students in this study's sample.

between the government and the colleges.[5] Students in PACE high schools are considerably disadvantaged: they have 10^{th} grade standardized test scores that are 1.5 standard deviations below those of regular college entrants and 0.49 standard deviations below the OECD average, 77 percent attend vocational high school tracks, 61 percent are categorized as socioeconomically vulnerable by the government, their family income is half the median Chilean income, and their most common choice is to not attend any form of higher education (nearly 60 percent), followed by attending two-year vocational programs (nearly 30 percent). PACE expanded college access dramatically, and more extensively than the context-based admission policies most studied so far.[6]

We would expect PACE to increase the college admissions and enrollments of top performing students in targeted schools. But given the considerable level of disadvantage of the students and the academic nature of the college programs, it is ex-ante unclear how persistent any such impacts would be. Also ex-ante unclear is how PACE could affect students who are not top-performing in their school, and how it could affect students while they are still in high school, for example by inducing a response in teachers' focus of instruction or students' study effort. High school impacts could in turn matter for the college persistence of those induced to enter college and, ultimately, for the persistence of the college enrollment impacts.

To answer all these questions, we construct a new dataset that links administrative data to survey data we collected in schools. The dataset is longitudinal and follows $9,006$ targeted students for 9 years: from 9^{th} grade to five years after high school graduation. Thanks to high-quality administrative records, we observe standardized achievement measures, grades in school, students' socioeconomic and demographic characteristics, and the full path of education choices, from the type of high school to the higher education choices and persistence or graduation up to five years after leaving high school. We further collected survey data in schools, which we linked to the administrative data through student, classroom and school identifiers. Our survey data include information on students' effort and standardized achievement test scores in the last high school year, on students' beliefs about their relative and absolute ability, on teacher effort, focus of instruction and grading practices, and on school inputs such as remedial and college entrance exam classes.

The dataset is one of the most comprehensive datasets constructed to date on the transition from high school to higher education of a population targeted by an admission policy. Compared

[5]Of the 39 institutions participating in the centralized admission system, 29 signed the agreement and offered PACE slots (Figure A2 shows the quality distribution of PACE seats and of all regular seats offered through the centralized admission system). Higher-education institutions outside the system do not have minimum admission requirements and many provide vocational and shorter degrees.

[6]For comparison, students targeted by the Texas Top Tep (TTT) and Californian Eligibility in the Local Context (ELC) context-based policies on average score comparably or better than other entrance-exam-test takers in Texas and other college applicants in California (already positively selected populations); around 20 percent of those in schools targeted by the TTT are socioeconomically vulnerable (i.e., eligible for reduced or free school meal), and the median income of those around eligibility cutoffs in schools targeted by the ELC is 90% of the median Californian income. See section 3.4.

to existing studies of admission policies, it contains new variables on high school students and is novel in linking information from multiple actors in schools. For example, virtually nothing is known about the beliefs of high school students targeted by admission policies, or the focus of instruction and effort of their teachers.[7] The dataset, therefore, gives us the opportunity to examine mechanisms of admission policy impacts in the pre-college phase that could not be precisely analyzed before. A further innovation of this study is that we identify policy impacts through the randomization of the policy that took place in 2016 with the explicit purpose of evaluation.[8] Thus, the unique design of PACE, the dataset, and the randomized experiment provide the ideal setting to understand the effects of large admission advantages for the first time.

The first set of findings is that PACE increased the college admissions and enrollments of disadvantaged students by 4.1 and 3.1 percentage points (p.p.), corresponding to a 36 percent increase in admissions and enrollments compared to the control group. Considering the high level of disadvantage of the targeted students, the government considered these effects satisfactory, and chose to keep the policy in place.[9] The effects are concentrated among students who in 10^{th} grade, before the experiment started, were in the top 15 percent of their high school, while students in the bottom 85 percent experienced no significant change in college admissions and enrollments. The enrollment effects, however, decreased significantly over time: five years after leaving high school, the PACE impacts on continuous enrollment or graduation from college were 1.1 percentage points, around a third of the effects four years earlier.[10] For comparison, context-based admission programs such as the Texas Top Ten and the Californian Eligibility in the Local Context achieved substantially more persistent impacts on the relatively better-prepared populations they targeted.[11]

[7] Kapor, 2020 uses survey data on applications collected in the schools targeted by the Texas Top Ten (TTT). Golightly, 2019 uses administrative data on high school performance of students targeted by the TTT. Akhtari, Bau, and Laliberte, 2022 use administrative data on SAT scores, high school performance and applications before and after bans on race-based affirmative action in the United States, and survey data on students' time on homework and whether they received guidance from a counsellor.

[8] Paper co-author Michela Tincani is leading the experimental evaluation of PACE together with the Ministry of Education and the Ministry of Finance, and co-authored several policy reports, including those officially released by the Ministries (Cooper, Guevara, Rivera, Sanhueza, and Tincani, 2019; Cooper, Sanhueza, and Tincani, 2020; Cooper, Guevara, Kinder, Rivera, Sanhueza, and Tincani, 2022).

[9] Following a presentation by paper co-author Michela Tincani and her collaborators in the Ministries of Education and Finance to the Budget Office of Chile in May of 2019, the then right-leaning Piñera government chose to keep PACE in place, as these early results were considered a success. The policy was first introduced by the left-leaning Bachelet government.

[10] The enrollment effects at five years are significantly positive in the sub-sample of top-performing students, to which the policy was targeted, but significantly lower than the impacts on first-year enrollment in this group too.

[11] The impacts of the Texas Top Ten on enrollment or graduation six years after leaving high school were three-quarters of those in the first year (Black, Denning, and Rothstein, 2023). The near-threshold students enrolling into a selective college under the Eligibility in the Local Context had a 75 percent probability of graduating (Bleemer, 2021), compared to a 58 percent probability of persistence or graduation at five years among all those from the top 15 percent of PACE schools who entered college (which arguably includes better prepared students than the near-threshold ones).

The second set of findings is that PACE had a negative impact of 0.1 standard deviations on study effort and achievement in high school, as measured through our survey. The result is confirmed using administrative data: PACE decreased GPA in the core subjects (e.g. Mathematics, language) in the last high school year. Crucially, PACE reduced precisely the dimensions of pre-college human capital that predict persistence in college in the very disadvantaged group we study. Therefore, the endogenous response of pre-college effort and achievement could have contributed to the waning enrollment impacts of PACE by affecting the college preparedness of college entrants under PACE.

To understand the PACE impacts in schools, we analyzed all the mechanisms we specified in the pre-analysis plan (students' response to incentives, teacher grading, teachers' effort and focus of instruction, changes in school inputs), and an additional one motivated by the finding that the impacts are negative (a reduction in the perceived returns to college). We find evidence in support of students' response to incentives.[12] The evidence is most consistent with students responding to perceived, rather than actual incentives: by linking our survey data on believed outcomes to actual outcomes, we document that most students have large over-optimism about their absolute and relative (within-school) ability, likely mis-perceiving their distance from regular and preferential admission cutoffs. Consistent with the widespread belief biases, the negative impacts on pre-college effort and achievement are widespread too, unlike what would be expected under rational expectations (see e.g. Bodoh-Creed and Hickman, 2018), where the sign of the effect should vary across students depending on their distance from regular and preferential admission cutoffs. The evidence, therefore, suggests many behave as if without PACE an admission is within reach, and with PACE it is guaranteed.

The reduced-form findings so far suggest that college preparedness reduced as an effect of the change in admission rules. This is a novel finding suggesting a potential role for school interventions. But without more structure, it is impossible to measure whether the policy impacts on pre-college effort mattered for the persistence of the PACE impacts: the latter depends on who self-selects into college under PACE, which we do not observe in the data under counterfactual effort responses. For this reason, we develop a dynamic structural model of pre-college effort, entrance-exam taking, admissions and enrollments, with and without PACE, that delivers the college preparedness of college entrants as an endogenous outcome. The model

[12] We are confident that the GPA and achievement reductions are not the result of a change in the ability composition of students in the treatment group, which could occur when students strategically select into high schools offering admission advantages. First, the announcement that a school was in PACE was made after the deadline for school enrollment in the 11^{th} grade, and as students need to be in a PACE schools for the last two high school years to benefit from the percent rule, they did not have an incentive to change school at a later time either. Second, the student characteristics are balanced across treatment groups (Table 1), indicating lack of strategic high school selection. Third, we further analyzed school transitions in and out of PACE schools around the time of our experiment and we find no systematic relation between baseline test scores and entering or leaving a PACE school (Supplementary Table G2). Finally, strategic high school enrollment typically induces more advantaged students to enter schools where preferential admission policies are in place, leading to an observed *increase*, not decrease, in GPA and test scores.

allows us to simulate the impacts of hypothetical school interventions designed to improve the college preparedness of college entrants under large admission advantages, which is of great policy relevance.

In the model, students have heterogeneous preferences for college that vary with their observed and unobserved characteristics, and when choosing pre-college effort, they anticipate the impact it will have on their perceived admission likelihoods. We can relax rational expectations thanks to the high-quality measures of beliefs we collected, which, we show, can independently predict high-stake outcomes up to five years after we administered the survey.[13] The perceived likelihood of a regular admission depends on the perceived entrance exam score, and the perceived likelihood of a preferential admission depends on the perceived GPA rank. Both depend on the choice of effort. Rank depends also on the effort of school peers, a strategic interaction.[14] Informed by survey evidence suggesting that students do not expect pre-college effort to affect college persistence, in the model the payoff from college enrollment does not depend on pre-college effort. The model can successfully replicate the experimental findings, including those that would be hard to fit with models that assume rational expectations.

The model allows us to quantify the magnitude of the perceived incentive effects of PACE. We quantify that students believe the policy reduced the returns to effort considerably, by 77%. They believe that one study hour per week increases the admission likelihood by 6.9 p.p. absent PACE, and by only 1.6 p.p. when PACE is introduced.

To understand whether school interventions that affect pre-college effort could improve the college preparedness of college entrants, we perform two counterfactual policy simulations. First, we correct students' over-optimistic beliefs about the entrance exam score and the GPA rank. We assign to students rational expectations, and solve for the Bayesian Nash Equilibrium of the tournament game taking place in PACE schools.[15] We find that eliminating belief biases in high school affects both the baseline ability of the students who self-select into college (the *selection* channel), and the pre-college effort they exerted while in high school (the *effort* channel), both of which predict persistence. Since over-optimism leads high-ability students to incorrectly perceive an admission as guaranteed and under-provide effort, and low-ability

[13] The predictive validity exercise is more nuanced than simply showing that our belief measures can independently predict outcomes far in the future. We find that the belief measures behave the way they should if they were capturing what we expect them to capture. The entrance exam score affects the admission likelihood in both the treatment and control groups, and accordingly, we find that the belief about the entrance exam independently predicts entrance exam taking, admission, enrollment and persistence up to five years later in both groups. In contrast, the within-school rank strongly affects the admission likelihood in the treatment group but not in the control group, and accordingly, we find that the belief about the rank independently predicts those same outcomes in the treatment group, but not in the control group.

[14] To capture such strategic interaction in a setting with biased beliefs, we implement an established approach from the behavioral game theory literature. We assume students choose effort to best respond to what they perceive the within-school admission cutoff to be, which we have elicited, without imposing equilibrium beliefs (see e.g. Stahl and Wilson, 1995; Costa-Gomes and Zauner, 2003; Camerer, Ho, and Chong, 2004; Costa-Gomes and Crawford, 2006; Crawford and Iriberri, 2007).

[15] We find that at the estimated parameter values, the BNE is unique.

students to incorrectly perceive it as within reach and over-provide effort, eliminating over-optimism increases the effort of high-ability students and decreases that of low-ability students. As effort affects the admission credentials, eliminating the effort under- and over-provisions increases the admissions of the high ability and decreases those of the low ability, improving the ability of college entrants by 0.08 standard deviations according to 10^{th} grade test scores, and their pre-college effort by 0.31 study hours per week (or 0.60 standard deviations). Correcting pre-college belief errors about entrance exam scores and GPA rank, therefore, could improve the college preparedness of those who self-select into college under large admission advantages.

In the second counterfactual experiment, we consider an alternative school intervention because some policymakers consider providing rank information controversial. We simulate the impacts of informing high school students in PACE schools of the importance of pre-college effort for persistence in college. We assign to students payoffs from enrolling in college that depend on pre-college effort to the extent it predicts college persistence. Since students are forward-looking, this counterfactual changes the continuation value of effort in high school. We find that this intervention is less effective than correcting belief errors. It would improve the pre-college effort of college entrants by only 0.09 study hours per week (or 0.18 standard deviations), and it would not change the baseline ability of college entrants. Given the widespread over-optimism about admission chances, this intervention would increase also the pre-college effort of those who end up not being admitted, with ambiguous welfare implications.

2 Contributions to the Literature

This paper makes three main contributions. First, it provides the first evidence of the impacts of context-based admissions targeted at very disadvantaged students who score very low on high school standardized tests, and finds that college preparedness in this group is elastic to investments and incentives shortly before college. We study education outcomes in high school and up to five years after leaving high school, and extend the previous literature by studying these outcomes jointly.[16] This allows us to show that the policy negatively affected precisely the dimensions of pre-college human capital that matter for college persistence five years later. It increased college admissions and enrollments, but we found less persistent enrollment impacts

[16]For example, Kapor, 2020 studies impacts of the Texas Top Ten on college attainment abstracting from pre-college achievement, and Golightly, 2019 studies its impacts on pre-college achievement abstracting from college attainment. For other studies of the impacts of percent plans on college enrollment and persistence, see also Long, Saenz, and Tienda, 2010, Niu and Tienda, 2010, Daugherty, Martorell, and McFarlin, 2014. In the context of race- or ethnicity-based admission policies, a large literature studies impacts on college attainment, abstracting from impacts on pre-college achievement (see the review in Arcidiacono, Lovenheim, and Zhu, 2015). For evidence outside of the United States see, for example, Bagde, Epple, and Taylor, 2016, who estimate impacts of caste-based affirmative action in India on enrollment in engineering colleges and graduation at the end of the fourth year. The literature on pre-college impacts of affirmative action is smaller. Akhtari, Bau, and Laliberte, 2022 study impacts on pre-college academic performance of affirmative action bans in the United States, abstracting from college enrollment and later outcomes.

compared to previous studies of context-based admissions focusing on relatively better-prepared students (Black, Denning, and Rothstein, 2023; Bleemer, 2021). The results highlight the importance of college preparedness among very disadvantaged populations, and its nuanced role. An old argument against admission advantages is that they can lead under-prepared students to enter college (Sander, 2004; Arcidiacono et al., 2011; Ichino, Rustichini, and Zanella, 2022). Our results demonstrate that college preparedness can be elastic to investments in the last high school years, which themselves respond endogenously to admission rules. An important policy implication is that expanding admission advantages to very disadvantaged populations could lead to more persistent impacts if combined with school interventions shortly before college designed to improve the preparedness of college entrants.

Second, this paper provides the first experimental evidence of the impacts of admission policies. Previous studies examined the impacts of college admission on college attainment around admission cutoffs (e.g. Niu and Tienda, 2010; Zimmerman, 2014; Goodman, Hurwitz, and Smith, 2017), estimating local average effects. Thanks to our experimental research design, we can, for the first time, extrapolate away from admission cutoffs without relying on structural model assumptions (Kapor, 2020; Bleemer, 2021; Otero, Barahona, and Dobbin, 2021), or on the parallel trend assumptions of difference-in-differences designs (Black, Denning, and Rothstein, 2023; Bleemer, 2022).[17] We show that the positive impacts on college enrollment are concentrated among those who at baseline were in the top 15 percent of their school, but the negative impacts on pre-college effort and achievement were more widespread. Leveraging on the survey data we collected in schools, we find that a plausible explanation is that students responded to incentives under biased beliefs about their absolute and relative ability, a novel finding that suggests new avenues for the design of large admission advantages.

Third, this paper contributes to the structural literature modelling admission policies (e.g. Arcidiacono, 2005, Kapor, 2020, Otero, Barahona, and Dobbin, 2021) by endogenizing pre-college effort.[18] Building on the results from the survey data, the model relaxes rational expectations assumptions, thus also contributing to the literature on dynamic models of education choices under information frictions. The model extends standard dynamic choice models (e.g. Keane and Wolpin, 1997; Behrman, Tincani, Todd, and Wolpin, 2016) by simultaneously allowing for a subjective value function based on the perceived evolution of the state space, and a

[17]In Texas, the ban on race-based affirmative action and the introduction of the Texas top Ten were nearly simultaneous, making it difficult to isolate the impacts of one of these two policies using difference-in-difference strategies. Parallel trend assumptions are not always satisfied, as shown in Akhtari, Bau, and Laliberte, 2022 for the case of states that did and did not ban affirmative action following the Grutter v. Bollinger court ruling.

[18]To the best of our knowledge, so far this has only been done in Bodoh-Creed and Hickman, 2018, whose structural model includes both pre-college effort and college attainment as endogenous outcomes, and where pre-college effort affects admission likelihoods. The paper estimates the model with data from the United States, assuming that minority and majority students face different admission cutoffs by virtue of extant affirmative action policies. The study does not exploit changes in admission policies to identify their impacts. In contrast, we exploit RCT-based causal estimates of the impacts of PACE to estimate our model. The impacts on pre-college effort in our study, therefore, are not driven by model assumptions but by experimental findings.

true evolution of the state space that follows objective admission likelihoods. It contributes to the literature estimating dynamic models using data on both perceived and actual outcomes. Most relevant to this paper are Stinebrickner and Stinebrickner, 2014 and Arcidiacono, Hotz, Maurel, and Romano, 2020, who model information frictions during college.[19] In contrast, we model information frictions *before* college, and show that in a dynamic setting they can affect later high-stake outcomes such as college enrollment and college preparedness, even when they are short-lived.[20] The model provides entirely novel estimates of the incentive effects of admission advantages as perceived by the high school students they target, and of the likely impacts of informational interventions in schools on the college preparedness of college entrants under large admission advantages.[21]

3 Context, Randomization and Data

3.1 Context and PACE Policy

In this section we describe the context and policy as they were for our sample.

Definition of *selective college.* With selective college we refer to a college that participates in the centralized admission system (*Sistema Único de Admisión*), not to a college that has high admission requirements, which is the meaning attributed to selective college in other countries such as the United States. We refer to these colleges as selective colleges or, simply, colleges. To distinguish them from the colleges that do not participate in the centralized admission system, we use the term non-selective college to refer to the latter.[22]

[19]Other relevant papers include Bobba and Frisancho, 2019, who use belief and outcome data to estimate a model of the transition from middle to high school; Kapor, Neilson, and Zimmerman, 2020, who use data on beliefs and actual outcomes to estimate a static equilibrium model of school choice in a centralized school admission system; and d'Haultfoeuille, Gaillac, and Maurel, 2021, who develop a test for rational expectations that can be applied to data on perceived and actual outcomes that cannot be matched. Using data on choices and beliefs over the consequences of such choices, Giustinelli, 2016 estimates a model of parent-child choice of high school. Using data on beliefs and expected future choices (but not on actual outcomes), Van der Klaauw, 2012 and Delavande and Zafar, 2019 develop and estimate dynamic structural models of teacher careers and of university choice that do not impose rational expectations. See also Arcidiacono, Aucejo, Maurel, and Ransom, 2016, who estimate (without using belief data) a dynamic structural model of schooling and work decisions where individuals have imperfect information about their schooling ability and labor market productivity.

[20]Boneva and Rauh, 2020 collected survey data in British high schools and showed that first-generation students perceive lower returns to college than those with parents who attended college.

[21]Mounting evidence shows that providing information about absolute and relative ability can successfully and cheaply correct belief errors and choices (Bobba and Frisancho, 2019; Azmat, Bagues, Cabrales, and Iriberri, 2019; Hakimov, Schmacker, and Terrier, 2022). Therefore, the simulations correcting those beliefs can be interpreted as an approximation to the likely effects of best-case informational interventions. See also Hastings, Neilson, and Zimmerman, 2015, who find that a cheap intervention providing information about wages of graduates from different majors in Chile changed students' major choice.

[22]Selective 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 most provide vocational and shorter degrees.

Regular channel admissions. Students wishing to go to a selective 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.

PACE. In line with global statistics, college enrollment in Chile is unequal across socioeconomic lines. Students from families in the top income quintile are over three times more likely to enroll than students from families in the bottom income quintile (Figure G1 of the supplementary material). PACE was introduced to increase college admissions among disadvantaged students. The government selected the schools to be targeted by PACE using the school-level vulnerability index (*Indice de Vulnerabilidad Escolar*), based on students' socioeconomic characteristics, to indentify schools serving underprivileged students.

Students in high schools participating in PACE can apply to a selective college through the regular channel, like any other student in the country. Moreover, they receive a guaranteed admission to a selective college, that can be used only in the year immediately after graduating from high school, if they satisfy three conditions. First, the grade point average in grades 9 to 12 must be in the top 15% of the high-school cohort.[23] Second, like in the Texas and California percent plans (Horn and Flores, 2003), the student must take the entrance exam, even though the score does not affect the likelihood of obtaining a PACE admission. When students decide whether to take the exam, they have not yet been told whether they have graduated in the top 15% of their school. Third, the student must attend the PACE high school continuously for the last two high-school years (eleventh and twelfth grade), and participate in light-touch orientation classes (two hours per month on average) that are offered to all students in PACE high schools.[24]

Other features of PACE include the following. i) Unlike the percent plans in Texas and California (see Table 1 in Horn and Flores, 2015), there are no coursework requirements in addition to graduating in the top 15%. ii) Optional tutoring sessions in college are available to those who enroll via PACE. iii) PACE college seats are supernumerary: they do not replace regular seats but are offered in addition to them. Therefore, the introduction of PACE did not make it mechanically harder to obtain regular admission. iv) PACE seats span the same majors as regular seats and are of similar quality, as measured by the average entrance exam score of regular entrants into each college-major pair (Figure A2). v) A student can obtain

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

[24] The Texas top ten percent plan shares this feature. The PACE orientation classes cover the college application process and study techniques and often replace orientation classes already offered by the schools (MinEduc, 2018).

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

3.2 Randomization and Balancing Tests

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

When a school first enters PACE, only the cohort of eleventh graders is entered into the program. The randomized expansion concerned the cohort who started eleventh grade in March 2016. Before starting the school year, students who were enrolled in schools randomly selected to be treated were informed their school was in the PACE program. This announcement was made after the school enrollment deadline; thus, we did not observe strategic selection into high schools (see footnote 12). The control schools were not entered into the PACE program; they were not promised participation. Figure A1 illustrates the timeline. Grades in the first two high-school years (9 and 10) were already determined when students in treated schools were informed they were in a PACE school. But students who wished to affect their four-year GPA average had two school years to do so.

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

3.3 Data Construction

Table 2 lists the administrative and primary data sources. We linked them through unique student, classroom and school identifiers and built a longitudinal dataset that follows $9,006$ students for nine years, from ninth grade to five years after leaving high school.

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

Table 1: Sample Balance Across Treatment and Control Groups

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

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

and PACE channel admissions, enrollments and persistence or graduation up to five years after high school graduation. To gain insights on outside options, we collected administrative data on enrollments and persistence or graduation up to five years after leaving high school in all higher education programs outside of selective colleges. These are vocational programs (typically lasting up to two years), and academic programs in mostly private non-selective colleges, which do not participate in the centralized admission system.[25]

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

[25]See Kapor, Karnani, and Neilson, 2022 for a description of these off-platform options.

Table 2: Overview of Data

Dataset	Variables	Collected	Source
1. *SIMCE*	Achievement test scores, background characteristics	Grade 10	Admin
2. *SEP*	Very-low-SES classification (*Prioritario* student)	Grade 10	Admin
3. School records 1	High-school enrollment	Grades 9-12	Admin
4. Student survey	Study effort, beliefs about self and others	Grade 12	Primary
5. Teacher survey	Effort and focus of instruction of Mathematics and language teachers	Grade 12	Primary
6. School-principal survey	Support classes, assessment methods, classroom formation	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	Admin
9. Higher education records	Entrance exam (PSU) scores, applications, admissions, enrollments and graduation or persistence at five years in selective colleges via regular channel, seat selectivity, enrollments and graduation or persistence at five years in vocational higher-education institutions and non-selective colleges	Years 1-5 after high school graduation	Admin
10. PACE program records	Allocation of PACE seats in selective colleges, applications, admissions, enrollments and graduation or persistence via PACE channel, seat selectivity	Years 1-5 after high school graduation	Admin

Note. – SIMCE: *Sistema Nacional de Evaluación de Resultados de Aprendizaje*, SEP: *Subvención Escolar Preferencial.*

high schools by Behrman et al., 2015 and Todd and Wolpin, 2018, complemented with questions on entrance exam preparation. Third, we elicited subjective beliefs about future outcomes (i.e., college graduation and wages) and returns to effort (i.e., the productivity of effort for entrance exam scores and GPA). Finally, we surveyed mathematics and Spanish teachers, and school principals, to obtain information on the policy response of schools.

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

3.4 Descriptive Analysis

We now describe the disadvantaged students targeted by PACE, and their higher education choices absent preferential admissions.

Fact 1: Students targeted by PACE score substantially worse on high school standardized tests than regular entrants in selective colleges, and come from poorer households. Figure 1 shows the distribution of standardized tests scores in 10^{th} grade among students targeted by PACE and among regular college entrants, standardized in the population of 10^{th} graders. Students in targeted schools score 1.47 standard deviations below regular entrants on average. Their median score corresponds to the fourth percentile of scores among regular entrants. Even those who graduate in the top 15% of targeted schools score substantially worse than regular college entrants. They score 0.88 standard deviations below regular entrants on average. Their median score corresponds to the fourteenth percentile of scores among regular entrants. For reference, we draw the average high school standardized test scores in OECD countries: the majority of targeted students score below the OECD mean, the majority of regular entrants score above the OECD mean.

Table A2 shows that students in targeted schools are substantially more disadvantaged than the average Chilean student along several dimensions of socioeconomic status, for example, their family income is half that of the average Chilean student. Family income in this group is 53% of the median household income in Chile (54% if focusing on students graduating in the top 15% of targeted schools), and 31% of the family income of regular entrants (32% if focusing on top-graduating students), whose average family income of CLP 904,354 per month is 70% above the median Chilean income.

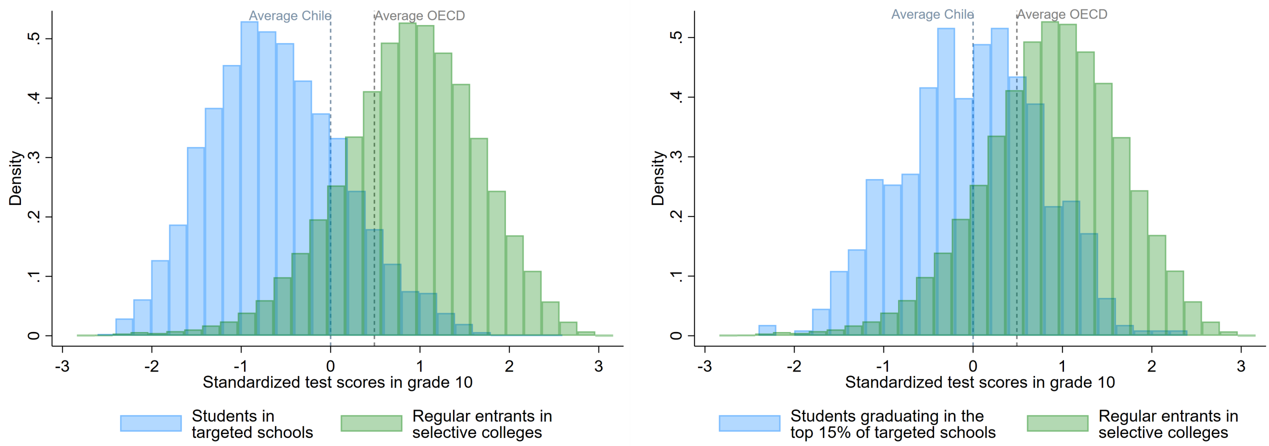

Figure 1: Distributions of standardized test scores in 10^{th} grade. Test scores are standardized in the population of 10^{th} grade students in 2015. *Notes:* Targeted students are from the control schools. Every bar represents 0.20 standard deviations. The average score in the OECD is calculated using PISA scores, re-scaled to be comparable to the SIMCE scores. Details of the re-scaling can be found in Section G.3 of the Supplementary Material.

For comparison, the students in California around the Eligibility in the Local Context (ELC) preferential admission cutoff have family incomes that are 90% of the median Californian income (Bleemer, 2021, Table 1). High school standardized test scores of these students are not reported, but their entrance exam (SAT) scores are above the average score among all college applicants (Bleemer, 2021, Table 1), which is a positively selected population.[26] Of the students targeted by the Texas Top Ten (TTT) policy, $22 - 23\%$ are eligible for free or reduced school meals (Black, Denning, and Rothstein, 2023, Table 1). In contrast, 61% of the students targeted by PACE are eligible for welfare programs due to their extremely vulnerable socioeconomic circumstances (*Alumno Prioritario*). The students in all schools targeted by the TTT are representative of all SAT test takers in Texas, and those induced to enroll in a more selective college by the policy have entrance exam scores corresponding to the 89^{th} statewide percentile. Therefore, the targeted students score favourably within the already positively selected population of Texan SAT test takers. It should be clear from these statistics that PACE targets a substantially more disadvantaged population than the two most well-known context-based programs in the United States.

Fact 2: Only few students targeted by PACE attend college absent the policy. The most common outside option is not enrolling in any higher-education institution. Among those who attend higher education, the most common outside option is attending vocational programs, followed by attending non-selective (off-platform) colleges. Table 3 describes the educational choices of the typical students targeted by PACE absent PACE, i.e., the choices of students in the control group. Around two thirds of students take the college entrance exam (first row of Table 3), which aligns nicely with our survey data, where 63% report preparing for it. Even students with very low admission likelihoods prepare for and take the entrance exam (Figure A4). But, as the second row of the table shows, exam scores are well below the national average (-0.6 standard deviations). Upon observing their exam scores only 21.0% apply to college (third row). 11.4% of students are admitted and 8.5% enroll in college the first year after high school graduation.[27] Among students who graduate in the top 15% of schools targeted by PACE (Panel B in the Table), 90% take the entrance exam; their scores are 0.21 standard deviations below the average test taker's. Upon observing their score, just over half of those who took the exam apply to selective colleges. A minority of high school students graduating in the top 15%, 28.7%, enroll in college the first year after high

[26] The most likely ELC compliers were near- or above-threshold students from schools with below-median SAT scores. Within this group, incomes of near-threshold students were around 6.5% above the Californian median income as per Table 3 in Bleemer, 2021. Regarding SAT scores, students near the ELC cutoff score 137 points above the average Californian applicant. Among students near the eligibility cutoff from below-median SAT score schools, SAT scores were 158 SAT points below the average applicant (Bleemer, 2021, Table 3). Results for the SAT in standard deviations are not reported.

[27] For the students in this study the PACE slot could only be used in the year immediately after high school graduation. Therefore, we do not examine PACE impacts on later first-year enrollments.

school graduation. We construct a variable capturing continuous enrollment or graduation in a selective college five years since first enrolling, which is necessary for on-time graduation.[28] Panel A of Table 3 shows that 58 percent (i.e., $\frac{0.049}{0.085}$) of the students who enroll in the first year are still continuously enrolled or have graduated after five years. Panel B shows that this figure is similar ($\frac{0.182}{0.287}$ = 63 percent) in the sample of students who graduate in the top 15% of their school.

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

	Mean	Std. Deviation	N
	(1)	(2)	(3)
A. All Students			
Took college entrance exam	0.655	0.475	4,231
College entrance exam score | took exam	-0.602	0.611	2,773
Applied to college	0.210	0.407	4,231
Admitted to college	0.114	0.318	4,231
Enrolled in college	0.085	0.279	4,231
Still enrolled or graduated from college five years later	0.049	0.217	4,231
Enrolled in vocational institution	0.270	0.444	4,231
Enrolled in non-selective (off-platform) college	0.061	0.238	4,231
B. Students Graduating in Top 15%			
Took college entrance exam	0.901	0.299	628
College entrance exam score | took exam	-0.214	0.641	566
Applied to college	0.481	0.500	628
Admitted to college	0.364	0.481	628
Enrolled in college	0.287	0.453	628
Still enrolled or graduated from college five years later	0.182	0.386	628
Enrolled in vocational institution	0.250	0.433	628
Enrolled in non-selective (off-platform) college	0.118	0.323	628

Note. – Sample of students enrolled in the 64 control schools. The standardized test scores in 10^{th} grade is measured in standard deviations of test scores in the population of 10^{th} graders. The college entrance exam score is designed to have mean 500 and standard deviation 110 among all exam takers, we report the standardized score. A student is coded as being enrolled or having graduated in the 5^{th} college year if he/she enrolled in the first year and stayed continuously enrolled every year up until and including year 5, or if he/she enrolled in the first year and graduated in a year prior to year 5.

Panel A shows that, absent the policy, 27% of students in targeted schools enroll in vocational higher education programs, 6.1% in non-selective colleges, and 58.4% do not enroll in higher education. Panel B shows that, absent the policy, among the top performing students in targeted schools, 25.0% enroll in vocational higher education programs, 11.8% in non-selective colleges, and 34.5% do not enroll in higher education.[29]

[28]Theoretically, it could be possible for a student to take a one-year gap from a selective college, re-enroll again, and graduate in time. But this is highly unlikely in practice. Graduation refers to graduation in or before 2021 (the fourth year) as graduation data for 2022 (the fifth year) is not yet available. The Ministry will make it available during 2023.

[29]For comparison, 88.9% of the students around the ELC cutoff in California attended college, 3.9% community college, and only 7.2% did not enroll (Bleemer, 2021 Table 1). Of the students in schools targeted by the TTT in Texas, absent TTT 25% enrolled in a 4-year college, 32% in a community college, and the remaining 43% did not enroll in college in Texas (Black, Denning, and Rothstein, 2023 Table 1). Among TTT compliers, absent TTT 49% enrolled in a 4-year college, 18% in a community college, and the remaining 35% did not enroll in college in Texas (Black, Denning, and Rothstein, 2023 Table 2). Therefore, the students targeted by PACE are less likely to enroll in college absent preferential admissions than those targeted by the two most well-known context-based programs in the United States.

4 Experimental Policy Evaluation

To identify the policy impacts, we exploit the randomized assignment of schools to PACE, and estimate the following linear regression model:

$$Y_{is} = \alpha + \beta T_s + \lambda X_i + \eta_{is}, \tag{1}$$

where Y_{is} is the outcome of student i in school s, T_s is the treatment status of school s, and X_i is a vector of student i's baseline characteristics. The parameter of interest is β. The standard errors are clustered at the school level.

4.1 Findings

Experimental Finding 1: PACE increased college admissions and enrollments, but the enrollment effects decreased substantially over time. Figure 2 shows that students in schools randomly assigned to the treatment are 4.1 percentage points (p.p.) more likely to be admitted to college and 3.1 p.p more likely to enroll than students in control schools. These effects correspond to a 36% increase compared to admissions and enrollments in the control group. The enrollment effect tapers off over time. The effect on continuous enrollment in the fifth year or graduation by such time (which is an upper bound for the effect on on-time graduation) is 1.1 p.p. (p=0.140), corresponding to a 23% increase compared to the control group, and it is significantly different (p=0.001) from the treatment effect on first-year enrollments.

The enrollment effects are concentrated among students who, at baseline, were in the top 15% of their school according to GPA in grades 9 and 10; those in the bottom 85% experienced no change in their college enrollment (as shown in Appendix Tables A6 and A7). Among top-performing students, PACE increased college applications, admissions, and first-year enrollments, and the college enrollment impacts are still significant and positive in the fifth year since leaving high school, but significantly and substantially smaller than the impacts in the first year (Tables A4 and A6). Table A6 also shows that PACE lowered the enrollment of top-performing students in the outside options (vocational institutes and non-selective colleges), and had no impacts on first-year enrollments in higher education overall, nor on continuous enrollment in or graduation from higher education after five years.

For comparison, the Texas Top ten policy increased by 5.3 percentage points the likelihood that top-performing students from schools that do not normally send their graduates to selective colleges (the most likely compliers) enroll in the selective UT Austin, and by 3.9 percentage points the likelihood that they graduate from UT Austin within 6 years (Black, Denning, and Rothstein, 2023, Table 3). The effect after six years is 74% of the effect in the first year. In contrast, among top-performing PACE students, the treatment effect on continuous enrollment or graduation after five years is 45% of the enrollment effect in the first year.[30]

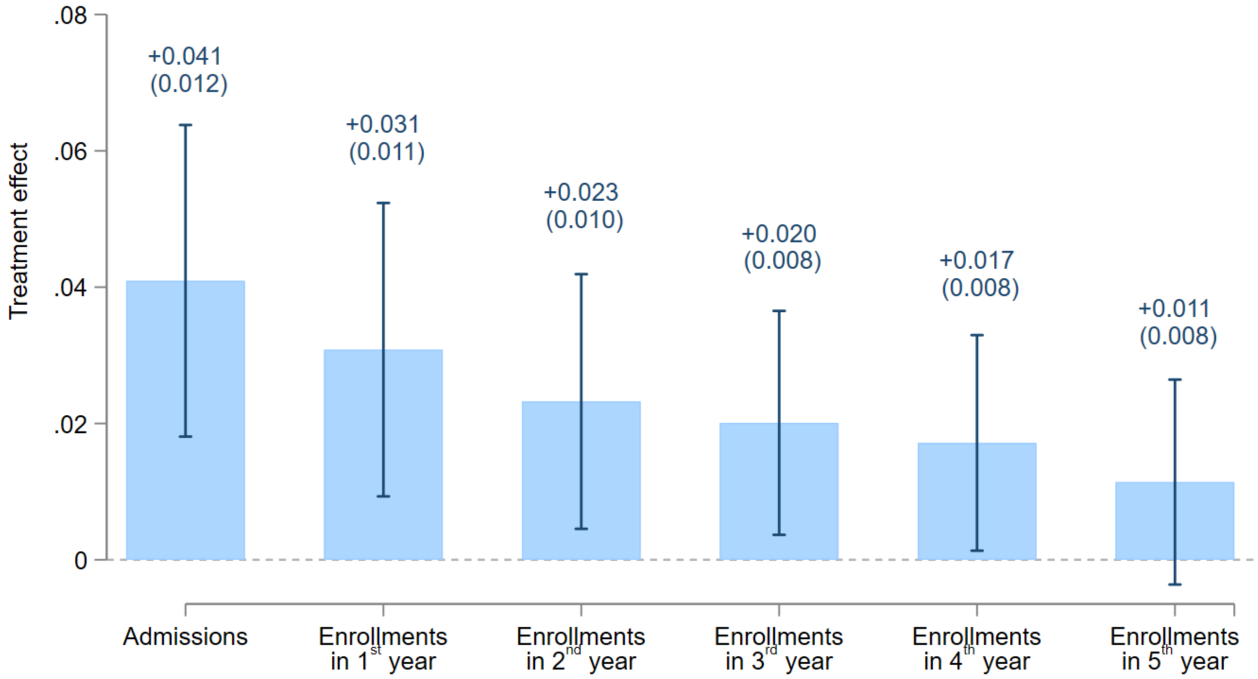

Figure 2: Effects of PACE on admissions and on enrollment or graduation of targeted students. The Figure reports OLS estimates from the estimation of parameter β in equation (1). The controls are: gender, age, indicator for very-low-SES student, baseline SIMCE test score, never failed a grade, and high school track (academic or vocational). The standard errors clustered at school level are reported in parenthesis, and the 95% confidence intervals constructed from them are shown. The enrollment variables capture continuous enrollment or graduation: a student is coded as enrolled (or as having graduated) in the t^{th} college year if he/she enrolled in the first year and has been continuously enrolled every year up until and including year t, or if he/she enrolled in the first year and has graduated in a year prior to t.

Experimental Finding 2: PACE lowered study effort and achievement before college. Given the decreasing impacts of PACE on college enrollment (Experimental Finding 1), we examine whether PACE had any impacts on pre-college academic preparedness. Columns (1) and (2) of Table 4 present results on the pre-specified outcomes. Students in treated schools perform 10% of a standard deviation worse than students in control schools on the standardized achievement test we administered. Column (2) shows that the treatment had a negative average effect on study effort of 9% of a standard deviation. The effect is driven by a reduction in study effort towards schoolwork inside and outside the classroom and in entrance exam preparation (Table A8). Using administrative outcome data, columns (3) and (4) show that the policy had a negative effect on the grades in the subjects tested on the entrance exam, and no effect on the

[30]We could not find as easily comparable statistics for the ELC program impacts in California, but 75 percent of those around the ELC admission cutoff graduated from selective colleges (Bleemer, 2021), while only 58 percent of students from the top 15 percent of PACE schools who entered college were still continuously enrolled or had graduated from college in the fifth year. This suggests that the ELC achieved more persistent impacts than PACE.

grades in the subjects not tested, suggesting students reduced their study effort towards PSU exam preparation and PSU exam subjects, without reallocating effort to other subjects. To understand whether this reduction could have contributed to the waning enrollment impacts, we examine whether these dimensions of pre-college human capital predict college persistence.

Table 4: Effect of PACE on Pre-College Achievement

	Test Score	Study Effort	12^{th} grade GPA	
			Tested subjects	Untested subjects
	(1)	(2)	(3)	(4)
Treatment	-0.099^{**}	-0.088^{**}	-0.151^{*}	-0.006
	(0.050)	(0.038)	(0.087)	(0.129)
Observations	6,054	5,631	6,046	4,288
R^2	0.259	0.047	0.220	0.109

Note.– The coefficients are OLS estimates. Standard errors were clustered at the school level. The standard set of controls (see notes in Figure 2) and Inverse Probability Weights were used. Field-worker fixed effects were used for columns (1) and (2). *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 column (1) is the number of correct answers on the achievement test, standardized. The outcome variable in column (2) is the standardized score predicted from the principal component analysis of the eight survey instruments reported in Table A8 of the Appendix. The outcome variables in columns (3) and (4) are the GPA in subjects tested and not tested on the PSU exam, standardized. *p $<$ 0.10; **p $<$ 0.05; ***p $<$ 0.01.

Fact 3: PACE lowered precisely the dimensions of pre-college human capital that predict persistence in college. Appendix Table A3 shows that, after controlling for student demographics, GPA in the last high school year strongly predicts continuous enrollment or graduation five years after entering a selective college, while the entrance exam score does not (column (1)). More specifically, GPA in core subjects like mathematics and language, which are tested on the entrance exam, is predictive of persistence, while GPA in subjects not tested on the entrance exam is not (column (2)). If GPA at the end of high school is produced by a combination of baseline ability and study effort during high school, the administrative measure of baseline ability and our survey measure of study effort should independently predict continuous enrollment or graduation five years after entering college. This is, indeed, what we find: both measures are predictive, even after conditioning on a rich vector of student characteristics that includes socioeconomic status, demographics, and high school type (columns (3) and (4)). Therefore, academic preparedness, especially competence in the core subjects, captures a combination of baseline ability and study effort in high school and appears important for college persistence in this population.

Validity of the survey-based Experimental Finding 2. While collecting measures of study effort and achievement was necessary because the administrative data lack standardized achievement and effort measures at the end of high school, and while such measures are the student-level outcomes that we specified in the pre-analysis plan, it is important to understand the validity of the results based on the survey measures. First, the negative impacts on the measures we collected and pre-specified are confirmed in the administrative GPA data, as shown. Our achievement test is on mathematics, which is a subject tested on the PSU entrance exam, for which the administrative data shows a negative impact as well.[31] Second, our measures have strong predictive validity: they can independently predict high-stake outcomes up until five years after the data collection, when our data end. For example, Table A10 shows that, controlling for student characteristics and baseline test scores, a one standard deviation increase in the achievement test score is associated with an increase in the probability that a student is enrolled in the fifth year of college of 3.0 p.p. (p=0.000), or 50% of the sample mean. The study effort measure has equally strong predictive validity. Lastly, the results are robust to using item response theory to calculate the achievement score (Table G1 of the supplementary material), and to using Lee, 2009 bounds (Appendix C).

4.2 Discussion

PACE increased the rate at which disadvantaged students are admitted to and enroll in college. But the impact on continuous college enrollment tapers off over time. This raises the question of whether large admission advantages like PACE may be leading students who lack college preparedness to enroll in college.

Much of the literature on admission advantages treats college preparedness as fixed (Arcidiacono, 2005; Arcidiacono, Lovenheim, and Zhu, 2015; Arcidiacono et al., 2011; Kapor, 2020). Yet, human capital is not a fixed trait, it can respond to the dynamic incentives introduced by the admission rules and to other changes occurring at the school level in response to these policies. Our Experimental Finding 2 establishes, for the first time through a randomized controlled trial, that preferential admission policies can causally change pre-college effort and achievement. Our rich data allowed us to further identify that PACE had a negative impact precisely on the dimensions of pre-college human capital that predict persistence in college. A key question is why this occurred. Answering it is the essential first step to understand whether large admission advantages like PACE *can* generate more persistent impacts on college enrollment. The next section examines the mechanisms behind the reduction in college preparedness.

5 Mechanisms

In this section we show results on all the potential mechanisms behind the pre-college human capital response that we specified in the pre-analysis plan, and on an additional mechanism motivated by the finding that the impacts on effort are negative. The pre-specified analysis of

[31] As described in Appendix A on fieldwork, the test questions were developed by professional testing agencies, and after extensive piloting we found that the best way to obtain a reliable measure was to introduce a reward linked to the performance on the test.

mechanisms examines: i) students' response to incentives, analyzed by examining the heterogeneity of the effect on pre-college effort and score on the achievement test by baseline absolute and relative ability and by examining subjective beliefs; ii) teacher grading, analyzed by examining the relationship between grades and standardized measures of achievement across treatment groups, and whether the grading practices differ across treatment groups; iii) teachers' focus of instruction and effort, and school inputs and practices, analyzed using survey measures we collected for this purpose among teachers and principals.[32] The mechanism not pre-specified is a reduction in perceived monetary returns to college, which could have discouraged students from preparing for the entrance exam.

5.1 Students' Response to Incentives

Preferential admissions introduce new admission requirements based on pre-college achievement. Since achievement is not a fixed trait but rather an outcome that responds to study effort, the introduction of new requirements can induce an endogenous response in study effort if students value college admission. Did students respond to incentives?

The negative average effects on pre-college effort and achievement are somewhat surprising throgh the lens of incentive response. Given that the students in our sample perform substantially below regular entrants and are admitted at low rates absent the policy (Facts 1 and 2), it is reasonable to expect that the policy brought a college admission within reach, *increasing* the returns to effort, rather than making an admission easier to obtain, *decreasing* the returns to effort. Through the lens of incentive response, therefore, the negative average impacts are surprising.

Experimental Finding 3: The negative effects on pre-college effort and achievement are spread across the absolute and relative (within-school) ability distributions. To better understand the effort response, we examine effect heterogeneity along baseline within-school rank and baseline ability. We split the sample into quintiles of baseline ability and baseline within-school rank, and estimate the regression from equation (1) on each sub-sample. The results are reported in Figure 3. We do not find evidence of encouragement effects on pre-college effort or achievement, anywhere along the baseline relative and absolute ability distributions, and we find the negative impacts are spread across baseline relative and absolute ability.

Such patterns of effect heterogeneity are hard to rationalize as a response to incentives under rational expectations. As shown in Bodoh-Creed and Hickman, 2018, when students rationally respond to the incentives embedded in percent rules like PACE, we would expect negative

[32] We also pre-specified parental involvement in their child education, but for time-budget reasons we could only add two questions to the student questionnaire on parental help: whether the mother and the father help the student with their homework. The treatment had no impact on these variables.

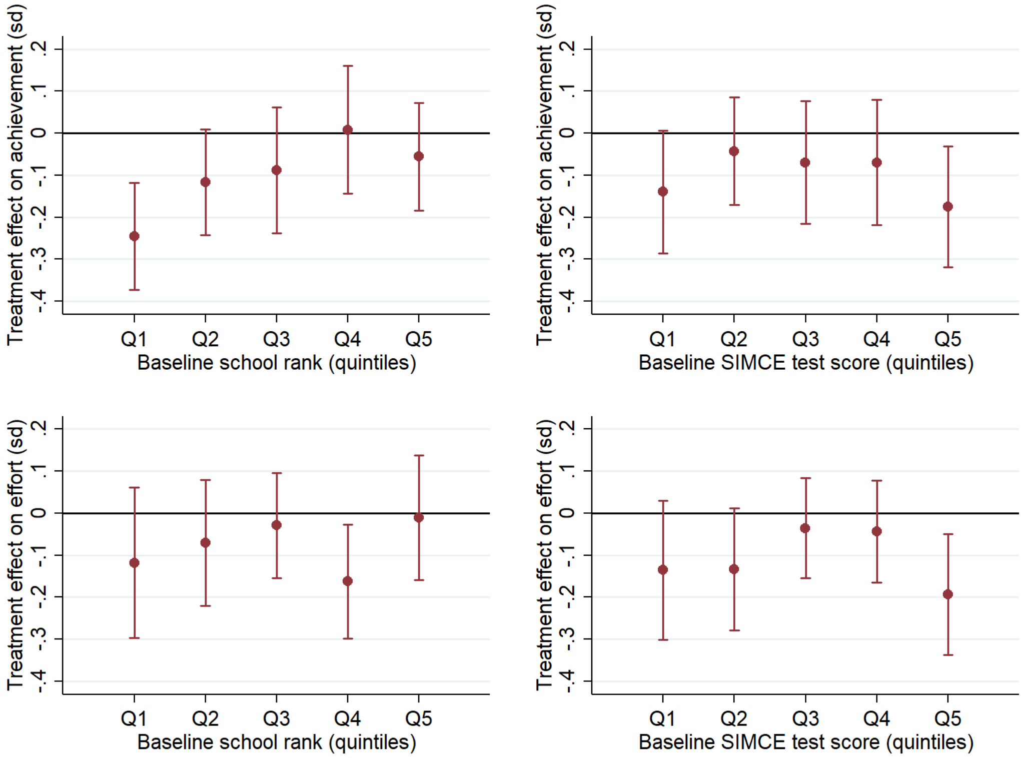

Figure 3: Heterogeneity of policy effects on pre-college effort and achievement. Notes: Each dot is the coefficient on *Treatment* from an OLS regression where: *Treatment* is a dummy variable indicating whether a student is in a school that was randomly assigned to be in the PACE program, the controls are the standard set of controls (see Figure 2), Inverse Probability Weights and field-worker fixed effects are used, the estimation samples are quintiles in the within-school rank based on 10^{th} grade GPA (left panel) and quintiles in the distribution of 10^{th} grade standardized test scores (right panel). The units of measurement of the treatment effects are standard deviations. The bars are 95% confidence intervals built using standard errors clustered at the school level.

impacts to be concentrated among students near regular admission cutoffs (high absolute ability in our sample of disadvantaged students) and well above the preferential admission cutoff (high relative ability within the school). For this group of students the policy lowered returns to effort by making guaranteed an admission that was previously only within reach under sustained effort. Conversely, we would expect positive impacts among students far from the regular admission cutoff (medium and low absolute ability in our sample) and near the top 15% within-school cutoff. For this group of students the policy increased returns to effort by bringing within reach an admission that was previously unattainable. But these are not the patterns we find.

A potential reason for not finding effects expected under rational expectations is that beliefs about own absolute and relative ability are systematically biased. Therefore, we examine students' beliefs next.

Fact 4: Students' beliefs about their absolute and relative ability are biased. Table 5 shows that students display large over-optimism over their PSU entrance exam score (first two lines), on average expecting a score that is 0.6 standard deviations above the score they actually obtain. Students also display large over-optimism about their within-school rank, with over 40% believing that their GPA is in the top 15%. Such relative-rank bias is due to misperceptions about others: students hold accurate beliefs about their own GPA (GPA is measured on a scale from 1 to 7 and on average the GPA students expect differs from the one they obtain by less than 0.1 GPA points), but, as they are never given relative feedback, they have a small belief bias about the 85^{th} GPA percentile in their school, of less than half GPA point (fourth row of the Table). The small belief bias in absolute terms translates into a large belief bias in relative terms because of strong grade compression, that we document in Figures G2 and G3 of the supplementary material.[33]

Table 5: Description of Subjective Beliefs

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

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

Examining belief heterogeneity, Figure A5 shows that students of all (absolute and relative) ability levels are over-optimistic; table A9 shows that belief biases do not vary systematically by socioeconomic background in our homogeneously disadvantaged sample. The findings align with existing evidence that over-optimism is widespread in many contexts, including education (Stinebrickner and Stinebrickner, 2014; Hakimov, Schmacker, and Terrier, 2022).[34]

Remark: Fact 4 and Experimental Findings 2 and 3 are consistent with a response to perceived incentives. As we do not have baseline belief data, we cannot estimate how the effort effects varied by baseline beliefs. However, the belief biases we documented help rationalize the effort response as a rational response to incentives, given biased beliefs: students on average believe they are high ability and high rank, which is the student type for whom we would expect effort reductions under rational expectations. To see why, note that optimism

[33]To document grade compression, first, we show that while grades can range from 1 to 7, effectively the vast majority of the grades are between 5 and 6.5. Second, we link grade data to baseline and endline standardized achievement measures, and show that grades do not discriminate substantially among students of different abilities, and much less than the endline standardized achievement measure does. See Figures G2 and G3 in the supplementary material.

[34]We have also collected beliefs about returns to effort, which we describe in section 6.3. As actual returns to effort are not directly observed in the data, we do not include them in this section, which describes errors in beliefs.

about the entrance exam could lead students to perceive a regular admission as within reach, and study for the entrance exam absent the policy, something most students in the control sample do (Table 3). The optimism about the within-school rank could lead students to perceive a preferential admission as guaranteed, and reduce effort when the policy is introduced (Experimental Finding 2). The belief data, therefore, appear consistent with students choosing effort based on perceived incentives. Additional suggestive evidence points to this channel: the negative effect on pre-college achievement is driven by students whose baseline GPA is well above the *perceived* cutoff (Appendix Figure A6), suggesting the negative impacts on pre-college investments were driven by those who perceived a preferential admission as guaranteed.

Why would students interested in college education lower their pre-college study effort, if it matters for persistence in college? One possible explanation is that they do not perceive pre-college effort as important for persistence in college. We have elicited the perceived likelihood of college graduation conditional on enrolling.[35] We find that half of the students are certain they will graduate if admitted, and three quarters believe they have a more than 50% chance of graduating. Crucially, PACE had no impact on this belief, despite its large negative impact on pre-college effort. Since this question was asked when the effort reductions had already occurred, this finding suggests that students do not believe that pre-college effort matters for college persistence.

Remark: likely reasons for the biases in beliefs. The large belief biases about the entrance exam are consistent with the sporadic exam preparation that takes place in these schools, and the limited knowledge of college in the students' families (over 90 percent have parents who did not study beyond secondary education).[36] The large belief biases about school rank, which are relevant for percentile-based plans like PACE (a central form of context-based admissions), could be typical of these schools too. Even though students have correct beliefs about their own GPA and only a small belief bias about the school cutoff in *absolute* terms, such small belief bias translates into a large belief bias about *relative* rank within the school because grades are compressed (as we documented). With compressed grades, biased beliefs about rank are likely whenever relative rank feedback is not provided. While this is a new finding in the literature on admission policies, grades that do not discriminate much between students could be common in schools where academic standing is not particularly salient, such as those that do not normally send students to college.

Virtually nothing is known about the beliefs of high school students targeted by admission policies in other contexts. While more research is required to establish how common belief biases

[35]The question can be translated into English as: *"If I get admitted to a selective college (not a technical institute), I will complete my studies".* The answers are on a 5-point Likert scale, from "Totally sure that I will not" to "Totally sure that I will".

[36]The fact that entrance exam preparation is sporadic was further confirmed to us in several focus groups recently implemented in PACE schools for a different project.

are in this group, the kinds of information frictions we documented may be common among very disadvantaged schools that do not normally lead to college. Therefore, policymakers wanting to introduce large admission advantages should reckon with the reality of the school environments such policies would encounter.

Predictive validity of the belief measures. For the findings depending on belief data to be reliable, it is important that beliefs capture something relevant about choice. We examine their predictive validity in Table A11, using high-stake outcomes collected up to five years after the data collection, when our data end. Our belief measures correlate with high-stake outcomes as we would expect them to do if they were capturing what they are designed to capture, as the following results show:

1. *The belief over the entrance exam score independently predicts all college-going outcomes, from entrance-taking to persistence in college.* Controlling for baseline characteristics and test scores, an increase in the believed entrance exam score of one standard deviation of the score distribution increases the probability that a students takes the entrance exam, applies to college, and is enrolled in college five years later. The associations are strong, for example, college enrollment five years later is increased by 4.1 p.p. (p=0.000), or 59% of the sample mean (Panel A column (7) of Table A11).[37] The believed PSU score remains a strong predictor of enrollment and persistence in college even when adding the actual PSU score as a control (Panel B), suggesting that optimism over the score correlates positively with unobserved preference for college, unobserved ability, or both.[38]

2. *The belief over the within-school GPA rank independently predicts college-going outcomes in the treatment group (where rank matters greatly for admission) but not in the control group (where it does not), as expected if students based their college investment decisions on the perceived admission likelihood and if our survey recovered credible measures of beliefs.* In the control group, we do not expect beliefs around the within-school GPA rank to affect whether students take the entrance exam (and later apply, enroll and persist in college), because the rank is not an important determinant of the admission likelihood. Panel C of Table A11 shows that this is indeed what we find. But in the treatment group, within-school rank affects a student's admission likelihood, therefore, we expect the belief over the rank to predict such outcomes. Panel D of Table A11 shows that this is indeed what we find. For example, an increase in the perceived lead over the cutoff by one GPA-point, controlling for baseline characteristics and ability, is not associated with persistence in college five years later in the control group (0.4 p.p., p=0.436), but it is

[37]The predictive validity of the belief over the entrance exam score examined in Table A11 uses the sample of control students, but the conclusions are the same using the sample of treated students.

[38]In this sample of test-takers, we eliminate the causal link between beliefs about the entrance exam and likelihood to take the exam, therefore, predictions within this sample are entirely correlational.

strongly associated with it in the treatment group (3.8 p.p., p=0.000, corresponding to 36% of the sample mean). Therefore, the survey measure of belief about relative ability correlates with high-stake outcomes as we would expect it to if it was an accurate measure.

We interpret the predictive validity results as follows. First, subjective beliefs are important in choice. Second, our survey recovered credible measures of these beliefs. Third, subjective beliefs likely correlate with unobserved determinants of college going, such as preferences and unmeasured ability, therefore, the structural model should take such correlation into account.

5.2 Changes in Teachers' Behaviors and School Practices

Teacher Grading. Teachers can decide who obtains a preferential seat through their grading. If in response to the percent plan policy they manipulate their grading in a way that weakens the link between achievement and GPA, students in treated schools would have a lower incentive to study to improve their grades. This could explain the negative impacts on effort.

The evidence does not support this mechanism. As shown, pre-college effort reductions resulted in grade reductions (Table 4). Accordingly, the mapping between standardized achievement and grades does not differ between treated and control schools (Supplementary Table G3), suggesting that grading did not respond to the treatment. Consistent with this result, school principals report similar grading practices across treatment groups (Supplementary Table G4).

Teacher Effort and Focus of Instruction. Teachers could change their focus of instruction (i.e., what portion of the ability distribution they target with their teaching), or they could change effort (class preparation hours and absence days) as an effect of percent plans like PACE. Appendix E.1 describes how we measured these teacher behaviors, and Appendix Table A12 shows that there is no evidence that such behaviors responded to the policy.

Schools. The curriculum is not a possible margin of policy response because the Ministry of Education mandates it. But school principals in treated schools may decide to offer fewer support classes, especially in regards to entrance exam preparation, as performing well on the exam is less critical for an admission. This, in turn, could directly lower students' pre-college achievement, especially in the subjects tested on the exam.

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 G4.

Principals may also choose to change the assignment of students to classrooms. We asked them a set of questions on classroom formation, and found no effects, as shown in Supplementary Table G5.

5.3 Reduction in Perceived Returns to College

If the light-touch orientation classes offered to PACE students negatively affected students' beliefs about the net returns to college, they could have generated the negative response of pre-college study effort. In the Chilean setting, Hastings, Neilson, and Zimmerman, 2015 found that providing information about graduate earnings can change students' college choices. Therefore, even though the orientation classes were not designed to provide information about returns to college, this is an important channel to consider.

We elicited beliefs about the monetary returns to a college degree at age 30, and about students' awareness of tuition costs. We find that the policy had no impact on students' beliefs about the monetary returns to college (Appendix E.2), which are large at 200% of age 30 earnings, 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 net returns to college.[39]

6 A Dynamic Model of Education Choices

6.1 From Experimental Evidence to a Model

The reduced-form results demonstrated the importance of college preparedness in the context of large admission advantages. Even students who perform at the top of their school score substantially below regular college entrants on high school standardized tests. While PACE achieved persistent impacts on their college attainment, the impacts waned substantially and significantly over time.

The results, however, suggest that college preparedness is not fixed. It is elastic to investments made in the last high school years, and such investments respond endogenously to the admission rules. This suggests that a promising area for intervention to improve the persistence of large admission advantages is to intervene in targeted high schools to improve the college preparedness of college entrants. As such interventions have not been implemented yet, we develop a structural model that allows us to simulate them.

For the model to be useful it must successfully explain the experimental findings, and deliver the college preparedness of college entrants as an endogenous outcome. To achieve this, we develop a dynamic model of pre-college effort, pre-college achievement, entrance-exam taking, admissions and enrollments that builds upon the reduced-form evidence. We model both a context without admission preferences and one with, and are able to succesfully replicate the

[39] The perceived returns we measured are similar to those measured among other samples of Chilean students of the same age (Hastings, Neilson, Ramirez, and Zimmerman, 2016).

experimental findings. The model delivers endogenously the distribution of college seats and the pre-college effort and baseline ability of those who self-select into college.

Informed by the belief data, we do not impose rational expectations but assume that high school students form beliefs about the returns to effort in securing an admission, and choose effort so as to maximize subjective value functions. Based on the admission credentials accumulated by students at the end of high school, admissions are realized according to objective admission likelihoods. Given the admission sets, students choose enrollments. Therefore, the choices students make in high school affect the allocation of college seats and the college preparedness of college entrants. Shaping those choices through strategically designed school interventions can affect the college preparedness of college entrants under large admission advantages.

The survey data highlighted large belief biases about absolute and relative ability. The first intervention we consider, therefore, eliminates such belief errors. The survey data also suggested that students do not perceive pre-college effort as important for college success. The second intervention we consider, therefore, communicates to students the importance of pre-college effort for college persistence.

6.2 Model

Observed and unobserved heterogeneity. Each student i is characterized by vectors x_i and y_{it-1} of baseline characteristics and baseline achievement measures, respectively, and by $k_i \in \{1, 2, ..., K\}$, a time-constant type unobserved by the econometrician but observed by the student (Heckman and Singer, 1984; Keane and Wolpin, 1994, 1997).[40] The number of types, K, is known to the econometrician. We let parameters that govern the preference for college, achievement and subjective beliefs depend on a student's type, to capture potential correlation between ability, preferences and beliefs that is not explained by observables. Not allowing for such correlation could lead to biased parameter estimates that mischaracterize the role of beliefs in choice (Wiswall and Zafar, 2015; Bobba and Frisancho, 2019). This modelling choice means that the model does not assume that the predictive validity of the belief measures we presented in section 5.1 is causal.

Timing. Figure 4 shows the model timeline. Before the first model period, students form beliefs about the top 15% cutoff in their high school and about how study effort maps into a GPA and an entrance exam score. These determine the *subjective* probabilities of a regular and preferential admission as a function of pre-college effort (represented in Figure 4 as $PrR(e)$ and $Pr15(e)$). Based on these beliefs, in period 1 students choose study effort so as to maximize its perceived present value. In period 2, students decide whether to take the PSU entrance exam.

[40]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.

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

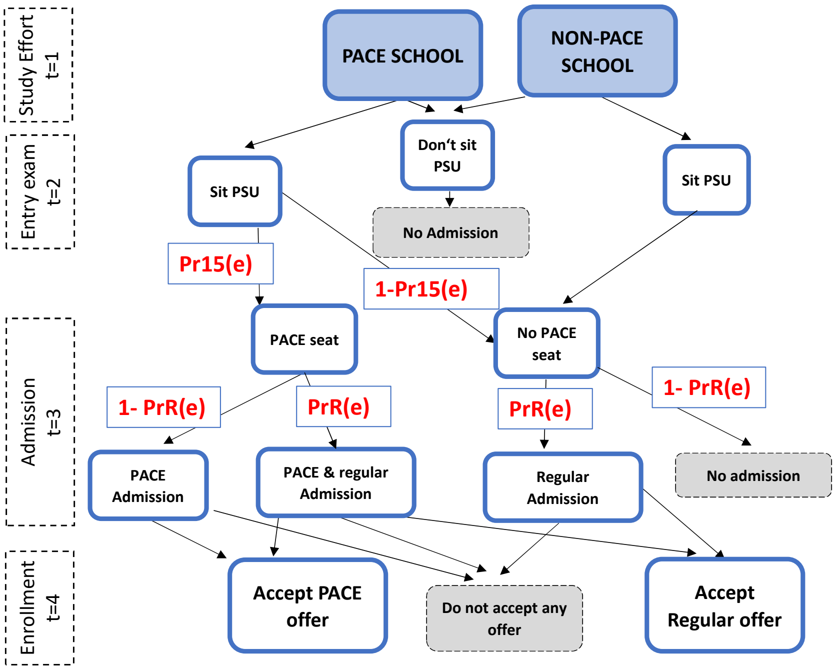

Figure 4: Model timeline.

Parameterization. Below we show how preferences and the objective and subjective production functions and admission probabilities enter the model. In Appendix F.1 we show how we parameterize them when we estimate the model.

Objective and subjective admission probabilities. The entrance exam score is produced through effort e_i:

$$PSU_i = PSU(e_i, y^{(1)}_{i,t-1}; \beta^P) + \epsilon^P_i, \tag{2}$$

where $y^{(1)}_{i,t-1}$ is a baseline standardized test score and ϵ^P_i is a normally distributed idyosincratic shock. Letting A^R_i be equal to 1 if student i obtains a regular admission and to 0 otherwise, and letting S_i be equal to 1 if student i takes the entrance exam and to 0 otherwise, the objective probability of a regular admission for those who take the entrance exam depends on the entrance exam score, and can be written as:

$$Pr(A^R_i = 1 | PSU_i, S_i = 1; \gamma). \tag{3}$$

But students base their pre-admission choices on beliefs about the PSU production function:

$$PSU_i^b = PSU^b(e_i, y_{i,t-1}^{(1)}, k_i; \beta^{Pb}) + \epsilon_i^{Pb}, \tag{4}$$

where normally distributed ϵ_i^{Pb} captures belief uncertainty around the expected score, and on beliefs about how the entrance exam score translates into a regular-admission chance (captured by the parameters γ^b):

$$Pr^b(A_i^R = 1|\overline{PSU}_i^b, S_i = 1; \gamma^b), \tag{5}$$

where $\overline{PSU}_i^b$ is the expected score.

Similarly, GPA is produced through effort:

$$GPA_i = GPA(e_i, y_{i,t-1}^{(2)}; \beta^G) + \epsilon_i^G, \tag{6}$$

where $y_{i,t-1}^{(2)}$ is baseline GPA and ϵ_i^G is a normally distributed idiosyncratic shock, potentially correlated with the PSU production shock. The objective probability of a preferential admission is determined by the joint distribution of the shocks in the school; preferential admissions are assigned to students in treated schools who take the entrance exam and whose GPA is in the top 15% of their school. But students base their pre-admission choices on beliefs about the GPA production function:

$$GPA_i^b = GPA^b(e_i, y_{i,t-1}^{(2)}, k_i; \beta^{Gb}) + \epsilon_i^{Gb}, \tag{7}$$

where normally distributed ϵ_i^{Gb} captures belief uncertainty around the expected GPA, and on beliefs about how the GPA translates into a preferential admission chance (captured by the parameters ξ^b):

$$Pr^b(A_i^P|\overline{GPA}_i^b, c\bar{1}5_i^b; \xi^b), \tag{8}$$

where $\overline{GPA}_i^b$ and $c\bar{1}5_i^b$ are the expected GPA and school cutoff and where A_i^P is equal to 1 if student i obtains a preferential admission and to 0 otherwise. Students in PACE schools, therefore, best-respond to their belief about the within-school cutoff ($c\bar{1}5_i^b$), and we do not impose that the beliefs are equilibrium ones. This modelling approach follows an established approach developed in the behavioral game theory literature (e.g. Stahl and Wilson, 1995; Costa-Gomes and Zauner, 2003; Camerer, Ho, and Chong, 2004; Costa-Gomes and Crawford, 2006; Crawford and Iriberri, 2007).[41]

Per-period utilities. In the first period, students derive utility from achievement, produced through effort, and face a cost of exerting effort, such that the per-period utility associated

[41] As we explain in detail in Appendix F.1, we assume that the survey answers on the expected entrance exam score, GPA and GPA cutoff capture the believed average outcome, and we allow for belief uncertainty around this average, which is absorbed by the γ^b and ξ^b parameters.

with each effort choice $e_i \in \{0, 1, ..., E\}$ is:

$$u_{i1}(e_i) = y(e_i, x_i, y^{(1)}_{i,t-1}, k_i; \alpha) - c(e_i; \xi), \tag{9}$$

where the cost function is assumed to be quadratic: $c(e_i; \xi) = \xi_1 e_i + \xi_s e_i^2$, with a constant normalized to zero because only the difference in utilities is identified. In period 2, students decide whether to take the entrance exam. The per-period utility from taking the exam is the sum of the cost of taking the exam (capturing monetary and non-monetary costs), and a standard logistic shock: $u^{S_i=1} = -c^S + \eta_i$.[42] The per-period utility from not taking the exam is normalized to 0 because only the difference in utilities is identified. In time period 3, admissions are realized.[43] In time period 4, when making enrollment decisions, students derive the following utilities from a regular and a preferential enrollment, respectively:

$$u_i^{ER} = \lambda^R_{0k_i} + \lambda_1 SES_i + \lambda_2 a_i + \lambda_3 q^R(PSU_i) + \nu_i^R, \tag{10}$$

$$u_i^{EP} = \lambda^P_{0k_i} + \lambda_1 SES_i + \lambda_2 a_i + \lambda_3 q^P(GPA_i) + \nu_i^P, \tag{11}$$

where $\lambda^P_{0k_i} = \lambda^R_{0k_i} + \delta^E$. The utility from not enrolling is normalized to 0. We let the enrollment utilities depend on: the type k_i; the socioeconomic status and ability (SES_i, a_i); the selectivity of the degree-program to which they are admitted (defined as the lowest entrance exam score among all regular entrants), which, approximating the allocation mechanisms, depends on the PSU score in the regular channel and on the GPA in the preferential channel, $q^R(PSU_i), q^P(GPA_i)$; and a standard-logistic utility shock.[44] When making pre-admission choices, students use their expected PSU and GPA to form beliefs about the quality of the degree programs to which they will gain admissions, but realized qualities depend on the objective PSU score and GPA achieved. Keeping selectivity constant, preferential and regular enrollments are allowed to give different utilities (the constants in equations (10) and (11) can differ), to capture differences across channels not captured by selectivity, as well as any utility cost or premium from enrolling as a preferential student. The enrollment preferences, which are relative to the outside option, capture tastes, barriers and outside options that vary by unobserved student characteristics (k_i) and by background and ability (SES_i, a_i). We do not let the enrollment utilities directly depend on pre-college effort because, as shown in the first

[42] The fee is approximately USD 30; most students in the sample can apply for a fee waiver. But disadvantaged students may face non-monetary barriers to taking entrance exams.

[43] We let preferential admissions carry a utility $\delta^A \neq 0$, because in the data we see a null PACE effect on entrance-exam taking that would be difficult to capture without a preferential admission disutility: PACE provides new admission opportunities to those who take the entrance exam, increasing the value of taking it, without increasing its cost. A possible micro-foundation for this parameter is pressure from parents and teachers to enroll through PACE once a PACE admission is obtained, if students would rather avoid enrolling preferentially.

[44] SES_i is an indicator for whether the student is identified as with very-low SES by the government; a_i is an indicator for whether a student is above or below median ability at baseline.

remark of section 5.1, the data suggest that students do not believe pre-college effort matters for college persistence.

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

$$V_t^b\left(\Omega_{it}\right) = \max_{d_{it} \in D_{it}} \left\{ u(d_{it}, \Omega_{it}) + E^b\left[V_{t+1}\left(\Omega_{it+1} | \Omega_{it}, d_{it}\right)\right] \right\} \tag{12}$$

where Ω_{it} is the state vector, which evolves from the initial condition according to *objective* production functions and admission probabilities, and d_{it} is the period choice.[45] We solve the problem by backward induction and find the value of the subjective value function in all decision periods and at all possible state space values. We compute the exact analytical solution, a sequence of optimal, non-randomized decision rules $\{d^*_{it}(\Omega_{it})\}$ that are deterministic functions of the state space Ω_{it}.[46]

6.3 Identification

We now discuss key measures we use, and how we identify the parameters governing subjective beliefs. In Appendix F.2 we discuss permanent unobserved heterogeneity, modelled following Heckman and Singer, 1984, Keane and Wolpin, 1994, 1997, and Wooldridge, 2005.

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

Subjective beliefs. We separately identify subjective beliefs from unobserved ability and preferences using the belief data we collected (Manski, 2004). The subjective probabilities of a regular and a preferential admission, conditional on taking the entrance exam ($S_i = 1$), are a function of effort e_i, and depend on the expected believed PSU score, $E[PSU^b_{k_i}(e_i, x_i)]$, the

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

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

expected believed GPA, $E[GPA^b_{k_i}(e_i, x_i)]$, and the believed top 15% cutoff in the school, $c\bar{1}5^b_i$, as shown in the following equations and, in more detail, in equations (24) and (25) in the Appendix:

$$Pr^b(A^R_i = 1|e_{it}, x_i, k_i, S_i = 1) = \Phi\left(\gamma^b_0 + \gamma^b_1 E[PSU^b_{k_i}(e_i, x_i)]\right), \tag{13}$$

$$Pr^b(A^P_i = 1|e_i, x_i, k_i, S_i = 1) = \Phi\left(\xi^b_0 + \xi^b_1(E[GPA^b_{k_i}(e_i, x_i)] - c\bar{1}5^b_i)\right), \tag{14}$$

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

First, we follow a standard approach from the behavioral game theory literature, and assume that students in treated schools best-respond to the perceived cutoff that we have elicited, without imposing equilibrium behavior (Stahl and Wilson, 1995; Costa-Gomes and Zauner, 2003; Camerer, Ho, and Chong, 2004; Costa-Gomes and Crawford, 2006; Crawford and Iriberri, 2007). Therefore, this argument of the function in (14) is observed.

Second, to identify the perceived returns to effort in the subjective production functions, in the right-hand side of (13) and (14), we do not rely on the cross-sectional relationship between expected outcomes and effort, because it cannot necessarily be interpreted as causal. Instead, we measured perceived returns with our survey. We elicited beliefs about the PSU score and the GPA that students expect to obtain under the actual and hypothetical effort levels. For example, for entrance exam scores, we asked:

Thinking of yourself, how many hours per week do you think you need to study, between August and December, to obtain...
... 600 or more on the PSU
... 450 or more on the PSU
... 350 or more on the PSU.

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

$$\sum_{j\in\{350,450,600\}} \frac{1}{3} \cdot \frac{j - PSU^b_i|_{e^o_i}}{h^{oj}_i - e^o_i}, \quad \text{if } h^{oj}_i \neq e^o_i. \tag{15}$$

Figure 5 shows the distribution of returns to effort in our sample (Table A13 summarizes the survey answers used to construct the returns). In estimation, we match moments of these distributions using their model counterparts. Naively matching them would introduce sample-selection bias because perceived returns are not observed among students who were not surveyed. To mitigate the issue we let parameters that govern the perceived returns depend on the unobserved student type, and we let the type distribution vary across students who were and were not surveyed. We then simulate the distributions of perceived returns conditional on being surveyed to build the model counterparts to the empirical moments.

To simulate perceived returns, we simulate the expected PSU score and GPA for each student at various values of hours of study. For example, consider distinct effort levels h_i^z and h_i^j and let $\widehat{PSU_i^b}(h_i)$ be the expected PSU score predicted by the model at effort level h_i. The simulated returns to effort are:

$$\frac{\widehat{PSU_i^b}(h_i^z) - \widehat{PSU_i^b}(h_i^j)}{h_i^{oz} - h_i^{oj}}, \quad \text{where } h_i^{oz} = h_i^z + \epsilon_i^{m.e.e.} \text{ and } h_i^{oj} = h_i^j + \epsilon_i^{m.e.e.}. \tag{16}$$

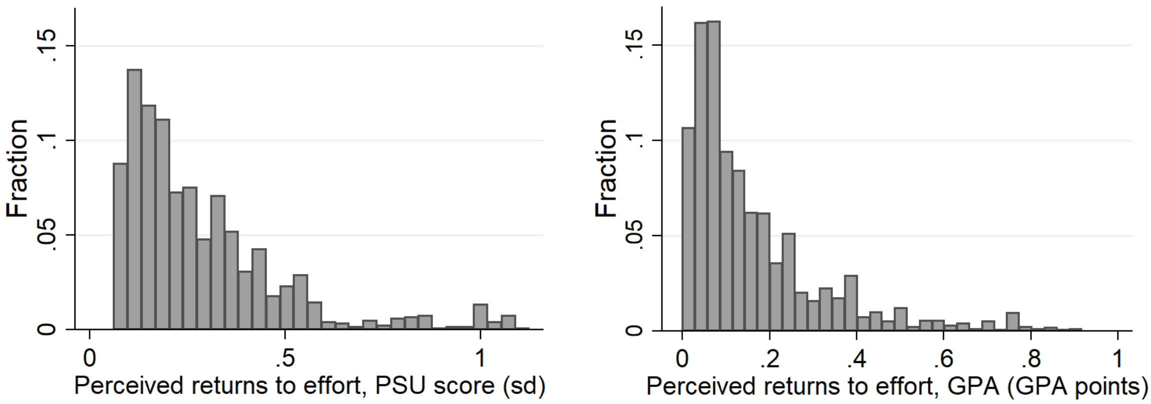

Figure 5: Distribution of perceived returns to effort, measured as the perceived impact of an additional hour of study per week in the semester (top 1% trimmed).

Having identified the parameters governing perceived returns to effort, we match the distributions of expected PSU scores and GPA to identify the remaining parameters of the perceived production functions. Then, all arguments of the subjective admission probabilities in (13) and (14) are either observed or identified. The relation between choices and these arguments identify the parameters of the subjective probabilities $(\gamma_0^b, \gamma_1^b, \xi_0^b, \xi_1^b)$. Appendix F.2 details how we mitigate potential endogeneity of these arguments by imposing additional exclusion restrictions, exploiting the experimental data variation wherever possible.

6.4 Estimation

Aside from the parameters of the regular admission probability (equation (21)) and of the selectivity of an admission (equations (26) and (27)), whose estimates we report in Table A14, 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 (ξ, c^S, λ), and the distribution of model shocks, measurement errors, and unobserved types $(\Sigma, \sigma^2_{m.e.y.}, \sigma^2_{m.e.e.}, \omega)$. We assume that there are two unobserved types ($K = 2$) that follow a logit distribution that depends on the ninth and tenth grade GPA average ($y^{(3)}_{it-1}$) and on an indicator for whether a student was surveyed in our data collection, D^s_i, to correct for survey attrition based on unobservables. Since the treatment was randomized, we can assume that types are identically distributed across treatment groups (i.e., balanced unobservables). Letting $X_i = [1, y^{(3)}_{it-1}, D^s_i]$:

$$Pr(k_i = \tau | X_i) = \frac{e^{X_i'\omega}}{1 + e^{X_i'\omega}}. \tag{17}$$

Estimation is by generalized indirect inference (Bruins et al., 2018), as in Altonji, Smith Jr, and Vidangos, 2013. In a first step, we estimate a set of auxiliary models that summarize the experimental findings and data patterns to be targeted in the structural estimation. In a second step, an outer loop searches over the parameter space, while an inner loop solves the dynamic model at each candidate parameter value and forms the criterion function. The latter is the distance between the auxiliary model estimates from the data and their counterparts from the simulated data. Appendix F.3 lists the auxiliary models and moment conditions.

At each parameter iteration θ, we simulate S datasets, where each simulation is a draw for the model shocks and the student type.[47] 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:

$$\hat{\theta} = \arg\min_{\theta} \left[\hat{\beta}(\theta) - \bar{\beta}\right]' W \left[\hat{\beta}(\theta) - \bar{\beta}\right] \tag{18}$$

where W is a positive definite weighting matrix. Generalized indirect inference, developed for dynamic discrete choice models like ours, ensures that the criterion function is differentiable and allows us to rely on a fast derivative-based optimization method to solve (18).[48]

7 Model Results

7.1 Estimation Results

Parameters. Estimates of the model parameters are in Table A15. Comparing the perceived and objective production functions shows that students hold overoptimistic beliefs about the returns to effort. In the objective production function of entrance exam score (GPA), the coefficient on effort is 0.161 standard deviations (0.037 GPA points, or 0.065 standard deviations).

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

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

But students, depending on their type (as defined in section 6.2), believe it is larger, between 0.262 and 0.331 standard deviations (0.148 and 0.353 GPA points, or 0.260 and 0.619 standard deviations). Therefore, both student types are overoptimistic. Those of the more optimistic type also have higher unobserved ability and preference for college. Therefore, ability, preferences and beliefs correlate with each other, highlighting the importance to allow for such correlation in estimation.

Model fit. As Appendix Table A16 shows, the model captures key facts and findings from the reduced-form analysis, and additional important data features such as the dynamics of the students' choice problem. The model can rationalize all the facts and findings at the core of our analysis, including those that would be hard to explain with standard rational expectation models.

The model can match the fact that a high proportion of students take the entrance exam, but a much lower proportion is admitted and enrolls in college absent the policy. It matches the positive treatment effects on admissions and enrollments, and negative on pre-college effort and achievement. At the same time, it captures very closely the belief biases over both absolute and relative ability. And finally, it matches correlations in choices over time. For example, it matches very closely the GPA of college entrants, overall and by treatment groups, even though it was not directly targeted in estimation. GPA of college entrants is the outcome of several choices that occur dynamically: the choice of pre-college effort directly affects GPA, and also indirectly affects the selection of college entrants by affecting a student's admission likelihood. The fact the model can capture endogenous outcomes and dynamic self-selection suggests that it provides a reasonable approximation to the dynamic decision process that students face. Finally, the Table specifies which moments were directly targeted in estimation and which were not, and shows that the model can fit both kinds of moments, improving our confidence in the model-based results.

Perceived incentive effect. Having estimated the model, we can use it to simulate the perceived returns to effort in the admission likelihood for students in the treatment and control group, and quantify the perceived incentive effects of PACE.

Absent PACE, the perceived return to effort in the admission likelihood is the derivative with respect to effort of the perceived likelihood of a regular admission. Under PACE, it is the derivative with respect to effort 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 for a student who exerts effort e and has a vector of initial conditions Ω_{i1}, the average perceived marginal returns to effort for student i can be approximated by the numerical derivative:

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

where $\Delta e = 1$. Using the distribution of initial conditions, we average this derivative across students to calculate the treatment effect on perceived returns to effort.

We find that PACE lowered the perceived return to effort in generating a college admission by 5.3 percentage points (p.p.), a 77% reduction compared to the perceived return to effort without PACE. Without PACE, our simulations indicate that students believe one additional hour of study per week in the first semester of the last high school year increases the likelihood of college admission by 6.9 p.p. on average. With PACE, this figure falls to 1.6 p.p. Therefore, students perceived that the policy considerably undercut their incentive to exert effort in the last high school year.

7.2 Counterfactual Experiments: Improving the College Preparedness of College Entrants through School Interventions

We define college preparedness as a vector containing high school standardized test scores at baseline (a measure of baseline ability) and the effort exerted in the last high school year, since they jointly and independently predict college persistence (Table A3). We simulate two counterfactual scenarios and examine how they change the college preparedness of college entrants under PACE. In the first, we simulate correcting the belief errors that students hold about their absolute and relative ability. In the second, we approximate a policy informing students of the importance of pre-college effort for persistence in college.

The college preparedness of college entrants is determined by the *selection* channel (i.e., the ability composition of college entrants) and the *effort* channel (i.e., how much effort they exerted in high school). Any intervention changing pre-college effort can affect it directly, through the effort channel, and indirectly, through the selection channel. To see how the selection channel works, notice that pre-college effort affects the perceived GPA rank and perceived entrance exam score, which in turn affect the perceived likelihoods of a regular and of a preferential admission and, therefore, the decision to take the entrance exam. Effort, therefore, affects the choice of taking the exam, the actual GPA rank, and the actual entrance exam score, which together determine the objective admission likelihood of each student. Effort, therefore, affects the selection of admitted students and college entrants. Our model captures all of these effects.

First counterfactual experiment: correcting beliefs about ability in PACE high schools. Had the students in PACE high schools had correct information about their relative and absolute ability, they would have exerted different levels of pre-college effort. In turn, both the selection of college entrants and their pre-college effort would have been different.

To simulate this counterfactual, we assume students have rational expectations. We assume they use objective rather than subjective production functions (for the GPA and the entrance exam score) and admission likelihood functions (for regular and preferential admissions). We then solve for the rational-expectations equilibrium of the tournament game that takes place in each school to award the preferential admissions, a high-dimensional fixed-point problem. This is a notoriously difficult problem to solve. Previous studies have simplified it by assuming that there is a continuum of individuals and that they differ only along one dimension (Hopkins and Kornienko, 2004; Bodoh-Creed and Hickman, 2018, 2019; Cotton, Hickman, and Price, 2020). But these simplifications are inappropriate in our setting: i) our populations are schools, which are limited in size, and ii) individuals differ in more than one dimension. Therefore, we develop an algorithm that allows us to relax them.[49] Appendix F.4 describes it.

The bars labelled "correct beliefs" of Figure 6 present the results of the counterfactual experiment. If students in PACE schools had correct beliefs, both the high school test scores at baseline (baseline ability) and the pre-college effort of the sub-sample that selects into college would have been larger, by 0.08 standard deviation and 0.31 study hours per week (corresponding to 0.6 standard deviations of the study hour distribution in the sample). Therefore, the selection and the effort channels are both empirically relevant channels through which informational interventions can affect the college preparedness of college entrants under large admission advantages.

We now examine how this counterfactual policy affects students' choices along the baseline test score distribution. Figure 7 shows the effect of eliminating belief errors on the pre-college effort and admissions of students in PACE schools, by 10^{th} grade (baseline) test scores. Recall that beliefs are over-optimistic, on average, at all baseline test score levels in our sample (Figure A5). Eliminating such over-optimism has opposite effects on effort depending on ability (left panel of Figure 7). Over-optimism leads high-ability students to incorrectly perceive an admission as guaranteed and under-provide effort, and low-ability students to incorrectly perceive it as within reach and over-provide effort. Therefore, eliminating it increases the effort of high-ability students and decreases that of low-ability ones. Since effort affects the likelihood of qualifying for an admission, effort under- (over-)provision results in under- (over-)admissions, so that eliminating over-optimism increases the admissions of the high ability and decreases those of the low ability (right panel). This explains why correcting belief biases results in a better selection of admitted students in terms of baseline test scores, who have also exerted

[49]We lower the dimensionality of the fixed point and solve for an approximated equilibrium. The intuition is that the strategies of others affect own payoffs only through the probability of a preferential admission. We posit a parametric approximation for this probability and solve for a fixed point in its parameters. We thank Nikita Roketskiy for suggesting this approach.

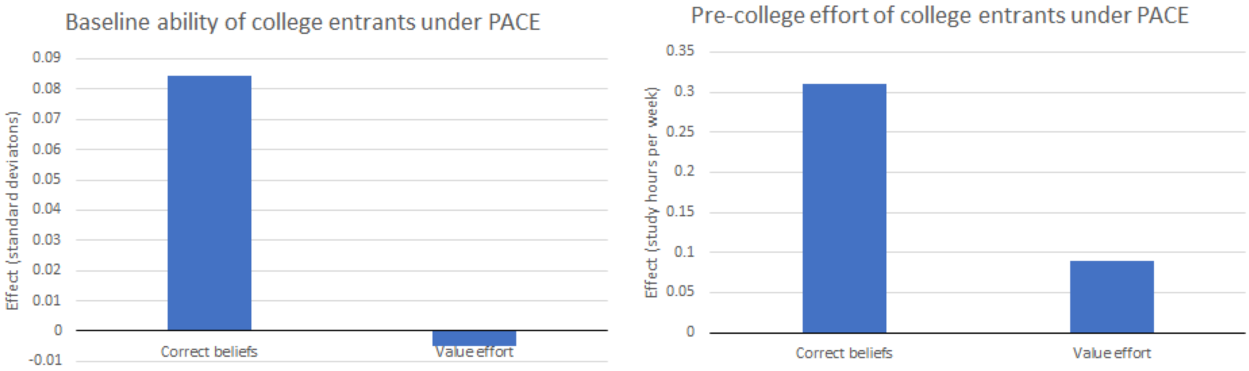

Figure 6: Counterfactual experiments simulating interventions in schools to shape the college preparedness of college entrants: the selection and effort channels. *Notes:* The panels show the effects of hypothetical interventions that correct belief biases (first bar) or that inform students of the importance of pre-college effort for persistence in college (second bar) on the college preparedness of college entrants under PACE. The left panel shows the effect on the high school standardized test scores at baseline, i.e., the 10^{th} grade, standardized in the population of 10^{th} graders (the selection channel). The right panel shows the effect on study hours per week in the first semester of the last high school year (the effort channel).

more effort while in high school. This intervention would also lower the pre-college effort of those who do not enter college (by 0.64 study hours per week, or 1.26 standard deviations).

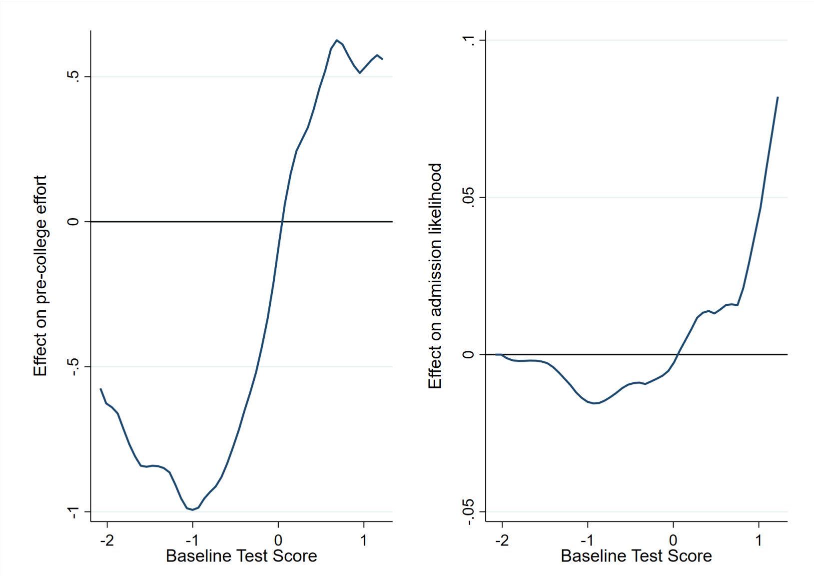

Figure 7: Effects of correcting belief biases in PACE schools on pre-college effort and on admission likelihood along the baseline test score distribution. *Notes:* Effort is measured in study hours per week in the first semester of the last high school year. Baseline test scores (standardized) are measured in 10^{th} grade.

Second counterfactual experiment: informing students of the importance of pre-college effort for college persistence. Some policymakers consider providing rank information controversial; they worry that it could promote unhealthy competition among students and, for this reason, are not actively pursuing this strategy.[50] Therefore, we consider an alternative policy to influence college preparedness: informing high school students targeted by large admission advantages of the importance of pre-college effort for persistence in college.

Recall that in the baseline model we do not allow effort to enter the utility from college enrollment (see the discussion below equations (10) and (11)), because the data suggest students do not believe pre-college effort is important for college persistence (section 5.1). In this counterfactual experiment, we assume that the utility students derive from college depends on pre-college effort. The idea behind this assumption is that a student becomes aware that exerting more effort in high school can make it easier to learn in college, reducing the likelihood of dropping out. We let effort enter the utility from college with a coefficient of 0.015, which captures how predictive each additional hour of effort is for persistence in college (see Table A3).[51] Since students are forward looking, this counterfactual changes the continuation value of study effort in high school. Therefore, it affects how much effort students wanting to go to college exert.

We must assume a process for counterfactual beliefs about the school cutoff, because the elicited beliefs about the cutoff were collected at the baseline distribution of effort in each school and are not appropriate beliefs in a counterfactual that changes the within-school distributions of effort. We assume that the belief bias over the rational expectations cutoff remains constant in the counterfactual, which means assuming that students remain as uninformed in the counterfactual as they were in the baseline scenario.[52]

The bars labelled "value effort" of Figure 6 show that the selection of students into college would stay substantially unchanged (left panel), while the pre-college effort of those who self-select into college would improve by 0.09 hours of study per week, corresponding to 0.18 standard deviations of the study hour distribution in the sample (right panel). This intervention, therefore, is not as effective at improving the college preparedness of college entrants as correcting belief errors about absolute and relative ability. Given the widespread over-optimism about admission chances, this intervention would also cause those who do not enroll in college to increase their pre-college effort (by 0.06 hours per week, or 0.12 standard deviations) so as to improve their college persistence, which has ambiguous welfare implications.

[50] This is what policymakers at the Chilean Ministry of Education told us.

[51] The utility normalization is such that the unit of measurement of utility is the standard deviation of the achievement test score at the end of high school. Therefore, we are assuming that the utility derived from predicted persistence as opposed to predicted dropout is the same as that derived from having achievement that is larger by one standard deviation.

[52] To do so, we calculate the difference between each student's elicited cutoff and the rational expectations cutoff in the baseline scenario (which we simulate), i.e., the belief bias over the rational expectations cutoff. In the counterfactual, we build the believed cutoff as the sum between the rational expectations cutoff at the counterfactual effort distribution (which we simulate) and the belief bias over the rational expectations cutoff.

8 Conclusions

We use an innovative randomized control trial and a comprehensive longitudinal dataset matching detailed administrative records with a data collection in schools developed specifically for this study to provide the first evidence on the impacts of college admission policies targeted at the very disadvantaged. The PACE policy in Chile eliminated the entrance exam requirement for students graduating in the top 15 percent of their school, and it targeted students who score 1.5 standard deviations below regular entrants on 10^{th} grade standardize tests, and who are considerably disadvantaged.

We present several novel findings from this unprecedented empirical setting. This paper focuses on impacts on education outcomes during high school and up to five years after leaving high school. We document that PACE increased college admission and first-year enrollment by 36 percent, but the impacts on continuous enrollment or graduation in the fifth year were around a third of the impacts in the first year. We also show that PACE had negative impacts on pre-college effort and GPA in core subjects (Mathematics, language), dimensions of pre-college human capital that independently predict persistence in college. The experimental research design allows us to examine policy impacts away from admission cutoffs, and we find that the effort impacts are widespread along the baseline ability distribution. Using novel survey data on the beliefs that students have about their entrance exam scores and GPA rank, we show that such evidence is most consistent with students reducing their effort in high school because they perceived that PACE undercut their incentive to exert effort to obtain a college admission. In fact, by matching students' expected entrance exams and GPA rank with administrative records, we document that students of all absolute and relative (within-school) abilities display large belief biases.

Together, the reduced-form findings suggest that college preparedness matters for the impacts of large admission advantages that reach severely disadvantaged populations, and that college preparedness is not fixed by late adolescence, it responds to effort investments made in the last high school year. This suggests that school interventions designed to shape pre-college effort investments could improve the persistence of the impacts of large admission advantages. The large belief biases we documented also suggest a margin for policy intervention. But without more structure, it is difficult to quantify how pre-college effort investments shape the college preparedness of those who self-select into college. Therefore, we develop and structurally estimate a dynamic structural model that allows us to perform the ex-ante evaluation of informational school interventions designed to shape pre-college effort investments and the college preparedness of college entrants.

The model extends the structural literature modelling admission policies by endogenizing pre-college effort and allowing for biased pre-college beliefs about ability. The model-based results suggest that correcting misperceptions would be effective at improving the college preparedness of college entrants: it would lead to a more positive selection of college entrants, and increase the pre-college effort of those who self-select into college. Informing students of the importance of pre-college effort for college persistence, instead, would have more modest impacts on the college preparedness of college entrants, and it would increase pre-college investments also among overly optimistic students who expect to enter college but who do not get admitted, with ambiguous welfare implications.

This study is the first to examine the impacts of context-based admission advantages on a very disadvantaged population, and it finds that they can improve college enrollment and persistence up to five years, when our data end. Our results can serve as a starting point for discussions about the optimal design of context-based admissions and suggest that such policies can improve the college attainment of students further down the academic preparedness distribution than previously found. Future studies should explore the labor market impacts of PACE. Our results, however, also highlighted challenges that may be specific to these school populations. We documented large biases in beliefs about absolute and relative ability and argue they interacted with policy effectiveness. Directly comparing these findings with other contexts is difficult because data on the beliefs of high school students targeted by admission policies are rarely collected. But our results suggest that policymakers wanting to expand admissions to more disadvantaged populations should reckon with the reality of the school environments such policies would encounter, and consider pairing the admission rules with tailored school interventions.

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Online Appendix

College Access When Preparedness Matters: New Evidence from Large Advantages in College Admissions

Michela M. Tincani, Fabian Kosse, and Enrico Miglino

May 15, 2023

A Fieldwork Information

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

Students filled out paper questionnaires. Schools allowed us to administer our survey during class time. Our survey displaced one lecture. It took students approximately 50 minutes to fill out the questionnaire. At the start of the data collection, fieldworkers explained that they would take an achievement test for the first 20 minutes, and that they would be entered into a lottery to win an iPad, with the number of lottery tickets determined by the number of correct answers.[53] At the 20-minute mark, fieldworkers told students to stop working on the achievement test and to proceed to the survey part of the questionnaire. If a student completed the achievement test before the 20 minutes were up, she was allowed to proceed to the survey.

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

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

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

B Additional Figures

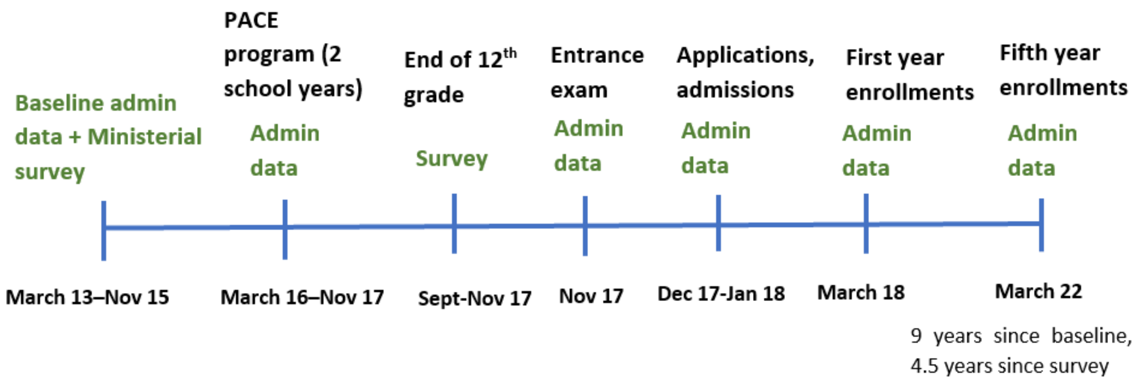


Figure A1: Timeline. Two-digit numbers refer to years (e.g. 13 means year 2013).

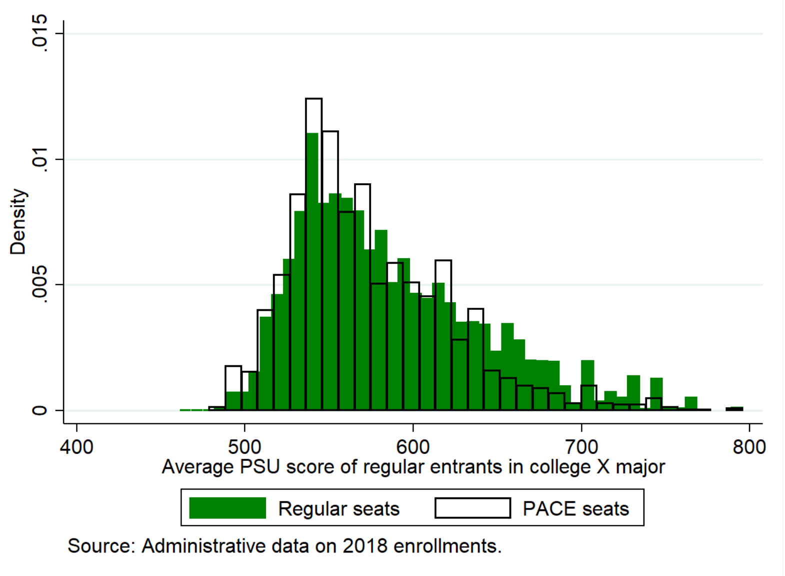


Figure A2: Quality distribution of PACE and regular college seats.

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

Figure A3: Selected survey questions.

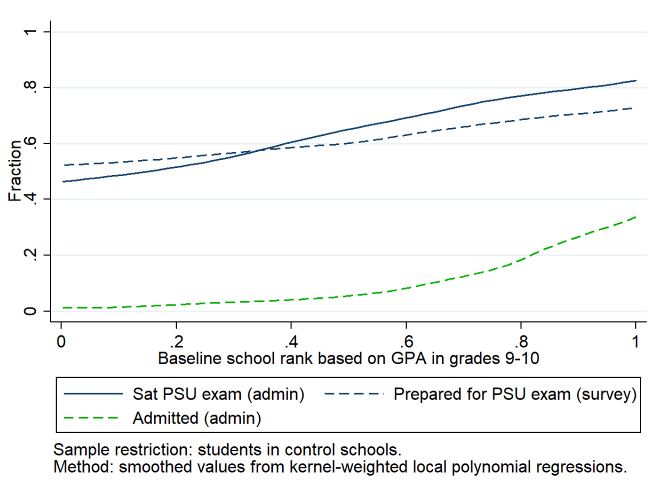
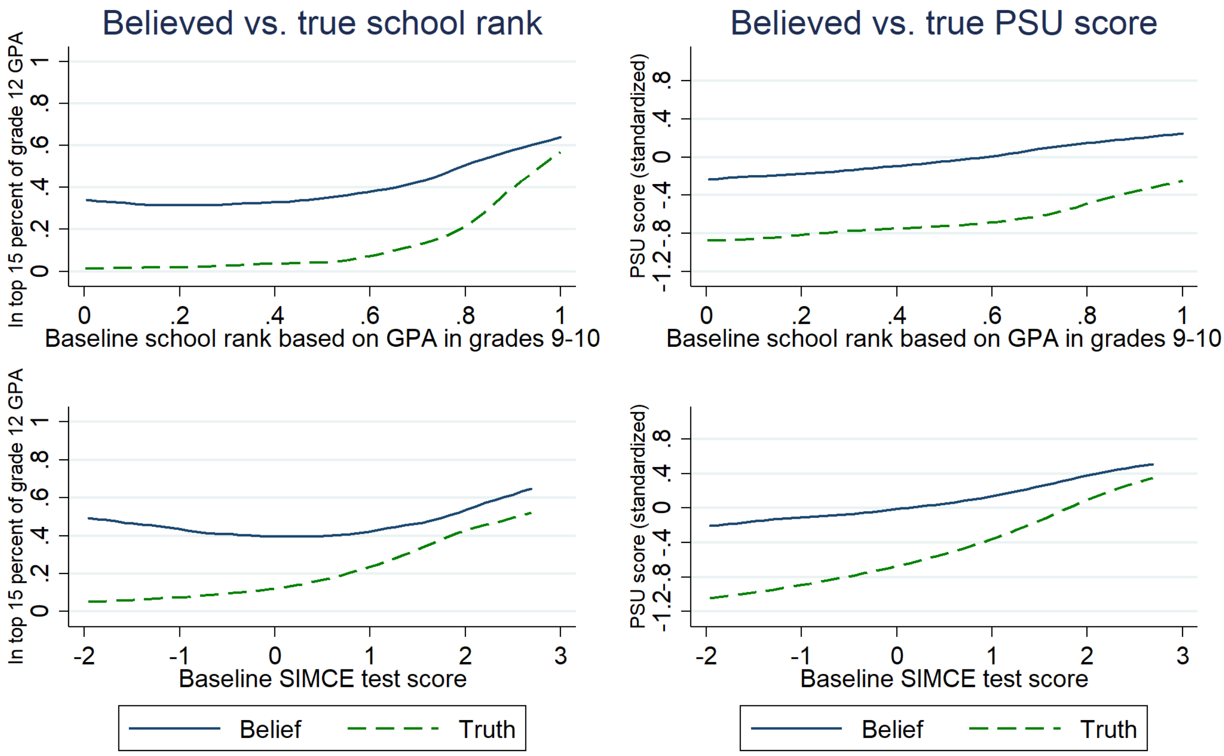


Figure A4: Decision to take and prepare for PSU entrance exam and objective admission likelihood.



Sample restriction: students in control schools.
Bottom panels trim students in top and bottom 1% of SIMCE distribution.
Method: smoothed values from kernel-weighted local polynomial regressions.

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

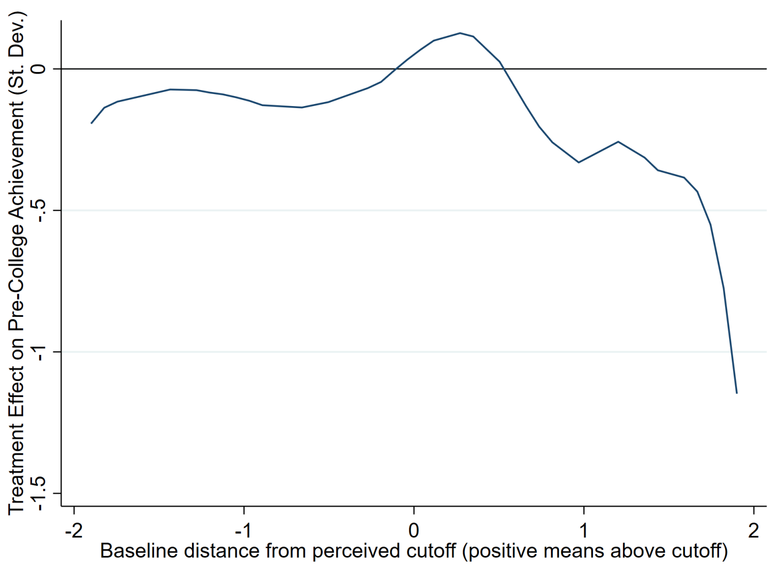


Figure A6: Suggestive evidence of a response to perceived incentives.

C Robustness analysis

Survey attrition. The response rate in our survey data is 69.4% percent in the control group, and it is not statistically significantly different in the treatment group, suggesting the absence of selective attrition. Table A1 presents Lee, 2009 bounds for the treatment effects, confirming that the estimated treatment effects are not due to selective attrition.

Table A1: 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 pre-college achievement and effort. In the first and second rows we use residuals from a regression of the outcomes on baseline test scores as the dependent variable. In the third and fourth rows we use the raw outcome variables. In all rows we scale the outcomes as in Table 4, to keep our analysis of bounds analogous to the main average treatment effects.

D Additional Tables

Table A2: Baseline characteristics of all students and of those targeted by the PACE policy

	All students	Targeted students
	(1)	(2)
Very low SES	0.40	0.61
Mother's education (years)	11.49	9.60
Father's education (years)	11.43	9.38
Family income (1,000 CLP)	600.10	291.66
SIMCE score (standardized)	0.00	-0.60
Rural resident	0.03	0.03
Santiago resident	0.30	0.17

Source.– SIMCE and SEP administrative data on 10^{th} graders in 2015. Note.– Very low SES indicates a student that the government classified as socioeconomically vulnerable (*Alumno 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 A3: Pre-College Academic Preparedness Predicts Persistence in College

	College persistence or graduation five years after high school graduation			
	(1)	(2)	(3)	(4)
GPA in 12^{th} grade (standardized)	0.104***			
	(0.020)			
GPA in 12^{th} grade, subjects tested on PSU (standardized)		0.100***		
		(0.020)		
GPA in 12^{th} grade, subjects not tested on PSU (standardized)		0.007		
		(0.026)		
PSU score (standardized)	0.031	0.071		
	(0.042)	(0.048)		
Study effort in last high school year (standardized)			0.062***	
			(0.022)	
Hours of study per week in last high school year				0.015**
				(0.006)
Baseline test score in 10^{th} grade (standardized)	0.023	-0.013	0.060**	0.058**
	(0.028)	(0.030)	(0.022)	(0.023)
Observations	1,015	741	737	750
R^2	0.061	0.064	0.048	0.048

Note. – Sample of students who enrolled in a selective college in the first year. The outcome variable is a dummy equal to one if five years later they are either still continuously enrolled or they have graduated, and zero otherwise. Results from OLS regressions. Inverse Probability Weights are used in columns (3) and (4). Standard set of control variables used: age, gender, very-low-SES, never failed a year, type of high school track (academic or vocational). The baseline test score is standardized in the population of students taking the SIMCE exam, the PSU is standardized in the population of exam takers. Standard errors in parentheses, clustered at school level. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

Table A4: Effects of PACE on College Applications and Admissions

	(1)	(2)	(3)	(4)	(5)	(6)
	All Sample		Bottom 85%		Top 15%	
	Applications	Admissions	Applications	Admissions	Applications	Admissions
Treatment	0.019	0.041***	0.000	0.011	0.147***	0.225***
	(0.019)	(0.012)	(0.018)	(0.009)	(0.037)	(0.030)
Control group mean	0.210	0.114	0.161	0.070	0.450	0.328
Observations	8,944	8,944	7,061	7,061	1,563	1,563
R^2	0.176	0.206	0.109	0.122	0.209	0.257

Note.– Columns (1) and (2) use the sample of all students in the experiment. Columns (3) and (4) use the sample of all students who at the end of 10^{th} grade, before the experiment started, were in the bottom 85% of their school according to GPA in the first two high school years. Columns (5) and (6) use the sample of all students who at the end of 10^{th} grade, before the experiment started, were in the top 15% of their school according to GPA in the first two high school years. The share of students in the top 15% at baseline is 18% because there are students with the same GPA average at baseline. "Control group mean" is the mean of the dependent variable in the control group (i.e., absent PACE). Results from OLS regressions. Treatment is a dummy equal to 1 if a school was randomly assigned to be in the PACE treatment, to 0 otherwise. All regressions use the standard set of controls (see notes under Figure 2). Standard errors clustered at the school level in parenthesis. *p < 0.10; **p < 0.05; ***p < 0.01.

Table A5: Effects of PACE on Continuous Enrollment or Graduation Over Time, All Sample

	Year 1	Year 2	Year 3	Year 4	Year 5
	A. Continuous enrollment in or graduation from college				
Treatment	0.031***	0.023**	0.020**	0.017**	0.011
	(0.011)	(0.010)	(0.008)	(0.008)	(0.008)
Observations	8944	8944	8944	8944	8944
R^2	0.172	0.133	0.124	0.116	0.110
	B. Continuous enrollment in or graduation from vocational HE institute				
Treatment	-0.024	-0.020	-0.016	-0.013	-0.017**
	(0.018)	(0.015)	(0.012)	(0.010)	(0.008)
Observations	8944	8944	8944	8944	8944
R^2	0.006	0.004	0.003	0.004	0.008
	C. Continuous enrollment in or graduation from non-selective college				
Treatment	-0.014	-0.011	-0.008	-0.010	-0.010*
	(0.012)	(0.009)	(0.007)	(0.006)	(0.006)
Observations	8944	8944	8944	8944	8944
R^2	0.005	0.007	0.008	0.009	0.010
	D. Continuous enrollment in or graduation from HE outside options				
Treatment	-0.038*	-0.031*	-0.024*	-0.022**	-0.027***
	(0.021)	(0.016)	(0.012)	(0.011)	(0.009)
Observations	8944	8944	8944	8944	8944
R^2	0.005	0.006	0.006	0.009	0.013
	E. Continuous enrollment in or graduation from any HE institute				
Treatment	-0.007	-0.008	-0.004	-0.005	-0.016
	(0.022)	(0.017)	(0.013)	(0.012)	(0.011)
Observations	8944	8944	8944	8944	8944
R^2	0.060	0.061	0.066	0.071	0.074

Note.–: Sample of all students in the experiment. Results from OLS regressions. Treatment is a dummy equal to 1 if a school was randomly assigned to be in the Treatment treatment, to 0 otherwise. All regressions use the standard set of controls (see notes under Figure 2). Standard errors clustered at the school level in parenthesis. HE stands for higher education. The HE outside options are vocational HE institutes and non-selective colleges. The construction of the outcome variable is explained in the notes under Figure 2. *p < 0.10; **p < 0.05; ***p < 0.01.

Table A6: Effects of PACE on Continuous Enrollment or Graduation Over Time, Sample of Those in the Top 15% of Their School at Baseline

	Year 1	Year 2	Year 3	Year 4	Year 5
		A. Continuous enrollment in or graduation from college			
Treatment	0.168***	0.132***	0.109***	0.094***	0.075***
	(0.029)	(0.026)	(0.025)	(0.025)	(0.023)
Observations	1563	1563	1563	1563	1563
R^2	0.210	0.169	0.161	0.154	0.155
		B. Continuous enrollment in or graduation from vocational HE institute			
Treatment	-0.042	-0.041	-0.024	-0.034	-0.032*
	(0.030)	(0.026)	(0.025)	(0.021)	(0.017)
Observations	1563	1563	1563	1563	1563
R^2	0.046	0.032	0.023	0.020	0.019
		C. Continuous enrollment in or graduation from non-selective college			
Treatment	-0.059***	-0.048***	-0.039***	-0.036***	-0.035***
	(0.019)	(0.015)	(0.014)	(0.013)	(0.012)
Observations	1563	1563	1563	1563	1563
R^2	0.014	0.013	0.010	0.010	0.012
		D. Continuous enrollment in or graduation from HE outside options			
Treatment	-0.102***	-0.089***	-0.063**	-0.071***	-0.067***
	(0.032)	(0.027)	(0.026)	(0.022)	(0.018)
Observations	1563	1563	1563	1563	1563
R^2	0.046	0.031	0.018	0.019	0.021
		E. Continuous enrollment in or graduation from any HE institute			
Treatment	0.067**	0.043	0.046*	0.023	0.008
	(0.030)	(0.027)	(0.027)	(0.026)	(0.026)
Observations	1563	1563	1563	1563	1563
R^2	0.070	0.054	0.064	0.077	0.088

Note.–: Sample of all students who at the end of 10^{th} grade, before the experiment started, were in the top 15% of their school according to GPA in the first two high school years. Results from OLS regressions. Treatment is a dummy equal to 1 if a school was randomly assigned to be in the PACE treatment, to 0 otherwise. All regressions use the standard set of controls (see notes under Figure 2). Standard errors clustered at the school level in parenthesis. HE stands for higher education. The HE outside options are vocational HE institutes and non-selective colleges. The construction of the outcome variable is explained in the notes under Figure 2. *p < 0.10; **p < 0.05; ***p < 0.01.

Table A7: EFFECTS OF PACE ON CONTINUOUS ENROLLMENT OR GRADUATION OVER TIME, SAMPLE OF THOSE IN THE BOTTOM 85% OF THEIR SCHOOL AT BASELINE

	Year 1	Year 2	Year 3	Year 4	Year 5
	A. CONTINUOUS ENROLLMENT IN OR GRADUATION FROM COLLEGE				
Treatment	0.010	0.005	0.008	0.005	0.002
	(0.015)	(0.011)	(0.009)	(0.008)	(0.008)
	(0.009)	(0.007)	(0.006)	(0.006)	(0.006)
Observations	7061	7061	7061	7061	7061
R^2	0.097	0.070	0.062	0.054	0.049
	B. CONTINUOUS ENROLLMENT IN OR GRADUATION FROM VOCATIONAL HE INSTITUTE				
Treatment	-0.019	-0.017	-0.015	-0.008	-0.014
	(0.020)	(0.016)	(0.013)	(0.011)	(0.009)
Observations	7061	7061	7061	7061	7061
R^2	0.003	0.005	0.005	0.005	0.009
	C. CONTINUOUS ENROLLMENT IN OR GRADUATION FROM NON-SELECTIVE COLLEGE				
Treatment	-0.004	-0.003	-0.002	-0.004	-0.005
	(0.012)	(0.008)	(0.007)	(0.006)	(0.005)
Observations	7061	7061	7061	7061	7061
R^2	0.004	0.007	0.007	0.009	0.009
	D. CONTINUOUS ENROLLMENT IN OR GRADUATION FROM HE OUTSIDE OPTIONS				
Treatment	-0.023	-0.020	-0.017	-0.012	-0.019*
	(0.022)	(0.017)	(0.013)	(0.011)	(0.010)
Observations	7061	7061	7061	7061	7061
R^2	0.004	0.008	0.009	0.010	0.015
	E. CONTINUOUS ENROLLMENT IN OR GRADUATION FROM ANY HE INSTITUTE				
Treatment	-0.014	-0.014	-0.010	-0.006	-0.016
	(0.022)	(0.017)	(0.013)	(0.012)	(0.011)
Observations	7061	7061	7061	7061	7061
R^2	0.030	0.035	0.036	0.036	0.038

NOTE.–: Sample of all students who at the end of 10^{th} grade, before the experiment started, were in the bottom 85% of their school according to GPA in the first two high school years. Results from OLS regressions. Treatment is a dummy equal to 1 if a school was randomly assigned to be in the PACE treatment, to 0 otherwise. All regressions use the standard set of controls (see notes under Figure 2). Standard errors clustered at the school level in parenthesis. HE stands for higher education. The HE outside options are vocational HE institutes and non-selective colleges. The construction of the outcome variable is explained in the notes under Figure 2. *p < 0.10; **p < 0.05; ***p < 0.01.

Table A8: 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 Figure 2), 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 A9: Socioeconomic correlates of belief biases

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

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

Table A10: Validating Achievement and Effort Measures

	Sit PSU	Apply	Admitted	Enroll year 1	Enroll year 2	Enroll year 3	Enroll year 4	Enroll year 5
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	A. Achievement							
Achievement	0.060***	0.074***	0.057***	0.047***	0.039***	0.035***	0.032***	0.030***
	(0.009)	(0.008)	(0.008)	(0.008)	(0.007)	(0.006)	(0.006)	(0.006)
PSU score	No	No	No	No	No	No	No	No
Dep. var. mean	0.725	0.241	0.131	0.099	0.078	0.069	0.065	0.060
Observations	2,922	2,922	2,922	2,922	2,922	2,922	2,922	2,922
Pseudo-R^2	0.100	0.169	0.283	0.290	0.265	0.261	0.252	0.246
		B. Achievement, controlling for PSU score						
Achievement		0.037**	0.017**	0.015**	0.014**	0.013*	0.010	0.010
		(0.012)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
PSU score		Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dep. var. mean		0.333	0.183	0.136	0.107	0.095	0.089	0.083
Observations		2,122	2,122	2,122	2,122	2,122	2,122	2,122
Pseudo-R^2		0.238	0.556	0.504	0.425	0.401	0.394	0.380
	C. Study effort							
Study Effort	0.056***	0.069***	0.045***	0.037***	0.031***	0.030***	0.030***	0.029***
	(0.010)	(0.009)	(0.006)	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)
PSU score	No	No	No	No	No	No	No	No
Dep. var. mean	0.731	0.244	0.136	0.101	0.080	0.071	0.066	0.062
Observations	2,746	2,746	2,746	2,746	2,746	2,746	2,746	2,746
Pseudo-R^2	0.096	0.163	0.255	0.262	0.238	0.240	0.237	0.233
		D. Study effort, controlling for PSU score						
Study Effort		0.055***	0.018**	0.017**	0.016**	0.018**	0.019**	0.020***
		(0.010)	(0.007)	(0.007)	(0.007)	(0.009)	(0.008)	(0.008)
PSU score		Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dep. var. mean		0.334	0.186	0.138	0.109	0.097	0.091	0.085
Observations		2,010	2,010	2,010	2,010	2,010	2,010	2,010
Pseudo-R^2		0.241	0.550	0.501	0.425	0.401	0.397	0.384

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 Figure 2) 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 A8. *p < 0.10; **p < 0.05; ***p < 0.01.

Table A11: Validating Belief Measures

	Sit PSU	Apply	Enroll year 1	Enroll year 2	Enroll year 3	Enroll year 4	Enroll year 5
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	A. Believed PSU score						
Believed PSU score	0.048***	0.086***	0.066***	0.049***	0.042***	0.042***	0.041***
	(0.010)	(0.010)	(0.008)	(0.007)	(0.007)	(0.007)	(0.006)
PSU score	No	No	No	No	No	No	No
Dep. var. mean	0.768	0.272	0.113	0.089	0.079	0.073	0.069
Observations	2,401	2,401	2,401	2,401	2,401	2,401	2,401
Pseudo-R^2	0.089	0.161	0.287	0.252	0.249	0.246	0.245
	B. Believed PSU score, controlling for PSU score						
Believed PSU score		0.065***	0.036***	0.025***	0.020***	0.023***	0.025***
		(0.012)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)
PSU score		Yes	Yes	Yes	Yes	Yes	Yes
Dep. var. mean		0.354	0.147	0.116	0.102	0.095	0.089
Observations		1,848	1,848	1,848	1,848	1,848	1,848
Pseudo-R^2		0.239	0.505	0.421	0.396	0.393	0.381
	C. Δ Believed (GPA-cutoff), control group						
Δ Believed (GPA-cutoff)	0.000	−0.003	−0.004	0.002	0.002	0.002	0.004
	(0.011)	(0.011)	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)
Believed PSU score	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dep. var. mean	0.781	0.286	0.121	0.095	0.084	0.079	0.074
Observations	2,170	2,170	2,170	2,170	2,170	2,170	2,170
Pseudo-R^2	0.085	0.159	0.283	0.250	0.247	0.246	0.246
	D. Δ Believed (GPA-cutoff), treatment group						
Δ Believed (GPA-cutoff)	0.006	0.040***	0.059***	0.052***	0.047***	0.045***	0.038***
	(0.010)	(0.011)	(0.008)	(0.007)	(0.006)	(0.006)	(0.006)
Believed PSU score	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dep. var. mean	0.746	0.300	0.189	0.149	0.134	0.122	0.107
Observations	2,240	2,240	2,240	2,240	2,240	2,240	2,240
Pseudo-R^2	0.149	0.200	0.237	0.227	0.240	0.236	0.241

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 Figure 2) and Inverse Probability Weights. Sample restriction: students in control schools in Panels A-C, students in treatment schools in Panel D. 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. Δ Believed (GPA-cutoff) is the difference between the perceived own GPA and the perceived top 15% cutoff. *p < 0.10; **p < 0.05; ***p < 0.01.

Table A12: Treatment Effects on Teachers' Effort and Focus of Instruction

	(1)	(2)	(3)	(4)	(5)	(6)
	Effort (Prep Hours)		Effort (Absences)		Focus of Instruction	
	Mathematics	Language	Mathematics	Language	Mathematics	Language
Treatment	0.024	0.247	0.308	0.152	0.033	0.021
	(1.253)	(0.449)	(1.371)	(1.000)	(0.033)	(0.028)
Observations	271	316	271	316	271	316

Note.– Results from OLS regressions. The unit of observations are classrooms (there are one Mathematics and one Language teacher per classroom). The construction of the focus of instruction variable is described in section E.1 below. It ranges from 0 to 1 and higher values indicate targeting higher-ability students. Absences from work are measured in days per year. Standard errors in parentheses. Treatment is a dummy equal to 1 if a school is randomly allocated to have PACE, and equal to 0 otherwise. $* < 0.10$; $** < 0.05$; $* * * < 0.01$.

Table A13: Survey answers to hypothetical effort questions

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

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

Table A14: Parameters estimated outside of the model

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

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

Table A15: Parameter Estimates

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

Note. – Standard Errors bootstrapped using 50 bootstrap samples. Lagged test score standardized in the estimation sample. $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A16: Model Fit

	Sample	Data	Simulations	Targeted?
A. Descriptive Statistics				
Took college entrance exam	Control	0.655	0.491	Yes
College entrance exam score | took exam	Control	-0.601	-0.768	Yes
Admitted to college	Control	0.114	0.063	No
Enrolled in college	Control	0.085	0.048	No
B. Treatment Effects				
Achievement Test Score	All	-0.121	-0.072	Yes
Study hours	All	-0.258	-0.264	Yes
Admissions	All	0.041	0.042	No
Enrollments	All	0.031	0.021	Yes
Entrance-exam taking	All	-0.041	-0.013	Yes
C. Beliefs				
Believed minus actual entrance exam score | took exam	Control	0.591	0.609	No
Believed minus actual 12^{th} grade GPA	Control	-0.075	-0.060	No
Believes is in top 15% of school	Control	0.431	0.376	No
Perceived returns to effort, GPA	All	0.177	0.123	Yes
Perceived returns to effort, entrance exam	All	0.299	0.188	Yes
D. Dynamics				
Correlation(take entrance exam, enroll in college)	All	0.265	0.270	No
Correlation(admitted to college, enroll in college)	All	0.849	0.820	No
Correlation(academic high school track, enroll in college)	All	0.193	0.101	No
Correlation(baseline test scores, enroll in college)	All	0.392	0.308	No
12^{th} grade GPA of college entrants	Control	6.24	6.23	No
12^{th} grade GPA of college entrants	Treatment	6.27	6.27	No
12^{th} grade GPA of college entrants	All	6.26	6.25	No

Note. – The last column identifies statistics that were directly targeted in estimation and statistics that were not. Perceived returns to effort are the expected change in outcome for an additional hour of study per week in the semester. Expected entrance exam score is measured in standard deviations of the exam scores; expected and actual GPA are measured on a scale from 1 to 7. Study hours are measured in reported study hours per week in the semester. The treatment effects are obtained from OLS regressions that do not use fieldworker fixed effects.

E Additional Details on Analysis of Mechanisms

E.1 Construction of Teacher Variables

This Section explains how we constructed the teacher variables that enter Table A12 from the survey data that we collected among the Mathematics and Language teachers of the students in our sample.

Teacher effort. For each teacher we observe the hours the teacher spends to prepare his/her classes, and the number of days the teacher was absent from school.

Teacher's focus of instruction. This variable measures whether the teacher is targeting his/her teaching to a specific part of the student ability distribution.

For Mathematics and Language teachers separately we construct a variable indicating the difficulty level at which the teacher is teaching using survey questions about how much of various components of the curriculum the teacher covered during the term, coupled with the teacher's assessment of the difficulty level of each component. For example, for Mathematics we present the teacher with a list of the 4 subfields taken from the official national curriculum ("Algebra and Functions", "Geometry", "Statistics and Probability", "Trigonometry"), and for each subfield we present the teacher with a list of topics taken from the official national curriculum (for example, for "Algebra and Functions" two topics are "logarithmic and exponential function and analysis of their graphs" and "solution of second degree equations"). In all, we presented Mathematics teachers with 13 topics and Language teachers with 11 topics. For each topic, we first ask the teacher what percentage he/she was able to cover during the first semester (which was over when the data collection started). Second, we ask the teacher to think of the average student in his/her 12^{th} grade class, and tell us whether he/she thinks that this student would find the topic easy or difficult to understand. The answers to these questions were collected as 5-point Likert scales. Finally, we multiply the coverage and difficulty within each Mathematics (Language) topic and sum over all topics.

E.2 Beliefs over Returns to College Degree

Our survey included the survey instruments developed in Attanasio and Kaufmann, 2014 to elicit students beliefs about returns to a college degree. We elicited beliefs about the distribution of wages at age 30 with and without a college degree. We find that students think that, on average, the return to a college degree is 200 percent. This is in line with observed differences in wages between Chileans with and without a college degrees, and in line with results from other surveys on different samples of Chilean high-school students (Hastings, Neilson, Ramirez, and Zimmerman, 2016).

We found that the treatment did not have any impact on student beliefs about returns to education (no impacts on the mean nor on the variance of the returns), as reported in Table A17.

Table A17: Treatment effect on mean and variance of subjective earnings distributions at age 30, with and without a college degree.

	Expected Earnings (Elicited)		Expected Earnings (Estimated)		Variance of Earnings (Estimated)	
	Without	With	Without	With	Without	With
Treatment	-0.005	-0.004	-0.108	-0.102	-0.005	0.069
	(0.010)	(0.014)	(0.066)	(0.065)	(0.024)	(0.062)
Observations	3,339	2,048	4,219	2,674	4,219	2,674
R-squared	0.094	0.057	0.016	0.013	0.000	0.001

Note.– Standard errors clustered at school level. Inverse probability weights used. Expected earnings measured in million CLP. Variance measured in million CLP squared. Variance regressions are median regressions. "Without" means without a college degree. "With" means with a college degree. *Treatment* is a dummy variable indicating whether a student is in a school that was randomly assigned to be in the PACE program. Standard set of controls (gender, age, *Prioritario* student, SIMCE, never failed a year, school track). Significance: *p < 0.10; **p < 0.05; ***p < 0.01.

Expected (mean) earnings were directly elicited, and we also estimated them, together with the variance of earnings, from elicited c.d.f. values. We report results on both measures of expected earnings, for comparison.

The survey questions asked "How much do you expect to earn per month with (without) a college degree on average?" and "How likely are you to earn at least X pesos per month with (without) a college degree?" where X=200.000, 800.000 without a degree and X=300.000, $1,000.000$ with a degree. To calculate the mean and variance of expected earnings using the answers to these questions, we fit the reported c.d.f. values using log-normal distributions for each respondent in the sample. In the estimation sample we kept only the students that answered at least two questions for each scenario (with and without a degree), because we needed at least two c.d.f. values to estimate the mean and variance of the Log-normal distribution. Finally, we used the Generalized Method of Moments to find the mean and variance of the log-normal distribution that minimize the distance of the simulated mean and simulated c.d.f. values from their data analogues.

For variance regressions we use median regressions because the variance is very vulnerable to outlier survey responses in which a student gives the same probability to the likelihood that his/her earnings at age 30 will be above two different values.

F Technical Appendix

F.1 Structural model parameterizations

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

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

$$PSU_i = \beta_0^P + \beta_1^P e_i + \beta_2^P y_{i,t-1}^{(1)} + \epsilon_i^P, \tag{19}$$

$$GPA_i = \beta_0^G + \beta_1^g e_i + \beta_2^G y_{i,t-1}^{(2)} + \epsilon_i^G, \tag{20}$$

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

$$Pr(A_i^R = 1 | PSU_i, S_i = 1; \gamma) = \Phi(\gamma_0 + \gamma_1 PSU_i). \tag{21}$$

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

$$PSU_{it}^b = \beta_{0k_i}^{Pb} + \beta_{1k_i}^{Pb} e_{it} + \beta_2^{Pb} y_{it-1}^{(1)} + \epsilon_{it}^{PSU^b}, \quad \epsilon_{it}^{PSU^b} \sim N(0, \sigma_{PSU^b}^2) \tag{22}$$

$$GPA_{it}^b = \beta_0^{Gb} + \beta_{1k_i}^{Gb} e_{it} + \beta_2^{Gb} y_{it-1}^{(2)} + \epsilon_{it}^{GPA^b}, \quad \epsilon_{it}^{GPA^b} \sim N(0, \sigma_{GPA^b}^2) \tag{23}$$

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

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

$$\begin{aligned} Pr^b(A_i^R = 1 | e_{it}, y_{it-1}^{(1)}, k_i, S_i = 1) &= Pr\left(\overline{PSU}_{it}^b + \epsilon_i^{PSU^b} \geq \bar{c}^{Rb} + \epsilon_i^{c^{Rb}}\right) \\ &= \Phi\left(\frac{\overline{PSU}_{it}^b - \bar{c}^{Rb}}{\sqrt{\sigma_{PSU^b}^2 + \sigma_{c^{Rb}}^2}}\right) \\ &= \Phi\left(\gamma_0^b + \gamma_1^b \overline{PSU}_{it}^b\right), \end{aligned} \tag{24}$$

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

Letting $\overline{GPA}_{it}^b = \beta_0^{Gb} + \beta_{1k_i}^{Gb} e_{it} + \beta_2^{Gb} y_{it-1}^{(2)}$ denote the expected GPA, ϵ_i^{c15b} the mean-zero belief shock around the expected school cutoff[54], 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:

$$\begin{aligned} Pr^b(A_i^P = 1 | e_{it}, y_{it-1}^{(2)}, k_i, S_i = 1) &= Pr\left(\overline{GPA}_{it}^b + \epsilon_i^{GPA^b} \geq c_0 + c\bar{1}5_i^b + \epsilon_i^{c15b}\right) \\ &= \Phi\left(\frac{\overline{GPA}_{it}^b - c_0 - c\bar{1}5_i^b}{\sqrt{\sigma^2_{GPA^b} + \sigma^2_{c15^b}}}\right) \\ &= \Phi\left(\xi_0^b + \xi_1^b(\overline{GPA}_{it}^b - c\bar{1}5_i^b)\right), \end{aligned} \tag{25}$$

where $\xi_0^b = \frac{-c_0}{\sqrt{\sigma^2_{GPA^b}+\sigma^2_{c15^b}}}$ and $\xi_1^b = \frac{1}{\sqrt{\sigma^2_{GPA^b}+\sigma^2_{c15^b}}}$.[55] Given an expected GPA and an expected cutoff, uncertainty is generated by the uncertainty around own GPA ($\sigma^2_{GPA^b}$) and around the school cutoff ($\sigma^2_{c15^b}$), which are absorbed by the parameters ξ_0^b and ξ_1^b. As before, we assume normality.

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

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

$$q^R(PSU_i) = \lambda_0^R + \lambda_1^R PSU_i + \epsilon_i^{qR} \tag{26}$$

$$q^P(GPA_i) = \lambda_0^P + \lambda_1^P GPA_i + \epsilon_i^{qP}. \tag{27}$$

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

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

F.2 Additional identification details

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

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

Type-dependent heterogeneity in the utility from enrolling in college. Observationally identical students who face identical admission sets can make different enrollment decisions because of idiosyncratic preference shocks and because of permanent unobserved heterogeneity. To separately identify them we exploit the longitudinal aspect of our data. We observe student's preference-revealing choices at both the exam-taking decision stage and the enrollment stage. Unlike temporary preference shocks, permanent unobserved heterogeneity induces correlations in behavior over time, which allow us to pin down unobserved heterogeneity in the preference for college.

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

Second, we discuss how we mitigate potential endogeneity of the arguments of the subjective probability functions. For the subjective probability of a preferential admission, we use variation that comes from the experiment. The treatment makes this subjective probability salient:

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

F.3 Auxiliary Regressions and Moments

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

1. *Treatment Effect Regressions:*

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

2. *Descriptive Regressions:*

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

- Coefficient on the average between 9^{th} and 10^{th} grade GPA in a regression of study hours on this variable and on the standard controls (1).

3. *Descriptive Statistics:*

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

- Mean and variance of expected GPA and PSU score (4).

F.4 Equilibrium of the Tournament Game in the Counterfactual

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

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

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

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

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

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

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

Intuition for approximation. Solving for the rational expectations equilibrium requires solving for a multi-dimensional fixed point in the vector of decision rules in each school. To reduce the dimensionality of the problem, we find an approximation to the rational expectations equilibrium.[56] 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

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

through this probability that the strategies of others enter own payoffs. We posit a parametric approximation to this probability, $\check{P}^{15}(e_i, \gamma)$, where γ captures the strategy profiles of students $-i$. Let $\check{u}_i(e_i(\cdot), k_i, \check{P}^{15}(e_i, \gamma))$ denote i's approximated expected payoff.

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

- *given γ^*, each i and k_i chooses a decision rule $\check{e}_i(k_i)$ that maximizes his/her approximated expected payoff:*
$$\check{u}_i(\check{e}_i(k_i), k_i, \check{P}^{15}(\check{e}_i, \gamma^*)) \geq \check{u}_i(e_i^{'}(\cdot), k_i, \check{P}^{15}(e_i^{'}, \gamma^*))$$
for every $i = 1, 2, ..., n$, $k_i = 1, 2, ...K$ and for all $e_i^{'} \in \{0, 1, 2, ..., E\}$.
- *given the profile of decision rules $(\check{e}_1(k_1), \check{e}_2(k_2), ..., \check{e}_n(k_n))$, the approximated admission probability is close to the true admission probability for all i: $P_i^{15}(\check{e}_i, \check{e}_{-i}(\cdot)) \approx P^{15}(\check{e}_i, \gamma^*)$ $\forall i = 1, ..., n$.*

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

1. Draw types and shocks for all students and fix these draws across iterations.
2. From the data, estimate a linear probability model of the likelihood of a preferential admission as a function of own GPA and of baseline characteristics of the student (X_i) and of the school (Z_j) selected through LASSO:
$$Prob_i(Adm^P = 1 | GPA_{it}, X_i, Z_j) = \gamma_0 + \gamma_1 GPA_{it} + \gamma_2 X_i + \gamma_3 Z_j + \epsilon_{ij}$$
Let the estimates $\hat{\gamma}_0, \hat{\gamma}_2, \hat{\gamma}_3$ be fixed across iterations, let the estimate $\hat{\gamma}_1$ be our first guess in all schools: $\gamma_{1j}^{(s=0)}$. The goal is to find a fixed point in γ_{1j}.
3. At the current iteration s, let students believe that
$$\begin{aligned} P_i^{15(s)}(e_i, \check{e}_{-i}(\cdot)) &= P_i^{(s)} = \\ &= \hat{\gamma}_0 + \gamma_{1j}^{(s)} GPA_{it}(e_i; \epsilon_{it}^G) + \hat{\gamma}_2 X_i + \hat{\gamma}_3 Z_j. \end{aligned}$$
4. Given these beliefs, find the best reply of each student. Let $e_{it}^{(s)}$ be the utility maximizing effort that each student exerts.
5. Calculate $GPA_{it}^{(s)} = GPA(e_{it}^{(s)}; \epsilon_{it}^G)$. Assign PACE slots to those with a GPA in the top 15 percent of their school and who took the entrance exam.

6. From the simulated data on PACE slot allocations and $GPA(e_{it}^{(s)}; \epsilon_{it}^{G})$, compute $\gamma_{1j}^{(s+1)}$ by OLS.
7. If $\gamma_{1j}^{(s+1)}$ is sufficiently different from $\gamma_{1j}^{(s)}$, go back to point 3, otherwise stop.

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