Beliefs and the Incentive Effects of Preferential College Admissions: Evidence from an Experiment and a Structural Model *

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Abstract

We exploit a randomized control trial and a dynamic structural model to analyze how preferential college admissions affect pre-college effort and longer-term educational outcomes in Chile. The policy (PACE) guaranteed selective college admission to disadvantaged students graduating in the top 15% of their high school class. Using a dataset of 9,006 students combining administrative and survey data, we find PACE increased first-year enrollment in selective colleges by 3.0 percentage points (35% of the control mean), an effect waning to 1.5 percentage points (30%) by the fifth year. The policy reduced students' pre-college effort, likely due to biased beliefs regarding the returns to effort in college admission and persistence. Counterfactual simulations from the model show policymakers could mitigate these unintended disincentives while preserving PACE's college attainment gains by correcting students' beliefs about effort's returns in college persistence. Our results demonstrate that students' perceptions can critically shape the impacts of preferential admission policies.

1 Introduction

Young adults from better-off families are much more likely to attend college than those from worse-off families. 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; the gap is similarly high in other industrialized economies (OECD, 2024). 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 and have been severely restricted in some countries (Arcidiacono and Lovenheim, 2016; Feingold, 2023).

By altering the link between academic effort and admission chances, preferential admission policies change the study incentives of disadvantaged students while still in school (Coate and Loury, 1993). This paper analyzes how students' subjective beliefs about their admission chances and their future college success shape these incentive effects, and how study effort responses, in turn, influence who enters college under preferential admissions and how academically prepared they are. Understanding how students' perceptions shape the impacts of admission policies is essential for designing them effectively.

We study these questions in the context of Chile, which is uniquely well-suited for four reasons. First, it introduced a nationwide policy, PACE (*Programa de Acompañamiento y Acceso Efectivo a la Educación Superior*), that granted large admission advantages to students from disadvantaged schools. Second, Chile operates a transparent, centralized college admission system, allowing us to observe the actual incentives students face. Third, the country maintains rich longitudinal administrative data that track students from high school through college enrollment, which we could link to survey data on students' beliefs. Fourth, the rollout of PACE was randomized across high schools, allowing us to identify the policy's impacts.[1]

PACE targets students in disadvantaged high schools and guarantees admission to those graduating in the top 15% of their class to colleges participating in the centralized system, waiving the national entrance exam requirement. These colleges offer five-year (or longer) academically oriented programs, and their commitment to the policy is formalized through agreements with the government. PACE does not replace the regular route: students in PACE schools may still sit the national exam and compete for seats through the regular channel; the policy offers an additional pathway for top-ranked students. Students attending PACE high schools are substantially more disadvantaged than typical college entrants: their 10^{th}-grade standardized test scores are, on average, 1.4 standard deviations lower, and their household

[1] One of the paper's authors, Michela Tincani, led the experimental evaluation of PACE in collaboration with Chile's Ministry of Education and Ministry of Finance (Dirección de Presupuestos (DIPRES), 2022). Policy reports on the experimental evaluation of the program include Cooper, Guevara, Rivera, Sanhueza, and Tincani, 2019; Cooper, Sanhueza, and Tincani, 2020; Cooper, Guevara, Kinder, Rivera, Sanhueza, and Tincani, 2022.

income is roughly a third as high. PACE thus expanded access to selective colleges for a population dramatically underrepresented in the college system.

We constructed a new longitudinal dataset that links high-quality administrative records with original survey data collected in schools. The data follow 9,006 students who were in 11$^{\text{th}}$ grade in 2016 across 128 high schools—half randomly assigned to receive the PACE program and half to serve as controls. The administrative records cover students from 9$^{\text{th}}$ grade through five years after high school, and include detailed measures of academic performance, grades, demographics, and higher education outcomes, including enrollment and persistence.

To complement these administrative data, we designed and administered surveys to students in their final year of high school. The surveys capture students' beliefs about their academic ability (both absolute and relative), their perceived returns to effort, their expectations about college performance, and the monetary returns to college. We also collected data on self-reported effort and administered a standardized test to measure academic achievement. To understand how schools may respond to preferential admissions, we also surveyed teachers and principals about instructional focus, grading practices, and support classes. We linked all survey responses to the administrative data via unique student, classroom, and school identifiers.

There are two main experimental findings. First, PACE increased college admissions and enrollments among disadvantaged students by 4.1 and 3.0 percentage points— 36% and 35% increases relative to the control group. These effects were concentrated among students who, in 10$^{\text{th}}$ grade (before the experiment started), ranked in the top 15% of their high school cohort; students in the bottom 85% experienced no significant change in admissions or enrollment. While the initial effects were substantial, they declined over time: five years after high school, the impact on continuous enrollment or graduation from a selective college was 1.5 percentage points—a 30% increase relative to control students. Second, PACE reduced students' study effort and achievement in high school by 0.1 standard deviations. In contrast to the enrollment gains, these reductions were widespread, impacting students across the achievement distribution.[2]

To understand the waning enrollment impacts, we examine the type of selective colleges students attend and the characteristics of college entrants. We find no systematic changes in the selectivity, field of study, or geographic location of the programs students attend, suggesting that college match does not mediate the waning enrollment impacts over time. Instead, we cannot rule out changes in the composition of college entrants in terms of unmeasured baseline ability, nor differences in academic preparedness. Supporting the latter, we show that effort and achievement in the last high school year predict college persistence, suggesting that reduced

[2]The PACE policy was introduced by the Bachelet government and retained by the subsequent Piñera government after reviewing early experimental evidence on college admissions and enrollment impacts (Cooper, Guevara, Rivera, Sanhueza, and Tincani (2019)).

effort and achievement in high school may have left PACE students less prepared for college, contributing to lower persistence over time.

Next, we examine the mechanisms behind the observed reduction in pre-college effort. We find no evidence that PACE changed instructional practices or school-level academic support, nor did it affect students' perceptions of the monetary returns to college. These results suggest that neither school-side adjustments nor updated beliefs about the value of college explain the observed reduction in effort. Instead, we find evidence consistent with students responding to perceived incentives under the new admissions regime. By linking survey responses to actual academic outcomes in the administrative data, we document systematic over-optimism about both absolute and relative ability—suggesting that many students misperceived how close they were to the regular and preferential admission cutoffs. Effort reductions were concentrated among students who believed they were well above the PACE admissions threshold—consistent with the perception that PACE lowered the returns to effort in securing college admission. Additional belief data show that students were also over-optimistic about their likelihood to persist in college, and did not view high school effort as important for succeeding in college—suggesting they perceived little consequence for their future college success from reducing effort.

Motivated by these findings, we develop a dynamic structural model of students' educational choices to examine the implications of these belief distortions for the behavior of students and for how they would react to new, alternative PACE designs. In the model, students with different observed and unobserved characteristics make pre-college effort, entrance-exam taking, and enrollment decisions based on subjective beliefs about the returns to pre-college effort in securing regular and PACE admissions and in persisting in selective college. The model also includes objective admission and persistence likelihoods. In the model, belief-driven pre-college decisions can shape long-term college outcomes by affecting both who enters selective college and their likelihood to persist. We exploit our strategically designed survey questions to identify subjective returns to effort, and use experimental variation to help identify objective returns. The estimated model can match both targeted and untargeted treatment-effect patterns.

The first model result is that 77% of the observed association between pre-college effort and college persistence reflects a causal effect, while the remainder is due to unobserved variable bias—students more likely to persist also tend to exert more effort in school.

Second, we simulate a counterfactual in which both control and treated students have rational expectations to assess the role of belief distortions. Relative to the baseline with biased beliefs, students in both groups would exert less pre-college effort because they would no longer overestimate its returns in securing regular admission nor their likelihood of college persistence. Under rational expectations, PACE would not have reduced pre-college effort, as it would have slightly increased its returns by making college more attainable. Nevertheless, the PACE effect on enrollment would have weakened, as students would have correctly anticipated lower persistence and valued college entry less. Subjective beliefs, therefore, fundamentally shaped the effects of PACE.

Third, we simulate the effects of pairing PACE with an informational intervention that corrects the beliefs of treated students only. Although belief distortions help explain the decline in pre-college effort under PACE, correcting treated students' overoptimism about their admission and persistence chances would not eliminate this unintended effect—it would amplify it. Students who receive PACE with belief correction exert even less effort than students who only receive PACE. And while correcting beliefs improves the composition of college entrants from PACE schools in terms of baseline test scores, it ultimately reduces their persistence through lower pre-college effort. As a result, the treatment effects of this counterfactual policy are more negative for pre-college effort and smaller for admissions and long-term enrollment than the treatment effects of PACE alone.

A government interested in avoiding PACE's unintended impacts on pre-college effort could instead pair PACE with an intervention informing students about the role of pre-college effort in supporting college success, without correcting their other over-optimistic beliefs. This design mitigates the decline in effort without dampening enrollment gains. This suggests that which misperceptions are addressed can shape the impacts of preferential admissions.

Finally, we use the model to simulate the impacts of alternative top-percent cutoffs for preferential admissions. More generous cutoffs lead to larger gains in enrollment and persistence, but they also generate increasing numbers of dropouts. Beyond the current 15% cutoff, the effect on the number of college dropouts would exceed the effect on the number of college enrollees on track to graduate.

This paper contributes to the literature on preferential college admissions by providing unified evidence on how a preferential admission policy in Chile affected students' outcomes from before college entry to five years post high school. Two separate strands of the literature have documented impacts on pre-college academic outcomes (Golightly, 2019; Akhtari, Bau, and Laliberté, 2024; Khanna, 2020) and on longer-term college enrollment and persistence (Long, Saenz, and Tienda, 2010, Niu and Tienda, 2010, Daugherty, Martorell, and McFarlin, 2014, Bleemer, 2021, Black, Denning, and Rothstein, 2023).[3] This paper shows that the incentive effects on pre-college outcomes are not only policy-relevant per se, but they also matter for preferential admissions' longer-term impacts on college persistence. Additionally, the paper adds experimental evidence, which has so far remained limited.

Second, the paper contributes new evidence on how preferential admissions affect students and schools before college. Using linked administrative and large-scale survey data, we examine students, teachers, and school-level inputs. These data show that students' beliefs are central to

[3]See also Hastings, Neilson, and Zimmerman, 2012 for related evidence on K-12 responses to future educational opportunities, and Arcidiacono, Lovenheim, and Zhu, 2015 for a broad review of the literature on affirmative action in college admissions.

how they respond to preferential admissions. Prior work has shown that students' biased beliefs about their admissions chances affect application decisions (Larroucau et al., 2024; Hakimov, Schmacker, and Terrier, 2025); this paper shows that such beliefs also distort human capital investments before college in response to admissions policy.

Third, the paper contributes to the structural literature on preferential admissions (e.g., Arcidiacono, 2005; Kapor, 2024; Otero, Barahona, and Dobbin, 2023) by developing and estimating a dynamic model that endogenizes pre-college effort, incorporates subjective beliefs, and leverages experimental variation in estimation. While a small number of recent models incorporate effort decisions (i.e., Hickman, 2024; Grau, 2018; Borghesan, 2022), none of them have modeled the role of beliefs or used randomized variation in estimation. By relaxing rational expectations, the paper also contributes to a broader structural literature on beliefs in education. Existing work has focused on information frictions during college (e.g., Stinebrickner and Stinebrickner, 2014; Wiswall and Zafar, 2015; Arcidiacono et al., 2020), whereas this paper examines frictions before college and shows how they shape long-run educational outcomes. Related studies have used belief data to estimate static school-choice models (e.g., Bobba, Frisancho, and Pariguana, 2025; Kapor, Neilson, and Zimmerman, 2020), or incorporated expectations about future choices in dynamic models (e.g., Van der Klaauw, 2012; Delavande and Zafar, 2019). Finally, by combining experimental and structural methods, the paper contributes to the growing literature that uses Randomized Controlled Trials to discipline structural models (e.g., Todd and Wolpin, 2006, 2020; Attanasio, Meghir, and Santiago, 2011).

2 Context, Randomization and Data

2.1 Context and PACE Policy

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

Higher education in Chile. There are three categories of higher education institutions in Chile. Selective colleges are those that participate in the nationwide centralized admission system called *Sistema Único de Admisión* (SUA, Ministerio de Educacion, Chile (n.d.h)). They offer five-year (and longer) programs of an academic nature. They include the 27 public and private not-for-profit colleges that are part of the Council of Rectors of Chilean Universities (CRUCH) and 12 additional private colleges (Ministerio de Educacion, Chile / Subsecretaria de Educacion Superior, n.d.). Off-platform colleges offer academic programs and do not participate in the centralized admission system.[4] Finally, professional institutes and technical training centers do not have minimum admission requirements and provide vocational and shorter degrees.

[4]See Kapor, Karnani, and Neilson, 2024 for a description of these off-platform options.

In 2018, the shares of tertiary enrollments were 41% for selective colleges, 10% for off-platform colleges, and 49% for vocational institutes.

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 SUA system. Higher scores increase the likelihood of admission. The seat allocation follows a deferred acceptance algorithm where the PSU score is the most important component of programs' rankings of students (DEMRE, 2017; Rios et al., 2021; Kapor, Karnani, and Neilson, 2024).

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 B1). PACE was introduced to increase selective college admissions among disadvantaged students. The government selected the schools to be targeted by PACE using a school-level vulnerability index (*Indice de Vulnerabilidad Escolar*) based on students' socioeconomic characteristics, to identify 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. Moreover, they are guaranteed admission to a selective college, 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.[5] 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, and participate in light-touch orientation classes (two hours per month on average) that are offered to all students in PACE high schools.[6]

Other features of PACE include the following. i) Unlike the percent plans in Texas and California (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, PACE did not make it mechanically harder to obtain regular admission. iv) Of the 37 institutions participating in the centralized admission system,

[5]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 97.77%. Details of how the score is calculated can be found in Appendix A.1.

[6]The Texas Top Ten percent plan also offers orientation classes. The PACE orientation classes cover the college application process and study techniques and often replace orientation classes already offered by the schools (MinEduc, 2018).

29 signed an agreement with the government to offer PACE seats. The distribution of study fields is broadly similar across PACE and regular seats, but PACE seats are relatively more likely to be in the field of Education and less likely to be in the Social Sciences and Health (Figure B2). PACE seats are of similar quality to regular seats, as measured by the average entrance exam score of regular entrants in each program, although the most selective seats are under-represented (Figure B3). PACE seats are less likely than regular seats to be in the same province as students' high schools (0.50 vs. 0.60), but more likely to be in the same region (0.85 vs. 0.80), as shown in Table C1. v) The allocation process for PACE seats, described in detail in Appendix A.2, is separate from the regular admission process, such that a student can obtain both a PACE and a regular admission. If a student does not accept a PACE admission, that PACE seat remains vacant. vi) Nearly all students in PACE schools qualify for a full tuition waiver (*Gratuidad*) due to their low socioeconomic status.

2.2 Randomization and Balancing Tests

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

When a school first enters PACE, only the cohort of eleventh graders is entered into the program. The randomized expansion concerned the cohort who started eleventh grade in March 2016. Before starting the school year, students who were enrolled in schools randomly selected to be treated were informed their school was in the PACE program. This announcement was made after the school enrollment deadline; thus, we did not observe strategic selection into high schools (Appendix D.1.1). The control schools were not entered into the PACE program; they were not promised participation. Figure B4 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 (Ministerio de Educacion, Chile, n.d.b). 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).

Table 1: Sample Balance Across Treatment and Control Groups

	Control (1)	Difference between Treatment and Control (2)	p-Value (Difference equals zero) (3)	N (4)
Female	0.476	0.001	0.988	9006
		(0.054)		
Age (years)	17.541	0.031	0.561	9006
		(0.052)		
Very-low-SES student	0.602	0.014	0.489	9006
		(0.020)		
Mother's education (years)	9.553	0.081	0.631	6000
		(0.168)		
Father's education (years)	9.32	0.115	0.517	5722
		(0.178)		
Family income (1,000 CLP)	283.95	14.335	0.265	6018
		(12.794)		
SIMCE score (points)	221.355	7.600	0.151	8944
		(5.256)		
Never failed a year	0.970	-0.010	0.101	8944
		(0.006)		
Santiago resident	0.140	0.051	0.482	9006
		(0.073)		
Academic high-school track	0.229	0.055	0.451	9006
		(0.073)		
GPA in grades 9 and 10 (GPA points)	5.374	0.003	0.935	8970
		(0.031)		

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 (*Alumno Prioritario*). SIMCE is a standardized achievement test taken in 10^{th} grade. GPA is measured on a scale from 1 to 7.

2.3 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 and PACE channel admissions, enrollments and persistence or graduation up to five years after high school graduation, by type of college major (STEM and non-STEM). Table C2 provides a detailed list of the areas included in STEM according to the definition provided by the UCLA Higher Education Research Institute (2023). To gain insights on outside options, we

Table 2: Overview of Data

Dataset	Variables	Collected	Source
1. *SIMCE* (Agencia de Calidad de la Educacion, Chile, n.d.)	Achievement test scores, background characteristics	Grade 10	Admin
2. *SEP* (Ministerio de Educacion, Chile, n.d.e)	Very-low-SES classification (*Prioritario* student)	Grade 10	Admin
3. School records 1 (Ministerio de Educacion, Chile, n.d.g)	High-school enrollment	Grades 9-12	Admin
4. Student survey (Tincani and Kosse, 2017)	Study effort, beliefs about self and others	Grade 12	Primary
5. Teacher survey (Tincani and Kosse, 2017)	Effort and focus of instruction of Mathematics and language teachers	Grade 12	Primary
6. School-principal survey (Tincani and Kosse, 2017)	Support classes, assessment methods, classroom formation	Grade 12	Primary
7. Achievement (Tincani and Kosse, 2017)	Achievement test scores	Grade 12	Primary
8. School records 2 (Ministerio de Educacion, Chile, n.d.c,n)	GPA (overall and by subject), grade progression	Grades 9-12	Admin
9. Higher education records (Departamento de Evaluacion, Medicion y Registro Educacional (DEMRE), Universidad de Chile and Ministerio de Educacion, Chile, Subsecretaria de Educacion Superior, n.d.; Ministerio de Educacion, Chile / Servicio de Informacion de Educacion Superior (SIES), n.d.a,n; Miglino, Tincani, and Ministerio de Educacion, Chile, 2025)	Entrance exam (PSU) scores, applications, admissions, enrollments and graduation or persistence at five years in selective colleges via regular channel (STEM and non-STEM), seat selectivity, seat geographic location, 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 (Ministerio de Educacion, Chile, n.d.f; Miglino, Tincani, and Ministerio de Educacion, Chile, 2025)	Allocation of PACE seats in selective colleges, applications, admissions, enrollments and graduation or persistence via PACE channel, seat selectivity, seat geographic location	Years 1-5 after high school graduation	Admin

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

also 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.

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.3 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 high schools by Behrman et al., 2015 and Todd and Wolpin, 2018, complemented with questions on entrance exam preparation. In the reduced-form analysis, we combine these items into a standardized study-effort index; in the structural model, effort is measured directly as hours of study per week. 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). English translations of the key survey questions about beliefs are reported in Table C16. 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 D.1.2). 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.

2.4 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 1.42 standard deviations below regular entrants on average. Their median score corresponds to the fifth percentile of scores among regular entrants. Even those who graduate in the top 15% of targeted schools score substantially worse than regular college entrants, 0.89 standard deviations below on average. Their median score corresponds to the fifteenth 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 it.

Table C3 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 50% of the median household income in Chile, and 32% of the family income of regular entrants, whose average family income of CLP 904,354 per month is 56% above the median Chilean income.

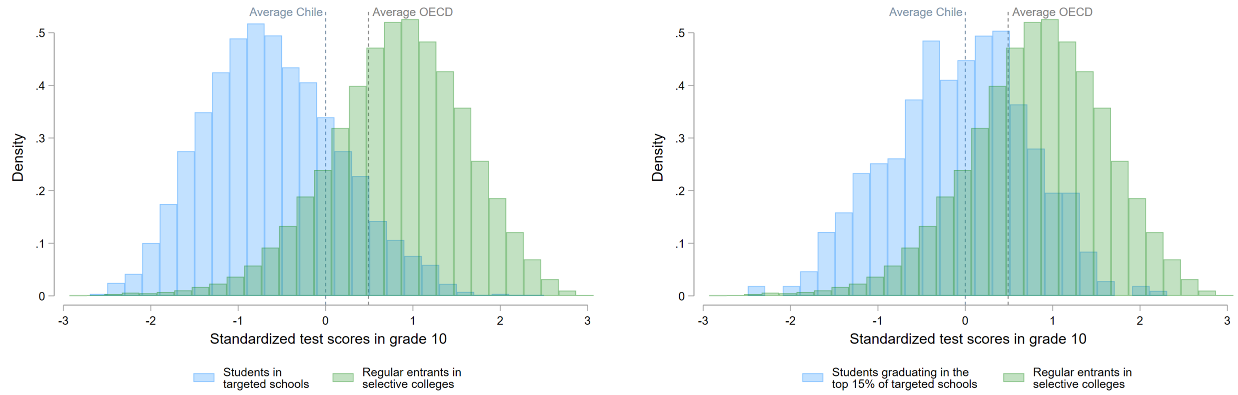

Figure 1: Distributions of standardized SIMCE test scores in 10^{th} grade. The scores are standardized in the population of 10^{th} grade students in 2015. Targeted students are those in schools targeted for PACE, we consider those assigned to the control group. The left panel includes all students in these schools, the right panel includes only those who graduate in the top 15% of their cohort. Each bar represents 0.20 standard deviations of the distribution of grade 10 test scores in the population of 10 graders. The average score in the OECD is calculated using PISA scores, re-scaled to be comparable to the SIMCE scores (for details see Appendix G.1).

PACE targets a substantially more disadvantaged population than the two most well-known percent plans in the United States. Students in California around the Eligibility in the Local Context preferential admission cutoff have family incomes that are 90% of the median Californian income (Bleemer, 2021, Table 1) and entrance exam (SAT) scores above the average score among all college applicants (Bleemer, 2021, Table 1). Of the students targeted by Texas Top Ten, $22-23\%$ are eligible for free or reduced school meals (Black, Denning, and Rothstein, 2023, Table 1), compared with 61% of students in PACE schools who are eligible for welfare programs. The students induced to enroll in a more selective college by the Texas Top Ten have entrance exam scores at the 89^{th} statewide percentile.

Fact 2: Absent PACE, only few targeted students attend selective college. Table 3 describes the educational choices of the typical students targeted by PACE absent PACE. Two thirds of students take the college entrance exam (second 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 B5). But, as the third row of the table shows, exam scores are well below the national average (-0.60 standard deviations). Upon observing their exam scores only 21.0% apply to selective colleges. 11.4% of students are admitted and, in the first year after high school graduation, 8.5% enroll in a selective college , located on average 136 km from their high school. Enrollment in selective colleges is almost equally divided between STEM and non-STEM majors. Students who enroll in selective colleges tend to have college peers who are academically higher-performing than themselves: their average college entrance test score is 0.54 standard deviations above the national average.

Table 3: Description of Choices and Outcomes in Control Schools

	Mean (1)	St.dev. (2)	N (3)
A. All Students			
Weekly study hours	4.24	2.81	2843
Took college entrance exam	.655	.475	4231
College entrance exam score | took exam	-.602	.611	2773
Applied to selective college	.21	.407	4231
Admitted to selective college	.114	.318	4231
Enrolled in selective college	.0848	.279	4231
Enrolled in selective college, STEM	.0404	.197	4231
Enrolled in selective college, non-STEM	.0444	.206	4231
Selectivity of program (college-major pair)	.544	.327	361
Distance in km from program (college-major pair)	135	233	356
Enrolled and persisted in selective college, year 5	.0499	.218	4231
Enrolled and persisted in selective college STEM, year 5	.0194	.138	4231
Enrolled and persisted in selective college non-STEM, year 5	.0262	.16	4231
Enrolled in vocational institution	.269	.443	4231
Enrolled in off-platform college	.0605	.238	4231
B. Students in Top 15% at baseline			
Weekly study hours	4.71	2.95	560
Took college entrance exam	.857	.35	735
College entrance exam score | took exam	-.245	.634	630
Applied to selective college	.45	.498	735
Admitted to selective college	.328	.47	735
Enrolled in selective college	.256	.437	735
Enrolled in selective college, STEM	.139	.346	735
Enrolled in selective college, non-STEM	.117	.322	735
Selectivity of program (college-major pair)	.674	.336	188
Distance in km from program (college-major pair)	128	215	187
Enrolled and persisted in selective college, year 5	.167	.374	735
Enrolled and persisted in selective college STEM, year 5	.0762	.265	735
Enrolled and persisted in selective college non-STEM, year 5	.0789	.27	735
Enrolled in vocational institution	.254	.436	735
Enrolled in off-platform college	.106	.308	735

Note. – Sample of students enrolled in control schools. The college entrance exam score is designed to have mean 500 and standard deviation 110 among all exam takers, we report the standardized score. The selectivity of the program is the average entrance exam score among all regular entrants in the selective college and major the student enrolled in. As a measure of distance we use the length (km) of the shortest path between the coordinates of the program and of the high school the student attended, implementing Vincenty formula to calculate distances on a reference ellipsoid. A student is coded as persisting in the fifth year if he/she enrolled in the first year after high school and stayed continuously enrolled in selective college every year up until and including year 5, or if he/she enrolled in the first year after high school and graduated from a selective college in a year prior to year 5. If a student transfers to a different selective college program without taking a break in their studies, they are still considered continuously enrolled in a selective college.

Among students who at baseline are in the top 15% of their school (Panel B in the Table), 86% take the entrance exam; their scores are 0.25 standard deviations below the average test taker's. Upon observing their score, around half (53%) of those who took the exam apply to selective colleges. Around a third of students are admitted and around a quarter enroll in a selective college, located on average 128 km from their high school. Top-15% students have similar rates of enrollment in STEM and non-STEM majors, and the average college entrance test score of college peers is 0.67 standard deviations above the national average.

We observe continuous enrollment in or graduation from a selective college five years since first enrolling, overall and by major type. Panel A of Table 3 shows that 59% of those who enroll in the first year are still continuously enrolled or have graduated after five years. Panel B shows that this figure is slightly larger in the sample of high-performing students (65%). The share of college entrants who persist is higher among those who enroll in non-STEM majors, both in the top-15% and in the whole sample.

Absent the policy, 26.9% of students in targeted schools enroll in vocational higher education programs, 6.1% in non-selective colleges, and 58.5% do not enroll in higher education. Among the top performing students in targeted schools, 24.7% enroll in vocational higher education programs, 11.8% in non-selective colleges, and 38.4% do not enroll in higher education (Panel B). We report in Appendix Table C7 descriptions of outcomes in both treated and control schools.

3 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.[7] The parameter of interest is β. The standard errors are clustered at the school level.

Experimental Finding 1: PACE increased selective college admissions and enrollments. 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 selective college and 3.0 p.p more likely to enroll than students in control schools, corresponding to a 36% and 35% increase compared to selective college admissions and enrollments in the control group. The effect on continuous

[7] We exclude from vector X_i mother' and father's education and household income because of a high number of missing observations for these variables.

enrollment in the fifth year or graduation by such time (which is an upper bound for the effect on on-time graduation) is 1.5 p.p., corresponding to a 30% increase compared to the control group, and it is significantly different (p=0.006) from the treatment effect on first-year enrollments. The smaller treatment effect in relative terms is consistent with lower persistence rates among college entrants from treated schools (56.7% , compared to 58.8% in the control group).

These impacts are concentrated among students who, at baseline, were in the top 15% of their school according to GPA in grades 9 and 10, as shown in Tables C4 and C6. Among top-performing students, PACE increased selective college applications, admissions, and first-year enrollments in selective colleges. Although selective college enrollment effects remained significant and positive five years after high school, they were smaller and significantly different (p = 0.000) from first-year effects: first-year enrollments increased by 16.6 p.p., 65% relative to the control group mean, while fifth-year enrollments showed an 8.7 p.p., or 52%, increase (Table C6). These results are consistent with larger persistence rates among college entrants from control schools, who persisted at a 65.4% rate, compared to a 61.4% rate among college entrants from treated schools. Back-of-the-envelope calculations suggest that if persistence rates had been the same across groups, the fifth-year enrollment effect would have been approximately 10.9 p.p., nearly 25% larger than the observed effect.[8] Table C6 also shows that PACE lowered the enrollment of top-performing students in the outside options (vocational institutes and non-selective colleges). And while it increased their first-year enrollments in higher education overall, it had no significant impacts on continuous enrollment in or graduation from higher education after five years.[9]

Experimental Finding 2: PACE lowered study effort and achievement before college. Columns (1) and (2) of Table 4 present results on the outcomes specified in the pre-analysis plan. 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 C8). Using additional administrative outcome data, columns (3) and (4) provide suggestive evidence that the policy had a negative effect on the grades in the subjects tested on the entrance exam (although this effect is insignificant when accounting for multiple hypothesis testing), and no effect on the grades in the subjects not tested. Together, the results suggest students reduced their study effort

[8]Even with identical persistence rates for control and treated students, the positive treatment effect on enrollment declines over time in absolute terms. Since the treatment group starts with more enrollees, a constant persistence rate results in more dropouts in absolute terms, gradually reducing the enrollment gap between the two groups.

[9]The regression results are robust to excluding the control variables, as can be seen by comparing average outcomes across treatment groups in Table C7.

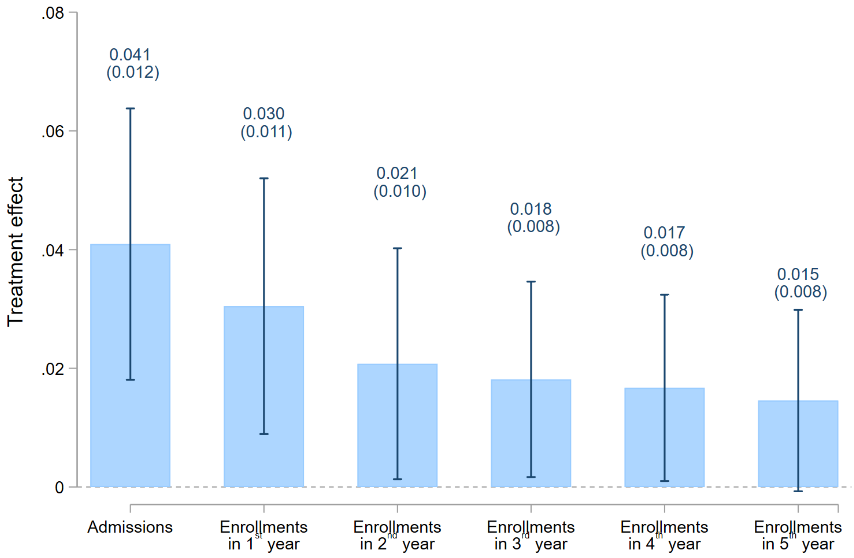

Figure 2: Effects of PACE on admissions and on enrollment or graduation over time. The Figure reports OLS estimates from the estimation of parameter β in equation (1). The controls are: gender, age, indicator for very-low-SES student, baseline SIMCE test score, never failed a grade, and high school track (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 in or graduation from a selective college: the outcome variable in the t^{th} year after high school is equal to one if the student enrolled in a selective college the first year and remained continuously enrolled in that selective college every year up until and including year t, or if he/she enrolled in a selective college the first year and graduated from it in a year prior to t or in t. The variables are set to zero in all other cases, including having never enrolled in a selective college. Table C4 reports the estimates of the admission effect and Table C5 of the enrollment effects.

towards PSU exam preparation and PSU exam subjects, without reallocating effort to other subjects.

As we show in Table C9, PACE did not significantly change the proportion of students taking the entrance exam (column 1), but the sample taking the exam appears more positively selected in the treatment group (column 2). Consistent with the reduction in effort and pre-college achievement, this results in similar entrance exam scores across treatment groups (column 3).

Appendix D.1.3 examines the validity and robustness of the survey-based findings, showing that the survey-based measures display good predictive validity on long-term outcomes, and that the results are robust to using item response theory to calculate the achievement score.

Table 4: Effect of PACE on Pre-College Outcomes

	Test Score	Study Effort Index (std.)	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)
Control mean	0.033	0.065	0.122	0.081
R-squared	0.259	0.047	0.220	0.109
Observations	6054	5631	6046	4288

Note.– The coefficients are OLS estimates. Standard errors were clustered at the school level. The standard set of controls (see notes under 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 study effort score predicted from the principal component analysis of the eight survey instruments reported in Appendix Table C8. The outcome variables in columns (3) and (4) are the GPA in subjects tested and untested on the PSU exam, standardized. The smaller number of observations in column (4) compared to column (3) reflects different grade-reporting rules across mandatory and optional courses. Romano-Wolf adjusted p-values (based on 1000 bootstrap replications for the family of 12^{th} grade GPA) in columns (3) and (4) are .199 and .967. Q-values for the family of 12^{th} grade GPA) in columns (3) and (4) are .205 and .933. * p<0.10; ** p<0.05; *** p<0.01

4 Mechanisms

4.1 Potential Drivers of Long-Term Treatment Effects on Enrollment

4.1.1 Selective college match

PACE may have led students to enroll in more demanding programs, causing larger dropout among students from treated schools. To examine this channel, we begin by analyzing its effects on enrollment in STEM versus non-STEM programs at selective colleges, as STEM majors are typically more academically challenging. We assign value zero to both STEM and non-STEM enrollment for students who do not enroll in any selective college. Tables C10 and C11 show that PACE increased enrollment rates in STEM and non-STEM fields almost equally, maintaining the same relative proportion between these fields as observed in the control group. The difference between treatment effects on STEM and non-STEM enrollment is always small and statistically insignificant across all years and subsamples, including the top 15% of students at baseline where enrollment impacts are concentrated. Thus, PACE did not change students' relative propensity to pursue STEM versus non-STEM programs at selective colleges.

Next, we examine the selectivity of degree programs chosen by students who enroll in selective colleges, where a degree program is defined as a college-major pair. We measure program selectivity using the average entrance exam score of regular-admission students, and set the outcome variable to missing for students who do not enroll in selective colleges. Columns (1) and (3) of Table C12, which control for student characteristics, show no statistically significant differences in program selectivity between college entrants from treated and control schools.[10]

We also examine the geographic distribution of enrollment by measuring the distance between students' high schools and their chosen selective college programs. Columns (2) and (4) of Table C12 show that, among selective college enrollees, students from treated schools enroll in programs that are closer to their high schools, though the difference is statistically insignificant.

The enrollment results align with the admission patterns, described in Appendix F. We do not find significant differences across treatment groups in the characteristics of the selective college programs to which students are admitted through the regular process (field of study, selectivity, and location). Similarly, there are no significant differences in admission patterns between the regular and PACE channels for students in PACE schools.

Finally, we examine whether college entrants from treated schools are more negatively selected on baseline measures of ability. Focusing on the sample of students in the top 15% of their school at baseline—where college impacts are concentrated—we find that entrants from treated schools have lower grade 10 standardized test scores (-0.05 standard deviations; Table C14), but this difference is statistically insignificant.

These findings suggest that a worsening of the college match in terms of selectivity, field of study, or increased distance to college (and associated factors like separation from support networks or higher costs) is unlikely to explain the higher dropout rates among students from treated schools. Moreover, we find no statistically significant evidence of more negative selection on observed baseline ability, although we cannot rule out a more negative selection on unmeasured ability.

4.1.2 Reductions in college preparedness

By reducing pre-college effort and achievement, PACE could have reduced college preparedness, leading to lower college persistence rates in the treatment group. To investigate this channel, we examine whether PACE lowered pre-college outcomes that predict persistence in college.

Appendix Table C15 shows that, after controlling for student characteristics, GPA in the last high school year strongly predicts continuous enrollment or graduation five years after entering a selective college, independently of the entrance exam score and of the baseline test score (column (1)). GPA in the subjects tested on the entrance exam correlates more strongly with persistence than GPA in untested subjects (column (2)). The PSU score independently predicts persistence, although not significantly (columns (1) and (2)). If GPA and the PSU score at the end of high school are produced by a combination of baseline ability and study

[10] Lee bounds (Lee, 2009)—which bound intensive margin impacts among always-enrollers—are large due to the substantial extensive margin effect on selective college enrollment, though they always include zero. We report them in Appendix Table C13.

effort during high school, the administrative measure of baseline ability and our survey measure of study effort should both predict persistence. This is indeed what we find: both measures are significantly predictive, even after conditioning on the rich vector of student characteristics (columns (3) and (4)).

This evidence suggests that PACE reduced pre-college outcomes that predict persistence. Competence in the core high school subjects, which are tested on the entrance exam, seems to matter most for persistence.

4.2 Potential Drivers of Negative Impacts on Pre-College Effort and Achievement

4.2.1 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?

Heterogeneity of impacts by absolute and relative ability. 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.

These patterns are hard to rationalize as a response to incentives under rational expectations. As shown theoretically in Bodoh-Creed and Hickman, 2018, when students rationally respond to the incentives of percent rules, negative impacts are concentrated among those around the regular admission cutoff but well above the preferential admission cutoff. For these students the policy lowered returns to effort by guaranteeing an admission that was previously only within reach under sustained effort. Conversely, we would expect positive impacts among students near the top 15% cutoff and for whom PACE brought 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.

Students' misperceptions about their absolute and relative ability. We elicited subjective expectations over the PSU entrance exam score using the survey question reported in

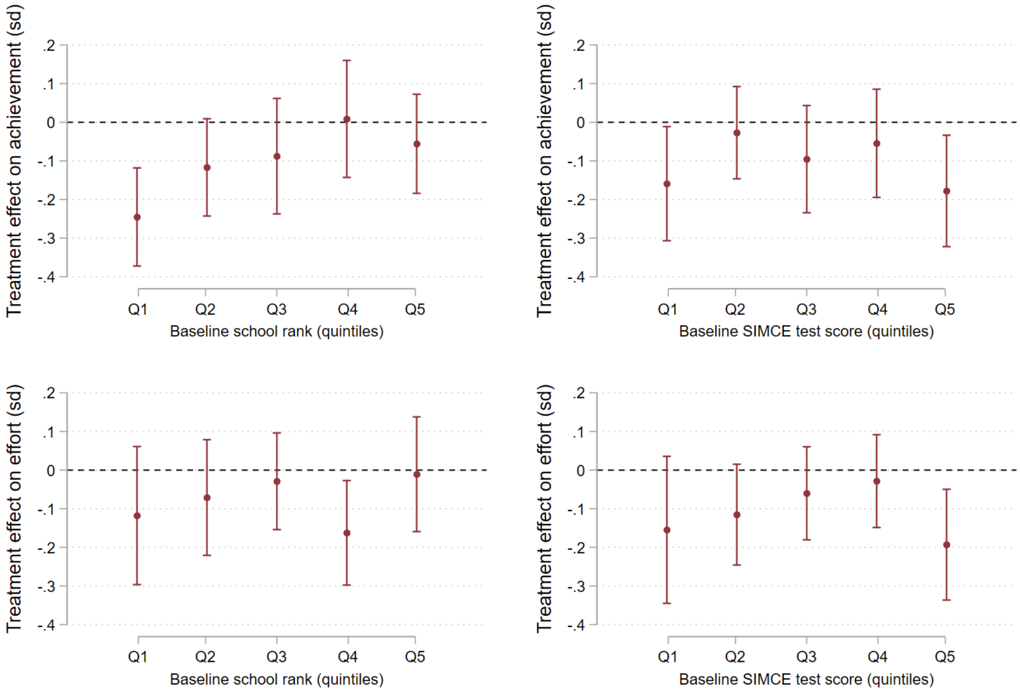

Figure 3: Heterogeneity of policy effects on pre-college effort and achievement. Notes: Each dot is the coefficient on *Treatment* from an OLS regression where: *Treatment* is a dummy variable indicating whether a student is in a school that was randomly assigned to be in the PACE program, the controls 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 9^{th} and 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. Q-values for the family of quintiles in the upper left panel are $q(Q1) = 0.001$, $q(Q2) = 0.161$, $q(Q3) = 0.329$, $q(Q4) = 0.700$, $q(Q5) = 0.415$. The respective q-values for the upper right panel are $0.087, 0.412, 0.213, 0.412, 0.087$. The respective q-values for the lower left panel are $0.640, 0.875, 0.952, 0.105, 1.000$. The respective q-values for the lower right panel are $0.172, 0.172, 0.225, 0.345, 0.048$.

the first row of Table C16. The answers were given as ranges of the score. Using the midpoint of the range as the measure of perceived score, 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 obtain. Figure B6 confirms the large over-optimism by plotting the histograms of the raw survey answers and of the actual scores.

We elicited subjective expectations over own GPA and the top 15% cutoff in the school using the survey questions reported in the second and third rows of Table C16. Students display large over-optimism about their within-school rank, with over 40% believing that their GPA is in the top 15%. While 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), they have a belief bias about the 85^{th} GPA percentile in their school of less than half GPA point (fourth row of the Table). This small belief bias in absolute terms is large in relative terms because of strong grade compression, that we document in Figures

B7 and B8.[11] These belief biases are consistent with the limited college experience of students' parents (over 90% did not study beyond secondary education) and the lack of relative rank feedback in PACE schools.

Table 5: Description of Subjective Beliefs

	Mean (1)	Std. Deviation (2)	N (3)
Believed entrance exam score (σ)	-.033	.92	2413
Believed minus actual entrance exam score | took exam (σ)	.591	.916	1853
Believed minus actual 12^{th} grade GPA (GPA points)	-.075	.552	2558
Expected top 15% cutoff	5.82	.846	3326
Actual minus believed top 15% cutoff in school (GPA points)	.401	.854	3326
Believes is in top 15% of school	.431	.495	2469

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 perceived GPA is above her perceived top 15% cutoff. Appendix Table C16 contains an English translation of the survey instruments we used to elicit the beliefs reported in this Table.

Examining belief heterogeneity, Figure B9 shows that students of all (absolute and relative) ability levels are over-optimistic; Table C17 shows that belief biases are only weakly related to socioeconomic background in our homogeneously disadvantaged sample, with small correlations with parental education. The findings align with existing evidence that over-optimism is widespread in many contexts, including education (Stinebrickner and Stinebrickner, 2014; Hakimov, Schmacker, and Terrier, 2025).

Response to perceived incentives. As students have biased beliefs about their relative rank in the school and performance on the entrance exam, a natural question is whether the impacts on pre-college outcomes are consistent with a response to perceived rather than actual incentives.

The belief patterns shown in Table 5 and Figure B6 point to this mechanism. Students tend to expect their entrance exam scores to be near the national average, which is close to the regular admission cutoffs. Believing that admission is within reach, most students in the control sample prepare for the entrance exam (Table 3). At the same time, students tend to perceive themselves as having a high within-school rank, which may lead them to believe that a preferential admission is guaranteed. On average, students view themselves as the type for whom PACE reduces the incentive to exert effort: those who are marginal for regular admission but confident of gaining preferential admission.

[11] First, we show that while grades can range from 1 to 7, the vast majority lie between 5 and 6.5. Second, we link grade data to baseline and end-line standardized achievement measures, and show that grades do not discriminate substantially among students of different baseline abilities, and much less than the end-line standardized achievement test we administered does.

To further explore this channel, we examine the heterogeneity of impacts on pre-college outcomes by perceived absolute and relative ability. If students respond to perceived incentives, the marginal utility of effort is highest at the perceived top 15% cutoff, as a small change in GPA can determine whether a student is in or out of the top 15% and thus eligible for a preferential admission. The incentive to exert effort decreases as students perceive themselves to be further from this cutoff, leading to smaller treatment effects, even negative for those who perceive themselves to be well above the cutoff. Moreover, the negative impacts on effort should be strongest among students who are not confident of gaining regular admission, and believe they are above the top 15% cutoff.

This is what we find, as shown in Table 6. We define the perceived distance from the cutoff as the absolute value of the difference between a student's perceived own GPA and the perceived GPA of the 85^{th} percentile in their school. We regress pre-college outcomes on the treatment indicator, the controls, the measure of perceived distance from the cutoff, and its interaction with both the treatment indicator and controls. We perform these regressions on all students (Panel A) and on the sub-sample of students who believe they are in the top 15% and at or below the median PSU score (Panel B).[12] As expected if students were responding to perceived incentives, the treatment effects on achievement, study effort, and GPA (both tested and untested subjects) diminish as students perceive themselves to be further from the cutoff. The coefficient on the interaction between treatment and perceived distance is negative for all pre-college outcomes and statistically significant for study effort and GPA in tested subjects. These effects are stronger for students who believe they are above the top 15% cutoff but at or below the median PSU score (Panel B). For this subgroup, treatment effects are positive at the perceived cutoff (the treatment coefficient is positive but imprecisely estimated, reflecting the low density of students who perceive themselves to be at the cutoff) but become negative as the perceived GPA rises further above the perceived cutoff. The evidence, therefore, is consistent with a response to perceived incentives.[13]

Appendix D.1.5 shows that the belief measures have good predictive validity properties, giving us confidence in the findings presented in this section.

[12] The minimum PSU required for regular admission varies by program, averaging 480. The median subjective expectation for PSU scores falls within the 450-600 range. In Panel B, we exclude students above this median (i.e., those expecting a PSU between 600 and 850), as these students likely perceive themselves to be in a relatively secure position for regular admission.

[13] A caveat of these results is that subjective expectations were not elicited at the experiment's baseline, raising the concern that the expectations themselves could have been influenced by the treatment. In Appendix D.1.4 we provide robustness checks demonstrating this is unlikely to drive the results in Table 6.

Table 6: Effect of PACE on Pre-College Outcomes by Perceived Distance from Cutoff

	Test Score	Study Effort Index (std.)	12^{th} grade GPA	
			Tested subjects	Untested subjects
	(1)	(2)	(3)	(4)
A. All students				
Treatment	-0.079	0.010	-0.091	0.128
	(0.058)	(0.057)	(0.071)	(0.115)
Perceived distance	0.858*	0.147	-0.105	-0.678
	(0.453)	(0.595)	(0.510)	(0.509)
Treatment × Perceived distance	-0.021	-0.141***	-0.125***	-0.077
	(0.039)	(0.052)	(0.039)	(0.063)
Control mean	0.134	0.105	0.205	0.128
R-squared	0.269	0.074	0.305	0.282
Observations	5055	4848	5053	3581
B. Students with perceived GPA > perceived cutoff, perceived PSU ≤ median				
Treatment	0.013	0.125	0.044	0.168
	(0.089)	(0.085)	(0.093)	(0.176)
Perceived distance	0.654	-0.020	0.876	-1.623
	(1.166)	(1.614)	(1.002)	(1.211)
Treatment × Perceived distance	-0.151	-0.295***	-0.164*	-0.168
	(0.107)	(0.086)	(0.098)	(0.152)
Control mean	0.206	0.258	0.479	0.389
R-squared	0.318	0.129	0.365	0.326
Observations	1281	1233	1281	911

Note.– The coefficients are OLS estimates. Standard errors were clustered at the school level. *Treatment* is a dummy variable indicating whether a student is in a school randomly assigned to be in the PACE program. *Perceived distance* is the absolute value of the difference between perceived own GPA and the perceived 85^{th} percentile of the GPA distribution in the school. In all regressions we include the standard set of controls (see notes under Figure 2), *Treatment*, *Perceived distance* and the interaction of *Perceived distance* with *Treatment* and with all controls. Inverse Probability Weights were used. Field-worker fixed effects, and field-worker fixed effects interacted with *Perceived distance*, were used for columns (1) and (2). See Appendix D for the survey questions used to elicit beliefs. Panel A is based on the sample of all survey respondents. Panel B is based on the sample of sample respondents who perceive themselves to have a higher GPA than the 85^{th} percentile in the school and a PSU score lower than or equal to the median perceived PSU. The PSU score ranges from 150 to 850 and the median perceived PSU score lies in the interval 450-600. The outcome variables are the same ones used in Table 4. The Romano-Wolf adjusted p-values (based on 1000 bootstrap replications) for the coefficient on *Treatment* × *Perceived distance* for the family of 12^{th} grade GPA) in columns (3) and (4) are .037 and .338 for Panel A and .368 and .426 for panel B. Sharpened q-values for the coefficient on *Treatment* × *Perceived distance* for the family of 12^{th} grade GPA) in columns (3) and (4) are .005 and .125 for Panel A and .241 and .241 for panel B. * p<0.10; ** p<0.05; *** p<0.01

4.2.2 Perceived college graduation likelihood and pre-college study effort

The evidence so far suggests that students on average lower their study effort because they perceive it is no longer needed to obtain a selective college admission. However, they would not do so if they believed pre-college effort was important to do well in college.

We elicited students' beliefs about their likelihood of graduating from a selective college, if they were to enroll in one (Table C16). We find that half of the students are certain they will graduate if admitted, and three quarters believe they have more than 50% chance of graduating. Figure 4 shows that, despite its large impacts on pre-college effort, PACE had only limited impacts on this subjective belief, which was elicited after the effort reductions had occurred. Only 3.7 percent of the sample appear to be affected, not answering "probably yes" when treated, opting instead for "equally likely" (2 percent), "probably not" (0.6 percent), and "definitely not" (1.1 percent).[14] Assigning numerical values to the survey answers reveals null effects on the average perceived graduation likelihood, irrespective of the regression specification (Appendix Table C18). The heterogeneity analysis reported in Appendix Figure B10 further shows that there was no substantial effect on the perceived graduation likelihood across baseline ability and within-school rank: regardless of the scale used to assign numerical values to the survey answers, the impacts hover around zero for most sub-samples, even among those who experienced considerable reductions in effort, achievement, or both, as per Figure 3. Together, these empirical results suggest that students do not perceive pre-college effort as important for persistence in college.

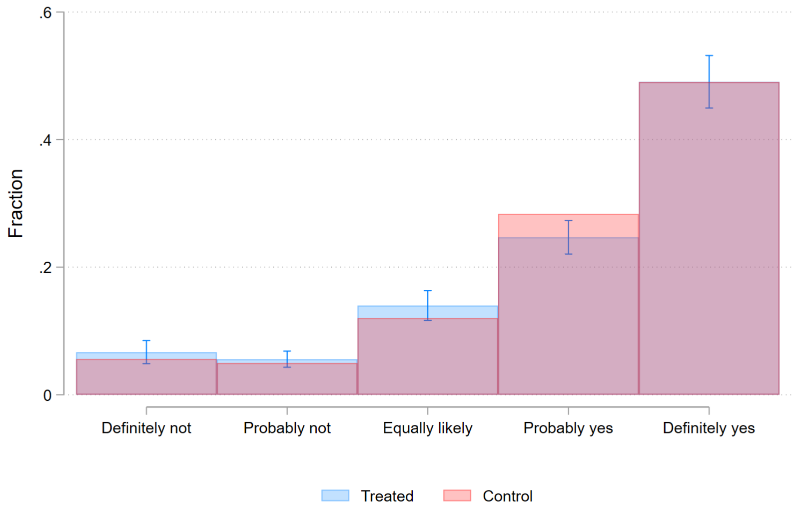

Figure 4: Distribution of survey responses regarding beliefs about the likelihood of graduating from a selective college conditional on enrolling, by treatment status (i.e., being in a PACE or control school). The figure includes 95% confidence intervals for the difference between the proportion of treated and of control students giving each answer. The confidence intervals were obtained from standard errors clustered at high school level. An English translation of the survey question can be found in Appendix Table C16.

[14]As is always the case with experimental data, we do not observe individual-level treatment effects, only averages. An alternative explanation to these patterns could be, for example, that 3.7 percent of the sample do not answer "probably yes", opting instead for "definitely yes", and an equal, offsetting fraction of the sample do not answer "definitely yes", opting instead for "equally likely" (2 percent), "probably not" (0.6 percent), and "definitely not" (1.1 percent). Any pattern that matches the net effects is consistent with the data. But these alternative, more convoluted explanations appear less likely.

4.2.3 Other mechanisms: Teachers, schools, and perceived returns to college

Teachers can influence who obtains a preferential seat by adjusting their grading. If, in response to the percent plan policy, teachers manipulate their grading in ways that weaken the link between academic achievement and GPA, students in treated schools may have weaker incentives to study. This could help explain the observed reductions in pre-college effort. Teachers may also respond to the policy by changing their own effort or shifting the focus of instruction, which could affect student achievement both directly and indirectly—if, for instance, changes in teacher behavior influence how much students study. Schools might also adjust their academic support offerings, particularly for entrance exam preparation. However, evidence from supplementary teacher and principal surveys, along with merged data on grades and standardized test scores, suggests that these channels are unlikely to drive the decline in pre-college effort (Appendix E.1).

If the light-touch orientation classes offered in PACE schools negatively affected students' beliefs about the net returns to college, they could have generated the reduction in pre-college study effort. Hastings, Neilson, and Zimmerman, 2015 show that providing information on graduate earnings can change college applicants' choices in Chile. Although the orientation classes were not designed to provide information about returns, this remains an important channel to consider.[15] Evidence from survey data on perceived returns to college (Tincani and Kosse, 2017; Miglino and Tincani, 2026a) indicate that this channel is unlikely to drive the decline in pre-college effort: we find no effects on expected earnings nor on awareness of financial aid (Appendix E.2).

5 A Dynamic Model of Education Choices

We develop a structural model of students' educational choices that rationalizes the experimental findings and allows us to go beyond them in three ways. It separates the causal effect of pre-college effort on college persistence from selection on unobserved characteristics, thereby addressing endogeneity concerns that arise when effort is a choice. It allows us to study how students' over-optimism shapes their behavior and determines how they respond to admission-based policies. And finally, it allows us to evaluate counterfactual PACE designs and information interventions that the experiment cannot directly assess.

To achieve this, the model incorporates several key features. First, it links pre-college choices to college outcomes. The reduced-form results in Section 4.1.2 show that pre-college effort predicts persistence in selective college, but they do not show that increasing effort would cause persistence to rise: effort is itself a choice, and students who choose higher effort may

[15] Perceived returns to college is an outcome that was not pre-specified in the pre-analysis plan. We added this outcome post hoc after observing declines in pre-college effort.

differ in unobserved traits that also affect college success. By jointly modelling effort choices, selection into college, and persistence as functions of observed and unobserved heterogeneity, and by exploiting experimental variation in effort, the model separates the correlational and causal components of the effort-persistence relationship.

Second, the model does not impose rational expectations. Students make pre-college and enrollment decisions based on subjective beliefs about their academic skills, admission chances, and likelihood of persisting in college. These beliefs are identified using our original survey data, while data on actual academic skills, admissions, and persistence outcomes allow us to identify the objective underlying processes.

We use the estimated model to simulate the long-term educational effects of alternative admission policies and to illustrate how beliefs shape students' responses to policy incentives and, ultimately, policy outcomes.

5.1 Students

At the experiment's baseline, the end of 10^{th} grade, each student i is characterized by observable demographics and achievement measures: age, gender, socioeconomic status (*Alumno Prioritario* classification, indicating very low SES), high school track (vocational or academic), treatment or control school status, Chilean region of residence, GPA and within-school GPA rank (based on 9^{th} and 10^{th} grade marks), and standardized test scores (SIMCE) in 10^{th} grade.

To allow for unobserved heterogeneity, students in the model are also characterized by a discrete permanent type, $k_i \in \{1, 2, ..., K\}$, known to them but not to the econometrician (Heckman and Singer, 1984; Keane and Wolpin, 1994, 1997), with the number of types, K, known to the econometrician. We allow some model parameters to vary by type. In estimation, we specify the type probability as a function of baseline variables (including an indicator for whether survey data is missing, to allow for survey non-response based on unobservables) and estimate the parameters of this probability along with the model parameters.

5.2 Model Description Period by Period

We model students' decisions from the start of 10^{th} grade to the college enrollment choice four months after high school completion. Students first form beliefs about their future admission scores (GPA and entrance exam score) and college persistence and how effort maps into them. They then choose study effort during the last two high school years and decide whether to take the entrance exam at high school graduation based on these beliefs, while admission scores and college admission follow objective stochastic processes. Students who took the exam and who receive an offer four months after high school graduation decide whether to enroll in college, basing their decision on their perceived college persistence. Conditional on enrollment, however, there is an exogenous objective probability of persisting. Given the short time horizon of these decisions, we impose no time discounting within the model.[16]

5.2.1 Time 0: Belief formation

Before students make any choices, they form beliefs relevant for choices.

Perceived PSU and regular admission. Students form the following belief about the production function of the PSU entrance exam score:

$$\begin{aligned} PSU_i^b &= \overline{PSU}_i^b + \epsilon_i^{Pb} \\ &= \beta_0^{Pb} + \beta_{1i}^{Pb} e_i \mathbf{1}(e_i < e_{kink,i}^{Pb}) + \beta_{2i}^{Pb} e_i \mathbf{1}(e_i \geq e_{kink,i}^{Pb}) \\ &\quad + \beta_3^{Pb} \mathrm{GPA}_{i,t-1} + \beta_4^{Pb} \mathrm{simce}_{i,t-1} + \epsilon_i^{Pb} \quad \epsilon_i^{Pb} \sim N(0, \sigma^2_{PSU^b}), \end{aligned} \tag{2}$$

where e_i is study effort, $\mathbf{1}(\cdot)$ is an indicator function equal to one if the expression in parenthesis is true and to zero otherwise, $\mathrm{GPA}_{i,t-1}$ is GPA in 9^{th} and 10^{th} grade, $\mathrm{simce}_{i,t-1}$ is the standardized test score in 10^{th} grade, and ϵ_i^{Pb} is belief uncertainty around the expected score $\overline{PSU}_i^b$, i.i.d. across students. Equation (2) is piecewise linear in effort with a kink point at $e_{kink,i}^{Pb}$. We allow students to hold heterogeneous beliefs about the returns to effort by letting the effort coefficients and kink point vary across students. The expected score ($\overline{PSU}_i^b$), effort (e_i), its perceived returns (β_{1i}^{Pb}, β_{2i}^{Pb}), and the kink point ($e_{kink,i}^{Pb}$) are obtained from survey data as explained in Section 6.1.

The subjective probability of regular admission conditional on taking the entrance exam is equal to the subjective probability that a student's believed score will be above the believed admission cutoff, over which the student forms a subjective probability distribution with mean $\bar{c}^{Rb}$. Letting A_i^R denote a dummy for regular admission, the subjective probability of regular admission is:

$$Pr^b(A_i^R = 1 | \overline{PSU}_i^b) = \Phi\left(\gamma_0^b + \gamma_1^b \overline{PSU}_i^b\right), \tag{3}$$

Appendix H.1 shows how this expression derives from uncertainty around own score and the admission cutoff. Students also form beliefs about program selectivity, conditional on regular admission, based on their expected entrance exam score. Specifically, they substitute $\overline{PSU}_i^b$ for PSU_i in the exogenous stochastic process that allocates regular seats (discussed in Section 5.2.4).

Perceived GPA and preferential admission.

[16]If students did discount future payoffs over this short horizon, we would over-estimate the cost of exerting effort in high school, the first decision period in the model. Our counterfactual experiments, then, would deliver lower bounds for the elasticity of effort.

Perceived GPA and preferential admission. Students form beliefs about their GPA in the last two high school years (Equation (17)) in a way that mirrors how they form beliefs about their PSU score (Equation (2)). The only substantive difference is that we model perceived returns to effort in GPA production as linear rather than piecewise linear. This difference is driven by the survey design: for GPA, students reported the study hours they believed were required to reach only two GPA thresholds, a fixed GPA of 5.5 and their perceived top-15 cutoff.[17]

In treated schools, the subjective probability of preferential admission conditional on taking the entrance exam is equal to the subjective probability that a student's believed average GPA in the four high school years will be above the believed preferential admission cutoff, which is the 85^{th} percentile of high school GPA in the school. Students form a subjective probability distribution over this cutoff; we allow the mean of this distribution, $\bar{c}_i^{15b}$, to vary across students. The survey elicited $\bar{c}_i^{15b}$. Following an established approach 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), we assume that students in treated schools best-respond to their belief about the within-school cutoff, and we do not impose that their beliefs are equilibrium ones. Letting A_i^P denote a dummy for preferential admission, the subjective probability of preferential admission is:

$$Pr^b(A_i^P = 1|\overline{GPA}_i^{(9-12),b}, \bar{c}_i^{15b}) = \Phi\left(\pi_0^b + \pi_1^b(\overline{GPA}_{it}^{(9-12),b} - \bar{c}_i^{15b})\right), \tag{4}$$

Appendix H.1 shows how this expression derives from uncertainty around own GPA and the cutoff. Students form beliefs also about program selectivity, conditional on preferential admission, based on their expected high school GPA. Specifically, they substitute $\overline{GPA}_i^{(9-12,b)}$ for GPA_i^{9-12} in the exogenous stochastic process that allocates preferential seats (discussed in Section 5.2.4).

Perceived persistence in selective college. Students form a belief about their likelihood of graduating from selective college, conditional on enrolling in one, directly elicited by our survey. We denote this probability by $pgrad_i^b$. Based on the evidence from section 4.2.1, we assume students do not believe the persistence probability depends on effort. Each student enters the model with a perceived persistence probability, which is constant over time.

5.2.2 Time 1: Choice of effort in high school

Students are characterized by a state-space vector Ω_{i1} containing the baseline characteristics, type, and a dummy for whether survey data is missing (see Section 5.1), and the beliefs that

[17] The expected GPA in the last two high school years, $\overline{GPA}_i^{(11-12,b)}$, is derived from survey data on the expected GPA in the four high school years, $\overline{GPA}_i^{(9-12,b)}$, combined with administrative data on GPA in the first two high school years, as explained in Appendix G.4.1.

do not depend on choices, $\bar{c}_i^{15b}$, $\beta_{1i}^{Pb}, \beta_{2i}^{Pb}, \beta_{1i}^{Gb}$ and $pgrad_i$ (see Section 5.2.1). In period 1, corresponding to the last two high school years, students choose study effort. They derive utility from the knowledge they acquire through study effort, and face a cost of exerting effort. The per-period utility associated with each effort choice $d_{i1} = e_i \in \{0, 1, ..., E\}$ is:

$$u_{i1}(d_{i1}, \Omega_{i1}) = \xi_{1k_i} d_{i1} + \xi_2 d_{i1}^2 \tag{5}$$

where the constant is normalized to zero because only the difference in utilities is identified. We let the coefficient on effort vary across student types to capture heterogeneity in preference for knowledge and effort cost.

5.2.3 Time 2: Choice to take the entrance exam

In period 2, students decide whether to take the PSU entrance exam. As in the real world, they do not yet know their entrance exam score or whether they are in the top 15% of their school, and must base their decision on beliefs about these outcomes based on Ω_{i1} and the effort choice from period 1. The per-period utility associated with the second period choice d_{i2} is:

$$u_{i2}(d_{i2}, \Omega_{i2}) = \begin{cases} -c_0^S + c_1^S T_i + \eta_i & \text{if } d_{i2} = \text{“Take the exam”} \\ 0 & \text{if } d_{i2} = \text{“Do not take the exam”} \end{cases} \tag{6}$$

where Ω_{i2} contains Ω_{i1}, the effort choice d_{i1}, and the realization of the current-period shock. T_i is a dummy equal to 1 if student i is in a treated school, equal to 0 otherwise, and η_i follows the standard logistic distribution. The per-period utility from not taking the exam is normalized to 0 because only the difference in utilities is identified. Parameter c_0^S captures the monetary and non-monetary costs of taking the exam.[18] Parameter c_1^S captures any treatment impact on the perceived value of taking the PSU. Students in treated schools receive orientation on the higher education system, including explanations of when the PSU is or is not required. If students previously believed the PSU was necessary for enrolling in vocational or non-selective institutions but learn that it is not, or if they believed the PSU was not necessary for enrolling in selective colleges but learn that it is, their perceived utility from taking the exam may decrease or increase. To simplify estimation, we do not let c_0^S and c_1^S depend on type. Nonetheless, forward-looking behavior guarantees that the type affects the choice of taking the exam through its impact on the value of college enrollment, discussed below.

[18]The fee is approximately USD 30, with most students in the sample eligible for a fee waiver. However, disadvantaged students can face non-monetary barriers to taking entrance exams.

5.2.4 Time 3: Admissions

In period 3, admissions to selective colleges through the regular and the PACE channels are realized according to objective admission chances, which depend on the entrance exam scores and GPAs actually achieved. We model the admission chances and the selectivity of the programs students are admitted to as exogenous processes that approximate the allocation mechanisms described in Section 2.1. Program selectivity is a reduced-form function: realized program selectivity reflects both the application portfolio submitted by the student and the admission rule that maps applications, scores, and GPA ranks into offers. We do not model application portfolios explicitly.

Regular channel admissions. These admissions are based on actual scores on the PSU entrance exam. With d_{i1} denoting the effort choice in time 1, the PSU score is produced according to the following function:

$$PSU_i = \beta^P_{0k_i} + \beta^P_1 d_{i1} + \beta^P_2 GPA_{i,t-1} + \beta^P_3 \text{simce}_{i,t-1} + \epsilon^P_i, \quad \epsilon^P_i \sim N(0, \sigma^2_{PSU}). \tag{7}$$

The type-specific intercept $\beta^P_{0k_i}$, varying across types, captures unmeasured heterogeneity in test taking ability. Given a PSU score, the probability that a student receives a regular admission is:

$$Pr(A^R_i = 1|PSU_i) = \Phi(\gamma_0 + \gamma_1 PSU_i + \gamma_2 PSU^2_i + \gamma_3 PSU^3_i), \tag{8}$$

where $\Phi(\cdot)$ is the standard Normal cumulative distribution function. Equation (8) approximates the allocation mechanism by assuming that regular admission chances depend solely on entrance exam scores. Conditional on receiving a regular admission, program quality q^R_i (measured as the average entrance exam score of the program's regular entrants) is determined as:

$$\begin{aligned} q^R_i \;=\; & \phi^R_0 + \phi^R_1 PSU_i + \phi^R_2 PSU^2_i + \phi^R_3 \text{simce}_{i,t-1} + \phi^R_4 \text{simce}^2_{i,t-1} \\ & + \phi^R_5 track_i + \phi^R_6 track_i \times \text{simce}_{i,t-1} + \eta^R_r + \mu^R_i \quad \mu^R_i \sim N(0, \sigma^2_{q^R}), \end{aligned} \tag{9}$$

where PSU_i is the entrance exam score, $\text{simce}_{i,t-1}$ is the 10^{th} grade standardized SIMCE score, $track_i$ is the high school track (academic or vocational), and η^R_r are region-of-residence fixed effects. Equation (9) captures the fact that the admission quality depends on the PSU score through the allocation mechanism, and on the application portfolio. The latter depends on several factors. The baseline SIMCE score and its square capture the students' background, the high school track reflects school-level application support (with the SIMCE interaction capturing support customization), and the region fixed effects capture local supply of college programs.

Preferential channel admissions. These admissions are based on actual within-school GPA ranks, considering average GPA in the four high school years. GPA in the last two high school years is produced according to the following function:

$$GPA_i^{11-12} = \beta_{0k}^G + \beta_1^G d_{i1} + \beta_2^G \text{GPA}_{i,t-1} + \beta_3^G \text{simce}_{i,t-1} + \epsilon_i^G \quad \epsilon_i^G \sim N(0, \sigma_{GPA}^2). \tag{10}$$

The type-specific intercept β_{0k}^G, varying across types, captures unmeasured heterogeneity in grade attainment ability. Students in control schools and students in treated schools who did not take the entrance exam do not receive preferential admissions. Among students in treated schools who took the exam, preferential admissions are assigned to those with a high school GPA in the top 15% of their school. High school GPA, GPA_i^{9-12}, is the average between GPA in the first two years, $GPA_{i,t-1}$, and GPA in the last two years, GPA_i^{11-12}. Conditional on receiving a preferential admission, program quality q_i^P is determined as:

$$\begin{aligned} q_i^P \;\; = \;\; & \phi_0^P + \phi_1^P GPA_i^{9-12} + \phi_2^P (GPA_i^{9-12})^2 + \phi_3^P \text{simce}_{i,t-1} + \phi_4^P \text{simce}_{i,t-1}^2 \\ & + \phi_5^P track_i + \phi_P^R track_i \times \text{simce}_{i,t-1} + \eta_r^P + \mu_i^P \quad \mu_i^P \sim N(0, \sigma_{q^P}^2). \end{aligned} \tag{11}$$

As in the regular channel, students need to submit preference lists. Equation (11) captures heterogeneity across students in application lists and in their GPA, which is the main factor determining the allocation of preferential college seats given application lists (Appendix A.2).

Discussion. For parsimony and to simplify estimation, we do not let equations (8), (9), and (11) depend on type. In the Chilean centralized system, regular admission chances and the quality of the regular and preferential programs to which a student is admitted are determined by admission scores and application portfolios. We abstract from application portfolios through two assumptions. First, we assume that portfolios affect where, but not whether, a student is admitted through the regular channel, and therefore let regular admission probabilities depend only on the observed entrance exam score. Second, we assume that observed characteristics adequately capture portfolio choices (in both the regular and preferential application processes) when predicting program quality. These restrictions allow us to exclude type from these equations and estimate them without solving the model; as discussed in Section 6.2, they provide a good fit to the data. The assumptions are made for convenience; allowing these equations to depend on type would complicate estimation, but our rich longitudinal data would, in principle, allow identification of type-dependent parameters.

5.2.5 Time 4: Choice to enroll

In model period 4, students decide whether to enroll in selective college and through which channel (regular or preferential), given their admissions. They may receive no admission, admission through one channel, or admissions through both channels. Their state space Ω_{i4} contains their admission set, their past choices that enter the current-period rewards, their belief about the likelihood of persisting in selective colleges, the current-period shock realizations, and their characteristics and type. Past beliefs about admission chances do not enter Ω_{i4}, but they shape enrollment decisions through their effects on earlier choices that determined the admission sets.

Students who enroll in selective colleges can drop out or persist, and these two outcomes will give different utilities. In time period 4, students form the expected utility of enrolling using the subjective probability of persisting, $pgrad_i^b$. The expected utility associated with each fourth period choice d_{i4} is:

$$u_{i4}(d_{i4}, \Omega_{i4}) = \begin{cases} \lambda_{0k_i} + pgrad_i^b(\lambda_0^G + q_i^R) + \nu_i^R & \text{if } d_{i4} = \text{“Enroll, regular”} \\ \lambda_{0k_i} + \delta + pgrad_i^b(\lambda_0^G + q_i^P) + \nu_i^P & \text{if } d_{i4} = \text{“Enroll, preferential”} \\ 0 & \text{if } d_{i4} = \text{“Do not enroll”} \end{cases} \quad (12)$$

where q_i^R and q_i^P are defined in equations (9) and (11), and ν_i^R and ν_i^P follow standard logistic distributions. The coefficient on the quality of the program is normalized to one, setting the scale for model utilities. The parameter δ captures any utility cost or premium associated with enrolling through the PACE channel. On one hand, students may derive utility from preferential enrollment if they value the additional tutoring reserved for PACE students. On the other hand, they may experience disutility if enrolling through PACE carries social stigma or undermines their self-image. The term $\lambda_0^G + q_i^J$, $J = R, P$ captures the additional value from persisting in college, which depends on college selectivity because degrees from more selective programs tend to lead to better job market opportunities.[19] The utility from non-enrollment is normalized to zero. Depending on their type, students have different enrollment utilities relative to the outside options, capturing heterogeneous tastes, barriers, and outside options.

5.2.6 Time 5: Persistence in selective college

In the final model period, students who enrolled in selective college face an exogenous probability of persisting. Specifically, a student who enrolled in selective college and exerted effort

[19] The expected utilities from enrolling are equal to $pgrad_i^b$ times the utility from enrolling and persisting (for the regular channel, this is $\lambda_{0k_i} + \lambda_0^G + q_i^R + \nu_i^R$) plus $(1 - pgrad_i^b)$ times the utility from enrolling and dropping out (for the regular channel, this is $\lambda_{0k_i} + \nu_i^R$).

d_{i1} in the first period is still enrolled five years after high school with the following probability:

$$Pr(Persist_i = 1|k_i, d_{i1}, \text{simce}_{i,t-1}) = \Phi(\rho_{0k_i} + \rho_1 d_{i1} + \rho_2 \text{simce}_{i,t-1}), \tag{13}$$

where $\Phi(\cdot)$ is the standard Normal cumulative distribution function. The persistence probability depends on students' unobserved type, the effort they exerted in high school, and baseline achievement.

5.3 Model Solution

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

$$V_t^b(\Omega_{it}) = \max_{d_{it}\in D_{it}} \left\{ u(d_{it}, \Omega_{it}) + E^b \left[V_{t+1}(\Omega_{it+1}|\Omega_{it}, d_{it}) \right] \right\}. \tag{14}$$

Ω_{it} evolves according to *objective* production functions and admission probabilities. 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}.

6 Survey Measures, Estimation and Identification

6.1 Survey Measures

To estimate the model we rely on survey measures of beliefs and study effort.

We obtain from the survey both the expected PSU score, $\overline{PSU}_i^b$, and students' perceived returns to effort in PSU production. The expected PSU score is an outcome variable, as it depends on effort. In contrast, the perceived returns are model initial conditions. To measure perceived returns to effort, we elicited students' beliefs about the effort required to achieve a PSU score of at least 350, 450, and 600. The left panel of Figure 5 shows the distribution of responses. To express returns in terms of the standardized PSU score, which is the unit used in the model, we standardize these hypothetical levels using the mean and standard deviation of PSU scores in the test-taking population. Letting 350^s, 450^s and 600^s denote the standardized levels, we compute β_{1i}^{Pb} and β_{2i}^{Pb} in equation (2) as follows:

$$\begin{aligned} \beta_{1i}^{Pb} &= \frac{450^s - 350^s}{e_i^{450} - e_i^{350}} \\ \beta_{2i}^{Pb} &= \frac{600^s - 450^s}{e_i^{600} - e_i^{450}}, \end{aligned} \tag{15}$$

where e_i^X, $X \in \{350, 450, 600\}$, is the hypothetical effort reported in the survey for each corresponding hypothetical PSU level. By construction, a kink occurs at the effort level the student believes is necessary to achieve at least 450 on the PSU, implying that $e_{kink,i}^{Pb} = e_i^{450}$.

We obtain from the survey both the expected GPA in high school, $\overline{GPA}_i^{9-12,b}$, and students' perceived return to effort in its production. The expected GPA is an outcome variable, as it depends on effort, while the perceived return is a model initial condition. To measure the perceived return to effort in GPA production, we follow a similar approach as the one for the PSU score, using students' beliefs about the effort required to obtain a GPA at least as large as two thresholds: a fixed level of 5.5. and a personalized benchmark—their perceived top 15 percent cutoff. The right panel of Figure 5 shows the distribution of responses. Appendix G.2 provides more details.

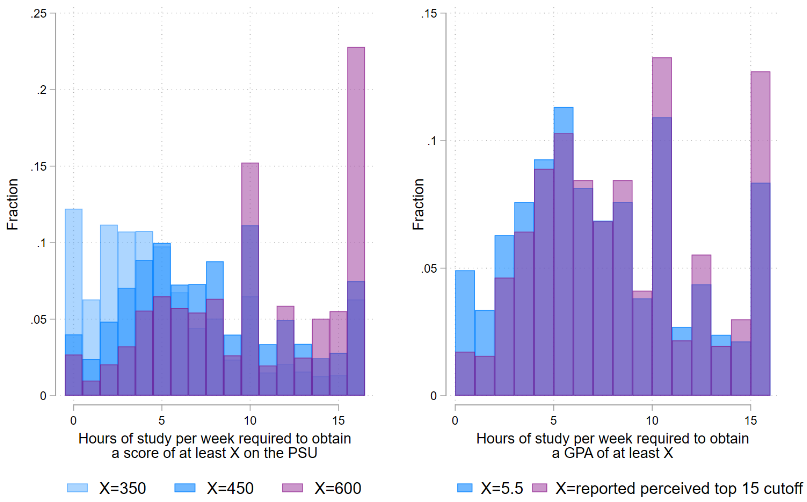

Figure 5: Distribution of answers to survey questions on perceived returns to effort. The reported perceived top 15 cutoff is on average 5.85 in the sample used to construct these histograms. The survey questions are reported in the last row of Table C16. The sample sizes for the left-side graph are: 5,344 for X=350, 5,442 for X=450, 5,469 for X=600. The sample sizes for the right-side graph are: 5,451 for X=5.5, 5,443 for X=reported perceived top 15 cutoff.

Appendix Table C19 presents descriptive statistics on the perceived returns to effort thus calculated, for GPA and PSU. We set to missing the responses that imply negative perceived returns to effort. Because the elicitation asks how many study hours per week are needed to reach progressively higher targets, a response implying that a higher target requires *less* effort contradicts the premise of the question; we therefore treat such responses as comprehension errors rather than as informative about beliefs, while retaining the positive responses, which are consistent with that premise.[20]

[20]The survey questions on perceived returns to effort refer to study hours per week, without specifying whether these are aimed at the PSU or GPA. We assume the responses reflect perceived returns to general academic effort rather than effort toward a specific outcome. Students with a negative perceived marginal effect of effort may have misunderstood the question as follows. Since they were answering a sequence of related questions, some may have interpreted the question "How many hours per week do you think you need to study

Descriptive statistics for the individual-specific measure of the expected top 15% cutoff for preferential admission, $\bar{c}_i^{15b}$, are presented in Table 5. Descriptive statistics for the individual-specific perceived likelihood of graduating from a selective college, $pgrad_i^b$, are presented in Section 4.2.2.[21]

Effort is measured through the survey question: "On average, how many hours a week did you study or do homework outside of class time during the first semester of this school year?". We referenced the first semester in the text of the question because it had concluded by the time we conducted the survey. We assume this survey instrument measures true effort with classical measurement error $\epsilon_i^{mee} \sim N(0, \sigma^2_{mee})$, i.i.d. across individuals and independent of all model initial conditions and shocks. We estimate σ^2_{mee} along with the model parameters.[22] Descriptive statistics for this variable are presented in Tables 3 and C7 . Both the effort question and the questions on perceived returns to effort were designed to refer to effort in the same unit—weekly study hours. This allows us to model perceived effort impacts as the product of effort and its returns.

For the beliefs that serve as model initial conditions, we impute missing values as detailed in Appendix G.3.

6.2 Estimation and Identification

While many parameters are either directly measured through the survey or can be estimated using regression analyses without solving the model, the key identification challenges concern the endogeneity of effort and persistent unobserved heterogeneity. To address these challenges, we exploit experimental variation and impose structural restrictions, solving the dynamic model. The remainder of this section discusses the parameters estimated without solving the model and key parameters estimated solving the model, by indirect inference. Identification of the remaining parameters is discussed in Appendix G.4.2.

6.2.1 Parameters estimated without solving the model

Perceived production functions. Thanks to the strategically designed survey questions, the parameters of the PSU and GPA perceived production functions (equations (2) and (17)) are either directly measured ($\beta_{1i}^{Pb}, \beta_{2i}^{Pb}, e_{kink,i}^{Pb}, \beta_{1i}^{Gb}$) or can be identified and estimated through simple

to obtain a GPA/PSU score of at least X?" as: "How many *additional* hours per week do you need to study to obtain a GPA/PSU score of at least X relative to the previous question?" rather than as an absolute number of study hours.

[21] We assign probabilities of 0, 0.25, 0.50, 0.75, and 1 to the Likert-scale responses.

[22] We treat reported study hours as a noisy measure of actual effort as self-reported time-use data is subject to recall bias (Bound, Brown, and Mathiowetz, 2001). In contrast, we assume that students' reported beliefs about the study effort required to achieve various hypothetical GPA and PSU levels (see the survey questions in the bottom row of Table C16) are free of measurement error. This assumption is justified by the fact that these belief-based responses do not require recalling past behaviors but instead involve forward-looking reasoning about academic performance.

OLS regressions, under the assumption that the measurement error on effort is orthogonal to the model's initial conditions ($\beta_0^{Pb}, \beta_3^{Pb}, \beta_4^{Pb}, \beta_0^{Gb}, \beta_2^{Gb}, \beta_3^{Gb}$). Appendix G.4.1 provides full details on identification and estimation. The parameter estimates are reported in Appendix Table G1, and the goodness of fit is shown in Appendix Figure G2. Perceived college persistence is elicited directly through a survey question and the evidence in Section 4.2.2 suggests it is not a function of effort.

Objective admission processes. Model equation (8), approximating the algorithm determining regular admissions, is estimated through a Probit regression in the sample of entrance-exam takers (Appendix Table C20). The selectivity equations (9) and (11) are estimated through OLS (Appendix Table C21). Appendix Figures B11 and B12 show the goodness of fit of these estimated processes. The PSU score is an excellent predictor of regular admission likelihood, and the estimated selectivity functions accurately capture how improvements in the relevant score translate into higher program selectivity.

6.2.2 Parameters estimated solving the model

We use 52 moments, fully listed in Appendix G.4.3, to identify the remaining 34 structural parameters by indirect inference. Appendix G.4.4 provides details of the criterion function and estimation algorithm.

Causal impacts of effort on objective outcomes. We exploit the experimental variation, which provided an exogenous shock to effort, to help identify the parameters governing the objective causal impacts of study effort on GPA, PSU, and college persistence.

In model equation (10) we impose that treatment does not enter the GPA production function directly, consistent with lack of evidence that treatment could have affected GPA through channels other than study effort, such as changes in grading, teaching, or class offerings (Section 4.2.3). Under this exclusion restriction, randomized assignment provides exogenous variation in effort that identifies the causal effect of effort on GPA. Specifically, we target the same first-stage and reduced-form relationships that would underpin a two-stage least squares design: treatment impacts on effort (first-stage) and on GPA (reduced-form).[23] Absent the experiment, identification of these effort effects would have to rely entirely on structural assumptions linking chosen, observed effort to unobservables affecting achievement. These structural assumptions, therefore, are imposed for convenience, rather than because they are required for identification.

[23]Because reported study effort is measured with error, when targeting moments based on study effort we use model-implied reported hours (obtained as the sum of latent effort and an orthogonal measurement error shock) rather than latent effort, ensuring the model-generated moments correspond to those from the data.

Identifying the causal impact of effort on the PSU score cannot rely on a simple instrumental-variables strategy, because PSU scores are observed only for students who choose to take the entrance exam. The structural model handles selection by jointly modelling the exam-taking decision and PSU performance as functions of the same latent type. Specifically, PSU performance is governed by equation (7),

$$PSU_i = \beta^P_{0k_i} + \beta^P_1 d_{i1} + \beta^P_2 GPA_{i,t-1} + \beta^P_3 \text{simce}_{i,t-1} + \epsilon^P_i,$$

and is observed only for students whose optimal period-2 choice is to take the exam.[24] The latent type k_i affects PSU performance through the type-specific intercept and affects selection into exam-taking through the continuation value of college options. Thus, selection is corrected by the joint model of choices and outcomes. Since treatment shifts effort incentives and exam-taking incentives but, by random assignment, does not shift the distribution of types, treatment-control variation in effort, exam-taking, and regular admissions helps discipline the productivity of effort in PSU conditional on this modeled selection.

The identification problem for persistence is analogous. Persistence is observed only for students who enroll in selective college, and enrollment is itself an endogenous choice affected by treatment. Enrollment and persistence depend on the same latent type: type enters enrollment utility through equation (12) and persistence through the type-specific intercept in equation (13). We impose that treatment does not directly enter the persistence equation.[25] Variation in first-year enrollment and fifth-year persistence across treatment and control groups, together with the joint modelling of enrollment and persistence, disciplines the productivity of effort in persistence conditional on selection into college.

Unobserved heterogeneity. We adopt a finite-mixture approach to model unobserved heterogeneity and assume two unobserved types ($K = 2$). The robustness analysis in Appendix D.2 shows that increasing the number of types does not substantially improve the model's ability to match the data, as only 4% of students are estimated to belong to a third type. Type probabilities follow a logit specification that depends on vector Z_i, consisting of gender, an indicator for whether the student at baseline was in the top 15% of the school based on GPA in grades 9-10, and an indicator for whether the student was surveyed in our data collection, to allow for survey attrition based on unobservables. By virtue of the randomization, type probabilities do not depend on treatment status.

[24]We assume that treatment does not directly affect PSU performance, consistent with the evidence from Section 4.2.3.

[25]This assumption is embedded in equation (13), which rules out direct effects of treatment on persistence beyond its indirect effects through effort and enrollment choices. If treatment had an additional direct positive effect on persistence, the estimated effect of effort on persistence would be upward biased.

The variables in Z_i are not added as direct determinants in the effort, PSU, GPA, enrollment, or persistence equations. Instead, they affect choices and outcomes only by shifting the probability of belonging to each latent type. Conditional on the other model initial conditions, we assume that gender, survey status, and baseline top-15 status affect choices and outcomes only through this type distribution. The auxiliary moments use this variation in two ways. Gender and survey status enter selected auxiliary regressions as regressors, so their coefficients summarize heterogeneity associated with these type-probability shifters. Baseline top-15 status is not used as a regressor; instead, we estimate key auxiliary moments separately in the full sample and in the baseline top-15 subsample, so differences across these samples summarize heterogeneity by baseline rank. These regression coefficients and sample-split moments discipline the type probabilities and type-specific parameters. Appendix G.4.2 and Appendix G.4.3 list the corresponding auxiliary moments.

7 Model Results

7.1 Estimation Results

Table C22 presents the parameter estimates. The descriptive evidence showed over-optimistic beliefs about GPA rank and PSU scores. Students also exhibit over-optimistic beliefs about their likelihood of persisting in selective colleges. Using the estimated equation (13), we predict actual persistence probabilities for all students in our sample and compare them with their perceived persistence probabilities ($pgrad_i^b$). While students, on average, expect a 77% likelihood of persistence, their actual persistence probability is only 39%.

We estimate that 25.7% of the sample belongs to type 1, while 74.3% belongs to type 2. Type 1 students display higher test-taking ability ($\beta_{01}^P > \beta_{02}^P$), greater likelihood of persisting in selective colleges upon enrollment ($\rho_{01} > \rho_{02}$), and derive higher utility (or lower cost) from effort ($\xi_{11} > \xi_{12}$). But they also have a lower preference for college compared to the outside option than type 2 students ($\lambda_{01} < \lambda_{02}$), suggesting a comparative advantage for the outside option despite an absolute advantage in all options.

Pre-college study effort has a positive causal effect on selective college persistence ($\rho_1 > 0$). To better interpret the magnitude of this effect, Table 7 shows OLS estimates of linear probability model versions of equation (13), estimated on simulated data. Column 1, which does not control for unobserved type, shows that the correlational return of one additional hour of study in college persistence is 1.3 percentage points (p.p.), closely aligned to the 1.7 p.p. obtained from similar regressions estimated on real data (column 4 of Table C15). However, type 1 students, who have a higher propensity to persist, also exert more effort on average. Therefore, after controlling for the type dummy, the coefficient on study hours reduces to 1 p.p. (column 2). These findings suggest that 77% of the correlational returns to effort represent a causal effect, while the remainder is driven by omitted variable bias.

Table 7: Returns to pre-college effort in persistence and omitted variable bias

	Simulated college persistence probability	
	(1)	(2)
Simulated study hours/week	0.013	0.010
Unobserved type 1		0.035
Outcome mean	0.370	0.370

Note.– The coefficients are OLS estimates of regressions of the simulated persistence probability on baseline Simce test scores and on simulated weekly study hours in high school. The second column includes a dummy for the unobserved student type as control variable. Simulations are performed using the structural-model estimation sample and the estimated model parameters. Outcome mean for this sample reported.

The estimated causal returns to effort suggest that the observed effort decline—approximately 0.20 study hours per week— can explain only a small share of the approximately 2-percentage-point lower persistence rate among college entrants from treated schools (Section 3). One fewer hour of study per week decreases the persistence likelihood by 1 p.p., while belonging to type 2 lowers it by 3.5 p.p. The majority of the gap, therefore, seems to be driven by selection on unobservables. In fact, the model indicates that the share of type 2 students (those least likely to persist) is 17 p.p. higher among college entrants from treated schools.

7.2 Model Fit

The structural model achieves a good fit. Table C23 compares the means and standard deviations of observed and simulated outcomes, separately for the control and treatment groups. The model closely replicates the observed averages for pre-college outcomes (such as hours of study and GPA), educational decisions (college entrance exam participation and enrollment), and college outcomes (admissions, program selectivity, and persistence). It under-predicts the selectivity of programs chosen by control group students and the likelihood of enrolling through the preferential admission channel when both offers are received, although it correctly captures the larger propensity to choose the regular seat in this case. The model closely matches the standard deviations of all variables, aside from an over-estimation of the variability of study hours.

The model successfully replicates the targeted treatment effects of interest. The simulated treatment effects on admissions, enrollment, and persistence closely align with the point estimates from the observed data and fall within their confidence intervals (Figure 6). In addition, the model matches the outcome means for the control group (Table C24). This holds for both the full sample and the subsample of students in the top 15 percent of the baseline GPA distribution, although for the latter sample it overestimates admissions and enrollments.

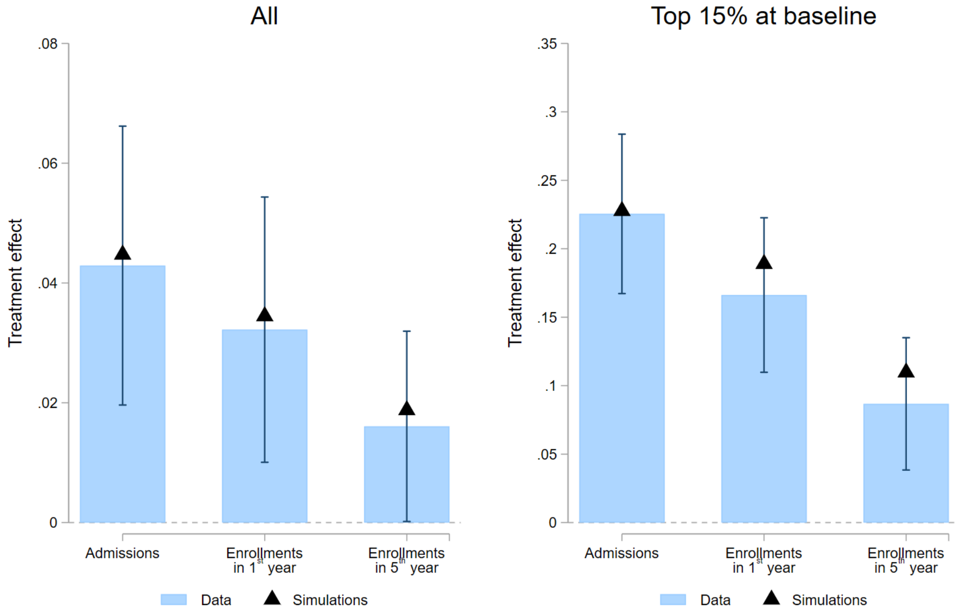

Figure 6: Model fit—*targeted* moments. Effects of PACE on admissions, enrollments and persistence. The left panel shows results for the sample of all students in the experiment. The right panel shows results for the sample of all students who at the end of 10^{th} grade, before the experiment started, were in the top 15% of their school according to GPA in the first two high school years. The bars represent treatment effects calculated using the actual data, with 95% confidence intervals reported. The triangles represent treatment effects calculated using the data simulated from the estimated structural model. The outcomes are constructed as in the main analysis in section 3.

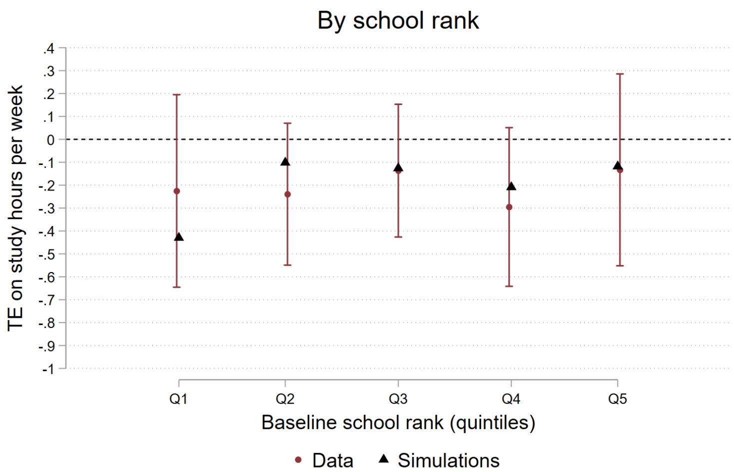

Figure 7: Model fit—*untargeted* moments. Heterogeneity of the effects of PACE on pre-college study effort by quintile of the GPA ranking in the first two high school years. The graphs plot, for each quintile, the estimated coefficients on treatment of an OLS regression where the outcome variable is hours of study per week and the standard set of controls is included. We use Inverse Probability Weights in both regressions. Regressions on actual data also include fieldworker fixed effects.

The model also broadly captures treatment effects on pre-college outcomes and their control means. While it cannot capture the reduction in 12^{th} grade GPA (columns 5 and 6 of Table 8), it replicates the average negative impact on study effort (columns 1 and 2), and closely matches the treatment effect on the likelihood of taking the college entrance exam (columns 7 and 8). The model also reproduces the negative interaction between treatment status and perceived distance from the admission cutoff (columns 3 and 4), correctly capturing the role of biased beliefs in shaping pre-college behavior. Importantly, the model reproduces also moments that were not explicitly targeted in the estimation. Figure 7 shows that it captures the heterogeneity in the treatment effect on weekly study hours across quintiles of baseline GPA.

Table 8: Model Fit - Effect of PACE on pre-College Outcomes

	Study hours/week		Study hours/week		12^{th} grade GPA		Take PSU	
	Data	Simulations	Data	Simulations	Data	Simulations	Data	Simulations
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	-0.193	-0.195	-0.078	-0.094	-0.056	-0.006	-0.028	-0.027
	(0.113)		(0.141)		(0.048)		(0.028)	
Treatment × Perceived distance			-0.263	-0.151				
			(0.113)					
Control mean	4.254	4.284	4.180	4.336	5.752	5.736	0.661	0.641

Note.– The coefficients are OLS estimates; standard errors clustered at the school level are reported in parentheses for the data columns. All regressions include all model initial conditions except region and survey missing. Field-worker fixed effects were used for columns (1)-(4). Inverse Probability Weights were used for columns (1)-(6). *Treatment* is a dummy variable indicating whether a student is in a school randomly assigned to be in the PACE program. *Perceived distance* is the absolute value of the difference between perceived own GPA and the perceived 85^{th} percentile of the GPA distribution in the school. The outcome variable in columns (1)-(4) is the number of hours of study per week. In columns (3) and (4) we add the interaction of *Perceived distance* with *Treatment* and with all the initial conditions and fieldworker fixed effects. The outcome variable in columns (5) and (6) are the GPA in grade 12, measured in GPA points (ranging from 1 to 7). The outcome variable in columns (7) and (8) is an indicator for sitting the college entrance exam. All regressions are estimated on the sample of students for whom the outcome variable is non-missing in the data.

7.3 Counterfactual Experiments

The counterfactuals use the model to examine how PACE would operate under alternative belief environments and policy designs. The evidence above points to three belief-related concerns. First, students are over-optimistic about academic outcomes relevant for admission, including their relative GPA rank. Second, they substantially overestimate their probability of persisting in selective college. Third, they do not perceive persistence to depend on pre-college effort, even though the model estimates a positive causal effect of effort on persistence. These biases can affect PACE in opposite directions: optimism about college success can increase take-up, while weak perceived returns to effort in college success can reduce pre-college effort. The counterfactuals therefore ask whether correcting beliefs would mitigate the effort response, whether broad belief correction would have unintended effects on take-up, and how changing the preferential-admission cutoff would alter enrollment and persistence.

We implement all simulations using students randomly assigned to the control group in the data. Because of random assignment, this sample is representative of students in PACE-eligible schools. The counterfactual estimates are therefore average treatment effects for the full simulation sample, or subgroup average treatment effects for the baseline top-15 sample.

7.3.1 Distortions due to belief errors

This first counterfactual is diagnostic: it removes all belief errors at once to assess how much distorted beliefs shape students' behavior and their response to the introduction of PACE.

Simulation details. We simulate an environment in which students hold rational expectations (RE), and compare their choices and outcomes with and without PACE. Under RE, students use objective rather than subjective functions for GPA and PSU production, regular and preferential admission likelihoods, and college persistence. This correction changes both perceived outcome levels, such as the probability of persisting in college, and perceived marginal returns, such as how effort affects admission chances and persistence. In PACE schools, students play a tournament game for the allocation of PACE seats, with seat assignments based on their simultaneous effort choices. We solve for the Bayesian Nash Equilibrium of this game using the fixed-point algorithm detailed in Appendix G.5.

Results. Table 9 compares the baseline simulated effect of PACE under estimated beliefs with the effect of PACE when students hold rational expectations. In a RE world, PACE would not have caused the observed reduction in effort, on average (row 1, column 2). By contrast, under estimated beliefs, the model reproduces the finding of an effort reduction (row 1, column 1). On one hand, under RE, PACE would have slightly raised the marginal returns to effort in admission at all effort levels—by less than one percentage point per additional weekly study hour (Appendix Figure B13). On the other hand, students under RE would have valued admission less, expecting much lower persistence chances than under the belief errors we documented. As a result, under RE, PACE would not have affected pre-college effort on average and would have had smaller enrollment impacts due to lower take-up rates (compare row 4 of Table 9 to row 4 column 1 of Table 10). This suggests that students' belief errors shaped the observed effort reductions in response to PACE, but also potentially boosted take up.[26]

7.3.2 The effects of PACE combined with informational interventions

We next consider policy-relevant belief corrections that could be combined with PACE. These counterfactuals distinguish broad belief correction from a targeted correction of the specific bias most directly linked to the effort response. Broad correction gives students rational expectations about all model objects. The targeted correction instead leaves students' over-optimism about their overall persistence probability in place, but corrects their belief about the marginal returns to effort in college persistence. In both counterfactuals, we compare each student's simulated

[26] Appendix H.2 discusses the additional result that under rational expectations, students would have exerted substantially less effort, regardless of PACE.

Table 9: SIMULATED PACE EFFECTS UNDER ESTIMATED BELIEFS AND UNDER RATIONAL EXPECTATIONS

	(1) Baseline PACE effects	(2) PACE effects under rational expectations
Study hours/week	-.0807	.0008
Took entrance exam	-.0253	.0057
Admitted	.0632	.0699
Enrolled	.046	.0435
Enrolled and persisted	.0236	.0191
Enrolled and dropped out	.0224	.0244

NOTE. – This table reports average simulated effects of introducing PACE under two belief environments. Column (1) uses the estimated baseline belief environment. Column (2) sets beliefs to rational expectations in both the no-PACE and PACE simulations. In each column, the reported effect is the simulated mean outcome with PACE minus the simulated mean outcome without PACE, averaged over the model simulation sample, which corresponds to all individuals in the control group in the data.

outcomes without PACE or belief correction to their simulated outcomes under PACE combined with the specified belief correction.

Simulation details. We consider two hypothetical additions to PACE: (i) correcting all beliefs, that is, giving students rational expectations (PACE + RE); (ii) correcting only beliefs about the returns to effort in persistence (PACE + Correct effort return beliefs). The second counterfactual is motivated by the finding that students do not perceive college persistence to depend on pre-college effort. It asks whether an information intervention that makes this effort-persistence link salient could preserve PACE's enrollment gains while avoiding the reduction in pre-college effort. Specifically, we set perceived persistence probability to

$$0 \leq \left(pgrad_i^b - \frac{\partial Pr(Persist_i = 1)}{\partial d_{i1}} \cdot d_{i1}^{bl}\right) + \frac{\partial Pr(Persist_i = 1)}{\partial d_{i1}} \cdot d_{i1} \leq 1,$$

where $pgrad_i^b$ is the perceived persistence probability at baseline, d_{i1} is the effort choice, and the derivative is the true marginal effect of effort on persistence, obtained by taking the derivative of the right-hand side of equation (13): $\rho_1 \cdot \phi(\rho_{0k_i} + \rho_1 d_{i1} + \rho_2 \text{simce}_{i,t-1})$. Probabilities below 0 (above 1) are set to 0 (1). The term $\left(pgrad_i^b - \frac{\partial Pr(Persist_i=1)}{\partial d_{i1}} \cdot d_{i1}^{bl}\right)$, where d_{i1}^{bl} is the effort exerted at baseline, represents the perceived persistence probability when exerting no effort, and guarantees that this counterfactual changes perceived persistence probabilities only when it changes effort choices. Thus, students still overestimate their baseline persistence probability, but they now correctly perceive how additional pre-college effort changes that probability.

Results. Figure 8 shows that correcting all beliefs reduces the impact of PACE on college admissions, first-year enrollment, and fifth-year enrollment relative to providing PACE alone. The results are driven by large reductions in pre-college effort. While PACE alone reduces study effort by less than one hour per week, combining PACE with an intervention correcting all beliefs reduces effort by over three hours per week, as students update their overly optimistic beliefs about the returns to effort and their likelihood to persist in college (Table 10, column 3).

This intervention also lowers entrance-exam-taking, a pre-requisite for admissions, especially among students with lower baseline test scores (Appendix Figure B14). Although full belief correction lowers the long-term enrollment gains from PACE, the last row of Table 10 reveals that by mitigating impacts on enrollment, it also mitigates impacts on the share of students who enroll only to later drop out (–13.6%, columns 1 and 3).

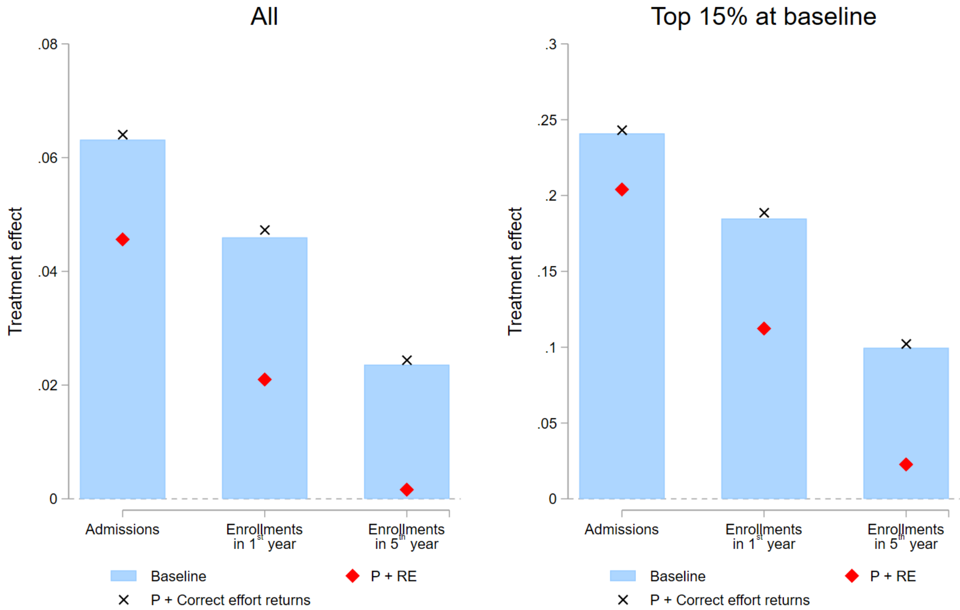

Figure 8: Simulated effects of various interventions on admissions, enrollments and persistence. The blue bars represent impacts under the baseline intervention (i.e., the introduction of PACE), the markers show effects under counterfactual interventions. The graphs are based on simulations from the control group sample. For each student, we simulate outcomes in the control condition and under three interventions, and calculate the interventions effect. The left panel shows simulations for the entire sample; the right panel is restricted to the sub-sample of 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 outcomes are constructed as in the main analysis in section 3. The three interventions are PACE (Baseline), PACE combined with rational expectations (P + RE), and PACE combined with correcting beliefs about the marginal effect of effort on the probability of persisting in college (P + Correct effort returns). See section 7.3.2 for simulation details.

Column 4 of Table 10 shows why targeting this particular bias matters. Correcting only beliefs about the return to effort in persistence avoids most of the pre-college effort reduction, while leaving the program's effects on admissions, enrollment, and fifth-year enrollment essentially unchanged, as also shown in Figure 8. Unlike full belief correction, this intervention does not reduce take-up by lowering students' perceived level of college persistence, nor does it generate heterogeneous reductions in entrance-exam-taking among lower-achieving students (Appendix Figure B14).

Finally, we show in Appendix H.3 that, for all the policy desings we consider, policy impacts on pre-college effort affect policy impacts on persistence through two channels: by changing the composition of students who enter college, and by altering the extent of their preparation during high school.

Table 10: Simulated effects of baseline and counterfactual interventions

	(1) PACE	(2) Rational expectations	(3) PACE+Rational expectations	(4) PACE+Correct effort returns
Study hours/week	-.081	-3.126	-3.125	-.017
Took entrance exam	-.025	-.105	-.1	-.022
Admitted	.063	-.024	.046	.064
Enrolled	.046	-.022	.021	.047
Enrolled and persisted	.024	-.017	.002	.024
Enrolled and dropped out	.022	-.005	.019	.023

Note. – This table shows average effects of various hypothetical interventions. For each individual in the control group in the data, we simulate a control condition in which no intervention is introduced, and various conditions in which the intervention indicated in the column heading is introduced. We calculate the intervention effect for each individual, and report here the sample average. Column 1 introduces PACE alone. Column 2 introduces rational expectations in the absence of PACE. Column 3 combines PACE with rational expectations. Column 4 combines PACE with information about the correct marginal effect of effort on the probability of persisting in college.

These results suggest that combining PACE with interventions fully correcting pre-college beliefs may exacerbate its negative impacts on pre-college effort and dampen positive impacts on college participation. This is because over-optimistic beliefs lead students to exert substantially more pre-college effort and accept college admissions at higher rates than they would under rational expectations, both with and without PACE. A government aiming to promote pre-college effort among students targeted by preferential admissions could instead design interventions that emphasize its importance for college success. Strengthening impacts on long-term college attainment, however, would require further targeted interventions aimed at improving the college preparedness of college entrants. Whether increasing long-term college attainment is itself a welfare-improving policy objective lies beyond the scope of our analysis.

7.3.3 The effects of PACE with alternative cutoffs for preferential admissions

The final counterfactual varies the generosity of the preferential-admission rule. This exercise targets a different policy margin: given students' biased beliefs about their relative GPA rank and their admission chances, changing the cutoff changes both eligibility and perceived incentives to exert effort.

Simulation details. Because we only elicited beliefs under the actual top-15% rule, we construct beliefs under alternative cutoffs by holding fixed each student's bias relative to the rational-expectations cutoff. Let

$$b_i^c = \bar{c}_i^{15b} - c_i^{15,RE}$$

denote student i's baseline cutoff belief bias, defined as the difference between the perceived top-15 cutoff and the rational-expectations top-15 cutoff.[27] For an alternative top-p rule, we

[27] We define the bias relative to the rational-expectations cutoff because this is the cutoff object the model can recompute under each counterfactual rule.

first solve for the rational-expectations cutoff $c_i^{p,RE}$ and then set the perceived cutoff to

$$\bar{c}_i^{pb} = c_i^{p,RE} + b_i^c.$$

We then solve the model under these counterfactual beliefs and simulate choices and outcomes.

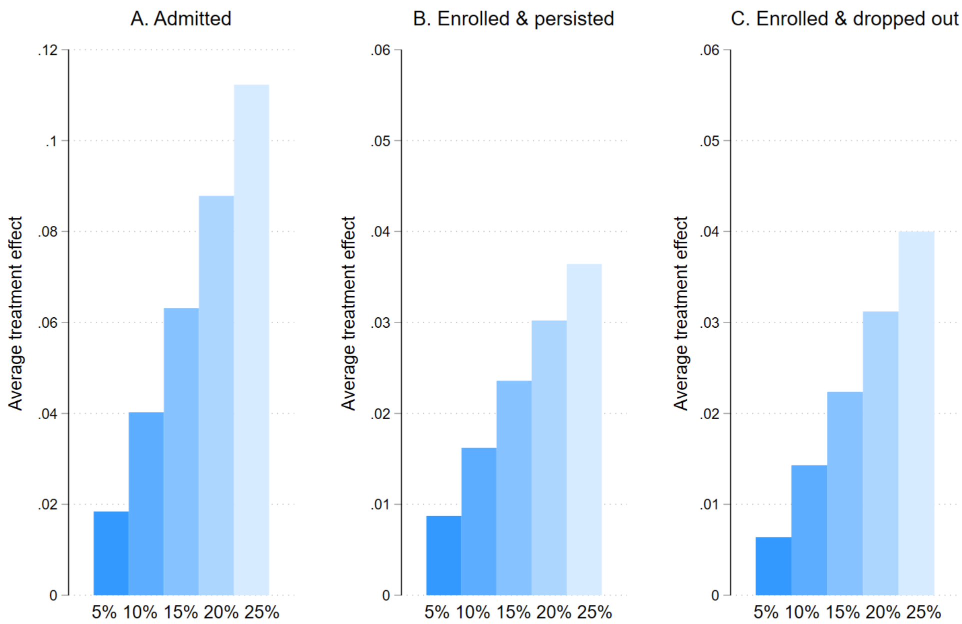

Figure 9: Simulated effects of PACE with alternative cutoffs on selective college outcomes. This figure shows average treatment effects of various hypothetical interventions that vary the within-school cutoff for the preferential admission from top 5% to top 25%. For each individual in the control group in the data, we simulate a control condition in which no intervention is introduced, and a condition in which PACE with the cutoff indicated on the x-axis is introduced. We calculate the intervention effect for each individual, and report the sample average.

Results. Figure 9 shows that more generous cutoffs lead to more students being admitted and persisting in selective college. However, they also lead to an increase in the number of students who enroll but subsequently drop out. Under the top 5%, 10% and 15% cutoff rules, the increase in enrollment followed by persistence exceeds the increase in enrollment followed by dropout. But beyond the 15% threshold, this pattern reverses. These differential effects reflect a shift in the composition of selective college entrants: as the program becomes more generous, it admits students with lower baseline test scores and weaker pre-college effort, reducing the average likelihood of persistence (Figure B15).

8 Conclusions

This paper examines how preferential college admission policies influence educational outcomes by changing high school students' study incentives, and how students' responses to such incentives are shaped by their perceptions. We leverage a unique experimental setting in Chile—the randomized rollout of the nationwide PACE program—combined with rich administrative and primary survey data.

PACE substantially increased selective college admissions and first-year enrollment among disadvantaged students, particularly those at the top of their high school cohorts. However, these gains faded over time, yielding smaller effects on enrollment five years after high school. At the same time, the program generated unintended consequences in the form of reduced pre-college effort and academic achievement.

Our data reveal systematic overconfidence among students about their academic standing and future college success. To examine how beliefs shape behavior and to evaluate counterfactual policies, we estimate a dynamic structural model with subjective expectations that exploits our survey data to relax standard rational-expectations assumptions (Manski, 2004). The estimated model shows that the short- and longer-term impacts of preferential admissions critically depend on students' beliefs. Counterfactual simulations where students in both treated and control schools hold rational expectations reveal that, contrary to students' perceptions, PACE did not offer widespread admission guarantees, but instead modestly increased the returns to effort on average, by making admission attainable. Belief errors were therefore central to the observed effort disincentives.

Pairing PACE with an informational intervention that corrects all belief errors, however, would further reduce pre-college effort and weaken enrollment and persistence effects compared to offering PACE alone. This occurs because, regardless of PACE, students' over-optimism about effort returns and college performance leads them to exert more effort and enroll at higher rates than they would under accurate beliefs, so fully correcting beliefs generates discouragement effects.

Our results highlight that ignoring students' subjective beliefs in policy design can generate unintended outcomes. Preferential admissions without carefully tailored informational interventions can discourage effort. We show that interventions emphasizing the role of pre-college effort in college success could mitigate these disincentives while preserving enrollment gains. Assessing how improving the college enrollment of disadvantaged students ultimately affects their welfare is beyond the scope of this study. An important direction for future research is to examine impacts on graduation and labor market outcomes, which are central to evaluating the trade-off between the costs of college attendance and its long-run benefits.

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Online Appendix

Beliefs and the Incentive Effects of Preferential Admissions: Evidence from an Experiment and a Structural Model

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June 24, 2026

A Additional Institutional Details and Fieldwork Information

A.1 Ministerial Formula to Calculate High School GPA

The Ministry ranks students within each school according to an adjusted high school GPA score, called *Puntaje Ranking de Notas*, calculated using a specific formula. Here, we report an English translation of official information on the formula, which can be found at https://demre.cl/paes/factores-seleccion/puntaje-ranking. Although the adjusted formula calculates each student's relative position compared to prior cohorts, the final ranking determining the within-school top 15% cutoff for PACE seats is computed exclusively among students within the same cohort and school.

First, each student's average grade in each high school year, GPA_{ig}, where $g = 9, 10, 11, 12$, is rescaled to range from 100 to 1000.[1] It is then transformed into a relative score, R_{ig}, broadly capturing where the student ranks compared to the three prior cohorts of students who completed the same grade in the same school in which the student attended that grade (called the "reference population"). Letting $\overline{GPA}_g$ denote the average grade in the reference population, and $maxGPA_g$ the highest grade in the reference population, this grade-specific relative ranking is computed according to the following formula:

$$R_{ig} = \overline{GPA}_g + (GPA_{ig} - \overline{GPA}_g)\frac{1000 - GPA_{ig}}{maxGPA_g - \overline{GPA}_g}$$

[1] The Department of Educational Assessment, Measurement, and Registration (DEMRE) publishes tables to rescale GPA, see for example https://demre.cl/proceso-admision/factores-seleccion/tabla-transformacion-nem-5-procesos-grupo-c.php.

where $\overline{GPA}_g$ and $maxGPA_g$ are expressed on the same 100 to 1000 scale as GPA_{ig}. The final score is then obtained as the following average:

$$adjGPA_i = \frac{R_{i1} + R_{i2} + R_{i3} + R_{i4}}{4}.$$

In our sample, the Pearson correlation coefficient between $adjGPA_i$ and the average of raw GPA in the four high school years is 97.72%.

The top 15% cutoff for PACE seats is determined by ranking students in the same school and cohort according to $adjGPA_i$. Within each school and cohort, the top 15% cutoff is determined as the 85^{th} percentile of $adjGPA_i$.

A.2 PACE Process for the Allocation of Preferential Seats

The PACE and regular applications must be submitted to the centralized system during the same time window. For the cohort included in this study, the PACE and regular admission processes were separate. Students could submit a PACE application list and, separately, a regular application list. In each list a student could include up to ten programs (i.e., college and major combinations), potentially different between the two lists. The two processes were entirely independent, and a student could obtain two admissions simultaneously, one from each process. Here we describe the PACE application and admission process.

For each program listed in their PACE applications, applicants receive a distinct application score, called *Puntaje de Postulación PACE* (PPP). The score is calculated based on the applicant's GPA during the four years of high school and attendance during the 11^{th} and 12^{th} grades. To reduce the occurrence of identical scores across applicants, the score is adjusted for each program, taking into account the program's geographic location and its positional ranking within the applicant's list of preferences.[2]

Applicants to each program are ranked in descending order based on their application score, and available PACE slots are allocated according to this sequence. Should the number of applicants exceed the available slots, those not immediately admitted are placed on a waiting list for their first-choice program. Subsequently, these candidates are considered for admission to the programs listed as their second choice, following the same order-based allocation process. This procedure is iteratively applied to applicants' subsequent choices. Once an applicant is

[2]The formula is $PPP = (0.8 * PRN + 0.2 * GPA) \cdot (1 + bonus_{attendance} + bonus_{geog}) + bonus_{listrank}$, where PRN is the *Puntaje Ranking de Notas* (PRN) used to identify the top 15% of students, which is based on the high school GPA with some adjustments (see Appendix A.1), and GPA is the raw high school GPA. The correlation coefficient between the raw GPA and the PRN is around 98%. The bonus for attendance rewards high school attendance and it reaches a maximum of 5% for students who did not miss a single day and drops to 0 for those missing 15% or more days. A bonus for geographic location of 3.5% (5%) is awarded for applications to universities in the same area of Chile (North, Center, or South) (region of Chile) as the student's high school, and the bonus for the rank of the program within the applicant list decreases with the program's rank; it is measured in score points, and it is 25 for applications listed first, typically representing less than 5% of the total score, and 0 for applications listed tenth.

accepted into a program, they are automatically withdrawn from consideration for any programs ranked lower on their preference list. This measure ensures that no applicant is admitted to more than one program. However, applicants remain eligible for programs ranked higher than their successful application, should they be initially placed on a waiting list for such programs. In instances where a student eligible for a guaranteed PACE slot fails to secure admission in any of their listed preferences, the Ministry of Education employs a proprietary algorithm to determine their placement.

A.3 Fieldwork Information

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

We designed and piloted the surveys. The achievement test questions were developed by the professional testing agencies Aptus Chile and Puntaje Nacional; we extensively piloted the test.

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 to students 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. 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 fieldworkers, the instructions were printed on the first page of the survey and the fieldworkers read them aloud. 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.

B Additional Figures

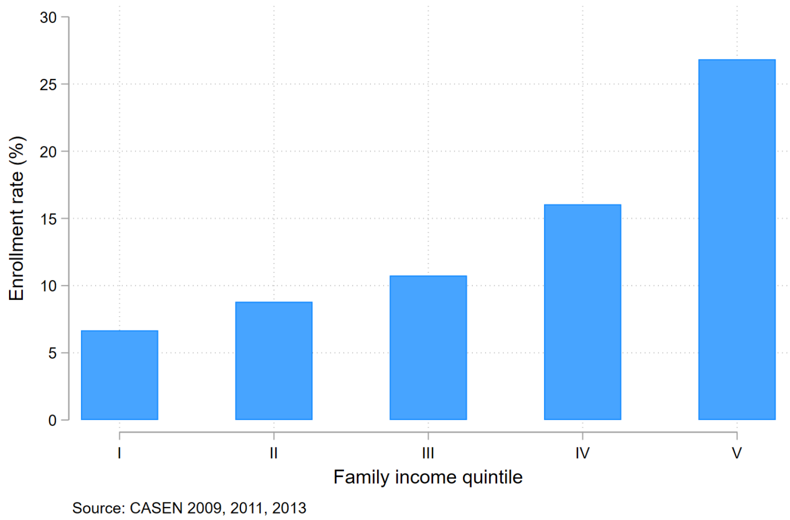


Figure B1: Percentage of 18-19 year-old who are enrolled in college in Chile by family income quintile.

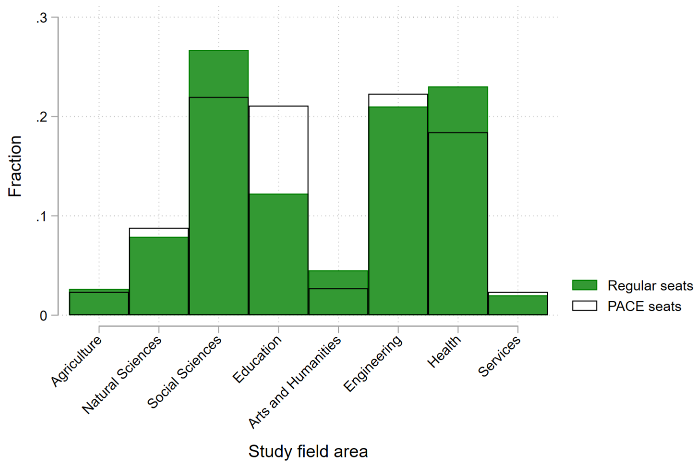


Figure B2: Study field distribution of PACE and regular seats in selective colleges. A degree program is a college and major combination. Source: Administrative data for the 2018 centralized admission process.

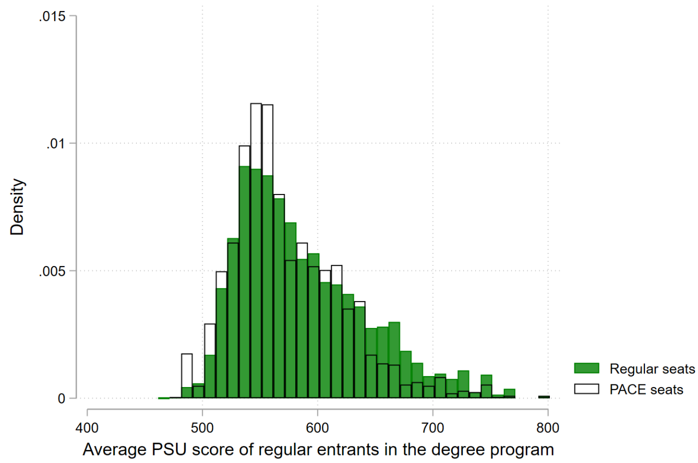


Figure B3: Distribution of selectivity of PACE and regular seats in selective colleges. Selectivity is measured as the average PSU entrance exam score among all regular entrants in the program, which is a college and major pair. Source: Administrative data for the 2018 centralized admission process.

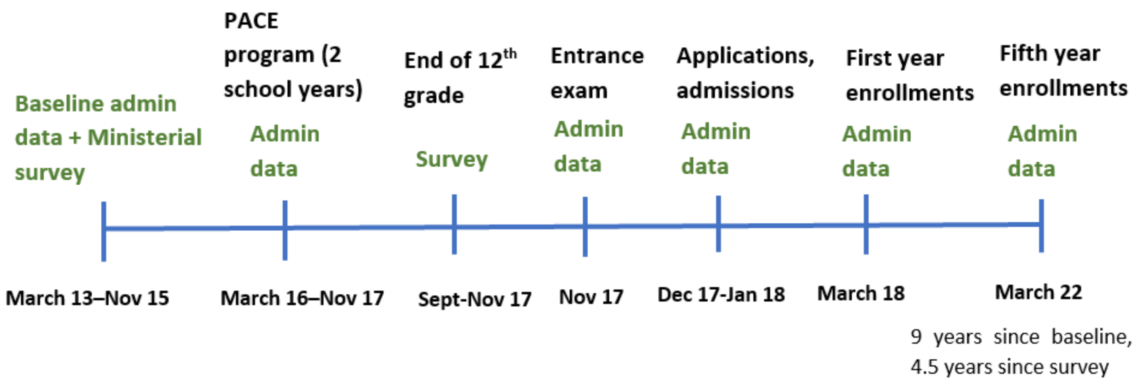


Figure B4: Timeline (months and years shown).

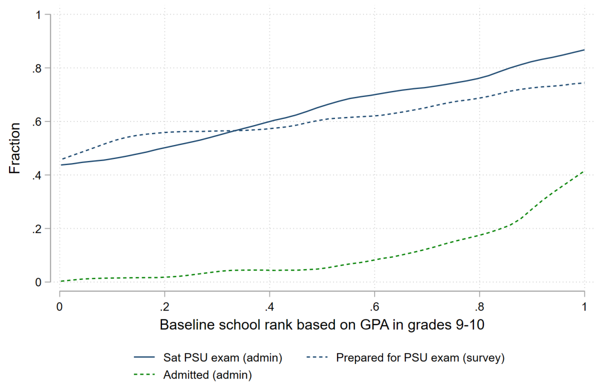


Figure B5: Decision to take and prepare for PSU entrance exam and objective admission likelihood. Sample of students in control schools. Method: smoothed values from kernel-weighted local polynomial regression.

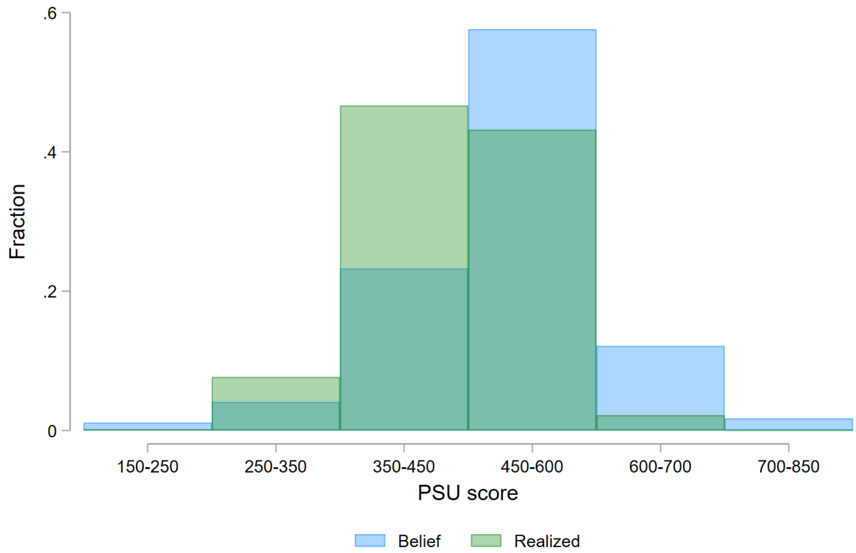


Figure B6: Distribution of beliefs and realizations over PSU score intervals. We elicited students' beliefs through the survey question 'Suppose that you will take the PSU entrance exam this year. What do you think your PSU score will be?' The possible answers are the intervals indicated on the x-axis. Both histograms focus on the sample of students that answered the survey question and took the PSU entry exam.

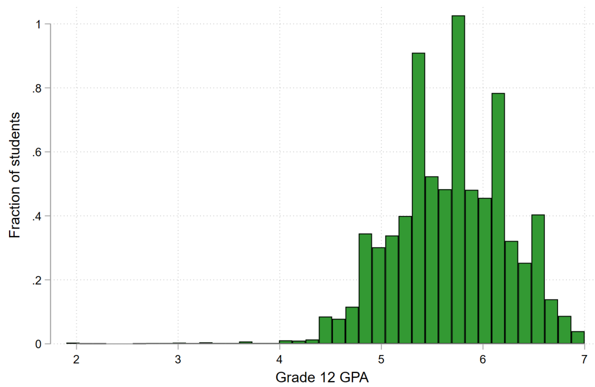


Figure B7: Evidence of grade compression: Histogram of 12^{th} grade GPA.

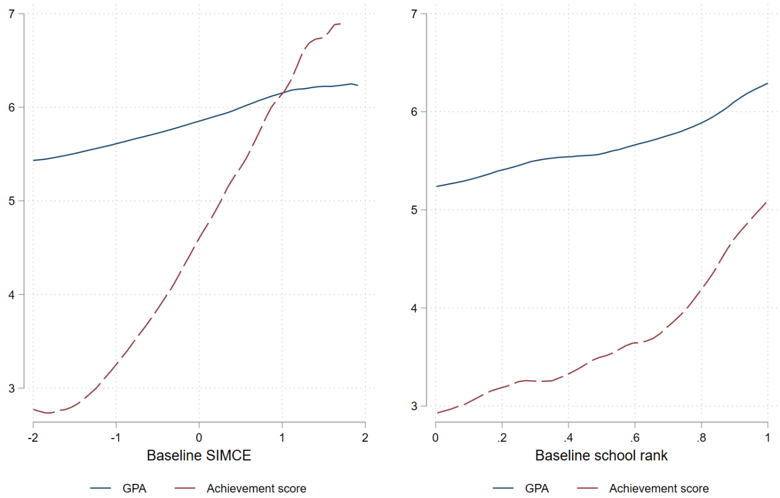


Figure B8: Evidence of grade compression: GPA does not discriminate between students as well as the achievement score does.

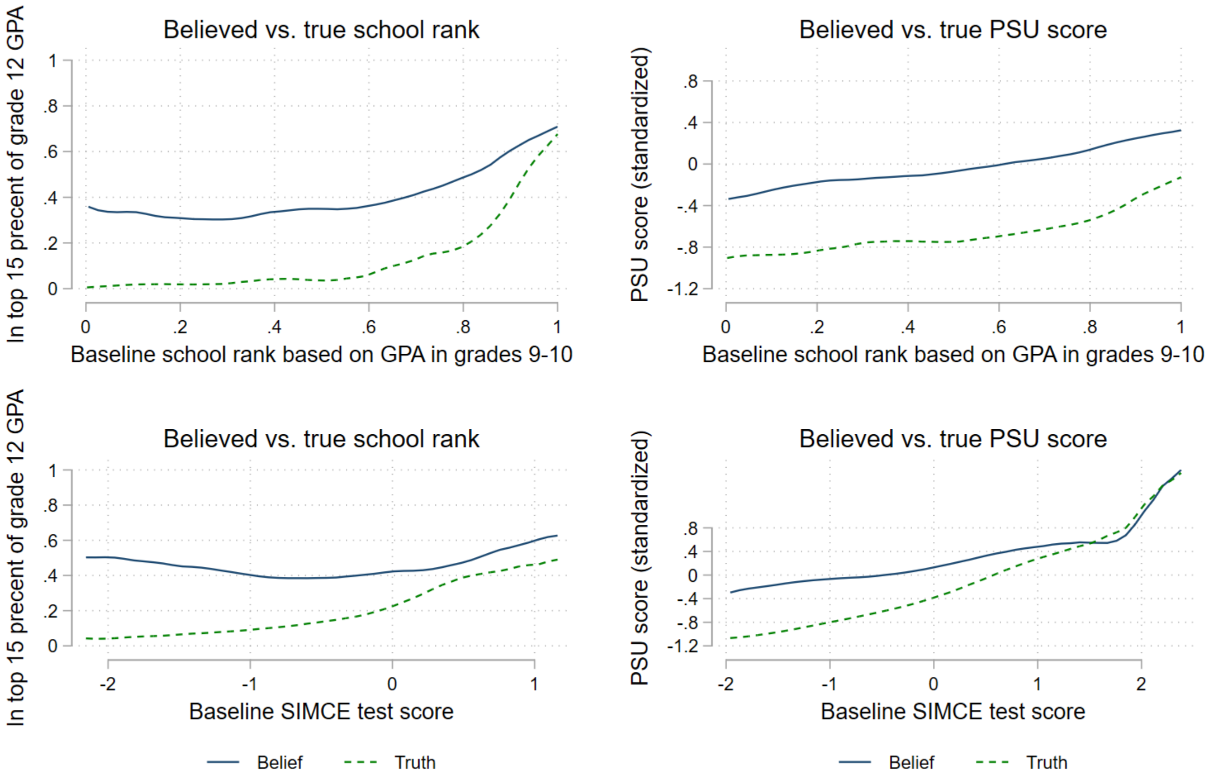


Figure B9: Heterogeneity of subjective beliefs by baseline within-school rank and by baseline test scores. Sample of students in control schools. The bottom graphs trim the top and bottom 1% of SIMCE scores. Method: smoothed values from kernel-weighted local polynomial regression.

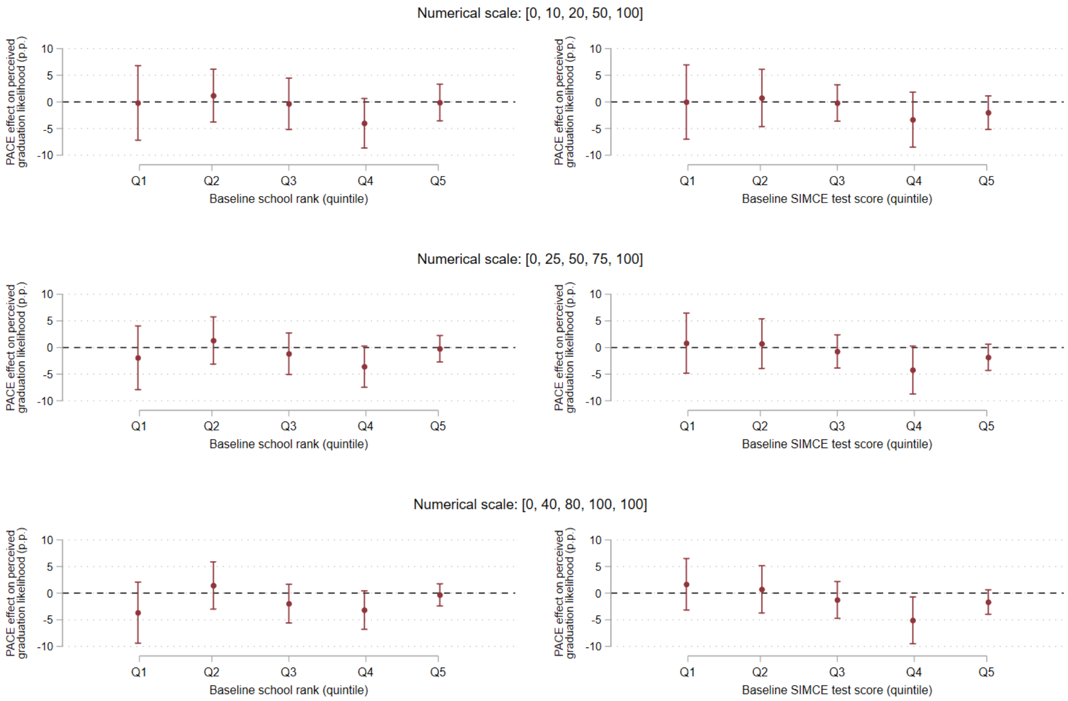


Figure B10: Heterogeneity of the effects of PACE on the perceived likelihood of graduating from selective college. Each dot is the coefficient on *Treatment* from an OLS regression where: *Treatment* is a dummy variable indicating whether a student is in a school that was randomly assigned to be in the PACE program, the controls are the standard set of controls, 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 panels) and quintiles in the distribution of 10^{th} grade standardized test scores (right panels). The units of measurements of the treatment effects are percentage points. The bars are 95% confidence intervals built using standard errors clustered at the school level. The survey responses were collected on a five-point Likert scale. Each row in the graph represents a different numerical assignment to assess the robustness of the results to variations in the numerical scale.

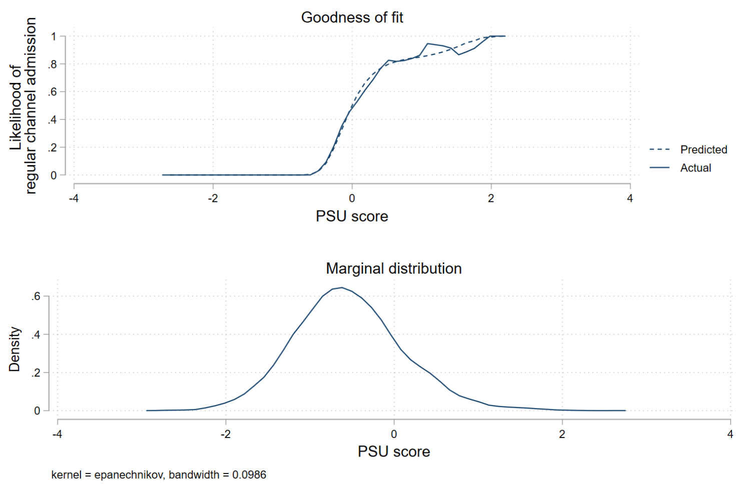


Figure B11: Goodness of fit of the regular admission likelihood function. This figure shows the fit of the function approximating the likelihood of obtaining a regular admission as a function of the PSU score, reported in equation (8). The top graph shows smoothed values from kernel-weighted local polynomial regressions. The bottom graph shows the marginal distribution of the PSU score among those in our study sample who took the PSU exam, which is the sample used to estimate the regular admission likelihood function. Table C20 provides the parameter estimates.

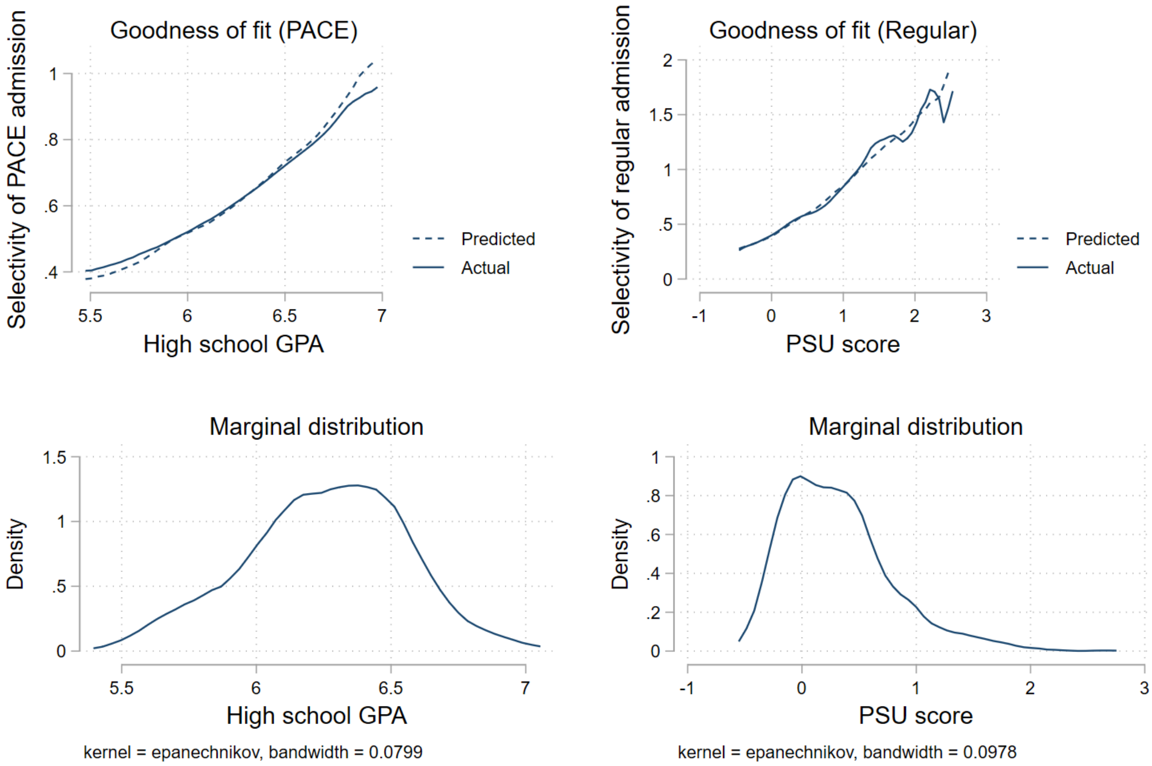


Figure B12: Goodness of fit of the admission selectivity functions. This figure shows the fit of the functions approximating the selectivity of the program to which an applicant is admitted through the PACE channel (left column) and the regular channel (right column), as a function of the relevant score. The functions are reported in equations (9) and (11). The relevant score is high school GPA for PACE admissions and PSU entrance exam score for regular admissions. Selectivity is measured as the average standardized PSU score of all regular entrants into the program defined as a selective college and major pair, in 2018. The top graphs show smoothed values from kernel-weighted local polynomial regressions. The bottom graphs show the marginal distribution of the scores in the populations with a PACE (left) or regular (right) admission, which are the samples used to estimate the selectivity functions. Table C21 provides the parameter estimates.

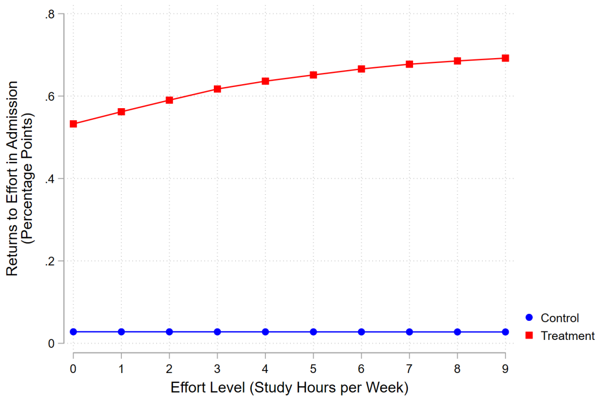


Figure B13: This Figure shows the simulated returns to effort in college admission probabilities at each effort level, comparing scenarios without and with the PACE treatment. The results are based on counterfactual simulations in which students have rational expectations in both cases.

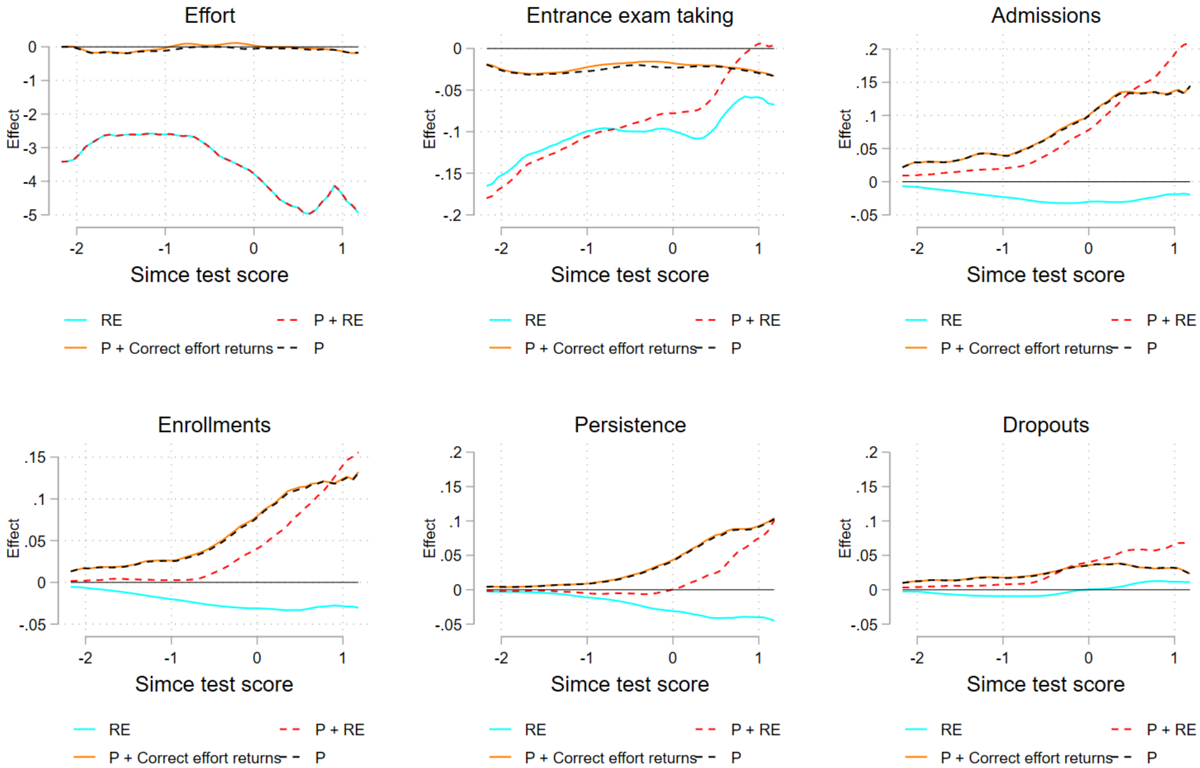


Figure B14: Simulated effects of various interventions, by baseline SIMCE test scores. These graphs are based on simulations from the control group sample. For each student, we simulate outcomes in the control condition, and under six counterfactual treatments. We calculate the treatment effect for each individual by taking the difference between the outcome in the treatment and in the control condition, and plot local polynomial graphs of treatment effects as a function of baseline Simce test scores (after trimming the top and bottom 1% of the score). The outcome "Effort" is measured in study hours per week, all other outcomes are rates. "Persistence" refers to the rate at which students in the sample enroll and persist; "Dropouts" refers to the rate at which students in the sample enroll and drop out. The six interventions are: (1) full information, debiasing all beliefs so students hold rational expectations, but without introducing any preferential admission policy (RE); (2) PACE and full information, debiasing all beliefs so students hold rational expectations and introducing PACE (P + RE); (3) PACE plus information debiasing beliefs about the probability of persisting in college only (P + DB pers); (4) PACE plus information debiasing beliefs about pre-college outcomes, only (P + DB pre-coll); (5) PACE plus information debiasing beliefs about the returns to effort in persistence, debiasing only the perceived-returns-to-effort (slope) component of the college-persistence likelihood while laving the perceived level biased (P + Value eff); (6) only PACE without any information intervention, i.e. the baseline scenario (P).

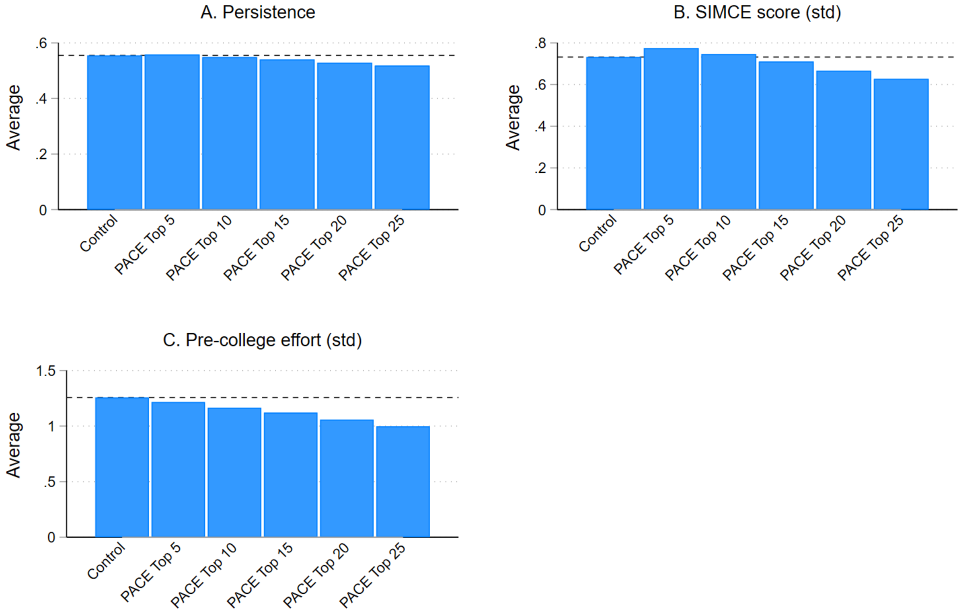


Figure B15: Selective college persistence and characteristics of selective college entrants. This figure shows average 5-year college-persistence rates, baseline test scores and pre-college effort for college entrants in the control group–where no intervention is introduced–and under five PACE designs characterized by different cutoffs for preferential admissions: top 5%, top 10%, top 15% (the baseline scenario), top 20%, top 25%. The control group mean is represented by the first bar and by the dashed horizontal line. SIMCE scores and pre-college effort are standardized to have mean zero and variance one in the control group.

C Additional Tables

Table C1: Geographic Location of Regular and PACE Seats

	Regular seats			PACE seats		
	Mean	St.dev.	N	Mean	St.dev.	N
	(1)	(2)	(3)	(4)	(5)	(6)
Within high school province	.6	.49	95294	.496	.5	2101
Within high school region	.797	.402	95294	.854	.353	2101

Note.– Geographic distribution of regular and PACE seats in selective colleges relative to the locations of applicants' high schools. Source: Administrative data for the 2018 centralized admission process.

Table C2: List of STEM majors

STEM majors
Biological Sciences
Physical Sciences
Natural Sciences, Mathematics and Statistics
Industry and Production
Engineering and Related Professions
Environmental Studies
Forestry, Agriculture, Fisheries
Health
Information and Communication Technology
Veterinary Medicine

Note.– Source: Miglino and Tincani (2026b). The major categorization corresponds to the subarea categorization established by UNESCO in the CINE-F classification defined in 2013 and being used by the OECD since 2016 (UNESCO, 2011). Data provided by the Chilean Ministry of Education (Ministry of Education, 2023). The distinction between STEM and Non-STEM majors follows the definition of STEM disciplines provided by the UCLA Higher Education Research Institute (2023).

Table C3: Baseline characteristics of all students and of those targeted by the PACE policy

	All students	Targeted students
	(1)	(2)
Very low SES	0.41	0.61
Mother's education (years)	11.44	9.60
Father's education (years)	11.38	9.38
Family income (1,000 CLP)	591.06	291.66
SIMCE score (standardized)	-0.00	-0.58
Rural resident	0.03	0.03
Santiago resident	0.39	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 (1): all students enrolled in Chilean high schools in 11^{th} grade. Sample restriction in column (2): students in the 128 experimental schools. All characteristics were collected before the start of the intervention.

Table C4: Effects of PACE on Selective College Applications and Admissions

	All sample		Bottom 85%		Top 15%	
	Applications	Admissions	Applications	Admissions	Applications	Admissions
	(1)	(2)	(3)	(4)	(5)	(6)
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)
RW-adj p-val	0.326	0.006	0.987	0.419	0.003	0.001
q-val	0.191	0.003	1.000	1.000	0.001	0.001
Control mean	0.210	0.114	0.161	0.070	0.450	0.328
R-squared	0.176	0.206	0.109	0.122	0.209	0.256
Observations	8944	8944	7061	7061	1563	1563

Note.– Columns (1) and (2) use the sample of all students in the experiment. Columns (3) and (4) use the sample of 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 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 slightly larger than 15% 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. 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. *RW-adj p-val* and *q-val* indicate Romano-Wolf adjusted p-values using 1000 bootstrap replications and q-values of the treatment effect, considering each sample as one family. *p < 0.10; **p < 0.05; ***p < 0.01.

Table C5: 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 selective college				
Treatment	0.030***	0.021**	0.018**	0.017**	0.015*
	(0.011)	(0.010)	(0.008)	(0.008)	(0.008)
RW-adj p-val	0.018	0.078	0.068	0.078	0.089
q-val	0.031	0.041	0.041	0.041	0.052
Control mean	0.085	0.068	0.057	0.053	0.050
Observations	8944	8944	8944	8944	8944
	B. Continuous enrollment in or graduation from vocational HE institute				
Treatment	-0.022	-0.018	-0.016	-0.015	-0.021**
	(0.018)	(0.015)	(0.012)	(0.011)	(0.010)
RW-adj p-val	0.348	0.348	0.348	0.348	0.110
q-val	0.250	0.250	0.250	0.250	0.250
Control mean	0.269	0.205	0.151	0.128	0.124
Observations	8944	8944	8944	8944	8944
	C. Continuous enrollment in or graduation from off-platform college				
Treatment	-0.014	-0.012	-0.009	-0.010	-0.011
	(0.012)	(0.009)	(0.007)	(0.007)	(0.007)
RW-adj p-val	0.299	0.294	0.299	0.231	0.212
q-val	0.338	0.338	0.338	0.338	0.338
Control mean	0.061	0.045	0.035	0.032	0.031
Observations	8944	8944	8944	8944	8944
	D. Continuous enrollment in or graduation from non-SUA HE institute				
Treatment	-0.036*	-0.030*	-0.025**	-0.025**	-0.032***
	(0.021)	(0.016)	(0.012)	(0.012)	(0.011)
RW-adj p-val	0.122	0.122	0.111	0.111	0.030
q-val	0.073	0.071	0.060	0.060	0.031
Control mean	0.329	0.251	0.186	0.160	0.155
Observations	8944	8944	8944	8944	8944
	E. Continuous enrollment in or graduation from any HE institute				
Treatment	-0.006	-0.009	-0.007	-0.008	-0.017
	(0.022)	(0.017)	(0.013)	(0.013)	(0.013)
RW-adj p-val	0.798	0.786	0.786	0.782	0.375
q-val	1.000	1.000	1.000	1.000	1.000
Control mean	0.414	0.318	0.243	0.213	0.205
Observations	8943	8944	8944	8944	8944

Note. – Sample of all students in the experiment. Results from OLS regressions. treat is a dummy equal to 1 if a school was randomly assigned to be in the treat treat, 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. Non-SUA HE refers to institutes that do not participate in the centralized admission system, that is, vocational HE institutes and off-platform colleges. The notes under Figure 2 explain how the outcome variables are constructed. *RW-adj p-val* and *q-val* indicate Romano-Wolf adjusted p-values using 1000 bootstrap replications and q-values of the treatment effect, considering the respective outcome variable in all years as one family. *p < 0.10; **p < 0.05; ***p < 0.01.

Table C6: 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 selective college				
Treatment	0.166***	0.126***	0.107***	0.095***	0.087***
	(0.029)	(0.028)	(0.026)	(0.026)	(0.025)
RW-adj p-val	0.001	0.002	0.002	0.003	0.004
q-val	0.001	0.001	0.001	0.001	0.001
Control mean	0.256	0.210	0.186	0.178	0.167
Observations	1563	1563	1563	1563	1563
	B. Continuous enrollment in or graduation from vocational HE institute				
Treatment	-0.043	-0.041	-0.029	-0.024	-0.026
	(0.030)	(0.026)	(0.024)	(0.021)	(0.021)
RW-adj p-val	0.297	0.272	0.329	0.329	0.329
q-val	0.345	0.345	0.345	0.345	0.345
Control mean	0.254	0.216	0.171	0.151	0.141
Observations	1563	1563	1563	1563	1563
	C. Continuous enrollment in or graduation from off-platform college				
Treatment	-0.058***	-0.048***	-0.037**	-0.039***	-0.040***
	(0.019)	(0.016)	(0.014)	(0.014)	(0.014)
RW-adj p-val	0.019	0.019	0.021	0.020	0.020
q-val	0.008	0.008	0.008	0.008	0.008
Control mean	0.106	0.084	0.068	0.065	0.064
Observations	1563	1563	1563	1563	1563
	D. Continuous enrollment in or graduation from non-SUA HE institute				
Treatment	-0.101***	-0.089***	-0.065**	-0.063***	-0.066***
	(0.032)	(0.027)	(0.026)	(0.023)	(0.023)
RW-adj p-val	0.013	0.009	0.017	0.016	0.015
q-val	0.006	0.006	0.007	0.006	0.006
Control mean	0.361	0.301	0.239	0.216	0.205
Observations	1563	1563	1563	1563	1563
	E. Continuous enrollment in or graduation from any HE institute				
Treatment	0.063**	0.037	0.041	0.032	0.021
	(0.030)	(0.028)	(0.026)	(0.027)	(0.027)
RW-adj p-val	0.109	0.308	0.221	0.308	0.443
q-val	0.243	0.318	0.316	0.318	0.429
Control mean	0.616	0.510	0.426	0.395	0.373
Observations	1562	1563	1563	1563	1563

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. treat is a dummy equal to 1 if a school was randomly assigned to be in the PACE treat, 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. Non-SUA HE refers to institutes that do not participate in the centralized admission system, that is, vocational HE institutes and off-platform colleges. The notes under Figure 2 explain how the outcome variables are constructed. *RW-adj p-val* and *q-val* indicate Romano-Wolf adjusted p-values

Table C7: Description of Choices and Outcomes in Control and Treated Schools

	Control			Treated		
	Mean	St.dev.	N	Mean	St.dev.	N
	(1)	(2)	(3)	(4)	(5)	(6)
A. All Students						
Weekly study hours	4.24	2.81	2843	3.99	2.76	3031
Took college entrance exam	.655	.475	4231	.636	.481	4775
College entrance exam score | took exam	-.602	.611	2773	-.484	.689	3037
Applied to selective college	.21	.407	4231	.259	.438	4775
Admitted to selective college	.114	.318	4231	.184	.388	4775
Enrolled in selective college	.0848	.279	4231	.139	.346	4775
Enrolled in selective college, STEM	.0404	.197	4231	.0685	.253	4775
Enrolled in selective college, non-STEM	.0444	.206	4231	.0704	.256	4775
Selectivity of program (college-major pair)	.544	.327	361	.62	.375	660
Distance in km from program (college-major pair)	135	233	356	92.9	156	657
Enrolled and persisted in selective college, year 5	.0499	.218	4231	.0787	.269	4775
Enrolled and persisted in selective college STEM, year 5	.0194	.138	4231	.0346	.183	4775
Enrolled and persisted in selective college non-STEM, year 5	.0262	.16	4231	.0373	.189	4775
Enrolled in vocational institution	.269	.443	4231	.239	.427	4775
Enrolled in off-platform college	.0605	.238	4231	.049	.216	4775
B. Students in Top 15% at baseline						
Weekly study hours	4.71	2.95	560	4.63	2.83	579
Took college entrance exam	.857	.35	735	.866	.341	828
College entrance exam score | took exam	-.245	.634	630	-.126	.749	717
Applied to selective college	.45	.498	735	.635	.482	828
Admitted to selective college	.328	.47	735	.594	.491	828
Enrolled in selective college	.256	.437	735	.46	.499	828
Enrolled in selective college, STEM	.139	.346	735	.252	.435	828
Enrolled in selective college, non-STEM	.117	.322	735	.208	.406	828
Selectivity of program (college-major pair)	.674	.336	188	.72	.403	369
Distance in km from program (college-major pair)	128	215	187	87.8	151	376
Enrolled and persisted in selective college, year 5	.167	.374	735	.283	.451	828
Enrolled and persisted in selective college STEM, year 5	.0762	.265	735	.14	.347	828
Enrolled and persisted in selective college non-STEM, year 5	.0789	.27	735	.116	.32	828
Enrolled in vocational institution	.254	.436	735	.192	.394	828
Enrolled in off-platform college	.106	.308	735	.0507	.22	828

Note. – Sample of students enrolled in control and treated schools. The college entrance exam score is designed to have mean 500 and standard deviation 110 among all exam takers, we report the standardized score. The selectivity of the program is the average entrance exam score among all regular entrants in the selective college and major the student enrolled in. As a measure of distance we use the length (km) of the shortest path between the coordinates of the program and of the high school the student attended, implementing Vincenty formula to calculate distances on a reference ellipsoid. A student is coded as persisting in the fifth year if he/she enrolled in the first year after high school and stayed continuously enrolled in selective college every year up until and including year 5, or if he/she enrolled in the first year after high school and graduated from a selective college in a year prior to year 5. If a student transfers to a different selective college program without taking a break in their studies, they are still considered continuously enrolled in a selective college.

Table C8: Average Treatment Effect on Pre-College Study Effort - Items

Panel A: At home	Study hours/week (std.)	Study days test	Assignm on time	
Treatment	-0.093**	-0.037	-0.105**	
	(0.041)	(0.041)	(0.042)	
R-W adjusted p	0.049	0.393	0.049	
q-val	0.039	0.139	0.039	
Panel B: In class	Take notes	Participate	Pay attention	Ask questions
Treatment	-0.121***	-0.054	-0.090**	-0.043
	(0.041)	(0.047)	(0.039)	(0.049)
R-W adjusted p	0.026	0.379	0.072	0.395
q-val	0.013	0.200	0.033	0.237
Panel C: PSU preparation	Prepare for PSU			
Treatment	-0.055***			
	(0.021)			

Note.– Dependent variabels in Panel A and B are standardized. The depenent variable in Panel C is binary. 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) and Inverse Probability Weights. Fieldworker fixed effects are excluded, as they absorb variation that the cluster-bootstrap procedure needs to compute reliable Romano-Wolf adjusted p-values. *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). Q-val indicate q-values of the treatment effect, calculated within family. 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 C9: Effect of PACE on College Entrance Exam

	Sat exam	Baseline ability exam takers	Exam score
	(1)	(2)	(3)
Treatment	-0.041	0.178*	0.004
	(0.028)	(0.093)	(0.024)
RW-adj p-val	0.299	0.208	0.868
q-val	0.207	0.207	0.402
Control mean	0.655	-0.605	-0.602
R-squared	0.112	0.110	0.430
Observations	8944	5779	5779

Note.– The coefficients are OLS estimates. Standard errors were clustered at the school level. All regressions use the standard set of controls (see notes under Figure 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 a dummy equal to 1 if the student took the college entrance exam, and 0 otherwise. The outcome variable in column (2) is the test score in 10^{th} grade (standardized in the population of 10^{th} graders). The outcome variable in column (3) is the standardized college entrance score. Columns (2) and (3) restrict the estimation sample to those who took the entrance exam. *RW-adj p-val* and *q-val* indicate Romano-Wolf adjusted p-values using 1000 bootstrap replications and q-values of the treatment effect, considering the outcomes in the table as one family. * p<0.10; ** p<0.05; *** p<0.01.

Table C10: Effects of PACE on Continuous Enrollment or Graduation Over Time in STEM and non-STEM majors, All Sample

	Year 1	Year 2	Year 3	Year 4	Year 5
A. Continuous enrollment in or graduation in STEM major in selective college					
Treatment	0.016**	0.011**	0.007	0.007	0.008*
	(0.007)	(0.006)	(0.005)	(0.005)	(0.005)
RW-adj p-val	0.057	0.068	0.180	0.180	0.123
q-val	0.121	0.121	0.121	0.121	0.121
Control mean	0.040	0.028	0.024	0.022	0.019
Observations	8944	8944	8944	8944	8944
B. Continuous enrollment in or graduation in non-STEM major in selective college					
Treatment	0.015**	0.008	0.009	0.008	0.005
	(0.007)	(0.007)	(0.006)	(0.006)	(0.005)
RW-adj p-val	0.107	0.340	0.198	0.214	0.353
q-val	0.258	0.313	0.258	0.258	0.313
Control mean	0.044	0.037	0.030	0.027	0.026
Observations	8944	8944	8944	8944	8944
p-value difference	0.908	0.632	0.828	0.888	0.640

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. The list of STEM majors is reported in Table C2. The notes under Figure 2 explain how the outcome variables are constructed. *RW-adj p-val* and *q-val* indicate Romano-Wolf adjusted p-values using 1000 bootstrap replications and q-values of the treatment effect, considering the respective outcome variable in all years as one family. *p-value difference* is the p-value of the difference of the STEM and non-STEM treatment effects. *p < 0.10; **p < 0.05; ***p < 0.01.

Table C11: Effects of PACE on Continuous Enrollment or Graduation Over Time in STEM and non-STEM majors, 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 in STEM major in selective college				
Treatment	0.089***	0.069***	0.051***	0.047**	0.046**
	(0.021)	(0.020)	(0.019)	(0.020)	(0.019)
RW-adj p-val	0.001	0.003	0.016	0.027	0.027
q-val	0.001	0.003	0.009	0.012	0.012
Control mean	0.139	0.097	0.086	0.082	0.076
Observations	1563	1563	1563	1563	1563
	B. Continuous enrollment in or graduation in non-STEM major in selective college				
Treatment	0.078***	0.048**	0.044**	0.035**	0.029*
	(0.021)	(0.019)	(0.017)	(0.017)	(0.016)
RW-adj p-val	0.003	0.037	0.036	0.068	0.095
q-val	0.001	0.021	0.021	0.026	0.035
Control mean	0.117	0.102	0.088	0.084	0.079
Observations	1563	1563	1563	1563	1563
p-value difference	0.725	0.464	0.773	0.644	0.508

Note. – Sample of all students in the experiment who were in the top 15% of their high school GPA ranking at baseline. 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. The list of STEM majors is reported in Table C2. The notes under Figure 2 explain how the outcome variables are constructed. *RW-adj p-val* and *q-val* indicate Romano-Wolf adjusted p-values using 1000 bootstrap replications and q-values of the treatment effect, considering the respective outcome variable in all years as one family. *p-value difference* is the p-value of the difference of the STEM and non-STEM treatment effects. *p < 0.10; **p < 0.05; ***p < 0.01.

Table C12: Comparison between selective college programs in which treated and control students enroll

	All students		Top 15%	
	Selectivity	Distance	Selectivity	Distance
	(1)	(2)	(3)	(4)
Treatment	0.053	-45.054	0.056	-44.354
	(0.032)	(41.123)	(0.040)	(45.332)
RW-adj p-val	0.332	0.410	0.443	0.455
q-val	0.257	0.257	0.475	0.475
Control mean	0.551	135.398	0.677	127.734
R-squared	0.192	0.021	0.206	0.021
Observations	971	1005	547	561

Note.– The coefficients are OLS estimates. Standard errors were clustered at the school level. All regressions use the standard set of controls (see notes under Figure 2). *Treatment* is a dummy variable indicating whether a student is in a school randomly assigned to be in the PACE program. The regressions are estimated on the sample of students from treated and control schools who enrolled in selective college. The outcome variables are the characteristics of the program they enrolled in the first year after high school. Panel A uses data from all students in the school, Panel B from those in the top 15% of their high school GPA ranking at baseline. As a measure of distance we use the length (km) of the shortest path between the coordinates of the program and the coordinates of the high school the student attended, implementing Vincenty formula to calculate distances on a reference ellipsoid. Selectivity is the average PSU score of all regular entrants in the program in 2018 (standardized). *RW-adj p-val* and *q-val* indicate Romano-Wolf adjusted p-values using 1000 bootstrap replications and q-values of the treatment effect, considering selectivity and distance in each sample as one family. * $p<0.10$; ** $p<0.05$; *** $p<0.01$

Table C13: Lee bounds for effects of PACE on selectivity and location of selective college program

	A. All students			
	Selectivity		Distance	
	Lower bound	Upper bound	Lower bound	Upper bound
Residuals	-0.173	0.258	-112.626	10.045
	(0.026)	(0.033)	(12.710)	(17.044)
Raw	-0.180	0.301	-110.574	7.122
	(0.028)	(0.037)	(12.694)	(17.323)
Total obs.	9006	9006	9006	9006
Selected obs.	974	974	1008	1008
	B. Top 15%			
	Selectivity		Distance	
	Lower bound	Upper bound	Lower bound	Upper bound
Residuals	-0.208	0.308	-110.175	17.010
	(0.036)	(0.043)	(15.915)	(21.985)
Raw	-0.260	0.332	-106.234	17.866
	(0.038)	(0.049)	(16.002)	(22.140)
Total obs.	1563	1563	1563	1563
Selected obs.	549	549	563	563

Note.– This table presents Lee (2009) bounds for the effects of PACE on the selectivity and location of the selective college programs in which students enroll. Numbers in parenthesis are the analytic standard errors provided by Lee (2009). As a measure of distance we use the length (km) of the shortest path between the coordinates of the program and the coordinates of the high school the student attended, implementing Vincenty formula to calculate distances on a reference ellipsoid. Selectivity is the average PSU score of all regular entrants in the program in 2018 (standardized). In the first and second rows we use residuals from a regression of the outcomes on the standard set of controls (see notes under Figure 2) as the dependent variables. In the third and fourth rows we use the raw outcome variables. *Total obs.* is the number of observations before the trimming procedure. *Selected obs.* is the number of observations after the trimming procedure and in the regression samples used for residualizing the outcomes.

Table C14: Change in Selection of College Entrants

	SIMCE	SIMCE
	(1)	(2)
Treatment	-0.047	-0.059
	(0.149)	(0.137)
Control mean	0.363	0.363
R-squared	0.001	0.097
Observations	569	569

Note.– This Table is based on the sample of students who, at the experiment's baseline, were in the top 15% of their school based on the GPA in grades 9 and 10. The sample is further restricted to college entrants. The coefficients are OLS estimates. Standard errors were clustered at the school level. The standard set of controls (see notes under Figure 2) is used in column (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 is the SIMCE test score in grade 10. * p<0.10; ** p<0.05; *** p<0.01

Table C15: Pre-college Outcomes and Persistence in Selective Colleges

	College persistence or graduation five years after high school graduation			
	(1)	(2)	(3)	(4)
GPA in 12^{th} grade (std)	0.142***			
	(0.019)			
GPA in 12^{th} grade tested subjects (std)		0.108***		
		(0.022)		
GPA in 12^{th} grade untested subjects (std)		0.057**		
		(0.026)		
PSU score (std)	0.047	0.072		
	(0.041)	(0.047)		
Study effort in last high school year (std)			0.075***	
			(0.023)	
Hours of study per week in last high school year				0.017***
				(0.006)
Baseline test score in 10^{th} grade (std)	0.001	-0.032	0.053**	0.052**
	(0.028)	(0.029)	(0.025)	(0.024)
Observations	1013	740	735	748
R^2	0.079	0.085	0.054	0.051

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). All regressions use the standard set of controls (see notes under Figure 2). The PSU score 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 C16: Belief elicitation

Belief over	**Question**	**Possible answers**
Expected score on the PSU entrance exam, $\overline{PSU}_i^b$.	Suppose that you will take the PSU entrance 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)
Expected own high school GPA, $\overline{GPA}_i^{(9-12),b}$.	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)[a]	Free format
Expected top 15% cutoff, i.e., 85^{th} percentile of the GPA distribution in the school, $\bar{c}_i^{15b}$.	Suppose that, in your school, there are 40 students in 12th grade. 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?[a,b]	Free format
Likelihood of graduating from selective college conditional on enrolling in one, $pgrad_i^b$.	If I enroll in a university (not a Technical Training Center or Professional Institute) thanks to a high PSU score, I will complete my studies.[c]	• Completely certain that I will not • More likely that I will not • Equally likely that I will and will not • More likely that I will • Completely certain that I will
Returns to effort in GPA and PSU productions, $\beta_{1i}^{Pb}, \beta_{2i}^{Pb}, e_{kink,i}^{Pb}, \beta_{1i}^{Gb}$.	How many hours per week do you think you need to study, between August and December 2017, to obtain a GPA/PSU score of at least X? [$X \in \{350, 450, 600\}$ for PSU, $X \in \{5.5$, answer to question on top 15% cutoff$\}$ for GPA].[d]	Free format

Note.– English translation of selected survey questions.

a. We used the Chilean term for GPA, *Notas de Enseñanza Media* (NEM), that refers to the grade point average across all four years of high school. Focus groups with students confirmed that this term is widely understood.

b. Focus groups with students showed that starting by asking about the student with the highest GPA, and then asking about the student with the 6th highest, improved question comprehension. Therefore, this is how we implemented the question.

c. Focus groups with students indicated that adding the wording 'thanks to a high PSU score' was necessary to ensure students understood the question was about selective colleges, which require obtaining a PSU score above an admission cutoff, and not non-selective colleges or vocational institutions. We are confident students interpreted this question as: "If I enroll in a selective college, I will graduate."

d. Although the GPA question referred to the study hours required in the final semester to attain a given average high school GPA, we do not assume that respondents first calculated the additional final-semester GPA needed to attain each four-year GPA target—a cognitively demanding calculation requiring students to account for their accumulated grades and the weight of the remaining semester. We instead treat the stated GPA values as achievement targets and use the difference in reported hours to measure perceived mapping from study effort to GPA achievement.

Table C17: 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	4570	3769

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 C18: Effect of PACE on perceived graduation likelihood

	(1)	(2)	(3)
Treatment	-0.016	-0.016	-0.011
	(0.014)	(0.014)	(0.011)
Outcome mean in the control group	0.776	0.776	0.776
Observations	5809	5809	5770
Controls	No	No	Yes
Fieldworker fixed effects	No	No	Yes
Inverse Probability Weights	No	Yes	Yes

Note.– The coefficients are OLS estimates. Controls are the standard set. Standard errors are clustered at the school level. The outcome variable is the student's perceived chance of graduating from selective college if they enroll. Perceived chances were elicited on a 5-point Likert scale and were assigned values of 0, 0.25, 0.50, 0.75 and 1 to construct this table. *** p<0.01, ** p<0.05, * p<0.10.

Table C19: Perceived Marginal Returns to Effort

Perceived marginal return to effort in:	Mean	Min	Max	N
	(1)	(2)	(3)	(4)
GPA, all survey answers	.103	-4.1	4.5	3442
GPA, excluding negative values	.319	0	4.5	2446
GPA, imputed when survey answer missing	.33	0	4.5	8949
PSU below kink, all survey answers	.345	-.909	.909	4018
PSU below kink, excluding negative values	.42	.057	.909	3716
PSU below kink, imputed when survey answer missing	.423	.057	.909	8963
PSU above kink, all survey answers	.457	-1.36	1.36	4168
PSU above kink, excluding negative values	.577	.085	1.36	3820
PSU above kink, imputed when survey answer missing	.587	.085	1.36	8962

Note. – This table presents descriptive statistics for the perceived returns to effort, constructed using the transformations in equations (15) and (16). Variables labeled as including all survey answers apply these transformations directly to the raw survey responses. The number of observations for GPA is lower because we exclude cases where the perceived top 15% cutoff equals the hypothetical value of 5.5—this would lead to division by zero in equation (16). Variables labeled as excluding negative values further omit observations where the calculated returns are negative. Imputations are performed only after removing survey responses that yield negative returns. Details on the imputation process for missing values are provided in Appendix G.3. In model estimation, we use perceived returns with imputed values where survey responses are missing.

Table C20: Parameters estimated outside of the model, regular admission likelihood

	Likelihood of Regular Admission
	(1)
PSU	2.659***
	(0.110)
PSU × PSU	-2.602***
	(0.214)
PSU × PSU × PSU	0.966***
	(0.177)
Constant	0.024
	(0.045)
Pseudo R-squared	0.543
Observations	5810

Note.– The Table reports estimates from a Probit regression model. Standard errors were clustered at the school level. The estimation sample includes all entrance-exam takers in our study sample. * p<0.10; ** p<0.05; *** p<0.01

Table C21: Parameters estimated outside of the model, program selectivity

	Selectivity PACE (1)	Selectivity Regular (2)
GPA grades 9-12	-2.288	
	(1.951)	
GPA grades 9-12 × GPA grades 9-12	0.218	
	(0.158)	
Simce	0.049	0.058***
	(0.032)	(0.014)
Simce × Simce	0.025	-0.012
	(0.023)	(0.010)
Academic × Simce	0.063	0.010
	(0.059)	(0.024)
Academic	0.034	0.032
	(0.042)	(0.025)
Region=4	0.178	0.030
	(0.140)	(0.090)
Region=5	0.302***	-0.022
	(0.108)	(0.081)
Region=7	0.231**	-0.034
	(0.104)	(0.068)
Region=8	0.221*	0.048
	(0.111)	(0.068)
Region=10	0.213*	-0.018
	(0.109)	(0.074)
Region=13	0.473***	0.047
	(0.107)	(0.072)
Region=14	0.173	-0.009
	(0.136)	(0.072)
Region=15	-0.141	-0.076
	(0.114)	(0.070)
PSU		0.325***
		(0.033)
PSU × PSU		0.084**
		(0.035)
Constant	6.081	0.380***
	(6.037)	(0.066)
R-squared	0.296	0.465
Observations	400	1063

Note.– The Table reports OLS estimates. Standard errors were clustered at the school level. Selectivity is measured as the average PSU score among all regular entrants into the degree program, defined as a selective college and major pair. The reference categories are the vocational track and the third region. The region refers to the location of the high school. The ninth and nearby tenth regions are lumped together, since only 1.32% of the sample went to school in the ninth region, and none of these students was admitted to college through PACE. The samples are: all those admitted through the PACE channel in column (1), all those admitted through the regular channel in column (2). * p<0.10; ** p<0.05; *** p<0.01

Table C22: Parameter Estimates

Symbol	Description	Estimate	Standard Error
	A. Preferences		
ξ_{11}	Linear term in utility from study hours, type 1	-0.006^{***}	(0.000)
ξ_{12}	Linear term in utility from study hours, type 2	-0.306	(0.189)
ξ_2	Quadratic term in utility from study hours	-0.006^{***}	(0.001)
c_0^S	Cost of taking the entrance exam	0.068^{***}	(0.006)
c_1^S	Treatment impact on the perceived value of taking the entrance exam	-0.112	(0.168)
λ_{01}	Constant in utility from enrolling and dropping out from college, type 1	0.292	(0.372)
λ_{02}	Constant in utility from enrolling and dropping out from college, type 2	0.766	(0.682)
λ_0^G	Constant in utility from graduating from college	0.228	(0.492)
δ	Additional utility from PACE enrollment	-0.897^{**}	(0.394)
	B. Technology		
β_{01}^G	Constant in GPA production, type 1	0.721^{*}	(0.411)
β_{02}^G	Constant in GPA production, type 2	1.151^{***}	(0.267)
β_1^G	Coefficient on study hours in GPA production	0.045	(0.044)
β_2^G	Coefficient on baseline GPA in GPA production	0.851^{***}	(0.048)
β_3^G	Coefficient on Simce in GPA production	0.119^{**}	(0.049)
β_{01}^P	Constant in PSU production, type 1	-0.122	(1.817)
β_{02}^P	Constant in PSU production, type 2	-1.248	(1.470)
β_1^P	Coefficient on study hours in PSU production	0.001	(0.027)
β_2^P	Coefficient on baseline GPA in PSU production	0.052	(0.277)
β_3^P	Coefficient on Simce in PSU production	0.248^{**}	(0.114)
ρ_{01}	Constant in persistence likelihood index, type 1	-0.015	(0.012)
ρ_{02}	Constant in persistence likelihood index, type 2	-0.109	(0.573)
ρ_1	Coefficient on study hours in persistence likelihood index	0.030	(0.025)
ρ_2	Coefficient on Simce in persistence likelihood index	0.587	(0.565)
	C. Subjective beliefs		
γ_0^b	Constant in index for subjective probability of regular admission	-0.412^{*}	(0.228)
γ_1^b	Coefficient on PSU in index for subjective probability of regular admission	9.813^{**}	(4.883)
π_0^b	Constant in index for subj. probability PACE admission	-4.169^{***}	(1.139)
π_1^b	Coefficient on distance from cutoff in index for subj. prob. PACE admission	0.072	(10.303)
	D. Unobserved heterogeneity and shocks		
ω_{20}	Constant in type probability index	1.514	(1.193)
ω_{21}	Coefficient on whether missing from survey in type probability index	3.157	(2.870)
ω_{22}	Coefficient on female in type probability index	-0.347^{***}	(0.100)
ω_{23}	Coefficient on baseline top 15% status in type probability index	-3.901	(2.861)
σ_{mee}	Standard deviation of measurement error on effort	0.132^{***}	(0.007)
σ_{GPA}	Standard deviation of GPA shock	0.344^{***}	(0.084)
σ_{PSU}	Standard deviation of PSU shock	0.046	(0.111)

Note. – Standard Errors in parenthesis, cluster-bootstrapped using 50 bootstrap samples. $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$. Estimation imposes parameter bounds centered at calibrated starting values, with selected parameters subject to positivity constraints. The replication package initializes at the estimated parameter vector and implements a local search with slightly relaxed convergence tolerances to reduce computation time, retaining 20 shock draws. Bootstrap estimations use the same constraints but reduce the number of shock draws from 20 to 2 for computational tractability. Full details are provided in the replication package.

Table C23: Model Fit - Description of Choices and Outcomes

	Data		Simulations	
	Mean	St.dev.	Mean	St.dev.
	(1)	(2)	(3)	(4)
A. Control				
Study hours/week	4.25	2.81	4.28	4.67
GPA grade 12	5.7	.559	5.7	.562
GPA grades 9-12	5.24	.429	5.53	.484
In top 15, GPA grades 9-12	.167	.373	.156	.363
Took college entrance exam	.661	.473	.641	.48
Admitted to selective college	.116	.321	.125	.331
Enrolled in selective college	.0866	.281	.0869	.282
Selectivity of program (college-major pair)	.544	.326	.453	.279
Enrolled and persisted in selective college, year 5	.0508	.22	.0475	.213
B. Treatment				
Study hours/week	3.99	2.74	4.14	4.6
GPA grade 12	5.67	.573	5.72	.575
GPA grades 9-12	5.23	.429	5.55	.495
In top 15, GPA grades 9-12	.169	.375	.155	.362
Took college entrance exam	.647	.478	.616	.486
Admitted to selective college	.19	.393	.188	.391
Admitted to selective college via PACE	.119	.324	.122	.328
Enrolled in selective college	.144	.351	.136	.343
Selectivity of program (college-major pair)	.622	.374	.624	.39
Enrolled and persisted in selective college, year 5	.0823	.275	.0786	.269
Enrolled pace if admitted both	.421	.494	.34	.474

Note. – Sample of students enrolled in control schools. Simulated test scores, hours of study and GPA in grade 12 are summarized in the sample for which the corresponding variable is nonmissing in the data. The selectivity of the program is the average entrance exam score among all regular entrants in the selective college and major the student enrolled in. A student is coded as persisting in the fifth year if he/she enrolled in the first year after high school and stayed continuously enrolled in selective college every year up until and including year 5, or if he/she enrolled in the first year after high school and graduated from a selective college in a year prior to year 5. If a student transfers to a different selective college program without taking a break in their studies, they are still considered continuously enrolled in a selective college.

Table C24: Fit of auxiliary models for TE on admissions, enrollments, persistence.

	Admissions		Enrollments		Persistence	
	Data	Simulations	Data	Simulations	Data	Simulations
	(1)	(2)	(3)	(4)	(5)	(6)
A. All students						
Treatment	0.052	0.055	0.040	0.042	0.018	0.023
	(0.010)		(0.010)		(0.008)	
Control mean	0.116	0.125	0.087	0.087	0.057	0.048
B. Top 15 percent at baseline						
Treatment	0.238	0.235	0.179	0.196	0.094	0.114
	(0.027)		(0.026)		(0.023)	
Control mean	0.328	0.435	0.256	0.302	0.182	0.179

Note.– This table shows treatment effects and control means that we aim to match in the model estimation. The coefficients are OLS estimates; standard errors clustered at the school level are reported in parentheses for the data columns. All regressions include all model initial conditions except region and survey missing. *Treatment* is a dummy variable indicating whether a student is in a school randomly assigned to be in the PACE program. The outcome variable in columns (1)-(2) and is an indicator for being admitted to a selective college via regular or preferential admissions. The outcome variable in columns (3)-(4) and is an indicator for being enrolled in a selective college one year after high school. The outcome variable in columns (5)-(6) and is an indicator for being enrolled in a selective college five years after high school. Regressions in panel A are estimated on the entire sample of students in experimental schools. Regressions in panel B are estimated on the sample of students who at the end of 10^{th} grade were in the top 15% of their school according to GPA in the first two high school years.

D Robustness Analysis

D.1 Experimental Analysis

D.1.1 Lack of strategic high school enrollments

The GPA and achievement reductions are unlikely to be 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 school for the last two high school years to benefit from the percent rule, they did not have an incentive to change schools at a later time either. Second, the student characteristics are balanced across treatment groups (Table 1), indicating a 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 found no systematic relation between baseline test scores and entering or leaving a PACE school (Table D1). Finally, strategic high school enrollment should induce 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.

Table D1: Analysis of school transitions

	In-flow into treated schools	Out-flow from treated schools
SIMCE score in 10^{th} grade (std)	0.006	-0.007
	(0.012)	(0.012)
Constant	0.088***	0.115***
	(0.017)	(0.017)
Observations	3925	4073

Note.– Probability to transition into or out of a school which was randomly assigned to be treated in 2016, in the experimental cohort under study. Coefficients are OLS estimates. Standard errors (clustered at school level) are displayed in parentheses. In column (1) the sample consists of all students who were enrolled in a treated school in 2016, the dependent variable is a dummy equal to one if, in 2015, the student was not enrolled in a school that was randomized to be treated in 2016. In column (2) the sample consists of all students who, in 2015, were enrolled in a school which was randomized to be treated in 2016. The dependent variable is a dummy equal to one if the student was not enrolled in a treated school in 2016. Both samples exclude students who in 2015 or in 2016 were enrolled in schools which participated in the PACE program but not as part of the randomized experiment. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

D.1.2 Survey attrition

The response rate in our survey data is 69.4% percent in the control group, and it is not statistically significantly different in the treatment group, suggesting the absence of selective attrition. This holds for the full sample and also for the Top 15% sample, see Table D2. Table D3 presents Lee (2009) bounds for the treatment effects, confirming that the estimated treatment effects are not due to selective attrition.

Table D2: Participation in the survey

	Participated in the survey	
	Full sample	Top 15% Sample
	(1)	(2)
Treatment	-0.033	-0.070
	(0.034)	(0.044)
Observations	9006	1563
R-squared	0.001	0.006

Note. – Column (1) uses the sample of all students in the experiment. Column (2) uses 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 not exactly 15% because there are students with the same GPA average at baseline and missings in the dependent variable. 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. Standard errors clustered at the school level in parenthesis. *p < 0.10; **p < 0.05; ***p < 0.01.

Table D3: Lee bounds for effects of PACE on achievement and effort

	Standardized achievement score		Standardized study effort	
	Lower bound	Upper bound	Lower bound	Upper bound
Residuals	-0.209	-0.024	-0.285	-0.012
	(0.032)	(0.033)	(0.036)	(0.036)
Raw	-0.163	-0.013	-0.268	0.005
	(0.037)	(0.039)	(0.037)	(0.037)
Total obs.	8944	8944	8944	8944
Selected obs.	6054	6054	5631	5631

Note.– This table presents Lee (2009) bounds for the effects of PACE on pre-college achievement and effort. Numbers in parenthesis are the analytic standard errors provided by Lee (2009). 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. *Total obs.* is the number of observations before the trimming procedure. *Selected obs.* is the number of observations after the trimming procedure and in the regression samples used for residualizing the outcomes.

D.1.3 Validity of the survey-based findings

We collected standardized achievement scores and measures of effort because this information is not available in the administrative data. GPA is available for all students but is not an achievement measure comparable across schools as it is graded within schools. The standardized PSU score is graded centrally, but it is available only for those who took the entrance exam, a selected sample. Our achievement measure does not suffer from this self-selection, and it correlates strongly with the PSU score (0.490), including with its Language component (0.437).[3] Several factors point to the validity of the survey-based outcomes. First, 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 D4 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 3.1 p.p. (p=0.000), or 50% of the sample mean. The study effort measure has equally strong predictive validity. Additionally, the results are robust to using item response theory to calculate the achievement score (Table D5).

[3] The correlation between the Mathematics and the Language components of the PSU exam is 0.410.

Table D4: Validating Achievement and Effort Measures

	Sit PSU (1)	Apply (2)	Admitted (3)	Enroll year 1 (4)	Enroll year 2 (5)	Enroll year 3 (6)	Enroll year 4 (7)	Enroll year 5 (8)
				A. Achievement				
Achievement	0.060***	0.074***	0.058***	0.047***	0.042***	0.036***	0.033***	0.031***
	(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
Control mean	0.725	0.241	0.133	0.099	0.082	0.071	0.066	0.062
Pseudo-R^2	0.099	0.169	0.280	0.290	0.275	0.264	0.253	0.247
Observations	2922	2922	2922	2922	2922	2922	2922	2922
				B. Achievement, controlling for PSU score				
Achievement		0.037***	0.016***	0.015**	0.017***	0.012*	0.010	0.010
		(0.012)	(0.006)	(0.006)	(0.006)	(0.007)	(0.006)	(0.007)
PSU score		Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean		0.333	0.183	0.136	0.113	0.098	0.091	0.085
Pseudo-R^2		0.238	0.556	0.504	0.449	0.417	0.409	0.397
Observations	.	2122	2122	2122	2122	2122	2122	2122
				C. Study effort				
Study effort index (std.)	0.056***	0.069***	0.045***	0.037***	0.032***	0.030***	0.030***	0.029***
	(0.010)	(0.009)	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)	(0.006)
PSU score	No	No	No	No	No	No	No	No
Control mean	0.731	0.244	0.136	0.101	0.084	0.072	0.067	0.064
Pseudo-R^2	0.096	0.163	0.255	0.262	0.243	0.240	0.235	0.232
Observations	2746	2746	2746	2746	2746	2746	2746	2746
				D. Study effort, controlling for PSU score				
Study effort index (std.)		0.055***	0.018***	0.017**	0.017**	0.017**	0.019***	0.018***
		(0.010)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
PSU score		Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean		0.334	0.186	0.138	0.115	0.099	0.092	0.087
Pseudo-R^2		0.241	0.550	0.500	0.446	0.416	0.412	0.403
Observations	.	2010	2010	2010	2010	2010	2010	2010

Note.– The Panels differ in the measure of achievement or of effort used as an explanatory variable and in whether the PSU score is used as a control, both highlighted in the title of each Panel. 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 C8. *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

Table D5: AVERAGE TREATMENT EFFECT ON PRE-COLLEGE ACHIEVEMENT SCORE USING IRT

	STANDARDIZED ACHIEVEMENT SCORE (IRT)	
Treatment	-0.084**	-0.081**
	(0.040)	(0.040)
Inverse probability weights	NO	YES
Observations	6054	6054
R^2	0.254	0.254

NOTE.– Coefficients are OLS estimates. Standard errors are clustered at the school level. Standard set of controls and with fieldworker fixed effects. Treatment is a dummy variable indicating whether a student is in a school that was randomly assigned to be in the PACE program. Scores are scaled using Item Response Theory models, and standardized to have mean zero and variance one. * p<0.10, ** p<0.05, *** p<0.01.

D.1.4 Robustness of the results on heterogeneity by beliefs (Table 6)

The subjective expectations were not elicited at the experiment's baseline. This raises the concern that these variables might have been influenced by the treatment. We address this issue as follows:

- Column (1) of Table D6 shows that the treatment had no statistically significant impact on the perceived GPA distance from the cutoff we use to build Table 6, for neither of the two samples used in the analysis. This measure relies on data on the perceived top 15% cutoff from a separate survey administered closer to the experiment's baseline, implemented in collaboration with the Ministry of Education (Ministerio de Educacion, Chile, n.d.d).[4]

- Column (2) of Table D6 shows that the treatment had only a small positive effect on the likelihood that a student expects a PSU score smaller than or equal to the median perceived PSU, which we use to restrict the sample in Panel B of Table 6. This effect is not significant once we control for multiple hypotheses testing, as evidenced by the Romano-Wold adjusted p-value and the q-value, reported in the Table. Moreover, Table D7 shows that the sub-sample used for the analysis in Panel B of Table 6 remains well balanced across the treatment and control groups in terms of observed baseline characteristics. The perceived PSU score is obtained from the survey question reported in the first row of Table C16.

[4] This survey was administered in April of the 12^{th} grade. Our research team designed the questions eliciting subjective expectations, while the Ministry administered the survey in schools. The English translation of the question we use is: "Think about the top 15% of students in your school's 12^{th} grade, those with the best GPA. What is the lowest GPA a student in your school would need to achieve to be in the top 15% of students with the best GPA? (ENTER A NUMBER FROM 1.0 to 7.0)". When the answer to this April survey question is missing, we use data from our main survey conducted in August and reported in the third row of Table C16.

This evidence gives us confidence that the results reported in Table 6 are unlikely to be driven by imbalanced unobservables.

Table D6: TREATMENT EFFECTS ON SUBJECTIVE EXPECTATIONS MEASURED IN 12^{th} GRADE

	Perceived distance from cutoff	Perceived PSU $\leq$ median
	A. All students	
	(1)	(2)
Treatment	0.021	0.026**
	(0.022)	(0.013)
RW-adj p-val	0.339	0.112
q-val	0.208	0.109
Control mean	0.601	0.871
R-squared	0.016	0.053
Observations	5055	4895
	B. Students with perceived GPA > perceived cutoff, perceived PSU $\leq$ median	
	(1)	
Treatment	-0.010	
	(0.037)	
Control mean	0.686	
R-squared	0.034	
Observations	1281	

NOTE.– The coefficients are OLS estimates. Standard errors were clustered at the school level. All regressions use the standard set of controls (see notes under Figure 2). *Treatment* is a dummy variable indicating whether a student is in a school randomly assigned to be in the PACE program. Panel A is based on the sample of all survey respondents. Panel B is based on the sample of sample respondents who perceive themselves to have a higher GPA than the 85^{th} percentile in the school and a PSU score lower than or equal to the median perceived PSU. *Perceived distance from cutoff* is the absolute value of the difference between a student's perceived own GPA and the perceived GPA of the 85^{th} percentile in their school. *Perceived PSU $\leq$ median* is a dummy variable equal to 1 if the student expected a PSU score lower than or equal to the median interval (150-600) and 0 otherwise (600-850). *RW-adj p-val* and *q-val* indicate Romano-Wolf adjusted p-values using 1000 bootstrap replications and q-values of the treatment effect , considering both variables as one family. * p<0.10; ** p<0.05; *** p<0.01

Table D7: Sample Balance Across Treatment and Control Groups, Students with Perceived GPA above the Perceived Cutoff and Perceived PSU Smaller than or Equal to the Median

	Control (1)	Difference between Treatment and Control (2)	p-value (difference equals zero) (3)	N (4)
Female	.511	-.021	.732	1537
	.	.063	.	.
Age (years)	17.396	.074	.153	1537
	.	.052	.	.
Very-low-SES student	.609	-.014	.619	1537
	.	.029	.	.
Mother's education (years)	9.632	-.208	.308	1106
	.	.203	.	.
Father's education (years)	9.389	.046	.853	1060
	.	.244	.	.
Family income (1,000 CLP)	276.357	14.228	.35	1118
	.	15.157	.	.
SIMCE score (points)	227.825	3.025	.52	1523
	.	4.686	.	.
Never failed a year	.984	-.003	.717	1523
	.	.007	.	.
Santiago resident	.142	.073	.359	1537
	.	.08	.	.
Academic high school track	.211	.06	.407	1537
	.	.072	.	.

Note.– Standard errors clustered at the school level are shown in even rows. 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. The sample is restricted to students who believe to rank in the top 15% and expect a PSU score equal to or lower than the median of the belief distribution (150-600).

D.1.5 Predictive validity of the belief measures.

We examine the predictive validity of the belief measures in Table D8, leveraging unique data linkages between elicited beliefs, their realizations and students' related choices.

Table D8: Validating Belief Measures

	A. Validity of PSU belief	
	PSU score	Sit PSU
Perceived PSU score	0.100***	0.050***
	(0.012)	(0.012)
Perceived PSU score × Treatment	0.013	0.018
	(0.023)	(0.014)
Sample mean	-0.502	0.746
Observations	3666	4895
R^2	0.450	0.124
P-val: Var + Var × Treat	0.000	0.000
	B. Validity of GPA belief	
	GPA minus cutoff	Sit PSU
Perceived GPA minus cutoff	0.092***	-0.001
	(0.012)	(0.011)
Perceived GPA minus cutoff × Treatment	0.010	0.025*
	(0.018)	(0.015)
Sample mean	-0.749	0.733
Observations	5055	5055
R^2	0.207	0.114
P-val: Var + Var × Treat	0.000	0.013

Note.– The outcome variable is indicated at the top of the column. Panels A studies the explanatory role of perceived PSU, Panels B studies the explanatory role of the perceived distance in terms of GPA points from the within-school cutoff. The perceived PSU score is standardized using the distribution of PSU scores among all exam-takers in the country. Perceive GPA minus cutoff is the difference between the perceived own GPA and the perceived top 15% cutoff. Within each panel, the belief variable is included uninteracted and interacted with treatment, to examine differences across treatment groups in the relationship between beliefs and outcomes. All regressions include as regressors the treatment dummy, and the standard set of controls (see notes under Figure 2) uninteracted and interacted with the treatment dummy. All regressions use Inverse Probability Weights. The last row of each panel reports the p-value for the effect of the belief variable on the outcome in the treatment group, obtained as the sum of the effect of the belief variable uninteracted and interacted with the treatment dummy. *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

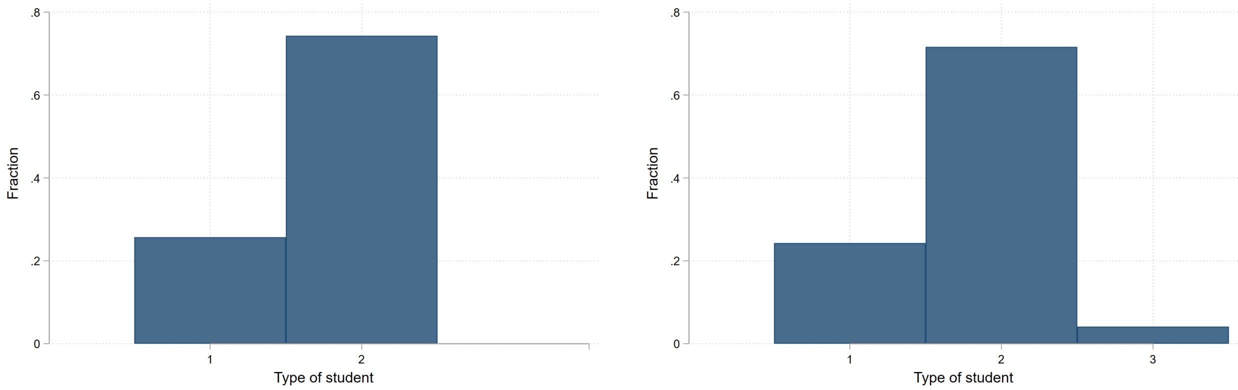
First, the belief measures reflect actual outcomes: perceived GPA distance from cutoff and perceived PSU are positively and significantly correlated with their respective realizations (column (1) of Panel A and B). Second, perceived PSU predicts the decision to take the entrance exam of both treated and control students, consistent with their incentive to obtain a regular admission (column (2) of Panel A). Third, perceived GPA distance from cutoff predicts the decision to take the entrance exam of treated students only, consistent with their incentive to obtain a preferential admission (column (2) of Panel B).

The decision to sit the entrance exam is taken before observing the realization of the GPA distance from cutoff and the realization of the PSU score. It is associated with both measures of beliefs in the way we would expect, suggesting these measures contain meaningful information on students' beliefs.

D.2 Structural Model Analysis: Estimation with Three Types

We re-estimated the model by introducing a third student type to assess whether accounting for additional time-invariant unobserved heterogeneity substantially alters the simulated outcomes. The results suggest this is not the case. As shown in Figure D1, the distribution of student types remains largely unchanged when moving from a two-type to a three-type specification. In the latter model, only 4 percent of students are assigned to the third type.

The model continues to have a good fit in terms of students' choices and outcomes (Table D9), as well as for most main treatment effects (Tables D10 and D11).



(a) Two types

(b) Three types

Figure D1: This figure shows the fraction of students of type 1 and 2 estimated in a model with two types (Panel A) and the fraction of students of type 1, 2 and 3 estimated in a model with three types (Panel B).

Table D9: DESCRIPTION OF CHOICES AND OUTCOMES. THREE TYPES.

	Data		Simulations	
	Mean	St.dev.	Mean	St.dev.
	(1)	(2)	(3)	(4)
A. CONTROL				
Study hours/week	4.25	2.81	4.6	4.72
GPA grade 12	5.7	.559	5.69	.59
GPA grades 9-12	5.24	.429	5.53	.448
In top 15, GPA grades 9-12	.167	.373	.156	.363
Took college entrance exam	.661	.473	.641	.48
Admitted to selective college	.116	.321	.115	.319
Enrolled in selective college	.0866	.281	.0932	.291
Selectivity of program (college-major pair)	.544	.326	.411	.268
Enrolled and persisted in selective college, year 5	.0508	.22	.0535	.225
B. TREATMENT				
Study hours/week	3.99	2.74	4.38	4.7
GPA grade 12	5.67	.573	5.7	.584
GPA grades 9-12	5.23	.429	5.54	.448
In top 15, GPA grades 9-12	.169	.375	.155	.362
Took college entrance exam	.647	.478	.601	.49
Admitted to selective college	.19	.393	.185	.388
Admitted to selective college via PACE	.119	.324	.122	.327
Enrolled in selective college	.144	.351	.141	.348
Selectivity of program (college-major pair)	.622	.374	.585	.383
Enrolled and persisted in selective college, year 5	.0823	.275	.0832	.276
Enrolled pace if admitted both	.421	.494	.366	.482

NOTE. – Sample of students enrolled in control schools. Simulated test scores, hours of study and GPA in grade 12 are summarized in the sample for which the corresponding variable is nonmissing in the data. The selectivity of the program is the average entrance exam score among all regular entrants in the selective college and major the student enrolled in. A student is coded as persisting in the fifth year if he/she enrolled in the first year after high school and stayed continuously enrolled in selective college every year up until and including year 5, or if he/she enrolled in the first year after high school and graduated from a selective college in a year prior to year 5. If a student transfers to a different selective college program without taking a break in their studies, they are still considered continuously enrolled in a selective college.

Table D10: Effect of PACE on Pre-College Outcomes. Three types.

	Study hours/week		Study hours/week		12^{th} grade GPA		Take PSU	
	Data	Simulations	Data	Simulations	Data	Simulations	Data	Simulations
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	-0.193	-0.176	-0.078	-0.069	-0.056	0.010	-0.028	-0.037
	(0.114)		(0.142)		(0.048)		(0.028)	
Treatment × Perceived distance			-0.263	-0.199				
			(0.114)					
Control mean	4.254	4.601	4.180	4.532	5.752	5.707	0.661	0.641

Note.– The coefficients are OLS estimates; standard errors clustered at the school level are reported in parentheses for the data columns. All regressions include all model initial conditions except region and survey missing. Field-worker fixed effects were used for columns (1)-(4). Inverse Probability Weights were used for columns (1)-(6). *Treatment* is a dummy variable indicating whether a student is in a school randomly assigned to be in the PACE program. *Perceived distance* is the absolute value of the difference between perceived own GPA and the perceived 85^{th} percentile of the GPA distribution in the school. The outcome variable in columns (1)-(4) is the number of hours of study per week. In columns (3) and (4) we add the interaction of *Perceived distance* with *Treatment* and with all the initial conditions and fieldworker fixed effects. The outcome variable in columns (5) and (6) are the GPA in grade 12, measured in GPA points (ranging from 1 to 7). The outcome variable in columns (7) and (8) is an indicator for sitting the college entrance exam. All regressions are estimated on the sample of students for whom the outcome variable is non-missing in the data.

Table D11: Fit of auxiliary models for TE on admissions, enrollments, persistence. Three types.

	Admissions		Enrollments		Persistence	
	Data	Simulations	Data	Simulations	Data	Simulations
	(1)	(2)	(3)	(4)	(5)	(6)
			A. All students			
Treatment	0.052	0.057	0.040	0.038	0.018	0.020
	(0.010)		(0.010)		(0.008)	
Control mean	0.116	0.115	0.087	0.093	0.057	0.053
			B. Top 15 percent at baseline			
Treatment	0.238	0.267	0.179	0.189	0.094	0.118
	(0.027)		(0.026)		(0.023)	
Control mean	0.328	0.326	0.256	0.269	0.182	0.161

Note.– This table shows treatment effects and control means that we aim to match in the model estimation. The coefficients are OLS estimates; standard errors clusteted at school level are reported in parentheses for the data columns. All regressions include all model initial conditions except region and survey missing. *Treatment* is a dummy variable indicating whether a student is in a school randomly assigned to be in the PACE program. The outcome variable in columns (1)-(2) and is an indicator for being admitted to a selective college via regular or preferential admissions. The outcome variable in columns (3)-(4) and is an indicator for being enrolled in a selective college one year after high school. The outcome variable in columns (5)-(6) and is an indicator for being enrolled in a selective college five years after high school. Regressions in panel A are estimated on the entire sample of students in experimental schools. Regressions in panel B are estimated on the sample of students who at the end of 10^{th} grade were in the top 15% of their school according to GPA in the first two high school years.

E Additional Details on Analysis of Mechanisms

E.1 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 (Table E1), suggesting that grading did not respond to the treatment. Consistent with this result, school principals report similar grading practices across treatment groups (Table E2).

Table E1: Teacher Grading

	12^{th} grade core GPA (standardized)	
Achievement Score	0.335***	0.247***
	(0.025)	(0.025)
Achievement Score × Treatment	-0.031	-0.052
	(0.035)	(0.034)
Baseline SIMCE test score	NO	YES
Observations	6046	6046
R^2	0.216	0.262

Note.– Coefficients are OLS estimates. Standard errors are clustered at the school level. Standard set of controls except for baseline SIMCE test score. Inverse Probability Weights used. *Core GPA* is the GPA in the core subjects, which are those tested on the PSU entrance exam. *Treatment* is a dummy variable indicating whether a student is in a school that was randomly assigned to be in the PACE program. *p < 0.10; **p < 0.05; ***p < 0.01.

Table E2: Survey of School Principals: Grading Methods and Support Classes

	(1) Teachers discuss	(2) Teachers adjust	(3) Support (general)	(4) Support PSU	(5) Frequency support
Treatment	-0.019	-0.020	-0.055	0.042	-0.113
	(0.069)	(0.078)	(0.089)	(0.082)	(0.155)
Observations	127	127	127	127	64

Note.– Coefficients are OLS estimates. Treatment is a dummy variable indicating whether a student is in a school that was randomly assigned to be in the PACE program. Outcome variables: dummy variables indicating whether teachers meet at the end of the year to discuss the grades of each student (column 1), whether teachers adjusts grades based on students' motivation, effort or other reason (column 2), whether the school offered support classes in any subject (column 3) and support classes for PSU entrance exam preparation (column 4) to the cohort of students under study. The outcome in the last column is the number of support classes per week. *p $<$ 0.10; **p $<$ 0.05; ***p $<$ 0.01.

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. Section E.1.1 describes how we measured these teacher behaviors, and Table E3 shows that there is no evidence that such behaviors responded to the policy.

Table E3: Treatment Effects on Teachers Effort and Focus of Instruction

	Effort (Prep Hours)		Effort (Absences)		Focus of Instruction	
	Mathematics	Language	Mathematics	Language	Mathematics	Language
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.007	0.264	0.282	0.134	0.022	0.022
	(1.129)	(0.450)	(1.223)	(1.001)	(0.030)	(0.028)
RW-adj p-val	0.998	0.976	0.996	0.996	0.976	0.976
q-val	1.000	1.000	1.000	1.000	1.000	1.000
Control mean	6.068	5.723	3.366	2.947	0.185	0.346
R-squared	0.000	0.004	0.001	0.000	0.006	0.007
Observations	308	315	308	315	308	315

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.1.1 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. *RW-adj p-val* and *q-val* indicate Romano-Wolf adjusted p-values using 1000 bootstrap replications and q-values of the treatment effect, considering all the outcomes in the table as one family. $* < 0.10$; $** < 0.05$; $*** < 0.01$.

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 Table E2.

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 Table E4.

Table E4: Survey of School Principals: Assignment of Students to Classrooms

	(1) Assignment Fixed	(2) Ability Tracking	(3) Random Assignment	(4) Alphabetical Assignment
Treatment	0.044	-0.049	-0.012	0.048
	(0.071)	(0.090)	(0.078)	(0.046)
Observations	127	93	127	127

Note.– Coefficients are OLS estimates. *Treatment* is a dummy variable indicating whether a student is in a school that was randomly assigned to be in the PACE program. The outcome variables are dummy variables indicating whether: a student must stay in the same class throughout high school (column (1)), the school allocate students to classrooms based on ability (column (2)), the school allocates students to classrooms at random (column (3)), the student allocates students to classrooms alphabetically (column (4)). *p $<$ 0.10; **p $<$ 0.05; ***p $<$ 0.01.

E.1.1 Construction of Teacher Variables

This Section explains how we constructed the teacher variables that enter Table E3 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 Reduction in Perceived Returns to College

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 (Section E.2.1), which are large at 82% of age 30 earnings. Such large perceived returns are similar to those measured in Hastings, Neilson, Ramirez, and Zimmerman, 2016 among Chilean students. The policy had no effect on students' awareness of financial aid (83.3% 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=0.351)). Therefore, the treatment did not affect students' perceived net returns to college.

E.2.1 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 82 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 E5.

Table E5: Effect of PACE on the Mean and the Variance of the Subjective Distribution of Earnings 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
	(1)	(2)	(3)	(4)	(5)	(6)
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)
RW-adj p-val	0.882	0.900	0.476	0.504	0.900	0.704
q-val	1.000	1.000	0.556	0.556	1.000	0.556
R-squared	0.094	0.057	0.016	0.013	0.000	0.001
Observations	3339	2048	4219	2674	4219	2674

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). *RW-adj p-val* and *q-val* indicate Romano-Wolf adjusted p-values using 1000 bootstrap replications and q-values of the treatment effect, considering all the outcomes in the table as one family. 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 Applications and admissions

Comparing regular applications and admissions between students in treated and control schools. PACE had null effects on the proportion of students sending a regular application and receiving a regular admission (Table F1). Descriptive statistics show that regular-channel applicants from treated schools tend to apply and be admitted to more selective majors than regular-channel applicants from control schools (first three columns of Table F2 and Figure F1), but the differences in the application and admission patterns are close to zero and statistically insignificant once we control for the different pool of applicants across treatment groups by including students' baseline characteristics (Panels A and C, columns (3) and (6) of Table F3).[5] Descriptive statistics also suggest that regular applicants from treated schools apply and are admitted to programs that are closer to their high school on average (first three columns of Table F2), because they apply at lower rates to programs that are more than 500km away (Figure F3). But these differences are statistically insignificant (columns (1) and (4) of Table F3). Finally, application and admission patterns across various majors are similar between treatment groups. The main exception is that students from treated schools are admitted at higher rates to natural sciences and lower rates to engineering—both STEM majors—compared to those from control schools (Figure F5). However, the lower regular-channel engineering admissions for treated school students are offset by higher PACE-channel engineering admissions (Figure F6). And importantly, regular applicants from both groups apply and are admitted to STEM and non-STEM majors at nearly identical rates (first three columns of Table F2, columns (2) and (5) of Table F3). Therefore, we do not find significant differences in admission patterns through the regular channel across treatment groups.

Comparing applications and admissions across the regular and PACE channels for students in treated schools. Top-performing students within their high school, who are those where PACE applications and admissions are concentrated, apply to more selective programs through the PACE than through the regular channel, but they are admitted to programs that are similarly selective across channels (Figure F2, Panel A of Table F2, and column (3) of Table F4). They apply and are admitted to programs that are similarly distant from their high school across the regular and PACE channels (Figure F4 and Panel A of Table F2), with only small and insignificant differences across channels (column (1) of Table F4). Finally, these students are slightly more likely to send applications to STEM programs through the PACE channel, although the difference is not statistically significant for the top choice once we account for multiple hypotheses testing (Panel A of Table F2 and Panels A and B, column (2) of Table F4). This is driven by listing slightly more engineering and health programs and

[5] There is a statistically significant difference at the 10% level for applications in the all-student sample, but the significance disappears once we account for multiple hypothesis testing (RW-adjusted p-value: 0.405).

slightly fewer education and arts programs (Figure F6). While there are some differences in the majors to which students are admitted across channels—notably, students are more likely to be admitted to engineering and less likely to be admitted to health programs through the PACE channel (Figure F6)—there are no statistically significant differences across channels in the STEM composition of the programs to which they are admitted (Panel C, column (2) of Table F4). Therefore, we do not find significant differences in the admission patterns through the regular and PACE channels for students in PACE schools.

F.1 Figures

F.1.1 Selectivity

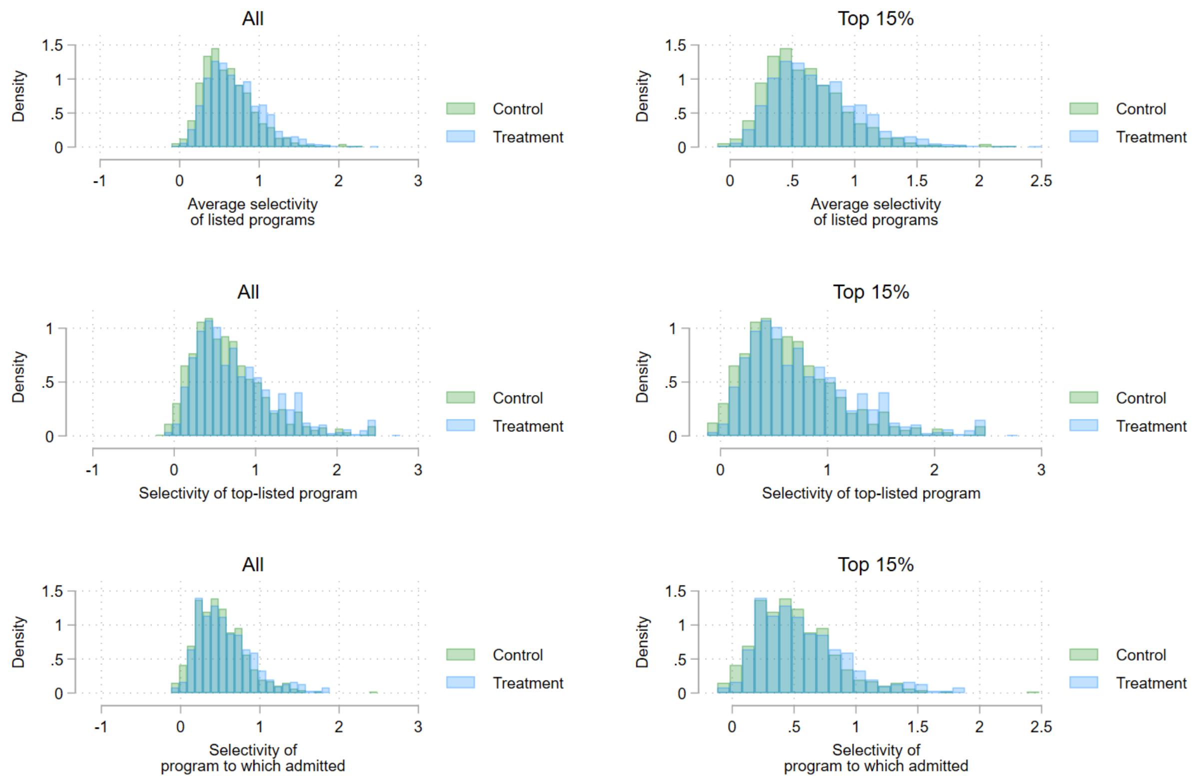


Figure F1: Selectivity of programs to which students apply through the regular channel and where they are admitted across treatment and control groups. The left panel shows all students regardless of their ranking in their high school, the right panel shows the students who were in the top 15% of their high school GPA ranking at baseline.

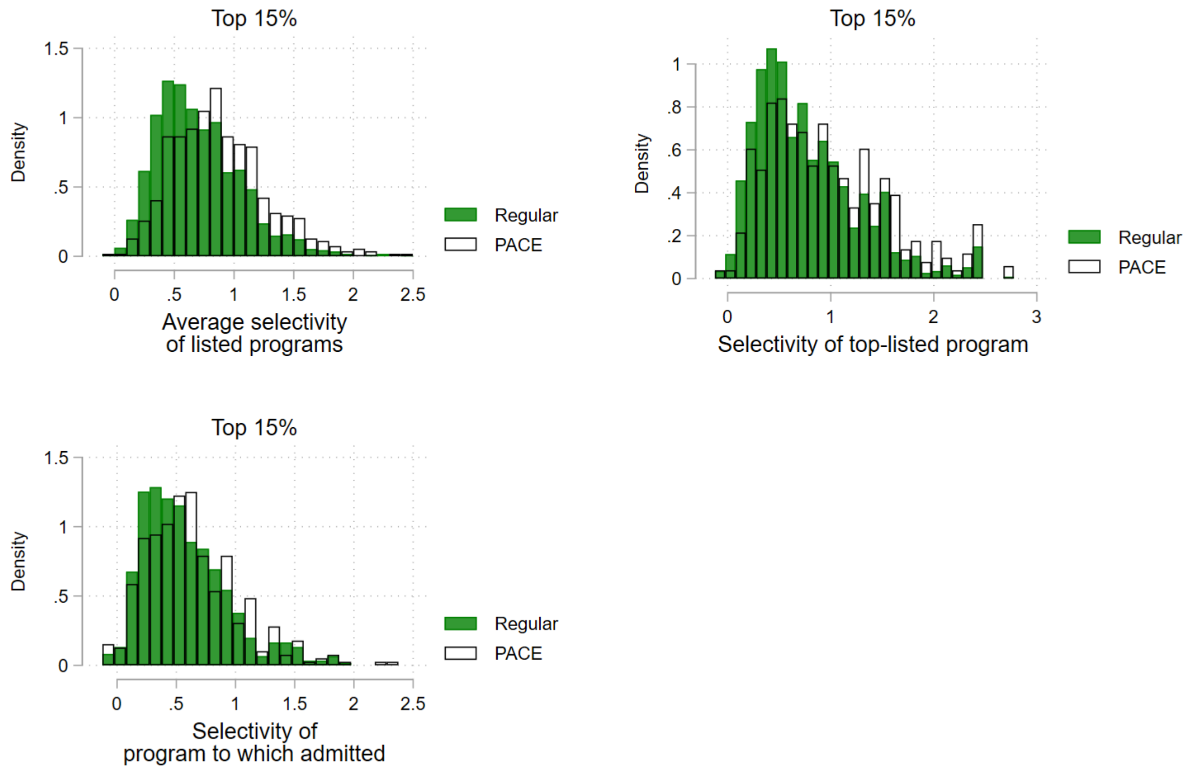


Figure F2: Selectivity of programs to which top 15% treated students apply and where they are admitted across regular and PACE application channels. These students attended treated schools and were in the top 15% of their high school GPA ranking at baseline.

F.1.2 Location

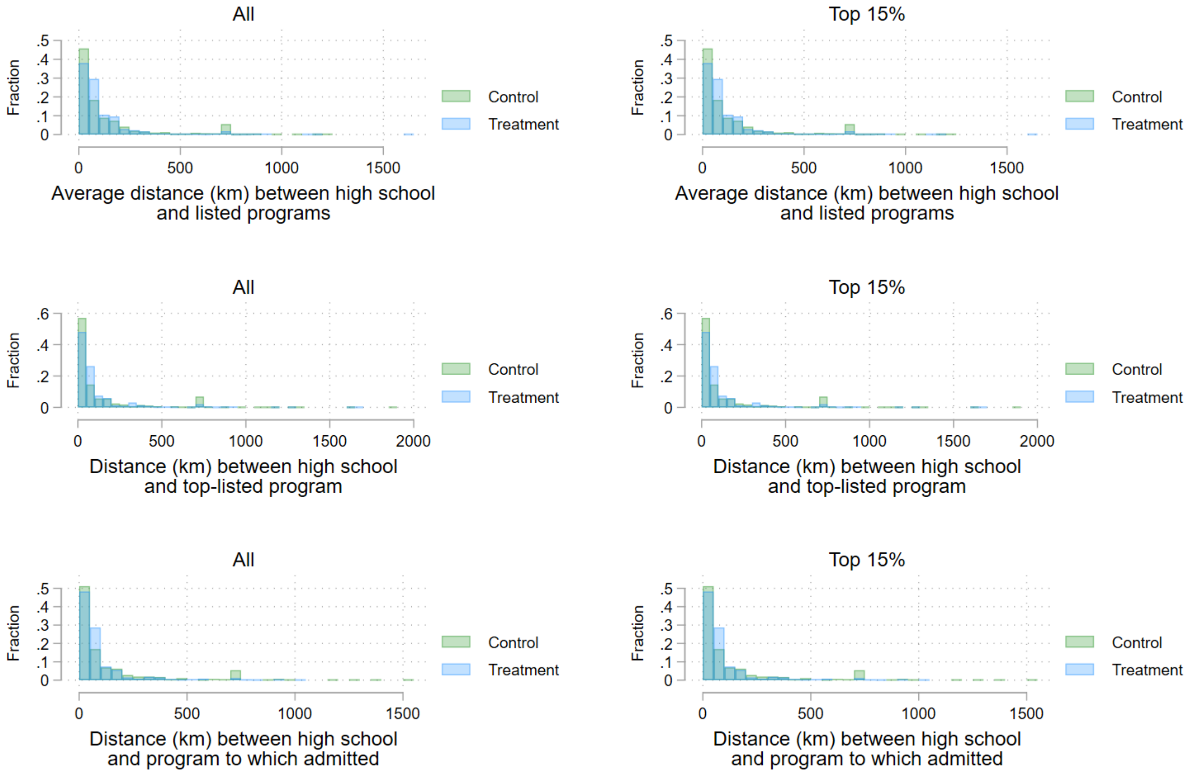


Figure F3: Location of programs to which students apply through the regular channel and where they are admitted through the regular channel across treatment and control groups. The left panel shows all students regardless of their ranking in their high school, the right panel shows the students who were in the top 15% of their high school GPA ranking at baseline.

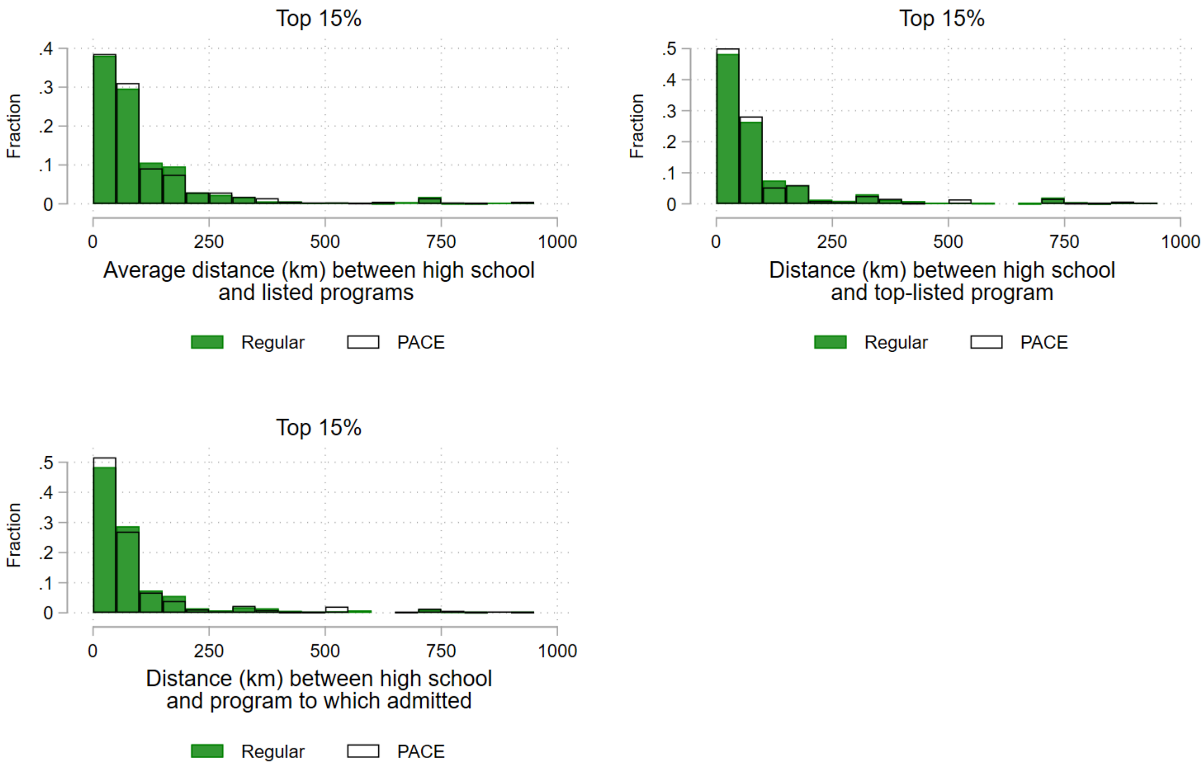


Figure F4: Location of programs to which top 15% treated students apply and where they are admitted across regular and PACE application channels. These students attended treated schools and were in the top 15% of their high school GPA ranking at baseline.

F.1.3 Study field

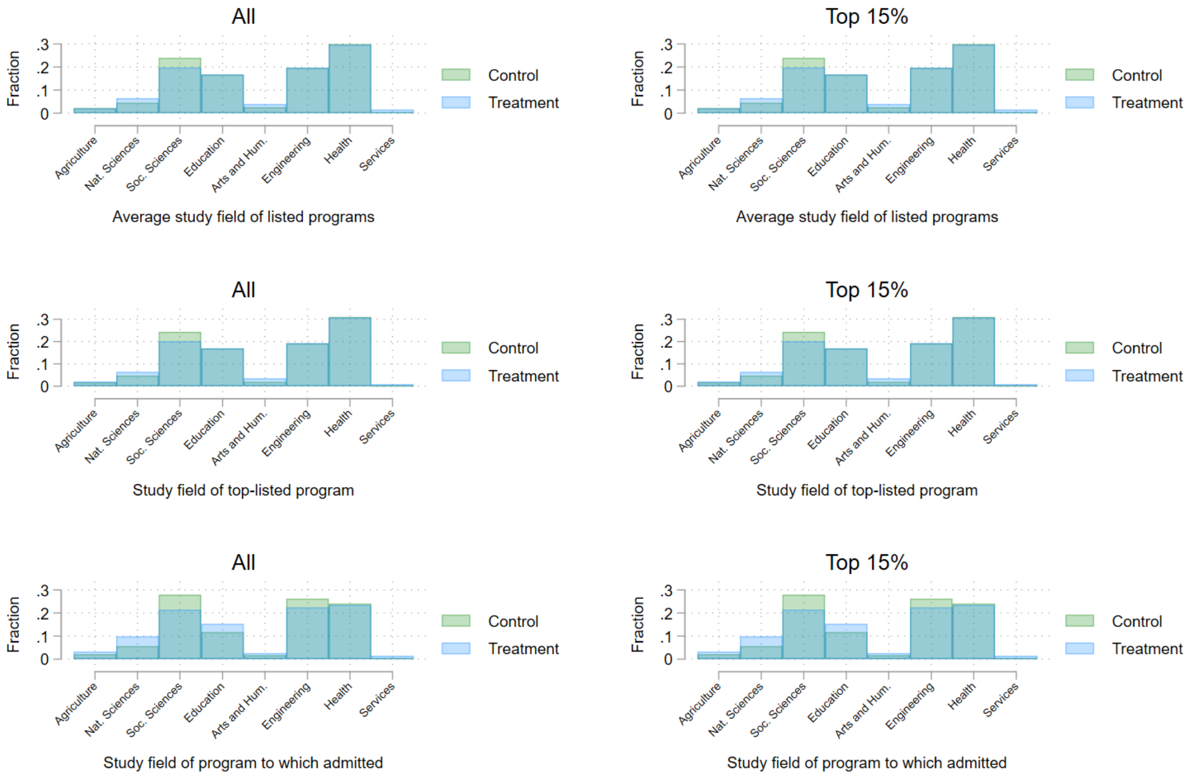


Figure F5: Study field to which students apply through the regular channel and where they are admitted through the regular channel across treatment and control groups. The left panel shows all students regardless of their ranking in their high school, the right panel shows the students who were in the top 15% of their high school GPA ranking at baseline. The average study field of listed programs is computed by taking the average across students of the fraction of programs listed by each student belonging to that study field.

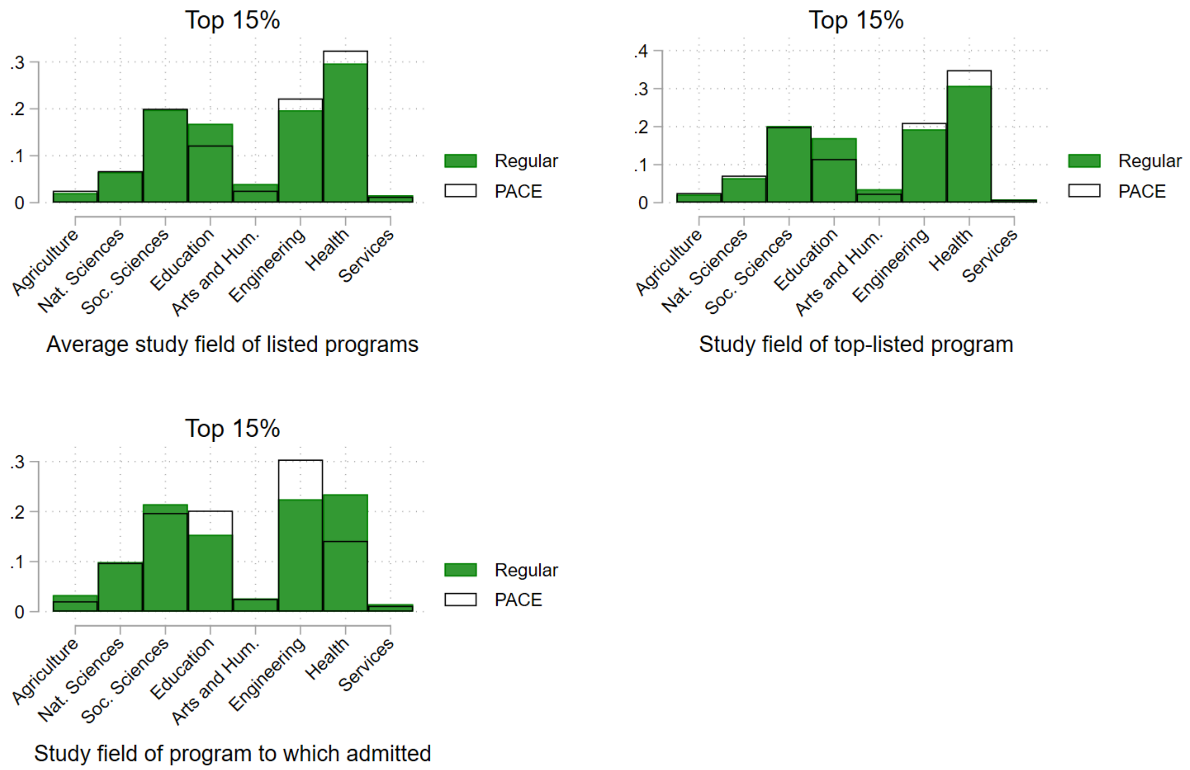


Figure F6: Study field to which top 15% treated students apply and where they are admitted across regular and PACE application channels. These students attended treated schools and were in the top 15% of their high school GPA ranking at baseline. The average study field of listed programs is computed by taking the average across students of the fraction of programs listed by each student belonging to that study field.

F.2 Tables

Table F1: Effects of PACE on Selective College Applications and Admissions through the Regular Channel

	All sample		Bottom 85%		Top 15%	
	Applications	Admissions	Applications	Admissions	Applications	Admissions
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.002	-0.009	-0.006	-0.005	0.048	0.001
	(0.020)	(0.011)	(0.018)	(0.010)	(0.039)	(0.027)
p-val(family: sample)	0.914	0.593	0.803	0.803	0.329	0.964
q-val(family: sample)	1.000	1.000	1.000	1.000	0.755	0.944
q-val(family: outcome)			0.755	1.000	0.755	1.000
Control mean	0.210	0.114	0.161	0.070	0.450	0.328
Observations	8944.000	8944.000	7061.000	7061.000	1563.000	1563.000

Note.– Columns (1) and (2) use the sample of all students in the experiment. Columns (3) and (4) use the sample of 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 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 slightly larger than 15% 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. 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-val(family: sample)* and *q-val(family: outcome)* indicate Romano-Wolf adjusted p-values using 1000 bootstrap replications and sharpened q-values of the treatment effect, considering each sample as one family. *q-val(family: sample)* indicate sharpened q-values of the treatment effect, considering the same outcome variable across sub-samples as one family. *p < 0.10; **p < 0.05; ***p < 0.01.

Table F2: Description of application lists and admissions to selective colleges.

	Regular applications			PACE applications		
	Mean	St.dev.	N	Mean	St.dev.	N
	(1)	(2)	(3)	(4)	(5)	(6)
A. Top 15%, Treated Students						
Average distance (km) from listed programs	118	162	450	118	164	439
Fraction of STEM among listed programs	.616	.393	450	.656	.37	439
Average selectivity of listed programs	.807	.378	450	.911	.403	434
Distance (km) from top-listed program	111	183	450	104	179	439
Top-listed program is STEM	.647	.479	450	.677	.468	439
Selectivity of top-listed program	.916	.556	450	1.02	.598	409
Distance (km) from program to which admitted	91.4	148	295	99.1	167	350
Program to which admitted is STEM	.619	.486	294	.583	.494	350
Selectivity of program to which admitted	.71	.39	295	.675	.405	317
B. Top 15%, Control students						
Average distance (km) from listed programs	143	215	331	.	.	.
Fraction of STEM among listed programs	.613	.396	331	.	.	.
Average selectivity of listed programs	.73	.373	331	.	.	.
Distance (km) from top-listed program	128	224	331	.	.	.
Top-listed program is STEM	.644	.48	331	.	.	.
Selectivity of top-listed program	.827	.511	331	.	.	.
Distance (km) from program to which admitted	131	221	230	.	.	.
Program to which admitted is STEM	.626	.485	230	.	.	.
Selectivity of program to which admitted	.638	.349	230	.	.	.
C. All, Treated students						
Average distance (km) from listed programs	114	157	1137	.	.	.
Fraction of STEM among listed programs	.559	.4	1137	.	.	.
Average selectivity of listed programs	.704	.356	1137	.	.	.
Distance (km) from top-listed program	107	177	1137	.	.	.
Top-listed program is STEM	.564	.496	1137	.	.	.
Selectivity of top-listed program	.773	.51	1137	.	.	.
Distance (km) from program to which admitted	93.9	144	607	.	.	.
Program to which admitted is STEM	.558	.497	606	.	.	.
Selectivity of program to which admitted	.573	.363	607	.	.	.
D. All, Control students						
Average distance (km) from listed programs	140	210	887	.	.	.
Fraction of STEM among listed programs	.541	.398	887	.	.	.
Average selectivity of listed programs	.61	.333	887	.	.	.
Distance (km) from top-listed program	128	228	887	.	.	.
Top-listed program is STEM	.549	.498	887	.	.	.
Selectivity of top-listed program	.663	.459	886	.	.	.
Distance (km) from program to which admitted	138	231	461	.	.	.
Program to which admitted is STEM	.56	.497	461	.	.	.
Selectivity of program to which admitted	.505	.327	460	.	.	.

Note. – This Table provides summary statistics on the programs to which students apply and are admitted through the regular and the PACE channels. Within each channel, students submit ranked preference lists, and can apply to a maximum of ten programs. Panels A and B restrict the sample to students who were in the top 15% of their high school GPA ranking at baseline. Panels C and D consider all students, regardless of their within-school rank. Treated students are those who attended schools randomly allocated to PACE, control students are those who attended schools randomly allocated to the control group. Columns (1) to (3) describe applications and admissions through the regular channel; columns (4) to (6) through the PACE channel. As a measure of distance we use the length (km) of the shortest path between the coordinates of the program and of the high school the student attended, implementing Vincenty formula to calculate distances on a reference ellipsoid. Selectivity is the average PSU score of all regular entrants in the program in 2018 (standardized).

Table F3: Comparison between regular application lists and admissions to selective colleges in the treatment and control groups.

A. All listed programs

	All students			Top 15%		
	Average distance	Fraction STEM	Average selectivity	Average distance	Fraction STEM	Average selectivity
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-27.697	0.008	0.060*	-26.639	-0.001	0.046
	(43.277)	(0.028)	(0.031)	(44.019)	(0.028)	(0.038)
RW-adj p-val	0.943	0.943	0.405	0.974	0.992	0.792
q-val	1.000	1.000	0.963	1.000	1.000	1.000
Control mean	140.212	0.541	0.609	142.629	0.613	0.730
R-squared	0.011	0.020	0.127	0.013	0.040	0.160
Observations	2013	2013	2013	781	781	781

B. Top-listed program

	All students			Top 15%		
	Distance	STEM	Selectivity	Distance	STEM	Selectivity
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-22.719	0.002	0.061	-19.616	-0.006	0.044
	(44.138)	(0.031)	(0.038)	(44.622)	(0.033)	(0.051)
RW-adj p-val	0.943	0.964	0.558	0.992	0.992	0.921
q-val	1.000	1.000	0.963	1.000	1.000	1.000
Control mean	128.125	0.549	0.662	127.591	0.644	0.827
R-squared	0.006	0.018	0.116	0.009	0.045	0.151
Observations	2013	2013	2012	781	781	781

C. Program to which admitted

	All students			Top 15%		
	Distance	STEM	Selectivity	Distance	STEM	Selectivity
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-47.525	-0.025	-0.016	-42.614	-0.018	-0.010
	(35.564)	(0.035)	(0.024)	(40.107)	(0.043)	(0.035)
RW-adj p-val	0.675	0.943	0.943	0.853	0.992	0.992
q-val	0.963	1.000	1.000	1.000	1.000	1.000
Control mean	138.261	0.558	0.506	130.659	0.626	0.638
R-squared	0.035	0.029	0.258	0.018	0.062	0.262
Observations	1064	1063	1063	525	524	525

Note.– The coefficients are OLS estimates. Standard errors were clustered at the school level. All regressions use the standard set of controls (see notes under Figure 2). *Treatment* is a dummy variable indicating whether a student is in a school randomly assigned to be in the PACE program. The regressions use data on regular application lists to selective colleges submitted by all treated and control students (columns 1-3) or by students in the top 15% of their high school GPA ranking at baseline (columns 4-6). The application preference lists are the lists of programs for which the student expressed their ranked preference, up to a maximum of ten. As a measure of distance we use the length (km) of the shortest path between the coordinates of the program and the coordinates of the high school the student attended, implementing Vincenty formula to calculate distances on a reference ellipsoid. Selectivity is the average PSU score of all regular entrants in the program in 2018 (standardized). *RW-adj p-val* and *q-val* indicate Romano-Wolf adjusted p-values using 1000 bootstrap replications and q-values of the treatment effect, considering all outcomes in each sample as one family. * p<0.10; ** p<0.05; *** p<0.01

Table F4: Comparison between PACE and regular application lists and admissions to selective colleges for treated student in Top 15%

	A. All listed programs		
	Average distance	Fraction STEM	Average selectivity
	(1)	(2)	(3)
PACE channel	0.174	0.041***	0.115***
	(5.457)	(0.014)	(0.018)
RW-adj p-val	0.967	0.043	0.001
q-val	0.513	0.012	0.001
Control mean	117.960	0.636	0.858
R-squared	0.018	0.053	0.168
Observations	889	889	884
	B. Top-listed program		
	Distance	STEM	Selectivity
	(1)	(2)	(3)
PACE channel	-5.140	0.034**	0.118***
	(7.017)	(0.017)	(0.026)
RW-adj p-val	0.805	0.264	0.003
q-val	0.501	0.080	0.001
Control mean	107.479	0.661	0.965
R-squared	0.014	0.050	0.149
Observations	889	889	859
	C. Program to which admitted		
	Distance	STEM	Selectivity
	(1)	(2)	(3)
PACE channel	5.541	-0.031	0.047
	(11.289)	(0.025)	(0.039)
RW-adj p-val	0.857	0.668	0.668
q-val	0.513	0.233	0.233
Control mean	95.561	0.599	0.692
R-squared	0.005	0.080	0.222
Observations	645	644	612

Note.– The coefficients are OLS estimates. Standard errors were clustered at the school level. All regressions use the standard set of controls (see notes under Figure 2). *PACE channel* is a dummy variable equal to 1 if the application (Panels A and B) or admission (Panel C) is through the PACE channel, 0 if it is through the regular channel. Within each channel, students submit ranked preference lists, and can apply to a maximum of ten programs. The regressions use data on regular and PACE selective college admissions and application lists to selective colleges, restricting the sample to students from treated schools who were in the top 15% of their high school GPA ranking at baseline. As a measure of distance we use the length (km) of the shortest path between the coordinates of the program and of the high school the student attended, implementing Vincenty formula to calculate distances on a reference ellipsoid. Selectivity is the average PSU score of all regular entrants in the program in 2018 (standardized). *RW-adj p-val* and *q-val* indicate Romano-Wolf adjusted p-values using 1000 bootstrap replications and q-values of the pace application coefficient, considering all outcomes in the table as one family. * p<0.10; ** p<0.05; *** p<0.01

G Technical Appendix

G.1 PISA score re-scaling

Figure 1 plots the histogram of tenth grade SIMCE test scores, and draws a line corresponding to the OECD mean for reference (at 0.49). Since the SIMCE tests are administered only nationally, we draw on data from PISA in Chile and in OECD countries to predict the SIMCE mean in OECD countries. This is the reasoning and procedure we follow:

- In 2015 the mean PISA scores of Chile were 447 in Science, 459 in Reading, 423 in Math.
- In 2015 the mean PISA scores of OECD were 493 in Science, 493 in Reading, 490 in Math.
- There is theoretically no minimum or maximum score in PISA; rather, the results are scaled to fit approximately normal distributions, with means around 500 score points and standard deviations around 100 score points.
- Therefore, OECD countries had a:
  - mean Science score of $\frac{493-447}{100} = 0.46$ standard deviations above the Chilean one;
  - mean Reading score of $\frac{493-459}{100} = 0.34$ standard deviations above the Chilean one;
  - mean Mathematics score of $\frac{490-423}{100} = 0.67$ standard deviations above the Chilean one;
- On average, OECD countries had mean PISA scores that were higher than the Chilean mean PISA score by $(0.46 + 0.34 + 0.67)/3 = 0.49$ standard deviations.
- Sources: Link 1, Link 2

G.2 Additional Details on Measuring Perceived Returns to Effort in GPA Production

Unlike with PSU, we assume the perceived returns to effort in GPA production are constant across GPA levels; since we only observe two hypothetical effort and GPA levels, we cannot measure nonlinearities. We express returns directly in GPA points rather than standardizing because GPA is already measured on a meaningful scale, the same used for the top 15 percent cutoff. Letting $e_i^{5.5}$ and $e_i^{T15_i}$ denote the reported hypothetical effort levels and $T15_i$ the reported perceived cutoff, we compute the perceived return to effort in GPA production, β_{1i}^{Gb} in equation (17), as follows:

$$\beta_{1i}^{Gb} = \frac{T15_i - 5.5}{e_i^{T15_i} - e_i^{5.5}}. \tag{16}$$

G.3 Imputation of Beliefs Serving as Initial Conditions

For students with missing responses on perceived returns to effort and selective college persistence, we impute $\beta^{Pb}_{1i}, \beta^{Pb}_{2i}, e^{Pb}_{kink,i}, \beta^{Gb}_{1i}, pgrad^b_i$ using a LASSO regression model. The model achieves an excellent fit, as shown in Figure G1, with model specification provided in the Figure notes. For students with missing responses on the perceived top 15% cutoff, we impute them with their answers to an earlier survey conducted by the Ministry of Education five months prior, which included a question measuring the same construct.[6] For any remaining missing values, we substitute the actual top 15% cutoff, ensuring that our findings on the role of biased beliefs can be conservatively interpreted as lower bounds.

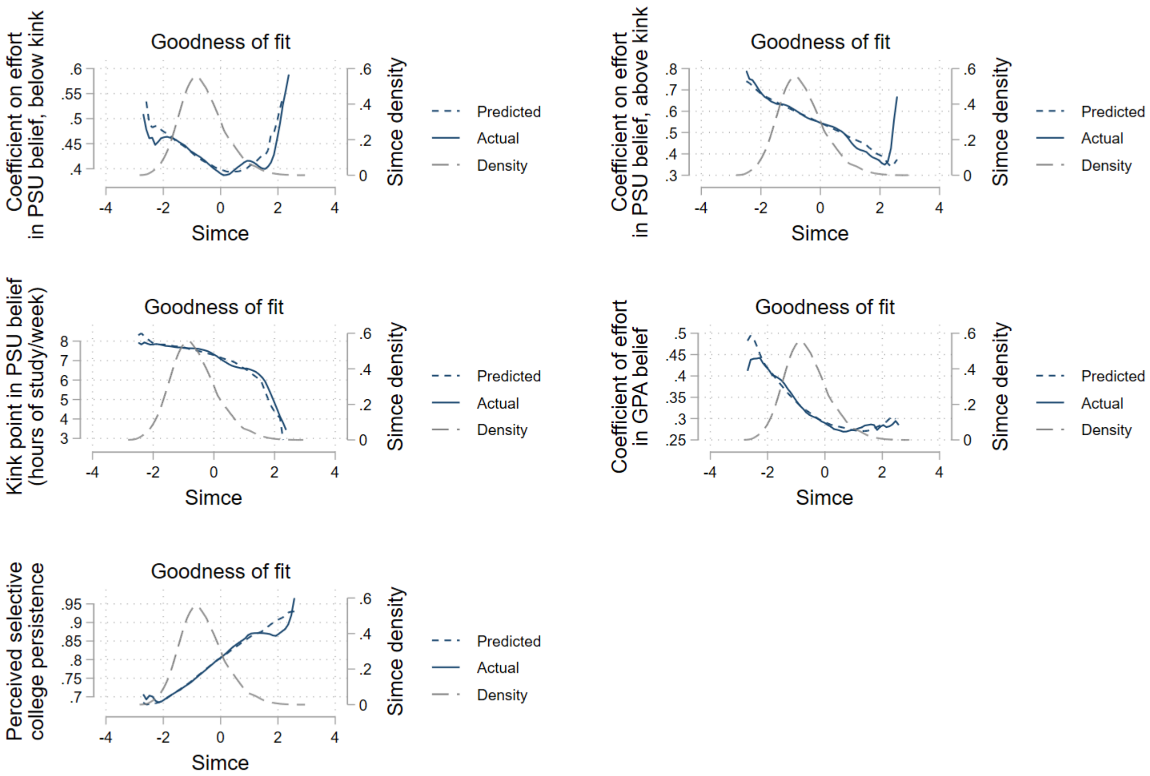


Figure G1: Goodness of fit of LASSO regression model used to impute missing data on beliefs that are initial conditions in the model: $\beta^{Pb}_{1i}, \beta^{Pb}_{2i}, e^{Pb}_{kink,i}, \beta^{Gb}_{1i}, pgrad^b_i$. The list of all potential predictors is: SIMCE score, gender, age, low-SES status, school track, second and third powers of SIMCE score and of age, and all 36 pairwise interactions between these variables.

[6] The question in the Ministerial survey was: "Think of the 15% of 12^{th} grade students in your school with the highest GPA. What is the lowest GPA a student in your school would need to achieve to be in the top 15% of students with the best GPA?". As in our main survey (see Table C16), the question uses the Chilean term for GPA that is widely understood to refer to the grade point average across all four years of high school.

G.4 Additional Details on Identification and Estimation

G.4.1 Identification and estimation of perceived production functions

We estimate perceived production functions outside of the model, exploiting survey measures of study effort, of perceived returns to study effort in producing GPA and the PSU score, and of the expected GPA and PSU score at the effort students exerted.

Perceived GPA production. Students form beliefs about their GPA in the last two high school years:

$$\begin{aligned} GPA_i^{(11-12,b)} &= \overline{GPA}_i^{(11-12,b)} + \epsilon_i^{Gb} \\ &= \beta_0^{Gb} + \beta_{1i}^{Gb} e_i + \beta_2^{Gb}\text{GPA}_{i,t-1} + \beta_3^{Gb}\text{simce}_{i,t-1} + \epsilon_i^{Gb} \quad \epsilon_i^{Gb} \sim N(0, \sigma^2_{GPA^b}). \end{aligned} \tag{17}$$

Recall that we obtain β_{1i}^{Gb} from the survey data, as explained in section 6.1. Consistent with the note to Table C16, β_{1i}^{Gb} captures the perceived effect of an additional hour of study per week during a given period on GPA in that same period–the object entering the production function in equation 17. Letting an o superscript indicate the observed effort measure, we have that: $e_i^o = e_i + \epsilon_i^{mee}$, where $\epsilon_i^{mee} \sim N(0, \sigma^2_{mee})$ is a measurement error shock that is independently distributed from all model shocks, true effort, and initial conditions. As a function of observed effort, e_i^o, this production function is:

$$\begin{aligned} GPA_i^{(11-12,b)} &= \overline{GPA}_i^{(11-12,b)} + \epsilon_i^{Gb} \\ &= \beta_0^{Gb} + \beta_{1i}^{Gb}(e_i^o - \epsilon_i^{mee}) + \beta_2^{Gb}\text{GPA}_{i,t-1} + \beta_3^{Gb}\text{simce}_{i,t-1} + \epsilon_i^{Gb} \\ &= \beta_0^{Gb} + \beta_{1i}^{Gb} e_i^o + \beta_2^{Gb}\text{GPA}_{i,t-1} + \beta_3^{Gb}\text{simce}_{i,t-1} + (\epsilon_i^{Gb} - \beta_{1i}^{Gb}\epsilon_i^{mee}). \end{aligned}$$

Subtracting the measured impact of effort, $\beta_{1i}^{Gb} e_i^o$, from both sides of the equation, we obtain:

$$\begin{aligned} GPA_i^{(11-12,b)} - \beta_{1i}^{Gb} e_i^o &= \overline{GPA}_i^{(11-12,b)} - \beta_{1i}^{Gb} e_i^o + \epsilon_i^{Gb} \\ &= \beta_0^{Gb} + \beta_2^{Gb}\text{GPA}_{i,t-1} + \beta_3^{Gb}\text{simce}_{i,t-1} + (\epsilon_i^{Gb} - \beta_{1i}^{Gb}\epsilon_i^{mee}). \end{aligned} \tag{18}$$

Setting the two right-hand expressions from the equations in (18) equal to each other and denoting $\overline{GPA}_i^{(11-12,b)} - \beta_{1i}^{Gb} e_i^o$ by $GPA_i^{b,net}$, we obtain:

$$GPA_i^{b,net} = \beta_0^{Gb} + \beta_2^{Gb}\text{GPA}_{i,t-1} + \beta_3^{Gb}\text{simce}_{i,t-1} + \nu_i^{Gb}, \tag{19}$$

where $\nu_i^{Gb} = -\beta_{1i}^{Gb}\epsilon_i^{mee}$. The left-hand side of equation (19) is data. In particular, we obtain the belief over GPA in the last two high school years through a combination of survey and administrative data. In the survey we elicited the expected GPA over the four high school years, $\overline{GPA}_i^{(9-12,b)}$. Since students already knew their GPA in years 9 and 10 when answering the survey, we obtain the belief over the GPA in the last two high school years as: $\overline{GPA}_i^{(11-12,b)} = 2\left(\overline{GPA}_i^{(9-12,b)} - \frac{1}{2}\overline{GPA}_i^{(9-10)}\right)$, assuming that the belief over the GPA in the first two years is correct. This assumption is realistic as students hold accurate beliefs even about their future GPA (section 4.2.1). Under the assumption that the measurement error shock is mean zero and orthogonal to all initial conditions, the conditional expectation $E[\nu_i^{Gb}|\text{simce}_{i,t-1}, GPA_{i,t-1}, \beta_{1i}^{Gb}]$ equals zero. Therefore, OLS estimation of equation (19) gives consistent estimates of β_0^{Gb}, β_2^{Gb} and β_3^{Gb}.

Perceived PSU production. We report below the perceived production function of the PSU entrance exam score from equation (2) :

$$
\begin{aligned}
PSU_i^b &= \overline{PSU}_i^b + \epsilon_i^{Pb} \\
&= \beta_0^{Pb} + \beta_{1i}^{Pb} e_i \mathbf{1}(e_i < e_{kink,i}^{Pb}) + \beta_{2i}^{Pb} e_i \mathbf{1}(e_i \geq e_{kink,i}^{Pb}) \\
&\quad + \beta_3^{Pb}\text{GPA}_{i,t-1} + \beta_4^{Pb}\text{simce}_{i,t-1} + \epsilon_i^{Pb} \quad \epsilon_i^{Pb} \sim N(0, \sigma_{PSU^b}^2).
\end{aligned}
$$

Recall that we obtain β_{1i}^{Pb} and β_{2i}^{Pb} from the survey data, as explained in section 6.1. For each student, we determine whether the perceived marginal return at the effort actually exerted is β_{1i}^{Pb} or β_{2i}^{Pb}, denoting this value as $\beta_{actual,i}^{Pb}$. Since exerted effort is measured with error, we cannot directly verify whether it exceeds the kink point. Instead, for students with a subjective expectation of the PSU ($\overline{PSU}_i^b$) equal to or larger than 450 we assume their effort is above the kink and the marginal return is β_{2i}^{Pb}. For students with a subjective expectation of the PSU below 450 we assume their effort is below the kink and their marginal return is β_{1i}^{Pb}.[7]
As a function of $\beta_{actual,i}^{Pb}$, the production function of PSU_i^b then is:

$$
\begin{aligned}
PSU_i^b &= \overline{PSU}_i^b + \epsilon_i^{Pb} \\
&= \beta_0^{Pb} + \beta_{actual,i}^{Pb} e_i + \beta_3^{Pb}\text{GPA}_{i,t-1} + \beta_4^{Pb}\text{simce}_{i,t-1} + \epsilon_i^{Pb}
\end{aligned}
$$

From the survey, we obtain a noisy measure of the effort a student actually exerted. As a function of observed effort, the production function of PSU_i^b is:

[7] For this assumption to be true, it is sufficient that the belief shock realization is the same for hypothetical and actual perceived PSU levels and that students interpret the hypothetical PSU levels in the survey questions used to construct the returns (reported in the last row of Table C16) as expected values, i.e., net of the realization of the belief uncertainty shock ϵ_i^{Pb}.

$$\begin{aligned}
PSU_i^b &= \overline{PSU}_i^b + \epsilon_i^{Pb} \quad (20)\\
&= \beta_0^{Pb} + \beta_{actual,i}^{Pb}(e_i^o - \epsilon_i^{mee}) + \beta_3^{Pb}\text{GPA}_{i,t-1} + \beta_4^{Pb}\text{simce}_{i,t-1} + \epsilon_i^{Pb}\\
&= \beta_0^{Pb} + \beta_{actual,i}^{Pb}e_i^o + \beta_3^{Pb}\text{GPA}_{i,t-1} + \beta_4^{Pb}\text{simce}_{i,t-1} + (\epsilon_i^{Pb} - \beta_{actual,i}^{Pb}\epsilon_i^{mee}).
\end{aligned}$$

Subtracting the measured impact of effort, $\beta_{actual,i}^{Pb}e_i^o$, from both sides of the equation, we obtain:

$$\begin{aligned}
PSU_i^b - \beta_{actual,i}^{Pb}e_i^o &= \overline{PSU}_i^b - \beta_{actual,i}^{Pb}e_i^o + \epsilon_i^{Pb} \quad (21)\\
&= \beta_0^{Pb} + \beta_3^{Pb}\text{GPA}_{i,t-1} + \beta_4^{Pb}\text{simce}_{i,t-1} + (\epsilon_i^{Pb} - \beta_{actual,i}^{Pb}\epsilon_i^{mee}).
\end{aligned}$$

Finally, setting the two right-hand expressions from the equations in (21) equal to each other, we obtain:

$$\overline{PSU}_i^b - \beta_{actual,i}^{Pb}e_i^o + \epsilon_i^{Pb} = \beta_0^{Pb} + \beta_3^{Pb}\text{GPA}_{i,t-1} + \beta_4^{Pb}\text{simce}_{i,t-1} + \epsilon_i^{Pb} - \beta_{actual,i}^{Pb}\epsilon_i^{mee},$$

and therefore, denoting $\overline{PSU}_i^b - \beta_{actual,i}^{Pb}e_i^o$ by $PSU_i^{b,net}$, we have that:

$$PSU_i^{b,net} = \beta_0^{Pb} + \beta_3^{Pb}\text{GPA}_{i,t-1} + \beta_4^{Pb}\text{simce}_{i,t-1} + \nu_i^{Pb}, \quad (22)$$

where $\nu_i^{Pb} = -\beta_{actual,i}^{Pb}\epsilon_i^{mee}$. The left-hand side of equation (22) is data. Under the assumption that the measurement error shock is mean zero and orthogonal to all initial conditions, the conditional expectation $E[\nu_i^{Pb}|\text{simce}_{i,t-1}, GPA_{i,t-1}, \beta_{actual,i}^{Pb}]$ equals zero. Therefore, OLS estimation of equation (22) gives consistent estimates of $\beta_0^{Pb}, \beta_3^{Pb}$ and β_4^{Pb}.

Estimates and goodness of fit. Table G1 reports the estimates of $\beta_0^{Pb}, \beta_3^{Pb}, \beta_4^{Pb}, \beta_0^{Gb}, \beta_2^{Gb}$ and β_3^{Gb}. To evaluate the goodness of fit, we compare the predicted perceived achievement scores at the reported effort levels to the actual perceived achievement scores reported in the survey. Specifically, we construct the predicted perceived PSU and GPA as follows:

$$\widehat{\overline{PSU}}_i^b = \hat{\beta}_0^{Pb} + \beta_{actual,i}^{Pb}e_i^o + \hat{\beta}_3^{Pb}\text{GPA}_{i,t-1} + \hat{\beta}_4^{Pb}\text{simce}_{i,t-1} \quad (23)$$

$$\widehat{\overline{GPA}}_i^{(11-12,b)} = \hat{\beta}_0^{Gb} + \beta_{1i}^{Gb}e_i^o + \hat{\beta}_2^{Gb}\text{GPA}_{i,t-1} + \hat{\beta}_3^{Gb}\text{simce}_{i,t-1}. \quad (24)$$

Table G1: Parameters estimated outside of the model, perceived PSU and GPA production

	$PSU^{b,net}$ (1)	$GPA^{b,net}$ (2)
GPA in grades 9-10	-0.116	0.076
	(0.070)	(0.068)
Simce test score in grade 10	0.360***	0.196***
	(0.044)	(0.045)
Constant	-1.401***	4.249***
	(0.397)	(0.395)
Observations	4815	5169

Note.– The Table reports OLS estimates of equations (22) and (19). Standard errors were clustered at the school level. The outcome variables are perceived achievement outcomes, net of the measured perceived impact of effort. * p<0.10; ** p<0.05; *** p<0.01

Figure G2 shows how the predicted perceived outcomes $(\widehat{\overline{PSU}_i^b}, \widehat{\overline{GPA}_i^{(11-12,b)}})$ compare to the perceived outcomes reported in the survey $(\overline{PSU}_i^b, \overline{GPA}_i^{(11-12,b)})$. To mitigate the influence of measurement error in effort (which affects e_i^o in equations (23) and (24)) on the predicted outcomes, we average both predicted and actual outcomes conditional on the SIMCE test score and GPA from grades 9 and 10. Averaging across students helps isolate the model's goodness of fit from noise due to measurement error. As seen in the figure, the fit is excellent in regions with non-negligible student density.

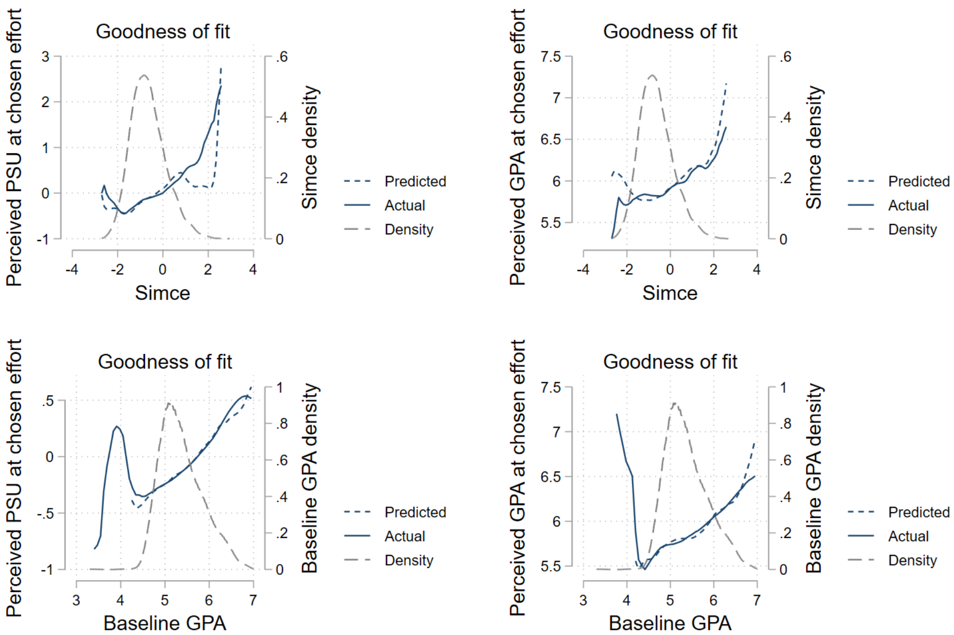


Figure G2: Goodness of fit of perceived PSU and GPA production functions at exerted effort levels. This figure shows the fit of the perceived production functions for PSU and GPA from equations (2) and (17). Predicted outcomes are constructed as in equations (23) and (24). Actual outcomes are obtained from the survey.

G.4.2 Identification of remaining parameters estimated through Indirect Inference

Perceived admission likelihoods. The perceived probability of regular admission, given in equation (30), is a function of perceived PSU and enters the decision to take the entrance exam. Its parameters are identified by matching the constant and slope from an auxiliary regression of exam participation on perceived PSU among control group students, for whom exam-taking is not influenced by preferential admissions. In the treatment group, the perceived probability of PACE admission, specified in equation (4), depends on the perceived distance from the cutoff and affects effort choice. We identify its parameters by matching the overall treatment effect on effort and the interaction coefficient between treatment status and perceived cutoff distance.

These parameters cannot be estimated via Probit models of admissions without solving the model, because they govern students' beliefs about the likelihood of admission rather than the objective admission likelihood itself. Instead, we estimate them by finding the parameter values that allow the model to match observed choices made under those beliefs.

Effort cost and effort measurement error. The coefficient on effort squared, ξ_2, is identified from the slope of study hours with respect to the baseline SIMCE test score. In the model, the marginal return to effort in the continuation value at time 1 depends on SIMCE (via the perceived admission functions); hence, students with different SIMCE optimally choose different efforts. The flow utility of effort, however, is not a function of SIMCE. Given separate identification of the continuation-value parameters (from admission processes estimated outside of the model and college-enrollment moments discussed below), how study hours vary with SIMCE isolates how the marginal flow utility of effort varies with effort, which helps identify the curvature parameter ξ_2. Any remaining unconditional effort variance in the treatment group helps identify the measurement error on effort.

Shocks. The variances of the GPA and PSU shocks are identified from the dispersion in GPA and PSU scores among control group students.

Entrance-exam taking. The parameters c_0^S and c_1^S in equation (6) are identified from the fraction of control group students who take the entrance exam and the null treatment effect on exam participation. Since PACE increases the option value of taking the exam, the null effect identifies the offsetting increase in perceived cost (or, equivalently, reduction in perceived benefit) captured by c_1^S.

Enrollment preferences. The coefficient on $pgrad_i^b$ in equation (12), λ_0^G, is pinned down by the coefficient in a regression of enrollment on $pgrad_i$ among admitted students in the control group. The additional (dis)utility from preferential enrollment, δ, is pinned down by the fraction choosing preferential enrollment among those admitted through both channels.

G.4.3 Auxiliary regressions and targeted moments for Indirect Inference

The auxiliary regressions below generate moments that we target through indirect inference. The variables that shift latent type probabilities enter these moments in two ways. Gender and survey status enter selected auxiliary regressions as regressors, so their coefficients summarize heterogeneity in choices and outcomes associated with those variables. Baseline top-15 status is not used as a regressor; instead, we compute several moments separately in the full sample and in the baseline top-15 subsample, allowing heterogeneity by baseline rank to discipline the latent-type distribution. For each candidate structural parameter vector, we solve the model, simulate choices and outcomes, re-estimate the same auxiliary regressions and sample-split moments on the simulated data, and compare the simulated auxiliary moments to their empirical counterparts.

We summarize below the classes of auxiliary regressions used in estimation and indicate which auxiliary regression parameters are targeted. We then list additional targeted moments.

Class 1: Treatment-effect regressions. Letting i denote a student and s a school, we estimate

$$Y_{is} = \alpha + \tau \, \mathrm{T}_{is} + X_{is}'\gamma + u_{is}, \tag{25}$$

with common controls X_{is} (model initial conditions, listed in Section 5.3), and where T_{is} denotes the treatment status. The targeted parameter is the treatment coefficient τ. We estimate this regression for $Y_{is} \in \{\text{admission}, \text{enrollment in year 1}, \text{enrollment in year 5}\}$ in both the full sample and the baseline top-15% subsample, and for $Y_{is} \in \{\text{weekly study hours}, \text{entrance-exam taking}\}$ in the full sample only. Moreover, we estimate this regression for fifth-year enrollment in the subsample of students who enrolled in the first year and were in the top 15% of their school at baseline.

In addition, we estimate a specification that allows the treatment effect on study effort to vary with students' perceived distance from the top-15-percent cutoff. Specifically, we estimate

$$Y_{is} = \alpha + \tau \, \mathrm{T}_{is} + \delta D_i + \kappa \, (\mathrm{T}_{is} \times D_i) + X_{is}'\gamma + (X_{is} \times D_i)'\eta + u_{is}, \tag{26}$$

where Y_{is} is weekly study hours and D_i is the perceived distance from the top-15-percent cutoff. The targeted auxiliary parameter is the interaction coefficient κ.

Class 2: Descriptive regressions. We estimate descriptive regressions of the form

$$Y_{is} = \alpha + X_{is}'\beta + \epsilon_{is}, \tag{27}$$

where i denotes the student and s the school. Outcomes Y_{is}, pre-determined regressors X_{is}, estimation samples, and targeted parameters are summarized in Table G2.

Table G2: Targeted parameters from descriptive regressions

Outcome Y_{is}	Sample	Pre-determined regressors X_{is}	Coefficients targeted
Entrance-exam taking	Full sample	Model initial conditions	Female, Missing survey
Weekly study hours	Full sample	Model initial conditions	Female
Weekly study hours	Full sample	Simce	Simce
First-year enrollment	Control group	Type-probability variables	Female, Missing survey, Constant
Admissions	Control group	Female, Missing survey	Female, Missing survey
Regular admission	Control group	GPA (9–10), Simce, Type vars	GPA (9–10), Simce
GPA	Full sample	Type vars, Model initial conditions	Female, Missing survey
Fifth-year enrollment	Full sample	Female, Missing survey, Simce	Female, Simce
First-year enrollment	Admitted, control group	Perceived graduation likelihood	Perceived graduation likelihood

Class 3: Relationships between endogenous model outcomes. Letting Y_{is}^1 and Y_{is}^2 denote two endogenous model outcomes, we estimate regressions of the form:

$$Y_{is}^1 = \alpha + \beta Y_{is}^2 + X_{is}'\gamma + \epsilon_{is}, \tag{28}$$

where X_{is} denotes pre-determined regressors. Outcomes, samples, regressors, and targeted coefficients are summarized in Table G3.

Table G3: Targeted parameters from regressions linking endogenous model outcomes

Outcome Y_{is}^1	Sample	Endogenous regressor Y_{is}^2	Pre-determined regressors X_{is}	Coefficients targeted
Entrance-exam taking	Control group	Expected PSU score	—	Expected PSU score, constant
12th-grade GPA	Full sample	Weekly study hours	Model initial conditions	Weekly study hours, baseline GPA, Simce

Moments. In addition to the auxiliary regressions described above, we match a set of means and variances for key outcomes.

We target average admission rates in the control group, both overall and among students in the top 15% of their school at baseline. Analogously, we match the share of treated students receiving a PACE admission, overall and within the baseline top 15% subgroup.

We target average enrollment rates in the first and fifth years after high school for control-group students, overall and in the baseline top 15%. In addition, we match first-year enrollment conditional on admission among top-15% students, separately by treatment status. We further match the proportion of students admitted through both the regular and PACE channels who accept the PACE admission.

Finally, for pre-admission outcomes, we match mean weekly study hours in the control group and among female students; the share taking the entrance exam in the control group and among male students with non-missing surveys; mean 12th-grade GPA in the control group; and persistence in top-15% rank from baseline to endline, separately for control- and treatment-group students.

Finally, we target the variances of key outcomes: 12th-grade GPA and weekly study hours in the control group, and PSU scores among control-group exam takers.

G.4.4 Estimation algorithm for Indirect Inference

In a first step, we estimate a set of auxiliary model parameters and of moments 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 moments from the data and their counterparts from the simulated data.

At each parameter iteration θ, we simulate S datasets, where each simulation is a draw for the model shocks and student type. Following Eisenhauer, Heckman, and Mosso, 2015, we set $S = 20$. 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:

$$\hat{\theta} = \arg\min_{\theta} \left[\hat{\beta}(\theta) - \bar{\beta}\right]' W \left[\hat{\beta}(\theta) - \bar{\beta}\right] \tag{29}$$

where W is a positive definite weighting matrix. As in Gayle and Shephard (2019), we use a matrix whose main diagonal elements are proportional to the inverse of the variances of the auxiliary parameters estimated from the data, and whose other elements are zero.[8]

To find the minimum of the criterion function, we use a derivative-free optimization algorithm, the Improved Stochastic Ranking Evolution Strategy, which is suitable for nonlinearly-constrained global optimization (Runarsson and Yao, 2005).

G.5 Equilibrium of the Tournament Game in the Rational Expectations Counterfactual

In the counterfactual that debiases all students' beliefs, we must solve for the Bayesian Nash equilibrium of the tournament game that awards preferential seats in PACE schools. This is a multidimensional fixed-point problem notoriously difficult to solve. Some studies have simplified it by assuming a continuum of individuals who differ only along one dimension (Hopkins and Kornienko, 2004; Bodoh-Creed and Hickman, 2019; Cotton, Hickman, and Price, 2020). As these simplifications are inappropriate in a setting where the populations, schools, are limited in size, and where individuals differ in more than one dimension, we adopt the different approach of lowering the dimensionality of the problem by solving for an approximation to the Bayesian Nash Equilibrium. The intuition is that the strategies of others affect own payoffs only through the probability of a preferential admission. By positing a parametric approximation for this probability, we can solve for a fixed point in its parameters, thus lowering the problem dimensionality.[9]

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 $(\epsilon_{it}^P, \epsilon_{it}^G)$.

[8]Specifically, we increase the weights of the treatment effects on admissions, enrollment, persistence, study effort, and entrance-exam-taking.

[9]We thank Nikita Roketskiy for suggesting this approach. All errors are our own.

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. Given an equilibrium profile of strategies for students $-i$, $e_{-i}^*(\cdot)$, each effort choice of student i maps into the expected probability of a preferential admission for student i: $P_i^{15}(e_i, e_{-i}^*(\cdot))$, where the expectation is taken with respect to others' types. It is only through this probability that the strategies of others enter own payoffs. We posit a parametric approximation to this probability, $\check{P}^{15}(e_i, \gamma)$, where γ captures the strategy profiles of students $-i$. Let $\check{u}_i(e_i(\cdot), k_i, \check{P}^{15}(e_i, \gamma))$ denote i's approximated expected payoff.

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

- *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 average GPA in the four high school years, 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 on treated schools, estimate a linear probability model of the likelihood of being in the top 15% in terms of high school GPA as a function of own high school GPA and of baseline characteristics of the student (X_i) and of the school (Z_j) selected through LASSO:
$$Top15_i = \gamma_0 + \gamma_1 GPA_{it} + \gamma_2 X_i + \gamma_3 Z_j + \epsilon_{ij}$$
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 for all schools j: $\gamma_{1j}^{(s=0)}$. The goal is to find a fixed point in γ_{1j}.
3. At the current iteration s, let students believe that the probability of being in the top 15% of the school is:
$$P_i^{15(s)}(e_i, \check{e}_{-i}(\cdot)) = \hat{\gamma}_0 + \gamma_{1j}^{(s)} GPA_{it}(e_i; \epsilon_{it}^G) + \hat{\gamma}_2 X_i + \hat{\gamma}_3 Z_j.$$
4. Given these beliefs, find the best response of each student by solving the dynamic programming problem. Let $e_{it}^{(s)}$ be the utility-maximizing effort that each student exerts.
5. Calculate $GPA_{it}^{(s)} = GPA(e_{it}^{(s)}; \epsilon_{it}^G)$ for each student, and simulate a dummy for whether each student's GPA is in the top 15% of their school (Sim_Top15_i).
6. From the simulated data on top 15% placements and $GPA(e_{it}^{(s)}; \epsilon_{it}^G)$, compute $\gamma_{1j}^{(s+1)}$ by OLS:
$$Sim\_Top15_i - \hat{\gamma}_0 - \hat{\gamma}_2 X_i - \hat{\gamma}_3 Z_j = \gamma_{1j}^{(s+1)} GPA_{it}^{(s)} + \eta_{ij}^{(s)}$$
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 $\gamma_{1j}^{(s)}$ and found a unique fixed point in each school.

H Additional Details and Results

H.1 Microfoundation of perceveid admission equations

The subjective probability of regular admission conditional on taking the entrance exam is equal to the subjective probability that a student's believed score will be above the believed admission cutoff, over which the student forms a subjective probability distribution: $c_i^{Rb} \sim N(\bar{c}^{Rb}, \sigma^2_{c^{Rb}})$. Letting A_i^R denote a dummy for regular admission, the subjective probability of regular admission is:

$$\begin{aligned} Pr^b(A_i^R = 1|\overline{PSU}_i^b) &= Pr\left(\overline{PSU}_i^b + \epsilon_i^{Pb} \geq \bar{c}^{Rb} + \epsilon_i^{c^{Rb}}\right) \\ &= \Phi\left(\gamma_0^b + \gamma_1^b \overline{PSU}_i^b\right), \end{aligned} \tag{30}$$

where $\gamma_0^b = \frac{-\bar{c}^{Rb}}{\sqrt{\sigma^2_{PSU^b} + \sigma^2_{c^{Rb}}}}$ and $\gamma_1^b = \frac{1}{\sqrt{\sigma^2_{PSU^b} + \sigma^2_{c^{Rb}}}}$ and $\Phi(\cdot)$ is the standard Normal cumulative distribution function. Given an expected PSU score, uncertainty about admission is generated by uncertainty around own score ($\sigma^2_{PSU^b}$) and the admission cutoff ($\sigma^2_{c^{Rb}}$), which are absorbed by the parameters γ_0^b and γ_1^b.[10]

In treated schools, the subjective probability of preferential admission conditional on taking the entrance exam is equal to the subjective probability that a student's believed average GPA in the four high school years will be above the believed preferential admission cutoff, which is the 85^{th} percentile of high school GPA in the school. Students form a subjective probability distribution over the top 15% cutoff in their school; we allow the mean of this distribution to vary across students: $c_i^{15b} \sim N(\bar{c}_i^{15b}, \sigma^2_{c^{15b}})$. The subjective probability of preferential admission then is:

$$\begin{aligned} Pr^b(A_i^P = 1|\overline{GPA}_i^{(9-12),b}, \bar{c}_i^{15b}) &= Pr\left(\overline{GPA}_i^{(9-12),b} + \epsilon_i^{G^b} \geq c_0 + \bar{c}_i^{15b} + \epsilon_i^{c15b}\right) \\ &= \Phi\left(\pi_0^b + \pi_1^b(\overline{GPA}_i^{(9-12),b} - \bar{c}_i^{15b})\right), \end{aligned} \tag{31}$$

where $\pi_0 = \frac{-c_0}{\sqrt{\sigma^2_{GPA^b} + \sigma^2_{c15^b}}}$ and $\pi_1^b = \frac{1}{\sqrt{\sigma^2_{GPA^b} + \sigma^2_{c15^b}}}$.[11] Given an expected GPA and an expected cutoff, uncertainty about admission is generated by uncertainty around own GPA ($\sigma^2_{GPA^b}$) and the school cutoff ($\sigma^2_{c15^b}$), which are absorbed by parameter π_1^b.

[10]Several papers in the beliefs literature in Economics impose functional form restrictions on subjective probabilities (e.g. Delavande and Zafar, 2019; Kapor, Neilson, and Zimmerman, 2020). We impose normality.

[11]Parameter c_0 is a net adjustment to the GPA and the cutoff to capture the fact that the top 15% rule is based on an adjusted GPA measure, while the GPA and cutoff survey questions referred to unadjusted GPA to facilitate question comprehension.

H.2 Additional Results on Rational Expectations

We find that under rational expectations, students would have exerted substantially less effort, regardless of PACE (columns 2 and 3 of Table 10). The second column of Table 10 shows the average effects of assigning rational expectations to students in the control group. Compared to the rational expectations scenario, students exert substantially greater pre-college effort and are more likely to take the entrance exam when they hold over-optimistic beliefs about the returns to effort in securing a regular admission and the likelihood of persisting in selective colleges (first two rows). Therefore, it is an empirical question whether pairing PACE with belief-correcting interventions would avoid pre-college effort reductions, thus fostering persistence, or have opposite effects through discouragement.

H.3 College Entrants under Alternative Policies

We analyze how the hypothetical interventions in Section 7.3.2 would affect the composition of college entrants and their persistence—an outcome of interest to colleges. Figure H1 describes college entrants under the no-intervention scenario (control condition) and the counterfactual interventions. Selection on test scores improves under the intervention that corrects all beliefs (third bar in Panel B). However, despite this improved selection, this intervention still results in college entrants with lower persistence rates (Panel A), because of substantial reductions in pre-college effort (Panel C). Unlike full belief correction, information on the importance of pre-college effort does not improve the baseline test scores of college entrants (fourth bar in Panel B), nor their pre-college effort (Panel C)—since the highest-achieving students (who are most likely to be admitted) tend to believe persistence is very likely, this information does not change much their beliefs—which helps explain why it increases pre-college effort in high school on average but does not substantially improve longer-term policy impacts. These findings show that when interventions affecting pre-college effort influence college persistence, they do so through two channels: changing the composition of students who enter college and altering the extent of their preparation during high school.

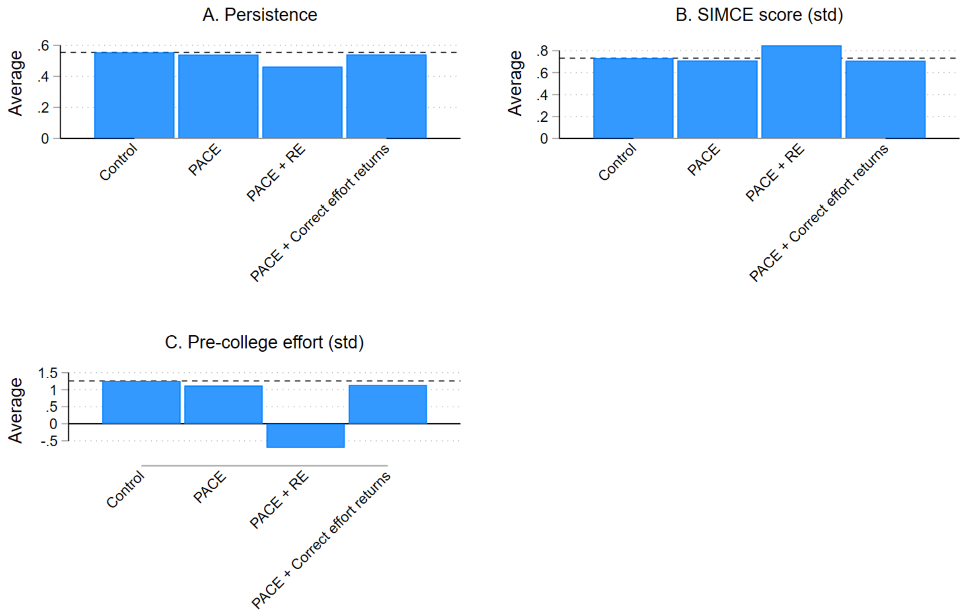


Figure H1: Selective college persistence and characteristics of selective college entrants. This figure shows average 5-year college-persistence rates, baseline test scores, and pre-college effort for college entrants in the control group–where no intervention is introduced–and under three hypothetical interventions. The control group mean is represented by the first bar and by the dashed horizontal line. The three interventions are PACE (Baseline), and the two counterfactual interventions described in section 7.3.2. SIMCE scores and pre-college effort are standardized to have mean zero and variance one in the control group.