Perceived Ability and School Choices: Experimental Evidence and Scale-up Effects*

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Abstract

This paper explores an information intervention designed and implemented within a school assignment mechanism in Mexico City. Through a randomized experiment, we show that providing a subset of applicants with feedback about their academic performance can enhance sorting by skill across high school tracks. We further integrate the experimental evaluation into an empirical model of schooling choice and outcomes to assess the impact of the intervention for the overall population of applicants. Feedback provision is shown to increase the efficiency of the student-school allocation, while congestion externalities are detrimental for the equity of downstream outcomes.

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1 Introduction

The increasing reliance on randomized evaluations in economics and related disciplines has been driven, in large part, by a desire to provide rigorous evidence to inform policymaking. Findings from field experiments have underpinned the adoption of more effective policies and programs by governments in a variety of settings (Banerjee and Duflo, 2011; Duflo, 2020). However, the ambition to translate experimental insights into large-scale policy interventions has often been hindered by a "scale-up problem," wherein treatment effects observed in controlled settings attenuate, or vanish altogether, when interventions are implemented at scale across more heterogeneous populations (Banerjee et al., 2017; Al-Ubaydli et al., 2020; Mobarak, 2022). Despite mounting evidence of this phenomenon across sectors including education, health, and private enterprise (see, e.g., Bold et al., 2018; Cameron et al., 2019; Araujo et al., 2021; List, 2022), the underlying mechanisms driving the negative correlation between the scale of a given initiative and the size of its impact remain poorly understood.

This paper represents an attempt to generalize the results from a randomized evaluation of an information intervention that provides students with individualized feedback about their academic skills. Education choices are made under uncertainty and rely on subjective expectations about present and future returns. Information provision may potentially resolve this source of subjective uncertainty. Yet, it is unclear to what extent students internalize signals that are informative for subsequent outcomes (Wiswall and Zafar, 2015a,b; Bobba and Frisancho, 2022). Perhaps even more fundamentally, informing a large number of agents in education markets is prone to generating a variety of spillover and equilibrium effects that may alter the inference drawn from small-scale studies (Heckman et al., 1998a,b).

Experimental evidence drawn from a subset of students demonstrates that the provision of performance feedback contributes to better aligning skills with high school tracks. This reallocation effect results in higher completion rates three years post-assignment. A model-based implementation of the same intervention at full scale shows that equilibrium effects would largely offset the positive impact on education outcomes. These findings offer novel insights on the channels through which cost-effective and ex-ante scalable policy solutions may fail to deliver the expected results when implemented at scale.

The setting of our empirical analysis is the secondary education market of the metropolitan area of Mexico City, in which a centralized clearinghouse coordinates admission to public high schools in the region. Close to 300,000 students apply every year to the system by submitting rank-ordered lists of their preferred high school programs during the last year of middle school. At the end of the school year, all applicants take a unique standardized admission test that determines priority in the assignment system and assesses curricular knowledge as well as verbal and analytical aptitude. The timing of the events, which is common across school/college assignment mechanisms in other countries, implies that high stake decisions regarding schooling and occupational trajectories may not incorporate relevant information about an applicant's academic skills. In our sample, over 80% of the students overestimate their performance in the test by the time they apply to the system.

We administer a mock version of the admission test among a socio-economically disadvantaged sample of students (N=2,493), and communicate individual score results to a randomly chosen sub-sample before the school rankings are submitted. Results from the experiment show that providing individual feedback on exam scores substantially shifts students' belief distributions regarding their own academic performance. We document relatively larger updates among lower performing students, who display wider gaps between the expected score and their actual performance in the mock exam. The performance feedback meaningfully influences sorting across high school tracks. Better performing (lower performing) students are more likely to get assigned into academic (non-academic) schools when compared to those in the control group.

The sorting patterns triggered by the intervention alter subsequent educational outcomes. Three years after school assignment, the probability of graduating from high school on time is, on average, 5.4 percentage points higher among under achieving students who received performance feedback. Although noisily estimated, this effect is sizable as it corresponds to a 13 percent increase when compared to the sample average in the control group. The observed gains in persistence throughout secondary education seem to be at least partly explained by an improved match between academic skills and schooling choices—for instance, lower-performing students do not systematically sort into easier-to-graduate schools as a result of the information intervention.

The score in the mock exam represents an informative signal about academic skills that is easy to replicate for the broader population of the applicants. However, a central challenge to scaling-up the randomized intervention in our setting is that extending performance feedback to all applicants would inevitably induce aggregate congestion and displacement effects within the centralized assignment system, potentially altering the equilibrium outcomes and the distributional implications of the policy. Using the experimental variation and data for all applicants we estimate a model of school choice to predict the distribution of preferences over schooling alternatives under the status quo and the counterfactual scenario of feedback provision.

The status quo prediction, based on estimated preference parameters for students in the control group, replicates the key features observed in the broader applicant population. Specifically, it closely tracks both school placement outcomes—including for students outside the experimental data's support—and the equilibrium cutoff scores at the school level. The counterfactual simulation of the information intervention accounts for congestion effects arising from aggregate shifts in demand. On the supply side, responses are straightforward to model in this context: schools admit applicants strictly in order of priority until they reach their capacity limits.

The provision of performance feedback at scale enhances the ex-ante efficiency of the student-school allocation. While there are no changes in the average participation rate to the admission process, the share of students assigned to their most preferred option increases by nine percentage points, from 16 percent to 25 percent, under performance feedback. However, these aggregate patterns mask substantial heterogeneity in the demand-side responses across applicants. The bulk of the changes in the school choices between the status quo and the information intervention are concentrated among applicants who are socio-economically better off (high SES). These students increase their demand for academic schools and symmetrically decrease the demand for selective and prestigious (elite) schools as a result of the information intervention. The lower demand-side pressure on elite programs crowds in high-achieving and low SES applicants.

We link the out-of-sample predictions based on the choice model with a school value added framework featuring substantial heterogeneity across students. The model further allows for equilibrium changes in school-level peer composition to affect education outcomes. We leverage the key features of the assignment mechanism in order to minimize the bias arising from the non-random assignment of students across schools. By opting out from elite schools towards other academic schools, high-SES applicants would increase school completion rates up to 10 percentage points depending on their admission score. Conversely, because more low-SES applicants are attending elite schools under performance feedback, they are now performing worse in terms of high-school graduation. This is particularly the case for those with a relatively high admission score, who would be 4-7 percentage points less likely to complete upper secondary education on time.

The discrepancy between the results we obtain from the large-scale analysis and the small-scale experimental evaluation can be explained by a displacement effect across applicants, which ultimately hampers inequality in education outcomes. In this sense, successfully scaling up the intervention may require providing targeted signals that are informative about the expected probability of graduation, conditional on attending different high-school programs.

There is growing evidence that information interventions in educational settings can influence subjective beliefs and individual choices, although their specific effects depend heavily on context, implementation, and design (see, e.g., Lavecchia et al., 2016; Haaland et al., 2023). We contribute to this literature by examining how perceptions of one's own ability affect decisions in a setting where beliefs are closely tied to high-stakes choices. While prior work has explored how feedback on academic performance influences educational decisions and outcomes (Azmat et al., 2019; Dizon-Ross, 2019; Bergman, 2021), we focus on how these effects vary with the scale of the intervention.

Evidence on the equilibrium effects of large-scale information interventions remains limited. In the context of educational policy, Andrabi et al. (2017) evaluate a market-level experiment in Pakistani villages, showing that providing information on school quality and pricing can shift aggregate educational outcomes. Neilson et al. (2019) explore the small and large scale effects of an intervention in Chile that delivers personalized school information to parents. Both studies rely on modeling assumptions about the supply side of the education market, which are central to explaining the observed improvements in school quality. In contrast, our setting features a centralized school assignment mechanism, which greatly simplifies the simulation of market equilibrium (Agarwal and Somaini, 2020). This structure enables us to more directly unpack the mechanisms through which the information intervention operates at scale.

The standard analysis of treatment effects in randomized trials typically assumes that an individual's treatment assignment does not influence the potential outcomes of others—a condition known as the no-interference assumption (Fisher, 1935; Imbens and Rubin, 2015). In practice, researchers often address potential interference by clustering units at a higher level where spillovers or equilibrium effects are assumed to be absent (Hudgens and Halloran, 2008; Muralidharan and Niehaus, 2017; Baird et al., 2018; Egger et al., 2022; Banerjee et al., 2023; Muralidharan et al., 2023). However, in tightly integrated markets, it may be infeasible to divide the population into isolated sub-markets. This paper adopts an alternative approach, demonstrating the value of combining field experiments with model-based estimation methods in order to study the sources of interdependence in a single interconnected market (Low and Meghir, 2017; Todd and Wolpin, 2023).

2 Context, Experimental Design, and Data

In this section, we first describe the relevant features of the study setting. We next provide a few details on the design and implementation of the information intervention. We finally discuss the rich combination of administrative and survey datasets that we use throughout the empirical analysis.

2.1 Centralized School Assignment in Mexico City

Since 1996, a local commission (COMIPEMS, by its Spanish acronym) of 16 upper secondary public institutions, or colleges, has centralized high school admissions in Mexico City's metropolitan area by means of an assignment mechanism. In 2014, the year of our intervention, over 238,000 students were placed in 628 public high schools, accounting for approximately three-quarters of enrollments in the entire metropolitan area. The remaining portion of high school students sought enrollment in public schools with open admission (10 percent) or private schools (15 percent).

Students apply to the centralized high school assignment system during the penultimate term of ninth grade (i.e., the final year of middle school). Prior to registration, they receive an information booklet outlining the timeline of the application process, relevant instructions, and a list of available schools. The booklet also includes basic school characteristics and the cutoff scores—defined as the admission exam scores of the lowest-ranked admitted students—for each school option over the past three years.

Along with the registration form, students complete a socio-demographic survey and submit a ranked list of up to 20 preferred schools. At the end of the school year, all applicants sit for a standardized achievement test. Admissions priority is based on the total score in this test. The matching algorithm processes applicants in order of priority, assigning each student to their highest-ranked school choice that still has available seats. This structure—submitting school preferences before taking the admission test—is not unique to the COMIPEMS system. Similar timing features are observed in several other centralized assignment mechanisms that use strict priority rules, as well as in some decentralized systems.[1]

[1]For instance, in centralized settings, this approach is also used in Ghana (Ajayi, 2022), Kenya (Lucas and Mbiti, 2014), Barbados (Beuermann and Jackson, 2020), Trinidad and Tobago (Beuermann et al., 2022), and certain Chinese provinces (Chen and Kesten, 2017). In decentralized contexts, such as the United Kingdom, students apply to universities before receiving their A-level exam results. In the Mexican case, this timing is designed to provide education authorities with an early "ballpark estimate" of the number of seats needed from participating colleges for each assignment cycle.

Each applicant is matched with one school. Whenever a tie in the score occurs for the last available spot in a given school, members of the local commission agree on whether to admit all of the tied students, or none of them. Unplaced applicants can request admission to other schools with available seats after the allocation process is over or search for a seat in schools with open admissions outside the system. When an applicant is not satisfied with their placement, they can request admission to another school in the same way unplaced applicants do.[2] In practice, the matching algorithm performs well: among all applicants who graduate from middle school and take the admission exam, only 12.8% remain unplaced and 3.2% are admitted through the second round of the matching process.

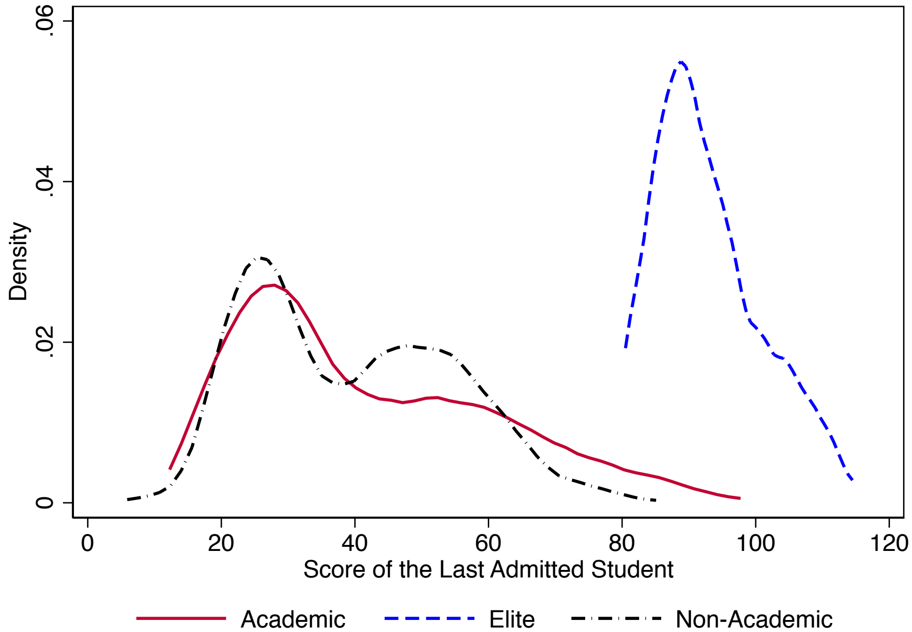
The Mexican system offers three educational tracks at the upper secondary level: General, Technical, and Vocational Education. Each of the 16 colleges within the assignment system offers a unique track. The general track is academically oriented and includes traditional schools that are more focused on preparing students for tertiary education. Technical schools cover most of the curriculum of general education programs, but they also provide additional courses allowing students to become technicians upon high school completion. The vocational track exclusively trains students to become technically adept.

A small sub-set of schools (32 out of 628) within the assignment system in Mexico City are affiliated with two higher education institutions (the National Polytechnic Institute and the National Autonomous University, IPN and UNAM by their Spanish acronyms), which are highly selective and prestigious universities, and as such the associated colleges are highly demanded. In what follows, we define UNAM- and IPN-sponsored high school programs as 'elite schools'. All the non-elite general track schools are considered 'academic schools' while the remaining technical and vocational programs are 'non-academic schools'.

Figure 1 depicts the distribution of cutoff scores across the three main types of high-schools, or tracks. Academic schools are, on average, slightly more selective than non-academic schools, but there is a large overlap across these two tracks. Some non-academic schools have gained popularity in the system due to their reputation in placing graduates in vocationally related occupations, which explains their relatively high cutoff scores. Elite schools clearly stand out in terms of selectivity.

[2]The assignment system discourages applicants from remaining unplaced and/or to list schools that they will ultimately not enroll in; specifically, participating in the second round will almost certainly imply being placed in a school that is not included in the student's original ranking.

Figure 1: Distribution of Cutoff Scores



Note: The cutoff score for each high school program refers to the lowest score in the admission exam of the students accepted there in the 2014 assignment process. 'Academic' schools are defined as the high school programs in the general track, 'Non-academic' schools are those in the technical and vocational tracks, and 'Elite' schools are affiliated with two higher education institutions (the National Polytechnic Institute and the National Autonomous University, IPN and UNAM by their Spanish acronyms).

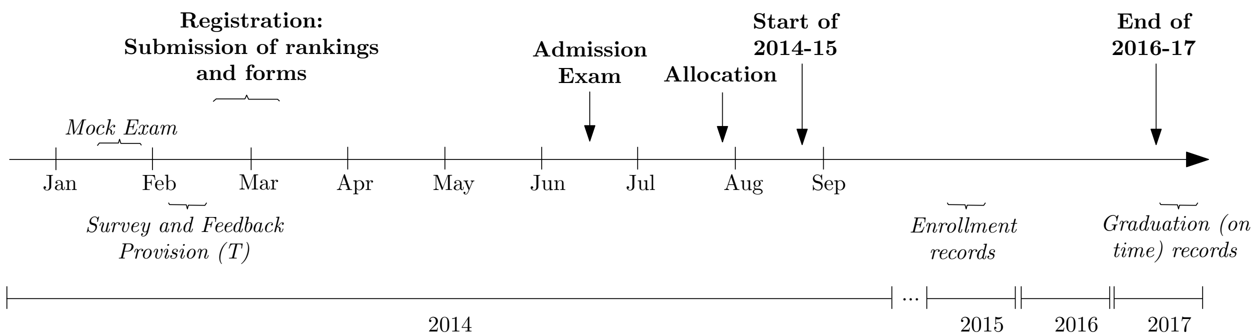
2.2 The Information Intervention

During the second half of the 2013-14 academic year, we implemented a mock exam in 90 middle schools (see Section 2.3). One or two weeks later, and just before the submission of the school rankings, we implemented a survey in those schools during which enumerators provided students with individual feedback on their performance in the mock exam. The delivery of the test scores took place in a setting secluded from other students or school staff in order to avoid reporting biases due to the influence of peers and/or social image concerns (Burks et al., 2013; Ewers and Zimmermann, 2015). Surveyors showed each student a personalized graph with two pre-printed bars: the average score in the universe of applicants during the 2013 edition of the school assignment mechanism and the average mock exam score in the student's class. Surveyors plotted a third bar corresponding to the student's score in the mock exam.[3] Figure 2 depicts the timing of the activities related to the intervention.

The mock exam was designed by the same institution responsible for the official admission

[3]Figure A.1 in the Appendix depicts a typical sheet of the performance information that is handed out to the students in the experimental sample. Both pre-printed bars served the purpose of providing the students with additional elements to better frame their own scores.

Figure 2: Timeline of Events



exam, in order to mirror the latter in terms of structure, content, level of difficulty, and duration (three hours). The test is comprised of 128 multiple-choice questions worth one point each, without negative marking, covering a wide range of subjects that correspond to the public middle school curriculum (Spanish, mathematics, social sciences and natural sciences) as well as mathematical and verbal aptitude sections.[4] We informed students, parents, and school principals about the benefits of additional practice for the admission exam. We also made sure that the school principal was able to assign the person who is usually in charge of the academic discipline and/or a teacher to proctor the exam, alongside the survey enumerators.

We argue that the score in the mock exam was easy to interpret for the applicants in the assignment mechanism while providing additional and relevant information about their own academic skills.[5] The linear correlation in our sample between performance in the mock exam and the actual exam is 0.82. In turn, the linear correlation between a freely available proxy of academic readiness, such as the middle school GPA, and the mock exam score is only 0.48 (see Figure B.2 in the Appendix). Both the scores in the admission exam and in

[4]Since the mock exam took place before the end of the school year, 13 questions related to curricular content that was not yet covered were not graded. We normalize the raw scores obtained in the 115 valid questions to the 128-point scale.

[5]In order to support the notion that students took the mock exam seriously, we look at the pattern of skipped questions (Akyol et al., 2021). Without negative marking, the expected value of guessing is always higher than leaving a question blank, which implies that students have no incentive to skip a question. Indeed, the average number of skipped questions in the mock exam was only 1.4 out of 128, and more than 80 percent of the students did not leave any question unanswered. Figure B.1 in the Appendix shows that the average patterns of skipping questions are more consistent with binding time constraints, rather than a lack of effort exerted in test taking. Furthermore, we do not find differential skipping patterns according to either the score in the admission exam or individual traits linked to effort and persistence.

the mock exam are strong predictors of later academic success.[6]

2.3 Sample Selection and Randomization

To select the experimental sample, we focus on middle schools with (i) a considerable mass of applicants, more than 30, in the 2012-2013 round of the centralized mechanism and (ii) that are located in neighborhoods with high or very high poverty levels (CONEVAL, 2018). The latter criterion was largely influenced by the literature showing that less privileged students tend to be relatively more misinformed when making educational choices (Avery and Hoxby, 2012; Hastings and Weinstein, 2008; Jensen, 2010). In our context, 44 percent of the applicants enrolled in schools from more affluent neighborhoods took preparatory courses for the admission exam before submitting their school rankings. This figure drops to 12 percent among applicants from schools in high poverty areas.

Schools that comply with our sample selection criteria are stratified by region and performance terciles. We group them into four geographic regions and terciles of school-average math test scores among ninth graders (see Section 2.4). Treatment assignment is randomized within strata at the school level. As a result, 44 schools are assigned to a treatment group in which we administer the mock exam and provide face-to-face feedback on performance, while 46 schools are assigned to the control group in which we only administer the mock exam. Within each school, we randomly select one ninth grade classroom to participate in the experiment. Since the provision of feedback about test performance took place during the survey, it cannot induce differential attrition patterns.

The match rate between the survey and the application records is 88 percent (2,828 students). As shown in Appendix Table B.2, the participation in the assignment system is balanced between the treatment and the control group. The experimental sample comprises the 2,493 applicants who were eligible for assignment through the matching algorithm.[7] Table 1 shows that the experimental sample is largely comparable to the general population of applicants in terms of academic credentials, such as GPA or college aspirations. However, the average applicant in our sample scores 4-points less in the admission exam than the average applicant in the universe (0.2 standard deviations). Consistent with our focus on relatively disadvantaged students, the applicants in the experimental sample are less likely

[6] As shown in Table B.1 of the Appendix, a one-standard-deviation increase in the mock exam score is associated with a 7.2 percentage-point increase (p-value=0.001) in the probability of graduating from high school on time.

[7] Table B.3 in the Appendix provides basic descriptive statistics and a balancing test of the randomization for various applicants' characteristics. Mean differences are very small in magnitude, with no significant discrepancies in any of the covariates detected across the treatment group and the control group.

Table 1: Applicants' Characteristics in the Population and in the Sample

	All Applicants Mean (Std. Dev)	Experiment Mean (Std. Dev)	All-Experiment Mean Difference [p-value]
Grade Point Average in middle school (GPA)	8.058	8.119	-0.061
	(0.871)	(0.846)	[0.001]
Has some disabilities (1=yes)	0.118	0.145	-0.027
	(0.323)	(0.352)	[0.000]
Scholarship in middle school (1=yes)	0.116	0.110	0.006
	(0.320)	(0.313)	[0.401]
Indigenous	0.041	0.093	-0.052
	(0.198)	(0.290)	[0.000]
Plans to attend higher education (1=yes)	0.662	0.670	-0.008
	(0.473)	(0.470)	[0.378]
Admission exam score	69.506	65.400	4.107
	(20.705)	(19.401)	[0.000]
One parent with at least tertiary education (1=yes)	0.236	0.147	0.089
	(0.425)	(0.354)	[0.000]
Average math score in middle school (z-score)	0.000	-0.208	0.208
	(1.000)	(0.712)	[0.000]
Neighborhood SES index (z-score)	0.000	-1.504	1.504
	(1.000)	(0.494)	[0.000]
Observations	284,412	2,493	

NOTE: The first two columns report means and standard deviation (in parentheses) of individual characteristics between the overall population of applicants and the experimental sample. The third columns displays mean differences and the associated p-values (in brackets) for the null hypothesis of equal means. The observations in the first column comprise all the applicants in the year 2014 who were eligible to be assigned through the matching algorithm. The observations in the second column comprise the experimental sample of the randomized information intervention.

to have parents with tertiary education, they attend middle schools with lower performing students, and reside in poorer neighborhoods.

2.4 Data and Measurement

Our analysis draws on several data sources. First, we have access to administrative data on different cohorts of applicants for several rounds of the assignment mechanism. These records include socio-demographic variables, such as gender, age, and parental education, among others. They also contain information on school preference rankings, admission exam scores, and placement outcomes. We link this dataset with the school-average math test scores in the national standardized examination (ENLACE) applied in ninth grade.

Second, we collect detailed survey data with information on the subjective distribution of beliefs about performance in the admission exam for the students in the experimental sample. In order to help students understand probabilistic concepts, the survey relies on visual aids (Delavande et al., 2011). We explicitly link the number of beans placed in a cup to a probability measure, where zero beans means that the student assigns zero probability to a given event and 20 beans means that the student believes the event will occur with certainty. Students are provided with a card divided into six discrete intervals of the score. Surveyors then elicit students' subjective expectations about test performance by asking them to allocate the 20 beans across the intervals to represent the chances of scoring in each bin. Appendix A provides more details on the elicitation of the individual data on beliefs in our setting. There are a few students with missing values in the beliefs data (247 observations, or 10% of the sample), which implies an effective sample size of 2,246 applicants for the analysis presented in Section 3.2. The incidence of missing values is balanced between the treatment and the control group (coeff.=0.006, p-value=0.367).

Third, we assemble and harmonize longitudinal data on the schooling trajectories through upper secondary education for the students in the experimental sample. The resulting dataset allows us to measure high school enrollment, drop-out during the tenth grade, and graduation on time from high school (twelfth grade)—that is, three years after the assignment process where the experiment took place (2014). It is not possible to track those applicants who end up enrolling in schools outside the centralized system. About 80 percent of the applicants in the control group enroll by the next academic year in the high school program in which they were assigned through the centralized process. However, only 45 percent successfully graduate from high school after three years. These figures clearly reflect inadequate academic progress through upper secondary education, due to either school dropout or grade retention, both strong indicators of a mismatch between schooling careers and students' individual skills.

Fourth, we match the individual identifiers for all the applicants who participated in the centralized assignment system with their ENLACE scores in twelfth grade. This is a good proxy for the probability of graduating from almost any high school in the country, including private schools (Dustan et al., 2017; Estrada and Gignoux, 2017; Dustan, 2020).[8] As Appendix Table B.4 shows, more than three quarters of the students who eventually com-

[8]The UNAM's representatives opted for not administering the ENLACE exam to its students (16 high-school programs out of 628 participating schools in the centralized assignment system, see Section 2.1). The ENLACE test was discontinued in 2014, and so we construct the indicators for high-school graduation for the ninth graders in the 2010 cohort of applicants.

plete secondary education they do so in the statutory three-year period. This share is pretty much stable across high-school tracks. To the extent that we cannot track delayed graduation rates for the experimental sample, we use on-time graduation as our main outcome of interest throughout the empirical analysis.

3 Experimental Evidence

Providing information about individual performance in the mock exam potentially allows students to revise their beliefs and thereby make high-school track choices that are better aligned with their academic potential, which, in turn, may lead to better educational outcomes. In this section, we document the effect of the performance feedback on subjective expectations about academic performance, as well as realized outcomes regarding school placement and subsequent schooling trajectories.

3.1 Empirical Model

We consider linear regression models of the following form:

$$Y_{ij} = \alpha_0 + \alpha_1 T_j + \alpha_2 A_{ij} + \alpha_3 A_{ij} T_j + \boldsymbol{\delta' X_{ij}} + \epsilon_{ij}, \tag{1}$$

where Y_{ij} is an individual-level outcome (expected or realized) for student i in one of the 90 middle schools j of the experiment. The indicator variable T_j takes a value of one if the school is in the treatment group and hence its students receive performance feedback in the mock exam, and zero otherwise. The A_{ij} variable is a standardized index of academic achievement, which is obtained as the weighted average of the GPA in middle school, the score in the mock exam, and the score in the admission exam.[9] The vector $\boldsymbol{X_{ij}}$ contains a set of dummy variables that correspond to the randomization strata (location $\times$ school-average test score indicators), pre-determined characteristics (gender, type and day-shift of the school of origin, previous experience with practice exams providing feedback, aspirations to attend higher education, an index of personality traits, an index of parental characteristics, and a household asset index), as well as a set of indicator variables for whether each of the covariates has missing data (Zhao and Ding, 2024). Finally, ϵ_{ij} is an individual error term that is arbitrarily correlated within school j and i.i.d across schools.

[9] A GLS-weighting approach (Anderson, 2008) increases efficiency by ensuring that outcomes that are highly correlated with each other receive less weight, while outcomes that are uncorrelated and thus represent new information receive more weight.

The parameter α_1 measures the average treatment effect of receiving the performance feedback on the outcome Y_{ij}, while α_3 captures how students differentially respond to the feedback in terms of the achievement index, A_{ij}. This specification captures the fact that inaccurate beliefs about academic proficiency can shape the perceived value of attending a given high-school program. The performance feedback can potentially alter those beliefs as well as the slope of students' outcomes with respect to their actual academic readiness. We estimate the parameters of equation (1) by OLS. Given the relatively large array of hypotheses considered throughout the analysis, we complement the usual asymptotic inference by computing p-values that are adjusted for multiple hypothesis testing across different families of outcomes (List et al., 2019).[10]

3.2 Subjective Expectations about Test Performance

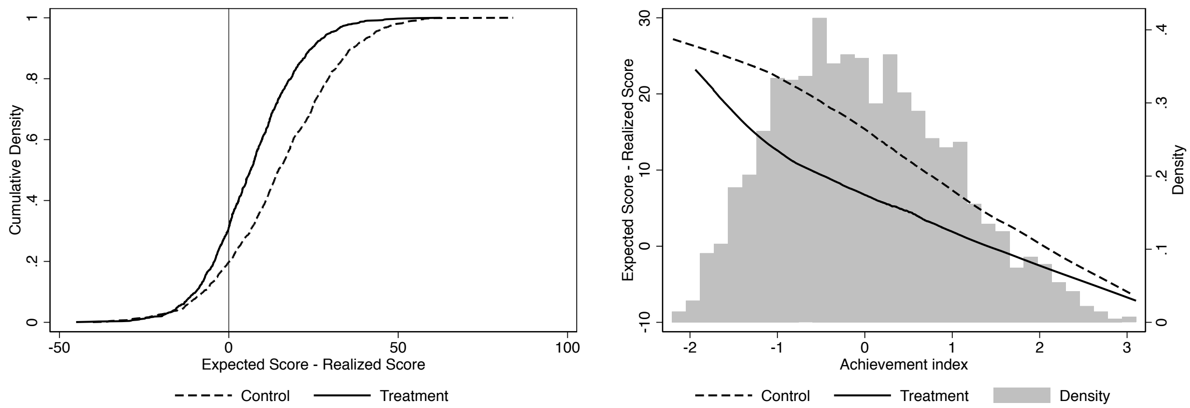
Panel A in Figure 3 displays the cumulative distributions of the perception gap, defined as the difference between the expected score and the realized performance in the mock exam, for students in the treatment and control groups.[11] In the control group, over 80% of the applicants overestimate their performance in the test. The performance feedback substantially shifts to the left the distribution of the perception gap, with an average gap of 6.5 points for treated applicants and 14.7 points for control applicants (out of a 128-point scale). Panel B in Figure 3 presents evidence on the relationship between the perception gap and our index of academic achievement for students in the treatment and control groups. Updates on the expected score in response to performance feedback occur along the entire distribution of the achievement index, with relatively larger gap reductions among lower performing students.

Table 2 shows the OLS estimates of the effect of the information intervention on different moments of the individual distribution of beliefs about test performance. The first column documents that providing feedback about test performance decreases the mean of the belief distributions by 6.9 points out of a sample average of 75.6 in the control group (p-value = 0.001). We find a similar effect when we alternatively consider the median of the individual

[10] The Romano-Wolf correction (Romano and Wolf, 2005a,b, 2016) asymptotically controls the family-wise error rate, that is, the probability of rejecting at least one true null hypothesis among a family of hypotheses under test. This correction is considerably more powerful than earlier multiple-testing procedures, given that it takes into account the dependence structure of the test statistics by re-sampling from the original data.

[11] Assuming a uniform distribution within each interval of the score, the expected scores are constructed as the summation over intervals of the product of the mid-point of the bin and the probability assigned by the student to that bin.

Figure 3: Gap between Expected Scores and Realized Scores in the Mock Exam



(a) Cumulative Density of the Gap

(b) Gap and Academic Achievement

NOTE: Panel A shows the cumulative density of the difference between the expected scores and the realized scores in the mock version of the admission exam. Panel B shows non-parametric locally weighted estimates of the relationship between the perception gap and the achievement index. For more details on the elicitation of beliefs in the survey data, refer to Appendix A.

belief distributions in the second column, with an 11% drop relative to the corresponding sample average in the control group (p-value = 0.001). The negative treatment effect on expected performance in the mock test is partly attenuated by the positive updating patterns among the highest performing students. An increase of one standard deviation in the achievement index corresponds to a right-shift in the location of the belief distribution by 2.9 (for the mean) and 3.1 points (for the median) in the treatment group relative to the control.

The estimates reported in the last two columns of Table 2 document that the performance feedback meaningfully decreases the uncertainty of students' predictions, with average reductions in the dispersion of the individual belief distributions of 2.8 points (p-value = 0.001). This corresponds to 11% (for the standard deviation) and 16% (or the inter-quartile range) of the sample average in the control group. We find limited evidence of heterogeneous updating on the second moment of the belief distributions by the value of the achievement index.

Overall, these results establish that providing information about individual performance in the mock exam allows applicants to substantially revise their expectations about academic readiness. The evidence underscores the informativeness of the performance feedback for all students in our sample. However, the magnitude and direction of the adjustment is strongly associated with the level of academic achievement.[12]

[12] In our companion paper (Bobba and Frisancho, 2022), we explore in further details the process of belief updating spurred by the performance feedback.

Table 2: Performance Feedback and Beliefs about Test Performance

	Mean	Median	Std. Dev.	IQR
Treatment	-6.935	-8.892	-2.773	-2.789
	[0.000]	[0.000]	[0.000]	[0.000]
	{0.001}	{0.001}	{0.001}	{0.001}
Achievement index	4.550	4.839	-0.621	-1.234
	[0.000]	[0.000]	[0.021]	[0.011]
	{0.001}	{0.001}	{0.010}	{0.007}
Treatment × Achievement index	2.908	3.109	-0.441	-0.875
	[0.000]	[0.000]	[0.232]	[0.193]
	{0.001}	{0.001}	{0.106}	{0.106}
Mean Control	75.6	78.8	17.4	24.2
Number of Observations	2246	2246	2246	2246
Number of Clusters	90	90	90	90
R-squared	0.289	0.282	0.082	0.057

NOTE: The dependent variable "Mean" is constructed as the summation of the mid-values in each discrete interval of the support multiplied by the associated probability assigned by the student. The dependent variable "Median" is defined as the midpoint of the interval in which the cumulative density first surpasses 0.5 (11/20 beans or more). The dependent variable "Std. Dev." is constructed as the square root of the summation of the mid-values in each discrete interval of the support multiplied by the square of the associated probability assigned by the student minus the square of the constructed mean. The dependent variable "Inter-Quantile Range (IQR)" is defined as the difference between the midpoints of the intervals that accumulate 75 percent and 25 percent of the probability mass. For more details on the elicitation of beliefs in the survey data, refer to Appendix A. The Achievement Index is a GLS-weighted average (Anderson, 2008) of the GPA in middle school, mock exam score, and exam score. *p*-values reported in brackets refer to the conventional asymptotic standard errors while those in curly brackets are adjusted for testing each null hypothesis across multiple outcomes through the step-wise procedure described in Romano and Wolf (2005a,b, 2016). Both inference procedures take into account the clustered structure of the individual error terms at the middle school level.

3.3 School Placement

Table 3 presents OLS estimates of the impact of the information intervention on the distribution of applicants across high school tracks. On average, the likelihood of assignment to non-academic programs increases by 4.6 percentage points, with a corresponding decline in academic placements. Although these average effects are not statistically significant (*p*-value = 0.11), we observe stronger evidence of a composition effect by the level of academic performance. Specifically, a one-standard-deviation increase in the performance index among treated students is associated with a 6.5 percentage-point reduction in the probability of being placed in a non-academic program—a 16 percent decrease relative to the control group mean. Estimates in the final column of Table 3 indicate that the intervention does not significantly affect the probability of assignment to elite schools.

Exposure to performance feedback does not systematically affect admission exam scores,

Table 3: Performance Feedback and Placement Outcomes

	Non-Academic	Academic	Elite
Treatment	0.046	-0.044	-0.002
	[0.077]	[0.078]	[0.861]
	{0.110}	{0.110}	{0.842}
Achievement index	-0.079	-0.086	0.165
	[0.000]	[0.000]	[0.000]
	{0.002}	{0.001}	{0.001}
Treatment × Achievement index	-0.065	0.041	0.024
	[0.015]	[0.045]	[0.247]
	{0.020}	{0.065}	{0.253}
Mean Control	0.453	0.418	0.129
Number of Observations	2493	2493	2493
Number of Clusters	90	90	90
R-squared	0.100	0.061	0.336

NOTE: The dependent variable is an indicator variable that is equal to one if the applicant is assigned to a given group of schools (i.e., Non-Academic, Academic, and Elite schools). The achievement index is a GLS-weighted average (Anderson, 2008) of the GPA in middle school, mock exam score, and exam score. p-values reported in brackets refer to the conventional asymptotic standard errors, while those reported in curly brackets are adjusted for testing each null hypothesis across multiple outcomes through the step-wise procedure, as described in Romano and Wolf (2005a,b, 2016). Both inference procedures take into account the clustered structure of the error terms at the middle school level.

or the likelihood of assignment in the matching process (see Appendix Tables B.2 and B.5). This suggests that the treatment effects on school placement are primarily driven by changes in applicants' school rankings, which are shown in Table B.6 in the Appendix. Overall, these results indicate that providing performance feedback meaningfully influences school placement within the assignment system. Higher-achieving students in the treatment group are more likely to be placed in academic—but not elite—programs.

3.4 Educational Trajectories

As shown in the previous sub-section, the provision of performance feedback likely improved sorting patterns across high-school tracks. This reallocation effect within the assignment mechanism may potentially alter students' academic trajectories and downstream educational outcomes.

The point estimates in the first two columns of Table 4 show no meaningful differences in high school enrollment rates or first-year dropout rates between students in the treatment and control groups. However, the results reported in the third column of Table 4 show that

Table 4: Performance Feedback and High School Outcomes

	Enrollment	Dropout 1st year	Graduation on Time
Treatment	-0.003	0.012	0.022
	[0.789]	[0.668]	[0.252]
	{0.936}	{0.920}	{0.497}
Achievement index	0.068	-0.095	0.138
	[0.000]	[0.001]	[0.000]
	{0.001}	{0.003}	{0.001}
Treatment × Achievement index	-0.021	-0.006	-0.032
	[0.352]	[0.807]	[0.088]
	{0.590}	{0.936}	{0.198}
Mean Control	0.813	0.248	0.447
Number of Observations	2493	2024	2358
R-squared	0.045	0.076	0.090

NOTE: The dependent variable "Enrollment" denotes an indicator variable that is equal to one if students enroll in the high school programs they were assigned to, and zero otherwise. The dependent variables "Dropout, 1st year" captures whether the student stopped attending classes or actively dropped out of school, conditional on enrollment. The dependent variable "Graduation on Time" denotes an indicator variable that is equal to one if the student successfully completes the high school programs three years after placement in tenth grade and zero otherwise. The achievement index is a GLS-weighted average (Anderson, 2008) of the GPA in middle school, mock exam score, and exam score. p-values reported in brackets refer to the conventional asymptotic standard errors, while those reported in curly brackets are adjusted for testing each null hypothesis across multiple outcomes through the step-wise procedure, as described in Romano and Wolf (2005a,b, 2016). Both inference procedures take into account the clustered structure of the error terms at the high school level.

the probability of graduating on time (unconditional on enrollment in tenth grade) is 5.4 percentage points higher for students who receive performance feedback and who score one-standard deviation below the mean (=2.2p.p+3.2p.p) when compared to equally achieving students who do not receive any feedback.[13] Importantly, as shown in Table B.8 in the Appendix, the observed gains in persistence throughout secondary education do not seem to be explained by the fact that lower-performing students tend to sort into easier-to-graduate schools as a result of the information intervention.

While statistically imprecise, the magnitude of this effect is economically significant, as it corresponds to a 13 percent increase in high-school graduation rates when compared to the sample mean in the control group of 0.45. The effect size on the rate of high-school

[13] We were unable to obtain the high-school graduation records for approximately 5% of the students in our sample, which explains the discrepancy in the number of observations between column 1 and column 3 of Table 4. The associated Lee bounds (Lee, 2009) are narrow and broadly consistent with the point estimates reported in the main text (see Appendix Table B.7).

graduation roughly coincides with the magnitude of the impact of a one-deviation increase of the score in the mock exam (see Appendix Table B.1).[14]

4 Scaling-up the Information Experiment

The evidence presented in the previous section suggests that providing students with information about their academic proficiency during the transition from lower to upper secondary education can improve the allocation of skills across high school tracks. In this section, we embed the randomized intervention within a discrete choice model of schooling decisions allowing us to extrapolate the impact of performance feedback beyond the experimental sample. We then use a school value added model to link the out-of-sample predictions about the student-school allocations with subsequent graduation outcomes.

4.1 Preferences Over School Characteristics

We model the indirect utility that student i gets from attending school j as:

$$u_{ij} = \alpha_{s(j)} + \boldsymbol{\beta'_{s(j)}} \boldsymbol{x_i} + \boldsymbol{\gamma'} \boldsymbol{x_i} d_{ij} + \boldsymbol{\rho'} \boldsymbol{x_i} c_j + \epsilon_{ij}, \tag{2}$$

where the composite term $\alpha_{s(j)} + \boldsymbol{\beta'_{s(j)}} \boldsymbol{x_i}$ denotes the net returns of attending a particular college s, i.e. a group of high-school programs j that share the same track (non-academic, academic, or elite) and that belong to the same public institution of upper secondary education.[15] The vector $\boldsymbol{x_i}$ contains an array of standardized individual characteristics, which broadly capture skill measures and demographics (observed or unobserved to the applicant), such as the score in the mock exam, the cumulative GPA in middle school, an index of socio-economic conditions in the neighborhood of residence of the applicants, the average ENLACE math score in the students' middle school of origin, and parental education. The same vector $\boldsymbol{x_i}$ of individual characteristics is also interacted with the geodesic distance d_{ij}

[14] Figure B.3 in the Appendix visually displays the relationship between the rates of graduation on time from secondary education and the achievement index separately for the treatment and control groups. While there is a small effect of performance feedback along the entire distribution of academic achievement, its impact on schooling trajectories becomes more clearly visible around the left tail. Since under-achieving students also tend to have lower graduation rates, the information intervention effectively contributes to "leveling the playing field" in our setting.

[15] The size of the experimental sample is too small to precisely estimate the 600+ school-specific intercepts and the associated interaction terms. We thus group the high-school programs in the centralized system into 16 college-specific intercepts, $\alpha_{s(j)}$, and the associated student-college match effects, $\beta_{s(j)}$. By doing so, we substantially reduce the number of parameters that need to be estimated in equation (2).

(in kilometers) between the location of the middle school of applicant i and high-school program j as well as with the degree of selectivity of each high-school program c_j, which we measure through the admission cutoff score in the previous round of the assignment mechanism.

The vector of parameters $\boldsymbol{\gamma}$ captures the average commuting cost of attending a particular high school program while accounting for potential heterogeneity across applicants. Analogously, $\boldsymbol{\rho}$ embeds any utility cost or benefit associated to being assigned to schools with a given level of peers' quality and/or academic requirements. Tuition fees are negligible in this setting and they do not vary between schools in the same college s, so that the small differences in the out-of-pocket expenses across high-school programs are captured by the $\alpha_{s(j)}$ parameters.

The preference shock, ϵ_{ij}, is assumed to be i.i.d. across i and j, following a type-I extreme value distribution with normalized scale and location. Conditional on $\boldsymbol{x_i}$, d_{ij} is assumed orthogonal to ϵ_{ij}. This assumption is usually invoked in the school choice literature (see, e.g., Agarwal and Somaini, 2020). It is violated if students systematically reside near the schools for which they have idiosyncratic tastes. This assumption becomes plausible in our case as we have rich micro-data on students. In addition, priorities in the school assignment mechanism do not depend on student locations, thereby alleviating issues related to residential sorting.

Since discrete choice models depend on differences in payoffs, we normalize the deterministic part of the utility of not being assigned to any school program within the assignment system to zero. This outside option captures the value of not attending high-school, or the value of any other labor market entry opportunity not directly observed in the data.

4.2 Estimating Preferences

We have access to individual-level data on rank-ordered lists and placement outcomes. Both sources of information are potentially valuable for estimating preference parameters. However, school rankings may deviate from true preference orderings due to the cap of 20 schools in the submitted rank-ordered lists (Haeringer and Klijn, 2009; Calsamiglia et al., 2010) or possible strategic mistakes in applications (Hassidim et al., 2017; Artemov et al., 2023).

A more robust estimation approach relies on the assumption that the realized matching equilibrium is stable, which is likely satisfied in the large-market matching mechanism that we study. Under stability, the observed match between an applicant and a given school can be interpreted as the outcome of a discrete choice model with individual-specific choice sets

(Fack et al., 2019). These choice sets solely depend on the scores in the admission exam for most programs.[16]

The parameters of the indirect utility function in (2) are estimated by maximum likelihood separately for the treatment and the control samples. This strategy allows for the possibility that feedback provision can alter the choice environment in which applicants operate.

Table 5 shows the selected estimates of preference parameters over high-school tracks and programs' selectivity that are re-scaled by the disutility of the distance (or willingness to travel). The estimated distribution of preferences over school characteristics differ somewhat between the applicants who received the performance feedback and those in the control group, as shown in the third column of the table. For instance, higher-SES students (i.e. those who reside in more socio-economically advantaged neighborhoods and/or who attended better middle schools) who receive the performance feedback attach a more negative value to an elite school. The size of the estimated effect for a one-standard-deviation increase in the neighborhood SES index is equivalent to commuting to a school that is 8.5 km away when compared to the applicants at the mean of the SES distribution. The corresponding commuting cost for the applicants in the control group is 1.5 km, which is not statistically different from zero.

Instead, the willingness to travel attached to the degree of selectivity of the schools—as measured by the cutoff scores from the previous year—shows a positive gradient with respect to both academic achievement and SES, which is steeper for the applicants in the treatment group when compared to those in the control group.

The positive and negative gradients discussed above tend to offset each others for treated students' valuations, thereby possibly explaining the lack of both average and heterogeneous effects on assignment into elite schools, as shown in the third column of Table 3. Taken together, these patterns of heterogeneity in the estimated preference distributions within the the experimental sample shape the sorting and displacement effects of the intervention at scale that we later document in Section 5.

4.3 Out-of-Sample Predictions

Table B.9 in the Appendix displays the full set of estimated parameters for the more flexible specification at the college-level, as depicted in equation (2). We use these estimates to

[16]Elite schools further impose a GPA requirement of at least 7 out of 10 points. Most of the applicants to those high school programs meet this requirement (more than 90 percent in every round of the assignment system).

Table 5: Willingness to Travel for School Characteristics

	Control Sample WTT Est (Std. Err.)	Treated Sample WTT Est (Std. Err.)	Control-Treated T-test [p-value]
$[\text{Elite}]_j \times [\text{Mock Score}]_i$	-0.2144	-2.3344	1.0516
	(1.3191)	(1.5246)	[0.2931]
$[\text{Elite}]_j \times [\text{SES Index}]_i$	-1.5265	-8.4959	2.0903
	(1.8019)	(2.8053)	[0.0367]
$[\text{Elite}]_j \times [\text{Middle-School Math Score}]_i$	0.7386	-2.9515	1.7464
	(1.2932)	(1.6710)	[0.0809]
$[\text{Academic}]_j \times [\text{Mock Score}]_i$	0.6327	-1.4862	2.0833
	(0.6604)	(0.7735)	[0.0373]
$[\text{Academic}]_j \times [\text{SES Index}]_i$	-0.8522	0.2950	-0.6901
	(0.9252)	(1.3809)	[0.4902]
$[\text{Academic}]_j \times [\text{Middle-School Math Score}]_i$	-0.0962	0.2064	-0.2664
	(0.6732)	(0.9144)	[0.7899]
$[\text{Non-Academic}]_j \times [\text{Mock Score}]_i$	1.1555	-1.1870	2.3631
	(0.6517)	(0.7470)	[0.0182]
$[\text{Non-Academic}]_j \times [\text{SES Index}]_i$	-0.2780	-1.9743	-1.0007
	(0.9432)	(1.4085)	[0.3171]
$[\text{Non-Academic}]_j \times [\text{Middle-School Score}]_i$	0.2538	-0.4473	0.6388
	(0.6673)	(0.8713)	[0.5230]
$[\text{Cutoff Score}]_j \times [\text{Mock Score}]_i$	0.2707	0.6569	-1.0162
	(0.2378)	(0.2965)	[0.3097]
$[\text{Cutoff Score}]_j \times [\text{SES Index}]_i$	0.7705	1.0730	-0.4850
	(0.3479)	(0.5175)	[0.6277]
$[\text{Cutoff Score}]_j \times [\text{Middle-School Math Score}]_i$	0.1628	1.2866	-2.3816
	(0.2887)	(0.3733)	[0.0173]

NOTE: This table displays maximum-likelihood estimates that are normalized by the distance coefficient for selected match coefficients of equation (2). Standard errors reported in parenthesis are computed using the delta method. The third column displays the t-statistics and the associated p-values (in brackets) for the null hypotheses of equal coefficients between the control and the treated samples. The full set of model estimates at the college-level is reported in Table B.9.

predict the indirect utilities for the universe of applicants in the centralized assignment system. Since the students outside of the experimental sample do not take our mock exam, we replace that covariate in the vector $\boldsymbol{x_i}$ with the admission exam score. We run the Serial Dictatorship algorithm that is in place in the assignment system relying on the priority criteria and school capacities. School preferences are vertical (i.e., school programs simply accept or reject prospective applicants in descending order based on their exam scores until seat capacities are met), hence this algorithm delivers the unique stable matching equilibrium

Table 6: Model Fit on Average Assignment Outcomes by SES Categories

	Very Low SES		Low SES		Middle SES		High SES	
	Data	Model	Data	Model	Data	Model	Data	Model
Applied in the system (1=yes)	1.00	0.97	1.00	0.99	1.00	0.99	1.00	1.00
Assigned in the system (1=yes)	0.91	0.91	0.88	0.92	0.86	0.93	0.84	0.94
Non-Academic schools, vocational track	0.16	0.18	0.14	0.13	0.13	0.10	0.10	0.08
Non-Academic schools, technical track	0.30	0.27	0.27	0.27	0.25	0.25	0.22	0.23
Academic, above-median selectivity	0.23	0.20	0.30	0.28	0.30	0.31	0.32	0.33
Academic, below-median selectivity	0.17	0.21	0.09	0.13	0.04	0.08	0.03	0.04
Elite schools	0.13	0.14	0.20	0.18	0.28	0.26	0.34	0.32
Selectivity (z-cutoff score)	0.32	0.24	0.65	0.56	0.96	0.90	1.21	1.15

NOTE: The averages displayed in the odd columnd are computed from the data of the assignment mechanism in the year 2014 (see Section 2). The averages displayed in the even columns are computed by running the Serial Dictatorship algorithm that is in place for the COMIPEMS system, using the estimated preferences of the control group (see Table B.9 in the Appendix), the individual scores in the admission exam, and the school capacities as inputs.

allocation (Roth and Sotomayor, 1992).[17]

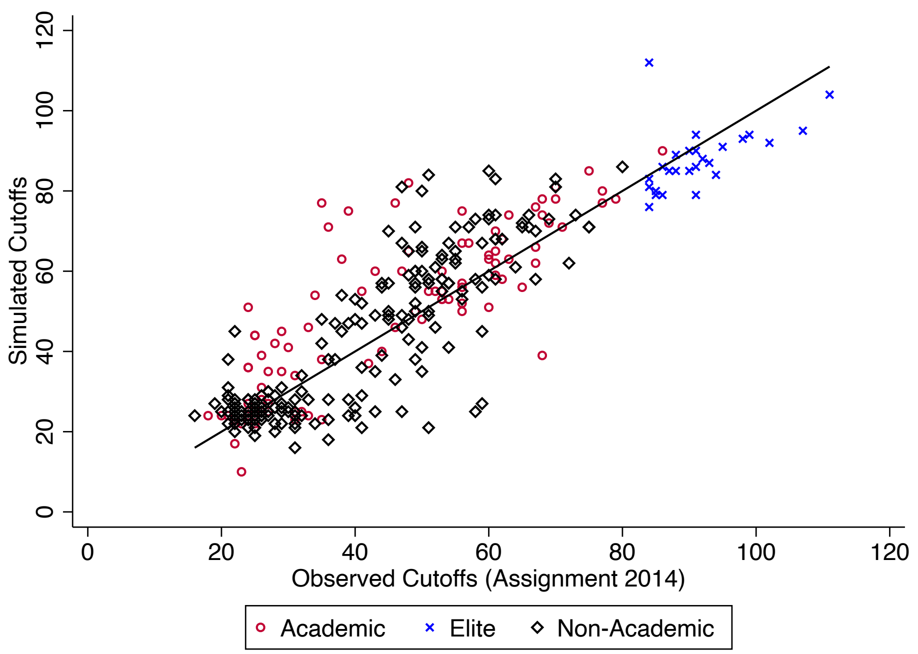
In Table 6 we compare the average outcomes of the school assignment system with those generated through the matching equilibrium using the estimated preferences of the applicants in the control group. Mean-differences are very small for the outcomes considered across the entire SES distribution. This result was not guaranteed *a priori*, given the fact that the experiment is targeted toward applicants from relatively disadvantaged backgrounds (see Section 2.3).

Another way to assess the validity of the extrapolation is by looking at the equilibrium cutoff scores. The linear correlation between the observed cutoff scores and the model-based cutoff scores is 0.88. Figure 4 provides a scatter plot of the relationship between the cutoff scores in the model and in the data for the schools in the assignment mechanism. As expected, the fit of the model improves for more selective options with high cutoff scores—mostly elite schools, but also for a few academic and non-academic options. These schools are more likely to be oversubscribed, which implies that the associated cutoff scores are well-defined equilibrium objects under stable matching (Azevedo and Leshno, 2016; Fack et al., 2019).

This evidence broadly supports the validity of the out-of-sample predictions based on the estimated distribution of preferences for the experimental control group. Therefore, we postulate that the corresponding predictions based on the estimated preferences for the applicants in the treatment group likely approximate a counterfactual scenario in which the

[17]While we estimate the school choice model by assuming stable matching but not truth-telling (see Section 4.2), we can allow students to be truthful when studying matching outcomes. This holds as long as preference estimates are consistent (Artemov et al., 2023).

Figure 4: Model Fit on Cutoff Scores



NOTE: In this figure, we report the cutoff scores for the 429 school programs (68% of the total participating programs) that are contained in the choice sets of the applicants of the experimental sample. For an analogous chart with the cutoffs of all the 628 school programs, refer to Figure B.4 in the Appendix. The observed cutoffs are computed from the data of the assignment mechanism in the year 2014 (see Section 2). The simulated cutoff scores displayed in the scatter plot are computed by running the Serial Dictatorship algorithm that is in place for the COMIPEMS system using the estimated preferences of the control group (see Table B.9 in the Appendix), the individual scores in the admission exam, and the school capacities as inputs.

broader population of applicants would be given additional information about their academic skills. This is akin to implementing a policy that mandates the universal implementation of a mock exam or, alternatively, disclosing admission exam scores to the applicants before the submission of the rank-ordered lists (see Figure 2).

4.4 Linking Sorting across Schools with Education Outcomes

We consider a potential outcomes framework that maps any student-school match into educational outcomes. In particular, we posit that the potential outcome of student i if she is matched to school j can be written as:

$$Y_{ij} = \delta_{s(j)} + \boldsymbol{\gamma'_{s(j)}} \boldsymbol{x_i} + \boldsymbol{\lambda'} \boldsymbol{\bar{x}_j} + \nu_{ij}, \tag{3}$$

where, as before, $s(j)$ denotes a particular college (i.e., group of high-school programs). The vector $\boldsymbol{x_i}$ contains the same standardized individual characteristics of equation (2) except for the mock score (i.e. the cumulative GPA in middle school, an index of socio-economic conditions in the neighborhood of residence of the applicants, the middle school-average of ENLACE math test scores, and parental education). In this framework, $\delta_{s(j)}$ measures the average effect of college s, $\boldsymbol{\gamma_{s(j)}}$ corresponds to the vector of match effects for students with observed type $\boldsymbol{x_i}$ in college s, and ν_{ij} denotes any unobserved factor influencing education outcomes. We allow for equilibrium changes in peer composition to affect education outcomes by taking advantage of the fact that the parameters of the value added model (3) vary at the college level. Therefore, we can include the vector $\boldsymbol{\bar{x}_j}$ of average skills and demographics at the school level.

Students are not randomly assigned to schools or colleges. However, school placement under the assignment mechanism depends exclusively on two student-level observable factors: ranked-order lists (ROL) and the scores in the admission exam. We add ROL fixed effects in equation (3) in order to compare outcomes between students with the same school rankings and other observable covariates (Angrist and Rokkanen, 2015; Abdulkadiroglu et al., 2020).[18] We posit that the remaining variation in school placement is due to idiosyncratic differences in the score of the admission test (e.g., a good or a bad exam day) that are assumed to be uncorrelated with the error term ν_{ij}.

Table 7 reports OLS estimates for selected coefficients from the value-added model, aggregated at the track level, both without and with ROL fixed effects. The estimates differ somewhat across the two specifications, indicating that controlling for school rankings helps account for some of the unobserved factors influencing student allocation. On average, attending an elite high school program reduces the probability of on-time graduation by 18 percentage points, relative to both academic and non-academic programs, against a baseline on-time graduation rate of 41 percent following school assignment. Some of the interaction effects based on student skills and demographics are statistically significant, though they are notably smaller in magnitude than the average treatment effects. Appendix Table B.10 presents the full set of OLS estimates at the college level for our preferred specification with ROL fixed effects.

5 Counterfactual Simulations

In this section, we use the empirical framework introduced in Section 4 to assess the effects of a counterfactual, large-scale implementation of the information intervention. We begin

[18]Under the serial dictatorship, the only part of the ranked-order lists (ROL) that ultimately plays a role in the allocation is the subset of schools ranked in cut-off descending order. We use those schools to construct the ROL fixed effects.

Table 7: Estimates of the Value Added Model (On-time Graduation)

	OLS	OLS with ROL fixed effects
$[\text{Elite}]_j$	-0.192	-0.175
	(0.006)	(0.024)
$[\text{Elite}]_j \times [\text{GPA}]_i$	0.081	0.069
	(0.004)	(0.008)
$[\text{Elite}]_j \times [\text{SES index}]_i$	0.027	0.014
	(0.004)	(0.009)
$[\text{Elite}]_j \times [\text{Parent Education}]_i$	0.005	0.002
	(0.003)	(0.006)
$[\text{Academic}]_j$	0.027	-0.002
	(0.003)	(0.013)
$[\text{Academic}]_j \times [\text{GPA}]_i$	0.020	0.011
	(0.002)	(0.005)
$[\text{Academic}]_j \times [\text{SES index}]_i$	0.001	0.009
	(0.002)	(0.006)
$[\text{Academic}]_j \times [\text{Parent Education}]_i$	-0.002	-0.006
	(0.003)	(0.006)
Number of Observations	182,824	182,824

Note: This table displays OLS estimates and asymptotic standard errors (in parenthesis) for selected coefficients of equation (3). The full set of estimates is reported in Table B.10 in the Appendix.

by examining how student-school allocations under feedback provision differ from the status quo. Next, we analyze the intervention's impact on school choices, along with the resulting equilibrium effects that emerge in a centralized assignment system with fixed school capacities. Finally, we quantify the changes in educational outcomes, highlighting how these effects are shaped by the sorting patterns induced by feedback provision at scale.

5.1 Aggregate Matching Outcomes

Table 8 highlights aggregate outcomes of the school assignment mechanism under both the status quo and the information intervention scenarios. As shown in the first row, there is no change at the extensive margin of the admission process: despite the provision of performance feedback, there is no evidence that students opted out of the centralized assignment system in favor of outside options.

The share of students successfully assigned increases by 2 percentage points. While modest, this improvement speaks directly to a gain in the efficiency of the matching equilibrium. More notably, students are more likely to be matched to schools they prefer under the infor-

Table 8: The Effect of the Information Intervention on Aggregate Outcomes

	Status Quo	Performance Feedback	Difference
Applied in the system (1=yes)	0.99	0.99	0.00
Assigned in the system (1=yes)	0.89	0.91	0.02
Rank of assigned school	6.41	5.43	-0.98
Assigned in top choice	0.16	0.25	0.09
Assigned in elite schools	0.22	0.22	0.00
Assigned in academic schools	0.41	0.40	-0.01
Assigned in non-academic schools	0.37	0.38	0.01

NOTE: The average outcomes displayed in the first (second) column are obtained by running the Serial Dictatorship algorithm using as inputs the estimated school valuations of the experimental control (treatment) group, as shown in Table B.9 in the Appendix, the individual scores in the admission exam, and the school capacities.

mation intervention. The average student is assigned to a school ranked one position higher on their preference list compared to the status quo (5.4 vs. 6.4). Similarly, the proportion of students assigned to their top-ranked school increases by 9 percentage points, from 16 percent to 25 percent.

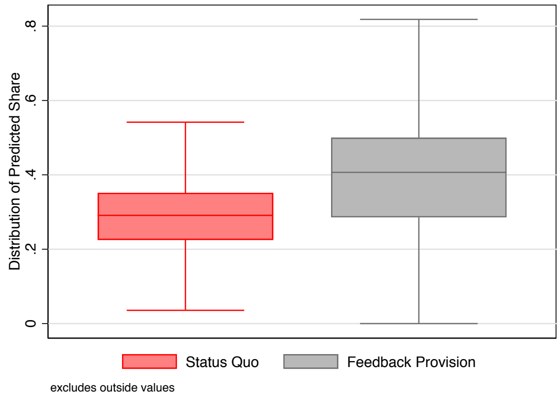
On average, there is no change in the aggregate distribution of students across tracks. The final rows of the table show that the shares of students placed in elite, academic, and non-academic programs remain virtually unchanged across the two scenarios. However, this overall stability of the assignment system masks substantial heterogeneity in the sorting patterns triggered by the information intervention, which we explore in the following subsections.

5.2 School Choices

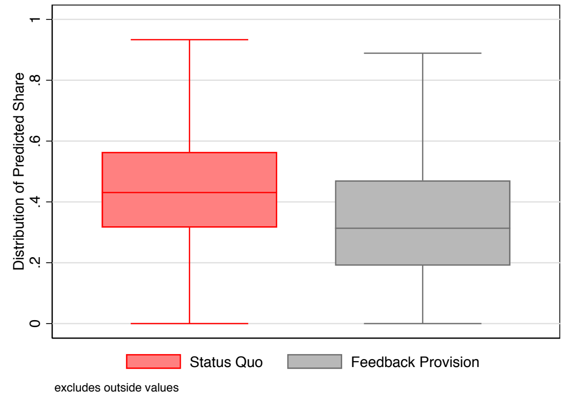
We compute the predicted shares of academic and elite programs among the set of school programs that yield higher utility than the outside option for each applicant, under both the status quo and the feedback scenarios. Panels A and B of Figure 5 reveal that the provision of performance feedback leads to a general increase in the demand for academic schools, accompanied by a corresponding decline in the demand for elite schools.

Consistent with these shifts in preferences, Figure B.5 in the Appendix shows that cutoff scores for most elite programs slightly decline under the information intervention, while those for academic programs increase on average. Overall, the combined changes in demand and resulting movements in cutoff scores help explain the limited aggregate effect of the intervention on average sorting across high school tracks, as documented in the final rows of Table 8.

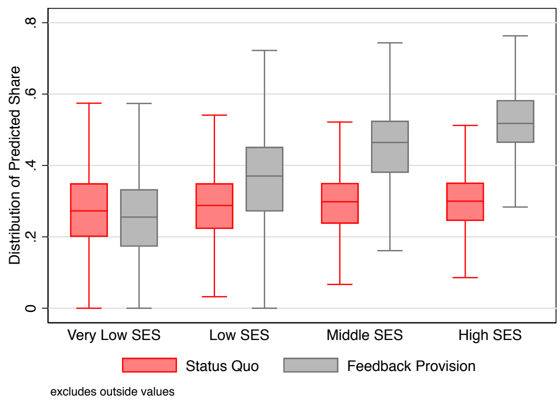
Figure 5: The Effect of Providing Performance Feedback on Track Choices



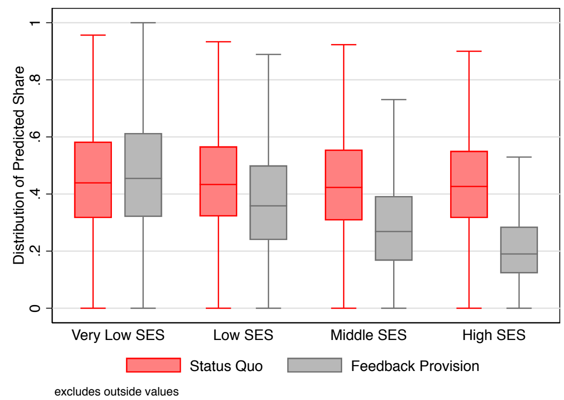
(a) Aggregate Shares of Academic Schools



(b) Aggregate Shares of Elite Schools



(c) Shares of Academic Schools by SES

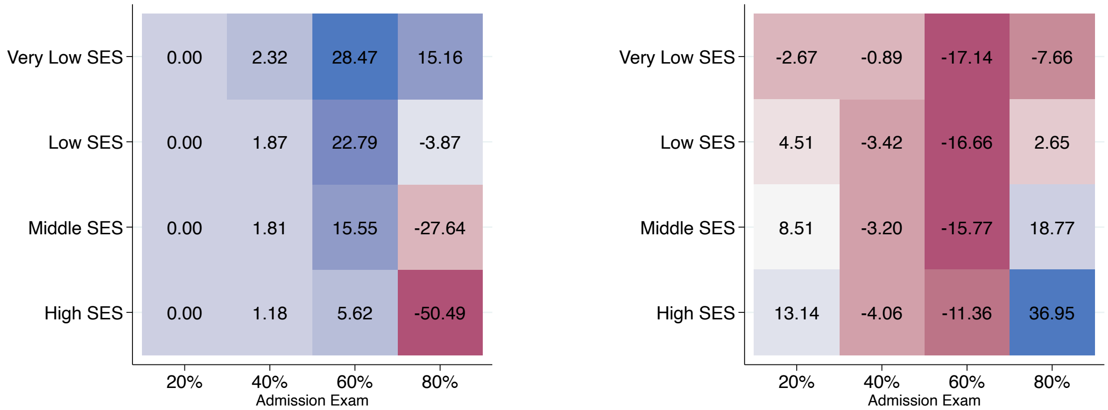


(d) Shares of Elite Schools by SES

NOTE: This figure displays box- and-whisker plots for the shares of academic schools and elite schools as implied by the estimated preference distributions for the control group and the treatment group (see Table B.9 in the Appendix). The central lines within each box denote the sample medians, whereas the upper and lower level contours of the boxes denote the 75th and 25th percentiles, respectively. The whiskers outside of the boxes denote the upper and lower adjacent values, which are values in the data that are furthest away from the median on either side of the box, but are still within a distance of 1.5 times the interquartile range from the nearest end of the box (i.e., the nearer quartile).

Panels C and D of Figure 5 replicate the analysis of predicted shares of academic and elite school choices under the two scenarios, this time disaggregated by discrete categories of the SES index. The overall shift in school preferences observed in the upper panels is primarily driven by relatively better-off applicants—those residing in neighborhoods with lower poverty rates. This pattern aligns with the estimates from the school choice model presented in Table 5, which reveal a strong negative gradient in the perceived value of attending an elite school with respect to socio-economic status among treated students. It is important to note that the experimental sample was composed primarily of disadvantaged students. Therefore, the limited responsiveness of low-SES applicants to the intervention in terms of high school track

Figure 6: The Effect of Providing Performance Feedback on High-School Admission (Percentage Points)



(a) Elite Schools

(b) Academic Schools

NOTE: This figure shows the percentage changes between the Information Policy and the Status Quo scenarios in the shares of applicants that are assigned to academic schools (Panel A) and elite schools (Panel B) by discrete categories of socio-economic status (Y-axis) and the score in the admission exam (x-axis).

choices is consistent with the evidence discussed in Section 3.3 (see in particular Table B.6 in the Appendix).

5.3 School Placement

The school choice responses triggered by the information intervention, as documented above, are likely to generate equilibrium effects within the system. In particular, lower demand from high-SES students frees up seats in elite programs, making room for disadvantaged students with high admission scores. Figure 6 documents the presence of those displacement patterns in the matching equilibrium under performance feedback when compared to the status quo. As shown in Panel A, the share of low-SES students in the top two quintiles of the score distribution that are placed in elite schools increases by over 20 percentage points. Conversely, Panel B shows that high-SES students in the top quintile of the score distribution are 37 percentage points more likely to be assigned to academic (non-elite) programs.

This crowd-in effect for low-SES applicants in elite programs could not be detected in the small-scale randomized evaluation. Indeed, as reported in the final column of Table 3, the experimental evidence shows no significant impact of performance feedback on the probability of assignment to elite schools—underscoring the importance of displacement effects across applicants in the large-scale counterfactual scenario.

5.4 Educational Trajectories

To assess the impact of the system-wide reallocation induced by the information intervention on educational outcomes, we use the predicted effects from the value-added model presented in Section 4 (see Appendix Table B.10 for the full set of results). The model estimates indicate that attending an elite school has a sizable negative effect on the likelihood of completing upper secondary education. Panel A of Figure 7 illustrates that, under the information intervention, high-SES students who shift from elite to academic (non-elite) schools experience substantial gains in school completion—up to 10 percentage points, depending on their admission score.

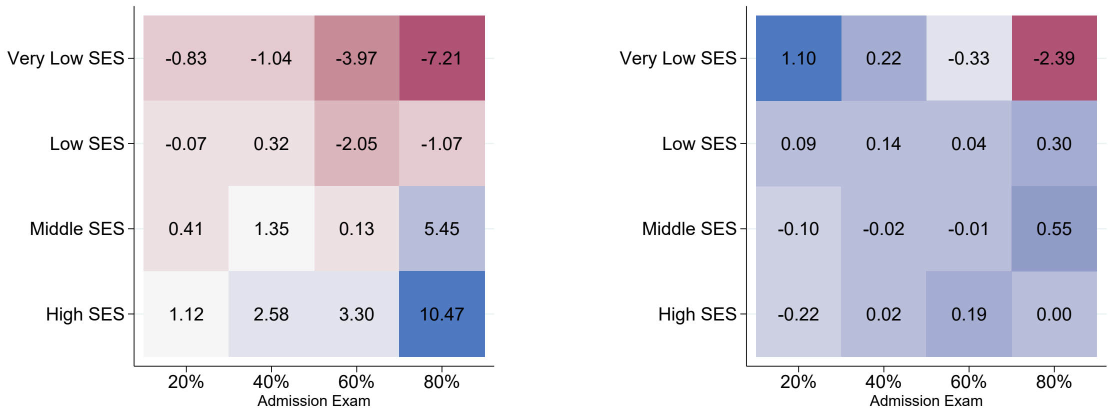
In contrast, the crowd-in of low-SES students into elite schools—documented under the feedback scenario—results in worse graduation outcomes for this group. This effect is especially pronounced among high-achieving low-SES applicants, whose probability of graduating on time decreases by 4 to 7 percentage points. These findings demonstrate that the displacement effect induced by the large-scale implementation of the information intervention would largely offset the positive impact uncovered in the randomized experiment on educational outcomes, as discussed in Section 3.4.

To replicate the conditions of the experimental setting, we simulate both school assignment and educational outcomes under a counterfactual scenario in which performance feedback is provided exclusively to very low-SES applicants—who constitute 18 percent of the 2014 applicant cohort. Panel B of Figure 7 displays the resulting effects on graduation rates. As anticipated, the limited scope of the intervention yields impacts that are concentrated among the targeted low-SES students, with negligible spillover effects on the broader applicant pool. Consistent with the experimental results reported in Table 4, the simulation reveals a modest but positive effect on graduation rates for low-achieving students within the targeted group.

6 Conclusion

We leverage a randomized experiment in Mexico City to examine the small and large scale effects of providing performance information in a centralized high school assignment system. The intervention offered a subset of applicants timely feedback based on a mock version of the standardized admission exam used for school placement. Compared to the control group, students who received this feedback were matched to schools better aligned with their abilities, leading to improved educational outcomes three years later. While the long-

Figure 7: The Effect of Providing Performance Feedback on High-School Graduation (Percentage Points)



(a) Full Scale Implementation (b) Feedback Targeted to Very Low SES

NOTE: This figure shows the percentage changes, by discrete categories of socio-economic status (Y-axis) and the score in the admission exam (x-axis), between the Information Policy and the Status Quo scenarios in the shares of applicants who complete the on time the high-school program of their assignment in the centralized system.

term effects remain an open question for future research, the findings point to a positive impact of the information intervention on student outcomes.

In order to explore the broader implementation of the policy targeting a larger and more diverse population of students, we incorporate the experimental variation into a flexible school choice model. The model is validated by comparing its out-of-sample predictions—based on estimated preferences from the control group—to the observed allocation outcomes for the full population of assigned applicants. We then link the out-of-sample predictions about the student-school allocations to downstream educational outcomes through a school value added model, which we estimate using the realized sorting patterns for the universe of the applicants in the assignment system. The school choice responses predicted by the model under the scaled-up intervention reveal significant heterogeneity across the applicant pool. In equilibrium, these responses generate a displacement effect within the assignment system that would essentially dampen the positive impacts on educational outcomes observed in the randomized experiment.

Our findings potentially contribute a novel perspective on the role of information provision in centralized education markets. Providing students with more accurate information about their academic skills improves the ex-ante efficiency of the student-school allocation. However, the distributional consequences are considerably more complex. In our setting, they hinge critically on the direction and magnitude of congestion externalities across applicants.

Although low-cost informational policies can enhance decision-making and system-level efficiency, their design must be carefully tailored to account for equilibrium responses and potential unintended consequences. In particular, targeting feedback to specific subpopulations and combining it with complementary support mechanisms may help ensure that equity and efficiency gains are preserved at scale.

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Appendices

A Experimental Instructions

We collect rich survey data with detailed information on the subjective distribution of beliefs about performance in the admission exam. In order to help students understand probabilistic concepts, we explicitly linked the number of beans placed in a cup to a probability measure, where zero beans means that the student assigns zero probability to a given event and 20 beans means that the student believes the event will occur with certainty. Students were provided with a card divided into six discrete intervals of the score. Surveyors then elicited students' expected performance in the test by asking them to allocate the 20 beans across the intervals so as to represent the chances of scoring in each bin.

We include a set of practice questions before eliciting beliefs (authors' translation from Spanish):

1. How sure are you that you are going to see one or more movies tomorrow?
2. How sure are you that you are going to see one or more movies in the next two weeks?
3. How sure are you that you are going to travel to Africa next month?
4. How sure are you that you are going to eat at least one *tortilla* next week?

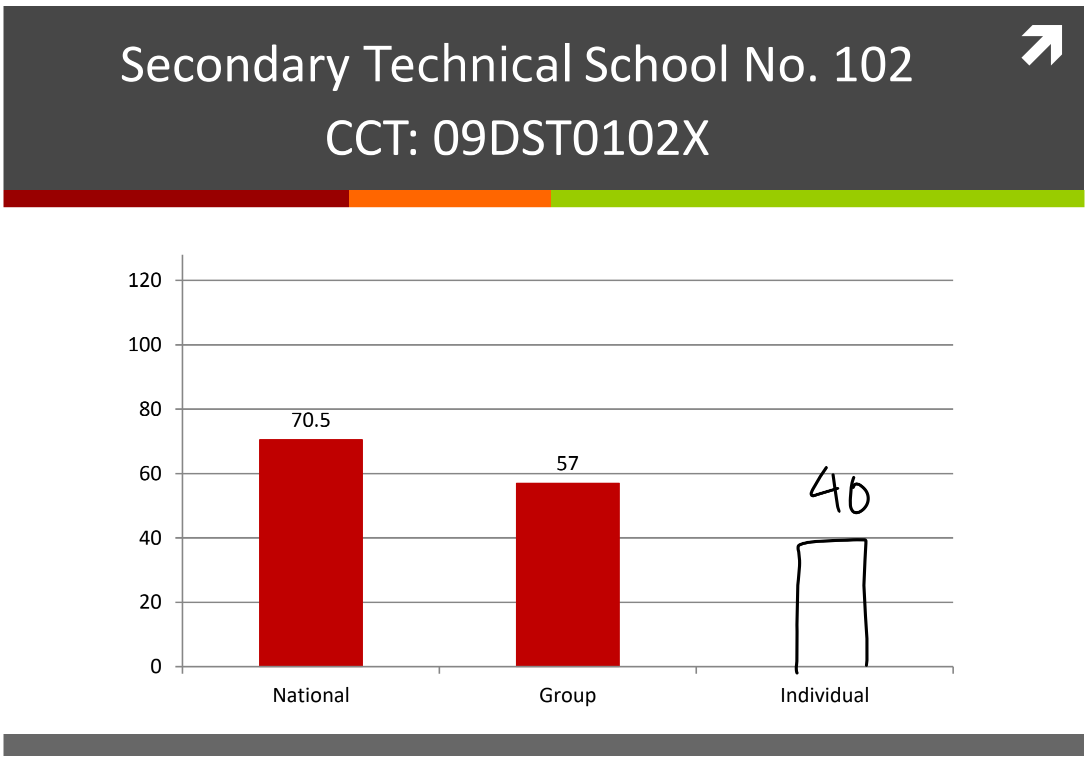
If respondents grasp the intuition behind our approach, they should provide an answer for question 2 that is larger than or equal to the answer in question 1, since the latter event is nested in the former. Similarly, respondents should report fewer beans in question 3 (close to zero probability event) than in question 4 (close to one probability event). Whenever students made mistakes, the surveyor repeated the explanation as many times as necessary before moving forward. We are confident that the elicitation of beliefs has worked well since only 11 students (0.3%) ended up making mistakes in these practice questions. The survey question eliciting beliefs reads as follows (authors' translation from Spanish):

> "Suppose that you were to take the COMIPEMS exam today, which has a maximum possible score of 128 and a minimum possible score of zero. How sure are you that your score would be between ... and ..."

During the pilot activities, we tested different versions with more or less discrete categories and/or more or fewer beans in order to assess the trade-off between coarseness of the grid and students' ability to distribute beans across all intervals. We settled for six intervals with 20 beans as students were at ease with that format. Only 6% of the respondents concentrate all beans in one interval, which suggests that the grid was too coarse only for a few applicants. The resulting individual ability distributions seem well-behaved: using the 20 observations (i.e., beans) per student, we run a normality test (Shapiro and Wilk, 1965) and reject it for only 11.4% of the respondents.

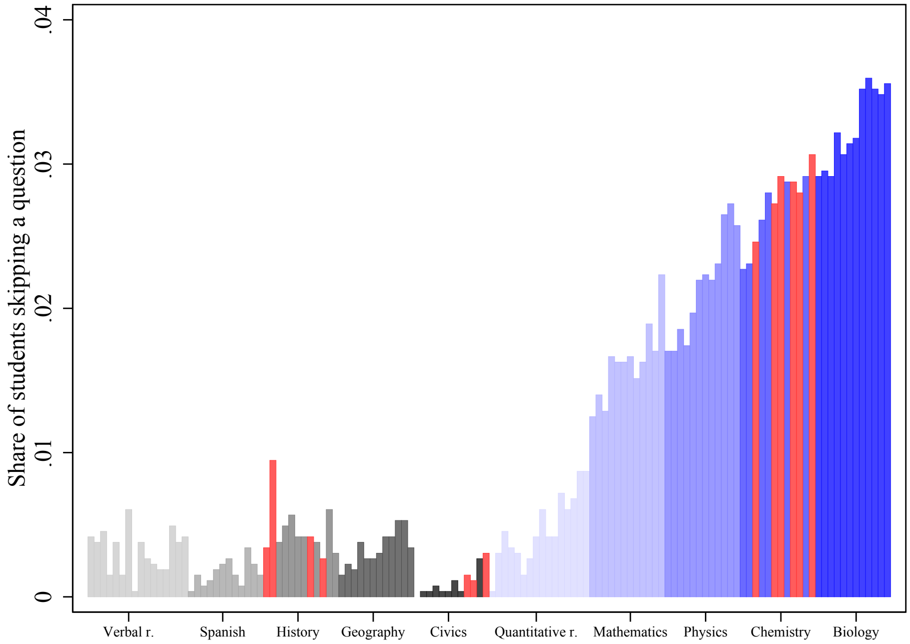
The delivery of individual scores takes place at the beginning of the follow up survey. Surveyors show the student a personalized graph with two pre-printed bars: the average score among the universe of applicants during the 2013 round and the average mock exam score of his classmates. During the delivery, the surveyors plotted a third bar corresponding to the individual's score in the mock test. Figure A.1 depicts a sample of the sheets used to deliver information to the students in the experiment.

Figure A.1: Sample of the Performance Delivery Sheet



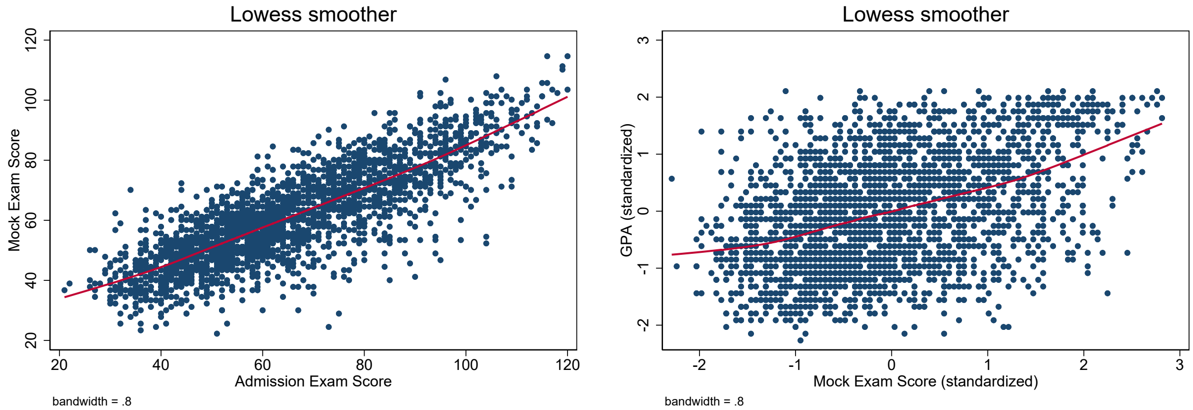
B Additional Figures and Tables

Figure B.1: Average Skipping Patterns in the Mock Exam



Note: The x-axis orders the 128 questions of the exam in order of appearance. Different colors are used to group together questions from the same section in the exam. Questions in red are the ones excluded from grading since the school curriculum did not cover those subjects by the time of the application of the mock exam.

Figure B.2: Correlates of the Mock Exam Score

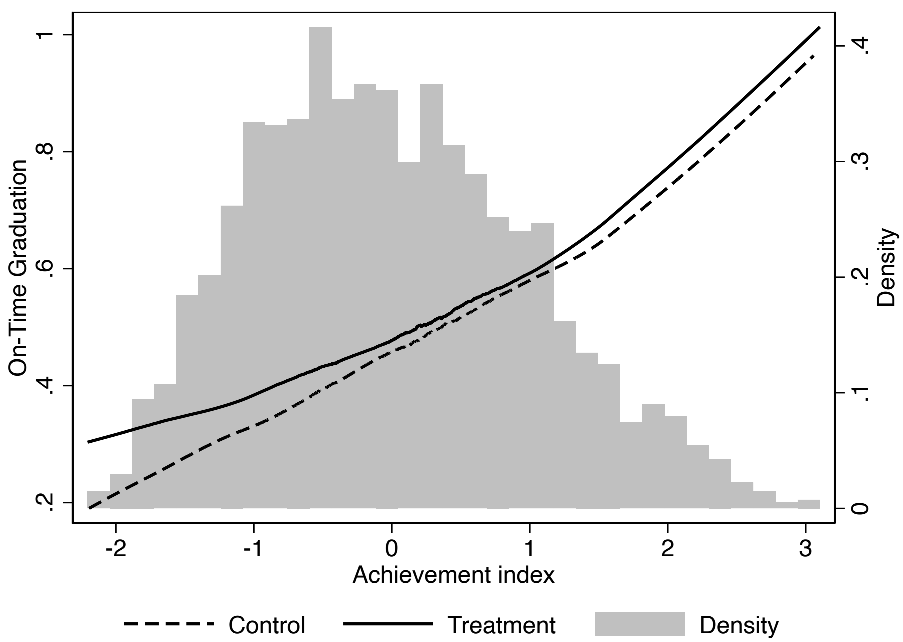


(a) Admission Exam Score

(b) Middle-school GPA

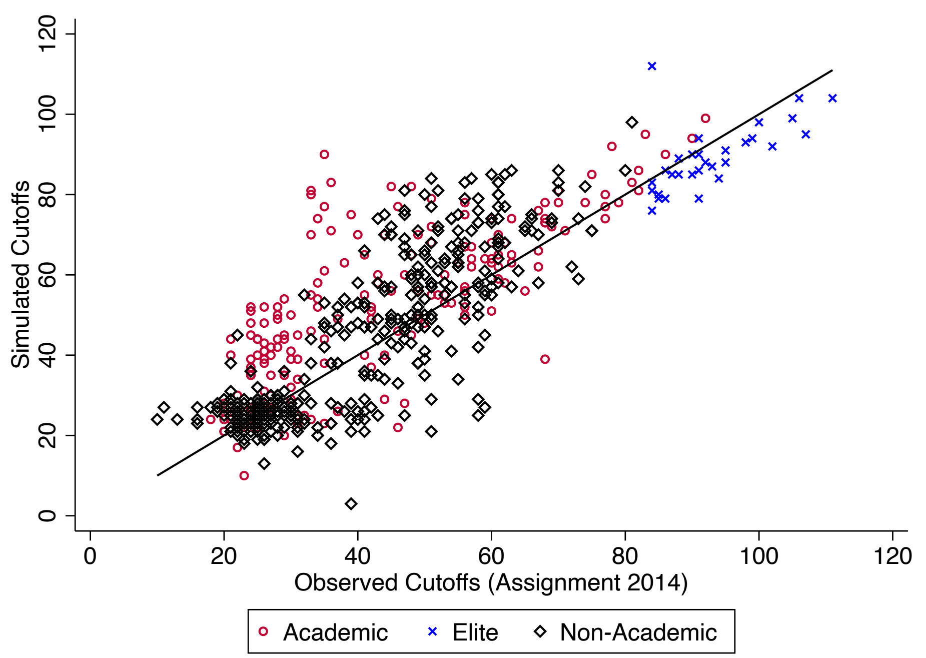
NOTE: This figure depicts scatter plots of the bi-variate relationship between the mock exam score and the admission exam score (Panel A), as well as between the mock exam score and the (standardized) Grade Point Average in middle school (Panel B). Overlaid on the scatters, we show non-parametric locally weighted estimates of the same relationships.

Figure B.3: On-time Graduation and Academic Performance



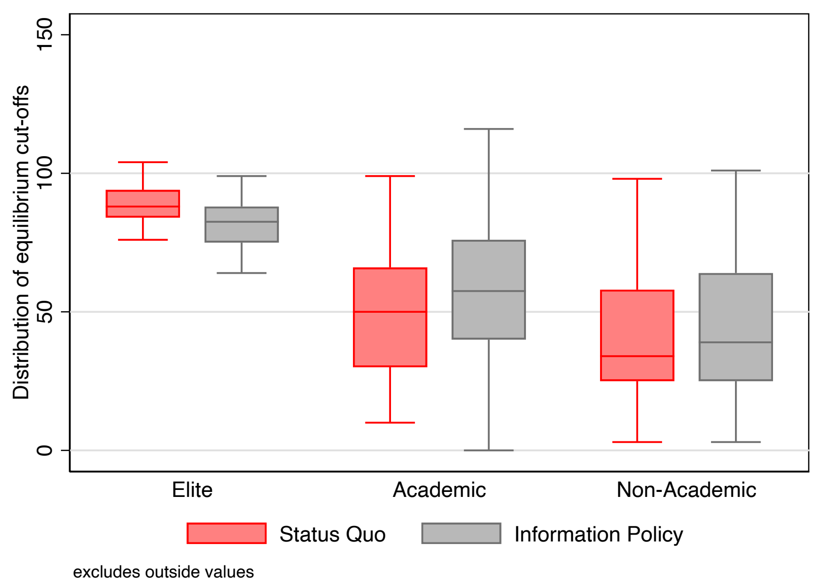
NOTE: This plot depicts non-parametric locally weighted estimates of the relationship between the graduation on time and the achievement index, which is a GLS-weighted average (Anderson, 2008) of middle school GPA, mock exam score, and exam score. "On-Time Graduation" denotes an indicator variable that is equal to one if the student successfully completes the assigned high school program in three years after placement in the centralized system, and zero otherwise.

Figure B.4: Model Fit on Schools' Cutoff Scores for All Schools



NOTE: The observed cutoffs are computed from the data of the assignment mechanism in the year 2014 (see Section 2). The simulated cutoff scores displayed in the scatter plot are computed by running the Serial Dictatorship algorithm that is in place for the COMIPEMS system using the extrapolated school valuations from the experimental control group, the individual scores in the admission exam, and the school capacities as inputs.

Figure B.5: The Effect of Providing Performance Feedback on Cutoff Scores



NOTE: The simulated cutoff scores are computed by running the Serial Dictatorship algorithm that is in place for the COMIPEMS system using the predicted school valuations based on the control group (red bars) and the treatment group (grey bars). The corresponding estimates of the school choice model (2) are reported in Table B.9 in the Appendix. The central lines within each box denote the sample medians, whereas the upper and lower level contours of the boxes denote the 75th and 25th percentiles, respectively. The whiskers outside of the boxes denote the upper and lower adjacent values, which are values in the data that are furthest away from the median on either side of the box, but are still within a distance of 1.5 times the interquartile range from the nearest end of the box (i.e., the nearer quartile).

Table B.1: Performance in the Mock or Admission Exam and On-time Graduation

	Control Group	Control Group	All Applicants
Mock exam score (standardized)	0.072		
	[0.001]		
Admission exam score (standardized)		0.055	0.061
		[0.009]	[0.001]
Mean Dependent Variable	0.447	0.447	0.407
Number of Observations	1130	1207	195824
R-squared	0.019	0.011	0.015

NOTE: This Table shows OLS estimates of the relationship between the individual scores in the mock test or the admission exam and an indicator variable of whether students have completed upper secondary education in the statutory three years since enrollment in 10th grade. p-values reported in brackets refer to the conventional asymptotic standard errors, which take into account the clustering of the error terms at the high school level.

Table B.2: Treatment Effects on Application Outcomes

	Participates COMIPEMS	Exam Score	Length of ROL	Max cutoff in ROL	Min cutoff in ROL
Treatment	0.000	-0.669	0.126	1.641	-0.366
	[0.987]	[0.348]	[0.564]	[0.247]	[0.637]
	{0.983}	{0.908}	{0.982}	{0.777}	{0.982}
Achievement index	0.023	16.147	0.079	4.019	4.368
	[0.000]	[0.000]	[0.475]	[0.000]	[0.000]
	{0.001}	{0.001}	{0.978}	{0.001}	{0.001}
Treatment $\times$ Achievement index	-0.002	0.223	-0.108	0.262	0.483
	[0.777]	[0.582]	[0.489]	[0.757]	[0.510]
	{0.982}	{0.982}	{0.978}	{0.982}	{0.978}
Mean Control	0.881	65.541	9.465	90.491	35.022
Number of Observations	3160	2493	2493	2493	2493
Number of Clusters	90	90	90	90	90
R-squared	0.609	0.735	0.032	0.266	0.243

NOTE: Standard errors clustered at the middle school level. All specifications include a set of dummy variables which corresponds to the randomization strata, pre-determined characteristics (sex, characteristics of the school of origin, previous experience with practice exams providing feedback, aspirations to attend college, an index of personality traits, an index of parental characteristics, and a household asset index), and indicator variables for whether each of the covariates has missing data. Sample in column 1 includes all students in the survey records. Sample in columns 2-5 consists of placed applicants. The achievement index is a GLS-weighted average (Anderson, 2008) of the GPA in middle school, mock exam score, and exam score. p-values reported in brackets refer to the conventional asymptotic standard errors while those reported in curly brackets are adjusted for testing each null hypothesis across multiple outcomes through the step-wise procedure described in Romano and Wolf (2005a,b, 2016). Both inference procedures take into account clustering of the error terms at the middle school level and the block randomization design.

Table B.3: Summary Statistics and Randomization Check

	Control Group	Treatment Group	Treatment-Control
Mock exam score	60.540	62.366	1.496
	(15.416)	(16.290)	[0.163]
Exam score	65.541	65.248	-0.169
	(19.516)	(19.284)	[0.893]
GPA (middle school)	8.116	8.122	-0.013
	(0.846)	(0.846)	([0.777]
Scholarship in MS	0.106	0.115	0.007
	(0.308)	(0.319)	[0.642]
Grade retention in MS	0.263	0.233	-0.026
	(0.440)	(0.423)	[0.294]
Does not skip classes	0.971	0.971	-0.001
	(0.169)	(0.169)	[0.944]
Plans to go to college	0.670	0.671	-0.003
	(0.470)	(0.470)	[0.903]
Male	0.444	0.461	0.016
	(0.497)	(0.499)	[0.427]
Disabled student	0.142	0.148	0.006
	(0.349)	(0.355)	[0.719]
Indigenous student	0.085	0.101	0.017
	(0.278)	(0.302)	[0.219]
Does not give up	0.878	0.889	0.015
	(0.327)	(0.315)	[0.279]
Tries his best	0.735	0.722	-0.016
	(0.442)	(0.448)	[0.462]
Finishes what he starts	0.720	0.712	-0.015
	(0.449)	(0.453)	[0.442]
Works hard	0.725	0.739	0.010
	(0.447)	(0.439)	[0.644]
Experienced bullying	0.142	0.152	0.010
	(0.349)	(0.359)	[0.429]
Parental background and supervision	0.032	0.058	0.011
	(0.786)	(0.760)	[0.751]
High SES (asset index)	0.463	0.480	0.015
	(0.499)	(0.500)	[0.573]
Took prep courses	0.488	0.467	-0.026
	(0.500)	(0.499)	[0.314]
Exam Preparation	0.421	0.443	0.027
	(0.494)	(0.497)	[0.405]
Previous mock exam	0.269	0.290	0.017
	(0.444)	(0.454)	[0.649]
Previous mock exam with feedback	0.133	0.166	0.028
	(0.340)	(0.372)	[0.408]
Observations	1,290	1,203	2,493

NOTE: The first two columns report means and standard deviations (in parenthesis). The last column displays the OLS coefficients of the treatment dummy along with the p-values (in brackets) for the null hypothesis of zero effect.

Table B.4: On-time and Delayed Graduation Rates (Percentage Points)

	On-time Graduation	1-year delayed	2-year delayed	3-year delayed
Elite	47.0	54.6	58.4	60.8
Academic	37.6	44.2	47.4	49.6
Non-Academic	38.6	44.9	48.5	50.8
All	39.2	45.7	49.2	51.5

NOTE: The columns show graduation rates for each school track from 3 to 6 years after admission for the cohort of applicants in the 2007 round of the assignment mechanism. The statutory high-school duration in Mexico is three years. We do not condition on a student graduating from the assigned track to calculate these graduation rates.

Table B.5: Performance Feedback and Admission Outcomes

	Placed in 1st Round	Placed Any	Ranking of placement school
Treatment	-0.004	-0.006	0.141
	[0.796]	[0.719]	[0.411]
	{0.963}	{0.961}	{0.804}
Achievement index	0.068	0.064	-0.690
	[0.000]	[0.000]	[0.000]
	{0.001}	{0.001}	{0.001}
Treatment × Achievement index	-0.007	-0.005	-0.000
	[0.647]	[0.751]	[1.000]
	{0.934}	{0.963}	{1.000}
Mean Control	0.857	0.884	3.692
Number of Observations	2824	2824	2493
Number of Clusters	90	90	90
R-squared	0.068	0.080	0.085

NOTE: Standard errors clustered at the middle school level. All specifications include a set of dummy variables which corresponds to the randomization strata, pre-determined characteristics (sex, characteristics of the school of origin, previous experience with practice exams providing feedback, aspirations to attend college, an index of personality traits, an index of parental characteristics, and a household asset index), and indicator variables for whether each of the covariates has missing data. Sample in columns columns 2-3 include all students who are matched in the administrative records of the COMIPEMS exam. Sample in column 3 consists of placed applicants. The achievement index is a GLS-weighted average (Anderson, 2008) of the GPA in middle school, mock exam score, and exam score. p-values reported in brackets refer to the conventional asymptotic standard errors while those reported in curly brackets are adjusted for testing each null hypothesis across multiple outcomes through the step-wise procedure described in Romano and Wolf (2005a,b, 2016). Both inference procedures take into account clustering of the error terms at the middle school level and the block randomization design.

Table B.6: Performance Feedback and School Rankings

	Non-Academic	Academic	Elite
Treatment	0.001	-0.001	-0.000
	[0.936]	[0.928]	[0.999]
	{0.997}	{0.997}	{0.998}
Achievement index	-0.031	-0.054	0.084
	[0.002]	[0.000]	[0.000]
	{0.003}	{0.001}	{0.001}
Treatment $\times$ Achievement index	-0.032	0.030	0.002
	[0.011]	[0.008]	[0.894]
	{0.015}	{0.013}	{0.997}
Mean Control	0.365	0.336	0.299
Number of Observations	2493	2493	2493
Number of Clusters	90	90	90
R-squared	0.154	0.129	0.266

NOTE: The dependent variable is the share of high school programs in the school rankings submitted by each applicant that belong to a given group of schools (i.e., Non-Academic, Academic, and Elite schools). The achievement index is a GLS-weighted average (Anderson, 2008) of the GPA in middle school, mock exam score, and exam score. p-values reported in brackets refer to the conventional asymptotic standard errors, while those reported in curly brackets are adjusted for testing each null hypothesis across multiple outcomes through the step-wise procedure, as described in Romano and Wolf (2005a,b, 2016). Both inference procedures take into account the clustering of the error terms at the middle school level.

Table B.7: Lee Bounds for the Effect of the Performance Feedback on Graduation on Time

	All Sample		Mock Score $\leq$ Median		Mock Score > Median	
	Lower	Upper	Lower	Upper	Lower	Upper
Lee Bounds	0.016	0.039	0.041	0.063	-0.016	0.009
	[0.504]	[0.088]	[0.137]	[0.02]	[1.547]	[0.806]
Number of Observations	2493		1171		1322	
% Observations Trimmed	0.022		0.025		0.022	

NOTE: This table reports Lee bounds (Lee, 2009) in order to account for potentially non-random sample selection in the indicator variable for whether or not students graduate from secondary education three years post-assignment. The column 'Below Median' considers the sub-sample of applicants with a value of the achievement index below the median in the sample. The column 'Above Median' considers the sub-sample of applicants with a value of the achievement index above the median in the sample. p-values reported in brackets refer to the conventional asymptotic standard errors.

Table B.8: Treatment Effects on High-School Graduation Adjusted for Skills and Preferences

	Preferences	Placement
Treatment	0.001	0.001
	[0.906]	[0.939]
	{0.970}	{0.970}
Achievement index	-0.013	-0.016
	[0.000]	[0.000]
	{0.001}	{0.001}
Treatment × Achievement index	0.005	0.009
	[0.101]	[0.115]
	{0.164}	{0.164}
Mean Control	0.417	0.428
Number of Observations	2484	2236
Number of Clusters	90	90
R-squared	0.413	0.217

NOTE: The dependent variable "Preferences" is the estimated average graduation rate for the school programs in the students' school rankings, as predicted by the value added model (3). Analogously, the dependent variable "Placement" is the estimated graduation rate of the assigned school. Data for UNAM-sponsored high school programs is not available, hence the discrepancy in the number of observations in both columns when compared to the Tables in the main text (N=2,493). The achievement index is a GLS-weighted average (Anderson, 2008) of the GPA in middle school, mock exam score, and exam score. p-values reported in brackets refer to the conventional asymptotic standard errors, while those reported in curly brackets are adjusted for testing each null hypothesis across multiple outcomes through the step-wise procedure, as described in Romano and Wolf (2005a,b, 2016). Both inference procedures take into account the clustering of the error terms at the middle school level.

Table B.9: Estimates of the School Choice Model

	Control Sample	Treatment Sample
Cole1-Aca	2.306	-0.0909
	(0.000)	(0.882)
Cole2-NonAca	-0.447	-1.901
	(0.453)	(0.002)
Cole3-Aca	2.143	-2.597
	(0.292)	(0.340)
Cole4-NonAca	1.499	-13.12
	(0.189)	(0.997)
Cole5-NonAca	2.160	0.999
	(0.002)	(0.182)
Cole6-NonAca	1.572	-0.345
	(0.001)	(0.540)
Cole7-Elite	3.124	0.101
	(0.000)	(0.923)
Cole8-Elite	10.47	-0.209
	(0.047)	(0.943)
Cole9-nonAca	0.274	-0.415
	(0.580)	(0.520)
Cole10-NonAca	-0.183	-1.621
	(0.768)	(0.029)
Cole11-Aca	1.246	-0.694
	(0.026)	(0.333)
Cole12-NonAca	-0.237	-0.657
	(0.629)	(0.261)
Cole13-Aca	0.741	0.397
	(0.075)	(0.438)
Cole14-Aca	-13.09	-2.049
	(0.998)	(0.761)
Cole15-Elite	4.380	-0.222
	(0.000)	(0.843)
Cole16-Elite	3.511	-2.062
	(0.006)	(0.178)
Cole1-Aca×Mock Score	-0.122	-0.401
	(0.565)	(0.063)
Cole2-NonAca×Mock Score	0.269	0.0496
	(0.249)	(0.817)
Cole3-Aca×Mock Score	0.338	-0.268
	(0.637)	(0.821)
Cole4-NonAca×Mock Score	0.154	0.511
	(0.714)	(1.000)

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Table B.9 Estimates of the School Choice Model – Continued from Previous Page

	Control Sample	Treatment Sample
Cole5-NonAca×Mock Score	0.595	-0.459
	(0.067)	(0.090)
Cole6-NonAca×Mock Score	0.261	-0.0889
	(0.199)	(0.660)
Cole7-Elite×Mock Score	-0.232	-0.862
	(0.533)	(0.024)
Cole8-Elite×Mock Score	-1.732	-0.938
	(0.060)	(0.321)
Cole9-nonAca×Mock Score	0.267	-0.745
	(0.204)	(0.002)
Cole10-NonAca×Mock Score	0.271	-0.628
	(0.267)	(0.006)
Cole11-Aca×Mock Score	0.167	-0.309
	(0.471)	(0.183)
Cole12-NonAca×Mock Score	0.194	-0.352
	(0.362)	(0.080)
Cole13-Aca×Mock Score	0.122	-0.487
	(0.502)	(0.006)
Cole14-Aca×Mock Score	0.788	0.716
	(1.000)	(0.755)
Cole15-Elite×Mock Score	-0.0397	-0.609
	(0.925)	(0.126)
Cole16-Elite×Mock Score	-0.181	-0.558
	(0.716)	(0.317)
Cole1-Aca×GPA	-0.453	-0.366
	(0.011)	(0.053)
Cole2-NonAca×GPA	-0.396	-0.515
	(0.049)	(0.010)
Cole3-Aca×GPA	0.0169	-0.248
	(0.976)	(0.765)
Cole4-NonAca×GPA	-0.508	0.0465
	(0.166)	(1.000)
Cole5-NonAca×GPA	-0.663	-0.187
	(0.018)	(0.450)
Cole6-NonAca×GPA	-0.516	-0.760
	(0.003)	(0.000)
Cole7-Elite×GPA	-0.337	-0.549
	(0.238)	(0.079)
Cole8-Elite×GPA	-0.445	-0.843
	(0.527)	(0.246)
Cole9-nonAca×GPA	-0.143	-0.195

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Table B.9 Estimates of the School Choice Model – Continued from Previous Page

	Control Sample	Treatment Sample
	(0.437)	(0.338)
Cole10-NonAca×GPA	-0.745	-0.200
	(0.001)	(0.351)
Cole11-Aca×GPA	-0.312	-0.103
	(0.102)	(0.619)
Cole12-NonAca×GPA	-0.528	-0.367
	(0.002)	(0.043)
Cole13-Aca×GPA	-0.446	-0.175
	(0.004)	(0.282)
Cole14-Aca×GPA	-0.329	0.311
	(1.000)	(0.840)
Cole15-Elite×GPA	-0.261	0.0693
	(0.408)	(0.836)
Cole16-Elite×GPA	-0.187	0.491
	(0.630)	(0.318)
Cole1-Aca×SES Index	-0.0335	-1.163
	(0.913)	(0.003)
Cole2-NonAca×SES Index	-0.431	-1.181
	(0.224)	(0.003)
Cole3-Aca×SES Index	0.350	-2.105
	(0.802)	(0.270)
Cole4-NonAca×SES Index	-0.665	-0.122
	(0.206)	(1.000)
Cole5-NonAca×SES Index	0.506	-0.103
	(0.202)	(0.815)
Cole6-NonAca×SES Index	-0.184	-0.976
	(0.517)	(0.006)
Cole7-Elite×SES Index	-0.522	-2.060
	(0.297)	(0.000)
Cole8-Elite×SES Index	4.464	-1.837
	(0.314)	(0.225)
Cole9-nonAca×SES Index	0.406	0.307
	(0.181)	(0.448)
Cole10-NonAca×SES Index	-0.119	-0.824
	(0.732)	(0.054)
Cole11-Aca×SES Index	0.0254	-0.936
	(0.937)	(0.029)
Cole12-NonAca×SES Index	-0.238	-0.268
	(0.404)	(0.459)
Cole13-Aca×SES Index	0.215	0.271
	(0.390)	(0.388)

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Table B.9 Estimates of the School Choice Model – Continued from Previous Page

	Control Sample	Treatment Sample
Cole14-Aca×SES Index	-0.249	-0.500
	(1.000)	(0.872)
Cole15-Elite×SES Index	-0.0760	-2.415
	(0.900)	(0.000)
Cole16-Elite×SES Index	-0.226	-2.522
	(0.747)	(0.002)
Cole1-Aca×Middle-School Math Score	0.155	-0.0929
	(0.495)	(0.724)
Cole2-NonAca×Middle-School Math Score	0.415	-0.330
	(0.088)	(0.198)
Cole3-Aca×Middle-School Math Score	0.403	-0.375
	(0.587)	(0.769)
Cole4-NonAca×Middle-School Math Score	0.176	0.357
	(0.750)	(1.000)
Cole5-NonAca×Middle-School Math Score	-0.418	0.544
	(0.308)	(0.081)
Cole6-NonAca×Middle-School Math Score	0.113	0.170
	(0.604)	(0.472)
Cole7-Elite×Middle-School Math Score	-0.168	-0.838
	(0.650)	(0.045)
Cole8-Elite×Middle-School Math Score	0.492	1.222
	(0.709)	(0.254)
Cole9-nonAca×Middle-School Math Score	0.0751	0.460
	(0.750)	(0.106)
Cole10-NonAca×Middle-School Math Score	0.728	0.296
	(0.017)	(0.290)
Cole11-Aca×Middle-School Math Score	0.234	-0.255
	(0.348)	(0.412)
Cole12-NonAca×Middle-School Math Score	0.122	-0.188
	(0.616)	(0.489)
Cole13-Aca×Middle-School Math Score	0.0217	0.589
	(0.910)	(0.008)
Cole14-Aca×Middle-School Math Score	0.408	1.953
	(1.000)	(0.332)
Cole15-Elite×Middle-School Math Score	0.810	-0.390
	(0.049)	(0.343)
Cole16-Elite×Middle-School Math Score	0.415	-0.157
	(0.390)	(0.780)
Cole1-Aca×Parent Higher Education	-0.508	0.209
	(0.247)	(0.682)
Cole2-NonAca×Parent Higher Education	-0.409	-0.0186

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Table B.9 Estimates of the School Choice Model – Continued from Previous Page

	Control Sample	Treatment Sample
	(0.408)	(0.972)
Cole3-Aca×Parent Higher Education	-0.948	-13.59
	(0.451)	(0.994)
Cole4-NonAca×Parent Higher Education	-0.636	0.606
	(0.573)	(1.000)
Cole5-NonAca×Parent Higher Education	-1.391	0.329
	(0.121)	(0.595)
Cole6-NonAca×Parent Higher Education	-0.829	0.0533
	(0.066)	(0.910)
Cole7-Elite×Parent Higher Education	-1.399	0.741
	(0.032)	(0.296)
Cole8-Elite×Parent Higher Education	-15.81	-13.96
	(0.991)	(0.988)
Cole9-nonAca×Parent Higher Education	-0.915	-0.658
	(0.088)	(0.302)
Cole10-NonAca×Parent Higher Education	-1.268	-0.191
	(0.039)	(0.747)
Cole11-Aca×Parent Higher Education	-0.453	-0.283
	(0.421)	(0.661)
Cole12-NonAca×Parent Higher Education	-0.910	0.0912
	(0.118)	(0.852)
Cole13-Aca×Parent Higher Education	-0.506	0.177
	(0.223)	(0.677)
Cole14-Aca×Parent Higher Education	-0.627	-13.84
	(1.000)	(0.994)
Cole15-Elite×Parent Higher Education	-2.705	1.004
	(0.000)	(0.165)
Cole16-Elite×Parent Higher Education	-1.668	1.256
	(0.044)	(0.171)
Distance (Km)	-0.271	-0.202
	(0.000)	(0.000)
Distance (Km)×Mock Score	0.0144	0.0169
	(0.046)	(0.023)
Distance (Km)×GPA	0.00383	0.00763
	(0.585)	(0.312)
Distance (Km)×SES Index	0.000575	0.0457
	(0.961)	(0.003)
Distance (Km)×Middle-School Math Score	0.0185	0.0215
	(0.035)	(0.033)
Distance (Km)×Parent Higher Education	0.0239	0.00427
	(0.142)	(0.808)

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Table B.9 Estimates of the School Choice Model – Continued from Previous Page

	Control Sample	Treatment Sample
Cutoff Score	1.063	1.510
	(0.000)	(0.000)
Cutoff Score×Mock Score	0.103	0.170
	(0.151)	(0.019)
Cutoff Score×GPA	0.126	0.154
	(0.052)	(0.023)
Cutoff Score×SES Index	0.0763	0.227
	(0.470)	(0.073)
Cutoff Score×Middle-School Math Score	0.110	0.262
	(0.212)	(0.002)
Cutoff Score×Parent Higher Education	0.519	-0.0602
	(0.003)	(0.727)
N	637,901	590,526

NOTE: This table displays the full set of maximum-likelihood estimates and standard errors (in parenthesis) for the parameters of the school choice model (2).

Table B.10: Estimates of the School Graduation Model

	On-time graduation
Cole2-NonAca	-0.174
	(0.023)
Cole3-Aca	0.056
	(0.052)
Cole4-NonAca	-0.028
	(0.201)
Cole5-NonAca	0.095
	(0.030)
Cole6-NonAca	0.055
	(0.017)
Cole7-Elite	-0.109
	(0.023)
Cole8-Elite	-0.050
	(0.055)
Cole9-nonAca	0.085
	(0.037)
Cole10-NonAca	0.249
	(0.035)
Cole11-Aca	-0.030
	(0.032)
Cole12-NonAca	0.060
	(0.028)

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Table B.10 Estimates of the School Graduation Model – Continued from Previous Page	
	On-time graduation
Cole13-Aca	0.146
	(0.023)
Cole14-Aca	0.130
	(0.164)
GPA	0.160
	(0.005)
SES index	0.017
	(0.006)
Parent Education	0.004
	(0.004)
Middle-School Math Score	0.015
	(0.004)
Cole2-NonAcaXGPA	-0.105
	(0.010)
Cole3-AcaXGPA	0.031
	(0.022)
Cole4-NonAcaXGPA	0.054
	(0.048)
Cole5-NonAcaXGPA	0.045
	(0.015)
Cole6-NonAcaXGPA	-0.002
	(0.008)
Cole7-EliteXGPA	0.054
	(0.008)
Cole8-EliteXGPA	0.098
	(0.027)
Cole9-nonAcaXGPA	-0.015
	(0.017)
Cole10-NonAcaXGPA	0.024
	(0.015)
Cole11-AcaXGPA	-0.006
	(0.015)
Cole12-NonAcaXGPA	0.002
	(0.011)
Cole13-AcaXGPA	-0.008
	(0.008)
Cole14-AcaXGPA	0.044
	(0.081)
Cole2-NonAcaXSES index	-0.006
	(0.012)
Cole3-AcaXSES index	0.021
	(0.040)

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Table B.10 Estimates of the School Graduation Model – Continued from Previous Page

	On-time graduation
Cole4-NonAcaXSES index	-0.045
	(0.049)
Cole5-NonAcaXSES index	0.037
	(0.019)
Cole6-NonAcaXSES index	-0.022
	(0.009)
Cole7-EliteXSES index	0.003
	(0.010)
Cole8-EliteXSES index	0.014
	(0.034)
Cole9-nonAcaXSES index	0.005
	(0.019)
Cole10-NonAcaXSES index	-0.015
	(0.018)
Cole11-AcaXSES index	-0.002
	(0.016)
Cole12-NonAcaXSES index	-0.019
	(0.011)
Cole13-AcaXSES index	-0.007
	(0.009)
Cole14-AcaXSES index	0.111
	(0.097)
Cole2-NonAcaXParent Education	0.009
	(0.012)
Cole3-AcaXParent Education	0.028
	(0.018)
Cole4-NonAcaXParent Education	0.075
	(0.059)
Cole5-NonAcaXParent Education	0.009
	(0.017)
Cole6-NonAcaXParent Education	0.010
	(0.008)
Cole7-EliteXParent Education	0.009
	(0.006)
Cole8-EliteXParent Education	0.034
	(0.018)
Cole9-nonAcaXParent Education	0.016
	(0.020)
Cole10-NonAcaXParent Education	0.000
	(0.019)
Cole11-AcaXParent Education	0.019
	(0.020)

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Table B.10 Estimates of the School Graduation Model – Continued from Previous Page

	On-time graduation
Cole12-NonAcaXParent Education	-0.008
	(0.016)
Cole13-AcaXParent Education	0.002
	(0.007)
Cole14-AcaXParent Education	0.008
	(0.043)
Cole2-NonAcaXMiddle-School Math Score	-0.021
	(0.008)
Cole3-AcaXMiddle-School Math Score	0.024
	(0.019)
Cole4-NonAcaXMiddle-School Math Score	-0.004
	(0.058)
Cole5-NonAcaXMiddle-School Math Score	0.052
	(0.020)
Cole6-NonAcaXMiddle-School Math Score	0.017
	(0.007)
Cole7-EliteXMiddle-School Math Score	0.004
	(0.007)
Cole8-EliteXMiddle-School Math Score	0.006
	(0.024)
Cole9-nonAcaXMiddle-School Math Score	0.027
	(0.020)
Cole10-NonAcaXMiddle-School Math Score	0.047
	(0.020)
Cole11-AcaXMiddle-School Math Score	0.064
	(0.020)
Cole12-NonAcaXMiddle-School Math Score	0.055
	(0.013)
Cole13-AcaXMiddle-School Math Score	0.016
	(0.008)
Cole14-AcaXMiddle-School Math Score	-0.194
	(0.115)
School-Average GPA	0.024
	(0.010)
School-Average SES index	-0.021
	(0.011)
School-Average Parent Education	-0.011
	(0.012)
School-Average Middle-School Math Score	0.000
	(0.012)
Constant	0.378

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Table B.10 Estimates of the School Graduation Model – Continued from Previous Page

	On-time graduation
	(0.012)
N	182,824

NOTE: This table displays the full set of OLS estimates and standard errors (in parenthesis) of the parameters of the school effectiveness model (3). The ROL fixed effects are included in the regression but they are not reported. The sample includes all the assigned applicants to the centralized system in the year 2010 except for the 15% of applicants who are assigned to the UNAM-sponsored high-schools (2 colleges out of 16 participating colleges).