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The Effects of Performance Feedback on School Choice

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Abstract[*]

This paper designs and implements a field experiment that provides students from less advantaged backgrounds with individualized feedback on academic performance during the transition from middle to high school. The intervention reduces the gap between expected and actual performance, as well as shrinks the variance of individual ability distributions. Guided by a simple Bayesian model, the paper empirically documents the interplay between variance reductions and mean changes of beliefs in shaping curricular choices. The shift in revealed preferences on high school tracks enabled by the intervention affects schooling trajectories, with better-performing students being assigned into more academically oriented options.

JEL classifications: D83, I21, I24, J24
Keywords: Information, Bayesian updating, Biased beliefs, School choice

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1 Introduction

Forward-looking investments in human capital are, by nature, made under uncertainty and rely on (subjective) expectations about present and future returns. Access to information and knowledge is thus crucial to help parents and/or students make sound education choices. In particular, since students from less privileged backgrounds tend to face more acute information frictions,[1] providing them with tools to enable well-informed human capital investments may enhance social and economic mobility.

A recent literature in the economics of education documents how access to information on school characteristics, labor market returns, and financial aid, among other factors, affects education choices.[2] However, few studies focus on the role of *perceived* individual traits as a determinant of schooling investments.[3] Evidence from lab experiments documents the widespread presence of overestimation in positive individual traits, such as the ability to perform a given task (see, e.g., Dunning, Heath, and Suls (2004), Moore and Healy (2008), Eil and Rao (2011)). While some studies investigate the implications of optimistic beliefs for corporate investment decisions,[4] there is a dearth of evidence on the potentially perverse effect of overconfidence in the realm of education decisions. This is an important issue since youth misperceptions of their own talent and skills may lead them to favor choices with high average returns but low individual-specific returns.

By overlaying a field experiment in a setting where beliefs are closely linked to high-stakes choices, this paper attempts to understand how individual expectations of own academic ability shape curricular decisions in upper secondary education. The context of the study is the centralized assignment mechanism used in the metropolitan area of Mexico City to allocate students into public high schools. Two institutional features are key for our research design. First, the assignment system is regulated by strict and observable criteria: stated preferences on schools and scores in

[1] See, e.g., Ajayi (2013) and Avery and Hoxby (2012).

[2] Nguyen (2008), Jensen (2010) provide evidence on the effects of providing information about population-average returns to education, while Attanasio and Kaufmann (2014), Kaufmann (2014), Wiswall and Zafar (2015), Wiswall and Zafar (2015), Hastings, Neilson, and Zimmerman (2015) more narrowly focus on the role of subjective beliefs about future earnings. Hastings and Weinstein (2008), Mizala and Urquiola (2013) document the role of providing information about school quality. More recently, Andrabi, Das, and Khwaja (2016) evaluates a bundled intervention that provides individual performance information to households with school age-children and average school performance to both households and schools. Dustan (2014) explores how students rely on older siblings in order to overcome incomplete information about the schools that belong to the same assignment mechanism considered in our paper. Finally, Hoxby and Turner (2014), Carrell and Sacerdote (2013), Dinkelman and Martinez (2014) study information interventions about application procedures and financial aid opportunities.

[3] Altonji (1993) and Arcidiacono (2004) are notable exceptions, who incorporate the notion of uncertainty about ability into the probability of completing a college major in order to distinguish between ex ante and ex post returns to a particular course of study. See also the recent survey by Altonji, Arcidiacono, and Maurel (2016).

[4] For example, it has been suggested that some managers, including CEOs, have more faith in their firm or in those that they acquire than is warranted. See Daniel, Hirshleifer, and Subrahmanyam (1998), Malmendier and Tate (2005, 2008).

a standardized scholastic admission exam. Second, applicants are required to submit their rank-ordered lists of schools before taking the admission test.

We focus on a sub-sample of potential applicants who come from the least advantaged neighborhoods within the catchment area of the school assignment mechanism. These students are less likely to have access to previous informative signals on their own academic potential. Although preparatory courses are relatively popular in this setting, the supply is mostly private and requires out-of-pocket expenditures that poorer students cannot always afford. While freely available signals are also available, they may be too noisy due to their low-stakes nature and/or their limited correlation with academic ability.

We administer a mock version of the admission test and elicit both prior and posterior subjective probabilistic distributions of individual performance therein. We take special care in measuring the first two moments of the belief distribution in order to test the specific predictions derived from a simple model of track choice with Bayesian agents. Our design also includes a pure control group of applicants who do not take the mock exam to allow us to distinguish between the effect of taking the test and the effect of receiving performance feedback. We communicate individual scores to a randomly chosen subset of applicants and observe how this information shock affects subjective expectations of academic ability, choices of high school tracks, and later schooling trajectories.

There are large discrepancies between expected and actual performance in the test. Providing feedback on individual performance in the mock test substantially reduces this gap. Consistent with Bayesian updating, applicants who receive negative (positive) feedback relative to their pre-treatment expectations adjust their mean posterior beliefs downward (upward), and this effect is more pronounced amongst those with greater initial biases. Irrespective of the direction of the update, the treatment also reduces the dispersion of individual belief distributions.

To better understand the transmission of beliefs into the demand for schools offering different curriculums, we develop a simple track choice model that incorporates the role of the mean as well as the dispersion of the belief distribution. In the model, uncertainty about graduation from the academic track is a function of both moments of the belief distribution while the continuation payoff from an academic high school solely depends on expected academic ability. The model predicts that the impact of changes in mean ability on track choices is monotonic but that changes in the precision of the perceived ability distribution can either enhance or dilute the effects of mean updating. In particular, conditional on mean beliefs, variance reductions in markets with more stringent requirements tend to reduce the probability of graduation from an academic high school (and, consequently, the returns from that track).

Our estimates broadly confirm these predictions. We find an increase in the share of academic schools listed in the application portfolios among students who receive positive feedback and who live in municipalities with more lenient graduation requirements. We also find a symmetric reduction in the demand for academic options among those who receive negative feedback and who live in municipalities with relatively high graduation requirements. Interestingly, the treatment does not generate any systematic changes in terms of the number of choices listed or the degree of selectivity of the preferred options, which suggests that the observed effects on the schooling portfolios enabled by the intervention reflects a substitution pattern across tracks.

In general, the information feedback generates a shift in revealed preferences on education modalities that leads to a higher correlation between academic ability, as measured by the individual scores in the mock exam, and the share of academic options chosen. More importantly, these changes in preferences translate into real placement outcomes within the assignment mechanism. Such better alignment between academic skills and curricular choices may potentially spur positive performance effects later on. Indeed, dropout rates by the end of the first year in high school tend to decline for applicants who are affected by the intervention, although this effect is not statistically significant.

This is one of the few papers to provide experimental evidence on the role of subjective beliefs on academic ability in educational choices. In the context of one school in the United States, Bergman (2015) studies how removing information frictions between parents and their children affects academic achievement. Dizon-Ross (2014) analyzes a field experiment conducted in Malawi that provides parents with information about their children's academic performance and measures its effect on schooling investments. Taking advantage of a natural experiment, Azmat and Iriberri (2010) evaluate the effect of providing relative performance feedback information on students' effort and subsequent performance. Relying on observational data, Arcidiacono, Hotz, and Kang (2012), Stinebrickner and Stinebrickner (2012, 2014) document the role of beliefs about future performance in college major choices and dropout decisions, while Giustinelli (2016) studies how subjective expected utilities of both parents and students shape high school track choices.

We contribute to this small but growing body of literature by focusing on the mechanisms that can generate heterogeneous responses across different groups of beneficiaries of such informational policies. In particular, we generate and test predictions on the differential roles of the mean and the variance of the individual belief distribution in schooling choices and show that their interplay can have important implications for the design and the interpretation of the effects of similar interventions.

This paper is also related to a long-standing theoretical literature on the formation and consequences of self-perceptions in the context of Bayesian learning models (see, e.g., Carrillo and Mariotti (2000), Bnabou and Tirole (2002), Zabojnik (2004), Köszegi (2006)). To date, there is little evidence on how self-perceptions affect high-stakes individual decisions. One recent exception is Reuben, Wiswall, and Zafar (2015), who study the role of overconfidence in explaining gender differences in college major choices. Our paper takes a step along those lines by linking survey-elicited measures of the distribution of beliefs on own academic ability to administrative data on schooling choices and outcomes.

2 Context and Data

2.1 The School Assignment Mechanism

Since 1996, the Metropolitan Commission of Higher Secondary Public Education Institutions (COMIPEMS, by its Spanish acronym) has centralized public high school admissions in the Metropolitan area of Mexico City, which comprises the Federal District and 22 neighboring municipalities in the State of Mexico. This commission brings together nine public educational institutions that place students in schools based on a single standardized achievement exam.[5] In 2014, the COMIPEMS system offered over 238,000 seats in 628 public high schools.

Halfway through the academic year, students in the last year of middle school receive a booklet which includes a calendar outlining the application process with corresponding instructions, as well as a list of available schools and their basic characteristics (location, modality or track, and specialties, if applicable). The COMIPEMS website publishes past cut-off scores for each option (i.e. school-specialty combinations) in the previous three rounds.

Students register between late February and early March. In addition to the registration form, students fill out a socio-demographic survey and a ranked list of, at most, 20 educational options. The admission exam is administered in June and the assignment process occurs in July. The commission requests the submission of preferences before the application of the exam under the claim that this helps them plan ahead the supply of seats in a given round.

In order to allocate seats, applicants are ranked in descending order according to their exam scores. A placement algorithm goes through the ranked list of students and places each student in their top portfolio option with available seats. There are no tie-breaking rules in place; whenever ties occur, institutions decide between admitting all tied students or none of them.

Applicants whose scores are too low to guarantee a seat in any of their preferred schools can go to schools with available seats after the assignment process is over, or they can enroll in schools with open admissions outside the system (i.e., private schools or schools outside the COMIPEMS participating municipalities). Assigned applicants are matched with only one schooling option. If

[5] The participating institutions are: Universidad Nacional Autónoma de México (UNAM), Instituto Politécnico Nacional (IPN), Universidad Autónoma del Estado de México (UAEM), Colegio Nacional de Educación Profesional Técnica (CONALEP), Colegio de Bachilleres (COLBACH), Dirección General de Educación Tecnológica Industrial (DGETI), Dirección General de Educación Tecnológica Agropecuaria (DGETA), Secretaría de Educación del Gobierno del Estado de México (SE), and Dirección General del Bachillerato (DGB). Although UNAM prepares its own admission exam, it is equivalent in terms of difficulty and content to that used by the rest of the system. UNAM schools also require a minimum cumulative grade point average (GPA) of 7.0 (on a scale of 10.0) in junior high school.

an applicant is not satisfied with his placement, he can search for another option in the same way unassigned applicants do.[6]

The COMIPEMS matching algorithm is similar to a serial dictatorship mechanism, whereby agents are ranked (by their score in the placement exam in this case) and allowed to choose, according to that priority order, their favorite good from amongst the remaining objects. Whenever agents are able to rank all objects, truthful revelation of preferences on goods is a weakly dominant strategy. In our setting, constraints to the portfolio size and uncertainty about individual ranking in the pool of applicants may lead stated preferences to deviate from actual preferences. For instance, applicants may strategically list schools by taking into account the probability of admission into each of them.[7]

2.2 The Supply Side

The Mexican system offers three educational modalities, or tracks, at the upper secondary level: General, Technical, and Vocational Education. The general track, which we denote as the academically oriented track, includes traditional schools 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 completion of high school. The vocational track exclusively trains students to become professional technicians. Each school within the COMIPEMS system offers a unique track. In technical and vocational schools, students also choose a specialization.[8]

In general, the supply of schools made available through the COMIPEMS system is geographically accessible, although there is some variation across neighborhoods. The average number of high schools located in a municipality is 16, with a standard deviation of 11.9.[9] Beyond geographic proximity, individual preferences and other school attributes may significantly reduce

[6] The assignment system discourages applicants to remain unplaced and/or to list options they will ultimately not enroll into. By definition, the residual options at the end of the centralized allocation process are not included in the preference lists submitted by unplaced or unhappy applicants.

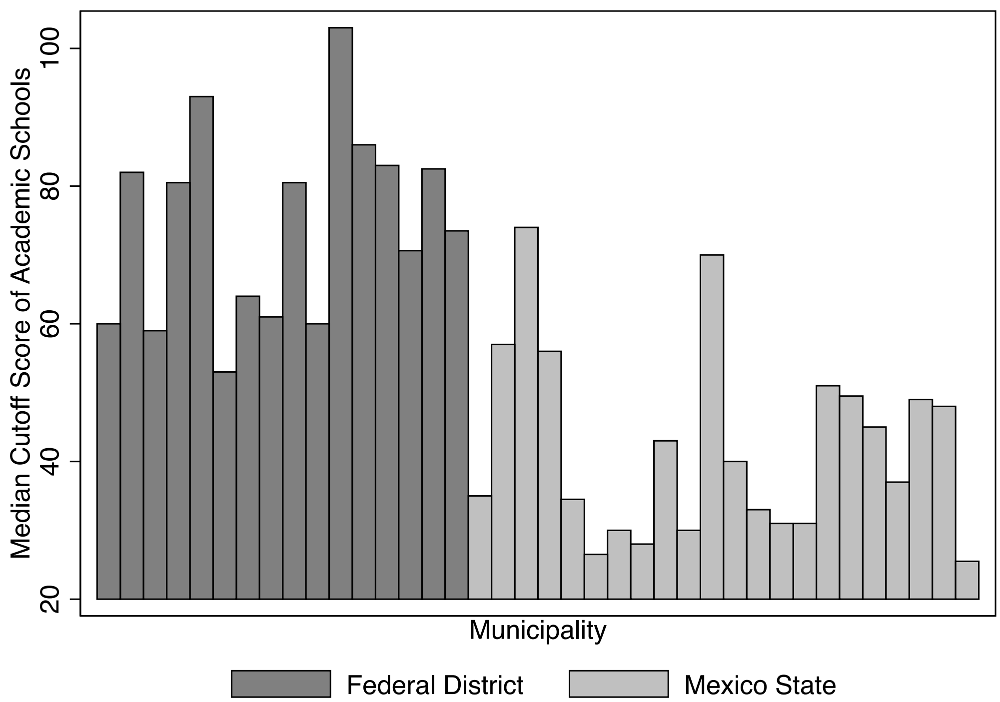
[7] Indeed, a third of the applicants in our sample do not list any option with the previous year's admission cutoff above their expected score. Even among those who include options with cutoffs above their mean beliefs, we observe that these represent less than half of the options included in their rank-ordered lists.

[8] All three modalities are conducive to tertiary education, but wide disparities exist across tracks in the transition between upper secondary and higher education. Data from a nationally representative survey for high school graduates aged 18-20 (ENILEMS, 2012) confirm that those who attended technical or vocational high schools in the metropolitan area of Mexico City are indeed less likely to enroll in a tertiary education institution (33 and 38 percent, respectively) and are more likely to work after graduating from high school (6 and 19 percent, respectively) when compared to those who graduated from academically oriented high schools.

[9] On average, the closest high school is located 1.4 miles away from the school of origin of the applicants in our sample, and about 10 percent of the options (63 schools) are located at most 10 miles away from the school of origin.

the applicants' set of feasible and desirable schools, and this may explain why most applicants do not fill the 20 slots available in the preference lists.[10]

Figure 1. Neighborhood Differences in Academic Requirements



Note: Cutoff scores for each high school program refer to the previous year (2013), and are made available to the applicants through the official COMIPEMS website. Source: COMIPEMS administrative data, 2014.

The system naturally generates ability sorting, both across schools due to the assignment algorithm, and across neighborhoods due to the geographic distribution of schools. Figure 1 depicts the geographic variation in the median cutoff score in academic schools across municipalities. In general, there is substantial heterogeneity across locations in the Mexico City area. It is also clear that the municipalities in the Federal District impose higher academic standards than those located in the State of Mexico. However, sorting across education modalities is less evident in the data. There is, indeed, a large degree of overlap between admission cutoff scores across high school tracks. The support of the cutoff distributions for schools offering technical and vocational programs is embedded in the wider support of cutoffs for schools offering academic programs.

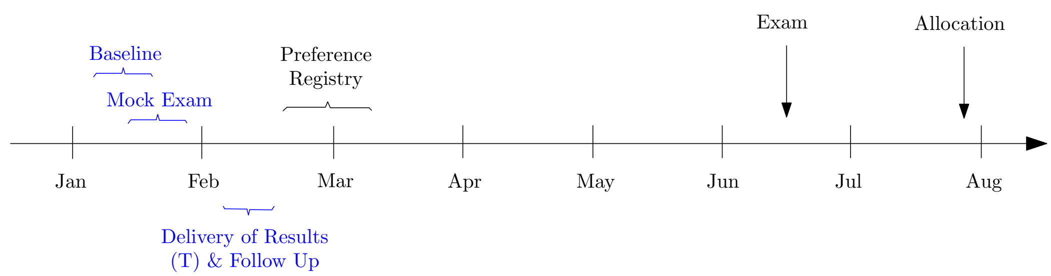
[10] The median student in our sample applies to 10 schooling options, 10 percent of the students request less than five options, and 2 percent fill all of the 20 school slots. Roughly two-thirds of the applicants in our sample are assigned to a school within their top four choices. About 13 percent remain unplaced after the matching algorithm and 2 percent are subsequently admitted into one of the schooling options with remaining slots available.

2.3 *Data and Measurement*

One of the advantages of our paper is that it relies heavily on administrative records. The main source of data is the admission records from the COMIPEMS assignment process of the year 2014 that allow us to observe the full ranked list of schooling options requested, the score in the admission exam, the cumulative GPA in middle school, and placement outcomes. We link these records to the socio-demographic survey filled out at registration, which provides us with variables such as gender, age, household income, parental education and occupation, personality traits, and study habits, among others. We further match administrative individual records for the academic year 2014-2015 to obtain information on attendance and grades by subject for those applicants enrolled in their assigned high school.[11]

We complement administrative records with two rounds of survey data and, for those who took the mock exam, individual scores on the mock exam. Figure 2 depicts the timing of the activities related to the intervention during the application process. The baseline survey was conducted over the last two weeks of January 2014, and the mock exam was administered two or three days after the baseline. The follow-up survey was conducted in the second and third weeks of February 2014, right before the submission of the preference lists.

Figure 2. The School Assignment Process and the Intervention: Timeline of Events



NOTE: COMIPEMS rules in place in 2014.

In both surveys, we collected detailed data on the subjective distribution of beliefs on performance in the exam. In order to help students understand probabilistic concepts, we relied on visual aids (Delavande, Giné, and McKenzie, 2011). In particular, we explicitly linked the number of beans placed in a cup to a probability measure, where zero beans means that the student assigns

[11] About 25 percent of the students in our sample could not be matched with the schooling option in which they were admitted through the system. Although some mismatches in students' identifiers may partly explain such discrepancy, most of it is driven by non-enrollment. Enrollment conditional on assignment does not vary across tracks, although it is much higher in the Federal District (87 percent) than in the State of Mexico (68 percent).

zero probability to a given event and 20 beans means that the student believes the event will occur with certainty.[12]

Students were provided with a card divided into six discrete intervals of the score in the admission exam. Surveyors then elicited students' expected performance by asking them to allocate the 20 beans across the six intervals so as to represent the chances of scoring in each bin.[13] The survey question 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 ..."

Assuming a uniform distribution within each interval of the score, mean beliefs 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. The variance of the distribution of beliefs is obtained as the summation over intervals of the product of the square of the mid-point of the bin and the probability assigned to the bin.

The delivery of feedback on performance in the mock exam took place at the beginning of the follow-up survey. Surveyors showed a personalized graph with two pre-printed bars: the average score in the universe of applicants during the 2013 edition of the COMIPEMS system and the average mock exam score in the class of each applicant. During the interview, a third bar was plotted corresponding to the student's score on the mock exam.[14]

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[12] We include a set of check questions before collecting beliefs:

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). We are confident that the collection of beliefs works well since only 11 students out of 4,127 (0.27 percent) made mistakes in these check questions. Whenever students made mistakes, the surveyor reiterated the explanation as many times as was necessary before moving forward.

[13] During the pilot activities, we tested different versions with fewer bins and/or fewer beans. Students seem to be at ease manipulating 20 beans across six intervals, and hence we keep this version to reduce the coarseness of the grid. 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 percent of the respondents. Only 6 percent of the respondents concentrate all the beans in one interval, which suggests that the grid was too coarse only for a few applicants.

[14] Both the elicitation of beliefs about exam performance and the delivery of the score occurred in private in order to avoid social image concerns when reporting (Ewers and Zimmermann, 2015).

3 Experimental Design

3.1 Sample Selection and Randomization

In order to select the experimental sample, we impose several criteria on the universe of potential COMIPEMS applicants. First, we focus on ninth-graders in general or technical schools, excluding schooling modalities which represent a minor share of the existing educational facilities in the intervention area, such as *telesecundarias*. Second, we focus on schools with a considerable mass of COMIPEMS applicants in the year 2012 (more than 30). Third, we choose to focus on students in schools from neighborhoods with high or very high levels of marginalization since they are the most likely to benefit from our intervention due to low exposure to previous signals on their academic performance.[15]

Schools that comply with those criteria are grouped into four geographic regions (see Figure A.1) and terciles of the school-average performance amongst ninth graders in a national standardized test aimed at measuring academic achievement (ENLACE, 2012). We select at most 10 schools in each of the 12 resulting strata. Some strata that are less dense participate with fewer schools, which explains why the final sample is comprised of 90 schools. Whenever possible, we allow for the possibility of oversubscription of schools in each strata in order to prevent fallbacks from the sample due to implementation failures.

Treatment assignment is randomized within strata at the school level. As a result, 44 schools are assigned to the treatment group in which we administer the mock exam and provide face-to-face feedback on performance, and 46 schools are assigned to a "placebo" group in which we only administer the mock exam without reporting the test results. Since compliance with the treatment assignment was perfect, the 28 over-sampled schools constitute a pure control group that is randomized out of the intervention and is only interviewed in the follow-up survey.[16] Within each school in the final experimental sample, we randomly pick one ninth-grade classroom to participate in the experiment.

Our initial sample size is 3,001 students assigned to either the treatment or the placebo group at baseline. Only 2,790 students were present on the day of the exam and a subset of 2,544 were also present in the follow-up survey. Since the actual treatment was only delivered at the end of the follow up survey, feedback provision does not generate differential attrition patterns. Adding the 912 students from the control group yields a sample of 3,456 observations with complete

[15] Data from the 2012 edition of the assignment system shows that, on average, 33 percent of applicants took a preparatory course of some kind before submitting their schooling choices. This share ranges from 44 percent to 12 percent across schools in neighborhoods with low and high levels marginalization, respectively.

[16] As shown in Figure A.1, some strata are not populated for this group.

survey and exam records. The final sample consists of 3,100 students who can be matched with the COMIPEMS administrative data.[17]

Table 1 provides basic descriptive statistics and a balancing test of the randomization for the main variables used in the empirical analysis. Consistent with the random treatment assignment, no significant differences are detected across groups.

3.2 Mock Exam

The mock exam was designed by the same institution that prepares the official admission exam in order to mirror the latter in terms of structure, content, level of difficulty, and duration (three hours). The exam had 128 multiple-choice questions worth one point each, without negative marking for wrong answers.[18] To reduce preparation biases due to unexpected testing while minimizing absenteeism, we informed students about the application of the mock exam a few days in advance but did not tell them the exact date of the event.[19]

We argue that the score in the mock exam was easy to interpret for the applicants while providing additional and relevant information on their academic skills. On one hand, the intervention took place after all informative and application materials had been distributed. Those materials provide prospective applicants with detailed information on the rules, content, structure and difficulty of the admission exam. On the other hand, we show that the mock exam score is a good predictor of future academic performance beyond the informativeness of other readily and freely available signals such as grade point average (GPA) in middle school. Using data on the placebo group (applicants who took the mock test without receiving feedback), we run an OLS regression with high school fixed effects of the academic outcomes in the first year of high school on middle school GPA and mock exam score. Although there is a strong and significant correlation between past and current grades (coeff.=0.61, std.err.=0.043), a one SD increase in the mock exam score is associated with an increase of 0.21 SD units (std. err.=0.045) in high school GPA.[20]

[17] The 10 percent discrepancy between the survey data and the administrative data reflects applicants' choices not to participate in the COMIPEMS assignment system.

[18] Since the mock test took place in February, before the school year was over, 13 questions related to the curriculum covered between March and June of the last grade of middle school were excluded from the grading. Out of eight questions in the History, Ethics, and Chemistry sections, four, three, and six were excluded, respectively. We normalize the raw scores obtained in the 115 valid questions to correspond to the 128-point scale before providing feedback to the treatment group.

[19] In order to guarantee that the mock test was taken seriously, we informed students, their parents, and the principals of the schools in the sample about the benefits of additional practice for the admission exam. We also made sure that the school principal sent the person in charge of the discipline and/or a teacher to proctor the exam along with the survey enumerators. We argue that this last feature is important given the hierarchical nature of Mexican schools, particularly at basic schooling levels. The sample correlation between performance in the mock exam and the actual exam is 0.82.

[20] The linear correlation in our sample between the GPA in middle school and the score in the mock exam is 0.45.

Table 1. Summary Statistics and Randomization Check

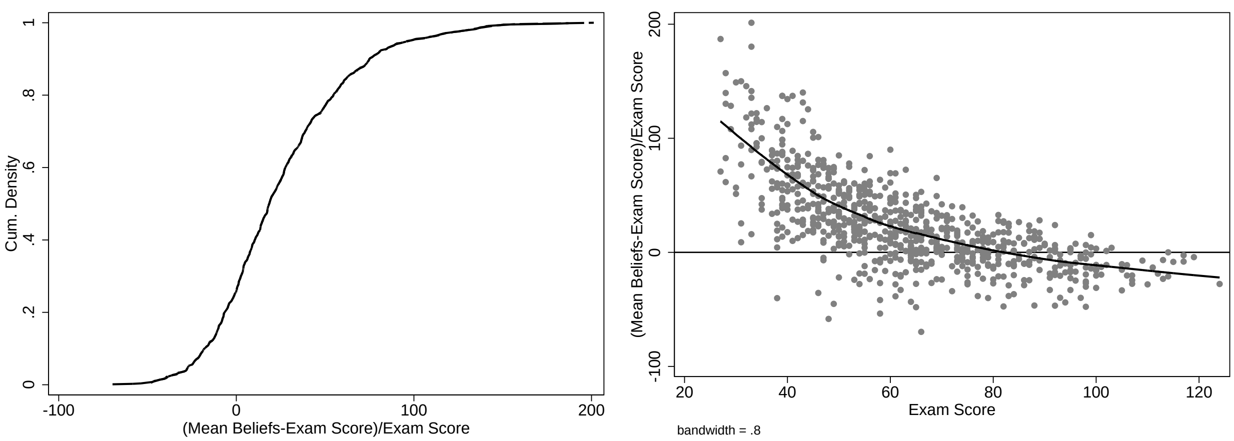
	Placebo	Treated	Control	T-P	P-C	T-C
	(1)	(2)	(3)	(4)	(5)	(6)
Mean prior beliefs	74.39	74.45		0.015		
	(14.42)	(14.40)		[0.98]		
SD prior beliefs	18.06	17.62		-0.526		
	(8.29)	(8.33)		[0.25]		
Mock exam score	58.77	60.75		1.654		
	(15.62)	(16.40)		[0.13]		
	(19.65)	(19.93)	(19.50)	[0.84]	[0.86]	[0.93]
GPA (middle school)	8.094	8.126	8.049	0.011	0.059	0.065
	(0.87)	(0.84)	(0.85)	[0.83]	[0.34]	[0.31]
	(0.29)	(0.30)	(0.32)	[0.58]	[0.13]	[0.23]
Gender (male)	0.469	0.497	0.478	0.024	-0.001	0.022
	(0.50)	(0.50)	(0.50)	[0.17]	[0.95]	[0.24]
Previous mock exam (dummy)	0.287	0.305	0.269	0.017	-0.001	0.018
	(0.45)	(0.46)	(0.44)	[0.64]	[0.98]	[0.72]
Previous mock-exam w/ results	0.179	0.193	0.151	0.012	0.010	0.023
	(0.38)	(0.39)	(0.36)	[0.73]	[0.79]	[0.59]
Attend prep. course	0.519	0.497	0.419	-0.027	0.067	0.045
	(0.50)	(0.50)	(0.49)	[0.37]	[0.08]	[0.25]
Morning shift (middle school)	0.618	0.664	0.779	0.007	-0.118	-0.110
	(0.49)	(0.47)	(0.41)	[0.94]	[0.28]	[0.31]
Lives w/ both parents	0.784	0.795	0.749	0.010	0.042	0.050
	(0.41)	(0.40)	(0.43)	[0.60]	[0.08]	[0.04]
Parents with higher ed.	0.122	0.126	0.112	0.007	-0.021	-0.016
	(0.33)	(0.33)	(0.32)	[0.71]	[0.33]	[0.52]
SE index (above-median)	0.491	0.527	0.476	0.025	-0.001	0.022
	(0.50)	(0.50)	(0.50)	[0.32]	[0.96]	[0.47]
Currently working	0.324	0.306	0.382	-0.021	-0.044	-0.065
	(0.47)	(0.46)	(0.49)	[0.33]	[0.13]	[0.022]
Plans to attend college	0.729	0.718	0.689	-0.014	0.013	-0.002
	(0.45)	(0.45)	(0.46)	[0.50]	[0.66]	[0.94]
Missing value (any control variable)	0.344	0.369	0.323	0.028	-0.018	0.008
	(0.48)	(0.48)	(0.47)	[0.22]	[0.55]	[0.79]
Number of observations	1192	1101	807	2293	1999	1908

NOTE: Columns 1-3 report means and standard deviations (in parenthesis). Columns 4-6 display the OLS coefficients of the treatment dummy along with the p-values (in brackets) for the null hypothesis of zero effect. Strata dummies included in all specifications, standard errors clustered at the school level.

3.3 (Biased) Beliefs, Track Choices, and Schooling Outcomes

Due to the timing of the application process (see Section 2.1), students are left to choose a set of high school programs without having a good idea of their academic skills. To further motivate the rationale of our intervention, we start by showing how expected performance early in the application process compares to actual performance in the exam. Using data from the control group, panel (a) of Figure 3 plots the cumulative density of the gap between mean beliefs and scores in the admission exam as a percentage of the score.[21] Approximately three-quarters of the students in the control group expect to perform above their actual exam score. While the average student has a 25 percent gap relative to his actual admission exam score, the average gap among those with upward-biased beliefs is more than double than that of students with downward-biased beliefs. Panel (b) of Figure 3 shows that students with the lowest scores tend to have upwardly biased beliefs, while the best-performing students have mean beliefs below actual performance. Both upward and downward biases are observed for intermediate levels of exam score.

Figure 3. Gap between Expected and Actual Exam Score



(a) (b)

NOTE: Panel (a) shows the cumulative density of the gap between mean beliefs and scores in the COMIPEMS admission exam as a percentage of the exam score for the control group. For the same sample, panel (b) depicts the relationship between the expectation gap and the score in the admission exam. The thick line comes from a locally weighted regression of the gap on exam scores. Source: Survey data (February, 2014) and COMIPEMS administrative records (2014).

Next, we provide evidence on the potential skill mismatch that biased beliefs may generate in terms of high school track choices and admission outcomes. As before, we rely on data from

[21] After controlling for the performance in the mock test, mean beliefs correlate positively and significantly with the GPA in middle school, self-reported hours per week dedicated to homework/study, perseverance, wealth (proxied by household durable goods), college aspirations, and students' subjective ranking in their class. No systematic relationship is found with respect to the gender of the applicants.

the control group and run an OLS regression of the share of academic options listed on applicants' mean beliefs and mock scores. Mean beliefs have a positive effect on students' demand for academic schools while the estimated coefficient for actual test performance is close to zero and statistically insignificant. This pattern also holds when we focus on admission outcomes. Indeed, a one standard deviation increase in *expected* test performance is associated with an increase of 3.5 percent in the probability of being admitted into the academic track (std. err.=0.016), with no effects of the *actual* test on performance. This evidence suggests that students who are unaware of their academic skills may discard schooling options with potentially higher expected returns—e.g., vocational and technical options for weaker students and academically oriented options for students with higher test scores.

In addition, the lack of a significant correlation between the probability of admission in the academic track and the exam score suggests that biased beliefs not only influence school application portfolios but may also have real consequences for later trajectories. To show this, we rely on the placebo group and regress achievement measures during the first year of high school on an indicator variable of overconfidence based on initial beliefs and mock exam score. Relative to students who underestimate their performance and conditional on the score in the admission exam, students enrolled in academic programs with upwardly-biased beliefs are 8 percent more likely (std. err.=0.044) of being held back—i.e., fail in three or more subjects—and their GPAs are 0.23 SD lower.[22] On the contrary, high school performance among students enrolled in technical or vocational programs is not significantly affected by the direction of bias in beliefs on academic ability.

All in all, we show that students who *think* they are good enough to go to academic programs demand them relatively more often, and, irrespectively of their performance in the admission exam, they are more likely to get into such a program. However, preliminary evidence suggests that the observed bias in perceptions about own academic ability may lead students to make sub-optimal curricular choices with detrimental effects on subsequent academic performance. This seems to be particularly the case for those holding upward-biased beliefs, which prove to be endemic in our sample.

4 Model

4.1 Bayesian Learning

Students are endowed with academic ability, q_i, which is modeled as a draw from an individual-specific distribution:

[22] On average, 14 percent of enrolled applicants in the placebo group drop out, and 19 percent are held back during the first year of high school. Dropout rates are relatively higher in the vocational track (24 percent), while repetition rates are slightly more frequent in the academic track (20 percent).

$$q_i \sim N(\mu_i, \sigma_i^2). \tag{1}$$

They do not observe q_i but know its underlying distribution.[23] Measures of academic performance (e.g., school grades or standardized test scores) as well as other types of feedback (from teachers, peers, parents, etc.) provide students with noisy signals s_i about q_i:

$$s_i = q_i + \epsilon_i$$

where $\epsilon_i \sim N(0, \sigma_\epsilon^2)$. The distribution of ϵ_i is known and the same for all individuals.

Each signal received leads to a Bayesian update in beliefs:

$$E(q_i|s_i) = \mu_i + (s_i - \mu_i)\frac{\sigma_i^2}{(\sigma_i^2 + \sigma_\epsilon^2)} \tag{2}$$

$$Var(q_i|s_i) = \left[1 - \frac{\sigma_i^2}{(\sigma_i^2 + \sigma_\epsilon^2)}\right]\sigma_i^2. \tag{3}$$

4.2 Track Choices

Students choose a schooling curriculum they wish to pursue in high school, if any. Their choice between staying out of school ($j = O$), pursuing an academic career ($j = A$), and obtaining a technical degree ($j = T$) clearly depends on the net expected returns from each alternative, which can be functions of own academic ability. However, since q_i is not observed, students make choices based on beliefs about their own ability. In particular, let $q_j^\star$ be the minimum academic ability cutoff required to comply with the academic requirements to graduate from track $j = \{\mathrm{A}, \mathrm{T}\}$, where $q_A^\star > q_T^\star$. Since staying out of school is always an option, we let $q_O^\star = 0$.

Student i's expected utility from each alternative is given by:

$$U_{ij} = Pr(q_i > q_j^\star)\ V_{ij}, \tag{4}$$

where V_{ij} is the net present discounted value of attending track j for student i. For example, the option value of going to college is included in V_{iA}, while V_{iT} features the expected value of labor market entry after a high school technical degree. For some students, V_{iO} may be relatively high, binding the number of schools considered acceptable choices.

For our purposes, we do not need to explicitly formalize the role of other determinants of V_{ij}, such as family income, access to credit, and network connections. In order to derive precise

[23] Alternatively, q_i can also be defined as the accumulation of both innate and acquired academic ability. Since individuals cannot observe their stock, they have a perceived distribution in mind.

predictions on the effects of expected q_i on track choices, the model abstracts from school attributes other than the modality offered.[24] We posit and argue in favor of the following two assumptions:

Assumption 1 (Role of mean)**.** *V_{iA} and V_{iO} are non-decreasing functions of mean beliefs about academic ability, whereas V_{iT} does not depend on those beliefs.*

In this model, q_i is conceived as a specific skill that is geared toward academic endeavors. Since the outside option may entail subsequent academically-oriented choices and occupations, we do not rule out a positive role of mean beliefs in V_{iO}. As μ_i increases, U_{iO} may also increase.

Assumption 2 (Role of variance)**.** *The variance of the belief distribution does not enter V_{ij}.*

This choice restricts the role of uncertainty in beliefs by allowing it to operate only through the perceived probability of graduation from high school. As students go on through their subsequent schooling trajectories implied by track choices in high school, they continue to receive academic ability signals that will further reduce the variance of their beliefs. To keep things simple and given the potentially diminishing role of uncertainty about own academic ability as time goes by, we assume away the role of variance in V_{ij} for all j.[25]

For ease of exposition, we further impose the normalization that $q_T^\star = 0$,[26] and we accordingly derive the expected changes in the *relative* demand for the academic track due to updates in beliefs about academic ability:

$$\frac{\partial U_{iA}}{\partial \mu_i} = \frac{1}{\sigma_i}\phi\left(\frac{q_A^\star - \mu_i}{\sigma_i}\right) V_{iA} + \left[1 - \Phi\left(\frac{q_A^\star - \mu_i}{\sigma_i}\right)\right]\frac{\partial V_{iA}}{\partial \mu_i} \geq 0, \tag{5}$$

$$\frac{\partial U_{iA}}{\partial \sigma_i} = \phi\left(\frac{q_A^\star - \mu_i}{\sigma_i}\right)\left(\frac{q_A^\star - \mu_i}{(\sigma_i)^2}\right) V_{iA} \geq 0 \quad \text{if } (q_A^\star - \mu_i) \geq 0, \tag{6}$$

where Φ and ϕ denote, respectively, the CDF and the PDF of the standard normal distribution. Equations (5) and (6) show that positive feedback always increases the value of schools from track A through the positive effect of mean beliefs on both $Pr(q_i > q_j^\star)$ and V_{ij}, while changes in the dispersion of the ability distribution can either reinforce or counteract the effect of updates in mean beliefs on track choices through their effect on $Pr(q_i > q_j^\star)$. In particular, given the same μ_i, variance reductions in settings with relatively lenient academic requirements (low $q_A^\star$) increase the

[24] For instance, higher academic ability may yield greater complementarities in academic schools with better peers. We do not find evidence in favor of this channel (see columns 4, 5 and 6 of Table A.2).

[25] Using the high school records for the applicants in the control group as a proxy for the (short-term) returns to track choices, we provide some evidence that is consistent with assumptions 1 and 2. A one SD increase in mean beliefs for the students enrolled in academically-oriented high schools is associated with an increase of 0.40 SD units in high school GPA (std. err.= 0.079), whereas the estimated coefficient of mean beliefs for those enrolled in technical or vocation programs is close to zero and statistically insignificant. In all high school tracks, the estimated coefficients of the variance of the individual belief distributions on GPA are very small and statistically insignificant.

[26] In fact, $q_A^\star$ could be interpreted as the difference between the cutoffs of track A and track T.

probability of graduation in the academic track while the opposite occurs in settings with relatively more stringent graduation standards (high $q_A^\star$).[27]

4.3 Effects of the Intervention

According to the simple framework discussed above, the signal provided with the intervention should lead students in the treatment group to update both the mean and the dispersion of their belief distribution. The position of the signal vis--vis prior mean beliefs—i.e., the term $(s_i - \mu_i)$ in (2)—determines both the direction and the strength of the update. In turn, (3) shows that the posterior variance is independent of the direction of the update and depends on the value of σ_ϵ^2 relative to σ_i^2. For instance, a signal that is as noisy as the prior distribution of beliefs halves the variance of the prior, regardless of the direction of the update.

In our setting, students rank a set of n_i schools, where $n_i \leq 20$, with n_{iA} schools from the academic track and $n_{i\mathrm{T}}$ schools from the technical or vocational track. Although each individual may have an underlying full ranking of all the schools available and the outside option, in the data we only observe the ranking of the schools that are valued above U_{iO}. Hence, we focus on the share of academic schools included in the submitted portfolio, $\frac{n_{iA}}{n_i}$, as our main outcome variable on choices.

We expect the intervention under study to have differential effects depending on the position of the signal relative to mean priors and on the graduation requirements that applicants are likely to face depending on the supply of high schools in their area of residence.[28] Applicants who receive positive feedback and who live in municipalities with low q_A^* are likely to increase the share of academic schools in their portfolio. In these low-cutoff settings, both the shift in mean and the variance reduction in beliefs enabled by the signal increases the probability of graduation from the academic track, reinforcing the positive effect of mean beliefs on the payoff associated with this track. Analogously, students who receive negative feedback and who live in municipalities with high q_A^* are expected to decrease the share of academic schools in their portfolio. For applicants who receive positive feedback in high cutoff municipalities or those who receive negative feedback in low cutoff municipalities, the net effect of the intervention on track choices is ambiguous and will depend on the relative strength of the variance reduction on the probability of graduation

[27] This mechanism echoes the literature on aspirations and their motivational role, where greater aspiration gaps can lead to aspiration frustration (Ray, 2006).

[28] Although the centralized process allows the applicant to select schools in different tracks and municipalities across the entire metropolitan area of Mexico City, there is a strong degree of geographic segmentation in preferences and admission outcomes. On average, 42 percent of the options listed in the application portfolios are located in the same municipality of the applicants' middle school. Consequently, nearly half of the assigned applicants in our sample are admitted into a high school that is located in the same municipality of their residence and the vast majority of them are assigned to a school in the same State as their middle school (93 percent in the Federal District and 79 percent in the State of Mexico, respectively.)

vis--vis the opposite effect of the update in mean beliefs on both the probability of graduation and the payoff function.

Since the value of the outside option is likely to be affected by the update in beliefs, another potential outcome of interest is the size of the individual portfolio. However, the model has ambiguous predictions on the effect of the feedback provided on the number of options due to the fact that updates in mean beliefs simultaneously affect V_{iO} and V_{iA}.

5 Beliefs and Track Choices: Evidence

5.1 Belief Updating

The estimates reported in columns 1 and 2 of Table 2 suggest that taking the exam without the provision of performance feedback does not generate any differential updating behavior. We can thus confidently focus on the comparison between the treatment and placebo groups in order to study the impacts of the intervention.

Table 2. Beliefs about Exam Performance: Average Treatment Impacts

Sample	Placebo & Control		Treatment & Placebo		
Dep. Var.	Mean Posterior	SD Posterior	Mean Posterior	SD Posterior	Abs.Gap
	(1)	(2)	(3)	(4)	(5)
Exam Taking	1.483	0.905			
	(1.281)	(0.626)			
Score Delivery			-7.525	-2.626	-6.596
			(0.945)	(0.420)	(0.642)
Mean of Placebo	75.61	17.45	75.61	17.45	19.59
Observations	1999	1999	2293	2293	2293
R-squared	0.129	0.041	0.287	0.083	0.290
Clusters	74	74	90	90	90

NOTE: Standard errors clustered at the school level are reported in parenthesis. Sample of ninth graders in schools that belong to the treatment group, the placebo group and the control group. All specifications include a set of dummy variables which correspond to the randomization strata, the score in the mock exam (columns 3-5) and the following set of pre-determined characteristics (see Table 1 for details): gender (male), previous mock-test, previous mock-test with results, attendance in preparatory course, morning shift, both parents in the household, parents with higher education, SES index (above median), currently working, plan to attend college, and a dummy for whether or not one of the above has a missing value.

Columns 3 and 4 show that students' subjective beliefs about their academic ability respond to the provision of information about *own* performance.[29] Conditional on taking the mock exam,

[29] The intervention provides applicants with a "bundled" signal, which comprises three separate pieces of information about performance in the admission test: i) the individual score on the mock test, ii) the average score on the mock test among applicants in the same class, and iii) the average score on the admission test among the previous cohort of applicants. Point iii) was mainly aimed at scaling the effects of i) and ii). As detailed in Appendix B, we use some

mean beliefs in the treatment group decrease on average by 7.5 points, while the standard deviation of beliefs goes down by about 2.6 points. Relative to the placebo group, these effects represent roughly a 10 percent and a 15 percent reduction in the mean and standard deviation, respectively.

The aggregate patterns shown in columns 3 and 4 of Table 2 mask the potential heterogeneous effects of the treatment depending on the direction of the update. Indeed, the estimated negative coefficient of the treatment on mean posteriors in the full sample can be explained by the fact that about 80 percent of the applicants in our sample have scores that are below their baseline mean beliefs. Irrespective of the direction of the update, column 5 in Table 2 confirms that the intervention closes the gap between expected and actual performance by 6.6 points, which is about a third of the mean in the placebo group.

Guided by the Bayesian set-up presented in Section 4.1, we next estimate the impact of the intervention on beliefs by the expected direction of the update and by initial priors. Column 1 in Table 3 shows that, on average, mean beliefs increase by about 2.8 points among applicants who receive positive feedback while they adjust downwards by 9.9 points among those receiving negative feedback. The estimates reported in columns 2 and 3 reveal that the apparent source of heterogeneity in the responses to positive and negative feedback can be explained by preexisting differences in the gap between expected and actual performance across these sub-samples (see Section 3.3).[30] Among those who receive positive feedback, the positive treatment impact on mean posteriors increases as the gap between the mock exam and the mean priors gets larger. On the other hand, the negative impact of the treatment among those who get a mock exam score below their mean priors is less stringent among those with smaller (less negative) initial gaps.

survey questions on students' self-perceptions of their relative ranking in the class in order to shed some light on the role of ii) vis--vis i) in explaining the estimated effects of the treatment on beliefs discussed in this Section. The results reported in Table B.1 document that the observed updating patterns are unlikely to be driven by changes in subjective expectations about relative ranking within the class.

[30] Figure A.2 further illustrates this point by plotting the final gap as a function of the initial gap, separately for the treatment and the placebo groups. Although the treatment symmetrically closes the gap for both negative and positive starting gaps, the distribution of the initial gap shows that those who get positive feedback have less room to adjust.

Table 3. Beliefs about Exam Performance: Heterogeneous Treatment Impacts

Dependent Variable	Mean Posterior			SD Posterior		
Treatment and Placebo Sample	All	Positive Feed-back	Negative Feed-back	All	Positive Feed-back	Negative Feed-back
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment × (Positive Feedback)	2.786			-3.623		
	(1.317)			(0.766)		
Treatment × (Negative Feedback)	-9.854			-2.423		
	(0.915)			(0.428)		
Positive Feedback	-14.533			3.104		
	(1.135)			(0.601)		
Treatment		-1.156	-2.340		-0.317	0.084
		(1.634)	(1.112)		(1.967)	(0.949)
Treat × (Mock Score-Mean Prior)		0.335	0.351			
		(0.130)	(0.053)			
(Mock Score-Mean Prior)		-0.515	-0.640			
		(0.111)	(0.035)			
Treat × (SD Prior)					-0.159	-0.140
					(0.100)	(0.055)
SD Prior					0.553	0.589
					(0.067)	(0.045)
Mean of Placebo	75.61	70.91	76.59	17.45	18.50	17.23
Observations	2293	441	1852	2293	441	1852
R-squared	0.35	0.51	0.46	0.10	0.36	0.38
Clusters	90	84	90	90	84	90

NOTE: Standard errors clustered at the school level are reported in parenthesis. Sample of ninth graders in schools that belong to the treated and the placebo group. Positive (negative) feedback applicants are defined as those with mean baseline beliefs that are lower (higher) than the realized value of the score in the mock exam. All specifications include a set of dummy variables which correspond to the randomization strata, the score in the mock exam and the following set of pre-determined characteristics (see Table 1 for details): gender (male), previous mock-test, previous mock-test with results, attendance in preparatory course, morning shift, both parents in the household, parents with higher education, SES index (above median), currently working, plan to attend college, and a dummy for whether or not one of the above has a missing value.

Estimates presented in column 4 show that the treatment also induces a reduction in the dispersion of individual beliefs, with a more pronounced effect among applicants receiving positive feedback (p-value for the null hypothesis of equality between the two coefficients equal to 0.12). Columns 5 and 6 show that differences in the dispersion of the priors between these sub-samples drive this result.

This evidence is thus broadly in line with (2) and (3) in Section 4.1. Belief updating seems to result from a convex combination of priors and the signal received: the larger the initial gap between mean priors and the signal received, the more extreme is the update in terms of the first moment of beliefs. Also, higher initial uncertainty in priors yield stronger updates in the second moment of beliefs.

5.2 Track Choices

The simple model exposed in Section 4 generates specific predictions depending on the direction of the update and the location of the applicant. These results come from the interplay between the first and second moments of the ability distribution in the updating process enabled by the information intervention.

Table 4 reports empirical evidence on the heterogeneous impacts of the treatment on the demand for the academically oriented high school programs. We first examine how the treatment impacts vary with the direction of the update. The OLS estimates reported in column 1 show that applicants receiving positive feedback in the treatment group increase their shares of requested academic options by 8.3 percent when compared to their counterparts in the placebo group. This is a substantial effect, as it corresponds to approximately 18 percent of the sample mean in the placebo group. In turn, the large reductions in mean beliefs observed amongst the applicants receiving negative feedback in the treatment group (see Table 3) does not appear to translate into any corresponding change in the demand for academically oriented programs.

We argue that the evidence of this differential response in track choices largely reflects the joint effect of changes in mean and variance across settings with different graduation standards in academic programs. In order to understand the role of the first and the second moments of the subjective ability distributions, we exploit variations in high school academic requirements across municipalities (see Figure 1). We rely on admission cutoffs as a proxy for graduation requirements in academic programs. Accordingly, we define municipalities with high academic requirements as those for which their median academic cutoff is above the average mean posterior in the experimental sample.[31]

[31] According to this definition, approximately 10 percent of the applicants in our sample live in municipalities classified as having high academic requirements. On average there are no differences in mean prior beliefs (T-stat=1.31), the standard deviation of prior beliefs (T-stat=-0.48), or mock score results (T-stat=0.752) between these applicants and those living in municipalities with low academic requirements.

Table 4. Treatment Impacts on High School Track Choices

Dependent Variable	Share of Academic Schools		
Treatment and Placebo Sample	All	Positive Feedback	Negative Feedback
	(1)	(2)	(3)
Treatment×(Positive Feedback)	0.083		
	(0.029)		
Treatment×(Negative Feedback)	-0.005		
	(0.017)		
Positive Feedback	-0.057		
	(0.022)		
Treatment		0.101	0.006
		(0.032)	(0.018)
Treatment×(High Requirements)		-0.174	-0.110
		(0.096)	(0.042)
High Requirements		0.009	0.088
		(0.052)	(0.023)
Mean of Placebo	0.51	0.46	0.52
Observations	2293	441	1852
R-squared	0.086	0.164	0.086
Clusters	90	84	90

NOTE: Standard errors clustered at the school level are reported in parenthesis. Sample of ninth graders in schools that belong to the treated and placebo groups. Positive (negative) feedback applicants are defined as those with mean baseline beliefs that are lower (higher) than the realized value of the score in the mock exam. All specifications include a set of dummy variables which correspond to the randomization strata, the score in the mock exam and the following set of pre-determined characteristics (see Table 1 for details): gender (male), previous mock-test, previous mock-test with results, attendance in preparatory course, morning shift, both parents in the household, parents with higher education, SES index (above median), currently working, plan to attend college, and a dummy for whether or not one of the above has a missing value.

Columns 2 and 3 in Table 4 show estimates of the treatment impacts on track choices by the stringency of the graduation requirements, confirming the predictions of the model. Those who receive positive feedback in low cutoff settings experience a large positive treatment impact (10 percent) in the share of academic options requested. However, the incentives to increase the demand for academic options enabled by mean updates is entirely offset in municipalities with high $q^{\star}_{A}$. Among those applicants who receive negative feedback we observe a significant reduction in the demand for academic options in the municipalities with high academic requirements, which is where the reduction in the variance reinforces the reduction in academic payoffs. Finally, the applicants who update their mean beliefs downwards but face more lenient graduation standards do not change the share of academic options they request.

It may be that students consider a broader market than the municipality where they reside in order to form expectations about the probability of graduating from high school. As a robustness check, we redefine the stringency of the academic requirements at the state level. Table A.1 presents the results when a dummy for residing Federal District is used to measure exposure to a higher $q_A^\star$, largely confirming the estimates reported in Table 4.

6 Further Evidence

6.1 Other School Choices

Table A.2 presents additional effects of the treatment on other margins of the schooling portfolios submitted by the applicants in our sample. We do not find any effect of the treatment on the average number of choices submitted (columns 1, 2 and 3). This evidence indicates that the effect of the intervention on the share of academic options is mainly the result of a "reshuffling" pattern within the application portfolios. The estimated coefficients reported in columns 2 and 3 of Table 4 imply an average composition effect in the school portfolios of roughly two schooling options, both for applicants who receive positive feedback in low-requirement settings and for those receiving negative feedback in high-requirement settings. We also discard any effects on the selectivity of the portfolio, as measured by the average admission cutoff of the options submitted (columns 4, 5 and 6 in Table A.2).

Beyond the school choices submitted within the assignment system, the process of belief updating induced by the intervention may trigger other behavioral responses of potential interest. For instance, it is likely that the observed changes in the perceived ability distributions may influence students' motivation to prepare for the actual admission exam.[32]

Table 5 shows that applicants who receive negative feedback reduce their scores by approximately 10 percent of a standard deviation with respect to the placebo group (column 1). Moreover, if we look at the heterogeneous impacts of the treatment, we find that those who receive positive feedback in municipalities with more lenient academic graduation requirements also experience discouragement effects in study effort due to the intervention (column 2). These students may not find it worthwhile to exert effort after they learn they performed better than expected in a setting with fairly low academic standards.

[32] A very small number (38) of applicants submit their preferences in the system but do not take the admission exam. This share corresponds to 1.6 percent of the sample, and it does not vary systematically with either the expected direction of the update or with the stringency of the academic requirements across states.

Table 5. Treatment Impacts on Study Effort

Dependent Variable	Score in the Admission Exam		
Sample	All	Positive Feedback	Negative Feedback
	(1)	(2)	(3)
Treat × (Positive Feedback)	-1.340		
	(1.096)		
Treat × (Negative Feedback)	-1.869		
	(0.848)		
Positive Feedback	-1.861		
	(0.852)		
Treatment		-2.318	-1.445
		(1.159)	(0.891)
Treatment × (High Requirements)		0.527	-3.267
		(5.089)	(3.156)
High Requirements		-5.009	3.934
		(1.861)	(2.831)
Mean of Placebo	64.93	78.41	62.05
Observations	2253	437	1816
R-squared	0.713	0.752	0.654
Clusters	90	84	90

NOTE: Standard errors clustered at the school level are reported in parenthesis. Sample of ninth graders in schools that belong to the treated and placebo groups. Positive (negative) feedback applicants are defined as those with mean baseline beliefs that are lower (higher) than the realized value of the score in the mock exam. All specifications include a set of dummy variables which correspond to the randomization strata, the score in the mock exam and the following set of pre-determined characteristics (see Table 1 for details): gender (male), previous mock-test, previous mock-test with results, attendance in preparatory course, morning shift, both parents in the household, parents with higher education, SES index (above median), currently working, plan to attend college, and a dummy for whether or not one of the above has a missing value.

6.2 Schooling Outcomes

The analysis discussed so far conveys the key message that the intervention under study is bound to generate a variety of opposing treatment impacts on school choices along several dimensions—i.e., the direction of the update in beliefs and the perceived academic standards in high school. In fact, Table 6 shows that there are no average treatment impacts on track choices, admission, or high school outcomes. Nevertheless, the intervention seems to increase the sensitivity of the demand for academic programs with respect to performance in the mock exam. Compared to students in the placebo group, a one standard deviation increase in the score in the mock exam is associated with an increase of 4.1 percentage points in the share of academic options requested among treated students (see interaction effect in column 1, Table 6). This effect amounts to an increase of 8 percentage points with respect to the sample mean or, alternatively, a change of approximately one schooling option in the portfolio of the average applicant. Estimates in column 2 show that a similar pattern holds for the probability of admission into an academic school, conditional on assignment in the system. Since we found small effects on the scores in the admission exam (see Table 5), this effect is mostly driven by the underlying changes in preferences on tracks induced by the intervention.[33]

Table 6. Track Choices, Admission, and High School Outcomes

Sample	Treatment & Placebo		
Dependent Variable	Share Academic	Admission Academic	High School Drop-out
	(1)	(2)	(3)
Treatment×Mock Score (z-score)	0.041	0.059	-0.017
	(0.013)	(0.027)	(0.017)
Treatment	0.012	-0.026	0.022
	(0.016)	(0.026)	(0.021)
Mock Score (z-score)	-0.016	0.004	-0.062
	(0.009)	(0.022)	(0.013)
Mean Dependent Variable	0.518	0.477	0.148
Number of Observations	2293	2045	1530
R-squared	0.087	0.067	0.083
Number of Clusters	90	90	90

NOTE: Standard errors clustered at the school level are reported in parenthesis. Sample of ninth graders (columns 1 and 2) and tenth graders (columns 3) that are assigned to the treatment and the placebo group. Track assignment in column 2 is defined conditional on assignment within the system. Drop-out in column 3 is defined conditional on enrollment in the high school assigned through the system. All specifications include a set of dummy variables which correspond to the randomization strata, the score in the mock exam and the following set of pre-determined characteristics (see Table 1 for details): gender (male), previous mock-test, previous mock-test with results, attendance in preparatory course, morning shift, both parents in the household, parents with higher education, SES index (above median), currently working, plan to attend college, and a dummy for whether or not one of the above has a missing value. The specifications in columns 3 and 4 further include fixed effects at the high school level.

To the extent that academic options likely require higher academic standards, students who substitute away from vocational and technical programs in favor of academically oriented options

[33] Unconditionally on the track, the likelihood of assignment within the system (11 percent) does not vary systematically either with the treatment or with its interaction with the score in the mock exam (results not reported but available upon request).

may be penalized in terms of subsequent academic achievement.[34] The evidence reported in column 3 of Table 6 suggests that this is not the case in our setting. In fact, as the alignment between academic skills and placement outcomes improves, dropout rates are also reduced, as shown by the negative sign for the interaction between mock exam score and treatment. In other words, as measured academic ability increases, the treatment drives up the demand for academic options and the probability of getting into a school from the academic track while reducing the probability of dropping out during the first year of high school. Although not statistically significant possibly due to the smaller number of observations in the high school records, this result may be indicative of the potential better match between students' academic skills and their education decisions enabled by the intervention.[35]

7 Conclusion

Investments in schooling occur early in the life cycle and have long term consequences in the labor market. A lack of adequate information about students' academic potential may partly explain poor educational outcomes by preventing some households from taking full advantage of schooling opportunities. This is a particularly important issue in our setting and in many other developing countries, where education is often the most common bet on social and economic mobility.

In this paper, we document the results from a field experiment that provides youth with individualized information on their own academic potential, which is meant to effectively alter career decisions during a critical period of their schooling trajectories. Our findings show that students face important informational gaps related to their own academic potential and that closing these gaps has a sizable effect on track choices in high school. The intervention successfully aligns academic skills and curricular choices, with better performing students being assigned into more academically oriented high school programs and experiencing lower dropout rates one year after admission. Taken together, this evidence underscores the potential for longer-term impacts of the intervention on academic trajectories and possibly labor market outcomes.

This study is one of the first to provide evidence on the differential role that the first two moments of the belief distribution play in determining schooling investment decisions. Both theoretically and empirically, we show that updates in the precision of beliefs are pivotal in explaining the effect of subjective expectations on educational choices. In particular, we show that ignoring the changes in the dispersion of individual beliefs may systematically confound the effects of individualized information on academic ability on high school track choices.

[34] Previous studies document a negative effect on grades and/or wages among minority students who end up going to selective majors or colleges due to affirmative action preferences (e.g., Bertrand, Hanna, and Mullainathan (2010) and Frisancho and Krishna (2015)).

[35] Administrative data (Secretaria de Educacion Publica, 2012) shows that 61 percent of dropout during the upper secondary level in Mexico occurs in the first grade.

Our results highlight the potential role of policies aimed at disseminating information about individual academic skills in order to provide students with better tools to make well-informed curricular choices. In the particular context we study, a cost-effective way to scale up the intervention under study may be to reverse the timing of the application process, allowing applicants to choose their preferred schools after taking the admission exam and receiving their scores. An alternative policy may be to incentivize middle schools to implement mock tests and deliver score results before students submit the rank-ordered lists of schools within the centralized assignment mechanism.

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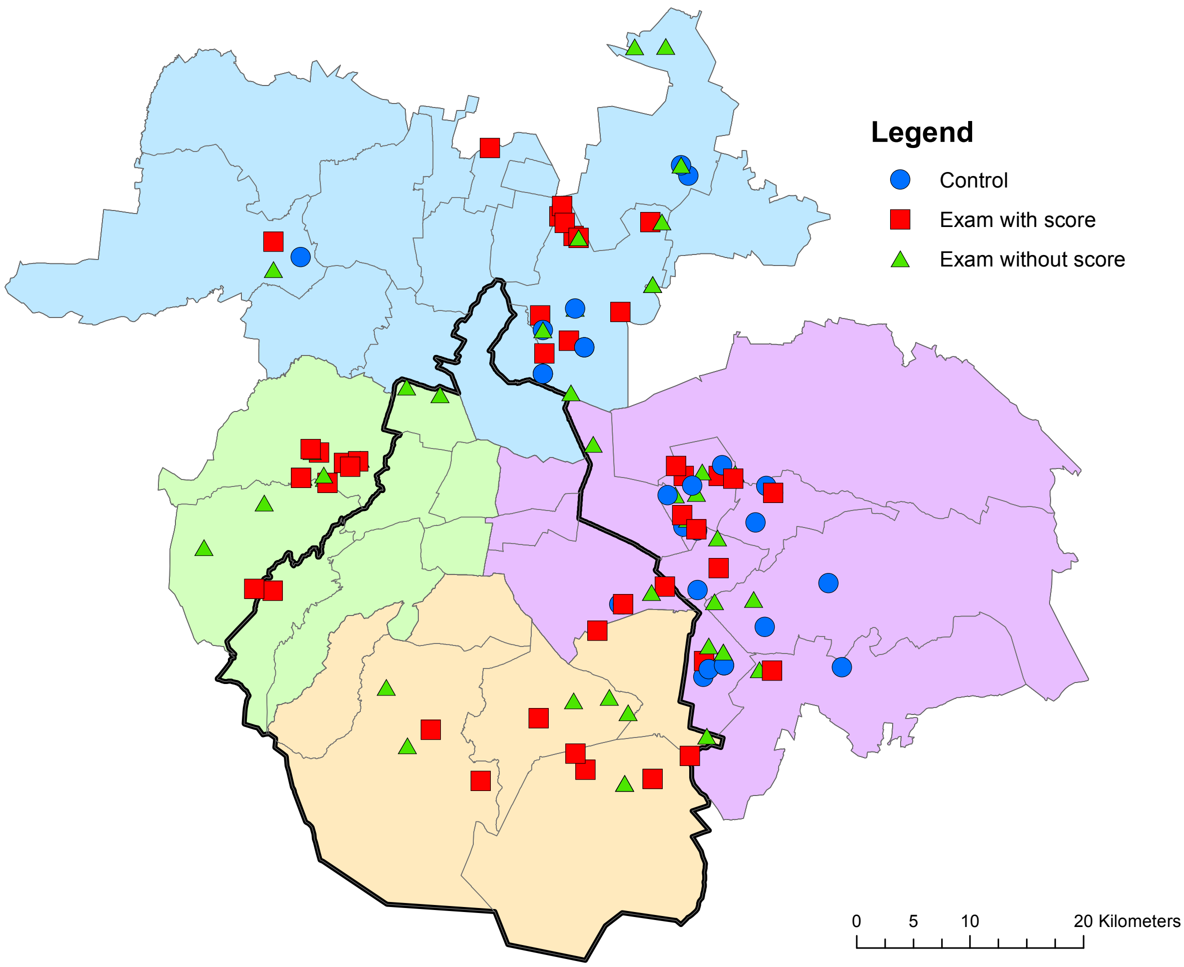
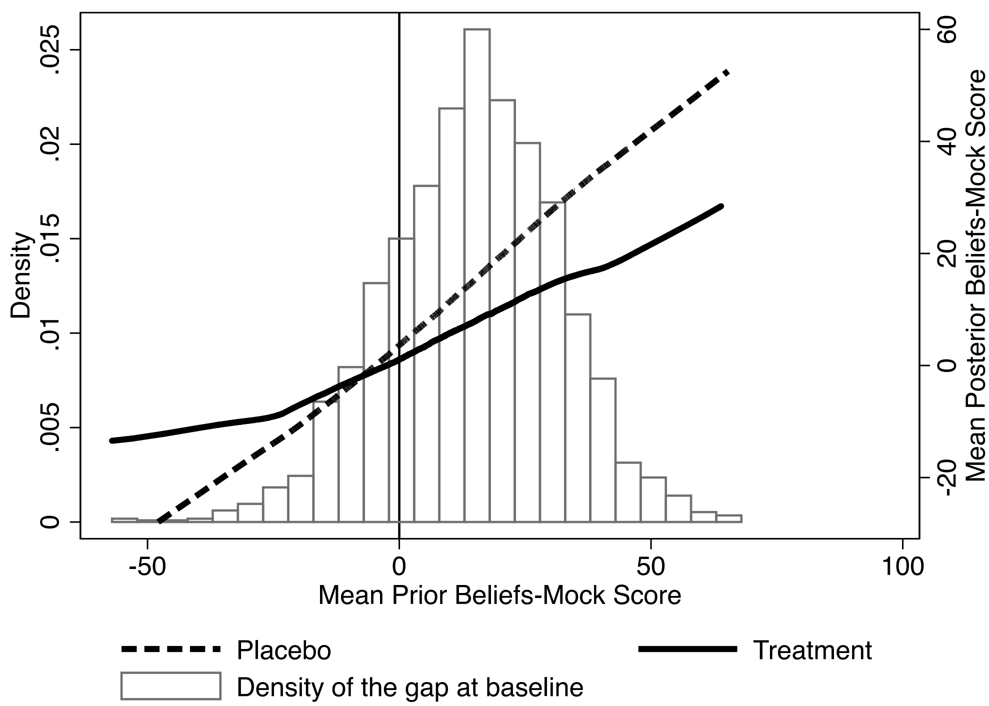
A Additional Figures and Tables

Figure A.1. The Metropolitan Area of Mexico City and the Schools in the Sample



Note: The thick black line denotes the geographic border between the Federal District and the State of Mexico. The thin grey lines indicate the borders of the different municipalities that participate in the COMIPEMS system. The four geographic regions that, combined with discrete intervals of school-average achievement scores, form the basis of the twelve strata underlying the stratification procedure described in Section 3 are shaded in different colors.

Figure A.2. Change in Gaps between Expected and Realized Performance



Source: Survey data and COMIPEMS administrative data, 2014.

Note: The histogram shows the empirical density of the gap between expected and realized performance. The overlaid lines are non-parametric estimates - based on locally weighted regression smoothers—of the relationship between the gap in expected and realized performance, before and after the treatment delivery.

Table A.1. Treatment Impacts on High School Track Choices: Academic Requirements at the State Level

Dependent Variable	Share of Academic Schools	
Sample	Positive Feedback	Negative Feedback
	(1)	(2)
Treatment	0.116	0.019
	(0.033)	(0.019)
Treat×(Federal District)	-0.118	-0.095
	(0.068)	(0.033)
Federal District	0.152	-0.017
	(0.065)	(0.034)
Mean Dep. Var. in Placebo	0.46	0.52
Number of Observations	441	1852
R-squared	0.170	0.091
Number of Clusters	84	90

NOTE: Standard errors clustered at the school level are reported in parenthesis. Sample of ninth graders in schools that belong to the treated and placebo groups. Positive (negative) feedback applicants are those with mean baseline beliefs that are lower (higher) than the realized value of the score in the mock exam. All specifications include a set of dummy variables which correspond to the randomization strata, the score in the mock exam and the following set of pre-determined characteristics (see Table 1 for details): gender (male), previous mock-test, previous mock-test with results, attendance in preparatory course, morning shift, both parents in the household, parents with higher education, SES index (above median), currently working, plan to attend college, and a dummy for whether or not one of the above has a missing value.

Table A.2. Treatment Impacts on Other Characteristics of School Portfolios

Dependent Variable	Number of Options			Average Cutoff		
Sample	All	Positive Feedback	Negative Feedback	All	Positive Feedback	Negative Feedback
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment × (Positive Feedback)	-0.106			1.547		
	(0.348)			(1.752)		
Treatment × (Negative Feedback)	0.095			0.540		
	(0.238)			(1.031)		
Positive Feedback	-0.400			-3.481		
	(0.241)			(1.284)		
Treatment		0.030	0.151		1.927	0.882
		(0.374)	(0.243)		(1.913)	(1.092)
Treatment × (High Requirements)		0.229	-0.635		-5.056	-1.598
		(1.336)	(0.958)		(4.772)	(2.599)
High Requirements		0.537	0.131		1.008	6.246***
		(0.730)	(0.458)		(3.141)	(1.464)
Mean of Placebo	0.51	0.46	0.52	0.51	0.46	0.52
Number of Observations	2293	441	1852	2293	441	1852
R-squared	0.046	0.050	0.057	0.331	0.388	0.327
Number of Clusters	90	84	90	90	84	90

NOTE: Standard errors clustered at the school level are reported in parenthesis. Sample of ninth graders in schools that belong to the treated and placebo groups. Positive (negative) feedback applicants are defined as those with mean baseline beliefs that are lower (higher) than the realized value of the score in the mock exam. All specifications include a set of dummy variables which correspond to the randomization strata, the score in the mock exam and the following set of pre-determined characteristics (see Table 1 for details): gender (male), previous mock-test, previous mock-test with results, attendance in preparatory course, morning shift, both parents in the household, parents with higher education, SES index (above median), currently working, plan to attend college, and a dummy for whether or not one of the above has a missing value.

B Relative Vs. Absolute Updating

Using the information from the socio-demographic survey completed upon registration, we can measure students' self-perceptions about their relative standing in the classroom in four courses: Math, Spanish, History, and Biology. These four variables are given values of -1, 0, and 1 depending on the student classifying himself as below, as good as, and above other students in the classroom. We construct a composite measure of initial relative ranking and sum up over these variables to classify students into three groups of initial relative beliefs. If the sum is negative, students are assumed to have individual beliefs below the expected classroom mean whereas those with positive sums are assumed to have individual beliefs above their classroom mean. We can then compare these initial relative beliefs to each applicant's actual ranking in the classroom based on their performance in the mock exam, and identify the group of right-updaters and left-updaters in terms of relative beliefs.[36]

According to this definition, 16 percent of the applicants in the placebo group are classified as right-updaters. Among those, only 31 percent are also classified as right-updaters in terms of their individual beliefs, indicating the presence of substantial discrepancies between the two definitions. Table B.1 presents the results from OLS regressions similar to the ones reported in columns 1 and 2 in Table 3, but now we add indicator variables for relative updating behaviors as well as their interaction terms with the treatment variable (the excluded category is the no relative update status).[37] The estimates reveal the presence of some updating in relative terms, notably for those who reported themselves as better than the average in their class. However, the main treatment impacts through changes in perceptions of own performance on the test in absolute terms are very similar to the ones reported in columns 1 and 2 in Table 3, in both magnitude and precision. This evidence suggests that updating in absolute terms induces a direct change in the individual belief distribution far beyond the indirect effect that changes in relative beliefs may have.

[36] More precisely, those variables are constructed as follows. Students with positive (negative) relative beliefs whose mock exam score is either between 5 points above and 5 points below the average in the classroom or 5 points below (above) the average are assumed to be more likely to update downward (upward) in terms of their relative ranking. Students with expected relative rankings that are consistent with their ranking in the distribution of the mock exam score in the classroom are considered to be non-updaters.

[37] We lose 162 observations (7 percent of the sample) due to missing values in the students' self-perceptions variable collected in the registration survey. As expected though, the treatment is orthogonal to the resulting censoring in the estimation sample.

Table B.1. Relative Updating: Treatment Impacts on Posterior Beliefs

Sample	Treatment & Placebo	
Dep. Var.	Mean Posterior	SD Posterior
	(1)	(2)
Treatment × Positive Feedback	3.085	-3.638
	(1.331)	(0.813)
Treatment × Positive Feedback	-8.257	-2.303
	(0.967)	(0.581)
Positive Feedback	-13.798	3.400
	(1.189)	(0.619)
Treatment × (Positive Feedback - class)	0.575	-0.918
	(1.310)	(0.894)
Treatment × (Negative Feedback - class)	-4.143	-0.092
	(1.273)	(0.807)
Positive Feedback - class	-1.966	-0.404
	(0.979)	(0.538)
Negative Feedback - class	4.816	0.236
	(0.942)	(0.607)
Mean Dependent Variable	75.61	17.45
Number of Observations	2131	2131
R-squared	0.37	0.10
Number of Clusters	90	90

NOTE: Standard errors clustered at the school level are reported in parenthesis. Sample of ninth graders in schools that belong to the treated and the placebo group. Positive (negative) feedback applicants are those with mean baseline beliefs that are lower (higher) than the realized value of the score in the mock exam. Positive (negative) feedback - class applicants are those with mock exam score that is either between 5 points above and 5 points below the average in the classroom or 5 points below (above) the average relative to students' self-perceptions about their standing in the classroom. All specifications include a set of dummy variables which correspond to the randomization strata, the score in the mock exam and the following set of pre-determined characteristics (see Table 1 for details): gender (male), previous mock-test, previous mock-test with results, attendance in preparatory course, morning shift, both parents in the household, parents with higher education, SES index (above median), currently working, plan to attend college, and a dummy for whether or not one of the above has a missing value.