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Learning About Oneself: The Effects of Signaling Academic Ability on School Choice*

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Abstract

This paper examines the role of perceived academic ability in shaping curricular choices in secondary school. We design and implement a field experiment that provides individualized feedback on performance in a mock version of the admission test taken to gain entry into high school in the metropolitan area of Mexico City. This intervention reduces the gap between expected and actual performance, shrinks the variance of the individual ability distributions and shifts stated preferences over high school tracks, with better performing students choosing more academically-oriented options. Such a change in application portfolios affects placement outcomes within the school assignment system, while it does not seem to entail any short-term adjustment costs in terms of high school performance. Guided by a simple model in which Bayesian agents choose school tracks based on their perceived ability distribution, we empirically document the interplay between variance reductions and mean changes in beliefs enabled by the information intervention.

Keywords: information, Bayesian updating, biased beliefs, school choice.

JEL Codes: D83; I21; I24; J24.

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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. Recent studies document how access to information about school characteristics, labor market returns, and financial aid, among other factors, affect education choices.[1] However, few studies focus on the role of *perceived* individual traits as a potentially key determinant of schooling returns and investments.[2]

This paper attempts to understand how individual perceptions about own academic ability shape schooling choices. This is an important issue since distorted beliefs may generate a mismatch between students' academic skills and their human capital investments, with long-lasting consequences in the labor market. We overlay a field experiment within the assignment mechanism to allocate students into public high schools, currently taking place in the metropolitan area of Mexico City. Two features of this setting are crucial for our design. First, the assignment system is regulated by strict and observable criteria, that is, stated preferences over schools and admission test scores. Second, applicants are required to submit their ranked 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 in the area, since they are less likely to have access to previous informative signals about their own academic potential. Although preparatory courses are relatively popular in this setting, the supply is mostly private and requires out-of-pocket expenditure 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 about individual performance therein. In order to distinguish between the effect of taking the test and the effect of receiving performance

[1]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*a*]; Wiswall and Zafar [2015*b*]; Hastings et al. [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, while 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 this paper. Finally, Hoxby and Turner [2014]; Carrell and Sacerdote [2013]; Dinkelman and Martinez [2014] study information interventions about application procedures and financial aid opportunities.

[2]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 et al. [2016].

feedback, we include a pure control group in which students do not take the mock exam. We communicate individual scores to a randomly chosen subset of applicants and observe how this information shock affects subjective expectations about academic ability, choices over high school tracks, and later academic trajectories.

Before the intervention, there are large discrepancies between expected and actual performance in the test. Providing feedback about 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 the individual belief distributions.

Applicants in the treatment group shift their stated preferences over high school tracks, with better performing students systematically opting for more academically-oriented options in their application portfolios. The resulting improvement in the alignment between academic skills and curricular choices also affects admission outcomes within the assignment system, suggesting the scope for longer-term impacts of the intervention on schooling and labor market trajectories. Finally, we do not detect any significant effects of the intervention on schooling outcomes at the end of the first year of high school. This suggests that students who substitute away from vocational and technical high school programs in favor of academically-oriented options are not penalized in terms of short run academic achievement.

To better understand the mechanisms at play, we develop a simple Bayesian learning model that incorporates the role of the expected value of the ability distribution in the returns from the academic track, as well as the role of the dispersion of beliefs in the probability of successfully getting into and graduating from that track. The model predicts that the impact of changes in mean ability on track choices is monotonic. However, changes in the precision of the perceived ability distribution can either enhance or dilute the effects of mean updating, depending on the location of the mean relative to the minimum ability cutoff required to succeed in the academic track. Put simply, more precise self-views may lead to lower (perceived) chances of success in the academic track whenever the associated schooling requirements are high enough.

We empirically document the implications of the interplay between the first and second moments of the belief distribution. To do so, we exploit the existing variation in the stringency of the academic requirements within the metropolitan area of Mexico City. Our results confirm that variance reductions systematically confound upward or downward changes in mean beliefs. Accordingly, the effect of a positive updating is only found in areas with more lenient academic requirements, whereas the effect of a negative updating is concentrated in areas with more stringent requirements.

This is one of the few papers to provide experimental evidence on the role of subjective beliefs about academic ability on educational choices. 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. In the context of one school in the US, Bergman [2015] studies how removing information frictions between parents and their children affect academic achievement. Relying on observational data, Arcidiacono et al. [2012]; Stinebrickner and Stinebrickner [2012, 2014] document the role of beliefs about future performance on college major choices and drop-out 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 documenting the differential roles of the mean and the variance of the individual belief distribution as a potential channel behind the observed responses to individualized information about academic skills.

This paper is also related to a long-standing theoretical work on the formation and consequences of self-perceptions in the context of Bayesian learning models (see, e.g., Carrillo and Mariotti [2000]; Bénabou and Tirole [2002]; Zabojnik [2004]; Köszegi [2006]). Some empirical studies document the widespread presence of the overestimation of positive individual traits such as intelligence and beauty [Dunning et al., 2004; Moore and Healy, 2008; Eil and Rao, 2011]. However, there is little evidence to date on how these self-perceptions affect real-world decisions. One recent exception is Reuben et al. [2015], who study the role of overconfidence in explaining gender differences in college major choices. This paper takes a step along those lines by linking survey-elicited measures of the distribution of beliefs about own academic ability to administrative data on high-stake schooling choices and outcomes.

2 Context

2.1 The COMIPEMS 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 places students in schools based on a single standardized achievement exam.[3] In 2014, the COMIPEMS system offered over 238,000 seats in 628 public high schools.

The timeline is as follows. 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, education modality, and specialties, if applicable). Past cut-off scores for each school-specialty in the previous three years are provided in the COMIPEMS website.

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. Requesting the submission of preferences before taking the exam is intended to help the system plan ahead for 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. Whenever ties occur, the 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 an applicant is not satisfied with his placement, he can search for another option in the same way unassigned applicants do.[4]

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 over

[3]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 of 7.0 cumulative grade point average (GPA) in junior high school.

[4]Clearly, 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.

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 [Chen and Pereira, 2015].[5]

2.2 High School Tracks

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 academic 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; for example, in technical and vocational schools, students also choose a specialization.

All three modalities are conducive to tertiary education, although 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, and are more likely to be working after graduating from high school when compared to students who attended the academic track.[6]

The high school programs that are made available through the system are geographically accessible, although there is some variation across neighborhoods. 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% of the options (63 schools) are located at most 10 miles away from the school of origin. Beyond geographic proximity, individual preferences and other school attributes may significantly reduce the applicants' set of feasible and desirable schools, and this may explain why most students do not fill the 20 slots available in the preference lists.

[5]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 ranked ordered lists.

[6]Among graduates from the technical or vocational tracks, the probability of enrolling in college is 33 and 38 percentage points below that of graduates from the academic track, respectively. Similarly, compared to graduates from the academic track, graduates from the technical or vocational tracks are 6 and 19 points more likely to enter the labor force with a high school diploma, respectively.

The system naturally generates ability sorting across schools due to the assignment algorithm, based on admission scores and preferences. 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.

2.3 The Role of (Biased) Beliefs on School Choices

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. We provide evidence on two fronts to justify the intervention under study: (i) students have biased beliefs about their academic ability; and (ii) school choices are driven in part by these biased beliefs. These two facts combined may generate misallocations of students across school tracks.

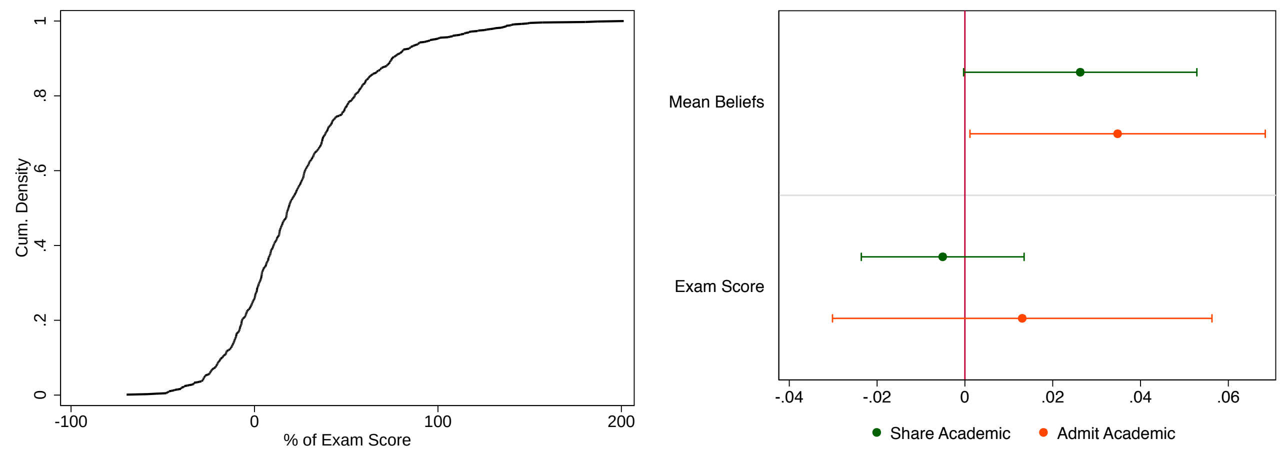
We start by showing how expected performance early in the application process compares to actual performance in the exam. Using data from a pure control group that is not exposed to the intervention (see Section 3.3), panel (a) of Figure 1 plots the cumulative density of the gap between mean beliefs (see Section 3.2) and scores in the admission exam as a percentage of the score. Approximately three quarters of the students in the pure control group expect to perform above their actual exam score. While the average student has a 25% gap relative to his actual admission exam score, the average gap among those with upward biased beliefs is more than double that of students with downward biased beliefs.[7]

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 the control group. Panel (b) of Figure 1 reports the estimated coefficients, and the 95% confidence intervals from regressions of preference and admission outcomes on mean beliefs and performance in the admission exam. The results in green correspond to the regression of the share of academic options listed, while the results in orange correspond to the regression of admission into the academic track. Mean beliefs have a positive, albeit fairly small, effect on students' demand for academic schools: a one standard deviation increase in expected test performance is associated with an average increase in the share of requested academic options of 2.6 percentage points. On the contrary, the estimated coefficient for actual test performance is close to zero. This pattern also holds when we focus on admission outcomes.

[7]Figure A.1 in Appendix shows that students with the lowest scores tend to have upwardly biased beliefs while best performing students have mean beliefs below actual performance. Both upward and downward biases are observed for intermediate levels of exam score.

A one standard deviation increase in expected test performance is associated with an increase of 3.5 percentage points in the probability of being admitted into the academic track. The lack of a significant correlation with the exam score suggests that biased beliefs not only influence school application portfolios but that they also have real consequences on later trajectories. Students who *think* they are good enough to go to academic programs demand them relatively more often, so that even after conditioning on performance in the exam, they are more likely to get into one such program.[8]

Figure 1: Expected Ability and School Choices



(a) Gap between Expected and Actual Exam Score

(b) Track Choice and Placement

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) displays the standardized OLS coefficients and the 95% confidence intervals of mean beliefs and exam scores on the shares of high school academic programs and on an indicator variable that equals one if the applicant gains admission into an academic program. Source: Survey data (February, 2014) and COMIPEMS administrative records (2014).

3 The Intervention

3.1 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

[8]Applicants could be aware of their academic potential but still choose to attend technical or vocational tracks due to a better match with other unobserved individual traits. If this were the case, the provision of feedback about academic performance (and the associated change in beliefs) would have little effect on high school track choices.

duration (three hours). The exam had 128 multiple-choice questions worth one point each, without negative marking for wrong answers.[9] 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.[10]

We argue that the achievement measure that we provided was easy to interpret for the applicants while providing additional and relevant information about their academic skills. On one hand, the intervention took place after all informative and application materials had been distributed (see Section 2.1 and Figure 3). Those materials provide prospective applicants with detailed information about the rules, content, structure and difficulty of the admission exam.

On the other hand we can show that, beyond other skill measures most readily and freely available during the application period such as the grade point average (GPA) in middle school, the mock exam score is a good predictor of future academic performance.[11] Figure 2 displays the standardized OLS coefficients for the effects of the GPA in middle school, and the score in the mock exam on the GPA at the end of the first year of high school for those applicants in our sample who took the mock exam without receiving feedback (i.e., the placebo group—see Section 3.3). Although there is a very high correlation between past and current grades, the role of the GPA is not differentially informative about performance in high school by track. On the contrary, the score in the mock exam is mostly predictive of future performance in the academically-oriented track. A one standard deviation increase in the score is associated with an increase of 0.4 points in the GPA for those students enrolled in academic programs, which roughly corresponds to an increase of 6% with respect to the mean in the sample.

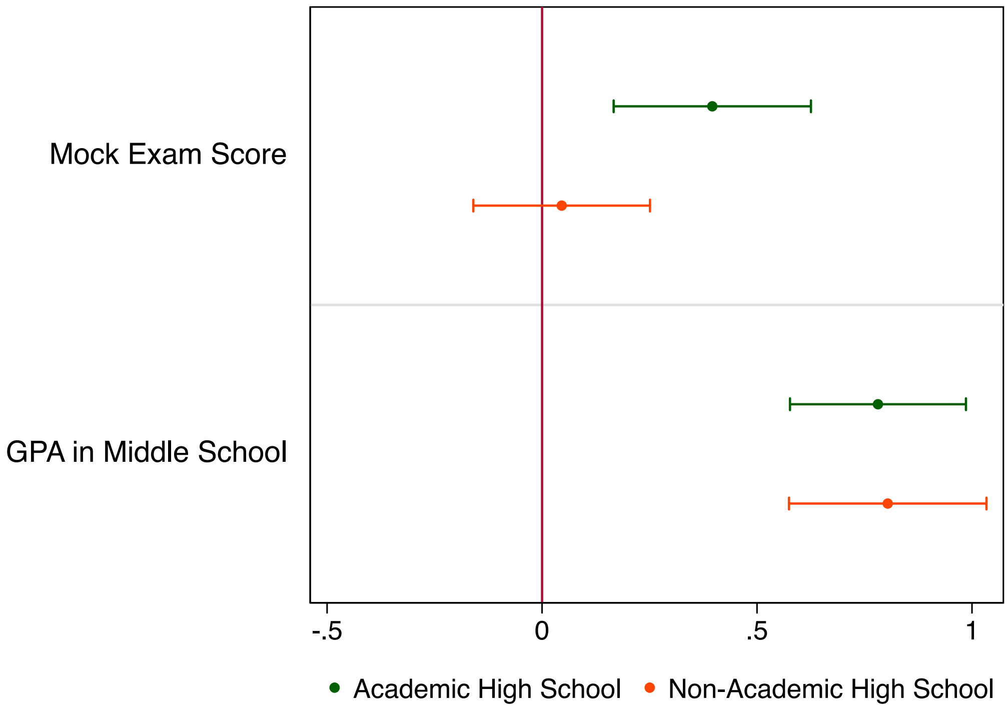
The delivery of the individual scores took place at the beginning of the follow up survey (see Section 3.2). Surveyors showed a personalized graph with two pre-printed bars: the average score in the universe of applicants during the 2013 edition of the COMIPEMS system;

[9] 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.

[10] In order to guarantee that the mock test was taken seriously, we inform students, their parents, and the principals of the schools in the sample about the benefits of additional practice for the admission exam. We also make sure that the school principal sends 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 in basic schooling levels. The sample correlation between performance in the mock exam and the actual exam is 0.82.

[11] After controlling for school fixed effects, the linear correlation between the GPA in middle school and the score in the mock exam is 0.25.

Figure 2: Predictors of High-School GPA (1st year)



Note: Standardized OLS coefficients of a regression of the GPA at the end of the first year in high school on the GPA in middle school and the score in the mock exam, controlling for high school fixed effects. Source: Survey data (February, 2014), COMIPEMS administrative records (2014), and high school records (2015).

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 in the mock exam.[12]

3.2 Data and Measurement

The main source of data used in this paper are administrative records from the COMIPEMS assignment process. At the individual level, we link the socio-demographic survey filled out at registration,[13] the full ranked list of schooling options requested, the score in the admission exam, the cumulative GPA in middle school, and placement outcomes. We further collect individual-level data on attendance and grades by subject for the academic year 2014-2015 for those applicants enrolled in one of the COMIPEMS high schools.[14]

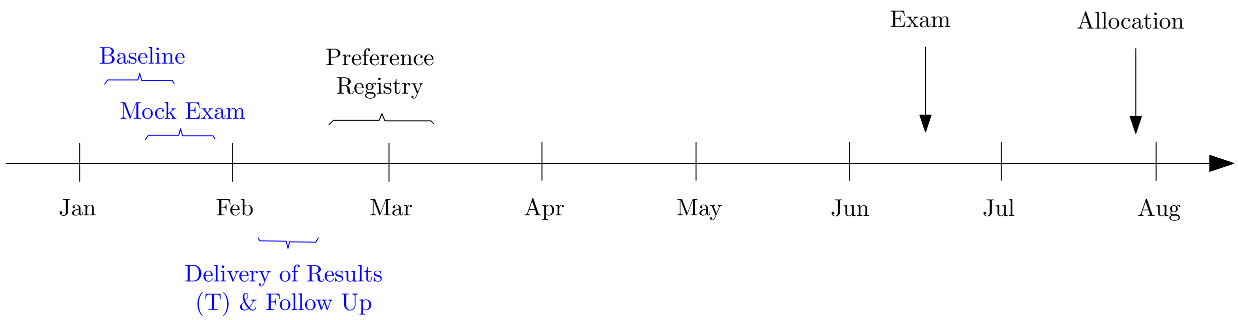
[12]Both the elicitation of beliefs about exam performance and the delivery of the score occurred in private in order to avoid social image concerns when reporting [Ewers and Zimmermann, 2012].

[13]The form collects information on gender, age, household income, parental education and occupation, personality traits, and study habits, among others.

[14]About 25% 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

We complement this information with the individual scores in the mock exam and two rounds of survey data. Figure 3 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. In both surveys, we collected detailed data on the subjective distribution of beliefs about performance in the exam.

Figure 3: The School Assignment Process and the Intervention: Timeline of Events



Note: COMIPEMS rules in place in 2014.

In order to help students understand probabilistic concepts, we relied on the use of visual aids [Delavande et al., 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 zero probability to a given event and 20 beans means that the student believes the event will occur for sure.[15]

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

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such discrepancy, most of it is driven by non-enrollment.

[15] 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%) made mistakes in these check questions. Whenever students made mistakes, the surveyor reiterated the explanation as many times as was necessary before moving forward.

allocate the 20 beans across the six intervals so as to represent the chances of scoring in each bin.[16] The survey question reads as follows (authors' translation from Spanish):

> "Suppose that you 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.

3.3 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 about their academic performance.[17]

Schools that comply with those criteria are grouped into four geographic regions (see Figure A.2) 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 resulting 12 strata. Some strata that are less dense participate with less 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 fall backs 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

[16]During the pilot activities, we tested different versions with less 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.

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

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 informing about 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.[18] 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 survey and exam records. The final sample consists of 3,100 students who can be matched with the COMIPEMS administrative data.[19]

3.4 Descriptives

Table A.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. Before the intervention, about 30% of the students in our sample had taken at least one COMIPEMS mock exam and roughly half of these had received feedback about their performance therein. By the time of the submission of the preference lists, approximately half of the students had attended preparatory courses for the admission exam. About a third declared themselves to be working (either paid or unpaid), and only 12% have parents with complete higher education. Almost 70% of the students in our sample plan to go to college.

The median student in the placebo group applies to 10 schooling options, slightly fewer than 10% of the applicants in that group request less than five options while under 2% fill all of the 20 school slots. The average portfolio share of academic options is 51%, while the average share of technical and vocational options are 37% and 12%, respectively. Roughly two thirds of the applicants are assigned to a school within their top four choices.

About 14% of the applicants in the placebo group remain unplaced after the matching algorithm described in Section 2.1, and 3% are subsequently admitted into one of the schooling options with remaining slots available. Among placed applicants, 47% are admitted into

[18] As shown in Figure A.2, some strata are not populated for this group.

[19] The 10% discrepancy between the survey data and the administrative data reflects applicants' participation decisions in the COMIPEMS system.

a school from the academic track, 36% gain admission into the technical track and the remaining 17% gain admission into the vocational track. The vast majority of the applicants in the placebo group are assigned to a high school that is located in the same State as their middle school (93% in the Federal District and 79% in the State of Mexico), indicating a large degree of segmentation across the two schooling markets.

High school records for the placebo group reveal that enrollment conditional on assignment does not vary across tracks, although it is much higher in the Federal District than in the State of Mexico. While 87% of the applicants who are initially assigned to a school in the Federal District enroll in that school, only 68% of the applicants in the State of Mexico do so.[20] Among enrolled applicants in the placebo group, we find that 14% drop out and 19% are held back during the first year of high school. Dropout rates are relatively higher (24%) in the vocational track.

4 A Bayesian Learning Model

4.1 Belief Updating

Academic ability is a draw q_i from an individual-specific distribution:[21]

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

Students do not observe their own ability directly, although they know its underlying distribution. Measures of academic performance (e.g., school grades, standardized test scores) and 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 $\epsilon_i \sim N(0, \sigma_\epsilon^2)$.[22] We assume that each signal leads agents to update their beliefs in a

[20]The lower enrollment rates in the State of Mexico are in part due to the access to other public schools outside the COMIPEMS process.

[21]We take as given the first and second moments of the individual distribution of ability at a certain point in time. After controlling for actual test performance, mean beliefs in the baseline survey correlate positively and significantly with the GPA in middle school, the reported weekly hours of homework study, one personality trait that proxies perseverance, a composite wealth-index based on household durable goods, a dummy for whether or not applicants expect to attend college and an index of students' subjective ranking in their class. No systematic relationship is found with respect to the gender of the applicants.

[22]In practice, the perceived noisiness of each signal may vary across individuals. We do not consider this source of heterogeneity in the model to the extent that it is not observable in the data and it does not systematically alter any of the predictions.

Bayesian fashion:

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

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

The sign of $(s_i - \mu_i)$ in Equation 2 determines the direction of the update, e.g., students who perform better than expected update their mean beliefs upwards while those who do worse than expected adjust their mean beliefs downwards. Henceforth, we refer to students who score higher than they expected as "upward-updaters" and label students who did worse than they expected as "downward-updaters". Equation 3 shows that the posterior variance $\sigma_i^{2'}$ 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 (i.e., $\sigma_\epsilon^2 = \sigma_i^2$) halves the variance of the prior regardless of the value of s_i.

Although every signal generates an update, the magnitude of the change in posterior beliefs depends on individual priors according to the following expressions:

$$\frac{\partial \mu_i^{'}}{\partial \mu_i} \quad = \quad 1 - \frac{\sigma_i^2}{(\sigma_i^2 + \sigma_\epsilon^2)} \geq 0 \tag{4}$$

$$\frac{\partial \mu_i^{'}}{\partial \sigma_i^2} \quad = \quad (s_i - \mu_i)\left[\frac{\sigma_\epsilon^2}{(\sigma_i^2 + \sigma_\epsilon^2)^2}\right] \geq 0 \text{ if } (s_i - \mu_i) \geq 0 \tag{5}$$

$$\frac{\partial \sigma_i^{2'}}{\partial \mu_i} \quad = \quad 0 \tag{6}$$

$$\frac{\partial \sigma_i^{2'}}{\partial \sigma_i^2} \quad = \quad \frac{\sigma_\epsilon^4}{(\sigma_i^2 + \sigma_\epsilon^2)^2} \geq 0. \tag{7}$$

Equations 4 and 7 show that there is a monotonic positive relationship between priors and posteriors for both moments of the ability distribution. Equation 5 reveals that the dispersion in the priors plays a role in the determination of mean posteriors, and that this effect depends on the realization of the signal relative to mean priors. Noisier priors lead to higher (lower) mean posteriors among those who perform better (worse) than expected. Equation 6 implies that mean priors do not mediate the updating process in the variance.

4.2 Track Choices

Based on beliefs about their own ability, students make choices about the schooling curriculum they wish to attend. Let $q_j^\star$ be the minimum ability cutoff required to comply with the academic requirements in track j. Only students with ability above this cutoff will be able to succeed in this track, where academic success encompasses both admission and graduation. Student i chooses school-track j based on the following expected utility function:

$$U_{ij} = Pr(q_i > q_j^\star)\, V_{ij}, \tag{8}$$

where $V_{ij} \geq 0$ is the (gross) payoff placed by student i on attending track j. Under the distributional assumption of Equation 1, the first term in Equation 8 can be written as $1 - \Phi\left(\frac{q_j^\star - \mu_i}{\sigma_i}\right)$, where $\Phi(\cdot)$ denotes the Standard Normal CDF (and ϕ the associated PDF).[23] We further allow V_{ij} to be independent of the variance in beliefs and a non-decreasing function of mean beliefs: $\partial V_{ij}/\partial \mu_i \geq 0$.[24] Irrespective of the track, students with higher academic ability tend to derive higher payoffs in their educational and labor market trajectories.

In practice, we consider the choice between the academically-oriented curriculum ($j = A$) and the technical or vocational curriculum ($j = NA$). We assume that $q_A^\star > q_{NA}^\star$ (see Table C.1 for supporting evidence of this assumption) and, for simplicity, we normalize q_{NA}^* to zero. We further assume that V_{iNA} is not a function of academic ability, which is consistent with Figure 2.

Clearly, updates in beliefs about academic ability will affect the relative demand for high school programs from the academic track:

$$\frac{\partial U_{iA}}{\partial \mu_i} = \frac{1}{\sigma_i}\phi\left(\frac{q_A^\star - \mu_i}{\sigma_i}\right) V_{iA} + \left[1 - \Phi\left(\frac{q_A^\star - \mu_i}{\sigma_i}\right)\right]\frac{\partial V_{iA}}{\partial \mu_i} \geq 0, \tag{9}$$

$$\frac{\partial U_{iA}}{\partial \sigma_i} = \phi\left(\frac{q_A^\star - \mu_i}{\sigma_i}\right)\left(\frac{q_A^\star - \mu_i}{(\sigma_i)^2}\right) V_{iA} \geq 0 \quad \text{if } (q_A^\star - \mu_i) \geq 0 \tag{10}$$

$$\frac{\partial U_{iNA}}{\partial \mu_i} = \frac{\partial U_{iNA}}{\partial \sigma_i} = 0. \tag{11}$$

More importantly, the model generates two key predictions on the effect of the provision of feedback about academic performance on track choices. First, upwardly updates in mean beliefs unequivocally increase the value of options from track j, both through an increase in V_{ij} and a higher likelihood of success. Second, variance reductions affect the relative utility

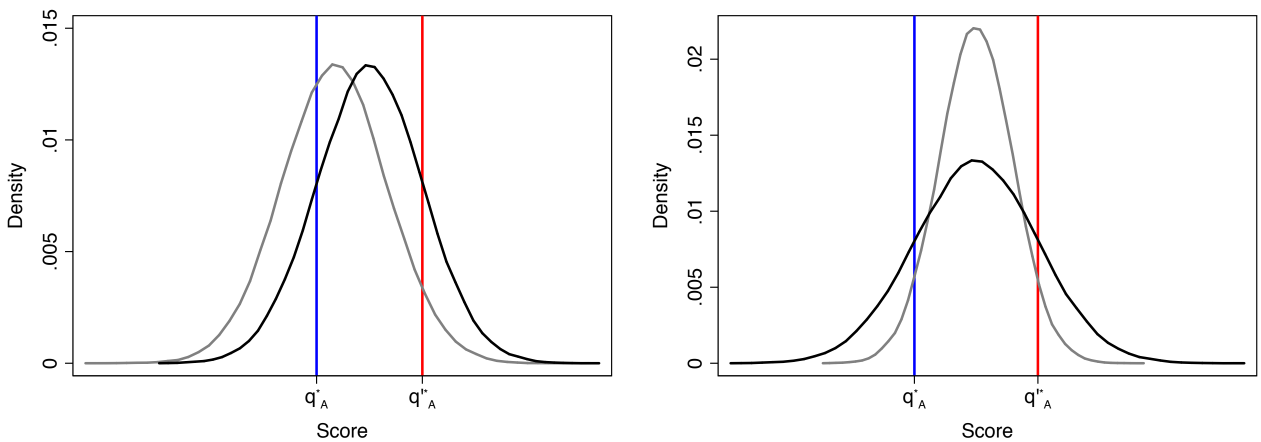
[23] The individual ability distributions elicited in the survey resemble the normal distribution. Using the 20 observations (i.e., beans) per student, we run a normality test [Shapiro and Wilk, 1965] and reject it for only 11.4% of the respondents. Only 6% of the respondents concentrate all beans in one interval. These few rejections seem to be driven by the use of a grid that is too coarse for a few applicants.

[24] A more general expression for the expected utility function is $U_{ij} = \int_{q_j^\star} V_j(q_i) d\Phi(q_i)$. Equation 8 is a special case in which schooling payoffs depend only on the mean beliefs, instead of the entire ability distribution. The logic of the simpler model discussed in the text (see Equations 9-11) extends to the more general version.

of academic track options only through an effect on the likelihood of success. However, this effect depends on the location of the mean posterior relative to q_A^*. That is, changes in the dispersion of the ability distribution enabled by the receipt of the signal can either reinforce or counteract the effect of updates in mean beliefs. In sum, the net effect of the signal on the demand for academic programs depends on the location of $\mu_i^{'}$ relative to q_A^*.

Panels (a) and (b) of Figure 4 illustrate the interplay of changes in the mean and in the variance of the individual ability distributions on the (perceived) likelihood of success in the academic track, $1 - \Phi\left(\frac{q_j^{\star} - \mu_i}{\sigma_i}\right)$. Irrespective of the ability cut-off value, an upward update in mean beliefs increases the likelihood of success, which consequently increases the expected utility of academic track options. On the other hand, mean-preserving contractions in the ability distribution can have differential effects on the likelihood of success depending on the value of q_A^*. Variance reductions in settings with relatively lenient academic requirements, i.e., low q_A^*, increase the likelihood of success in the academic track while the opposite occurs in settings with relatively more stringent admission and graduation standards, i.e., high q_A^*.[25]

Figure 4: The Role of Changes in the Ability Distribution on the Likelihood of Success



(a) Mean Changes

(b) SD Changes

NOTE: Illustrative graph, not based on real data. The blue and red vertical bars in both panels depicts two hypothetical levels of academic requirements, where $q_A^* < q_A^{\prime *}$. Panel (a) displays two normal density functions, where the one depicted in grey features a lower mean. Panel (b) displays two normal density functions, where the one depicted in grey features a lower variance.

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

5 Empirical Evidence

5.1 Belief Updating

According to the simple framework discussed in Section 4.1, 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. These patterns are confirmed by the OLS estimates presented in Table 1. In columns 1 and 2 we first show that, relative to the control group, the average effect of taking the mock exam without the delivery of the results is negligible on both moments of the belief distribution. Taking the exam without the provision of performance feedback does not seem to 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.

Columns 3-4 show that students' subjective beliefs about their academic ability respond to the provision of information about *own* performance.[26] Conditional on taking the mock exam, mean beliefs in the treatment group decrease on average by 7.5 points while the standard deviation of beliefs reduces by about 2.6 points. Relative to the placebo group, these effects represent roughly a 10% and a 15% reduction in the mean and standard deviation, respectively.

The aggregate patterns shown in columns 3 and 4 of Table 1 mask the potential heterogeneous effects of the treatment on upward and downward updaters. Indeed, the estimated negative coefficient of the treatment on mean posteriors in the full sample can be explained by the fact that about 80% of the applicants in our sample have scores that are below their mean prior (i.e., baseline) beliefs. Irrespective of the direction of the update, column 5 in Table 1 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.

Motivated 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 2 shows that, on average, mean beliefs increase by about 2.8

[26] 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 in the mock test, (ii) the average score in the mock test among applicants in the same class, and (iii) the average score in 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 survey questions on students' self-perceptions about their relative ranking in the class in order to shed some light on the role of (ii) vis-à-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.

Table 1: 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 Dep. Var.	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: * significant at 10%; ** significant at 5%; *** significant at 1%. OLS estimates, 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 A.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.

points among upward-updaters while they adjust downward by 9.9 points among downward-updaters. These results are consistent with the variation in the initial gap between expected and actual performance across these sub-samples (see Section 2.3).[27] Estimates presented in column 2 show that the treatment also induces a reduction in students' dispersion of their beliefs, with a more pronounced effect among upward-updaters (although we cannot reject equality of the two coefficients: p-value=0.12).

Columns 3 and 4 of Table 2 present OLS estimates of the effect of priors on posteriors. The sign of the estimated coefficients for the placebo group are broadly consistent with the corresponding Bayesian predictions.[28] The estimated coefficients of the interaction terms between the treatment indicator and the two moments of the prior distribution have opposite

[27] These estimates are in line with (2) in Section 4, where the transmission of the signal into mean beliefs results from a convex combination of mean 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 mean beliefs, which is precisely what we observe in the data. Figure A.3 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 upward-updaters have less room to adjust.

[28] The negative and significant relationship between the dispersion of priors and mean posteriors observed in column 3 can be explained by the fact that most students in our sample (80%) are downward-updaters.

Table 2: Beliefs about Exam Performance: Heterogeneous Treatment Impacts

Sample	Treatment & Placebo			
Dependent Variable	Mean Posterior	SD Posterior	Mean Posterior	SD Posterior
	(1)	(2)	(3)	(4)
Treat×(Upward Updater)	2.786**	-3.623***		
	(1.317)	(0.766)		
Treat×(Downward Updater)	-9.854***	-2.423***		
	(0.915)	(0.428)		
Upward Updater	-14.533***	3.104***		
	(1.135)	(0.601)		
Treatment			5.118	-0.042
			(4.136)	(2.269)
Treat×(Mean Prior)			-0.194***	0.002
			(0.042)	(0.022)
Mean Prior			0.523***	-0.005
			(0.039)	(0.015)
Treat×(SD Prior)			0.121*	-0.148***
			(0.065)	(0.055)
SD Prior			-0.101**	0.591***
			(0.047)	(0.040)
Mean Dependent Variable	75.61	17.45	75.61	17.45
Number of Observations	2293	2293	2293	2293
R-squared	0.346	0.095	0.429	0.368
Number of Clusters	90	90	90	90

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. OLS estimates, 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. Downward (upward) updaters are defined as those with mean baseline beliefs that are higher (lower) 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 A.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.

signs when compared to the coefficients on priors. That is, the signal provided through the intervention reduces the dependence of posteriors on priors. This is particularly beneficial for the applicants in our sample, who appear to be quite inaccurate in their initial predictions (see Panel (a) of Figure 1).

5.2 Track Choices, Admission, and High School Outcomes

The OLS estimates reported in Table 3 show that, on average, the intervention does not appear to affect track choices, admission, or high school outcomes. However, it increases the sensitivity of the demand for academically-oriented programs with respect to performance in the mock exam (see column 1). 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. This effect amounts to an increase of 8 percentage points with respect to the sample mean, which corresponds to 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, which is mostly driven by the underlying changes in preferences over tracks induced by the intervention.[29,30]

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 may be penalized in terms of subsequent academic achievement.[31] The evidence reported in columns 3 and 4 of Table 3 suggests that this is not the case in our setting. The track change induced by the treatment does not seem to be associated with any negative effects on drop-out or grades during the first year of high school.[32]

[29]Unconditionally on the track, the likelihood of assignment within the system (11%) 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). We condition on school assignment in the specification reported in column 2, hence the drop in the number of observations with respect to column 1.

[30]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 (column 1). This indicates that the effect of the intervention on the share of academic options is mainly the result of a composition effect in the application portfolio that favors academically-oriented options, rather than simply adding more options from the academic track. We also discard any effects on the selectivity of the portfolio, as measured by the average cutoff of the options submitted (column 2). We do find a small and significant effect on the share of UNAM options (column 3), which could be explained by the fact that UNAM schools exclusively offer academic programs. In fact, this effect vanishes once we incorporate technical options sponsored by the other university affiliated institution, IPN (column 4), suggesting that the treatment does not trigger an additional demand for selective schooling options beyond its effects on academic programs. Finally, we find a reduction in the share of alternatives in the same municipality of residence (column 5) which could reflect a broader search of additional academic options to replace non-academic ones.

[31]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 et al. [2010] and Frisancho and Krishna [2015]).

[32]Drop-out is defined conditional on enrollment in the high school assigned through the system (75%). This explains the drop in observations in column 3 vis-a-vis column 2. Enrollment rates do not vary systematically neither with the treatment nor with its interaction with the score in the mock exam (results

Table 3: Track Choices, Admission, and High School Outcomes

Sample	Treatment & Placebo			
Dependent Variable	Share Academic	Admission Academic	High School Drop-out	High School GPA
	(1)	(2)	(3)	(4)
Treatment× Mock Exam Score	0.041***	0.059**	-0.012	-0.049
	(0.013)	(0.027)	(0.021)	(0.072)
Treatment	0.012	-0.026	0.025	-0.037
	(0.016)	(0.026)	(0.024)	(0.069)
Mock Exam Score (z-score)	-0.016*	0.004	-0.034*	0.336***
	(0.009)	(0.022)	(0.018)	(0.049)
Mean Dependent Variable	0.518	0.477	0.148	7.662
Number of Observations	2293	2045	1529	1302
R-squared	0.087	0.067	0.380	0.440
Number of Clusters	90	90	90	90

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. OLS estimates, standard errors clustered at the school level are reported in parenthesis. Sample of ninth graders (columns 1 and 2) and tenth graders (columns 3 and 4) that are assigned to the treatment and the placebo group. 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 A.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.

6 Mechanisms

6.1 Belief Updating and Track Choices

The simple framework exposed in Section 4.2 suggests that changes in the first and the second moments of the ability distribution interact to determine high school track choices. This prediction has key implications for understanding the schooling responses of the informational intervention. Since the treatment induces a reduction in the variance of the ability distributions (see Tables 1 and 2), any increase (decrease) in the demand for academic programs resulting from higher (lower) mean beliefs is most likely reinforced (counteracted) in settings with more lenient requirements. Conversely, any positive (negative) shock on mean beliefs is counteracted (reinforced) in settings where those requirements are more stringent.

not reported but available upon request). The GPA in high school is not observed for drop-outs (15%), hence the difference in the number of observations between columns 3 and 4. In order to take into account school-specific trends in high school outcomes, we include high school fixed effects in columns 3 and 4.

We first examine how the treatment impacts vary with the direction of the update. Table 4 reports heterogeneous impacts of the treatment on the demand for the academic track among upward and downward updaters. The OLS estimates reported in column 1 reveal that the treatment is associated with an average increase of 8.3 percentage points in the share of requested academic options among upward-updaters. This is a substantial effect of approximately 18% of the sample mean in the placebo group, which implies an average composition effect in the application portfolios of roughly two schooling options. In turn, the large reductions in mean beliefs observed amongst downward-updaters in the treatment group (see Table 2) do not appear to translate into any corresponding change in the demand for academically-oriented programs.

As mentioned above, we argue that the presence of this differential response in track choices largely reflects the joint effects of change in mean and variance across settings with different ability cutoffs. In order to understand the role of the first and the second moments of the subjective ability distributions behind this effect, we exploit variations in high school academic requirements within the metropolitan area of Mexico City. Accordingly, we generate indicator variables for the location (State) of the applicants' middle schools and utilize the fact that there is a strong degree of segmentation in placement outcomes across the two States (see Section 3.4). Relative to the State of Mexico, it is more likely to observe a positive gap between the minimum ability cutoff and mean beliefs in the Federal District due to higher admission and graduation standards.[33] Thus, variance reductions in the Federal District will tend to reduce the benefit from academically-oriented options, regardless of the realization of the signal. The opposite holds for the State of Mexico, where variance reductions will most likely increase the expected value of academic alternatives.

The OLS estimates reported in column 2 of Table 4 show indeed that among the group of upward-updaters in the State of Mexico the treatment has a large positive impact (12 percentage points) in the share of academic options requested. However, the incentives to increase the demand for academic options enabled by mean updates to the treatment is entirely offset in the Federal District, where more stringent academic requirements are in place. The results for downward-updaters in column 3 also seem to reflect the role of the dispersion of individual belief distributions on track choice responses to the treatment. We observe a reduction in the demand for academic options among applicants in the Federal District, where the reduction in the variance of the belief distributions is likely to reinforce

[33] Appendix C provides different pieces of evidence that broadly support the notion that the probability of admission within the COMIPEMS system as well as high school outcomes in the academically-oriented track are systematically lower in the Federal District when compared to the neighboring State of Mexico.

Table 4: Belief Updating and High School Track Choices

Dependent Variable	Share of Academic Schools		
Sample	All	Upward-updaters	Downward-updaters
	(1)	(2)	(3)
Treatment×(Upward-updater)	0.083***		
	(0.029)		
Treatment×(Downward-updater)	-0.005		
	(0.017)		
Upward-updater	-0.057**		
	(0.022)		
Treatment		0.120***	0.019
		(0.033)	(0.020)
Treatment×(Federal District)		-0.118*	-0.084***
		(0.061)	(0.030)
Federal District		0.149**	-0.050
		(0.068)	(0.031)
Mean Dependent Variable	0.51	0.46	0.52
Number of Observations	2293	441	1852
R-squared	0.086	0.171	0.092
Number of Clusters	90	84	90

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. OLS estimates, 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. Downward-(upward) updaters are defined as those with mean baseline beliefs that are higher (lower) 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 A.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 effect of changes in mean beliefs.[34]

The observed differential response to the treatment reported in column 1 of Table 4 can

[34]Table A.3 checks if the null impact on the number of options of the portfolio persists when we look at the differential treatment effects by the direction of the update and location. Indeed, the number of options listed is unaffected even if we look at the differential effect by direction of the update (column 1) or by the direction of the update and by location (column 2). In general, this implies that the result of substitution of options across tracks holds.

thus be explained, to a large extent, by a composition effect. Since approximately three quarters of the applicants in the sample reside in the State of Mexico, the previous result most likely reflects the reinforcing effect of the variance among upward updaters and its corresponding counteracting effect for downward-updaters in this location.

6.2 Dealing with the Endogeneity of Beliefs

The empirical relationship between beliefs and choices is contaminated by unobserved heterogeneity. Individuals with greater confidence, for example, may have higher expected academic ability and tend to demand more academic options. Thus, simply regressing the share of academic options requested on observed belief posteriors is unlikely to be informative in terms of the effect of the mean and the dispersion of the subjective ability distributions on track choices.

Our intervention allows us to generate instrumental variables for actual posteriors based on the predictions of the Bayesian model. Given the score in the mock exam, we can construct predicted posteriors based on the priors of each student.[35] These are plausibly credible instruments since: (i) the Bayesian predictions for both the mean and the dispersion of the ability distribution are highly correlated with their observed counterparts (particularly amongst the group of students who receive feedback on their performance); and (ii) they are likely to affect choices only through the corresponding changes in beliefs induced by the intervention.

Given the same observed posteriors, applicants in the treatment group should exhibit a better alignment of their academic ability with the share of academic options requested. That is, the coefficient of the interaction term between the predicted mean posteriors and the treatment indicator should be positive. In turn, the sign of the coefficient of the interaction term between the treatment indicator and the predicted standard deviation of the posterior depends on the rigidity of the academic requirements in the location of residence. For example, the same observed dispersion of the posterior is likely to be associated with a reduction in the share of academic options among applicants in the treatment group in the Federal District. Given that $(q_A^* - \mu_i)$ is more likely to be positive in this setting, the drop

[35] We impute missing values for 122 observations with zero variance at baseline, which roughly corresponds to 5% of the sample (balanced between the treatment and the placebo groups). We input σ_i^2 based on the average of the empirical belief distributions at baseline and make some assumptions on σ_ϵ^2 to construct the theoretical moments predicted by the Bayesian benchmark. In particular, we impose an upper bound and set σ_ϵ^2 to the value of the variance of the residual that results from regressing the mock exam score on the same set of individual, household, and school characteristics that we use as control variables throughout the analysis (see Table A.1 for details).

Table 5: The Effects of (Bayesian) Beliefs on High School Track Choices

Sample	Treatment & Placebo		
Dependent Variable	Mean Posterior	SD Posterior	Share Academic
Estimator	OLS	OLS	2SLS
	(1)	(2)	(3)
Bayesian Mean Posterior	0.648***	-0.027	
	(0.052)	(0.020)	
(Bayesian SD Posterior) × (Federal District)	0.572***	1.191***	
	(0.157)	(0.105)	
(Bayesian SD Posterior) × (Mexico State)	0.392***	1.266***	
	(0.131)	(0.085)	
Treatment × (Mean Posterior)			0.047**
			(0.024)
Mean Posterior			-0.000
			(0.002)
Treatment × (SD Posterior) × (Federal District)			-0.008***
			(0.003)
Treatment × (SD Posterior) × (Mexico State)			-0.002
			(0.003)
(SD Posterior) × (Federal District)			0.000
			(0.003)
(SD Posterior) × (Mexico State)			0.001
			(0.002)
Treatment			0.076
			(0.054)
Mean Dependent Variable	72.45	16.61	0.52
Number of Observations	2171	2171	2171
R-squared	0.334	0.281	0.085
Number of Clusters	90	90	90
Weak IV Test:			
Kleibergen-Paap Chi-sq (p-value)			49.68
			(0.000)

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. OLS and 2SLS estimates, 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. 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 A.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.

in variance will reduce the value of academic options (see Equation 10).

Table 5 presents the resulting estimates. Columns 1 and 2 display the first-stage OLS coefficients of observed posteriors on Bayesian posteriors, which confirms a robust and systematic relationship between the two.[36] Column 3 presents the corresponding 2SLS estimates, whereby the observed posteriors are instrumented with their Bayesian counterparts. The estimated coefficient of the interaction effect between the treatment and mean posteriors is positive. The triple interaction effect between the treatment indicator, the standard deviations posteriors, and the indicator variable for the Federal District confirms that the treatment-induced reduction in variance leads to a reduction in the share of academic options in this setting.

6.3 Other Schooling Responses

Beyond the school choices submitted within the assignment system, the process of belief updating induced by the treatment 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 exam. Here we provide some evidence in favor of this channel by examining the effects of the treatment on performance in the admission exam.[37]

Column 1 in Table 6 displays the estimated treatment impacts on individual scores in the admission exam by the expected direction of the update. While there seems to be no discernible effect on test scores among upward-updaters, downward-updaters have lower scores by approximately 10% of a standard deviation with respect to the placebo group. In line with the results discussed in Section 6.1, the discouragement effect of the intervention on subsequent study effort varies depending on the stringency of the academic requirements between States. In particular, the negative effect on effort appears to be concentrated among applicants from the Federal District who experience a reduction of about 37% of a standard deviation of the score in the admission exam (see column 3).

The evidence reported here is consistent with the results discussed in Sections 6.1 and 6.2 as to the interplay between variance reductions and mean changes in the ability distribution enabled by the information intervention.[38]

[36] Figure A.4 displays the empirical relationship between the observed and predicted posteriors in the treatment group.

[37] 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% 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.

[38] Columns 4 and 6 of Table A.3 in Appendix A show a negative effect of the treatment on the preferences

Table 6: Belief Updating and Study Effort

Dependent Variable	Standardized Score in the Admission Exam		
Sample	All	Upward-updaters	Downward-updaters
	(1)	(2)	(3)
Treatment × (Upward-updater)	-0.068		
	(0.056)		
Treatment × (Downward-updater)	-0.095**		
	(0.043)		
Upward-updater	-0.094**		
	(0.043)		
Treatment		-0.075	-0.005
		(0.065)	(0.042)
Treatment × (Federal District)		-0.093	-0.368***
		(0.125)	(0.094)
Federal District		0.060	0.214**
		(0.103)	(0.097)
Mean Dependent Variable	0.02	0.71	-0.12
Number of Observations	2253	437	1816
R-squared	0.713	0.750	0.659
Number of Clusters	90	84	90

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. OLS estimates, 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. Downward-(upward)updaters are defined as those with mean baseline beliefs that are higher (lower) 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 A.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.

over school selectivity among downward-updaters from the Federal District. These findings offer further evidence in favor of the role of the reduced dispersion of the belief distributions as a potential channel behind the observed schooling responses. A downward push in expected ability in settings with high academic requirements leave small chance of being successful in relatively challenging schools among the group of downward-updaters mostly affected.

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, especially in developing countries. In this paper, we document the results from a field experiment that provides youth with individualized information about their own academic ability.

Our findings show that agents 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 information intervention successfully aligns students' measured academic skills with their curricular choices, and does not entail short run adjustment costs in terms of high school performance. Although we lack data on longer term trajectories, we speculate that the observed changes in school placement within the system may potentially bear real consequences on subsequent schooling and labor market outcomes.

This study is 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 channeling the effect of beliefs 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 about academic ability on high school track choices.

Beyond the changes in the variance of the individual belief distributions, other mechanisms triggered by the intervention may be consistent with the observed differential impacts when we condition on the direction of the belief updating. Some behavioral biases (e.g., Bénabou and Tirole [2002]; Köszegi [2006]) may explain why in their choices students tend to ignore freely available information about future schooling returns. However, this interpretation is at odds with the estimates that we obtain when we condition on the different schooling sub-markets within the Metropolitan area of Mexico City, in which upward and downward-updaters symmetrically respond to the intervention in terms of both track choices and performance in the admission exam.

Taken together, these results highlight the potential role of policies aimed at disseminating information about individual academic skills in order to ameliorate the skill mismatch in curricular choices. In the particular context we analyze here, 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 portfolios after taking the admission exam and receiving their scores. An alternative policy may be to incentivize schools to implement mock tests and deliver score results before students submit schooling portfolios to the centralized assignment mechanism.

The analysis presented in this paper has some limitations. For example, in the model outlined in Section 4 we take as given the cross-sectional variation of the prior distribution of beliefs. Our data allows for a more general analysis on the dynamics of belief formation during the process of human capital accumulation. This question goes beyond the scope of this paper and is left for future research.

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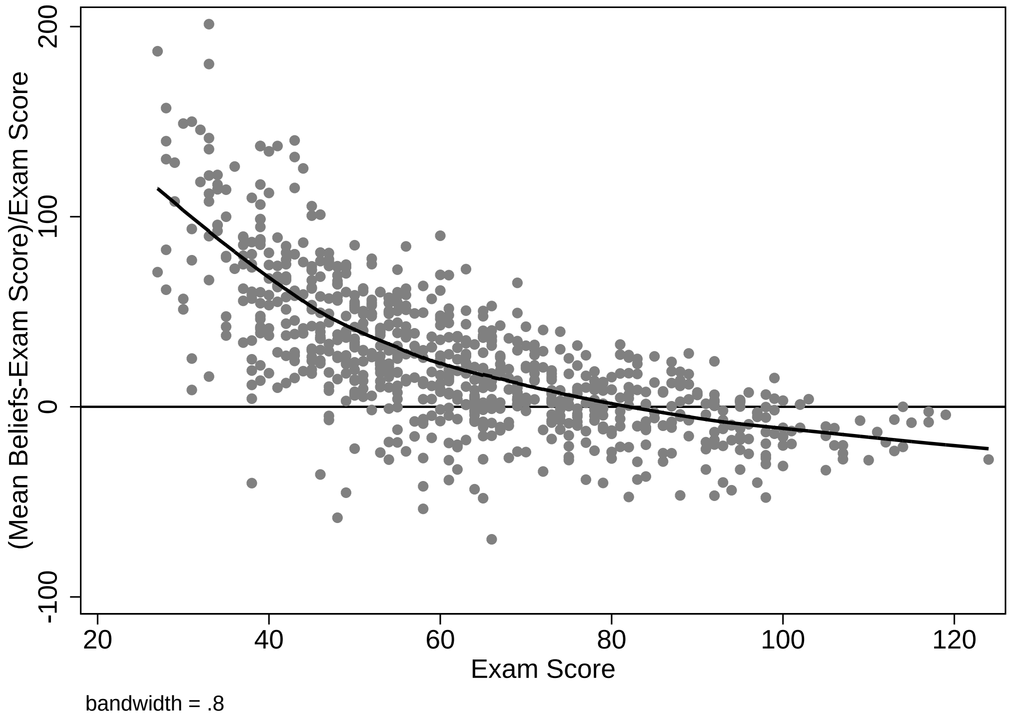
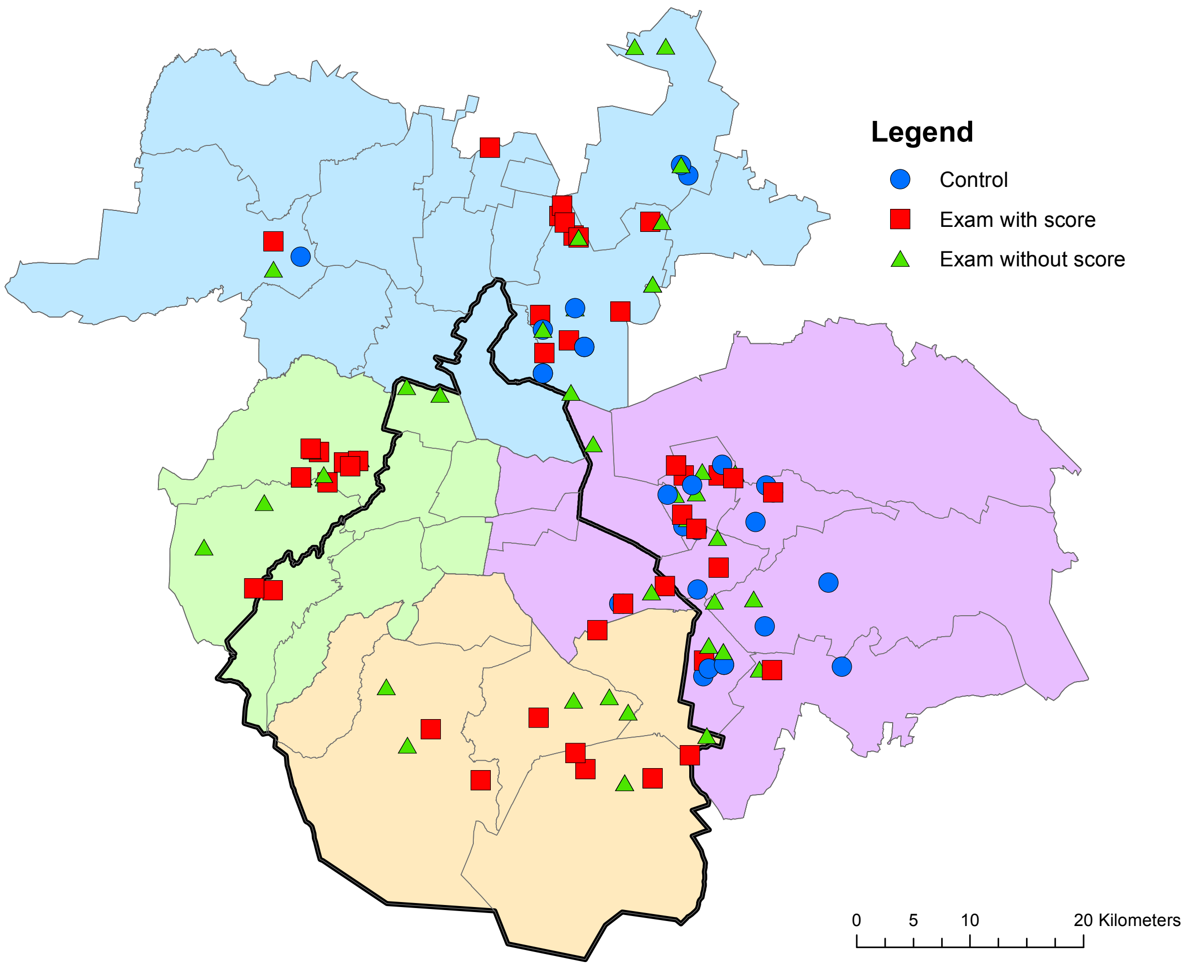
A Additional Figures and Tables

Figure A.1: Gap between Mean Beliefs and Exam Score by Score



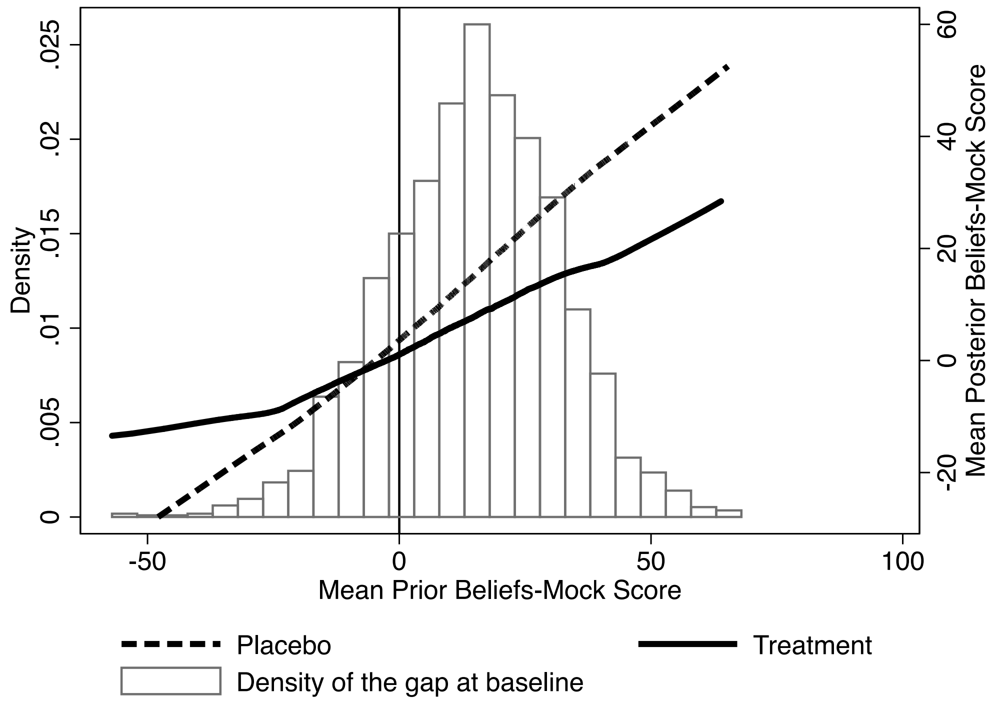
Note: The thick black line comes from a locally weighted regression of the gap on exam score for the control group. Source: Survey data and COMIPEMS administrative data, 2014.

Figure A.2: The Metropolitan Area of Mexico City and the Schools in the Sample



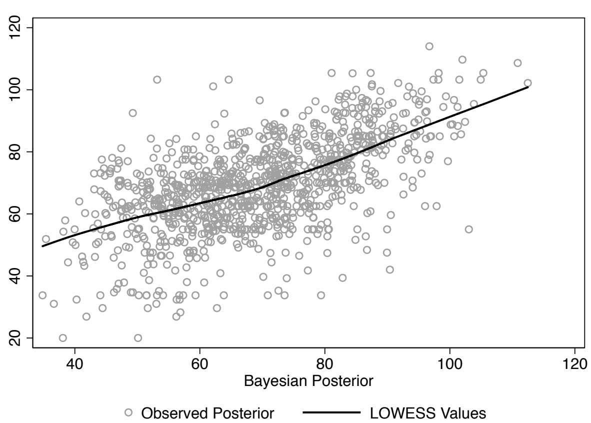
Note: The thick black line denotes the geographic border between the Federal District and the State of Mexico. The thin grey lines indicate the borders of the different municipalities that participate in the COMIPEMS system. The four geographic regions that, combined with discrete intervals of school-average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

Figure A.3: Change in Gaps between Expected and Realized Performance

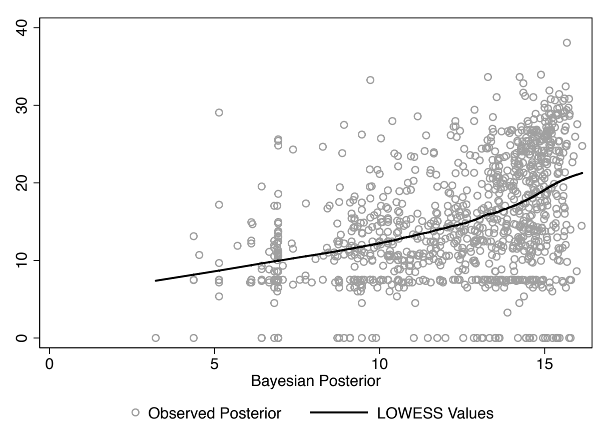


Source: Survey data and COMIPEMS administrative data, 2014.
Note: The histogram shows the empirical density of the gap between expected and realized performance. The overlaid lines are non-parametric estimates - based on locally weighted regression smoothers—of the relationship between the gap in expected and realized performance, before and after the treatment delivery.

Figure A.4: Observed vs Bayesian Updating Patterns



(a) Mean



(b) SD

Source: Survey data for the treatment group (January and February, 2014).
Notes: Local linear smoother.

Table A.1: Summary Statistics and Randomization Check

	Placebo (1)	Treated (2)	Control (3)	T-P (4)	P-C (5)	T-C (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]		
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]
COMIPEMS pre-registration	0.484	0.514	0.563	0.008	-0.106	-0.099
	(0.50)	(0.50)	(0.49)	[0.89]	[0.16]	[0.20]
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.

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

Sample	Treatment & Placebo				
Dep. Var.	Number of Options	Average Cutoff	Share UNAM	Share UNAM & IPN	Share Own Municipality
	(1)	(2)	(3)	(4)	(5)
Treatment × Mock Exam Score	-0.147	1.161	0.019**	0.015	-0.023*
	(0.153)	(0.741)	(0.009)	(0.013)	(0.014)
Treatment	0.052	0.696	0.002	-0.005	-0.018
	(0.229)	(1.036)	(0.012)	(0.018)	(0.027)
Mock-Exam Score (z-score)	0.311***	3.425***	0.028***	0.061***	-0.032***
	(0.113)	(0.546)	(0.007)	(0.010)	(0.009)
Mean Dependent Variable	9.412	63.597	0.187	0.313	0.407
Number of Observations	2293	2293	2293	2293	2293
R-squared	0.044	0.328	0.208	0.243	0.213
Number of Clusters	90	90	90	90	90

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%.
OLS estimates, 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. All specifications include a set of dummy variables which correspond to the randomization strata and the following set of individual and school characteristics (see Table A.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.3: Treatment Impacts on Other Characteristics of School Portfolios: Heterogeneous Impacts

Dependent Variable	Number of Options			Average Cutoff		
Sample	Treatment & Placebo	Upward-updaters	Downward-updaters	Treatment & Placebo	Upward-updaters	Downward-updaters
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × (Upward-updater)	-0.106			1.547		
	(0.348)			(1.752)		
Treat × (Downward-updater)	0.095			0.540		
	(0.238)			(1.031)		
Upward-updater	-0.400			-3.481***		
	(0.241)			(1.284)		
Treatment		0.001	0.167		2.782	1.307
		(0.391)	(0.279)		(1.908)	(1.209)
Treatment × (Federal District)		-0.020	-0.371		-4.557	-3.894**
		(0.874)	(0.461)		(3.558)	(1.888)
Federal District		-0.220	0.909*		12.295**	5.391**
		(1.206)	(0.529)		(5.960)	(2.665)
Mean Dependent Variable	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	0.059	0.331	0.401	0.327
Number of Clusters	90	84	90	90	84	90

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. OLS estimates, 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. Downward-(upward) updaters are defined as those with mean baseline beliefs that are higher (lower) 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 A.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.[39]

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 2 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).[40] 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 about own performance in the test in absolute terms are very similar to the ones reported in columns 1 and 2 in Table 2, in both magnitude and precision. This evidence suggests that updating in absolute terms induces a direct change in the individual belief distribution far and beyond the indirect effect that changes in relative beliefs may have.

[39] 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.

[40] We lose 162 observations (7% 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 × Upward-Updater	3.085**	-3.638***
	(1.331)	(0.813)
Treatment × Downward-Updater	-8.257***	-2.303***
	(0.967)	(0.581)
Upward-Updater	-13.798***	3.400***
	(1.189)	(0.619)
Treatment × Upward-Updater (class)	0.575	-0.918
	(1.310)	(0.894)
Treatment × Downward-Updater (class)	-4.143***	-0.092
	(1.273)	(0.807)
Upward-Updater (class)	-1.966**	-0.404
	(0.979)	(0.538)
Downward-Updater (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

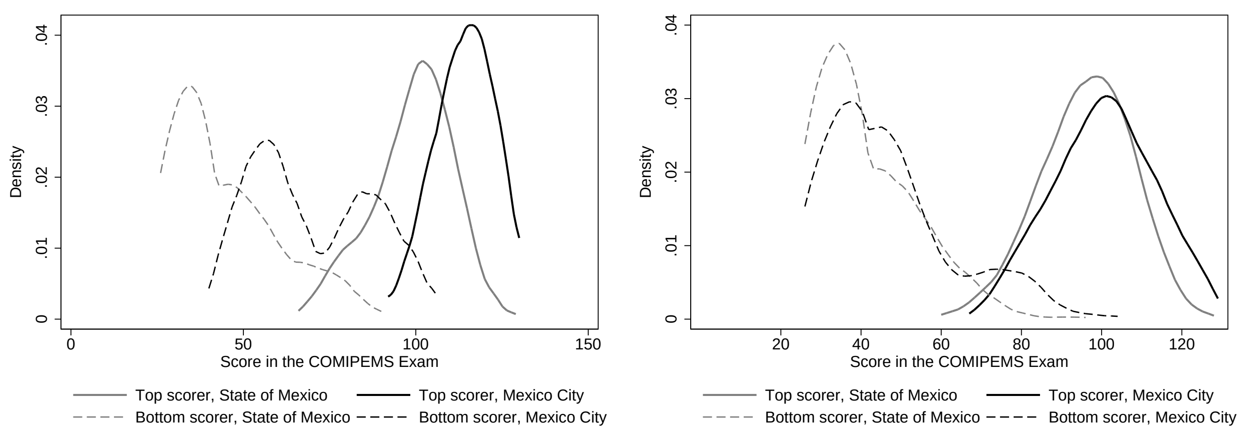
Note: * significant at 10%; ** significant at 5%; *** significant at 1%.
OLS estimates, standard errors clustered at the school level are reported in parenthesis. Sample of ninth graders in schools that belong to the treated and the placebo group. Downward-(upward) updaters are defined as those with mean baseline beliefs that are lower (higher) than the realized value of the score in the mock exam. Downward-(upward) updaters (class) 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 A.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.

C Admission and Graduation Standards: Federal District vs. State of Mexico

We provide several pieces of evidence that are consistent with the assumption that within Mexico City the education system in the Federal District requires higher admission and graduation standards than the system in the State of Mexico. First, we calculate the probabilities of getting placed into a school from the academic track by location, conditional on requesting it as the first option. Using administrative data on the 2010 COMIPEMS round, we find that the conditional probability of getting placed in an academic option in the Federal District is 0.56, much lower than in the State of Mexico where it is 0.74. In comparison, the difference across locations in the probability of admission into a non-academic track is much smaller, with 0.66 in the Federal District and 0.74 in the State of Mexico.

Second, we rely on the COMIPEMS score as a standardized measure of initial academic credentials to have a sense of the level of peer competition faced by students across location and tracks. Panel (a) in Figure C.1 shows that among academic options, both the distribution of minimum and maximum scores in the Federal District dominate the distributions in the State of Mexico. This is not the case in the non-academic track (see panel (b)), where the distributions of top and bottom scorers do not differ much across locations.

Figure C.1: Distribution of Maximum and Minimum Admission Scores Across Locations, by Track



(a) Academic

(b) Non-Academic

Source: COMIPEMS administrative data, 2010.

Third, using the high school records for our sample, we check for differences in schooling outcomes at the end of the first year of high school by location and track. We control for academic achievement using middle school GPA and the score in the mock exam, the same set of applicants' characteristics used throughout the empirical analysis, and also include fixed effects at the subsystem level. Columns 1 through 3 in Table C.1 show that students enrolled in the academic track tend to perform worse with respect to those enrolled in technical and vocational programs, in terms of higher drop-out and failure rates (i.e., three or more subjects with a grade less than six) as well as lower GPA, and particularly so in the Federal District.

Table C.1: High School Outcomes by Location and Track

	(1) Drop-out	(2) Fail	(3) GPA
DF×(Academic Track)	-0.094*	-0.003	-0.362*
	(0.046)	(0.082)	(0.173)
DF×(Non-Academic Track)	-0.087	-0.083	0.044
	(0.066)	(0.109)	(0.153)
Academic Track	-0.100***	0.026**	-0.248***
	(0.007)	(0.011)	(0.032)
Mock Exam Score (z-score)	0.004	-0.022	0.191**
	(0.019)	(0.019)	(0.060)
GPA in Middle School (z-score)	-0.075***	-0.092**	0.507***
	(0.013)	(0.035)	(0.025)
Mean Dependent Variable	0.16	0.18	7.65
Number of Observations	799	799	668
R-squared	0.177	0.169	0.442

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. OLS estimates with fixed effects at the subsystem level. Standard errors clustered at the subsystem level are reported in parenthesis. Longitudinal sample of tenth graders that were originally assigned to the placebo group. All specifications include a set of dummy variables which correspond to the randomization strata and the following set of individual and school characteristics (see Table A.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.