Perceived Ability and School Choices*

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Abstract

We study the role of youth's subjective expectations about their own ability in shaping school choices in secondary education. A field experiment that provides ninth-graders in urban Mexico with individualized feedback about their academic skills generates exogenous variation in beliefs that is used to isolate their role in driving students' allocation across high schools. We find that mean beliefs increase the value of attending academically-oriented schools, while students with greater dispersion in their beliefs find this curricular track less attractive. These results are in line with the heterogeneous treatment impacts on school choices, since the feedback spurs differential changes in the location of beliefs and overall large variance reductions that either reinforce or counteract the effect of changes in the first moment. The information intervention induces a steeper gradient of the relationship between academic achievement and the demand for academic schools. This reallocation of skills across tracks improves the match between students and schools, as measured by the rate of high-school graduation on time.

Keywords: Information, Subjective expectations, Beliefs updating, Biased beliefs, School choice, Discrete choice models, Control function, Truth-telling, Stable matching.

JEL codes: D83, I21, I24, J24.

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1 Introduction

Most of our choices are made under uncertainty and rely on subjective expectations about present and future returns. Investments in human capital crucially hinge on these expectations as they are typically made very early in the life-cycle and often impose high switching costs. For instance, academically-oriented secondary schools are well-equipped to prepare students for college but provide limited skills and training for those who choose not to continue their education or those who opt for technical/vocational higher education careers. Even under complete information about the characteristics and the labor market returns of alternative schooling trajectories, biased misperceptions about own talent and skills may lead to misallocation insofar as students end up choosing alternatives with high average returns but low individual-specific returns.

This paper studies the role of youth's subjective expectations about their own ability in shaping school choices in secondary education and how these choices affect subsequent schooling trajectories. This specific source of subjective uncertainty has been largely overlooked in the recent literature on information provision and schooling decisions.[1] We design and implement a field experiment that provides students with individualized feedback on their academic skills during the transition from middle to high school. The context of the study is a centralized assignment mechanism that allocates students across high school programs in Mexico City according to applicants' school rankings and performance on an achievement test. Since students submit their school choices *before* taking the admission exam, they rely on perceptions about their own academic skills when making high-stakes decisions about future academic trajectories. We administer a mock version of the admission test, communicate individual scores to a randomly chosen subset of applicants, and elicit probabilistic statements about performance beliefs in the admission test using bean counts. The research design also includes a pure control group of applicants who do not take the mock test, allowing us to distinguish between the effects of taking the test and receiving performance feedback. In this setting, the score in the mock exam provides students with a signal about their own academic potential that is easy to interpret and contains relevant information on individual-specific returns across schooling careers.

Data from the control group show that there are large discrepancies between perceived and measured performance in the test, especially among students with low exam scores who tend to hold upwardly biased beliefs about their own academic achievement. Results from the experiment show that providing feedback on individual performance substantially shifts the location of the

[1] Several studies have studied the role of information about labor market outcomes, school quality, or financial aid and application procedures on schooling decisions. A few recent contributions include Hastings and Weinstein [2008]; Jensen [2010]; Carrell and Sacerdote [2013]; Mizala and Urquiola [2013]; Hoxby and Turner [2014]; Dinkelman and Martinez [2014]; Wiswall and Zafar [2015*a*]; Hastings et al. [2015]; Bleemer and Zafar [2018]; Dustan [2018].

individual belief distributions while reducing the dispersion, whereas taking the test only increases the dispersion. The provision of performance feedback also generates a steeper gradient of the relationship between the demand for academically-oriented schools and the score in the mock test, with better performing (lower performing) students increasing (decreasing) the share of academic options in their school rankings. This choice response alters the realized skill composition across high-school tracks in our sample. Unique follow-up administrative data further enable us to track the medium-run consequences of the change in the sorting patterns by ability triggered by the intervention. Three years after school assignment, the probability of graduating from high school on time is on average 8 percentage points higher among students who received performance feedback, which corresponds to a 15-percent increase relative to the control group.

We next propose and estimate a school choice model that incorporates the role of students' beliefs about their performance in the test as a proxy for their perceptions about own academic ability. Crucially, we allow for both the mean and the dispersion of the individual belief distributions to shape the heterogeneous preferences over school characteristics. The experimental variation in beliefs induced by the information intervention is introduced in the model through a simple two-step control function approach. We find that ignoring the endogeneity of subjective beliefs in the school choice model greatly underplays their role in driving the observed sorting patterns across schools, especially for the second moment of the belief distributions. Mean beliefs about test performance have positive effects on the value of attending an academically-oriented school. Conditional on the mean, students who are more uncertain about their performance in the test find it less attractive to attend academically-oriented schools. The relatively large magnitudes and the opposing signs of the estimated preference parameters for the two moments of the belief distributions may potentially explain the pattern of heterogeneity of the treatment effects on track choices along the test score distribution as well as the associated medium-run impacts on high-school trajectories.

In order to quantitatively illustrate this mechanism, we simulate the school choice model at estimated parameters using the longitudinal variation in beliefs for students in the treatment group and compare the predicted choice probabilities before and after the delivery of performance feedback. Students with higher test scores are likely to update their beliefs upwards in response to performance feedback, thereby strengthening the positive effect of the associated decrease in the dispersion of beliefs. Among students who instead update downwards after receiving performance feedback, the reduction in the dispersion counteracts the negative effect of changes in the mean of beliefs. These two effects offset each other among students with test scores around the mean of the distribution, completely undoing the impact of performance feedback on choices, whereas the negative effect of changes in the mean of the belief distributions dominates for students at the bottom of the score distribution. The estimated model further allows us to decompose the relative importance of exam taking when compared to performance feedback on track choices. While the net effect of personalized performance feedback can get diluted due to the interplay between simultaneous changes in the mean and the dispersion of the subjective ability distributions, uninformative signals, that exclusively increase the noise in beliefs, may have a more direct pass-through into choices and outcomes. Indeed, we show that among worse-performing students, the negative effect of exam taking on the demand for academic schools is comparable in magnitude to the impact of performance feedback in spite of much larger changes in beliefs triggered by the more accurate ability signal.

The study of individual choices under uncertainty traditionally relied on revealed preferences in order to estimate demand-side parameters. However, preferences and expectations cannot be recovered from choice data alone, since observed choices may be consistent with different configurations of the constructs of interest [Manski, 2002; Magnac and Thesmar, 2002]. Our paper fits within a broad and long standing body of work addressing this under-identification problem by directly measuring subjective expectations and using them in conjunction with choice data. Several studies along these lines have focused on measures of beliefs related to the labor market returns of human capital investments,[2] but relatively fewer studies have explored the role of perceived individual traits. Altonji [1993] and Arcidiacono [2004] introduce the notion of uncertainty about ability into the probability of completing a college major and more recent work documents the role of beliefs about future performance on college major choices and dropout decisions [Arcidiacono et al., 2012; Stinebrickner and Stinebrickner, 2012, 2014; Arcidiacono et al., 2016]. Notably, Kapor et al. [2018] measure the role of biased beliefs about admission chances on school choices.

Few papers in the literature have explicitly acknowledged the possibility that elicited measures of subjective beliefs may be jointly determined with choices and outcomes [Lochner, 2007; Bellemare et al., 2008; De-Paula et al., 2014; Delavande and Kohler, 2015]. One promising strategy, recently explored by Delavande and Zafar [forthcoming], attempts to identify the relationship between beliefs and choices by directly measuring beliefs and expected outcomes under counterfactual states. Our work follows the approach in Wiswall and Zafar [2015*a*], which relies on an information experiment to estimate a model of college major choice that incorporates the role of beliefs about own ability along with a variety of forecasts about future events. We build on this line of work by more narrowly focusing on the role perceptions about own ability and eliciting

[2] Kaufmann [2014]; Attanasio and Kaufmann [2014]; Wiswall and Zafar [2015*b*] measure perceived education returns (as well as perceived earnings risk and perceived unemployment risk in some cases). Giustinelli [2016] studies how subjective expected utilities of both parents and students shape high school track choices. Hastings et al. [2016] evaluate the role of earnings and cost expectations on degree choice and dropout in college. More recently, Delavande and Zafar [forthcoming] circumvent identification issues, relying on beliefs and outcome data for counterfactual states.

subjective expectations in a context where beliefs are tightly linked to concrete, immediate, and high-stakes choices. The longitudinal span of our data allows us to go further and assess the longer-term impacts of feedback provision on schooling outcomes. The methodological approach pursued here also shares with a few recent papers the idea of leveraging external sources of variations induced by field experiments in order to credibly identify and estimate empirical choice models that, in turn, are used to unpack the mechanisms through which the experimental intervention affects behaviors [Attanasio et al., 2012; Galiani et al., 2015; Duflo et al., forthcoming].

This paper is also related to a recent strand of the economics of education literature that uses natural or field experiments to uncover the mechanisms through which feedback on students' academic ability affects schooling outcomes. Azmat and Iriberri [2010]; Azmat et al. [2018]; Elsner and Stinebrickner [2017] consider the role of students' ordinal rank on their effort and subsequent performance. Andrabi et al. [2017] evaluate a bundled intervention that provides individual performance information and average school performance to both households with school-age children and schools. Bergman [2015] studies the role of information frictions between parents and their children in the United States, while Dizon-Ross [2018] conducts a field experiment in Malawi in which parents are provided with information about their children's academic performance. Our work uncovers and quantifies the role of a novel channel through which the provision of performance feedback shapes school choices and subsequent trajectories: the interplay between simultaneous changes in both the mean and the dispersion of the individual distributions of beliefs about own ability. It may thus help interpreting some of the findings in these studies that typically document heterogenous responses of feedback provision by students and/or parents.

The remainder of the paper is structured as follows. Section 2 describes the context of the feedback provision experiment, the different sources of data that we have collected, and the research design. Section 3 presents the reduced-form impacts of the intervention on beliefs, school choices, and longer-term schooling outcomes. In Section 4 we develop the empirical school choice model, discuss identification and estimation issues, and present the resulting estimates of the preference parameters. Using the estimated model, Section 5 reports simulation results that shed light on some of the channels through which the information intervention had an effect on school choices while Section 6 concludes.

2 The Feedback Provision Experiment

2.1 Context

Since 1996, a local commission (COMIPEMS, by its Spanish acronym) has administered public high school admissions in Mexico City's metropolitan area through a centralized assignment mechanism. In 2014, over 238,000 students were placed in 628 public high schools, accounting for roughly three-quarters of enrollment in the area. The remaining 25 percent of high schools students enrolled in either other public schools with open admission (10 percent) or private schools (15 percent).

Students apply to the COMIPEMS system during the next to last term while in ninth grade – i.e., the last year of middle school. Prior to registration, they receive a booklet outlining the timing of the application process and corresponding instructions, as well as a list of available schools, their basic characteristics, and cut-off scores in the last three rounds. In addition to the registration form, students fill out a socio-demographic survey and a ranked list of, at most, 20 schools. At the end of the school year, all applicants take a unique standardized achievement test.[3] Based on their scores, students are ranked in descending order and the matching algorithm goes down the list to sequentially assign applicants to their most preferred schooling option with available seats. Each placed applicant is matched with one school. Whenever ties occur, members of the Commission agree on whether to admit all tied students or none of them. Unplaced applicants can request admission in other schools with available seats after the allocation process is over or search for a seat in schools with open admissions outside the system. Whenever applicants are not satisfied with their placement, they can request admission to another school in the same way unplaced applicants do. In sum, the assignment system discourages applicants to remain unplaced and/or list schools they will ultimately not enroll in, as placement through the second round will almost surely imply being placed in a school not included in the student's original ranking. In practice, the matching algorithm performs quite well: only 11% of the applicants in our sample remain unplaced, and 2% are admitted through the second round of the matching process.

The Mexican system offers three educational tracks at the upper secondary level: General, Technical, and Vocational Education. Each school within the assignment system offers a unique track. The general track is academically oriented and includes traditional schools more focused on preparing students for tertiary education. Technical schools cover most of the curriculum of

[3]The submission of school preferences *before* the application of the admission exam is an unusual feature of the COMIPEMS system relative to other centralized assignment mechanisms based on priority indexes. The timing of the events in the application process is meant to provide the system with a ballpark estimate of the number of seats that should be provided through the matching process.

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. A set of 16 technical schools within the assignment system are affiliated with a higher education institution (the National Polytechnic Institute, IPN by its Spanish acronym). These are highly selective options and graduating cohorts usually enroll in tertiary education programs sponsored by the IPN. In what follows, we group general track and IPN-sponsored schools into an "academic" track while all remaining technical and vocational schools are assigned to a "non-academic" track.

Data from a nationally representative survey of individuals aged 26-35 in urban Mexico (ENTELEMS, 2008) shows that attending the general track yields a positive premium of 12 percentage points over the other tracks in terms of average hourly wages. However, these higher returns seem to be driven by those who complete college, which is an uncertain event as only 60 percent of the graduates from the general track do so.[4] This explains the greater degree of dispersion in the wage distribution among those who do not complete college: the ratio of the SD to the mean of hourly wages is 1.37 among high-school graduates of the general track while it is only 0.84 among their counterparts from technical and vocational schools.

2.2 Data and Measurement

Admission records from the 2014 assignment process allow us to observe school preference rankings, admission exam scores, cumulative GPA in middle school, and placement outcomes. We link these records to data from the registration form, which includes additional socio-demographic variables such as gender, age, household assets, parental education and occupation, personality traits, and study habits, among others. We also collected and harmonized additional administrative records from each of the nine high-school institutions that cater to the centralized assignment system for the academic years 2014-15 and 2016-17 – i.e., the first and last statutory year of high school for the students who participate in the 2014 round of the school assignment mechanism. These data allow us to measure enrollment and graduation on time from the upper secondary level for the students in our sample.

We complement the administrative data with individual records from the application of a mock version of the admission exam. The mock exam was designed by the same institution that prepares the official admission exam in order to mirror the latter in terms of structure, content, level of difficulty, and duration (three hours). The test is comprised of 128 multiple-choice questions worth

[4] In the non-academic track, less than 40 percent of the graduates from technical or vocational high schools finish college.

one point each, without negative marking.[5] 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. In order to guarantee that the mock test was taken seriously, we also informed parents and school principals about the benefits of additional practice for the admission exam. We also made sure that the school principal sent the person in charge of the academic discipline and/or a teacher to proctor the exam along with the survey enumerators.[6]

We argue that the score in the mock exam was easy to interpret for the students in our sample while providing additional and relevant information about their academic skills. The linear correlation in our sample between performance in the mock exam and the actual exam is 0.82. Moreover, this relationship does nor vary along the exam score distribution. In turn, the linear correlation between a freely available signal such as the middle school GPA and the admission exam score is only 0.48. Controlling for middle school GPA, the mock exam score also predicts success in high school: a one SD increase in the mock exam score is associated with a 2.6 percentage-point increase (std.err.=0.030) in the probability of graduating from high school on time.

We collect rich survey data with detailed information on the subjective distribution of beliefs about performance in the admission exam. In order to help students understand probabilistic concepts, the survey relied on visual aids [Delavande et al., 2011]. We explicitly linked the number of beans placed in a cup to a probability measure, where zero beans means that the student assigns zero probability to a given event and 20 beans means that the student believes the event will occur with certainty. Students were provided with a card divided into six discrete intervals of the score. Surveyors then elicited students' expected performance in the test by asking them to allocate the 20 beans across the intervals so as to represent the chances of scoring in each bin.[7] The survey

[5]Since the mock test took place before the end of the school year, 13 questions related to curriculum content that was not yet covered were not graded. 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.

[6]We further look at the pattern of skipped questions, as this seems to be the main driver of biases due to non-serious behavior in multiple choice test taking [Akyol et al., 2018]. Without negative marking, the expected value of guessing is always higher than leaving a question blank, which implies that students have no incentive to skip a question. Indeed, the average number of skipped questions in our mock exam was only 1.4 out of 128, and more than 80 percent of the students did not leave any question unanswered. Figure A.1 in the Appendix shows that the skipping behavior in the mock exam is more consistent with binding time constraints rather than lack of seriousness. In addition, we do not find differential skipping patterns according to either the score in the admission exam or personality traits related to effort and persistence.

[7]We include a set of practice questions before eliciting 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?

question eliciting beliefs reads as follows (authors' translation from Spanish):

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

During the pilot activities, we tested different versions with less bins and/or fewer beans to evaluate the trade-off between coarseness of the grid and students' ability to distribute beans across all intervals. We settled for six intervals with 20 beans as students were at ease with that format. Only 6% of the respondents concentrate all beans in one interval, which suggests that the grid was too coarse only for a few applicants. The resulting individual ability distributions seem well-behaved: using the 20 observations (i.e., beans) per student, we run a normality test [Shapiro and Wilk, 1965] and reject it for only 11.4% of the respondents. 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 minus the square of mean beliefs. We alternatively consider the median, defined as the midpoint of the interval in which the cumulative density of beans first surpasses 0.5 (11 beans or more), and the inter-quantile range, defined as the difference between the midpoints of the intervals that accumulate 75% and 25% of the beans.

Figure 1 depicts the timing of the activities related to the intervention (in italics) as well as the important dates of the assignment process and of the school calendar year (in bold). Students took the mock exam early during the second half of the 2013-14 academic year. The survey was administered one or two weeks after the application of the mock test, right before the submission of the school rankings. Both the elicitation of beliefs about exam performance and the delivery of individual feedback on test performance occurred during the survey, in a setting secluded from other students or school staff in order to avoid the role of peer effects and/or social image concerns when reporting [Ewers and Zimmermann, 2015; Burks et al., 2013]. After a first elicitation of beliefs, surveyors showed each student a personalized graph with two pre-printed bars: the average

4. How sure are you that you are going to eat at least one *tortilla* next week?

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

score in the universe of applicants during the 2013 edition of the school assignment mechanism and the average mock exam score in her class. Both pre-printed bars served the purpose of providing the student with additional elements to better frame her own score, which is the main object of interest of the analysis. Surveyors plotted a third bar corresponding to the student's score in the mock exam and then elicit again the subjective distributions of performance in the exam.

2.3 Sample Selection and Randomization

To select the experimental sample, we focus on middle schools with a considerable mass of applicants in the 2012 placement round (more than 30) and that are located in neighborhoods with high or very high poverty levels (according to the National Population Council in 2010). The latter criterion responds to previous evidence that shows that less privileged students tend to be relatively more misinformed when making educational choices [Hastings and Weinstein, 2008; Avery and Hoxby, 2012]. In the year 2012, 44 percent of the applicants enrolled in schools from more affluent neighborhoods took preparatory courses before submitting their school rankings, but this figure drops to 12 percent among applicants from schools in high poverty areas. Among the applicants in our sample, 16 percent report previous exposure to a mock test of the admission exam with performance feedback, and this share is balanced across treatment arms (see Table 1). Despite our focus on less advantaged students, Table B.1 in the Appendix shows that our sample of ninth-grade students is largely comparable to the general population of applicants in terms of initial credentials such as GPA in middle school or admission exam score.

Schools that comply with the criteria imposed are grouped into four geographic regions and terciles of school average performance amongst ninth graders in a national standardized test aimed at measuring academic achievement (ENLACE, 2012). Treatment assignment is randomized within strata at the school level. As a result, 44 schools are assigned to a treatment group in which we administer the mock exam and provide face-to-face feedback on performance, 46 schools are assigned to a "placebo" group in which we only administer the mock exam, and 28 schools constitute a pure control group. Beliefs are measured twice for students in the treatment group, both before and after the provision of feedback, and once for students in the placebo and in the control group. Within each school, we randomly pick one ninth grade classroom to participate in the experiment.[8]

[8] We select at most 10 schools in each of the 12 strata. Whenever possible, we allow for the possibility of over-subscription of schools in each stratum in order to prevent fall backs from the sample due to implementation failures. 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. Some strata are less dense than others and hence contributed to the final sample with fewer schools, which explains why schools that belong to the control group are present in 8 out of 12 strata. Figure A.2 in the Appendix shows the geographic locations of the schools that participate in the experiment.

The mock exam was administered to 2,978 students in 90 schools, and a subset of 2,732 were also present in the follow-up survey. Since the delivery of feedback about test performance took place during the survey, it cannot induce differential attrition patterns. Adding the 912 students from the 28 schools of the control group yields a sample of 3,644 observations. Among those, 89 percent (3,251 students) are matched with the administrative data of the school assignment system. The discrepancy between the survey and the administrative data is driven by students' choices not to participate in the assignment system, and it is balanced across treatment arms (see column 1 of Table B.2 in the Appendix). We focus on the 2,825 of applicants who are assigned in the first round of the matching algorithm since only school rankings (and thus, beliefs) and exam scores matter for their placement.[9] Table 1 provides basic descriptive statistics and a balancing test of the randomization for the pre-determined covariates used in the empirical analysis. Very few and erratic significant differences are detected across treatment arms.

3 Reduced Form Evidence and Treatment Impacts

3.1 Subjective Expectations About Test Performance

We first provide some descriptive patterns of the beliefs elicited in the survey using data from the control group. Figure 2 shows that students allocate very low probability mass to the event of scoring in the lowest or in the highest score interval. The median of beans is zero in the first interval, it increases monotonically over the score support up to the fourth interval, and then goes back to zero in the top interval. The figure displays a large degree of variability of beliefs in intermediate areas of the support, which is consistent with less bunching of the beans over those intervals. Columns 1 and 2 of Table 2 present some correlation patterns between individual characteristics and the first two moments of the belief distributions. Male students as well as those with higher GPAs in middle school and previous exposure to mock exams that provided feedback tend to have higher mean beliefs about their performance in the admission exam. Background also seems to matter, as children from more educated fathers report belief distributions with higher means. Some personality traits can further explain the cross-section of mean beliefs about performance in the admission exam. The variance of the belief distributions is, in turn, less correlated with observable characteristics; the only salient patterns are that male students and those who ascribe themselves as 'perseverant' tend to have tighter belief distributions. These patterns are largely confirmed when

[9]The exposure to the mock test or the performance feedback therein does not systematically affect the fraction of applicants assigned in the first or second round of the assignment process (see columns 2 and 3 of Table B.2 in the Appendix).

we focus on the median and the inter-quantile range as alternative measures of the location and the scale of the individual distributions of beliefs (see columns 3 and 4 of Table 2).

Figure 3 goes on to characterize the gap between subjective expectations and realized performance in the admission test for students in the control group. We define the perception gap based on mean beliefs (solid line) but also introduce more flexible definitions that take into account the dispersion in the individual belief distributions by adding/subtracting one standard deviation to/from mean beliefs (dashed lines). Panel (a) of Figure 3 plots the cumulative densities of these alternative definitions of the perception gap. Focusing on the solid line, about three-quarters of the sample expect to perform above their actual exam score. The divergence between mean beliefs and the score represents, on average, 24 percent of actual performance in the exam, and it seems to be twice as large among students with upwardly biased beliefs (39 percent) than among those with downwardly biased beliefs (17 percent). Even after accounting for the variance in the individual belief distributions, we confirm that students hold inaccurate perceptions about their own performance: the score in the admission test falls outside a one-standard-deviation window around mean beliefs for roughly half of students in the control group. Panel (b) in Figure 3 further shows that students with lower scores tend to have both larger and noisier gaps, as well as over-optimistic expectations about their performance in the test. Instead, students with higher scores have both more accurate and more precise beliefs, and they tend to underestimate their academic skills.

Providing information about individual performance in the mock exam allows students to substantially revise their beliefs. The OLS estimates reported in Table 3 show that mean beliefs in the treatment group decrease on average by 6 points (column 1), while the standard deviation of beliefs goes down by about 1.5 points (column 2). The magnitude of these effects on both moments are relatively similar and they are equivalent to 8-9 percent of the corresponding means in the control group. Taking the mock test without the provision of performance feedback does not generate any differential updating behavior in terms of the mean, but instead seems to increase the noise in students' performance predictions. The standard deviation of the subjective belief distributions increases on average by 1.2 points. This effect is comparable in magnitude to the simultaneous reduction in the dispersion of beliefs generated by the performance feedback.

The average effect of performance feedback in the mock test on the location of the belief distributions masks diverging shifts among applicants with perception gaps of opposing signs (see Figure 3). Column 3 of Table 3 shows that, conditional on exam taking, the delivery of the individual scores in the mock test shrinks the absolute value of the perception gaps by 6.7 points on average. The magnitude of this effect is quite large as it is equivalent to a third of the mean absolute gap in the placebo group.[10]

The top two panels of Figure 4 present non-parametric estimates of the relationship between the perception gaps and the score in the mock exam as well as the relationship between the individual standard deviations of beliefs and the score in the mock exam, estimated separately for students in the treatment group and in the placebo group. The evidence displayed in Panel (a) shows that the update on mean beliefs in response to performance feedback occurs across the entire distribution of the mock test, with corresponding larger gap reductions among lower performing students as they start off with larger biases. As shown in Panel (b), changes in the second moment of the belief distributions are in turn more predominant among better performing students.

3.2 School Choices and Assignment Patterns

In this setting, schools differ in terms of the curricular track, or modality, that they offer. Academically oriented high-school programs tend to provide students with general skills and adequate training to pursue a college education. Non-academic schools, either technical or vocational, focus more on fostering specific skills that are geared toward the access to the labor market after secondary education. Schools also vary greatly in terms of their selectivity and, in turn, the level of their academic requirements for graduation. The assignment mechanism described in Section 2.1 generates sorting across schools based on individual performance in the admission exam. In the context of the assignment mechanism under study, the admission cut-off score is a good proxy for peers' quality and the associated level and pace of instruction – e.g., median scores are almost perfectly correlated with cut-off scores. Since equilibrium cut-off scores in 2013 are observable by the applicants at the time they submit their school rankings for the 2014 placement round, we rely on them to measure selectivity and construct an indicator based on whether the cut-off for any given school falls above or below the median across all schools (irrespectively of the curricular track). As shown in Figure A.3 in the Appendix, academic schools are more selective on average than non-academic schools although there is a large overlap in the cut-off score distributions across tracks.

Relying again on data from the control group, we start by documenting the role of subjective beliefs on school choices and the related placement patterns within the school assignment mechanism. Column 1 of Table 4 shows the estimates of an OLS regression of the share of academic options listed in the school rankings on z-scores of mean beliefs, exam scores, and middle

[10]All the results shown in Table 3 are robust to alternative measures of the location and the scale of the subjective belief distributions, such as the median and the inter-quantile range (75th-25th percentile). See Table B.3 in the Appendix.

school GPA. Mean beliefs have a positive and significant effect on students' demand for academic schools: a one-standard-deviation increase in expected test performance is associated with an average 4.3-percentage points increase in the share of academic options in the school rankings, which is approximately a 7 percent increase when compared to the sample average. The magnitude of the estimated effect of GPA is very similar, whereas the size of the coefficient estimated for the score in the admission test is roughly half and it is not statistically different from zero. Column 2 of Table 4 shows the effect of the same covariates on the probability of being admitted into an academic school. A one-standard-deviation increase in expected test performance is associated with an increase of 4.1 percentage points in the fraction of students admitted into an academic program. The coefficient on exam scores is not statistically significant, and it is not different from the coefficient of mean beliefs (p-value=0.88). In sum, students who think they are a good match with academic programs demand them relatively more and, conditional on their performance in the admission exam, they are more likely to get admitted into one of those programs.

We next discuss whether and how the information intervention alters the realized sorting patterns of students across tracks. To do so, we rely on the score in the mock exam as a proxy of academic achievement that is realized before students submit their school rankings. This implies that we can only compare students in the treatment and in the placebo groups for this part of the analysis, since we do not have information on the score in the mock test for the students in the control group. As mentioned in Section 2.1, school placement under the assignment mechanism exclusively depends on two student-level observable factors: individual school rankings and the score in the admission exam. To the extent that the intervention does not systematically alter exam scores (see column 5 in Table B.2 in the Appendix),[11] any treatment-placebo differences in the final assignment of students across curricular tracks is mainly driven by the observed differential changes in the demand for academic programs. Panel (c) of Figure 4 depicts non-parametric estimates of the slope of the demand for academic programs with respect to the score in the mock test for both the treatment group and the placebo group. The demand for these programs becomes more sensitive to realized achievement for students who receive performance feedback. The vertical difference between these two curves suggests the presence of a negative treatment effect on the share of academic options for students in the bottom half of the score distribution and a positive treatment effect for students with scores in the upper half of the distribution.

[11] These results suggest that the effect of the informational intervention on behavior is short-lived and does not significantly affect the effort exerted for the admission exam, which takes place more than 4 months after the provision of feedback about performance in the mock test. While we cannot rule out other distributional changes in the score of the admission exam due to exam taking and/or the provision of performance feedback, this evidence is consistent with recent experimental findings reported in Azmat et al. [2018], whereby the short-term responses to the provision of information on ordinal ranking to college students in Spain are completely diluted over time.

The OLS estimates reported in column 3 of Table 4 confirm that the provision of performance feedback does not affect the demand for academic programs for students with test scores around the sample average. The positive and significant estimated coefficient on the interaction term between the feedback provision indicator and the z-score in the mock test implies that a one-standard-deviation increase in the score of the mock test increases on average the share of academic schools requested by the applicants in the treatment group by 3.5 percentage points, which represents a 6 percentage points increase with respect to the average in the placebo group. This compositional change in the demand for academic schools significantly alters the assignment patterns realized under the mechanism, as shown in Panel (d) of Figure 4. The corresponding OLS estimates reported in column 4 of Table 4 imply that the fraction of students admitted into an academic program goes up by 4.8 percentage points in response to a one standard deviation increase in the mock exam score, which corresponds to a 10 percent increase relative to the average admission probability in an academic school among the placebo group.[12]

The estimates reported in column 1 of Table 5 show that the score in the admission exam has a more prominent role in explaining the composition of school rankings in terms of selectivity when compared to both mean beliefs and the GPA in middle school. This pattern is much starker when we consider the probability of assignment in a selective school in Column 2, which is in line with a placement mechanism that relies on the exam score to determine priority indexes. The evidence reported in Column 3 and 4 of Table 5 shows that the provision of performance feedback in the mock test does not systematically alter preferences for or assignment into selective schools.

3.3 High-School Trajectories

The centralized assignment mechanism seems to deliver school-placement outcomes that are satisfactory for the great majority of the applicants, at least in the short-run. About 80 percent of the students in the control group enroll in the school they were assigned in the first placement round. However, among these students, only 56 percent graduate on time from high school – i.e. three years after enrollment in tenth grade. There is some heterogeneity by track, with timely graduation rates in the academic and non-academic tracks at 66 and 45 percent, respectively, which may be partly explained by selection issues across tracks. These figures are not peculiar to the experimental sample (see Table B.1 in the Appendix) and they clearly reflect inadequate academic progress through upper secondary education due to either school dropout or grade retention, which are both

[12] All the results shown in Table 4 are robust to alternative measures of academic achievement, such as the score in the admission exam and the cumulative GPA in middle school, which are measured at the end of ninth grade – i.e., after the provision of performance feedback. See Table B.4 in the Appendix.

strong indicators of mismatch between schools and students.

As shown above, the provision of performance feedback improved the alignment between (measured) academic skills and track choices. The associated changes in school placement may thus result in a better match that can further foster individual performance along the education careers of the students in the treatment group. The OLS estimates reported in Column 1 of Table 6 show that, on average, there are no discernible differences in the high-school enrollment rates between students in the treatment and placebo groups when compared to those in the control group. However, conditional on enrollment, the probability of graduation on time is 8 percentage points higher for students who receive performance feedback when compared to those who did not take the test (Column 2). The magnitude of this average effect is quite remarkable, as it corresponds to a 15 percent increase in high-school graduation rates when compared to the sample average in the control group.[13] The average effect of exam taking on high-school graduation rates is positive and roughly half of the size of the coefficient of performance feedback, although it is not statistically different from zero. Also, we can barely reject equality between the coefficient of feedback provision and the one of exam taking (p-value=0.10). This last result suggests that the underlying changes in the dispersion of the belief distributions for the placebo group documented above (see Table 3) may have had an independent effect on school choices and, through those, on the resulting match between schools and students. We will revisit this result in Section 5, where we explore the link between changes in the two moments of the individual belief distributions and school choices.

We next rely on the score in the admission exam as a potential source of variation for the longer-term impacts of the intervention, as it is the most recent standardized measure of student achievement before entering high school. The corresponding estimates are reported in column 3 of Table 6. They reveal that the estimated effect of exam taking on high-school graduation rates accrues from the group of students who score in the bottom quintile of the distribution. The longer-term effects of performance feedback are present around both tails of the score distribution. Even though we find larger effects on timely graduation due to performance feedback than due to exam taking among students in the bottom quintile, we cannot reject that the two treatment effects are equal (p-value=0.22).

[13]Despite all our efforts, we were not able to obtain the enrollment and high school records from one school affiliated with the National University of Mexico State (UAEM), as well as the high school records from another public institution, the National Polytechnic Institute (IPN). In our sample, only one student was assigned to the UAEM school and 171 were assigned to IPN schools (6 percent).

4 School Choice Model

4.1 Empirical Framework

Let the indirect utility of student i from attending school j follow a random coefficients specification:

$$u_{ij} = S_j'\alpha_i + V_{ij}'\beta + \delta_j + \epsilon_{ij}. \tag{1}$$

In this model, S_j is a vector of school j specific factors such as the curricular track or the degree of selectivity. The vector V_{ij} includes observable student-school specific factors that may further shape school choices in this setting such as the physical distance between students and schools. Since the costs of commuting may vary depending on socio-economic status, distance is further interacted with a set of students' background variables (whether or not at least one parent has college education, whether or not the student lives with both parents, and being above or below the median of a household asset index). Abusing notation, we let δ_j capture unobserved school-institution characteristics and ϵ_{ij} represents unobserved idiosyncratic tastes for each school. With nearly 600 schools, it is not feasible to include school-specific constant terms in the model. However, schools are quite homogenous within the nine public institutions sponsoring the high-school programs within the assignment system. Each institution offers only one curricular track and the between-institution variation in cut-off scores is much larger than the corresponding variation within institutions.[14]

The parameter capturing student preferences for school characteristics is further allowed to depend on observable and unobservable characteristics. For simplicity, we consider a linear specification:

$$\alpha_i = \bar{\alpha} + \alpha_\mu \mu_i + \alpha_\sigma \sigma_i + v_i. \tag{2}$$

As shown in Section 2, the returns from attending the academic track in high school feature a clear gradient with respect to individuals' academic skills since they are conditional on successfully completing a college education. We use the mean, μ_i, and the standard deviation, σ_i, of the performance beliefs elicited in the survey in order to characterize the subjective ability distributions of the students in our sample.[15] The v_i term captures individual-specific characteristics unobservable to the researcher that may introduce differential valuation of S_j across students, including

[14] Qualitative evidence from the pilot stages of the intervention indicates that applicants tend to identify the different schools made available through the assignment system mainly through their affiliation with a given institution.

[15] For students in the treatment group, we use the belief distributions elicited after the delivery of performance feedback.

personality traits, parental support, and peer effects, among others.

Substituting (2) into (1), we obtain

$$u_{ij} = S_j'\mu_i\alpha_\mu + S_j'\sigma_i\alpha_\sigma + S_j'v_i + V_{ij}'\beta + \omega_j + \epsilon_{ij}, \tag{3}$$

where,

$$\omega_j = \delta_j + S_j'\bar{\alpha}. \tag{4}$$

We assume that ϵ_{ij} is i.i.d. over i and j with a type-I extreme value (Gumbel) distribution and v_i is i.i.d. over i with a normal distribution. The model described by equations (3)-(4) takes the form of a logit model with random coefficients, which generates flexible substitution patterns across different schooling alternatives.

4.2 Identification

Unlike standard empirical demand models (see, e.g. Berry [1994]; Nevo [2000]; Ackerberg et al. [2007]), here we are not interested in assessing counterfactual changes in observed characteristics that vary over alternatives but rather in changes in school choices induced by movements in the individual belief distributions. Hence, we do not need to separately identify $\bar{\alpha}$ from δ_j in (4). The remaining parameters of equation (3) can thus be identified within a traditional discrete choice framework with school-institution constant terms, ω_j, that capture the overall average valuation of each alternative. The inclusion of subjective perceptions of ability in (2) introduces an endogeneity problem, as μ_i and σ_i may be correlated with the unobservable individual traits captured by v_i. To overcome this issue, we leverage the variation in beliefs induced by the information intervention using a control function approach [Villas-Boas and Winer, 1999; Petrin and Train, 2009].[16]

The random assignment of exam taking and performance feedback across the students in our sample has been shown to differentially alter beliefs (see Table 3) but is otherwise independent of the unobserved component v_i and the random taste shock, ϵ_{ij}. Let treatment status be denoted by the couple of indicator functions $\{T_i, Z_i\}$, where T_i is takes the value of one for the students in the treatment group – i.e. exam taking and performance feedback – and is equal to zero otherwise while Z_i is equal to one for the students in the placebo group – i.e. only exam taking – and is equal to zero otherwise. The orthogonality condition implies that the conditional distribution of the unobserved preferences term in equation (3) depends on the instruments and the belief distributions

[16]For extensions of the control function approach in non-parametric and semi-parametric discrete choice models, see Blundell and Powell [2003, 2004].

only through the associated control function components, ξ_i^μ and ξ_i^σ:

$$F(v_i \mid \mu_i, \sigma_i, T_i, Z_i) = F(v_i \mid \xi_i^\mu, \xi_i^\sigma, T_i, Z_i) = F(v_i \mid \xi_i^\mu, \xi_i^\sigma), \tag{5}$$

which are reduced-form errors of linear projections of μ_i and σ_i on $\{T_i, Z_i\}$ and the full set of covariates that enter in (3). The random coefficients logit model augmented with these control function terms writes as:

$$u_{ij} = S_j'\mu_i\alpha_\mu + S_j'\sigma_i\alpha_\sigma + S_j'v_i + V_{ij}'\beta + \omega_j + (\xi_i^\mu \times S_j)'\lambda_\mu + (\xi_i^\sigma \times S_j)'\lambda_\sigma + \epsilon_{ij}. \tag{6}$$

Under assumption (5), the terms ξ_i^μ and ξ_i^σ in (6) act as a sufficient statistics that fully characterizes the correlation between performance beliefs and the unobserved preference component. The associated λ parameters are jointly identified with the other parameters of the school choice model and can be used to test directly for the endogeneity of beliefs.

4.3 Estimation

Estimation is carried out in two steps. In the first step, we fit linear models for each of the two moments of the belief distribution as a function of the two instruments along with school characteristics, household characteristics, high-school institution fixed effects and indicators for the randomization strata.[17] In the second step, the parameters of the school choice model are estimated by maximum likelihood using the first-step residuals as additional covariates, as shown in equation (6) above. Valid standard errors are obtained by clustered (or block) bootstrap replications of the two-step procedure, with the clusters being defined at the student-level.

A common approach in the school choice literature is to estimate preference parameters in (6) using a rank-ordered logit [Hausman and Ruud, 1987] for the school rankings submitted by the applicants. This estimator can be seen as a collection of conditional logit models: one for the top-ranked school being the most preferred, another for the second-ranked school being preferred to all schools except the one ranked first, and so on. This approach relies on the assumption that the school rankings reflect true preference orderings over schools. When there is limited uncertainty about admission outcomes as in our setting, students are likely to behave strategically. For example, students with a low priority index may skip highly selective schools they truly like since they expect a zero admission probability based on past cut-off scores and their beliefs about test performance [Haeringer and Klijn, 2009; Calsamiglia et al., 2010]. Incompleteness of the rankings, either due

[17] These strata dummies serve the only purpose of adjusting the first-step parameters for the design of the experiment (see Section 2.3), hence the linear predictions that generate the residuals do not include their estimated coefficients.

to the limit of 20 schools imposed by the system or self-censoring, reinforces the incentives to behave strategically.[18]

Recent approaches have proposed to rely on weaker assumptions to estimate student preferences with data from matching mechanisms based on the deferred-acceptance algorithm [Fack et al., forthcoming]. Under stability, or "envy-freeness" of the matching outcome, and given market-clearing cut-off scores, students are satisfied with their placement ex-post [Azevedo and Leshno, 2016]. Hence, one can estimate the parameters in (6) through a school choice model of placement with individual-specific choice sets, which are defined as the set of schools with equilibrium cut-off scores that are weakly lower than the applicant's score in the admission test. In our setting, students have access to very detailed information about schools' cut-off scores in the three past rounds of the assignment mechanism. These data greatly reduce the level of uncertainty regarding the supply side as they tend to be very persistent over time; indeed, the linear correlation between cut-off scores in 2013 and those realized in 2014 is 95 percent. In addition, the coincidence between ex-post feasible choice sets and ex-ante perceived feasible choice sets based on beliefs about own performance is remarkably high. Only 1.8% of the students have less than 90% of their feasible choice set contained in their expected feasible choice set. Expected choice sets are always larger than realized choice sets due to the presence of uncertainty in the belief distributions, and they fully contain realized choice sets for 82 percent of the applicants in our sample.

The final sample we use in estimation is comprised of 2,825 students and 589 school alternatives that are chosen by at least one student, for a total of 1,663,925 student-school observations. Imposing feasible choice sets for each student results in 1,329,441 observations.

4.4 Results

The full set of OLS parameters of the first-stage relationships for both the mean and the standard deviation of individual beliefs is reported in Table B.5 in the Appendix. The estimated treatment effects are very similar to the ones discussed in Section 3.1 and reported in Table 3. The results confirm a sizable and robust negative effect of performance feedback on both the mean and the standard deviation of beliefs and a positive effect of taking the mock test on the standard deviation of beliefs.

Table 7 presents maximum-likelihood estimates of the school choice model with the control function terms but without the random coefficients for school characteristics. Column 1 shows selected coefficients of interest estimated under a rank-ordered logit model while column 2 shows the

[18]The median student in our sample ranks 10 schools and only 2 percent of the students fill-in the entire list of 20 schools. Self-censoring in school rankings may be explained by psychological costs of ranking so many alternatives.

same coefficients estimated under a logit model for school placement with individual-specific feasible choice sets. While the estimated coefficients of the mean of the belief distributions about test performance are very similar across the two estimators, the coefficients of the dispersion of beliefs tend to be attenuated and less precisely estimated under the conditional logit specification. The magnitudes of other estimated coefficients also differ quite substantially between the two specifications, which suggest that the truth-telling assumption behind the observed school rankings may be violated in this setting. Under truth-telling, the parameters estimated in both models are consistent, but the rank-ordered logit is more efficient. Under stability, without truth-telling, only the conditional logit is consistent [Fack et al., forthcoming]. The associated Hausman test [Hausman, 1978] is reported in the bottom line of Table 7 and strongly rejects truth-telling in our data.

Table 8 presents the estimates of the random coefficients school choice model presented in equation (6) as our preferred model specification. Comparing columns 1 and 2, we notice remarkable differences in the estimated parameters associated with the individual perceptions about test performance when endogeneity of the beliefs is ignored. However, most of the other estimated coefficients are very similar in magnitude and precision under both specifications. The role of perceived academic ability in driving sorting patterns across schools is substantially attenuated when students' beliefs are considered exogenous in the discrete choice model. For instance, the estimated coefficient for the interaction effect between the standard deviation of beliefs about test performance and the academic track indicator becomes very large in magnitude and statistically significant once endogeneity is taken into account, whereas the corresponding estimate without the control function terms is negligible. This result underscores the importance of tackling possible endogeneity concerns in the estimation of choice models based on subjective expectations.[19]

Individual beliefs about test performance appear to have relatively large effects on the value of attending an academically-oriented school. A one-standard-deviation increase in the mean of the subjective ability distribution (15.3 points) is equivalent to a decrease in the physical distance to a given school of 3.8km-4.4km (depending on SES), which is more than one third of a standard deviation of the distance across all student-school pairs in the sample. Conditional on the mean of the subjective belief distributions, students who are more uncertain about their own skills find it less attractive to attend more academically-oriented schools. The distance-equivalent effect associated to a one-standard-deviation-increase in the dispersion of the belief distributions (7.9 points) is very similar in magnitude to that implied by a change in the mean, but with the opposite sign. Smaller

[19]Another indication of the bias in the coefficients estimated in column 1 of Table 8 is the fact that most control function terms in column 2 are statistically different from zero (see Table B.6 in the Appendix for the full list of parameter estimates) – as confirmed by the p-value of the F-Test for joint significance of the λ parameters of equation (6), which is reported in the last row of Table 8.

and less precise effects for the role of the location and the scale of the subjective distributions of test performance are found for the value of attending more selective schools. These results are broadly consistent with the reduced-form evidence reported in Tables 4 and 5, which reveal larger responses to performance feedback in terms of the choice of the curricular track than in terms of school selectivity.

The random coefficients estimates reveal a large degree of heterogeneity in preferences for the academic track. This finding documents the potential role of other individual determinants of students' preferences beyond the subjective belief distributions elicited in our survey. While we remain agnostic as to whether or not the information intervention may have altered some of these unobserved factors, the inclusion of random coefficients effectively captures their composite role in explaining school choices and sorting patterns. We did not include a random coefficient for the geodesic distance between students and schools as it features very limited dispersion across students after including the interaction terms with students' socioeconomic status.[20]

5 Using the Model To Unpack Treatment Impacts

5.1 Performance Feedback

The relatively large magnitudes and the opposing signs of the preference parameters related to the two moments of the distributions of perceived ability may provide a rationale behind the heterogeneous effects of performance feedback on choices (see Section 3). As previously shown in Table 3, providing information about individual performance in the mock test reduces the perception gap, moving the location of the distribution of perceived performance closer to actual performance. However, the intervention also reduces the variance in individual beliefs distributions, which could either reinforce or deter the effect through the mean on choices. The divergence in the location of the updating patterns seems to explain the larger effect on school choices among students with relatively higher scores in the mock test, who are also more likely to revise their beliefs upwards. Indeed, among students who update their mean beliefs upwards, the decrease in the dispersion of beliefs strengthens the positive effect of mean beliefs on the probability of choosing academic schools and/or more selective schools. Conversely, among those who update downwards, the reduction in variance counteracts the negative effect on mean beliefs, partially undoing the impact of performance feedback on choices. The net effect of feedback provision on school choices depends

[20]We tried different specifications for the control function terms, such as one specification with a more general correlation structure as well as a non-linear specification. We have also estimated the random coefficients discrete choice model using the median and the inter-quantile range as alternative measures of the location and the scale of the subjective ability distributions. These alternatives yield very similar results as shown in Table B.7 in the Appendix.

thus to the interplay between changes in the mean and in the dispersion of the distributions of perceived performance in the test.

In order to quantitatively illustrate this mechanism, we simulate the school choice model at estimated parameters under our preferred specification (see column 2 of Table 8) for the group of students in the treatment group for whom we have collected performance beliefs twice. We focus on track choices as this seems to be the most salient margin of response in school choices due to the provision of performance feedback. Choices in terms of school selectivity are indeed less responsive to beliefs, as shown in Section 3, as they are mostly determined by the placement allocation mechanism. We first impute the mean and the variance of the belief distributions elicited before the delivery of the feedback and predict choice probabilities under this prior scenario. We then proceed to progressively incorporate observed updates at the individual-level in the mean and in the variance using the distributions of beliefs about test performance elicited after the provision of performance feedback so as to disentangle their relative contribution on track choices. Given the relative small size of the experimental sample (roughly 3,000 students) when compared to the size of the applicants' pool (over 300,000 students), it is highly unlikely for the intervention to have aggregate consequences on the equilibrium admission cut-off scores. Thus, we can safely assume that the feasible choice sets are not altered by the receipt of performance feedback for students in the treatment group and keep them fixed in our simulations.[21]

Panel (a) of Figure 5 plots the average changes in the predicted choice probabilities for an academically-oriented high-school program by quintiles of the score in the mock test while panel (b) depicts the associated average changes in beliefs due to performance feedback. On average, students with test scores in the lowest quintile are 5 percentage points less likely to choose schools from the academic track after receiving performance feedback in the mock test. This drop in the demand for academic programs is mostly driven by large downward updates in the location of the distribution, which are responsible for a 7.5 percentage point decrease in choice probabilities of academic schools. The attenuating effect of the variance is smaller, in line with relatively lower variance reductions observed in this sub-sample. In turn, students in the second quintile of the mock exam distribution experience a close to zero effect of performance feedback on track choices, which can be explained by simultaneous effects on the two moments of the belief distribution that are similar in magnitude but with opposite signs. In the third and fourth quartiles we find modest increases in the demand for academic track schools (a 3 percentage-point increase in the choice probability), which are mostly explained by large reductions in dispersion of beliefs due to the

[21] Extrapolating our experimental findings at a larger scale would require simulating individual school preferences under the new information scenario and computing the resulting equilibrium cut-off scores. This exercise is not feasible in our setting due to the lack of data on elicited beliefs beyond the experimental sample.

performance feedback. The negative effect of the downward revision of mean beliefs is more than offset by the drop in the dispersion of the belief distributions. The ones who experience the largest positive changes in the probability of choosing an academic school are those in the top quintile of the score distribution. The 8 percentage point increase estimated for them is explained by the mutually reinforcing effects that the variance and the mean have on choices.

This evidence is in line with the non-parametric results on track choices shown in panel (c) of Figure 4, which document that the provision of performance feedback generates a steeper gradient in the demand for academic programs with respect to the score in the mock test, with relatively more pronounced effects on choices found among students with higher scores in the mock test.

5.2 Exam Taking

We have shown in Section 3 evidence on the impact of feedback provision. Since our focus is on mismatch, we relied on pre-treatment scores in the mock exam as a measure of ability and evaluated how school choices changed according to such measure. With the results from the model and the simulations at hand, we understand that this comparison yields an upper bound on the estimates of the treatment effect on track choices. Since the increase in the variance for students in the placebo group may have discouraged students to choose the academic track, the estimated treatment effect relative to the placebo group is likely an upper bound for the effect of the feedback provision on track choices.

We use the model to isolate and quantify the relative importance of exam taking when compared to feedback provision on track choices. To do so, we start by predicting choice probabilities based on the estimates of the school choice model (see column 2 of Table 8) and using the beliefs elicited in the survey for the group of students in the control group. We then perform counterfactual simulations by adding the estimated average treatment effect of exam taking to the individual standard deviations of beliefs, as shown in column 2 of Table 3. While admittedly imprecise, this approach aims to overcome the lack of longitudinal belief data for students outside of the treatment group. Panel (a) of Figure 6 reports the resulting average changes in the probability of choosing academically-oriented schools by quintile of the score in the admission exam, which is the only ability measure that is available for students in the control group. Exam taking would uniformly decrease the demand for academic schools by roughly 2 percentage points. Among students who score in the bottom quintile of the distribution of the admission exam, the drop in choice probabilities of academic schools is comparable to the one induced by performance feedback in spite of much larger changes in both the mean and the variance of beliefs triggered by the latter (see panel (b) of Figure 6).

All in all, the evidence drawn from these simulations may help reconcile the longer-term effects of the intervention on high-school trajectories. As we have shown in Section 3.3, the rates of graduation on time increase among students who receive performance feedback in the bottom and top quintiles of the exam-score distribution relative to the control group (see column 3 of Table 6). The results discussed in Section 5.1 showed that this focalized effect at the tails responds to the partial offset that simultaneous changes in mean and variance have on track choices, especially in intermediate ability levels. Uninformative signals, such as exam taking, that exclusively increase the noise in beliefs about academic ability may have a more direct pass-though into track choices. Indeed, the increase in variance induced by exam taking contracts the demand for academic schools among relatively under-performing students and almost to the same extent among their counterparts in the performance feedback group. The displacement of low performing students away from the academic track under exam taking improves their match with schools, thereby enhancing timely graduation rates in high school.

5.3 Counterfactual Updates

We finally consider an updating counterfactual inspired by recent experimental evidence that posits the presence of a 'good news effect' in updating patterns about individuals' own traits [Eil and Rao, 2011; Mobius et al., 2011]. We simulate choice probabilities based on the estimated parameters and the longitudinal belief data according to a scenario in which students in the treatment group change their beliefs from the prior to the posterior distribution only when the performance feedback exceeds the mean of the prior belief distributions. In the case in which the feedback falls short of the prior's mean, we assume that students discard the feedback by leaving their beliefs unchanged.

Figure 7 reports the simulation results for the updating counterfactual described above against the observed updating patterns as a benchmark. The resulting average changes in choice probabilities for an academically-oriented high-school program are reported in Panel (a) while the associated average changes in beliefs by quintiles of the score in the mock test are shown in Panel (b). The simulation results document that the demand for academic programs would monotonically increase across the score distribution. This is not surprising given the rather extreme assumptions on selective updating that we impose in this counterfactual scenario. Since the probability of updating upwards increases with ability (see panel b of Figure 5), the change in choice probabilities will follow the same trend.

When compared to the realized sorting patterns, the average effects in choice probabilities are comparable to the observed treatment effect for students in the top three quintiles of the score in the mock test while they diverge among worse performing students, who are more likely to update downward as a result of the performance feedback. The changes in choice probabilities documented under this counterfactual scenario suggest less assortative sorting patterns across high-school tracks in terms of students' skills when compared to those realized under the feedback-provision experiment. Lower performing students may end up placed into academic schools due to the excessive weight put on positive signals about their own skills, with potentially detrimental consequences on high-school trajectories.

6 Conclusion

Individuals' lack of adequate and timely information about their own academic potential partly explains unfit educational choices that may eventually lead to mismatch and dropout later on. This paper represents one of the first attempts to understand the channels through which the provision of relevant and personalized information about students' own academic ability alter school choices in secondary education and subsequent academic trajectories. We do this in the context of a large-scale centralized school assignment mechanism in Mexico City by combining a research design that provides students with randomized information about their performance in a standardized achievement test, the elicitation of the subjective distributions of academic achievement, and a structural model of school choices that incorporates the role of subjective beliefs.

Our first set of empirical findings show that students face important knowledge gaps related to their own academic potential and skills. Providing individualized feedback on academic performance substantially shifts the location of the individual belief distributions toward realized performance in the mock test and shrinks their dispersion. The treatment-induced changes in beliefs have real consequences on the sorting patterns across high-school tracks that seem to result in a better alignment between individual skills and education careers. Follow-up administrative data confirm that the information intervention effectively improves student outcomes at the end of high school, raising the probability of graduation on time by 8 percentage points.

In order to better understand the link between changes in beliefs and school choices, we propose and estimate a discrete choice model in which subjective beliefs about academic ability shape individual preferences over school characteristics. Simulation results based on the estimates of the model uncover a novel channel through which feedback provision alters school choices: the interplay between the location and the dispersion parameters of the individual distributions of perceived academic ability. We find that the reduction in the noisiness of beliefs caused by the intervention may either compensate or reinforce the effects of mean changes depending on the direction of the update. We also show that noisy signals that remove the mean-adjustment channel have greater pass-through into choices, with very heterogenous impacts along the ability distribution. We finally consider a counterfactual scenario consistent with selective updating based on a 'good news effect'. This exercise is informative about the likely effects of information-provision interventions whenever the updating patterns of the targeted beneficiaries are not rational as often assumed.

These results are informative far beyond the context of Mexico and the centralized admission system in place. Indeed, the setting is ideal to isolate the role of beliefs on school choices, but the implications of this analysis apply as well to other settings where students and parents make long-lasting choices about investments in human capital under uncertainty about individual attributes and skills. The evidence presented here highlights the potential role of policies aimed at disseminating information about individual academic skills in order to provide students with better tools to make well-informed schooling and career choice decisions.

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Figures and Tables

Figure 1: Timeline of Events

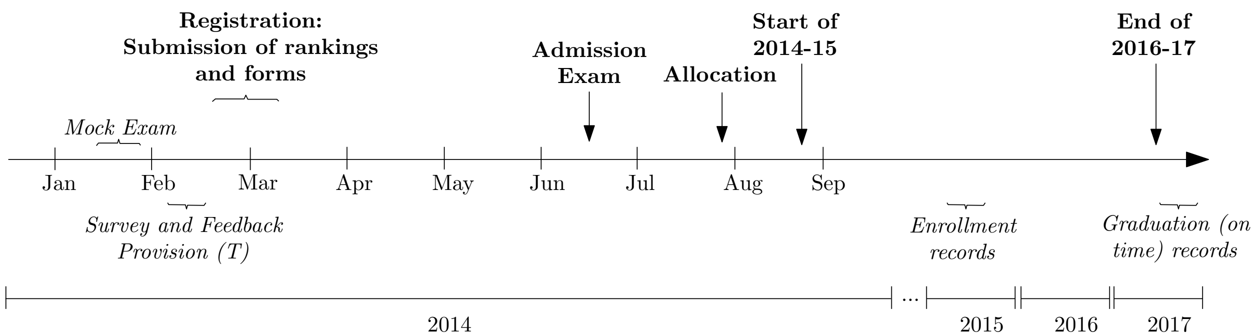
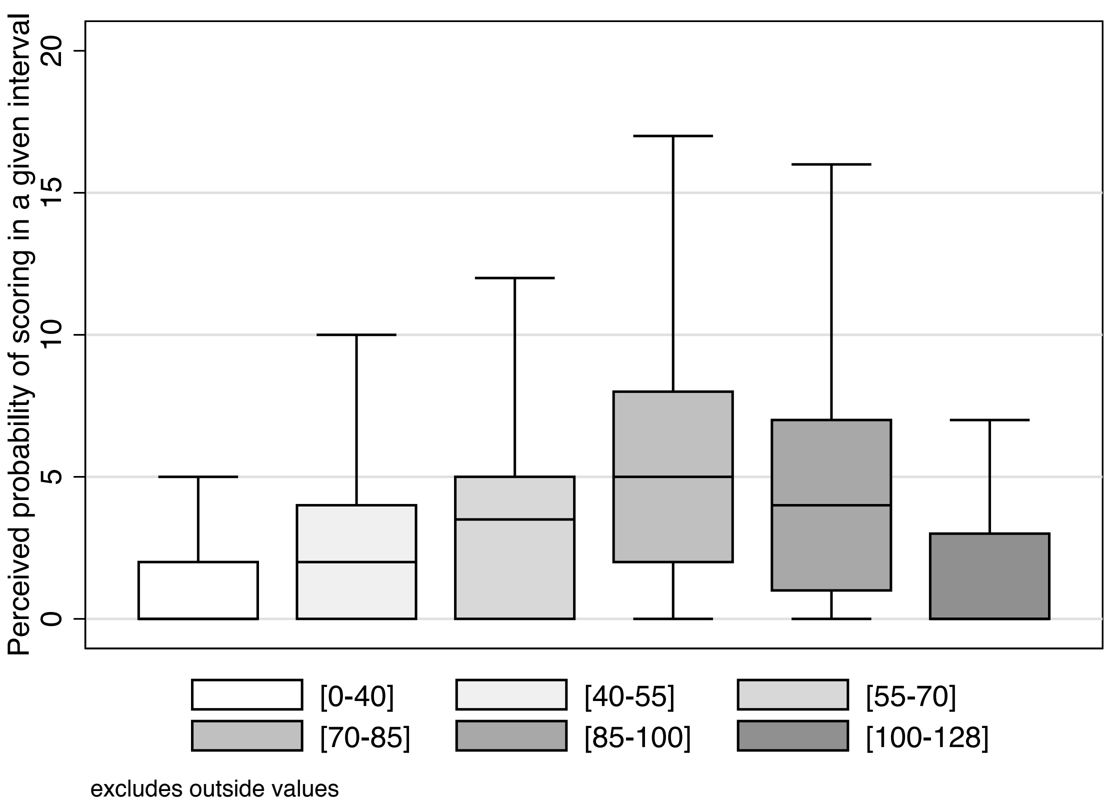
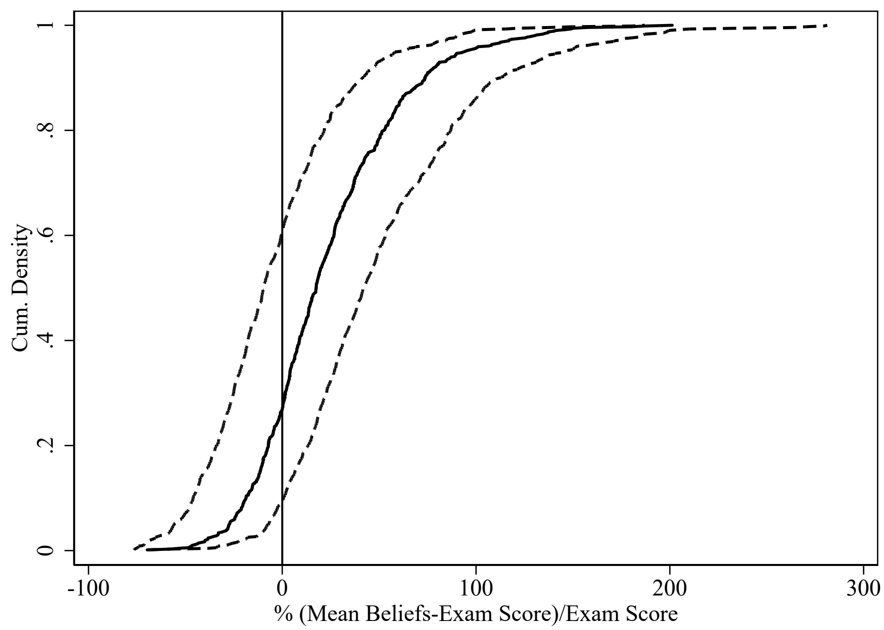


Figure 2: Distribution of beliefs

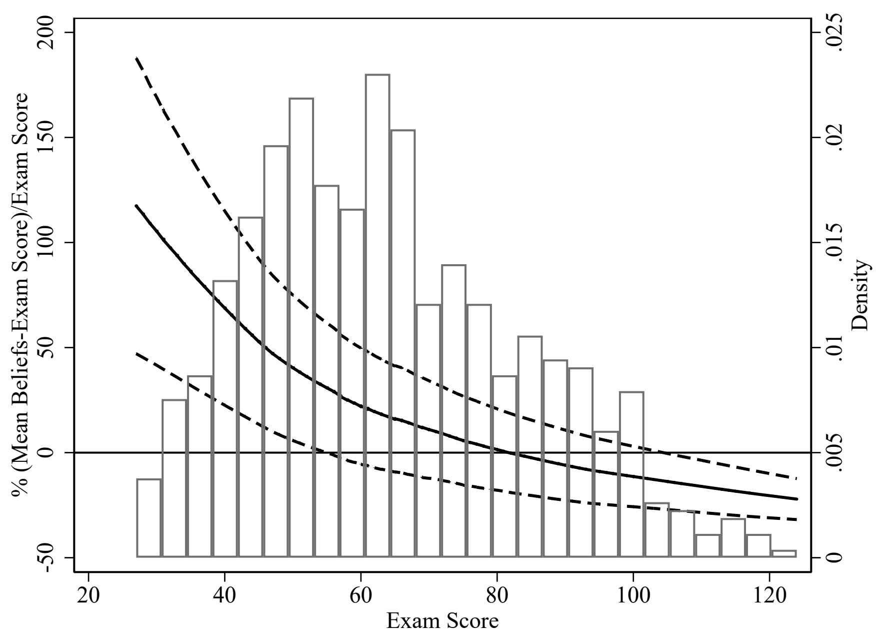


NOTE: Using data for the control group, the Figure plots different moments of the distribution of probability mass (beans) allocated to each interval. The extremes of the whiskers plot the lowest and highest values, while the floor and ceiling of the box denote the 25th and 75th percentiles. The line inside the box represents the median value. Note that extreme values are excluded. Source: Survey data.

Figure 3: Gap between Expected and Actual Exam Score - Control Group



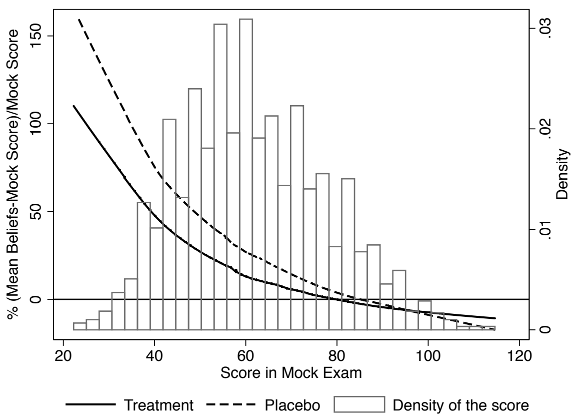
(a) Cumulative Distribution Function



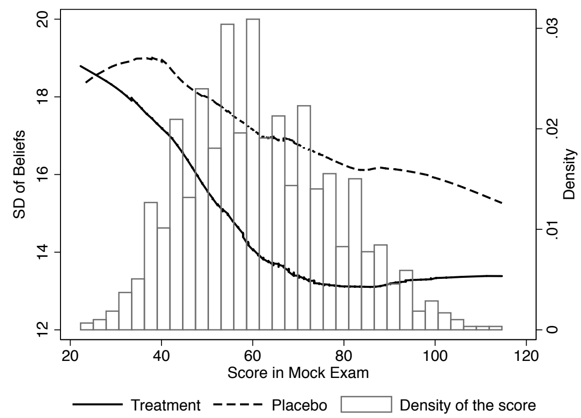
(b) Relationship with the Score in the Admission Exam

NOTE: Using data for the control group, panel (a) shows the cumulative density of the difference between mean beliefs and scores in the COMIPEMS admission exam as a percentage of the exam score. For the same sample, panel (b) depicts locally weighted regressions of the relationship between the relative gap in beliefs and the score in the admission exam. The dashed lines in both panels are obtained by adding/subtracting one standard deviation to/from the mean of the individual belief distributions. Source: Survey data and COMIPEMS administrative records.

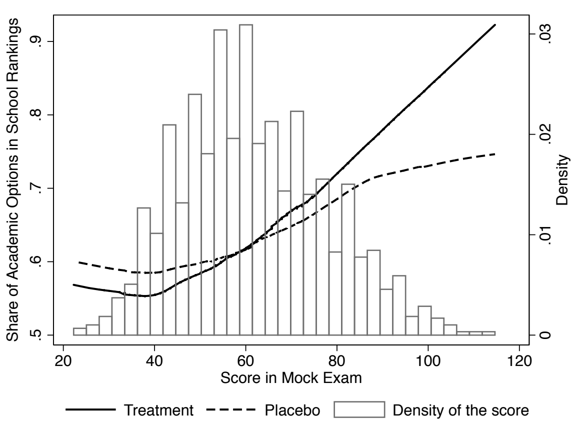
Figure 4: Treatment Impacts - Performance Feedback



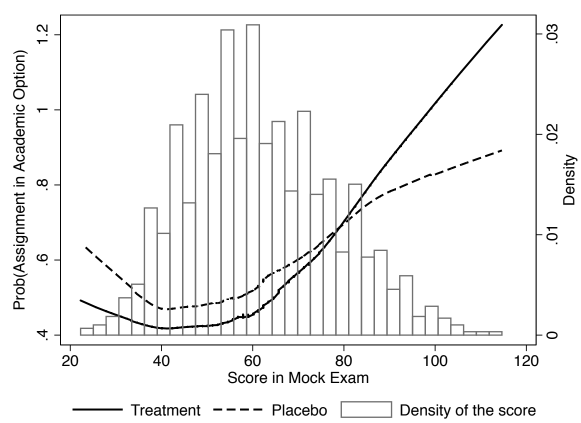
(a) Gap between Expected and Actual Exam Score



(b) Standard Deviation of Beliefs about Exam Score



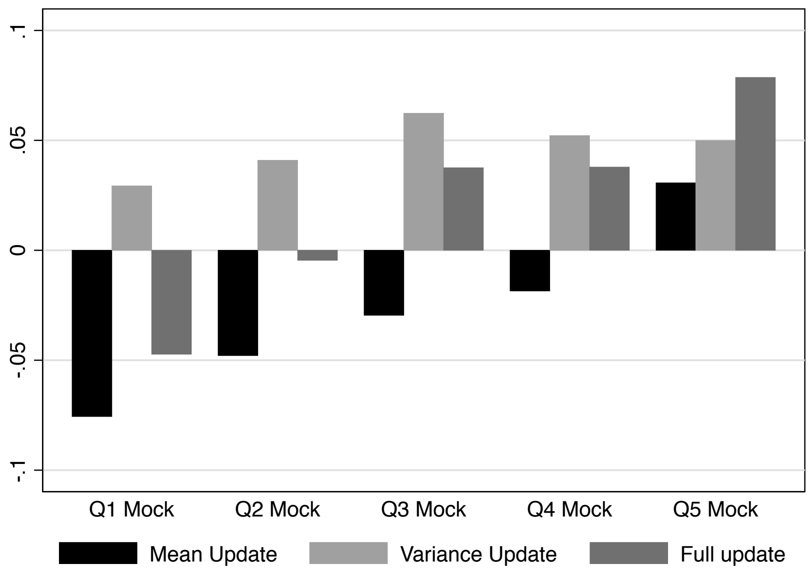
(c) Track Choice



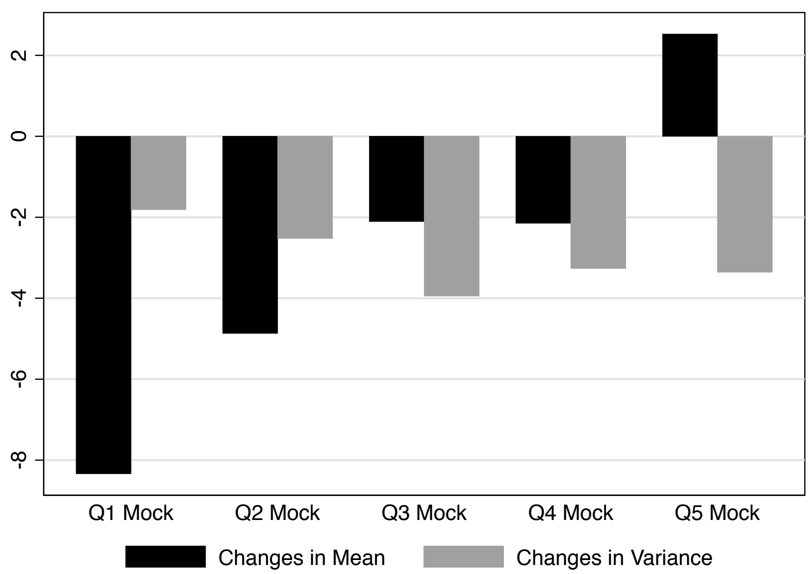
(d) Track Assignment

NOTE: Locally weighted regressions estimated separately for students in the treatment group and in the placebo group. Source: Survey data and COMIPEMS administrative records.

Figure 5: Performance Feedback – Mean and Variance Updates



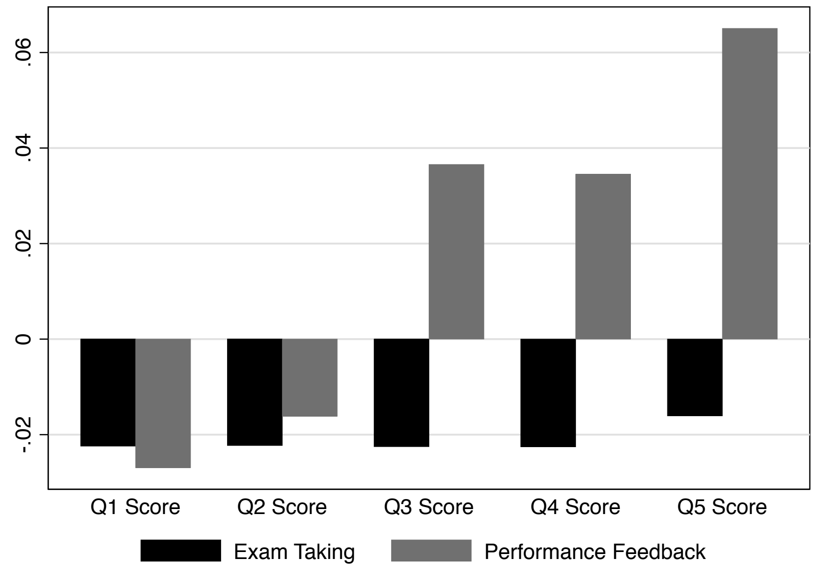
(a) Average Changes in Choice Probabilities for Academic Schools



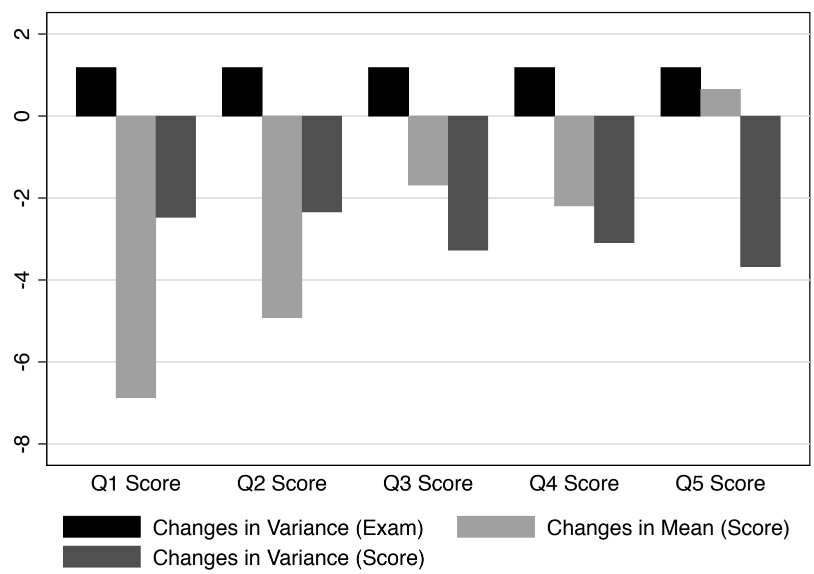
(b) Average Changes in Beliefs Before and After Performance Feedback

NOTE: Simulations based on the estimated random coefficients logit model (see column 2 of Table 8) using data for the treatment group. The bars in panel (a) denote the average difference in the individual choice probabilities computed using observed prior and posterior beliefs. 'Mean Update' denotes a scenario in which only the individual means of beliefs are set to the level of the posteriors. 'Variance Update' denotes a scenario in which only the individual standard deviations of beliefs are set to the level of the posteriors. The bars in panel (b) denote the average changes between posteriors and priors for students in the treatment group, recorded after and before the delivery of performance feedback. Source: Survey data and COMIPEMS administrative records.

Figure 6: Exam Taking Vs. Performance Feedback



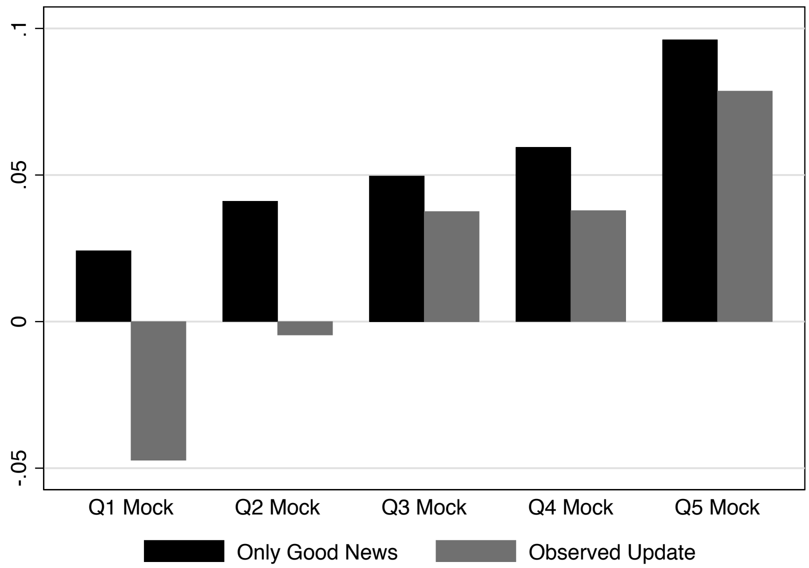
(a) Average Changes in Choice Probabilities for Academic Schools



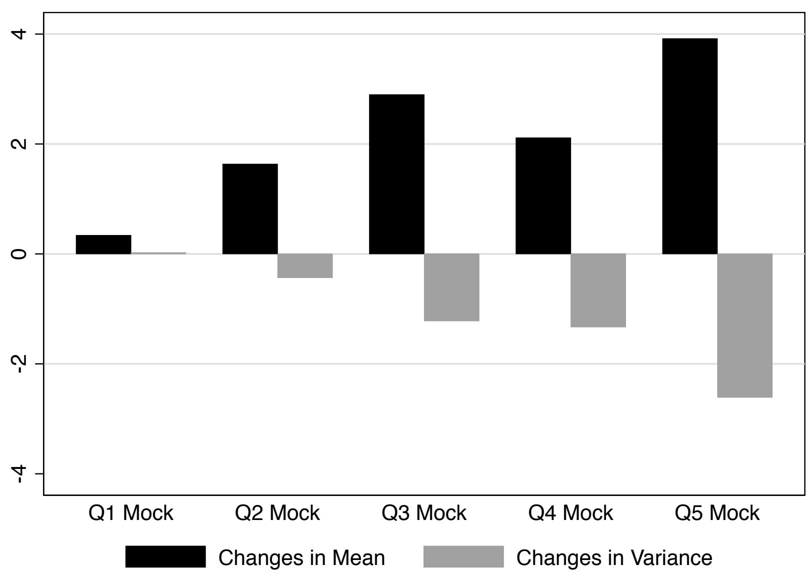
(b) Average Changes in Beliefs under Different Treatment Regimes

NOTE: Simulations based on the estimated random coefficients logit model (see column 2 of Table 8) using data for the control group (Exam Taking) and for the treatment group (Performance Feedback). The black bars in panel (a) denote the average difference in choice probabilities between a scenario with observed beliefs in the control group and a scenario in which we added the average treatment effect of exam taking (see column 2 of Table 3) to the individual standard deviations. The grey bars in panel (a) denote the average difference in the individual choice probabilities computed using prior and posterior beliefs in the treatment group. The black bars in panel (b) denote the corresponding average changes in beliefs for the control group. The lighter-grey and the darker-grey bars in panel (b) denote the corresponding average changes in beliefs for the treatment group. Source: Survey data and COMIPEMS administrative records.

Figure 7: Counterfactual Updates – The 'Good News' Effect



(a) Average Changes in Choice Probabilities for Academic Schools



(b) Average Changes in Beliefs under 'Only Good News' Scenario

NOTE: Simulations based on the estimated random coefficients logit model (see column 2 of Table 8) using data for the treatment group. The bars in panel (a) denote the average difference in the individual choice probabilities computed using observed prior and posterior beliefs. 'Only Good News' denotes a scenario in which both the means and the standard deviations of beliefs are set to the level of the posteriors for individuals with mean priors that are lower than the score in the mock test and they are set to the level of the priors for individuals with mean priors that are larger than the score in the mock exam. The bars in panel (b) denote the associated average changes between posteriors and priors for the treatment group under this 'Only Good News' scenario. Source: Survey data and COMIPEMS administrative records.

Table 1: Summary Statistics and Randomization Check

	Placebo (1)	Treated (2)	Control (3)	T-P (4)	P-C (5)	T-C (6)
Mock exam score	60.939	62.709		1.607		
	(15.582)	(16.419)		[1.080]		
GPA in middle school	8.139	8.152	8.118	-0.005	0.034	0.012
	(0.854)	(0.838)	(0.828)	[0.052]	[0.068]	[0.061]
Scholarship in middle school	0.110	0.114	0.140	0.001	-0.031	-0.020
	(0.313)	(0.318)	(0.347)	[0.016]	[0.021]	[0.018]
Grade retention in middle school	0.123	0.119	0.130	0.001	-0.009	-0.002
	(0.329)	(0.324)	(0.337)	[0.020]	[0.027]	[0.024]
Does not skip classes	0.972	0.976	0.960	0.006	0.014	0.004
	(0.165)	(0.153)	(0.195)	[0.010]	[0.012]	[0.016]
Plans to go to college	0.722	0.716	0.710	-0.009	-0.008	-0.020
	(0.448)	(0.451)	(0.454)	[0.022]	[0.031]	[0.030]
Male	0.439	0.466	0.473	0.024	-0.023	-0.029
	(0.496)	(0.499)	(0.500)	[0.022]	[0.027]	[0.026]
Disabled status	0.141	0.144	0.150	0.001	0.012	-0.000
	(0.349)	(0.351)	(0.357)	[0.017]	[0.022]	[0.024]
Indigenous ethnicity	0.087	0.104	0.105	0.021	-0.020	-0.019
	(0.282)	(0.306)	(0.307)	[0.015]	[0.019]	[0.020]
Does not give up	0.877	0.890	0.888	0.017	0.006	-0.023
	(0.329)	(0.313)	(0.316)	[0.016]	[0.019]	[0.023]
Tries his best	0.750	0.720	0.665	-0.029	0.022	0.039
	(0.433)	(0.449)	(0.472)	[0.022]	[0.030]	[0.029]
Finishes what he starts	0.727	0.714	0.697	-0.019	-0.008	0.014
	(0.446)	(0.452)	(0.460)	[0.020]	[0.026]	[0.027]
Works hard	0.735	0.740	0.706	0.003	0.017	0.025
	(0.442)	(0.439)	(0.456)	[0.024]	[0.031]	[0.031]
Experienced bullying	0.142	0.150	0.172	0.008	-0.006	-0.018
	(0.349)	(0.357)	(0.378)	[0.014]	[0.023]	[0.022]
Lives with both parents	0.796	0.807	0.750	0.014	0.056	0.050
	(0.403)	(0.395)	(0.433)	[0.018]	[0.027]**	[0.027]*
Works	0.319	0.313	0.383	-0.010	-0.065	-0.036
	(0.466)	(0.464)	(0.486)	[0.022]	[0.030]**	[0.032]
Mother with college degree	0.055	0.051	0.038	-0.004	0.007	-0.009
	(0.228)	(0.221)	(0.190)	[0.011]	[0.014]	[0.009]
Father with college degree	0.099	0.104	0.100	0.006	-0.004	-0.032
	(0.299)	(0.305)	(0.301)	[0.016]	[0.025]	[0.020]
High SES (asset index)	0.496	0.524	0.472	0.022	0.067	-0.018
	(0.500)	(0.500)	(0.500)	[0.027]	[0.034]*	[0.029]
Previous mock exam with feedback	0.147	0.191	0.167	0.041	0.004	-0.072
	(0.355)	(0.393)	(0.373)	[0.038]	[0.048]	[0.047]
N. Obs.	1089	1026	710	2115	1799	1736

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 standard errors (in brackets) for the null hypothesis of zero effect. * significant at 10%; ** significant at 5%. Strata dummies included in all specifications, standard errors clustered at the middle school level. Source: COMIPEMS administrative records.

Table 2: Correlates of Individual Beliefs

	Mean	SD	Median	IQR
	(1)	(2)	(3)	(4)
GPA (middle school)	5.506***	-0.378	6.168***	-0.373
	(0.887)	(0.365)	(0.961)	(0.780)
Scholarship in MS	0.435	1.072	0.103	0.552
	(1.442)	(0.855)	(1.681)	(1.524)
Grade retention in MS	0.709	1.340	1.849	0.845
	(1.884)	(1.071)	(2.128)	(1.663)
Does not skip classes	-6.120***	1.395	-6.037***	2.969*
	(1.701)	(1.028)	(1.908)	(1.590)
Plans to go to college	2.033	0.034	0.924	1.787
	(1.300)	(0.760)	(1.508)	(1.377)
Male	3.580***	-1.808***	4.260***	-1.535
	(1.070)	(0.634)	(1.219)	(1.351)
Disabled student	-4.143***	-0.624	-5.132***	1.424
	(1.326)	(1.089)	(1.497)	(2.155)
Indigenous student	-0.947	-0.092	-0.392	-3.517**
	(1.705)	(1.274)	(1.811)	(1.680)
Does not give up	1.761	0.239	2.102	0.709
	(1.427)	(1.082)	(1.622)	(1.810)
Tries his best	3.564**	-0.735	3.808**	-1.498
	(1.453)	(0.927)	(1.512)	(1.486)
Finishes what he starts	2.118	-1.954**	3.112	-3.091**
	(1.958)	(0.749)	(2.161)	(1.365)
Works hard	1.002	0.049	0.355	-1.303
	(1.791)	(1.117)	(2.028)	(1.704)
Experienced bullying	2.260	1.258	1.734	0.917
	(1.752)	(0.914)	(1.940)	(1.526)
Lives with both parents	0.821	0.129	1.066	0.434
	(1.349)	(0.868)	(1.673)	(1.441)
Works	0.065	-0.597	-0.316	-1.229
	(0.863)	(0.442)	(1.182)	(0.832)
Mother with college degree	1.077	0.669	0.786	3.551
	(3.941)	(2.301)	(4.068)	(4.390)
Father with college degree	5.745**	-0.302	5.392**	-1.969
	(2.215)	(1.294)	(2.187)	(2.045)
High SES (asset index)	1.376	0.015	1.066	-0.415
	(1.276)	(0.640)	(1.319)	(0.986)
Previous mock exam with feedback	2.560*	-1.038	3.311**	-1.612
	(1.441)	(0.817)	(1.335)	(0.990)
Number of Observations	710	710	710	710
R-squared	0.213	0.043	0.197	0.048
Number of Clusters	28	28	28	28

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the middle school level are reported in parenthesis. Sample of ninth graders in schools that belong to the control group. All regressions include a dummy indicating one or more covariates has missing data.

Table 3: Subjective Expectations About Performance in the Admission Test

Sample	All		Placebo and Treatment Groups
Dependent Variable	Mean Beliefs	SD Beliefs	|Mean Beliefs-Mock Score|
	(1)	(2)	(3)
Exam Taking	0.441	1.184*	
	(1.359)	(0.625)	
Perform. Feedback	-6.094***	-1.553**	
	(1.158)	(0.664)	
Perform. Feedback (vs. Exam Taking)			-6.713***
			(0.649)
Exam Taking= Feedback (P-value)	0.001	0.001	
Mean Control/Placebo	75.72	17.26	18.72
Number of Observations	2825	2825	2115
R-squared	0.086	0.044	0.095
Number of Clusters	118	118	90

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. OLS estimates. Standard errors clustered at the middle school level and they are reported in parenthesis. All specifications include a set of dummy variables which corresponds to the randomization strata, pre-determined characteristics (whether or not both parents cohabit, parental education, and an asset index), and an indicator variable for whether or not one or more covariates has missing data. The dependent variable 'Mean Beliefs' is constructed as the summation of the mid-values in each discrete interval of the score multiplied by the associated probability assigned by the student. The dependent variable 'SD Beliefs' is obtained as the square root of the summation of the square of the mid-value in each discrete interval of the score multiplied by the associated probability minus the square of mean beliefs. The dependent variable '|Mean Beliefs-Mock Score|' is constructed as the absolute value of the difference between mean beliefs and the score in the mock exam. The sample of Columns 1 and 2 is comprised of ninth graders in schools from the treatment group, the placebo group and the control group. The sample in Columns 3 is comprised of ninth graders from the treatment and placebo groups.

Table 4: Curricular Track

Sample	Control Group		Placebo and Treatment Groups	
Dependent Variable	Share Academic	Assigned Academic	Share Academic	Assigned Academic
	(1)	(2)	(3)	(4)
Mean Beliefs (z-score)	0.043***	0.041**		
	(0.013)	(0.018)		
Exam Score (z-score)	0.023	0.046		
	(0.014)	(0.028)		
GPA (z-score)	0.045***	0.076***		
	(0.013)	(0.021)		
Performance Feedback (vs. Exam Taking)			0.004	-0.043
			(0.019)	(0.029)
Performance Feedback×Mock Score			0.035**	0.048*
			(0.013)	(0.025)
Mock Score (z-score)			0.032***	0.082***
			(0.010)	(0.021)
Exam Score= Mean Beliefs (P-value)	0.371	0.883		
Mean Control/Placebo	0.62	0.52	0.63	0.55
Number of Observations	710	710	2115	2115
R-squared	0.117	0.080	0.104	0.081
Number of Clusters	28	28	90	90

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. OLS estimates. Standard errors clustered at the middle school level are reported in parenthesis. All specifications include a set of dummy variables which corresponds to the randomization strata, pre-determined characteristics (whether or not both parents cohabit, parental education, and an asset index), and an indicator variable for whether or not one or more covariates has missing data. The dependent variable 'Share Academic' denotes the share of high school programs in the school rankings submitted by each applicants that belong to the curricular modality of the General Track or are sponsored by the National Polytechnic Institute. The dependent variable 'Assign Academic' denotes an indicator variable that is equal to one if the applicant is assigned through the centralized mechanism to one high school program that belong to the curricular modality of the General Track or is sponsored by the National Polytechnic Institute and zero otherwise. The sample of Columns 1 and 2 is comprised of ninth graders in schools from the control group. The sample in Columns 3 and 4 is comprised of ninth graders from the treatment and placebo groups.

Table 5: School Selectivity

Sample	Control Group		Placebo and Treatment Groups	
Dependent Variable	Share Selective	Assigned Selective	Share Selective	Assigned Selective
	(1)	(2)	(3)	(4)
Mean Beliefs (z-score)	0.027**	0.026		
	(0.010)	(0.017)		
Exam Score (z-score)	0.061***	0.239***		
	(0.014)	(0.021)		
GPA (z-score)	0.033**	0.012		
	(0.013)	(0.023)		
Performance Feedback (vs. Exam Taking)			0.001	-0.012
			(0.017)	(0.027)
Performance Feedback×Mock Score			0.007	0.012
			(0.009)	(0.021)
Mock Score (z-score)			0.056***	0.192***
			(0.006)	(0.017)
Exam Score= Mean Beliefs (P-value)	0.066	0.001		
Mean Control/Placebo	0.66	0.55	0.77	0.66
Number of Observations	710	710	2115	2115
R-squared	0.221	0.298	0.337	0.287
Number of Clusters	28	28	90	90

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. OLS estimates. Standard errors clustered at the middle school level are reported in parenthesis. All specifications include a set of dummy variables which corresponds to the randomization strata, pre-determined characteristics (whether or not both parents cohabit, parental education, and an asset index), and an indicator variable for whether or not one or more covariates has missing data. The dependent variable 'Share Select' denotes the share of high school programs in the school rankings submitted by each applicants with cut-off score from the previous round of the assignment mechanisms (2013) that is above the median of cut-off scores in the sample. The dependent variable 'Assign Select' denotes an indicator variable that is equal to one if the applicant is assigned through the centralized mechanism to one high school program with cut-off score that is above the median of cut-off scores in the sample and zero otherwise. The sample of Columns 1 and 2 is comprised of ninth graders in schools from the control group. The sample in Columns 3 and 4 is comprised of ninth graders from the treatment and placebo groups.

Table 6: High-School Trajectories

Dependent Variable	Enrollment	Graduation on Time	
	(1)	(2)	(3)
Exam Taking	-0.012	0.046	
	(0.020)	(0.030)	
Performance Feedback	-0.006	0.084***	
	(0.019)	(0.029)	
Exam Taking $\times$Quintile 1 of Exam Score			0.123**
			(0.062)
Exam Taking$\times$Quintile 2 of Exam Score			0.034
			(0.059)
Exam Taking$\times$Quintile 3 of Exam Score			0.011
			(0.054)
Exam Taking$\times$Quintile 4 of Exam Score			0.041
			(0.062)
Exam Taking$\times$Quintile 5 of Exam Score			0.007
			(0.058)
Performance Feedback$\times$Quintile 1 of Exam Score			0.199***
			(0.061)
Performance Feedback$\times$Quintile 2 of Exam Score			0.070
			(0.060)
Performance Feedback$\times$Quintile 3 of Exam Score			-0.014
			(0.056)
Performance Feedback$\times$Quintile 4 of Exam Score			0.061
			(0.066)
Performance Feedback$\times$Quintile 5 of Exam Score			0.104**
			(0.050)
Exam Taking= Performance Feedback (P-value)	0.709	0.105	0.220
Mean Control Group	0.81	0.56	0.56
Number of Observations	2824	2173	2173
R-squared	0.040	0.106	0.120
Number of Clusters	461	393	393

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. OLS estimates. Standard errors clustered at the high school level are reported in parenthesis. All specifications include a set of dummy variables that corresponds to the public institution sponsoring the high schools participating in the centralized system, pre-determined characteristics (whether or not both parents cohabit, parental education, and an asset index), and an indicator variable for whether or not one or more covariates has missing data. The dependent variable 'Enrollment' denotes an indicator variable that is equal to one if students enroll in the high-school programs they were assigned in the first round of the assignment mechanism and zero otherwise. The dependent variable 'Graduation on Time' denotes an indicator variable that is equal to one if the students successfully complete the high-school programs they enrolled in and zero otherwise. The sample of Column 1 is comprised of the students in the treatment group, the placebo group and the control group except for one student with missing high-school enrollment records (see footnote 13 in the text). The sample of Columns 2 and 3 is comprised of the students in the treatment group, the placebo group and the control group who enrolled in the high-school programs assigned to them through the centralized assignment mechanism except for 172 students with missing high-school graduation records (see footnote 13 in the text).

Table 7: Parameter Estimates of Logit Models with Control Function Adjustment

	(1) Ranked-Order Logit	(2) Conditional Logit
Coefficients based on Perceived Ability:		
$\mu\times$ Academic Track	0.0430***	0.0400***
	(0.0083)	(0.0138)
$\sigma\times$ Academic Track	-0.1025***	-0.0871**
	(0.0251)	(0.0437)
$\mu\times$ Selective School	0.0316***	0.0344**
	(0.0088)	(0.0151)
$\sigma\times$ Selective School	-0.0769***	-0.0442
	(0.0214)	(0.0396)
Coefficients for School Characteristics:		
Academic Track	-0.8156	-1.0343
	(0.5465)	(0.8504)
Selective School	-0.2568	-0.8249
	(0.5358)	(1.0168)
Other Coefficients:		
Distance (km)	-0.2168***	-0.2612***
	(0.0061)	(0.0098)
Both Parents$\times$Distance	-0.0046	-0.0020
	(0.0052)	(0.0100)
Parent with College$\times$Distance	0.0159**	0.0164
	(0.0074)	(0.0132)
Above Median SE Index $\times$Distance	0.0257***	0.0311***
	(0.0046)	(0.0088)
Number of Observations	1663925	1329441
Log Likelihood at Convergence	-124434.5	-10920.95
H0: Students are weakly truth telling (p-value)†	0.00001	

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. Estimates obtained by maximum likelihood. Standard errors calculated with 50 bootstrap replications are reported in parenthesis. The sample of Column 2 is comprised of student-school observations with feasible choice sets. Both specifications school-institution fixed effects and the residuals of individual beliefs interacted with the indicator functions for the academic track and above-median cut-off score.

†Estimates of Column 2 are consistent under H0 and Ha. Estimates of Column 2 are inconsistent under Ha and efficient under Ho. If the model is correctly specified and the matching is stable, the rejection of the null hypothesis implies that (weak) truth-telling is violated in the data.

Table 8: Parameter Estimates of Random Coefficients Logit Models

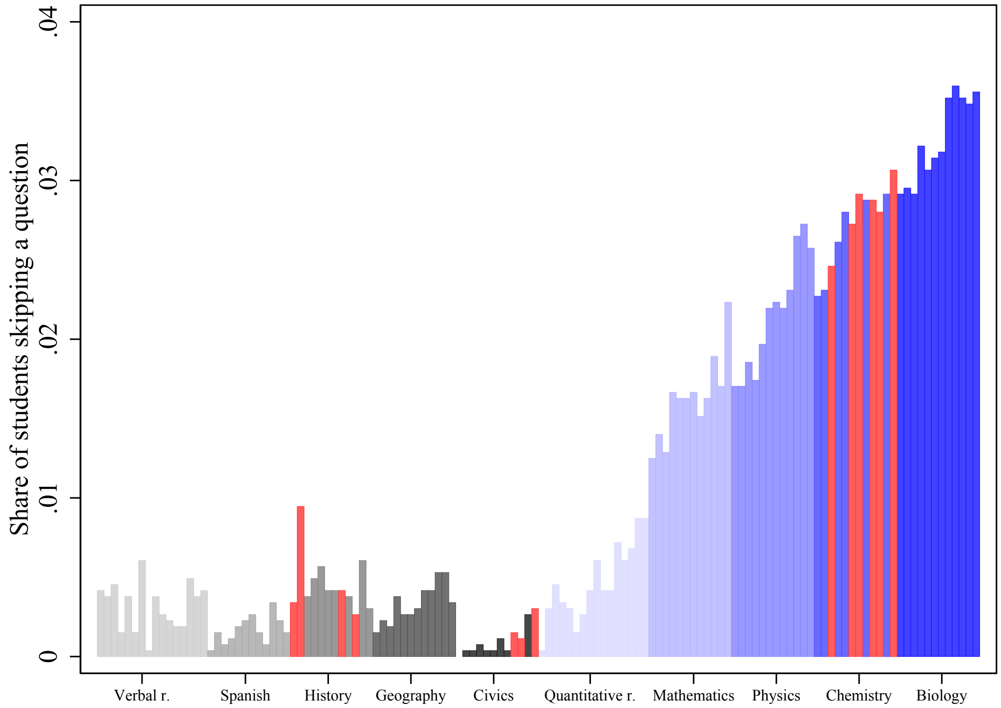
	(1) Without Control Function	(2) With Control Function
Coefficients based on Perceived Ability:		
$\mu\times$ Academic Track	0.0149***	0.0667***
	(0.0045)	(0.0207)
$\sigma\times$ Academic Track	0.0009	-0.1289**
	(0.0079)	(0.0629)
$\mu\times$ Selective School	0.0180***	0.0325*
	(0.0036)	(0.0169)
$\sigma\times$ Selective School	-0.0012	-0.0425
	(0.0062)	(0.0504)
Random Coefficients for School Characteristics:		
Academic Track (Mean)	-0.4879	-2.3166*
	(0.3728)	(1.3247)
Academic Track (SD)	1.8441***	1.8986**
	(0.2277)	(0.8172)
Selective School (Mean)	-0.2209	-0.7522
	(0.2895)	(1.0602)
Selective School (SD)	0.0604	0.0536
	(0.3013)	(0.1356)
Other Coefficients:		
Distance (Km)	-0.2645***	-0.2668***
	(0.0091)	(0.0122)
Both Parents$\times$Distance	-0.0039	-0.0019
	(0.0086)	(0.0104)
Parent with College$\times$Distance	0.0166	0.0157
	(0.0107)	(0.0130)
Above-median SE Index$\times$Distance	0.0315***	0.0334***
	(0.0076)	(0.0089)
Number of Observations	1329441	1329441
Log Likelihood at Convergence	-10906	-10900
F-Test of Control Function Terms (P-value)		0.08891

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. Estimates obtained by simulated maximum likelihood. Standard errors calculated with 50 bootstrap replications are reported in parenthesis. Sample of student-school observations with feasible choice sets. Both specifications include other individual characteristics interacted with distance as additional regressors, as well as school-institution fixed effects. The specification in column 2 also includes OLS residuals of students' beliefs interacted with the indicator functions for the academic track and above-median cut-off score. For a full list of the estimated coefficients in both models, see Table B.6 in the Appendix.

Appendix

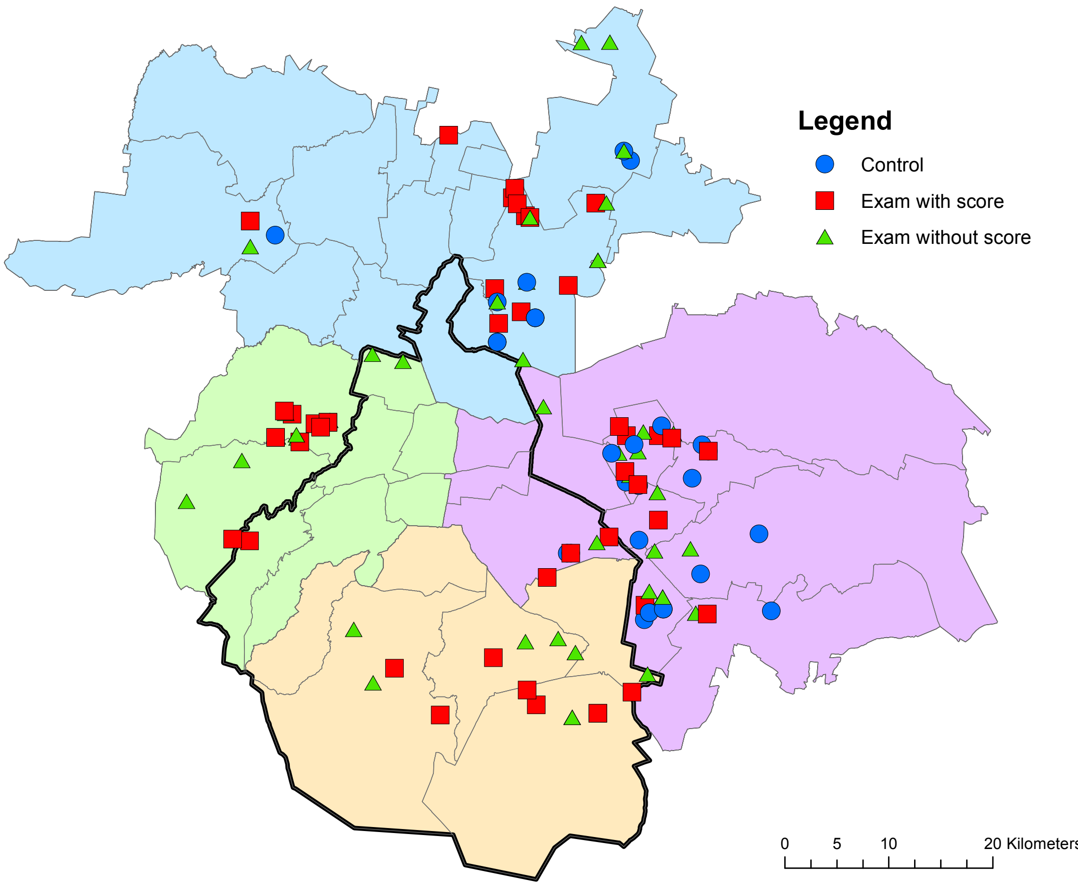
A Additional Figures

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



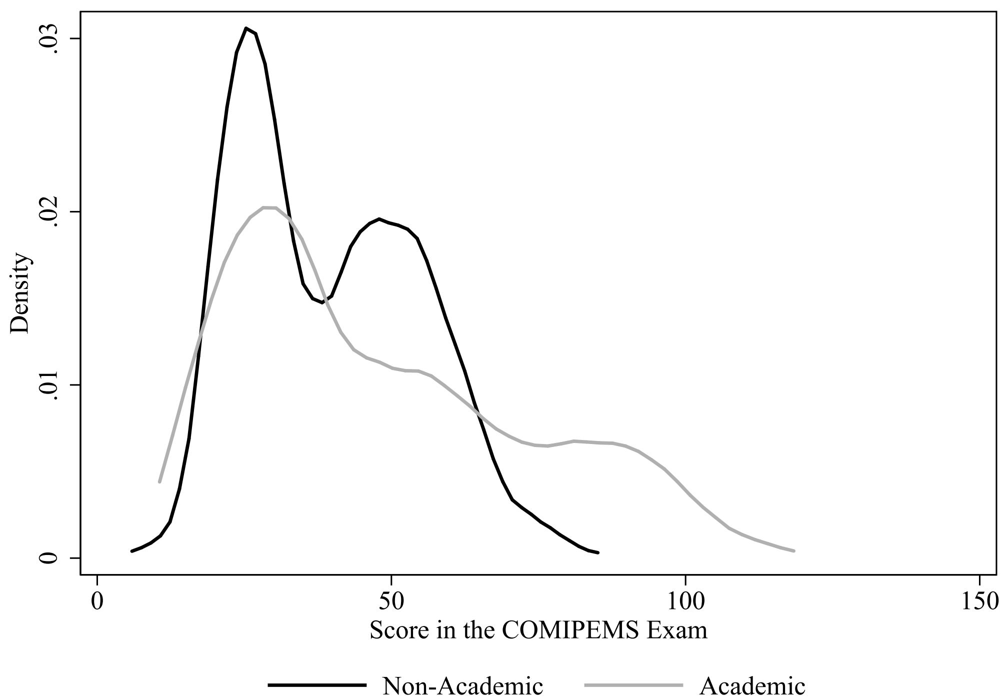
Note: The x-axis orders the 128 questions of the exam in order of appearance. Different colors are used to group together questions from the same section in the exam. Questions in red are the ones excluded from grading since the school curriculum did not cover those subjects by the time of the application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

Figure A.2: Geographic Location of the Middle Schools in the Experiment



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 neighborhoods (municipalities). The four geographic regions that, together with the terciles of the distribution of school performance, determine the strata in the experimental design (see Section 2.3) are shaded in different colors.

Figure A.3: Distribution of Cut-off Scores



Note: Cut-off scores for each high school program refer to the assignment process of the year 2014. Academic schools are defined as those in the general track and those sponsored by the National Polytechnic Institute (IPN). Source: COMIPEMS administrative data.

B Additional Tables

Table B.1: Comparing Population and Sample

Sample	All COMIPEMS		Experiment	
Statistic	Mean	SD	Mean	SD
Student Characteristics				
Works	0.273	0.446	0.333	0.471
Indigenous ethnicity	0.041	0.199	0.098	0.297
Disabled status	0.113	0.317	0.145	0.352
Scholarship in Middle School	0.112	0.315	0.119	0.324
Grade retention in Middle School	0.134	0.340	0.123	0.329
Plans to go to college	0.808	0.394	0.717	0.451
GPA (middle school)	8.130	0.894	8.138	0.842
Lives with both parents	0.746	0.436	0.788	0.409
Mother with college degree	0.117	0.321	0.049	0.217
Father with college degree	0.189	0.391	0.101	0.301
Assignment Outcomes				
Exam score	70.986	21.169	65.683	19.697
Academic Track	0.605	0.489	0.631	0.270
cut-off score for 2013	58.054	24.552	50.866	22.475
Distance from school of origin (Km)	7.052	6.267	9.540	4.814
Institution 1	0.161	0.367	0.107	0.309
Institution 2	0.351	0.477	0.532	0.499
Institution 3	0.175	0.380	0.158	0.364
Institution 4	0.004	0.061	0.011	0.103
Institution 5	0.089	0.284	0.061	0.239
Institution 6	0.007	0.085	0.002	0.050
Institution 7	0.143	0.350	0.075	0.264
Institution 8	0.070	0.256	0.055	0.227
Institution 9	0.001	0.033	0.000	0.019
High School Outcomes				
Enrollment	0.850	0.357	0.822	0.383
Graduation on Time (3 years)	0.477	0.499	0.588	0.492

NOTE: The 'All COMIPEMS' sample consists of all applicants in the year 2014 from the Mexico City metropolitan area who were assigned through the matching algorithm – i.e. the first round of the assignment process described in Section 2.1 (N=203,121). The statistics reported for high school outcomes refer to the cohort of applicants in the year 2006 for which comparable high school trajectories were constructed (N=184,816). The 'Experiment' sample consists of the sample students used throughout the empirical analysis (N=2,825).

Table B.2: Average Treatment Effects on Application and Admission Outcomes

	(1) Participates in COMIPEMS	(2) Placed in 1st Round	(3) Placed Any	(4) Length of Rankings	(5) Exam Score
Exam Taking	0.004	0.006	0.017	-0.076	-0.473
	(0.012)	(0.022)	(0.022)	(0.343)	(1.335)
Performance Feedback	0.005	0.002	0.009	-0.026	-0.149
	(0.011)	(0.023)	(0.024)	(0.344)	(1.295)
Mean Control	0.88	0.88	0.90	9.55	64.07
Number of Observations	3644	3251	3251	2825	2825
R-squared	0.377	0.028	0.032	0.025	0.107
Number of Clusters	118	118	118	118	118

NOTE: OLS estimates. Standard errors clustered at the middle school level are reported in parenthesis. Sample of ninth graders in schools from the treatment group, the placebo group, and the control group. All specifications include a set of dummy variables which corresponds to the randomization strata, pre-determined characteristics (whether or not both parents cohabit, parental education, and asset index), and an indicator variable for whether or not one or more covariates has missing data.

Table B.3: Subjective Expectations About Performance in the Admission Test – Alternative Measures for Location and Scale of the Individual Distributions

Dependent Variable	Median	IQR	|Median-Mock Score|
	(1)	(2)	(3)
Exam Taking	0.660	1.005	
	(1.418)	(1.043)	
Perform. Feedback	-7.715***	-1.811*	
	(1.221)	(0.985)	
Perform. Feedback (vs. Exam Taking)			-6.713***
			(0.649)
Exam Taking= Feedback (P-value)	0.001	0.001	
Mean Control/Placebo	78.95	23.98	18.72
Number of Observations	2825	2825	2115
R-squared	0.087	0.026	0.095
Number of Clusters	118	118	90

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. OLS estimates. Standard errors clustered at the middle school level are reported in parenthesis. The sample of columns 1 and 2 is comprised of ninth graders in schools from the treatment group, the placebo group, and the control group. The sample in column 3 is comprised of ninth graders from the treatment and placebo groups. The median is defined as the midpoint of the interval in which the cumulative density of beans first surpasses 0.5 (11 beans or more). The inter-quantile range (IQR) is defined as the difference between the midpoints of the intervals that accumulate 75 percent and 25 percent of the probability mass. All specifications include a set of dummy variables which corresponds to the randomization strata, predetermined characteristics (whether or not both parents cohabit, parental education, and asset index), and an indicator variable for whether one or more covariates has missing data.

Table B.4: Treatment Effects on Track Choices and Track Assignment – Alternative Achievement Measures

Sample	Placebo and Treatment Groups			
Dependent Variable	Share Academic	Assigned Academic	Share Academic	Assigned Academic
	(1)	(2)	(3)	(3)
Performance Feedback (vs. Exam Taking)	0.007	-0.038	0.010	-0.031
	(0.019)	(0.028)	(0.019)	(0.028)
Performance Feedback×Exam Score	0.026*	0.050*		
	(0.013)	(0.029)		
Exam Score (z-score)	0.042***	0.113***		
	(0.010)	(0.024)		
Performance Feedback×GPA			0.029**	0.066***
			(0.012)	(0.022)
GPA (z-score)			0.039***	0.054***
			(0.008)	(0.014)
Mean Placebo	0.63	0.55	0.63	0.55
Number of Observations	2115	2115	2115	2115
R-squared	0.105	0.106	0.108	0.071
Number of Clusters	90	90	90	90

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. OLS estimates. Standard errors clustered at the middle school level are reported in parenthesis. Sample of ninth graders in schools from the treatment group, the placebo group, and the control group. All specifications include a set of dummy variables which corresponds to the randomization strata, pre-determined characteristics (whether or not both parents cohabit, parental education, and asset index), and an indicator variable for whether or not one or more covariates has missing data.

Table B.5: Estimates of the First Stage – Full Specification

	(1) Mean Beliefs	(2) SD Beliefs
Exam Taking	0.306	1.274**
	(1.351)	(0.613)
Performance Feedback	-5.899***	-1.552**
	(1.122)	(0.657)
Academic Track	-0.002	0.009
	(0.011)	(0.006)
cut-off Score Above Median	2.335***	-0.391***
	(0.136)	(0.070)
Distance (km)	0.044**	-0.006
	(0.022)	(0.010)
Both Parents×Distance	-0.041**	0.006
	(0.021)	(0.010)
Parent with College×Distance	-0.025	-0.014
	(0.033)	(0.015)
Above Median SE Index ×Distance	-0.029	0.009
	(0.020)	(0.011)
Missing value ×Distance	-0.029	-0.003
	(0.026)	(0.015)
Both Parents	2.060**	-0.581
	(0.911)	(0.425)
Parent with College	4.893***	-0.077
	(1.199)	(0.612)
Above Median SE Index	3.039***	-1.052***
	(0.891)	(0.401)
Missing value	1.062	-0.543
	(1.055)	(0.651)
Institution 1	-1.120***	0.154***
	(0.098)	(0.043)
Institution 2	-1.214***	0.159***
	(0.100)	(0.043)
Institution 3	-1.361***	0.200***
	(0.097)	(0.042)
Institution 4	1.605***	-0.134*
	(0.172)	(0.072)
Institution 5	5.855***	-0.352
	(0.547)	(0.247)
Institution 6	7.090***	-0.258
	(0.619)	(0.289)
Institution 7	-1.571***	0.227***
	(0.111)	(0.048)
Institution 8	5.840***	-0.422
	(0.613)	(0.264)
Mean Placebo	75.49	15.71
Number of Observations	1329441	1329441
R-squared	0.096	0.048
Number of Clusters	118	118

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. OLS estimates. Standard errors clustered at the middle school level are reported in parenthesis. Sample of student-school observations with feasible choice sets. All specifications include a set of dummy variables which corresponds to the randomization strata.

Table B.6: Estimates of Random Coefficients Logit Model – Full Specification

	(1) Without Control Function	(2) With Control Function
$\mu\times$ Academic Track	0.0149***	0.0667***
	(0.0045)	(0.0207)
$\sigma\times$ Academic Track	0.0009	-0.1289**
	(0.0079)	(0.0629)
$\xi^{\mu}\times$ Academic Track		-0.0537***
		(0.0202)
$\xi^{\sigma}\times$ Academic Track		0.1313**
		(0.0624)
$\mu\times$ Selective School	0.0180***	0.0325*
	(0.0036)	(0.0169)
$\sigma\times$ Selective School	-0.0012	-0.0425
	(0.0062)	(0.0504)
$\xi^{\mu}\times$ Selective School		-0.0152
		(0.0169)
$\xi^{\sigma}\times$ Selective School		0.0419
		(0.0502)
Academic Track (Mean)	-0.4879	-2.3166*
	(0.3728)	(1.3247)
Academic Track (SD)	1.8441***	1.8986**
	(0.2277)	(0.8172)
Selective School (Mean)	-0.2209	-0.7522
	(0.2895)	(1.0602)
Selective School (SD)	0.0604	0.0536
	(0.3013)	(0.1356)
Distance (Km)	-0.2645***	-0.2668***
	(0.0091)	(0.0122)
Both Parents$\times$Distance	-0.0039	-0.0019
	(0.0086)	(0.0104)
Parent with College$\times$Distance	0.0166	0.0157
	(0.0107)	(0.0130)
Above Median SE Index$\times$Distance	0.0315***	0.0334***
	(0.0076)	(0.0089)
Missing value$\times$Distance	0.0002	0.0009
	(0.0111)	(0.0152)
Institution 1	-1.7604***	-1.6120***
	(0.0846)	(0.0948)
Institution 2	-0.1266	0.0377
	(0.1036)	(0.1158)
Institution 3	0.4118*	0.5238**
	(0.2139)	(0.2122)
Institution 4	-0.3339	-0.4694
	(0.3893)	(0.4398)
Institution 5	3.2279***	2.7541***
	(0.1578)	(0.2983)
Institution 6	4.4910***	3.9477***
	(0.1676)	(0.3192)
Institution 7	-1.7527***	-1.6039***
	(0.1266)	(0.1338)
Institution 8	0.4983	0.0083
	(1.0205)	(14.0419)
Number of Observations	1329441	1329441
Log Likelihood at Convergence	-10906	-10900
F-Test of Control Function Terms (P-value)		0.08891

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. Estimates obtained by simulated maximum likelihood. Standard errors calculated with 50 bootstrap replications are reported in parenthesis. Sample of student-school observations with feasible choice sets.

Table B.7: Parameter Estimates of Random Coefficients Logit Models – Alternative Specifications

	(1) Correlated Random Coeffs.	(2) Polynomial in CF terms	(3) Median as μ and IQR as σ
Coefficients based on Perceived Ability:			
$\mu\times$ Academic Track	0.0594***	0.0676***	0.0481***
	(0.0200)	(0.0215)	(0.0154)
$\sigma\times$ Academic Track	-0.1144*	-0.1280**	-0.1070*
	(0.0604)	(0.0640)	(0.0578)
$\mu\times$ Selective School	0.0329**	0.0337**	0.0266*
	(0.0154)	(0.0165)	(0.0138)
$\sigma\times$ Selective School	-0.0433	-0.0400	-0.0502
	(0.0400)	(0.0529)	(0.0450)
Random Coefficients:			
Academic Track (Mean)	-1.9911	-2.3883*	-0.7225
	(1.2754)	(1.2952)	(1.2361)
Academic Track (SD)	1.8663**	-1.9119**	1.8837
	(0.8116)	(1.4437)	(1.2466)
School Selectivity (Mean)	-0.7510	-0.8203	0.0504
	(1.0495)	(0.9903)	(0.9512)
School Selectivity (SD)	0.0559	0.0505	0.0557
	(0.1389)	(0.1335)	(0.1717)
Cov(Academic,Selectivity)	0.0828		
	(0.1022)		
Other Coefficients:			
Distance - Km	-0.2666***	-0.2667***	-0.2664***
	(0.0102)	(0.0103)	(0.0103)
Above Median SE Index$\times$Distance	0.0332***	0.0334***	0.0328***
	(0.0092)	(0.0084)	(0.0091)
Number of Observations	1329441	1329441	1329441
F-Test of Control Function Terms (p-value)	0.1804	0.3517	0.1783

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. Estimates obtained by simulated maximum likelihood. Standard errors calculated with 50 bootstrap replications are reported in parenthesis. Sample of student-school observations with feasible choice sets. All specifications include other individual characteristics interacted with distance, school-institution fixed effects, and OLS residuals of students' beliefs interacted with the indicator functions for the academic track and above-median cut-off score (not reported). The specification in Column 2 further includes the square terms of the OLS residuals of the first step and their interaction terms, all interacted with the indicator functions for the academic track and above-median cut-off score (not reported). The specification in column 3 includes the median and the Inter-Quantile Range (IQR) of the individual belief distributions as alternative measures for the location and the scale parameters.