Perceived Ability and School Choices*

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Abstract

This paper studies how youths' self-perceptions of ability affect their sorting patterns across schools. We design and implement a field experiment in which ninth-graders from less advantaged backgrounds in Mexico City are provided with individualized feedback about their performance on an achievement test. The treatment shifts both the mean and the variance of the subjective distributions of academic ability. This variation is embedded into a discrete choice model that characterizes the channels through which perceived ability shapes individual preferences over school characteristics. Follow-up data on schooling outcomes suggest that the information intervention improved the match between students and education choices.

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

JEL codes: D83, I21, I24, J24

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1 Introduction

Labor market outcomes vary dramatically across education levels as well as across majors among the college educated [Altonji et al., 2012, 2016]. In several educational systems around the world, tracking starts early and the choice of secondary schooling greatly impacts later trajectories [Dustmann et al., 2017]. For instance, academically oriented secondary schools better prepare students to pursue tertiary education but provide no relevant job market skills for those who do not have the talent, the interest, or the financial resources to continue their studies. Likewise, the benefits of attending schools with better peers and/or more prepared teachers may not materialize for relatively weaker students who may find the environment too challenging or feel out of pace with the level of instruction [Duflo et al., 2011]. Youth face different sources of uncertainty and rely on ex ante expectations of individual-specific returns from alternative education careers when choosing among them. Access to information and knowledge is thus crucial to help parents and/or students make sound school choices. In particular, since individuals from less privileged backgrounds tend to be relatively more misinformed [Hastings and Weinstein, 2008; Avery and Hoxby, 2012], providing them with tools to enable a better match among the available schooling alternatives may enhance social and economic mobility.

This paper focuses on one specific source of subjective uncertainty that has been largely overlooked in the recent literature on information provision and schooling choices: self-perceived academic skills.[1] Even under complete information about the characteristics and the labor market returns of alternative schooling careers, biased misperceptions of own talent and skills may lead to misallocations insofar as students may end up choosing alternatives with high average returns but low individual-specific returns. Our study fills this gap by studying how youth's subjective expectations about their own ability shape school choices and how these choices affect subsequent trajectories in secondary education.

We design and implement a field experiment that provides students from disadvantaged backgrounds with individualized feedback on their academic performance during the transition from middle to high school. We rely on the exogenous variation introduced by the experiment to identify

[1]Several studies have looked at the effects of information about labor market outcomes, school quality, or financial aid and application procedures on schooling decisions: 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].

and estimate the role of the first two moments of the subjective ability distributions on preferences regarding school attributes. Detailed follow-up administrative data further allow us to measure the medium-run effects of the information intervention on schooling outcomes.

The research design is nested within a large-scale assignment mechanism that allocates students across high school programs in Mexico City according to applicants' school rankings and performance on an achievement test. We administer a mock version of the actual test and communicate individual scores to a randomly chosen subset of subjects. We also elicit probabilistic statements about performance beliefs in the actual test using bean counts. Such a task appears a priori challenging, yet our approach turns out to be intuitive and accessible for the age group that the intervention targets. In our setting, the score in the mock exam provides students with a signal that is easy to interpret and contains relevant information about their academic potential. The design of the field experiment also includes a pure control group of applicants who do not take the mock test. This group allows us to distinguish between the effects of taking the test and receiving performance feedback.

Data from the control group show that there are large discrepancies between expected and measured ability, especially among students who perform poorly in the admission test. Conditional on performance, elicited beliefs about own ability are a strong predictor of the share of academically-oriented high school programs listed in the school rankings. Through their impact on school choices, (biased) beliefs also influence placement outcomes in the school assignment mechanism. Results from the experiment show that providing feedback on individual performance substantially shifts the location of the individual belief distribution while reducing its dispersion. The relative adjustment in the mean of the subjective ability distribution is symmetric with respect to the direction of the bias in beliefs. Instead, those who receive positive feedback on their performance reduce the level of uncertainty around their beliefs to a greater extent in both absolute and relative terms. These results are consistent with recent experimental evidence on partial deviations from the Bayesian benchmark in updating patterns of beliefs about individuals' own traits [Eil and Rao, 2011; Mobius et al., 2011].

We next propose and estimate a discrete choice model that incorporates the role of the mean and the dispersion of performance belief distributions in explaining students' preferences regarding schools. Identification of the key parameters of the school choice model is achieved by relying on the changes in beliefs induced by the experiment in a simple two-step control function approach. We use a recently proposed conditional maximum likelihood estimator [Fack et al., forthcoming] that is robust to strategic behavior in submitted school rankings. We find that ignoring the endogeneity of beliefs greatly underplays their role in school choices. The estimates show that students who think highly of their own academic skills find academically-oriented schools more appealing. They also tend to choose schools with relatively more stringent academic requirements, as measured by equilibrium admission cutoff scores. Conditional on the value of expected ability, the model predicts that students with noisier beliefs shy away from academic-oriented and more selective schooling alternatives.

The estimated preference parameters allow us to generate a set of counterfactual scenarios that shed light on the channels through which individual belief distributions affect observed sorting patterns across schools. We conduct simulations using data from the treatment group, for whom we collect beliefs before and after the delivery of feedback. Our results show that the impact of the information intervention on choices is concentrated among students who receive positive feedback. The average student who updates upwards experiences a 20-percentage-point increase in the probability of choosing a school offering an academically-oriented curriculum, while the average effect among students who revise their beliefs downwards is negative and much smaller in magnitude. We show that this differential response corresponds to the interplay between simultaneous changes in the first two moments of the individual belief distributions triggered by information provision: variance reductions reinforce the role of mean updates among students who revise their performance expectations upwards while they counteract mean updates among students who update downwards.

Overall, the treatment seems to generate higher-quality matches between students and schooling careers. Three years after the intervention, the probability of graduating on time from high school is 6 percentage points higher among students who received performance feedback, which corresponds to an 11-percent increase relative to the control group. This effect bundles the direct impact of the treatment on sorting patterns across schools as well as any subsequent behavioral response triggered by the change in educational trajectories. The magnitude of the effect is quite substantial, especially when compared to alternative (and much more expensive) policy interventions that are specifically aimed at reducing dropout levels in secondary education such as conditional cash transfers, scholarships, or counseling [Duryea et al., 2017; Carlana et al., 2018].

The study of individual choices under uncertainty traditionally relied on revealed preferences in order to infer the parameters of the utility function. However, preferences and expectations cannot be recovered from the 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] are two seminal contributions that introduce the notion of uncertainty about ability into the probability of completing a college major. 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. Our work follows the approach in Wiswall and Zafar [2015*a*], who rely on an information experiment and probabilistic beliefs about own ability along with a variety of forecasts about future events in order to estimate a model of college major choice. We elicit subjective expectations in a context where beliefs are tightly linked to concrete, immediate, and high-stakes choices among future schooling trajectories.

This paper is also related to a recent strand of the economics of education literature that leverages either natural or field experiments to uncover the mechanisms through which feedback on students' academic ability affects schooling outcomes. Azmat and Iriberri [2010]; 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 to households with school-age children and average school performance to both households 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 exper-

[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.

iment in Malawi in which parents are provided with information about their children's academic performance.

We contribute to both sets of literature along two main dimensions. First, we leverage an information experiment to estimate the role of self-perceptions about academic ability within an empirical model of school choice. Using the model, we uncover a novel channel through which the information intervention shapes school choices and subsequent trajectories: the interplay between simultaneous changes in both the mean and the dispersion in the distribution of beliefs about own ability. In this sense, our paper is also related to a set of recent studies that use the variation induced by field experiments in order to identify and estimate credible structural models that, in turn, are used to unpack the mechanisms through which the experimental intervention affects outcomes [Attanasio et al., 2012; Galiani et al., 2015; Duflo et al., forthcoming]. Second, the longitudinal span of our data allows us to go beyond *expected* outcomes and assess the gains derived from the provision of information in terms of one *realized* schooling outcome, which is fully comparable across schooling careers and across groups of individuals with possibly diverging belief-updating patterns.

2 The Experiment

2.1 Context

Since 1996, a Metropolitan Commission (COMIPEMS, by its Spanish acronym) has administered public high school admissions in the Mexico City area, comprised of the Federal District and 22 neighboring municipalities in the State of Mexico. In 2014, over 238,000 students were placed in 628 public high schools participating in the centralized assignment mechanism, accounting for roughly three-quarters of high school 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. Applicants are then ranked in descending order according to their scores in a single standardized achievement exam, which takes place at the end of the school year.[3]

A matching algorithm goes through the ranked list of applicants and sequentially assigns students to their most preferred schooling option with available seats. Whenever ties occur in a given school, members of the Commission agree on whether to admit all tied students or none of them. Applicants who are not placed can request admission in other schools with available seats in a second round of the assignment process or search for a seat in schools with open admissions outside the system. Assigned applicants are matched with only one school. Whenever applicants are not satisfied with their placement, they can request admission to another school in the same way unassigned applicants do. All in all, 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 COMIPEMS 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 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. All three modalities are conducive to tertiary education, but wide enrollment disparities exist across tracks. Data from a nationally representative survey for high school graduates aged 18-20 (ENILEMS, 2012) confirm that, compared to those who graduated from technical or vocational high schools, general track graduates in the metropolitan area of Mexico City are 34 percentage points more likely to enroll in a tertiary education institution and 9

[3]The submission of school preferences *before* the application of the admission exam is an unusual feature of the COMIPEMS system when compared to other centralized school 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 made available in each round of the matching process.

percentage points less likely to work after graduating from high school.

A set of 16 technical schools within the COMIPEMS system are affiliated with an established 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 same institution. Thus, in what follows, we group general track and IPN-sponsored schools into an "academic" track. All remaining technical schools and vocational schools are assigned to a "non-academic" track.

2.2 Data and Measurement

Admission records from the 2014 COMIPEMS assignment process allow us to observe individual school rankings, admission exam scores, cumulative GPA in middle school, and placement outcomes. We link these records to data from the registration form, which provides us with additional socio-demographic variables such as gender, age, household assets, parental education and occupation, personality traits, and study habits, among others. We also recover longitudinal trajectories for the applicants in our sample using administrative records at the individual level from their first academic year in high school (2014-2015) and their expected graduation year (2016-2017). The first set of records allows us to match placed applicants to the universe of students enrolled in each school. The second source of data allows us to determine if students in our sample graduated from high school in the statutory time for completing the upper secondary level (3 years).

We complement administrative data with records from the application of a mock version of the COMIPEMS admission exam. We also collect rich survey data that contain detailed information on the subjective distribution of beliefs about performance in the COMIPEMS admission exam. Figure 1 depicts the timing of the activities related to the intervention (in italics) as well as the important dates related to the assignment process and the school calendar year (in bold). Students took the mock exam early during the second half of the academic year. The survey and the provision of feedback for the treatment group took place one or two weeks after the application of the mock test, right before the submission of the school rankings. Beliefs for the treatment group were collected twice in the context of the survey, before and after the delivery of the individual score in the mock exam.

The mock exam was designed by the same institution that prepares the official admission exam in order to mirror the latter in terms of structure, content, level of difficulty, and duration (three hours). The test is comprised of 128 multiple-choice questions worth one point each, without negative marking.[4] 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.

Despite our efforts to make sure students took the mock test seriously, it may still be perceived as a low-stakes exam. Following Akyol et al. [2018], we look at the pattern of skipping questions, as this seems to be the main driver of biases in multiple choice exam scores. Without negative marking, there is no reason to skip a question since the expected value of guessing is always higher than leaving a question blank. We argue that students in our sample did take the test seriously. The average number of skipped questions in the mock exam was only 1.4, and more than 80 percent of the students did not leave any question unanswered. Figure A.1 shows that the skipping behavior observed in the data seems 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.

The linear correlation in our sample between performance in the mock exam and the actual exam is 0.82. More importantly, the correlation between the mock exam score and the exam score is constant along the distribution of the latter. In turn, the linear correlation between a freely available signal such as the middle school GPA and the mock exam score is only 0.45. The mock exam score also predicts success in high school, even after controlling for middle school GPA: a one SD increase in the mock exam score is associated with an increase of 0.20 SD units (std. err.=0.057) in high school GPA and a 2.6 percentage-point increase (std.err.=0.030) in the probability of graduating from high school on time.

[4]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.

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.[5] The survey question eliciting beliefs reads as follows (authors' translation from Spanish):

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

During the pilot activities, we tested different versions with less bins and/or fewer beans. Students seem to be at ease manipulating 20 beans across six intervals, and hence we kept this version to reduce the coarseness of the grid. The resulting individual ability distributions seem well-behaved. Using the 20 observations (i.e., beans) per student, we run a normality test [Shapiro and Wilk, 1965] and reject it for only 11.4% of the respondents. Only 6% of the respondents concentrate all beans in one interval, which suggests that the grid was too coarse only for a few applicants.

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

[5] We include a set of check 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?
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 collection of beliefs works well since only 11 students (0.3%) ended up making mistakes in these check questions.

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.

Both the elicitation of beliefs about exam performance and the delivery of the score in the mock exam occurred during the survey and in a setting secluded from other students or school staff in order to avoid social image concerns when reporting [Ewers and Zimmermann, 2015]. Surveyors showed each student a graph with two pre-printed bars: the average score among the universe of applicants in the 2013 edition of the COMIPEMS exam and the average mock exam score in each applicant's class. When delivering the feedback, surveyors added a third bar plotting each student's score in the mock exam. The first two components of the performance feedback were mainly aimed at scaling the effects of the third component, which is the main object of interest of the analysis.

2.3 Sample Selection and Randomization

We impose two restrictions to select the experimental sample from the universe of potential COMIPEMS applicants. First, we focus on schools with a considerable mass of applicants in 2012 (more than 30). Second, we consider schools located in neighborhoods with high or very high poverty levels (according to the National Population Council in 2010). Students in these areas are less likely to have access to previous informative signals about their own academic potential in general, and about their performance in the COMIPEMS exam in particular. Indeed, data from the 2012 edition of the assignment system show that, on average, 44 percentage of the applicants in schools located in more affluent neighborhoods took preparatory courses before submitting their school rankings, but this figure drops to 12 percent among 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.

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, without providing information about the test results, and 28 schools constitute a control group. Within each school in the experimental sample, we randomly pick one ninth grade classroom to participate in the experiment.[6]

We administer the mock exam to 2,978 students, and a subset of 2,732 were also present in the follow up survey. Since feedback provision was delivered at the end of the follow-up survey, the treatment does not generate differential attrition patterns. Adding the 912 students from the control group yields a sample of 3,644 observations with complete follow-up survey records and, in the case of the treatment and placebo groups, mock exam scores. Among those, 3,251 students are matched with the COMIPEMS administrative data. The 11 percent discrepancy between survey and administrative data reflects applicants' choices not to participate in the COMIPEMS assignment system, and it is balanced across treatment arms (see column 1 in Table A.1 in the Appendix). We further restrict the sample to those students who are assigned through the matching algorithm (i.e., we exclude those assigned in second round) as this is the relevant sample for the estimation of the school choice model (see Section 4.3). The final sample consists of 2,825 students in 118 schools. For consistency, we focus on these students throughout the analysis. Note that the treatment does not systematically affect placement outcomes (see columns 2 and 3 in Table A.1 in the Appendix).

Table 1 provides basic descriptive statistics and a balancing test of the randomization for the main variables used in the empirical analysis. Consistent with the random treatment assignment, very few and erratic significant differences are detected across groups. Table A.2 in the Appendix compares students' characteristics and outcomes in our sample and in the entire population of students assigned in 2014 through the COMIPEMS algorithm (first round). Given the criteria imposed to select the sample, it is not surprising that some socio-economic factors tend to be worse for the students in our sample when compared to the overall population – e.g., the fraction who hold a job while in secondary school or parental education. However, most academically-

[6]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. See Figure A.2 for a geographic representation of the schools in the experiment.

related factors such as GPA in middle school and the fraction who expect to attend college are quite similar across groups. Both the score in the admission test and the main patterns of school assignment are broadly comparable.

3 Subjective Expectations About Academic Ability

3.1 Descriptive Evidence

Using data from the control group, panel (a) of Figure 2 plots the cumulative density of the difference between mean beliefs and scores in the admission exam as a percentage of the score. The solid line measures the gap without taking into account the noise present in the distribution of beliefs, while the dashed lines add/subtract *individual* standard deviations to/from the mean of the individual belief distributions. Discarding the noise, about three-quarters of students 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 when some margin of error is taking into account, students in our sample reveal inaccurate predictions about their own performance: roughly half of students see their score in the admission test fall outside a one-standard-deviation window around their mean beliefs. Panel (b) of Figure 2 further shows that students with the lowest scores tend to have larger gaps between expected and actual performance, while the best-performing students hold more accurate beliefs.

Table A.3 in the Appendix presents some correlation patterns between individual characteristics and beliefs. Male students as well as those with higher GPAs in middle school tend to have higher mean beliefs and greater confidence in their predictions (i.e., lower variance). Mean beliefs are also significantly higher for students with previous exposure to mock exams that provided feedback, but this does not affect the variance. Some personality traits also seem to be linked to the formation of beliefs: students who describe themselves as trying their best tend to have higher mean beliefs, while students who consider themselves perseverant have lower variance in their belief distribution. No systematic relationship is found between beliefs and other background characteristics such as maternal education, indigenous origin, whether or not the student lives with both parents, or socioeconomic level.

Next, we provide suggestive evidence on the potential skill mismatch that biased beliefs may generate in terms of preferences over school attributes and admission outcomes. Again, relying on data from the control group we show the results of an OLS regression of the share of academic options listed in the school rankings on mean beliefs and exam scores. The left panel in Figure 3 reports the associated estimates, where both explanatory variables have been normalized to zero mean and unitary variance. Conditional on exam scores, 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 3.4-percentage points increase in the share of academic options in the school rankings. In turn, the coefficient estimated for actual test performance is smaller and barely significant. The right panel plots the effect of beliefs and actual performance on the probability of being admitted into one academic school. The estimates confirm that beliefs matter: a one standard deviation in *expected* test performance is associated with an increase of 3.8 percentage points in the probability of admission to the academic track. However, actual test performance does not significantly impact students' placement across tracks. This result suggests that students who think they are a good fit with academic programs demand them relatively more often, and, irrespectively of their performance in the admission exam, they are more likely to get into such programs. Notice that, in this setting, academic programs are not always the most demanded/selective ones (see Figure A.3).

3.2 Treatment Impacts

We now turn to discuss the impact of the intervention on individuals' perceptions about their own academic ability. The OLS estimates reported in Table 2 show that, by receiving information about their *own* score in the mock exam students, significantly update their subjective beliefs about their performance in the actual exam. However, taking the mock test without the provision of performance feedback does not generate any differential updating behavior with respect to students in the control group. The estimates reported in columns 1 and 2 show that, conditional on taking the mock exam, mean beliefs in the treatment group decrease on average by 5.7 points, while the standard deviation of beliefs goes down by about 1.7 points. Relative to the control group, these effects represent roughly a 7 percent and a 10 percent reduction in the mean and standard deviation, respectively. The estimates reported in Column 3 further show that, on average, the intervention shrinks the gap between expected and realized performance (as measured by the admission exam score) by 25 percent.

These average patterns mask substantial heterogeneity in the effects of the intervention on beliefs. We thus look at the differential effects of the treatment by *potential* direction of mean updates. We focus on the comparison of treatment and placebo groups and split the resulting sub-sample according to whether mean beliefs (elicited before the delivery of performance feedback for the treatment group) are greater or smaller than the score on the mock test. Notice that both variables used to categorize students are pre-determined and unaffected by the treatment.[7]

The estimates reported in Table 3 show that changes in mean beliefs correspond to the expected direction of the update. Since the extent of the divergence in mean priors is greater for students with upwardly biased beliefs, it makes sense that the intervention generates greater reductions in mean beliefs within this sub-sample. However, the size of the relative adjustments is symmetric and quite meaningful in both groups: the drop in mean beliefs among upwardly biased students represents 12 percent of the average mean beliefs in the placebo group, while downwardly biased students experience an 11 percent increase in their mean beliefs. Instead, those who receive a positive feedback shock reduce the level of uncertainty at a greater extent, both in absolute and relative terms. When compared to the average dispersion of beliefs in the placebo group, the update in the second moment among these students corresponds to a third, whereas it is 10 percent for those with upwardly biased beliefs.

4 School Choices

In this section, we develop a school choice model that incorporates the role of subjective expectations about academic ability in shaping students' preferences over the different schooling options available within the assignment system described in Section 2.1. The sorting patterns realized un-

[7]Within the treatment group, we can compare the expected direction of the update to the actual sign of the change in beliefs before and after the delivery of feedback. We confirm that our ex ante measure of the direction of the update is a good predictor of the actual update induced by the delivery of the score in the mock exam: after the delivery of the signal, 70 percent of the students who start off with upwardly biased beliefs adjust them downwards, while 75 percent of the student with downwardly biased students update their mean beliefs upwards.

der this mechanism exclusively depend 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 A.1 in the Appendix), any effect observed on placement outcomes corresponds to changes in school choices. Therefore, the estimated preference parameters allow us to conduct simulations to understand how the information intervention shaped school choices and, consequently, students' schooling trajectories.

4.1 Discrete Choice Model

Students' self-perceptions of their academic ability influence sorting patterns across schools through their effect on school choices. We postulate the role of two possible channels through which beliefs about own ability may alter the expected value of attending a given school. First, they may shape the value of the (ex ante) perceived match between students and schooling careers. We can incorporate this channel into our empirical model by measuring the degree of complementarity between perceived academic ability and the curricular modality offered by each schooling option, either academic or non-academic. Second, students' self-perceptions are also factored into the expected probability of successful completion or graduation. We can introduce this channel with interaction effects between students' perceived ability and the academic requirements of each schooling option.

Students' preferences regarding schools are represented by the following random utility model:

$$U_{ij} = (B_i \times S_j)'\alpha + S_j'\beta_i + V_{ij}'\delta + \omega_j + \epsilon_{ij}, \tag{1}$$

where B_i denotes student i's perceived ability. We characterize B_i with the mean (μ_i) and the standard deviation (σ_i) of the elicited distribution of expected performance in the exam (see Section 2.2). S_j is a vector of school j specific factors that interact with beliefs: the curricular track or modality (academically-oriented vs. technical or vocational) and the stringency of academic requirements, as measured by school j's cutoff score in the admission exam in the previous year. We use predetermined cutoff scores for 2013 because, unlike equilibrium scores realized during the assignment process under study, they are observable by applicants at the time they submit their choices (see Section 2.1). We acknowledge that the cutoff score captures other factors beyond academic requirements such as peers' quality (e.g., median scores are almost perfectly correlated with cutoff scores). We do not have information to empirically distinguish between these two channels, so we rather interpret the cutoff scores as sufficient statistics for both perceived quality and/or selectivity of the school.

The vector V_{ij} includes other school-specific factors that may 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, above or below median household asset index). Abusing notation, we let ω_j capture school-institution fixed effects[8] and ϵ_{ij} represents unobserved idiosyncratic taste for each school.

The parameters of interest are contained in the vector α, which captures sorting patterns across schools that are driven by students' own ability perceptions. The vector of parameters β_i is allowed to vary across students so as to capture the influence of other unobserved individual traits on preferences over the same school attributes, S_j. By assuming that ϵ_{ij} is i.i.d. over i and j with a type-I extreme value (Gumbel) distribution, the model described in (1) takes the form of a mixed logit [Train, 2003] with error components defined by the structure of the random coefficients β_i, which generate within-student correlation in their choice probabilities for different alternatives. We further assume that β_i is distributed according to a multivariate normal with mean β and diagonal variance-covariance matrix Σ.

4.2 Identification

The assignment algorithm described in Section 2.1 naturally generates ability sorting across schools. However, sorting across curricular tracks is less evident in the data. In fact, there is a large degree of overlap between admission cutoff scores across high school tracks as shown in Figure A.3: the support of the cutoff distributions for schools offering non-academic programs is embedded in the

[8] With nearly 600 schools, it is not feasible to include school fixed effects in the model. However, schools are quite homogenous within the nine public education institutions that take part of the COMIPEMS system: each institution offers only one curricular track and the between-institution variation in cutoff scores is much larger than the corresponding variation within institutions. Qualitative evidence from the pilot stages indicates that students tend to identify schools mainly through their affiliation with a given institution.

wider support of cutoffs for academic schools. We use this variation to separately identify the role of the two school-specific factors contained in the vector S_j.

Perceptions about own ability are likely to be the by-product of a host of unobservable factors (e.g., school environment, peers' influence, parental attitudes, etc.) that can also influence preferences over school characteristics. This introduces correlation between $B_i \approx \{\mu_i, \sigma_i\}$ and ϵ_{ij} in equation (1), which may lead to biased and inconsistent estimates of the preference parameters. We leverage the experiment to deal with this endogeneity issue. We rely on a control function approach that exploits the random assignment of students to receive feedback on their performance in the mock test. As we showed in Section 3.2, the first two moments of the individual belief distribution, μ_i and σ_i, significantly respond to performance feedback, whereas the mere fact of taking the exam does not play much of a role. Thus, we let $T_i = 1$ if student i belongs to the treatment group (test-taking and performance feedback) and $T_i = 0$ if he/she belongs to the placebo group (test taking without performance feedback) or control group and specify the following linear models:

$$\mu_i = \pi_{0\mu} + \pi_{1\mu} T_i + X_i' \pi_{2\mu} + \zeta_i, \tag{2}$$

$$\sigma_i = \pi_{0\sigma} + \pi_{1\sigma} T_i + X_i' \pi_{2\sigma} + \xi_i, \tag{3}$$

where X_i is the same vector of background characteristics that enters into (1) through vector V_{ij}.[9] Identification of the α parameters of the random utility model requires the exogenous shifter T_i to be correlated with the first two moments of the individual belief distributions and independent of the error terms in (1) and in (2)-(3). Formally, the conditional distribution of the unobservable idiosyncratic preferences in equation (1) is assumed to depend on observable individual and school-specific factors only through the error components of (2)-(3):

$$F(\epsilon_{ij} | \mu_i, \sigma_i, S_j, V_{ij}, \omega_j, T_i) = F(\epsilon_{ij} | \zeta_i, \xi_i, S_j, V_{ij}, \omega_j, T_i) = F(\epsilon_{ij} | \zeta_i, \xi_i) \tag{4}$$

Let us rewrite the discrete choice model in (1) as $U_{ij} = D_{ij} + \tilde{\epsilon}_{ij}$, where D_{ij} is the deterministic

[9]Parametric control function specifications have been proposed in the context of mixed logit models by Villas-Boas and Winer [1999] and Petrin and Train [2009]. For extensions of the control function approach in non-parametric and semi-parametric models, see Blundell and Powell [2003, 2004].

component of the utility and the new error term becomes:

$$\tilde{\epsilon}_{ij} = \epsilon_{ij} - (\zeta_i \times S_j)'\lambda_1 - (\xi_i \times S_j)'\lambda_2. \tag{5}$$

We assume that $\tilde{\epsilon}_{ij}$ is i.i.d. over i and j with a type-I extreme value (Gumbel) distribution. This flexible error structure preserves the scale of the utility specification in (1), which is normalized by setting the scale of the extreme value distribution for $\tilde{\epsilon}_{ij}$.

4.3 Estimation

Estimation is carried out in two steps. First, equations (2)-(3) are estimated by OLS. In a second step, the residuals $\hat{\zeta}_i$ and $\hat{\xi}_i$ are captured and introduced in the choice model (1) interacted with the school-specific factors S_j as control function components. Parameters in this step are estimated via simulated maximum likelihood. As detailed in Section 4.2, these additional terms account for the potential joint determination of individual beliefs and preferences over schools. A simple joint test of significance of the associated parameters – i.e., the λ parameters in equation (5) – can be performed in order to directly test this claim. Valid standard errors for the preference parameters are obtained by bootstrap replications of the two-step procedure.

A common approach in the school choice literature is to estimate (1) fitting a rank-ordered logit [Hausman and Ruud, 1987]. This model 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 two key assumptions. First, the school rankings submitted reflect students' true preference orderings over schools. Second, the number of schools ranked by any student is exogenous, in the sense that it should not be correlated with neither individual or school-specific factors that enter the random utility model (1), nor the score in the admission exam that determines students' priority indices in the assignment system.

The COMIPEMS matching algorithm (see Section 2.1) resembles a serial dictatorship mechanism with strict priority indexes. Agents are ranked (by their score in the placement exam) and allowed to choose, according to their priority order, their favorite object (school) from amongst the remaining options with available slots. When rankings are incomplete, as in the case of the

COMIPEMS system, agents may strategically leave out preferred goods with admission probabilities close to zero [Haeringer and Klijn, 2009; Calsamiglia et al., 2010]. Application data in our sample are consistent with strategic behavior: about 20 percent of the applicants exclude schools with the previous year's admission cutoff scores above their expected scores, μ_i.

An alternative approach consists in relaxing the assumption of (weak) truthfulness in school rankings and instead assume that the matching outcome between students and schools is stable [Fack et al., forthcoming]. This assumption is equivalent to imposing the (ex post) equilibrium outcome of the matching game between students and schools, under which every student makes the optimal choice given market-clearing cutoff scores.[10] When compared to truth-telling, stability is a more plausible assumption in our setting since (i) admission cutoffs are easy to predict based on the previous three years of history available to students prior to registration, (ii) there are many schools (more than 600) potentially available for each student, and (iii) the number of schools that are ranked is large (the median student in our sample ranks 10 schooling options, and only 7 percent of the students request five or less schools). Given the fairly small size of the experimental sample (roughly 3,000 students) relative to the size of the applicant pool (more than 300,000 students), it is unlikely that changes in the sorting patterns of the treated group in our sample have aggregate consequences for the system as a whole.

Under truth-telling, both models are consistent, but the rank-ordered logit is more efficient. Under stability, without truth-telling, only the conditional logit is consistent. Table A.4 in the Appendix compares the estimation results for simplified versions of the discrete choice model (1) without random coefficients and without control function terms. Under the assumption that the matching outcome is stable, the Hausman test [Hausman, 1978] strongly rejects truth-telling in our data.

We thus estimate the preference parameters with a conditional mixed logit model for school placement with personalized choice-sets as determined by individual scores in the admission exam and the equilibrium (ex-post) cutoffs. Let $\bar{J}_i$ denote the individual-specific feasible choice sets, defined as the subset of schooling options with a cutoff score that is weakly lower than the actual

[10]In practice, the stability-based estimator contains misspecification because some students may not be assigned to their favorite feasible school due to preference misrepresentation [Hassidim et al., 2017]. However, it can be shown that the fraction of students who are not assigned to their favorite feasible school converges to zero at an exponential rate [Azevedo and Leshno, 2016; Fack et al., forthcoming], implying that misspecification vanishes as market size increases. Therefore, the likelihood function based on stability is correctly specified in the limit.

score for each student, and let j^* denote the placement outcome. The corresponding simulated log likelihood function is:

$$SLL = \sum_{i=1}^{N}\sum_{j=1}^{J}\frac{1}{R}\sum_{r=1}^{R} D(\beta_{ij}^{(r)}) \times I(j = j^*) - \sum_{i=1}^{N} \ln\left(\sum_{k\in\bar{J}_i}\frac{1}{R}\sum_{r=1}^{R}\exp(D(\beta_{ik}^{(r)})\right), \qquad (6)$$

where, as before, $D(\cdot)$ is the deterministic component of the utility function and $\beta^{(r)}$ is the rth draw of the random coefficients from the normal density $\phi(\beta_i; \beta, \Sigma)$. Maximization of (6) is embedded in an iterative procedure with evaluation at $\beta^{(r)}$ and $\Sigma^{(r)}$ in each round.

As mentioned in Section 2.3, we exclude students placed in the second round of the assignment, where the algorithm is no longer in use, since they do not contribute to the likelihood function depicted in (6). The final sample we use in estimation is comprised of 2,825 individuals and 589 school alternatives,[11] for a total of 1,663,925 observations. After imposing feasible choice sets for each student, we end up with 1,329,441 student-school pairs.

4.4 Results

OLS estimates of the parameters of equations (2)-(3) are reported in Table 4. The estimated (pooled) treatment effects are very similar to the ones disaggregated by treatment arms, which are discussed in Section 3.2 and reported in Table 2. In Table A.5 in the Appendix we further report first stage robustness checks for a battery of alternative measures of beliefs. As expected, results are very stable across alternative measures of beliefs, confirming a sizable and robust effect of the intervention on both the mean and the standard deviation.[12]

Table 5 presents the estimates of the school choice model outlined above for selected parameters of interest (see Table A.6 in the Appendix for the full list of parameter estimates). In order to facilitate the interpretation of the estimated coefficients for the stringency of academic requirements, we discretize the cutoff score in the previous year and construct an indicator function for whether or not each school falls above or below the median in the sample. This enables a more

[11] We drop alternatives that are never chosen by any student in our sample.

[12] Robustness checks include imputing mean and variance using the minimum or maximum points of each discrete interval instead of the mid-point. We have also considered the median and the interquartile range as alternative measures of the location and scale parameters of the belief distribution. The corresponding estimates of the α parameters in the discrete choice model (1) are remarkably stable across these alternative measures of beliefs (see Table A.7 in the Appendix).

straightforward comparison to the coefficients estimated for the curricular track, which is also dichotomous.

Comparing columns 1 and 2 of Table 5, we first notice remarkable differences in the preference parameters associated with subjective beliefs estimated without and with the control function terms. The role of perceived academic ability in driving sorting patterns across schools is substantially attenuated when students' beliefs are considered exogenous in the utility function. For instance, the estimated coefficient for the interaction effect between the level of uncertainty about individual beliefs 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. On the contrary, most of the estimated coefficients of the other terms in the choice model (1) are very similar in both magnitude and precision under the two specifications. Another strong indication of the bias in the coefficients estimated in column 1 is the fact that most control function terms in column 2 are highly statistically significant, as confirmed by the p-value of the F-Test for joint significance of the interaction effects between the first stage residuals and school characteristics (see last row of Table 5 and Table A.6 in the Appendix for the individual coefficients). This result underscores the importance of tackling possible endogeneity concerns in the estimation of choice models based on subjective expectations.

Focusing on the results that deal with the endogeneity of beliefs, we find that students who think highly of their own academic skills find academically-oriented schools more appealing. They also tend to choose schools with relatively more stringent academic requirements, as measured by past entry cutoff scores. To put things in perspective, we interpret some coefficients in terms of an equivalent effect through changes in distance. Keep in mind that, in our sample, the average distance to any of the schools available through the assignment system is 9.5km (see Table A.2 in the Appendix).

Taking the treatment impacts on beliefs from Table 4 together with the estimated coefficients in Table 5, we can calculate the distance-equivalent average change in the valuation of academic schools generated by treatment-induced changes in beliefs. Among the low socio-economic group, a decrease in mean beliefs corresponding to the treatment impact reduces the average valuation of academic schools as much as an increase in the geodesic distance to the school of 1.2km ($\approx \frac{-5.5*0.06}{0.27}$). The effect for students with better socio-economic status is even greater and equivalent to an increase in distance of about 1.4km ($\approx \frac{-5.5*0.06}{0.24}$) due to their lower coefficient on distance. A slightly lower effect of beliefs is identified on the valuation of schools with above-median academic requirements. Additionally, we find that students who are more uncertain about their own skills find it less attractive to attend more academically-oriented schools. A reduction in the standard deviation of beliefs equivalent to the average treatment effect of -2.4 points (see column 2 of Table 4) leads to an increase in preferences for academic and/or more difficult schools that is roughly equivalent to the effect of the treatment-induced change in the mean. In sum, beliefs have relatively large effects on choices.

The estimated parameters of the random coefficients reveal a large degree of heterogeneity in preferences for the academic attributes of the schools, especially when compared to the magnitudes of the mean valuations.[13] These findings highlight the potential role of other individual determinants of students' preferences beyond the subjective belief distributions elicited (e.g., perceived skills unrelated to academic ability and personality traits). While we remain agnostic as to whether or not the information intervention may change some of these unobserved factors, the inclusion of random coefficients into the model effectively captures their composite role in explaining school choices and sorting patterns.[14]

4.5 Sorting Patterns Across Schools

The relatively large point estimates and the opposing signs of the preference parameters related to the two moments of the perceived ability distribution suggest that the information intervention may have had differential effects on choices. Among students who update their mean beliefs upwards, the decrease in the dispersion of beliefs may strengthen the positive effect of mean beliefs on the probability of choosing schools from the academic track and/or schools with relatively more stringent academic requirements. Conversely, among those who update downwards, the reduction in variance counteracts the negative effect on mean beliefs, partially undoing the impact of the

[13]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 (results not reported and available upon request).

[14]Even if the elements of β_i are uncorrelated such that Σ is diagonal, the unobserved portion of utility is still correlated over alternatives. We tried different specifications considering a more general correlation structure or different distributional assumptions of the random coefficients. We also tried non-linear specifications for the control function terms, including a series expansion of the residuals. All these alternatives yield very similar results (see Table A.8 in the Appendix).

information intervention on school choices.

In order to quantitatively illustrate the mechanisms at play, we simulate the school choice model at estimated parameters (see column 2 of Table 5) for the sub-sample of treated students by imputing the two moments of the belief distributions with the data elicited *before* feedback was provided. Starting from the predicted choice probabilities under the "no update" scenario, we can then compute average changes in school choices by simulating the effect of different updating counterfactual scenarios. In particular, we can progressively incorporate observed updates in mean and variance so as to disentangle their relative contribution to the overall treatment effect on choice probabilities.

One key assumption for this exercise is that the information intervention affects students' preferences regarding schools exclusively through changes in beliefs. This rules out the possibility that the delivery of performance feedback in the mock test altered other behaviors and responses within the assignment mechanism, such as participation in the COMIPEMS system, or applicants' priority indexes (i.e., scores in the admission exam). If these channels were also activated, they would inevitably alter the feasible choice sets of the applicants, which we keep fixed in the simulations. We test for the presence of these margins of adjustments in response to the treatment and are able to rule them out (see Table A.1 in the Appendix).

Figure 4 plots the average changes in the predicted choice probabilities for the academic attributes of the schools based on the model simulations.[15] We plot these changes by the sign of the actual change in mean beliefs that took place after the delivery of the signal. Panel (a) shows the average change in the probability of choosing a school from the academic track as a result of the provision of feedback. Comparing the "no update" to the "full update" scenario reveals that choice probabilities increase on average by 19 percentage points among students who updated their mean beliefs upwards. This overall effect can be decomposed into a 10 percentage point increase due to positive changes in the mean of the beliefs and a 9 percentage point increase due to the reduction in the dispersion of the belief distribution. Among those students who update their mean beliefs downwards, the overall effect of the intervention is negative but much smaller in magnitude. Even though the average probability of choosing a school from the academic track goes down by 13

[15]See Table A.9 in the Appendix for a similar decomposition on the distribution of treatment effects on choice probabilities (other than the mean).

percentage points due to a reduction in the mean of the beliefs, this drop is partly compensated by a 6 percentage point average increase in choice probabilities triggered by the decrease in the dispersion of beliefs.

A similar pattern is observed in Panel (b) of Figure 4 for the average changes in choice probabilities for schools with relatively more stringent academic requirements. While students who update their mean beliefs upwards experience large and positive average changes in their choice probabilities of alternatives with higher cutoffs, the average effect for those who update downwards is very close to zero. Among the latter group of students, greater precision in performance beliefs induced by feedback entirely dilutes the effect of mean updates.

The different magnitudes of the average treatment effects on choice probabilities across students with opposing updating patterns observed in Figure 4 is explained by the corresponding differential responses to the treatment in terms of beliefs. As discussed in Section 3.2, feedback provision triggered a much larger response in the second moment of the individual belief distribution among students who were originally downwardly biased when compared to students who start off with upward biases. However, the magnitudes of the relative adjustments in means for both groups are similar (see Table 3).

5 Schooling Outcomes

In general, high school completion rates in our setting are modest. Low success rates in high school are a measure of inadequate academic progress through upper secondary due to either dropout or grade retention, which are both strong indicators of low quality matches between schools and students. While most students in the control and placebo groups enroll in the school they were assigned to through the centralized assignment system (82 percent), only 59 percent of the entering cohort graduate from high school on time. There are some differences by track, with timely graduation rates in the academic and non-academic tracks at 66 and 45 percent, respectively. These patterns are not peculiar to our experimental sample and are in fact consistent with the overall trajectories of the entire universe of students from Mexico City who transit through the COMIPEMS assignment system (see Table A.2 in the Appendix).

Section 3 showed that the treatment induced large changes in subjective beliefs about own ability while Section 4 uncovered sizable effects on preferences regarding schools as a consequence of mean and variance updates. Together, the results suggest that the intervention effectively altered assignment outcomes and better aligned students' attributes with schools' characteristics. As such, we expect to observe important effects on subsequent schooling trajectories due to higher-quality matches.

To assess the effects of the intervention on subsequent academic performance, we follow students over time and collect administrative data from individual school of placement records both in terms of enrollment by the end of the first high school year and in terms of graduation by the end of the third and last year of high school.[16] Columns 1 and 2 in Table 6 present the average treatment effects on enrollment and graduation on time, respectively. On average, the treatment did not affect the probability of enrollment. However, conditional on enrollment, the average effect of the intervention on the probability of graduation on time is positive and significant, at 6 percentage points. This effect size corresponds to an 11 percent increase in the probability of timely graduation when compared to the sample average in the control and placebo groups.

Figure 5 displays the heterogeneous effects of the treatment by the admission exam score. The intervention successfully improves high school outcomes along the entire ability distribution, with larger effects among lower-ability students. This last result is suggestive of the potential redistributive impacts of the intervention. Feedback provision tends to affect more the final outcomes of the relatively weaker students who are also the ones who are initially more biased in terms of their beliefs (see panel b of Figure 2).

6 Conclusion

Investments in schooling start early in the life cycle and have persistent consequences on labor market trajectories. 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 decisions. Since the least advantaged households tend to have less access to economic and social resources, biased and/or noisy self-perceptions about individual traits may further exacerbate

[16]Despite all our efforts, we have not yet been able to obtain academic records from one public institution, the IPN, which contributes to the sample with only 182 students. This explains the difference in the number of observations in columns 1 and 2 of Table 6 with respect to the previous tables.

the existing disparities between students from different socio-economic backgrounds.

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 and subsequent academic performance. Our findings show that students face important informational gaps related to their own academic potential and skills. Providing individualized feedback on academic performance substantially corrects these biases. These changes in beliefs have real consequences as they induce differential sorting patterns that seem to better align individual skills and schools' academic attributes. We confirm that claim by looking at the medium-run effects of the intervention: the treatment improves students' outcomes at the end of high school, raising the probability of graduation on time by 6 percentage points. The longer term consequences of misaligned early investments in human capital are outside the scope of this study and hence are left for future research.

Taken together, this evidence 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. While we cannot extrapolate our results to a broader population of students, we have shown that the students in our sample are broadly comparable in terms of characteristics and outcomes to the entire population of students who are assigned through the centralized mechanisms in the Mexico City area. In the particular context we analyze, a cost-effective way to scale up the intervention under study may be to reverse the timing of the application process, allowing applicants to submit their school rankings after taking the admission exam and learning their scores. An alternative policy may be to incentivize middle schools to implement mock tests and deliver score results before students participate in the centralized assignment mechanism. Extrapolating our experimental findings at a larger scale would require simulating individual school preferences under the new information scenario and computing the resulting equilibrium cutoff scores, an exercise well beyond the scope of the paper. Our results still suggest that, in the transition to a system that provides information about own ability before submitting preferences, students with initially downward-biased beliefs are likely to be the main drivers behind changes in equilibrium cutoff scores.

On top of the specific implications for the intervention and the setting under study, two important general lessons can be derived from our results. First, the discrete choice model that we propose and estimate allows us to uncover a novel channel that can explain the observed impacts of the information intervention on school choices: the interplay between the location and the dispersion parameters of the individual distributions of perceived academic ability. In particular, 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. To the extent that most information disclosures or policy interventions aimed at disseminating information are likely to affect both parameters at the same time, this result may inform the design of new effective policies as well as the interpretation of a variety of existing policies with heterogeneous effects.

Second, while a recent and growing empirical strand of literature use subjective expectations data to identify and estimate models of individual choice behaviors (see, e.g., Van der Klaauw [2012] for a recent survey), very few papers have explicitly acknowledged the possibility that those measures of beliefs may be jointly determined with choices and outcomes. Our unique setting in which the model is empirically identified from realized experimental variation in beliefs allows us to document the extent to which neglecting these endogeneity concerns may lead to severely biased inference. This result nicely illustrates the advantages of combining subjective expectations data with external sources of variations in order to credibly identify and estimate empirical choice models. An alternative 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.

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Figures and Tables

Figure 1: Timeline of Events

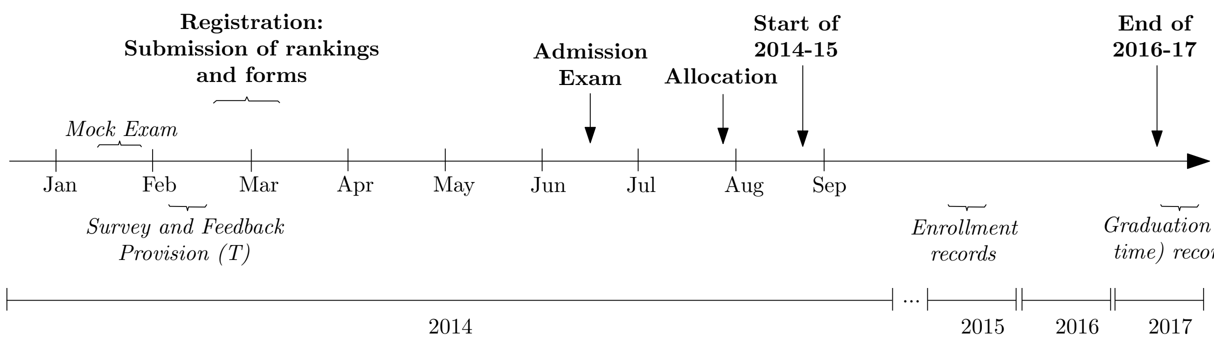
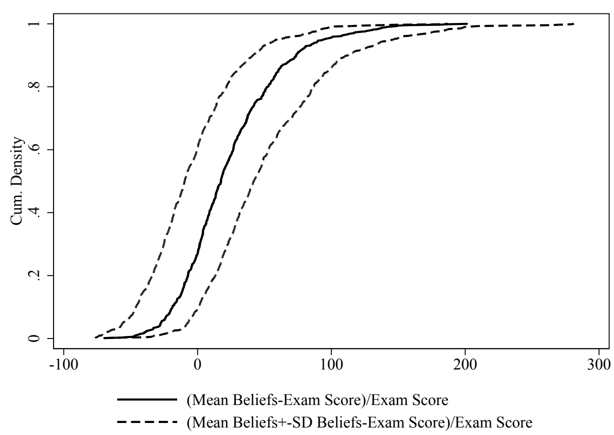
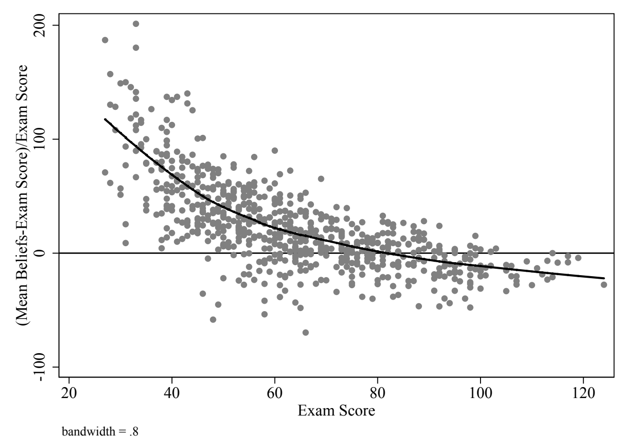


Figure 2: Gap between Expected and Actual Exam Score



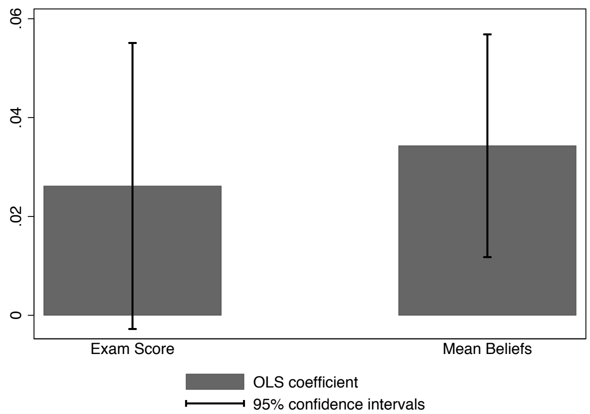
(a)



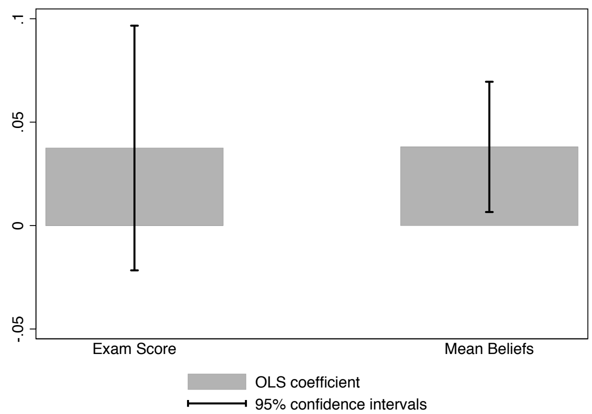
(b)

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 a scatter plot of the relationship between the divergence between beliefs and scores in the admission exam. The thick line comes from a locally weighted regression of the bias on exam scores. Source: survey data (February, 2014) and COMIPEMS administrative records (2014).

Figure 3: Role of Beliefs and Measured Ability on School Rankings and Placement



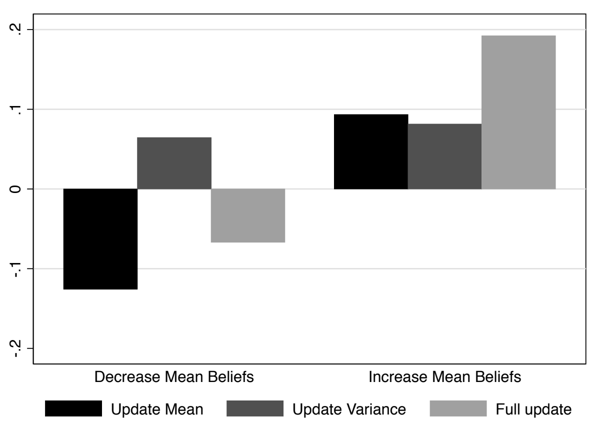
(a) Share of Academic Options in School Rankings



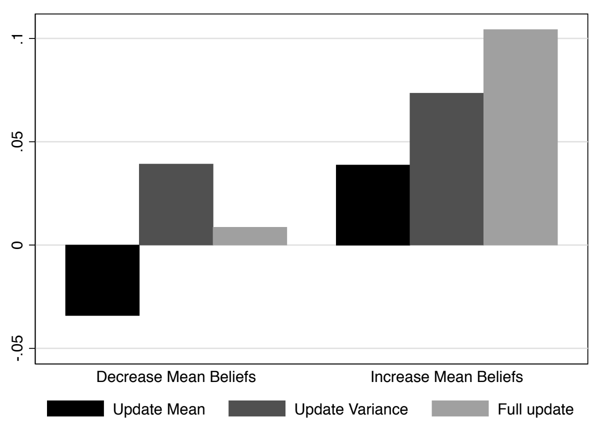
(b) Probability of Assignment in Academic School

NOTE: Bars in each chart represents estimated OLS coefficients of separate regressions using, respectively, the share of academic options in the school rankings (Panel a) and an indicator variable for the assignment in an academic school (Panel b) as dependent variables. All specifications include fixed effects at the middle school level as well as the middle school GPA as a control.

Figure 4: Average Treatment Effects on Choice Probabilities for School Attributes



(a) Academic Track



(b) Above-median Cutoff Score

NOTE: Simulations based on the estimated model (see column 2 of Table 5) using data for the treatment group. 'Update mean' denotes the average difference in the individual choice probabilities between the "pre-treatment" scenario and a counterfactual scenario of beliefs' updating with treatment-induced changes in mean beliefs. 'Sd update' denotes the average difference in the individual choice probabilities between the "pre-treatment" scenario and a counterfactual scenario of beliefs' updating with treatment-induced changes in the dispersion of beliefs. 'Full update' denotes the average difference in the individual choice probabilities between the "pre-treatment" scenario and the "post treatment" scenario.

Figure 5: Probability of Graduation on Time by Exam Score

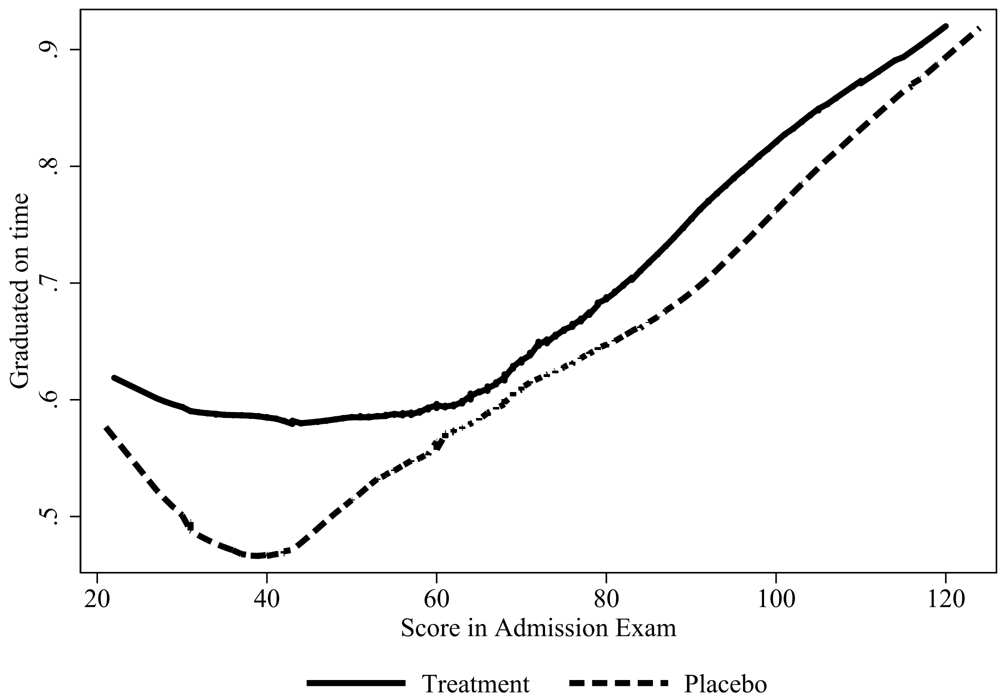


Table 1: Summary Statistics and Randomization Check

	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. Strata dummies included in all specifications, standard errors clustered at the middle school level. Source: COMIPEMS administrative records (2014).

Table 2: Average Treatment Impacts on Beliefs

Dependent Variable	Mean Beliefs	SD Beliefs	Abs(Mean Beliefs-Exam Score)
	(1)	(2)	(3)
Exam Taking	1.244	0.926	0.658
	(1.353)	(0.620)	(0.594)
Score Delivery	-5.655	-1.707	-3.571
	(1.210)	(0.673)	(0.579)
Mean Control	75.72	17.26	17.86
Number of Observations	2825	2825	2825
R-squared	0.098	0.049	0.041
Number of Clusters	118	118	118

NOTE: OLS estimates. Standard errors clustered at the level of the applicants' schools of origin and they 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, shift and type of middle school, pre-determined characteristics (gender, pre-registered, experience with mock exams providing feedback, lives with parents, parental education, and asset index), and an indicator variable for whether one or more of the control variables has missing data.

Table 3: Heterogenous Treatment Impacts on Beliefs

Sample	Upwardly Biased Beliefs		Downwardly Biased Beliefs	
Dependent Variable	Mean Beliefs	SD Beliefs	Mean Beliefs	SD Beliefs
	(1)	(2)	(3)	(4)
Score Delivery	-9.277	-1.614	7.255	-5.788
	(0.974)	(0.516)	(1.712)	(0.705)
Mean Gap in Placebo	20.92	20.92	-9.90	-9.90
Mean Dep. Var. in Placebo	78.55	16.66	64.36	19.67
Number of Observations	2303	2303	522	522
R-squared	0.128	0.036	0.221	0.202
Number of Clusters (Schools)	118	118	84	84

NOTE: OLS estimates. Standard errors clustered at the level of the applicants' schools of origin and they are reported in parenthesis. Sample of ninth graders in schools from the treatment group and the placebo group. All specifications include a set of dummy variables which corresponds to the randomization strata, shift and type of middle school, pre-determined characteristics (gender, pre-registered, experience with mock exams providing feedback, lives with parents, parental education, and asset index), and an indicator variable for whether one or more of the control variables has missing data.

Table 4: Estimates of the First Stage

	(1) Mean Beliefs	(2) SD Beliefs
Treatment (=1 Score Delivery)	-5.450	-2.384
	(1.082)	(0.481)
Live with Both Parents (=1 yes)	1.465	-0.461
	(0.715)	(0.357)
Parent with College (=1 yes)	5.606	-0.583
	(1.187)	(0.509)
Above Median SE index	2.809	-0.799
	(0.656)	(0.329)
Mean Placebo/Control	76.34	16.55
Number of Observations	1329441	1329441
R-squared	0.055	0.025
Number of Clusters	118	118

NOTE: OLS estimates. Standard errors clustered at the school level are reported in parenthesis. Sample of ninth graders in schools from all treatment arms. All regressions include a dummy indicating one or more of the control variables has missing data.

Table 5: Parameter Estimates of Mixed Logit Models (Selected Coefficients)

	(1) Without Control Function	(2) With Control Function
Coefficients based on Perceived Ability:		
$\mu\times$ Academic Track	0.0118	0.0615
	(0.0046)	(0.0231)
$\sigma\times$ Academic Track	0.0003	-0.1476
	(0.0080)	(0.0604)
$\mu\times$ Above-median Cutoff	0.0027	0.0211
	(0.0017)	(0.0099)
$\sigma\times$ Above-median Cutoff	-0.0124	-0.0998
	(0.0058)	(0.0463)
Random Coefficients:		
Academic Track (Mean)	-0.3300	-1.6896
	(0.3768)	(1.4487)
Academic Track (SD)	1.8694	1.9172
	(0.2291)	(0.9737)
Cutoff Score (Mean)	0.0505	0.0518
	(0.0029)	(0.0029)
Cutoff Score (SD)	0.0237	0.0254*
	(0.0051)	(0.0136)
Other Coefficients:		
Distance (Km)	-0.2707	-0.2707
	(0.0094)	(0.0100)
Above-median SE Index$\times$Distance	0.0317	0.0313
	(0.0078)	(0.0097)
Number of Observations	1329441	1329441
Log Likelihood at Convergence	-10753	-10741
F-Test of Control Function Terms (p-value)		0.00049

NOTE: 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 cutoff score. For a full list of the estimated coefficients in both models, see Table A.6 in the Appendix.

Table 6: Treatment Effects on High School Outcomes

Dependent Variable	Enrollment	Graduation on Time
	(1)	(2)
Score Delivery	0.001	0.062
	(0.015)	(0.022)
Mean Placebo & Control	0.82	0.59
Number of Observations	2824	2178
R-squared	0.031	0.087
Number of Clusters	461	393

NOTE: OLS estimates. Sample of ninth graders in schools from all treatment arms. All specifications include a set of dummy variables that corresponds to the public institution sponsoring the high schools participating in the centralized system, and the following set of pre-determined characteristics (see Table 1 for details): both parents in the household, parents with higher education, and SES index (above median). Standard errors clustered at the high school level are reported in parenthesis.

Perceived Ability and School Choices

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October 2018

Appendix - Not For Publication

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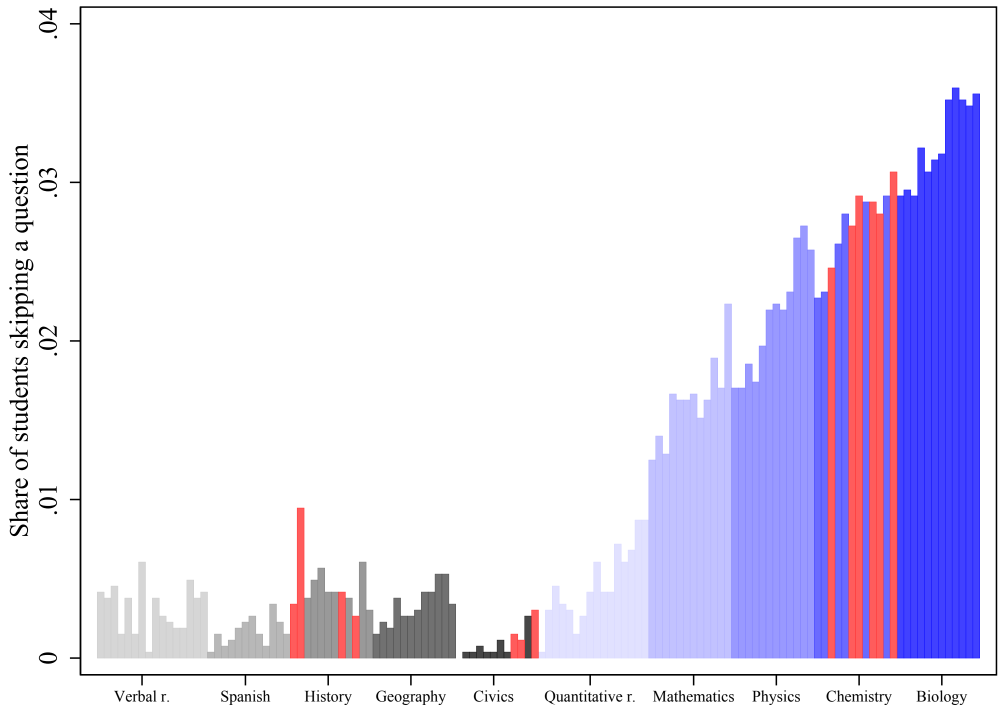
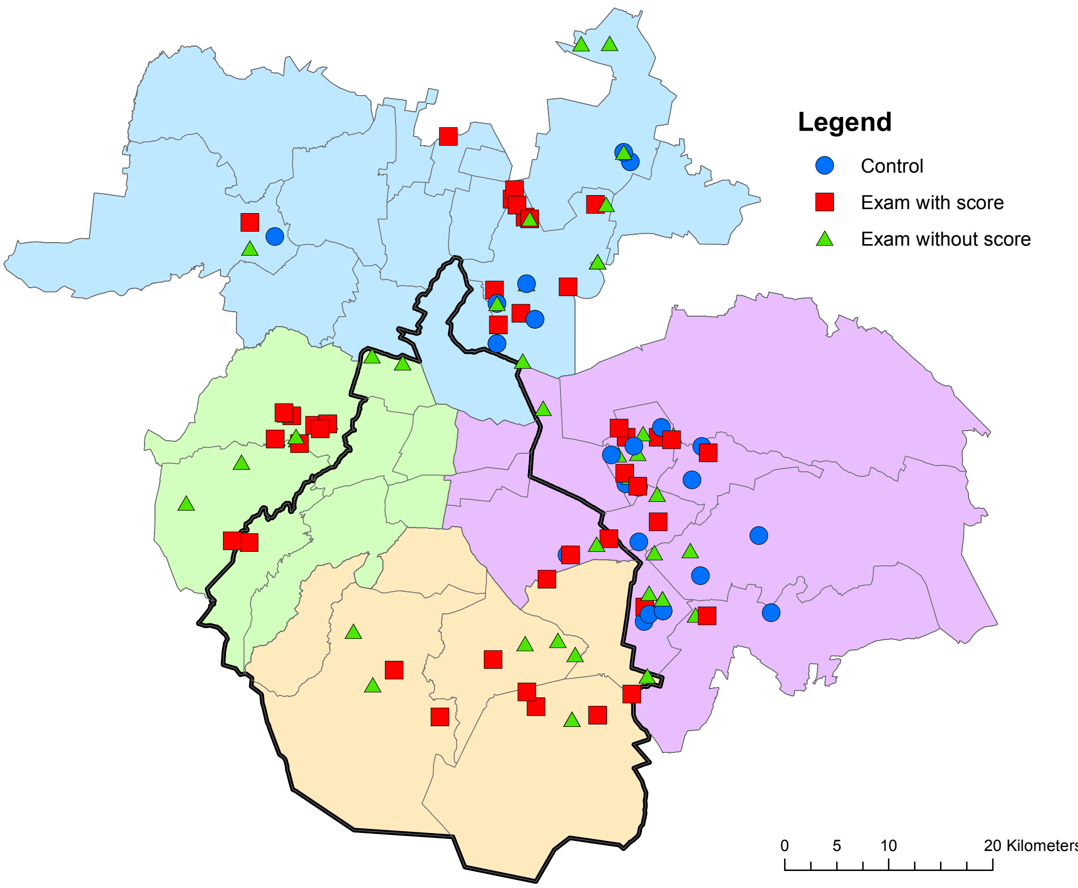
A Additional Figures and Tables

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



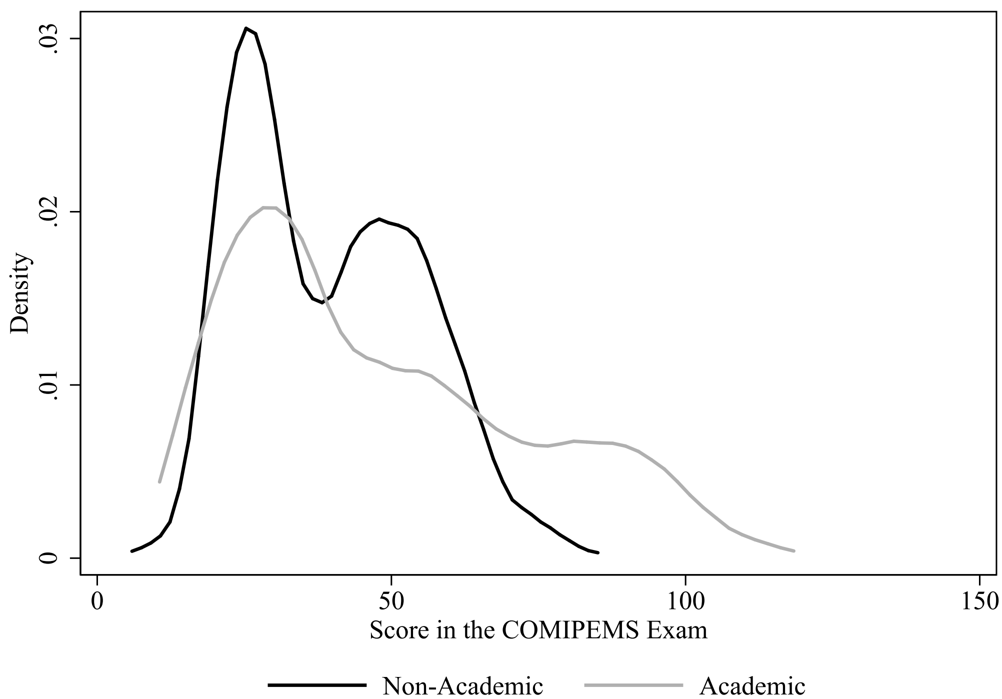
Note: The x-axis orders the 128 questions in the exam in order of appearance. Different colors are used to group together questions from the same section in the exam. Questions in red are the ones excluded from grading since the school curriculum did not cover those subjects by the time of the application 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, combined with discrete intervals of school-average achievement scores, form the basis of the twelve strata underlying the stratification procedure described in Section 2.3 are shaded in different colors.

Figure A.3: Distribution of Cut-off Scores



Note: Cutoff scores for each high school program refer to the matching process of the year 2014. Academic schools are defined as those in the general track and those supplied by the IPN. Source: COMIPEMS administrative data, 2014.

Table A.1: Average Treatment Impacts 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	0.010	0.004	0.016	0.108	0.288
	(0.010)	(0.022)	(0.022)	(0.323)	(1.406)
Score Delivery	0.009	0.002	0.010	0.128	0.318
	(0.010)	(0.022)	(0.023)	(0.328)	(1.358)
Mean Placebo & Control	0.89	0.87	0.89	9.56	65.34
Number of Observations	3644	3251	3251	2825	2825
R-squared	0.374	0.031	0.038	0.038	0.125
Number of Clusters	118	118	118	118	118

NOTE: OLS estimates. Standard errors clustered at the 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, shift and type of middle school, and the following set of pre-determined characteristics (see Table 1 for details): both parents in the household, parents with higher education, SES index (above median), gender, pre-registered for the assignment process, and previous experience with mocks that provide feedback. All regressions include a dummy indicating one or more of the control variables has missing data.

Table A.2: Comparing Population and Sample

	All Students		Experiment	
	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
Cutoff 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 students' sample consists of all COMIPEMS 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 cohorts of COMIPEMS in applicants in the year 2006 for which comparable high school trajectories were constructed (N=184,816). The 'Experiment' sample consists of the sample students that we use throughout the empirical analysis (N=2,825).

Table A.3: Correlates of Individual Beliefs

	Mean Beliefs	SD Beliefs
Male	4.339	-1.651
	(1.088)	(0.612)
Indigenous student	-0.064	-0.494
	(1.635)	(1.480)
GPA (middle school)	6.114	-0.763
	(0.818)	(0.349)
Lives with both parents	-1.158	0.427
	(1.400)	(0.893)
Mother with college degree	2.277	1.011
	(3.451)	(2.295)
Above Median SE index	1.007	-0.027
	(1.487)	(0.661)
Took prep courses	0.575	0.881
	(1.510)	(0.694)
Works	0.890	-0.911
	(1.021)	(0.483)
Plans to go to college	1.323	0.430
	(1.483)	(0.794)
Previous mock exam with feedback	2.806	-0.335
	(1.582)	(0.829)
Does not give up	-0.235	0.045
	(1.277)	(1.064)
Tries his best	3.026	-0.684
	(1.339)	(0.932)
Finishes what he starts	1.560	-1.415
	(2.013)	(0.721)
Works hard	0.073	0.103
	(1.828)	(1.179)
Constant	17.987	23.650
	(6.191)	(2.651)
Number of Observations	710	710
R-squared	0.252	0.142
Number of Clusters	28	28

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 that belong to the control group. Both regressions include middle school fixed effects and a dummy indicating one or more of the explanatory variables has missing data.

Table A.4: Testing Truth-Telling vs. Stability

	(1) Conditional Logit	(2) Ranked-Order Logit
Academic Track	-0.3094	-0.0619
	(0.0738)	(0.2420)
$\mu\times$ Academic Track	0.0144	0.0086
	(0.0009)	(0.0029)
$\sigma\times$ Academic Track	-0.0021	0.0001
	(0.0016)	(0.0053)
Cutoff Score Above Median	-0.3833	-0.2198
	(0.0846)	(0.2855)
$\mu\times$ Above Median Cutoff	0.0175	0.0182
	(0.0010)	(0.0035)
$\sigma\times$ Above Median Cutoff	-0.0038**	-0.0008
	(0.0019)	(0.0061)
Distance (km)	-0.2179	-0.2593***
	(0.0024)	(0.0088)
Both Parents$\times$Distance	-0.0051	-0.0036
	(0.0023)	(0.0083)
Parent with College$\times$Distance	0.0179	0.0172
	(0.0027)	(0.0103)
Above Median SE Index $\times$Distance	0.0265	0.0297
	(0.0020)	(0.0074)
Missing value $\times$Distance	0.0027	0.0013
	(0.0029)	(0.0108)
Institution 1	-1.0699	-1.7821
	(0.0271)	(0.0797)
Institution 2	0.3261	-0.2124
	(0.0339)	(0.0963)
Institution 3	0.5340	0.4123
	(0.1037)	(0.2098)
Institution 4	-1.0951	-0.2655
	(0.1493)	(0.3867)
Institution 5	0.7993	2.8804
	(0.0282)	(0.1366)
Institution 6	1.5864	4.1277
	(0.0269)	(0.1462)
Institution 7	-1.0694	-1.8414
	(0.0425)	(0.1197)
Institution 8	-0.3098	0.2546
	(0.1481)	(1.0144)
Number of Observations	1663925	1329441
Log Likelihood at Convergence	-124501	-10926.27
Ho: Students are (weakly) truth telling		
χ^2 Statistic	745.446	
P-value	0.00000	

NOTE: . Estimates in column 1 are consistent under Ho and Ha. Estimates in 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. Standard errors reported in parenthesis.

Table A.5: First Stage – Alternative Measures for Mean and SD of Beliefs

	(1)	(2)	(3)	(4)	(5)	(6)
	Mean Beliefs			SD Beliefs		
	Lower Bound	Upper Bound	Median	Lower Bound	Upper Bound	Interquantile Range
Treatment (=1 Score Delivery)	-5.025	-5.875	-7.064	-2.317	-2.507	-2.540
	(1.094)	(1.075)	(1.123)	(0.545)	(0.456)	(0.696)
Live with Both Parents (=1 yes)	1.573	1.356	1.758	-0.644	-0.292	-0.942
	(0.725)	(0.713)	(0.756)	(0.405)	(0.339)	(0.658)
Parent with College (=1 yes)	5.344	5.867	4.629	-1.194	0.004	-1.176
	(1.164)	(1.222)	(1.298)	(0.562)	(0.500)	(0.782)
Above Median SE index	3.042	2.576	2.842	-1.285	-0.333	-1.081
	(0.669)	(0.651)	(0.705)	(0.382)	(0.302)	(0.538)
Mean Placebo/Control	68.25	85.43	79.52	16.93	16.41	22.97
Number of Observations	1329441	1329441	1329441	1329441	1329441	1329441
R-squared	0.049	0.060	0.059	0.024	0.027	0.013
Number of Clusters	118	118	118	118	118	118

NOTE: OLS estimates. Standard errors clustered at the school level are reported in parenthesis. The specifications in columns 1 and 4 consider the minimum points of each discrete interval (instead of the mid-point) in order to compute the mean and the standard deviation of beliefs. The specifications in columns 2 and 5 consider the maximum points of each discrete interval (instead of the mid-point) in order to compute the mean and the standard deviation of beliefs. The specifications in columns 3 and 6 consider the median and the interquartile range (p75-p25) as alternative measures of the location and scale parameter of the belief distributions. Sample of ninth graders in schools from all treatment arms. All regressions include a dummy indicating one or more of the control variables has missing data.

Table A.6: Parameter Estimates of Mixed Logit Model – Full Specification

	(1) Without Control Function	(2) With Control Function
$\mu\times$ Academic Track	0.0118	0.0615
	(0.0046)	(0.0231)
$\sigma\times$ Academic Track	0.0003	-0.1476**
	(0.0080)	(0.0604)
$\zeta\times$ Academic Track		-0.0497
		(0.0227)
$\xi\times$ Academic Track		0.1503
		(0.0609)
$\mu\times$ Above-median Cutoff	0.0027	0.0211
	(0.0017)	(0.0099)
$\sigma\times$ Above-median Cutoff	-0.0124	-0.0998
	(0.0058)	(0.0463)
$\zeta\times$ Above-median Cutoff		-0.0053
		(0.0100)
$\xi\times$ Above-median Cutoff		0.1008
		(0.0470)
Academic Track (Mean)	-0.3300	-1.6896
	(0.3768)	(1.4487)
Academic Track (SD)	1.8694	1.9172
	(0.2291)	(0.9737)
Cutoff Score (Mean)	0.0505	0.0518
	(0.0029)	(0.0029)
Cutoff Score (SD)	0.0237	0.0254
	(0.0051)	(0.0136)
Distance - Km	-0.2707	-0.2707
	(0.0094)	(0.0100)
Both Parents$\times$Distance	-0.0028	-0.0029
	(0.0088)	(0.0107)
Parent with College$\times$Distance	0.0173	0.0171
	(0.0110)	(0.0149)
Above Median SE Index$\times$Distance	0.0317	0.0313
	(0.0078)	(0.0097)
Missing value$\times$Distance	-0.0004	-0.0011
	(0.0115)	(0.0122)
Institution 1	-1.6847	-1.6791
	(0.0862)	(0.0889)
Institution 2	-0.0695	-0.0551
	(0.1046)	(0.0932)
Institution 3	0.8539	0.8476
	(0.2188)	(0.2573)
Institution 4	-0.7382	-0.7566
	(0.3904)	(0.4468)
Institution 5	2.0061	1.9669
	(0.1795)	(0.2038)
Institution 6	3.0053	2.9562
	(0.1945)	(0.2315)
Institution 7	-1.5918	-1.5790
	(0.1279)	(0.1193)
Institution 8	-0.6512	-0.6845
	(1.0303)	(15.8223)
Number of Observations.	1329441	1329441
Log Likelihood at Convergence	-10753	-10741
F-Test of Control Function Terms (p-value)		0.00049

NOTE: Standard errors calculated with 50 bootstrap replications are reported in parenthesis. Sample of student-school observations with feasible choice sets.

Table A.7: Parameter Estimates of Mixed Logit Models – Alternative Measures for Mean and SD of Beliefs

	(1) Lower Bound	(2) Upper Bound	(3) Median and Interquantile Range
Coefficients based on Perceived Ability:			
$\mu\times$ Academic Track	0.0501	0.0816	0.0391**
	(0.0219)	(0.0308)	(0.0197)
$\sigma\times$ Academic Track	-0.1140	-0.1998	-0.1040
	(0.0506)	(0.0914)	(0.0476)
$\mu\times$ Above-median Cutoff	0.0204	0.0173	0.0276
	(0.0079)	(0.0148)	(0.0121)
$\sigma\times$ Above-median Cutoff	-0.0841	-0.0928	-0.0956
	(0.0324)	(0.0789)	(0.0427)
Random Coefficients:			
Academic Track (Mean)	-0.9284	-3.1215*	-0.1519
	(1.4083)	(1.6851)	(1.3074)
Academic Track (SD)	1.9203	-1.9301	-1.9060
	(1.3778)	(1.2409)	(1.3863)
Cutoff Score (Mean)	0.0519	0.0518	0.0518
	(0.0030)	(0.0029)	(0.0029)
Cutoff Score (SD)	0.0249	0.0250	0.0262
	(0.0149)	(0.0130)	(0.0122)
Other Coefficients:			
Distance - Km	-0.2706	-0.2709	-0.2707
	(0.0101)	(0.0119)	(0.0101)
Above Median SE Index$\times$Distance	0.0312	0.0315	0.0312
	(0.0098)	(0.0086)	(0.0098)
N. of Obs.	1329441	1329441	1329441
F-Test Ctrl Funct. (pval)	0.00047	0.00000	0.00138

NOTE: Estimates obtained by simulated maximum likelihood. Standard errors calculated with 30 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 cutoff score (not reported). The specification in column 1 considers the minimum points of each discrete interval (instead of the mid-point) in order to compute the mean and the standard deviation of beliefs. The specification in column 2 considers the maximum points of each discrete interval (instead of the mid-point) in order to compute the mean and the standard deviation of beliefs. The specification in column 2 considers the median and the interquartile range (p75-p25) as alternative measures of the location and scale parameter of the belief distributions.

Table A.8: Parameter Estimates of Mixed Logit Models – Alternative Specifications

	(1) Correlated Random Coeffs.	(2) Polynomial in CF terms	(3) Ln(Normal) Random Coeffs.
Coefficients based on Perceived Ability:			
$\mu\times$ Academic Track	0.0602	0.0630	0.0622
	(0.0233)	(0.0240)	(0.0240)
$\sigma\times$ Academic Track	-0.1432	-0.1559	-0.1475
	(0.0610)	(0.0640)	(0.0625)
$\mu\times$ Above-median Cutoff	0.0214	0.0216	0.0197
	(0.0084)	(0.0086)	(0.0082)
$\sigma\times$ Above-median Cutoff	-0.1007	-0.1012	-0.0943
	(0.0394)	(0.0401)	(0.0387)
Random Coefficients:			
Academic Track (Mean)	-1.6318	-1.7347	-1.7386
	(1.4436)	(1.5160)	(1.4951)
Academic Track (SD)	-1.791	-1.9632	1.9320
	(1.5102)	(1.4437)	(0.2612)
Cutoff Score (Mean)	0.0519	0.0517	-3.0489
	(0.0029)	(0.0030)	(0.0740)
Cutoff Score (SD)	0.0230	0.0255	0.4311***
	(0.0218)	(0.0165)	(0.1029)
Cov(Academic,Cutoff)	-0.0083		
	(0.0076)		
Other Coefficients:			
Distance - Km	-0.2709	-0.2709	-0.2703
	(0.0102)	(0.0101)	(0.0101)
Above Median SE Index$\times$Distance	0.0314	0.0314	0.0315
	(0.0099)	(0.0098)	(0.0098)
Number of Observations	1329441	1329441	1329441
F-Test Ctrl Function Terms (p-value)	0.00011	0.00026	0.00015

NOTE: Estimates obtained by simulated maximum likelihood. Standard errors calculated with 30 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 cutoff score (not reported). The specification in column 1 also includes the Var-Cov element of the random coefficients. 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 cutoff score (not reported). The specification in column 3 considers log-normally distributed random coefficient for cutoff score (instead of normal).

Table A.9: Distribution of Treatment Effects

	(1)	(2)	(3)	(4)	(5)
	Moments of changes in choice probabilities				
	min	p25	p50	p75	max
A. Decrease in mean beliefs					
Academic Track					
Mean Update	-0.603	-0.189	-0.087	-0.036	0.000
SD Update	-0.342	-0.013	0.025	0.119	0.858
Full Update	-0.760	-0.177	-0.043	0.023	0.740
Above-median Cutoff Score					
Mean Update	-0.300	-0.051	-0.018	-0.003	0.135
SD Update	-0.345	-0.003	0.007	0.055	0.728
Full Update	-0.459	-0.034	0.000	0.032	0.680
B. Increase in mean beliefs					
Academic Track					
Mean Update	0.001	0.034	0.067	0.135	0.423
SD Update	-0.593	0.001	0.067	0.160	0.721
Full Update	-0.402	0.054	0.175	0.311	0.835
Above-median Cutoff Score					
Mean Update	-0.095	0.007	0.021	0.057	0.325
SD Update	-0.289	0.001	0.041	0.123	0.591
Full Update	-0.274	0.011	0.065	0.163	0.639

NOTE: Simulations based on the estimated model (see column 2 of Table 5) using data for the treatment group. 'Mean update' denotes the distribution of the difference in the individual choice probabilities between the "pre-treatment" scenario and a counterfactual scenario of beliefs' updating with treatment-induced changes in mean beliefs. 'Sd update' denotes the distribution of the difference in the individual choice probabilities between the "pre-treatment" scenario and a counterfactual scenario of beliefs' updating with treatment-induced changes in the dispersion of beliefs. 'Full update' denotes the distribution of the difference in the individual choice probabilities between the "pre-treatment" scenario and the "post treatment" scenario.