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

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Abstract

Students' inaccurate perceptions about their own academic potential may alter schooling decisions by distorting the (perceived) returns to different educational tracks. In the context of the centralized assignment mechanism currently in place at the high-school level in the metropolitan area of Mexico City, we administer a mock version of the admission achievement test in a sample of ninth graders and communicate score results to a randomly chosen subset of them. The treatment is found to better align students' subjective expectations and school-track choices with their realized performance. As predicted by a simple bayesian learning model with self-image concerns, individual school choice responses are asymmetric depending on the process of beliefs updating induced by the intervention.

Keywords: school choice, incomplete information, unknown ability, ego utility, bayesian updating, over-confidence.

JEL Codes: D83; I21; I24; J24.

1 Introduction

Evidence from a variety of settings suggests that school choices are correlated with socioeconomic background, even after controlling for pre-determined measures of ability (e.g. test scores).[1] Several studies have been devoted towards the understanding of whether schooling decisions reflect idiosyncratic preferences of different groups or they are instead driven by background-related constraints. On the one hand, credit constraints are usually claimed to explain a large fraction of the persistent strong positive correlation between family income and schooling observed throughout a variety of settings (see, e.g., Lochner and Monge-Naranjo [2012] for a recent review). On the other hand, a recent body of literature has shown that poor information about population-average characteristics such as the returns to education [Jensen, 2010, 2012], school quality [Hastings and Weinstein, 2008], and financial aid opportunities [Dinkelman and Martinez, 2013] can negatively affect educational outcomes.

This paper tries to look at the role of a different informational barrier. Here, we explore whether inaccurate perceptions about ability generate misallocations through inadequate school choices. To the extent that students' preferences over schools partly depend on expected academic achievement, biased perceptions may distort expected individual-specific returns. Hence, biased perceptions may lead to sub-optimal school choices which can eventually have detrimental longer-term consequences both on educational and labor market trajectories. The existing literature has paid little attention to the role played by perceived ability in explaining human capital investments, partly because students' beliefs about their own academic skills are not observed in most datasets and partly because the empirical relationship between beliefs and school choices is fundamentally plagued by omitted variable bias and/or reverse causality (e.g. those holding higher perceptions about their own ability are also likely to prefer more academic-oriented schools).

Our study aims to overcome both empirical challenges in the relationship between perceived individual ability and school choices. In the context of a centralized and merit-based high-school admission system, we design an experimental intervention that provides students with a mock version of the admission exam used to rank and place students. We elicit subjective expectations about test performance as a proxy for perceived ability both before and after the exam and communicate individual scores to a randomly chosen group of students. This framework allows us to assess whether providing students with a salient (and unsolicited) signal enables them to update their beliefs about their own academic achievement

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[1]See for instance Ajayi [2011] for evidence on Ghana and Avery and Hoxby [2012] for the US case.

and, if so, how this change affects their revealed preferences over the schooling alternatives available within the admission system.

More specifically, we exploit the school assignment mechanism currently in place in the metropolitan area of Mexico City to allocate students to public high schools. Due to some of its particular features, this system offers an ideal setting for our purposes. First, applicants are required to submit their final preference sheets early during the application process - not only before the admission test but even before the majority of applicants start preparing for the exam. Second, applicants are allowed to submit a ranked list of up to twenty schooling options, which gives us the unique opportunity to observe the complete rankings of revealed preferences over a wide range of available high-school alternatives for each student. Third, placement in the system solely depends on those preferences and individual performance in the admission exam. Since we can measure any observed change in those two outcomes induced by the intervention, we are also able to track its effects on high-school assignment.

We document large *ex-ante* discrepancies between students' priors about their performance and their realized scores in the mock and the placement exams. Relative to the mock exam score, students' mean priors are on average off by almost 20 points based on a 128 points scale. Consistent with Bayesian updating, providing feedback about students' performance in the mock exam greatly reduces the gap between priors and performance, especially among students with more negative initial gaps and less noisy priors.

In Mexico, as well as in several other countries, track choices are crucial in determining future access to college and, consequently, labor market earnings. We first show that, on average, the belief updating patterns induced by our intervention are consistent with subsequent changes in school-track choices. On average, preferences for the academic-oriented track become (more) responsive to students' individual achievement. We also find that these changes in students' revealed preferences over school tracks pass through into final assignment outcomes, thereby suggesting the potential scope for longer-term impacts of the intervention on subsequent academic trajectories.

The information treatment varies across individuals depending on the idiosyncratic realizations of performance, hence the school choice responses to the intervention are likely to vary depending on the associated beliefs updating patterns. We study those effects through the lens of a bayesian learning framework with self-image concerns nested into our school choice setting. In the model, students gradually and rationally discover their academic ability by drawing an indefinite sequence of random ability signals. In turn, students? realized beliefs about their ability determine their school-track choices through a standard utilitar-

ian motive and an additional "ego utility" component. This second element generates an asymmetry in the responses of individuals holding different beliefs to the additional (and unsolicited) signal about their ability which is embedded in our intervention. Accordingly, students receiving a signal which is below their average prior beliefs are more likely to keep searching for more signals and hence they are less likely to align their choices in response to the treatment. On the other hand, students who receive a signal which is above their mean prior beliefs are likely to stop learning about their ability and are thus more prone to choose schooling options that are better aligned with the resulting more positive views about themselves.

We next explore empirically how school-track decisions respond to the process of beliefs updating spurred by the treatment. In spite of substantial decrease in beliefs observed immediately after the delivery of the ability signal, we find that, on average, students who receive a mock exam score which is below their average priors do not seem to alter their track-choice decisions vis-a-vis those who did not receive signal. On the contrary, students who receive a mock exam score which is above their average priors seem to translate the observed increase in beliefs into their track-choices. For those students, the treatment is associated with an average 8.3 percent increase in the share of academic-oriented options requested - which corresponds to almost 20 percent of the corresponding average share in the control group.

This paper speaks and contributes to different strands of literature. First, most studies that explore the determinants of school-choice decisions focus on either the role of tastes versus beliefs about future earnings [Arcidiacono, 2004, 2005; Attanasio and Kaufmann, 2009; Wiswall and Zafar, 2011] or the role of incomplete information about the educational system [Ajayi, 2011; Lai et al., 2009] so that the existing evidence on the role played by misperceptions about individual ability is still very scarce. Two exceptions are Stinebrickner and Stinebrickner [2013] who analyze the role of beliefs about future performance on college drop-out decisions, and Dizon-Ross [2014] who conducts a field experiment in Malawi to inform parents about their children's academic abilities and measures the effects of such intervention on remedial investments among primary-school students. Second, in spite of a series of theoretical papers on the interactions between preferences and beliefs (e.g. Bénabou and Tirole [2002], Compte and Postlewaite [2004], Köszegi [2006]) and a bulging experimental literature on the widespread presence of over-confidence in beliefs (see Moore and Healy [2008] for a review), there is very little evidence to date on how over and under-confidence in beliefs affect real-world decisions. Third, our paper is also related to a series of recent

studies which attempt to elicit and use probabilistic subjective expectations in developing countries (see, e.g.,Delavande et al. [2011b] for a review). To our knowledge, this is the first paper which collects individual data on subjective distributions about test performance in order to measure students' perceptions about their own academic ability.

The remainder of this paper proceeds as follows: Section 2 discusses the COMIPEMS school assignment system, the experimental design, the different data sources that we put together and briefly describes the resulting sample we use for the empirical analysis. Section 3 presents the results of the intervention, both in terms of the accuracy of student perceptions about their own ability as well as school-track choices and placement in the system. Section 4 explores, both theoretically and empirically, how individual school-track decisions respond to the process of beliefs updating generated by the intervention, and Section 5 concludes.

2   Context, Design and Data

2.1   The COMIPEMS Mechanism

Public high-school admission in the urban area of Mexico City is centralized and administered by the Metropolitan Commission of Higher Secondary Public Education Institutions (COMIPEMS, by its Spanish acronym) since 1996. The commission brings together nine public educational institutions who have all agreed to admit candidates through a standardized achievement exam, which is prepared by an independent public institution in charge of several evaluations in the country (CENEVAL).[2] In 2014, these institutions offered over 238,000 seats in about 650 public high-schools located in the Federal District as well as 22 neighboring municipalities.

The Mexican system offers three educational tracks at the higher secondary level: General, Technical, and Vocational Education (*Bachillerato General*, *Bachillerato Tecnológico*, and *Educación Profesional Técnica*, respectively). The second track partly covers the curriculum of general education programs but it also features additional courses allowing stu-

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[2]The nine institutions who offer schools through the COMIPEMS are: Universidad Nacional Autónoma de México (UNAM), Instituto Politécnico Nacional (IPN), Universidad Autónoma del Estado de México (UAEM), Colegio Nacional de Educación Profesional Técnica (CONALEP), Colegio de Bachilleres (COLBACH), Dirección General de Educación Tecnológica Industrial (DGETI), Dirección General de Educación Tecnológica Agropecuaria (DGETA), Secretaría de Educación del Gobierno del Estado de México (SE), and Dirección General del Bachillerato (DGB). Eight out of the nine institutions evaluate candidates based on the exact same test. Only UNAM prepares its own admission exam but it is equivalent to the exam used by the rest of the system both in terms of difficulty and content. Programs offered by UNAM additionaly require a 7.0 cumulative grade point average (GPA) in junior high school.

dents to become technicians upon completion of secondary schooling. In turn, a student opting for the vocational track is exclusively trained to become a professional technician. Only those who conclude programs in general or technical education are automatically eligible to pursue a university degree. Roughly 25% of the COMIPEMS' schools belong to general education track while 45% and 30% of them are technical and vocational programs, respectively. Each school offers a unique specific track but within both technical and vocational track schools, students also choose a specialization such as accounting or informatics.

The assignment process proceeds as follows. In late January, students in ninth grade —the final year of middle school— receive informational materials about the admission process. These materials include a list of the school (and specialties, if applicable) available in the admission process. Students can pre-register to the COMIPEMS system in person or online until mid February and they are then required to officially register in person sometime between late February and early March, depending on the students' family name. To apply, students fill out a registration form, a socio-demographic survey, and a list of up to 20 educational options, ranked in order of individual preference. In mid-June of that year, students take an standardized achievement admission exam. Assignment takes place at the end of July in the following fashion. First, each institution reports the maximum number of seats available for incoming students. Second, all applicants are ranked by their exam score, from highest to lowest. Third, a computer program scrolls down the student ranking, assigning each applicant to his highest-ranked option with available seats. The process continues until all students are assigned, with the exception of those whose score was too low to guarantee a seat in one of his preferred options.[3] Unassigned students can independently apply to COMIPEMS's schools with available seats at the end of the centralized assignment process or to schools with open admissions outside the system.

The COMIPEMS assignment mechanism make students' rankings quite informative about their underlying preferences over the different schooling alternatives made available through the centralized process. First, the matching mechanism used is equivalent to the deferred-acceptance algorithm originally proposed by Gale and Shapley [1962], which is strategy-proof. Under such mechanism, there is no penalty for ranking schools in true preference order within the set of listed choices.[4] Second, even though students are allowed to rank up to twenty

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[3]Whenever ties occur, the participating institutions decide between admitting all tied students or none of them.

[4]This contrasts with the Boston mechanism (formerly used by Boston public schools and several other school districts in the United States) which assigns students based on their first choices in the same way but then keeps these initial assignments for all subsequent rounds and does not allow higher priority students to displace students already assigned to a school in a preceding round. There are clear incentives for making

options only a minor percentage of them (less than 2% in our sample) fill up the entire preference sheet. This implies that students do not face a binding constraint in terms of the total number of options they are allowed to include in their ranking. Consequently, there is no strategic disadvantage in choosing a school for which the student has a low ex-ante probability of admission.

2.2   The Intervention

Our intervention seeks to provide students with relevant information about their own ability in order to observe its effects on beliefs and, ultimately, on choices. The main challenge we face is identifying a measure that is highly correlated with ability and that is expressed in a scale that is familiar to ninth graders. We thus rely on the COMIPEMS process and use the score in the admission exam as a proxy for ability. At the heart of our intervention, lies the notion that ninth graders who have gone through the pre-registration process are sufficiently familiar with the structure and scale of the COMIPEMS exam, providing us with a suitable environment to elicit subjective expectations about ability and to interpret the signal received by the treatment group.

We provide students with a mock version of the admission exam used by the COMIPEMS and assign a random portion of them to receive feedback on their performance prior to the submission of school preference rankings. Since taking the mock exam may, in itself, alter behaviors and school choices, we also collect information on a group of students who did not take the mock exam.

The mock exam was designed by CENEVAL in order to mirror the actual test in terms of structure, content, level of difficulty and duration (3 hours). The exam has 128 multiple-choice questions worth one point each, covering a wide range of subject matter corresponding to the public school curriculum (Spanish, mathematics, and social and natural sciences) as well as mathematical and verbal aptitude sections that do not correspond directly to curriculum. Thirteen questions related to the school curriculum covered between March and June of the third grade of middle school were excluded from the evaluation.[5]

We adjust the raw scores obtained in the 115 valid questions of the mock exam in order to use a 128-point scale to deliver the scores to the treatment group. The delivery of individual scores takes place at the beginning of the follow up survey. Surveyors show the student a personalized graph with three bars: the first one reflects his individual score while the

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a strategic first choice under the Boston mechanism which do not apply under the deferred-acceptance algorithm.

[5]Those questions refer to the History (4/8), Ethics (3/8), and Chemistry (6/8) portions of the exam.

other two bars show the average mock exam score of his classmates and the average score in the actual exam among applicants from his school during the 2013 round, respectively. We do not attempt to back-out the role of each of these information nudges on students' behavior. Instead, we interpret the treatment as the joint effect of these three distinct pieces of information.[6]

In order to select the experimental sample from the universe of potential COMIPEMS applicants, we apply the following protocol. First, we only keep schools in urban blocks (AGEB by its spanish acronym) with high or very high levels of marginalization. We believe that these students would be most likely to be affected by the intervention. Students in worse off neighborhoods have limited access to tutoring or training courses and may thus be less exposed to previous signals on their ability.[7] Second, we work with 9th graders in general or technical schools. We exclude *telesecundarias* (a type of middle school where classes are implemented through a television, without the physical presence of a teacher) and other types of schools which represent a minor share of the existing educational facilities in the intervention area. Third, we focus on schools with a considerable mass of COMIPEMS applicants in 2012. Thus, our universe of interest will be limited to schools with more than 30 applicants.

Schools that comply with these criteria are then grouped into four broad geographic regions and terciles of the school-average performance among ninth graders in a national standardized test aimed at measuring academic achievement (ENLACE). The geographic distribution of all the schools in the intervention sample is shown in Figure 1. Treatment assignment is randomized within each of the 12 resulting strata and at the school level: 44 schools are assigned to take the mock-exam and receive face-to-face feedback on performance, 46 schools are assigned to take the mock exam without being informed about the test results and 28 schools are assigned to a super-control group, in which we only collect follow up data. Within each of the schools in our sample, we randomly pick one section among the entire ninth grade cohort and interview the universe of students therein.

The baseline survey and the mock exam took place on a rolling frame basis. A baseline survey was conducted over the last two weeks of January 2014. Two or three days after the

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[6]Right after the delivery of the score, we ask treated students to evaluate on a 1 to 10 point scale the strength of each of these three signals. The median value for the individual score is 9/10, whereas median values for the relative ranking in the class and in the universe of applicants are 8/10.

[7]The socio-demographic survey from 2012 collected information on whether students took any preparatory course before submitting their schooling preferences. On average, 33% of applicants participated in one of these courses. This share ranges from 44% to 12% across schools in neighborhoods with low to very high levels marginalization.

survey was implemented in a given school, we administered the mock-exam. A follow-up survey was then conducted in the second and third weeks of February 2014, right before the registration period (February 19th-March 7th). Both baseline and follow-up surveys elicit subjective expectations about exam performance. Figure 2 depicts the timing of the application process and the intervention activities.

2.3   Subjective Expectations

As mentioned above, the registration process requires students to fill out a socio-demographic survey which collects very detailed information at the individual level. We thus design our baseline and follow up surveys so as to complement the information provided in the official data. In particular, we collect information on both retrospective and prospective preparation for the admission exam, previous exposure to mock exams or any other standardized achievement test, and social networks both within the class and in the school. Most importantly, an entire module is devoted to collect detailed information on the subjective distribution of beliefs about performance in the admission exam and high-school academic trajectories. In order to help students understand probabilistic concepts, we rely on the use of visual aids (see, e.g., Delavande et al. [2011a]). In particular, we explicitly link the number of beans placed in a small container to a probability measure, where 0 beans means that the student thinks that a given event is not going to occur while 20 beans means that the students believes the event will occur for sure.[8]

Students were provided with 20 beans and a card with 6 discrete intervals of the score in the entry exam (see Table 1). Surveyors then elicit students' expected performance in the COMIPEMS exam by asking them to allocate the beans in each score interval in the card.[9] The resulting individual beliefs distributions seem well-behaved. Only 6% of the

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[8]To make sure students understood the mechanics of the question and the main probabilistic concepts, we included a set of check questions:

  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?

Provided that 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 less beans in question 3 (close to zero probability event) than in question 4 (close to one probability event). Only 11 students out of 4,127 (0.27%) ended up making mistakes in these check questions.

[9]During the pilot activities, we tested two versions of this exercise with 4 and 6 intervals and 10 and 20 beans, correspondingly. Although we wanted as many discrete intervals as possible, we were afraid that 6

respondents concentrate the 20 beans in one interval. The mean belief distribution displays a large support between 20 and 120 points. Similarly, the average standard deviation is roughly 18 points, which resembles the standard deviation of the score in the mock exam in our sample (16 points). Using the 20 observations (i.e., beans) per student, we run the Shapiro-Wilk normality test under two cases in which we deal differently with the zero variance observations: (i) imputing a zero p-value and (ii) dropping these observations. In both cases, normality is rejected for only 15% of the respondents.

2.4 Sample Description

Among the 90 ninth-grade sections assigned to take the mock-exam, we interviewed 3001 students in the baseline survey but only 2790 were present the day of the exam, and 2544 students were found in all three visits. The overall attrition rate of 15% is orthogonal to treatment assignment. After augmenting this sample with 912 students who belong to the super-control group, we end up with 3,456 observations. Of those, we could match 3,100 (90%) with the official data collected within the COMIPEMS system which feature detailed socio-demographic characteristics (gender, age, household income, parental education and occupation, personality traits, among others) and schooling trajectories (GPA in middle-school, highest educational level aspired, study habits, among others) as well as the full ranked list of schooling options requested during the application process and information about students' placement (including their score in the admission exam).[10]

Table 2 provides basic descriptive statistics and a balancing test of the randomization for the main variables that we employ in the empirical analysis. Overall, no significant differences are detected across groups, which is consistent with the random treatment assignment. By the time baseline data was collected, about 30% of the students in our sample have taken at least one COMIPEMS mock exam and roughly half of them received feedback about their performance therein. Half of the students declare to have attended special training preparation for the COMIPEMS exam by the time registration occurs.

In line with the sample selection strategy, students in our sample are quite disadvantaged. About a third of them declare to be working, with or without a wage, and only 12% have

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was too demanding for the students. However, students seemed to have no problems manipulating 20 beans across 6 intervals and we decided to keep this version for the final instruments.

[10]The 10% discrepancy between the survey data and the administrative data is unlikely to be driven by a mismatch in the personal identifiers used to link the two data sources. Rather, it should be mainly interpreted as the result of enrollment decisions in the COMIPEMS. Given the scarcity of high-school alternatives in the public education system outside of the COMIPEMS system, this discrepancy could proxy the drop-out rate between middle school and high-school in the sample.

parents with complete higher education. Despite their adverse backgrounds, almost 70% of the students in our sample plan to go to college. On average, students in our sample expect to score about 74 out of 128 points if the placement exam were to be taken the day of the baseline survey. However, their average performance in the mock exam, two or three days after the baseline, is around 60 points.

Besides the expected changes in beliefs, the provision of feedback on performance or even taking the mock exam in itself may induce subsequent changes in study effort which may affect performance in the placement exam. Table 2 shows that this does not seem to be the case; the COMIPEMS exam score is balanced, not only across treatment and control groups, but also relative to the super control group.

3   Treatment Impacts

In what follows, we present evidence of the effect of the treatment on students' beliefs as well as on school-track choices and assignment. In order to isolate the role of the achievement feedback embedded in our intervention from the effects of taking the exam in itself, we compare average outcomes for students in the treatment group with those in the control group. We further rely on the auxiliary super-control group in order to document any potential effect that taking the mock exam may have on students' expectations about achievement.

3.1   Students' Perceptions about Achievement

The top panel of Figure 3 plots the empirical CDF of the mean beliefs in the baseline as well as of the realized scores in the mock exam and in the COMIPEMS placement exam, respectively, for the sample of 2,293 students in the treatment and control groups. While the mean belief distribution clearly dominates the distribution of mock exam scores, this pattern tends to fade out for high values of the support where there is an overlap between placement score and mean priors. This is not surprising since students had about three additional months to prepare between the mock and the placement exam. At the time of the year in which we administered the mock exam, students are far from concluding their preparation for the exam. Relative to the mock exam score, the score in the placement exam may incorporate additional study effort and other behavioral responses that take place over time and that should explain the systematic divergence between the two distributions as scores increase. Nevertheless, the linear correlation between these two performance measures remains quite substantial: 0.82.

Though we do not observe the individual distributions of academic performance, the fact that we observe two individual realizations of a similar standardized achievement exam in two different points in time gives us some mileage to measure the presence of overconfidence in our sample. The median student overestimates his test performance in a range comprised between 12 and 15 points. The lower panel of Figure 3 depicts the corresponding CDF of the individual gaps between individual mean priors and both performance measures. The support of the gap between expected and realized performance is quite large for both performance measures, fluctuating between -57 to 68 points. Overconfidence seems to be the norm in our sample, irrespective of the performance measure we use: between 71% and 81% of the students in our sample overestimate their ability to perform in the achievement test.

Table 3 presents OLS evidence behind the effect of the intervention on the distribution of students' subjective expectations about their performance in the test. Columns 1 reports treatment impacts on beliefs' accuracy, as measured by the absolute value of the gap between mean expected performance and the realized score in the mock exam. On average, the intervention induces students' to close their expectation gap by 6.6 points, which is about a third of the mean in the control group. Column 2 shows that providing students with information on their score in the mock exam reduces the average standard deviation of the subjective achievement distributions by roughly 15% relative to the average in the control group. Finally, columns 3 and 4 report evidence in favor of heterogenous treatment effects on beliefs' updating along two dimensions: (i) the magnitude of the gap between mean priors and the signal and (ii) the dispersion of the prior distribution of beliefs. Consistent with Bayesian updating, the effect of the treatment on gap reduction is more pronounced for students with larger negative gaps in the baseline and with less noisy priors.

In order to shed more light on the likely effects of the intervention on beliefs, Figure 4 depicts the CDF of the absolute value of the gap between mean priors and placement score for treatment, control and super-control groups (recall that this is the only performance measure we can use when we wish to consider the super-control group). Relative to the control and the super control groups, the gap between expected and realized performance as measured by actual score is narrower among students in the treatment group. More importantly, the gap distributions of the control and super-control groups lay on top of each other, suggesting that the receipt of individual feedback about test performance, rather than exam-taking *per se*, is what affects students' perceptions about their academic achievement.[11]

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[11]The corresponding (unreported) OLS estimates provide further support for this claim. The treatment significantly decreases the absolute gap between beliefs and realized performance by 3.41 points (std. err.=0.57) vis-a-vis the super-control and by 4.33 points (std.err.=0.53) vis-a-vis the control group. There

3.2 School Choices and Placement

We hereby consider the average share of general education programs elicited in the COMIPEMS application portfolio as a proxy for students' underlying demand for academically-oriented educational programs. We first restrict the analysis sample to the super-control group and regress the average share of solicited general education programs on students' expected mean priors and their score in the placement exam. Column 1 of Table 4 reports the associated OLS estimates - where both variables have been normalized to zero mean and a unitary variance in order to provide a straightforward interpretation of the magnitudes of the estimates. A one standard deviation increase in expected achievement is associated with a 2.3 percent increase in the average slope of the demand function for academic options. On the contrary, the estimated coefficient of the realized achievement as measured by the score in the placement exam is small and statistically insignificant. We next repeat this exercise for the probability of assignment in a school from the academic-oriented track as the dependent variable. The estimates, reported in Column 3 of Table 4, reveal that a similar pattern holds in for final placement, though with a higher point estimate (3.2 percentage points) for the slope of expected performance. These findings provides initial evidence of the effect that students' inaccurate perceptions about achievement have on schooling decisions and subsequent academic trajectories.

We next evaluate whether and how track choices and placement outcomes change as a result of the treatment. The OLS estimates reported in Column 2 of Table 4 show that our intervention increased the sensitivity of students' demand for academic-oriented programs with respect to their realized achievement. Compared to students in the control group, students who receive feedback on their performance in the mock exam exhibit increased demand elasticity with respect to academic performance. A one standard deviation increase in their mock exam score is associated with a 5.7 percent increase in the share of academic options in their portfolios vis-a-vis students who only take the test. Results in column 4 show that a similar pattern emerges for the probability of assignment in a school offering an academic curriculum, thereby providing further evidence on the likely effects of our intervention in terms of real outcomes and subsequent academic trajectories.

We further assess the robustness of these findings by using two alternative measures of academic achievement: the placement exam score and the Grade Point Average (GPA) in junior high-school. The corresponding OLS estimates are reported in Table 5 and they are

are no statistically significant differences in the absolute gap between the control and the super-control group. Results are available upon request.

remarkably consistent across all specifications, both in terms of signs and magnitudes.

4 Learning about Ability and School Choices

Overall, the body of evidence presented in Section 3 suggests that informing students about their performance in a standardized achievement test affects their perceptions about their academic achievement as well as their school-track choices. The signal embedded in our intervention varies across individuals depending on their idiosyncratic realization of performance, and so school choice behaviors are likely to be heterogeneous depending on the value of the signal with respect to the prior distribution of beliefs about achievement.

In order to uncover those effects, in this section we first embed our school choice context in a bayesian learning framework with self-image concerns [Köszegi, 2006]. In the model, students' beliefs about their ability determine their school-track choices through a standard utilitarian motive and an additional "ego utility" component. This second element generates an asymmetry in the incentives for individuals holding different beliefs to acquire additional information about their ability. We next use the empirical predictions of the model in order to motivate an empirical analysis of the heterogeneous effects of the intervention on school-track choices.

4.1 Model

Students do not observe their ability a but they know that it distributes $N(\mu_a, \sigma_a^2)$ in the population. The basic timing of the model is as follows. In period $t = 0$, students are able to observe an arbitrary (and possible infinite) number of signals about their own ability for free. In period $t = 1$, students make a school choice decision based on their updated beliefs. This decision generates a random performance outcome which is increasing in ability and that is realized after period $t = 1$.

Initial Beliefs and Updating at t=0. In period $t = 0$, signals are given by $s_0^j = a + \epsilon_0^j$, where $\epsilon_0^j \sim N(0, \sigma_s^2)$ and $j = 0, 1, \ldots$ labels the signal's order of drawing. After each s_0^j, the agent decides to keep drawing signals or to stop and move on to $t = 1$. Let F_0^j denote agent's cumulative distribution of beliefs in period $t = 0$ after j signals and assume it is derived from a Bayesian updating process.

Preferences and Choices at t=1. In period $t = 1$, students choose a school track k_1 based on their updated beliefs after drawing J signals in $t = 0$. Their utility function is given by

$$U = u(F_1) + x(s_1, k_1), \tag{1}$$

where performance in high school is given by $s_1 = a + \epsilon_1$ and $\epsilon_1 \sim N(0, \sigma_s^2)$.

The first term in (1) corresponds to the ego utility component which depends on the cumulative distribution of beliefs about ability at the beginning of $t = 1$ (normally distributed with mean μ_1). Choosing the academic track is denoted by $k_1 = 1$ while all other tracks choices are given by $k_1 = 0$. Technical or vocational tracks yield a zero-payoff irrespective of performance, s_1. In turn, the successful completion of an academic-oriented program depends on performance. The instrumental component of the utility function is

$$x(s_1, k_1) = \begin{cases} 0, & \text{if } k_1 = 0 \\ 1, & \text{if } k_1 = 1 \text{ and } s_1 > \underline{c} \\ -1, & \text{if } k_1 = 1 \text{ and } s_1 \leq \underline{c} \end{cases} \tag{2}$$

where $\underline{c}$ is the minimum performance required to be successful in the general track.

Agents do not observe s_1 but they know that the probability of success in the ambitious task is given by $\Pr(s_1 > \underline{c})$. They derive ego utility from thinking that they are good enough to perform successfully in the academic-oriented track. In particular, ego utility is 1 if the agent believes he would get a higher payoff from $k_1 = 1$ than from $k_1 = 0$ and zero otherwise. Under risk neutrality, $k_1 = 1$ is a better choice whenever $\Pr(s_1 > \underline{c}) \geq \frac{1}{2}$. With the distribution of s_1 centered around μ_1, $\Pr(s_1 > \underline{c}) \geq \frac{1}{2}$ if and only if $\mu_1 > \underline{c}$. Thus,

$$u(F_1) = \begin{cases} 1, & \text{if } \mu_1 > \underline{c} \\ 0, & \text{otherwise} \end{cases} \tag{3}$$

The differential effects of ability signals on school choices. In this framework, students' incentives to learn about their ability depend on whether the expected gain from getting more accurate information about the probability of success in the academic track outweighs the potential damage that new signals may cause to their ego. This trade-off generates an optimal stopping rule for students holding relatively high expectations about their future performance, whereas students with relatively low expectations about themselves

keep drawing signals both for ego and instrumental reasons.[12]

Our intervention provides an additional (and unsolicited) signal s_0^t about students' ability at some point during $t = 0$. Let μ_0 denote students' mean beliefs at the moment in which the intervention takes place and let μ_t denote mean beliefs after the score in the mock exam is delivered. Absent the treatment, and assuming for simplicity that the learning process outlined above has converged by the time of the intervention, those with $\mu_0 > \underline{c}$ choose the academic track and those with $\mu_0 \leq \underline{c}$ choose the other tracks. The effect of the treatment on school-track choices will thus fundamentally differ depending on whether $s_0^t \lessgtr \mu_0$. Students receiving a signal which is below their average prior beliefs are more likely to keep searching for more signals and hence they are less likely to align their choices in response to the treatment. On the other hand, students who receive a signal which is above their mean prior beliefs are likely to stop learning about their ability. As a result of the more positive views about their ability, they are more prone to shift their schooling preferences toward the academic-oriented track. Formally, if $\mu_0 < s_0^t$ the treatment-induced increase in beliefs may translate into a school-track change for those (marginal) students with $\mu_0 \leq \underline{c}$ and $\mu_t > \underline{c}$. If instead $s_0^t < \mu_0$, those with $\mu_0 > \underline{c}$ and $\mu_t \leq \underline{c}$ have an incentive to search for additional signals until they get a high enough signal which undoes the negative effect of the treatment on beliefs ($s_0^{t+j} > s_0^t$ such that $\mu_{t+j} > \underline{c}$).

4.2 Empirical Predictions and Results

The stylized model outlined in the previous sub-section features a number of empirical prediction that are testable in the school choice context under study. First, even though students are bayesian and thus unbiased in their beliefs about ability, the beliefs at the end of period 0 are on average biased upward - in the sense that there are more students who over-predict their ability than there those who under-predict it (see Figure 3 and the related discussion in section 3.1). Second, the asymmetric learning process sketched above not only implies over-confidence, but also a systematic relationship between the level of students' beliefs and the bias in them. In particular, students with relatively high beliefs are over-confident on average, whereas those with relatively low beliefs are unbiased on average. Figure 5 displays the empirical relationship between the share of over-confident students against mean beliefs in the baseline. Both measures of over-confidence, obtained using the realizations of test

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[12]The rather special functional forms chosen for equations (2) and (3) are quite innocuous for this result. In general it is required that (i) the ego utility component is concave for higher values of beliefs and convex for lower values, and (ii) there is a tight link between ego and instrumental utility (i.e. students not only like to believe they are good but also that they are good enough to be successful in the academic track).

performance in the mock exam and the placement exam respectively, reveal the presence of a clear increasing relationship mean beliefs.

Most importantly, the theoretical framework provided us with a rationale behind the presence of differential impacts of the treatment depending on the direction of the potential beliefs updating. Accordingly, we generate an indicator variable which takes the value of 1 if the score in the mock exam is greater than mean beliefs in the baseline (i.e. right-updaters) and 0 otherwise (i.e. left-updaters), and allow our treatment indicator to vary across these two categories. Column 1 in Table 6 shows that the treatment generates a greater adjustment in absolute terms in the belief distribution observed immediately after the delivery of the mock exam score. While left-updaters in the treatment group experience a reduction in their mean posteriors of almost 10 points, right-updaters only see their mean posterior move about 3 points to the right. This is not surprising given the evidence reported in Figure 5. The average expectation gap for left-updaters is 21 vis-a-vis 10.7 for right-updaters and thus the associated updating process generates a greater adjustment.

The change in posterior beliefs among right-updaters is sufficiently strong and persistent to generate an average increase of 8.3 percentage points in the share of academic options requested by those students, as shown in column 2 of Table 6. This is a large effect as it corresponds to 18 percent of the average in the control group. As predicted by the model, these students are more likely to stop searching for additional signals after receiving a high enough signal, and hence they are more likely to choose the academic-oriented track which is better aligned with their expected abilities. In turn, left updaters seem to only temporarily incorporate the signal in their beliefs.

The strong and significant effect identified on beliefs and the lack of effect on track choices suggests that left-updaters actively searched for additional signals after the intervention took place that partially undid the effects of the treatment on their beliefs. This story is somehow confirmed in our setting by the evidence presented in column 3 from Table 6. Since the date of the registration in the COMIPEMS system is solely determined by the first letter of the applicant's last name, irrespective of the school origin, the number of days between the delivery of the score in mock exam and the registration of school preferences should be orthogonal to any determinant of school choices. Using this additional source of variation, we find that left updaters who only had one day between the delivery of the score of the mock exam and the registration of their schooling preferences do in fact reduce the share of requested academic-oriented schools by 18 percent. Among these students, there is very little time to gather additional signals and thus the observed decrease in mean beliefs reported in

column 1 seem to carry real consequences for track-choice behaviors.

Column 4 in Table 6 presents heterogenous treatment effects for right updaters. According to the model discussed above, the higher the mean beliefs before the intervention the more likely the associated ability signal moves beliefs up to the point in which it makes sense for the students to choose the academic-oriented track. The estimates reported in column 4 provides evidence in favor of this prediction.

The treatment indicator is a valid instrument for students' beliefs provided that (i) there is no other channel through which the intervention might influence school-track choices and (ii) beliefs respond to the treatment (see column 1 of Table 6). Given the prediction of the model and the associated reduced-form evidence reported in column 2, we report in column 5 of Table 6 the instrumental variable results only for the sub-sample of right-updaters. For the average right-updater in the sample, the treatment-induced increase in mean beliefs is associated with an increase in the share of requested academic-options of roughly 10 percent $(= 2.79 \times 0.037)$.

5   Conclusions

Access to adequate and timely information is key for parents and students to make optimal educational choices. Though recent studies have analyzed the impact of providing relevant information on population-average characteristics, our paper is one of the few that has paid attention to the role played by individualized information about academic ability in explaining schooling decisions.

We exploited the COMIPEMS admission mechanism which is currently in place in the metropolitan area of Mexico City to allocate prospective students into public high-schools and designed an experimental intervention that provided students with a mock version of the admission exam used to rank and place students. This design allowed us to explore whether and how the provision of information about individual performance in an achievement test enables students to update their beliefs about their own academic ability and, how this change affects school choices at the upper secondary level.

We have documented that the intervention helps students aligning expectations about their own ability with actual achievement. The treatment also shifted school choices towards a positive assortative matching equilibrium in which better performers tend to choose (and be assigned to) more academic-oriented school-tracks. In spite of substantial changes in mean beliefs as a result of the intervention, the provision of individual performance feedback did

not translate into choices for all students in our setting. In particular, students who received negative feedback about their ability with respect to their mean beliefs did not alter their school-track decisions. On the contrary, those who receive positive feedback tend to choose a significantly higher share of schools that belong to the academic-oriented track.

This evidence is consistent with the presence of a hedonic concern for self-image into an otherwise standard bayesian learning framework. The distribution of beliefs captured in the follow up survey is likely to keep changing for the students who receive a bad signal, as this is detrimental for their egos and hence they keep on searching for additional information about their ability until they get a high enough signal which undoes the negative effect of the treatment on beliefs. Instead, for the average right-updater in the sample, the treatment-induced increase in mean beliefs is associated with an increase in the share of requested academic-options of roughly 10 percent.

Our analysis includes some limitations. For instance, longitudinal information on students' beliefs *after* the treatment would allow directly testing the key implication of the learning model on the presence of an asymmetric response to the provision of the performance feedback in our setting. Yet, further evidence which employs variations in the time elapsed between the provision of information about test performance and the submission of school preferences seems to indirectly confirm this hypothesis. Besides, while in Section 3 we speculate that our effects may be persistent along the students' educational trajectories, we currently lack information on later outcomes. In future work, we plan to investigate the potential longer-term consequences of our intervention on high-school dropout and academic achievement.

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Figures

Figure 1: The Schools in the Sample within the Metropolitan Area of Mexico City
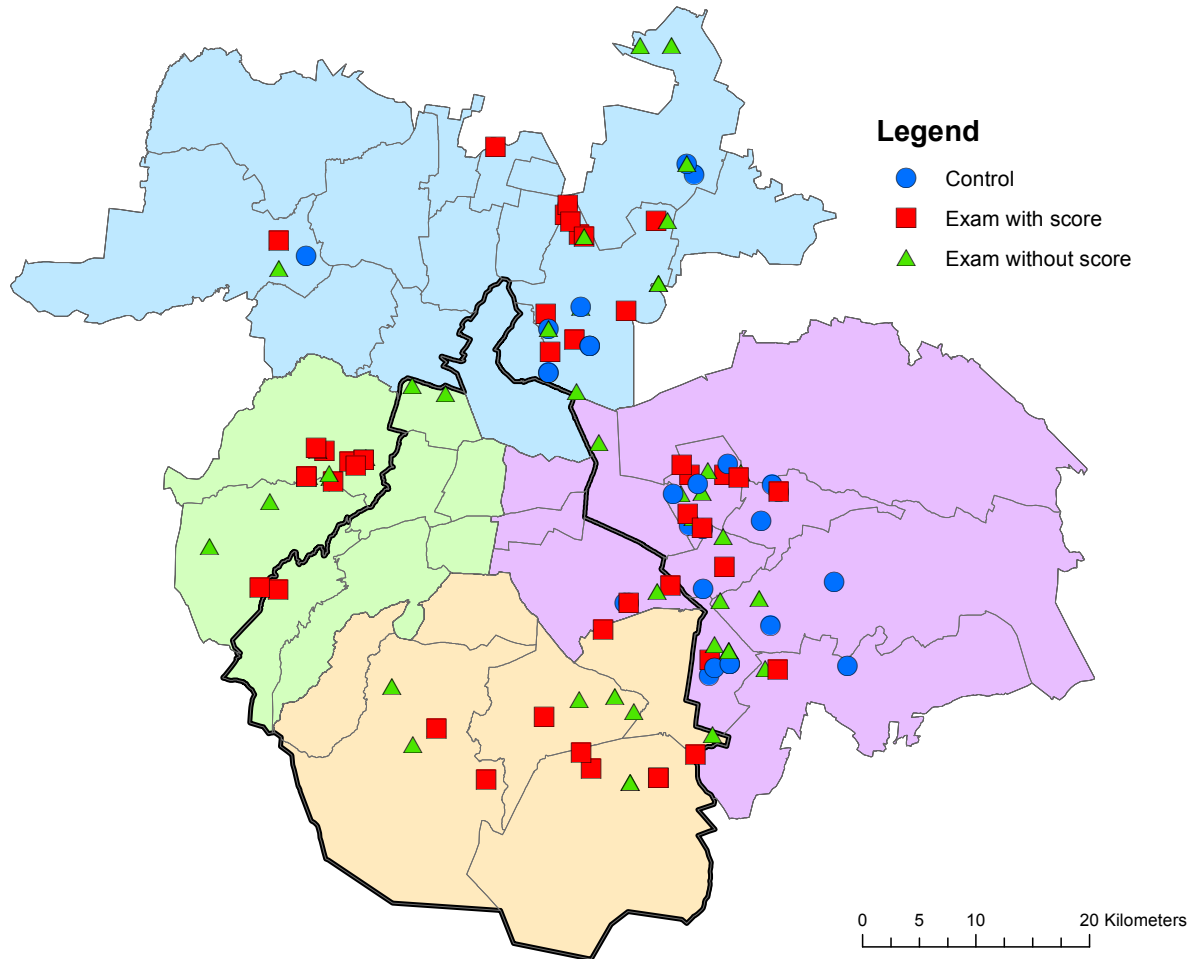

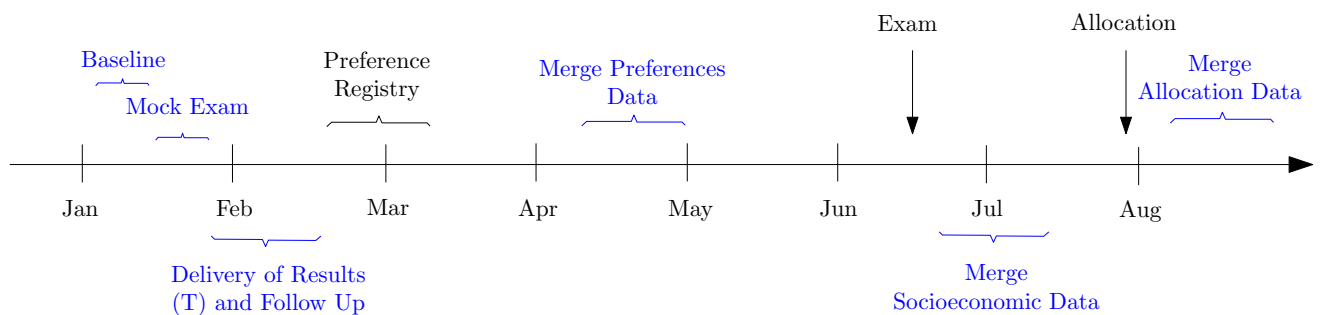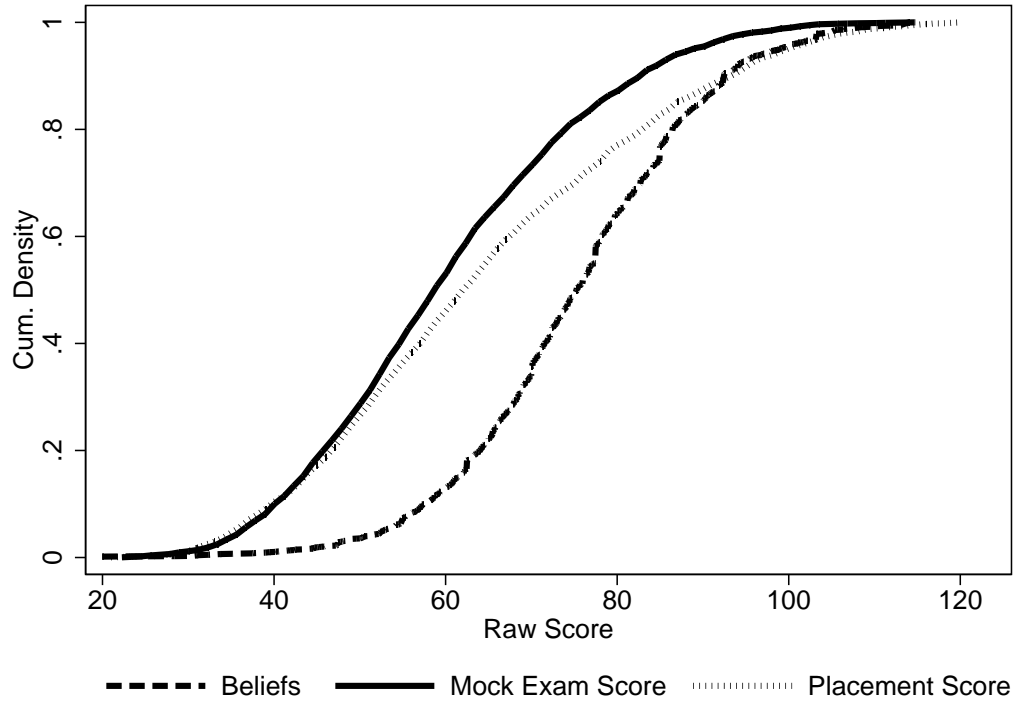Figure 2: The COMIPEMS Process and the Intervention: Timeline of Events

Figure 3: Baseline Beliefs and Realized Ability

(a) CDF of Mean Belief Priors and Test Performances
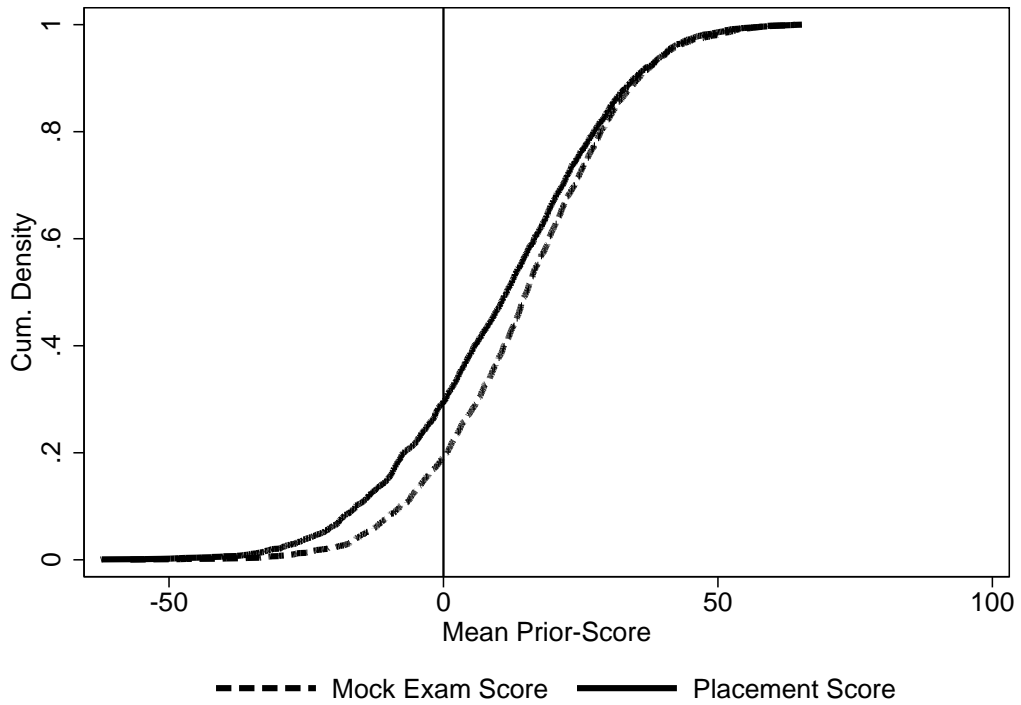
(b) CDF of Individual Gaps

Figure 4: Decomposition of Treatment Effects on Beliefs
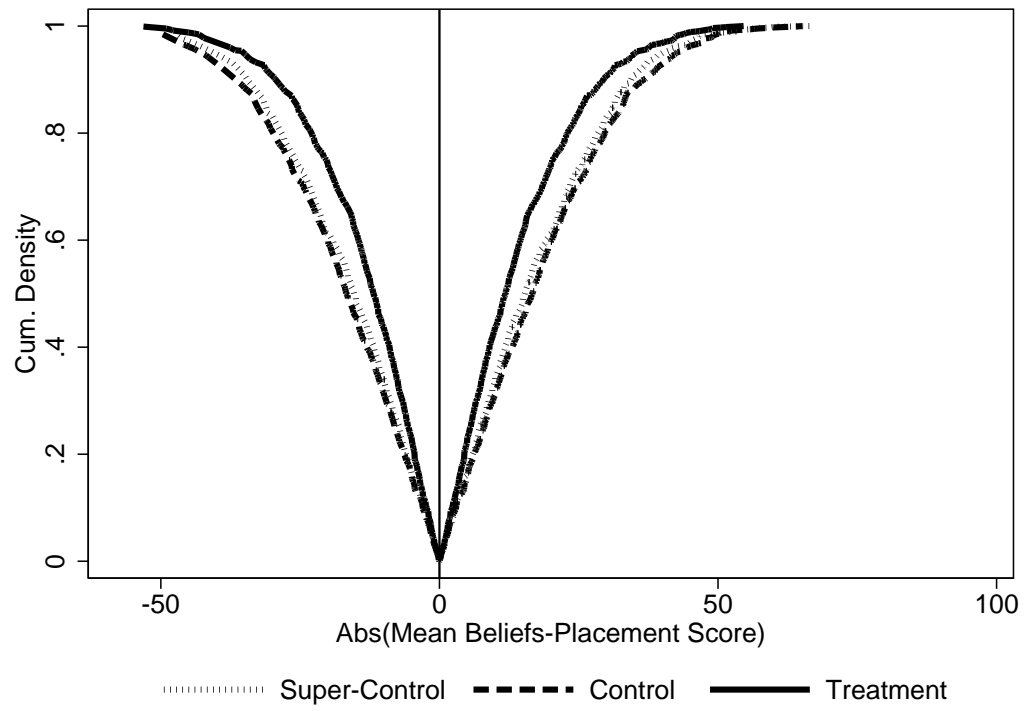
Figure 5: Over-confidence and Mean Prior Beliefs
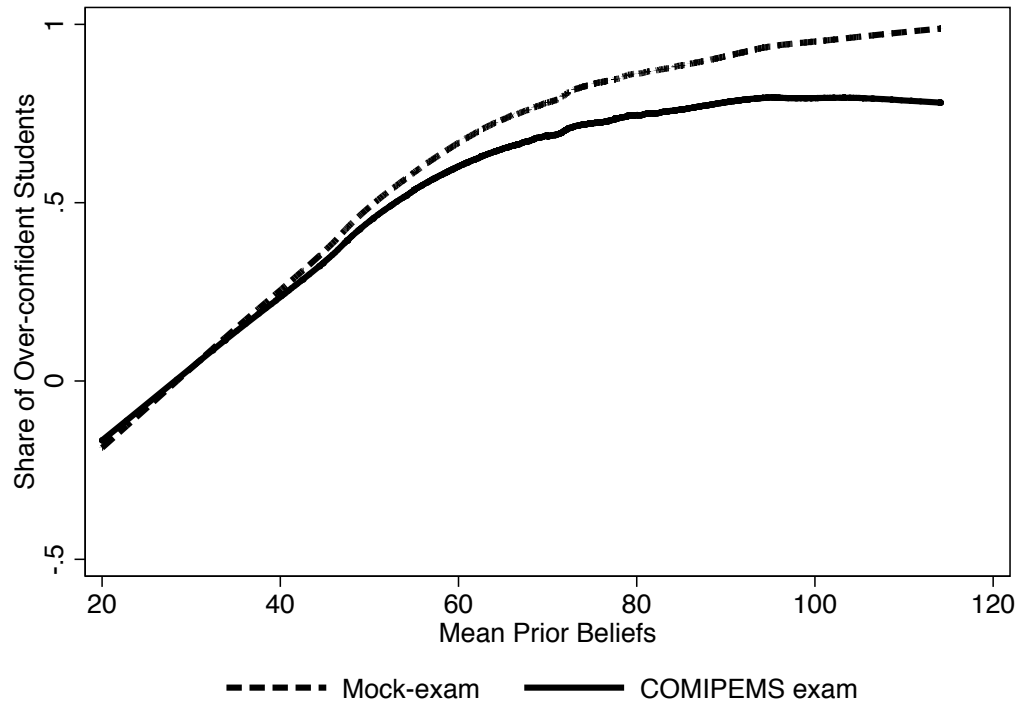
Tables

Table 1: Card Used to Elicit the Subjective Distributions about Performance in COMIPEMS Exam

Between 0 and 40	
Between 40 and 55	
Between 55 and 70	
Between 70 and 85	
Between 85 and 100	
Between 100 and 128	

NOTE: The survey question is the following (authors' translation from spanish): *We are going to do an exercise. Suppose that you take the COMIPEMS exam today, in which the maximum possible score is 128 and the minimum is zero. How sure are you that the following scenarios occur?*

Table 2: Summary Statistics and Randomization Check

	Control (C) (1)	Treated (T) (2)	Super-Control (SC) (3)	T-C (4)	C-SC (5)	T-SC (6)
Mean prior beliefs	74.39 (14.42)	74.45 (14.40)		0.015 [0.98]		
SD prior beliefs	18.06 (8.29)	17.62 (8.33)		-0.526 [0.25]		
Mock Exam score	58.77 (15.62)	60.75 (16.40)		1.654 [0.13]		
Placement exam score	64.93 (19.65)	64.88 (19.93)	63.09 (19.50)	0.235 [0.84]	-0.238 [0.86]	-0.129 [0.93]
GPA (middle school)	8.094 (0.87)	8.126 (0.84)	8.049 (0.85)	0.011 [0.83]	0.059 [0.34]	0.065 [0.31]
COMIPEMS enrollment	0.904 (0.29)	0.898 (0.30)	0.885 (0.32)	-0.007 [0.58]	0.027 [0.13]	0.019 [0.23]
COMIPEMS pre-enrollment	0.484 (0.50)	0.514 (0.50)	0.563 (0.49)	0.008 [0.89]	-0.106 [0.16]	-0.099 [0.20]
Gender (male)	0.469 (0.50)	0.497 (0.50)	0.478 (0.50)	0.024 [0.17]	-0.001 [0.95]	0.022 [0.24]
Previous mock-test (dummy)	0.287 (0.45)	0.305 (0.46)	0.269 (0.44)	0.017 [0.64]	-0.001 [0.98]	0.018 [0.72]
Previous mock-exam w/ results	0.179 (0.38)	0.193 (0.39)	0.151 (0.36)	0.012 [0.73]	0.010 [0.79]	0.023 [0.59]
Attend prep. course	0.519 (0.50)	0.497 (0.50)	0.419 (0.49)	-0.027 [0.37]	0.067 [0.08]	0.045 [0.25]
Morning shift (junior high-school)	0.618 (0.49)	0.664 (0.47)	0.779 (0.41)	0.007 [0.94]	-0.118 [0.28]	-0.110 [0.31]
Lives w/ both parents	0.784 (0.41)	0.795 (0.40)	0.749 (0.43)	0.010 [0.60]	0.042 [0.08]	0.050 [0.04]
Parents with higher ed.	0.122 (0.33)	0.126 (0.33)	0.112 (0.32)	0.007 [0.71]	-0.021 [0.33]	-0.016 [0.52]
SE index (above-median)	0.491 (0.50)	0.527 (0.50)	0.476 (0.50)	0.025 [0.32]	-0.001 [0.96]	0.022 [0.47]
Currently working (w/o wage)	0.324 (0.47)	0.306 (0.46)	0.382 (0.49)	-0.021 [0.33]	-0.044 [0.13]	-0.065 [0.022]
Plans to attend college	0.729 (0.45)	0.718 (0.45)	0.689 (0.46)	-0.014 [0.50]	0.013 [0.66]	-0.002 [0.94]
Missing value (any control variable)	0.344 (0.48)	0.369 (0.48)	0.323 (0.47)	0.028 [0.22]	-0.018 [0.55]	0.008 [0.79]

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

Table 3: Treatment Impacts on Perceptions about Exam Performance

	Abs(Gap)	SD Beliefs	Gap	Abs(Gap)
	(1)	(2)	(3)	(4)
Treatment	-6.596***	-2.626***	-1.967**	-10.421***
	(0.642)	(0.420)	(0.787)	(1.249)
TreatX(Prior-Mock Exam Score)			-0.368***	
			(0.036)	
(Prior-Mock Exam Score)			0.612***	
			(0.029)	
TreatX(SD Prior)				0.215***
				(0.053)
SD Prior				-0.205***
				(0.039)
Mean Dep. Var. in Control	19.59	17.45	15.96	19.59
Number of Obs	2293	2293	2293	2293
R-squared	0.290	0.083	0.539	0.299
Number of Clusters	90	90	90	90

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%.
Strata dummies included in all specifications, standard errors clustered at the school level. Sample of ninth graders in schools who belong to the treated and the control group. All specifications include the following set of pre-determined individual and school characteristics (see Table 2 for details): gender (male), previous mock-test, previous mock-test with results, attendance to preparatory course, morning shift, both parents in the household, parents with higher education, SES index (above median), currently working, plan to attend college, and a dummy for whether one of the above has a missing value.

Table 4: Treatment Impacts on School Choice and Placement

Dependent Variable	Portfolio Share of Academic Schools		Prob(Assignment in Academic Schools)	
Sample	SC	T&C	SC	T&C
	(1)	(2)	(3)	(4)
Mean Beliefs (z-score)	0.023*		0.032**	
	(0.013)		(0.015)	
Placement Score (z-score)	-0.007		0.019	
	(0.011)		(0.021)	
TreatXMock		0.041***		0.046*
		(0.013)		(0.026)
Mock Exam Score (z-score)		-0.016*		0.035
		(0.009)		(0.022)
Treatment		0.009		-0.027
		(0.016)		(0.024)
Mean DepVar in Control	0.514	0.518	0.426	0.425
Number of Obs	786	2293	786	2293
R-squared	0.084	0.087	0.049	0.075
Number of Clusters	28	90	28	90

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%.
Strata dummies included in all specifications, standard errors clustered at the school level. Sample of ninth graders in schools who belong to the treated and the control group. All specifications include the following set of pre-determined individual and school characteristics (see Table 2 for details): gender (male), previous mock-test, previous mock-test with results, attendance to preparatory course, morning shift, both parents in the household, parents with higher education, SES index (above median), currently working, plan to attend college, and a dummy for whether one of the above has a missing value.

Table 5: Robustness Checks: Alternative Ability Measures

Dependent Variable	Portfolio Share of Academic Schools		Prob(Assignment in Academic Schools)	
	(1)	(2)	(3)	(4)
TreatXExam	0.031**		0.053*	
	(0.013)		(0.028)	
Placement Score (z-score)	-0.017*		0.042*	
	(0.010)		(0.023)	
TreatXGPA		0.027**		0.055**
		(0.011)		(0.022)
GPA (z-score)		-0.006		0.023
		(0.008)		(0.015)
Treatment	0.013	0.012	-0.019	-0.020
	(0.017)	(0.017)	(0.024)	(0.025)
Mean DepVar in Control	0.520	0.519	0.433	0.428
Number of Obs	2253	2276	2253	2276
R-squared	0.086	0.083	0.081	0.071
Number of Clusters	90	90	90	90

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%.
Strata dummies included in all specifications, standard errors clustered at the school level. Sample of ninth graders in schools who belong to the treated and the control group. All specifications include the following set of pre-determined individual and school characteristics (see Table 2 for details): gender (male), previous mock-test, previous mock-test with results, attendance to preparatory course, morning shift, both parents in the household, parents with higher education, SES index (above median), currently working, plan to attend college, and a dummy for whether one of the above has a missing value.

Table 6: Treatment Effects by the Direction of the Beliefs' Updating

Dependent Variable	Mean Beliefs	Portfolio Share of Academic Schools			
Sample	All	All	Left-updaters	Right-updaters	
Estimator	OLS	OLS	OLS	OLS	IV
	(1)	(2)	(3)	(4)	(5)
TreatXRight	2.786**	0.083***			
	(1.317)	(0.029)			
TreatXLeft	-9.854***	-0.005			
	(0.915)	(0.017)			
Right-updater	-14.533***	-0.057**			
	(1.135)	(0.022)			
Treatment			-0.003	0.114***	
			(0.017)	(0.034)	
TreatXShortDelay			-0.177***		
			(0.060)		
Short Delay			0.083*		
			(0.046)		
TreatXMeanPrior				0.041*	
				(0.023)	
Mean Prior				-0.001	
				(0.028)	
Mean Beliefs					0.037**
					(0.016)
Mean Gap Right (ctrl)	10.7				
Mean Gap Left (ctrl)	21.0				
Mean Dep. Var. Right (ctrl)		0.46			
Mean Dep. Var. Left (ctrl)		0.52			
Cragg-Donald F-Stat					4.543
Number of Obs	2293	2293	1851	441	441
R-squared	0.346	0.086	0.083	0.169	
Number of Clusters	90	90	90	84	84

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%.
Strata dummies included in all specifications, standard errors clustered at the school level. Sample of ninth graders in schools who belong to the treated and the control group. All specifications include the following set of pre-determined individual and school characteristics (see Table 2 for details): gender (male), previous mock-test, previous mock-test with results, attendance to preparatory course, morning shift, both parents in the household, parents with higher education, SES index (above median), currently working, plan to attend college, and a dummy for whether one of the above has a missing value.