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

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Abstract

Students' inaccurate perceptions about their own academic skills may alter schooling decisions by distorting the (perceived) returns of 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 exam used to rank students in a sample of ninth graders and communicate score results to a randomly chosen subset of them. We report three main findings. First, the intervention induces important margins of adjustment between expected and realized performance in the exam. Second, preferences for the academic-oriented track become, on average, more responsive to students' individual achievement due to the treatment. Third, as predicted by a simple Bayesian learning model with self-image concerns, school choice responses are asymmetric depending on the direction of the beliefs updating process induced by the intervention.

Keywords: school choice, information, perceived ability, hedonic utility, Bayesian updating, overconfidence.
JEL Codes: D83; I21; I24; J24.

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"It is not only the most difficult thing to know oneself, but the most inconvenient one, too." – H.W. Shaw

1 Introduction

Educational investments are in nature long-term choices frequently made with incomplete information. Traditionally, human capital investment models assume that agents effectively take into account both current and future net payoffs along their educational trajectories. The truth is that both parents and students often lack relevant pieces of information when making critical schooling decisions. More so, recent evidence from Ghana [Ajayi, 2011] and the US [Avery and Hoxby, 2012; Hoxby and Turner, 2014], for example, suggests that students from less advantaged backgrounds are the ones who are worse affected by these informational barriers.

This paper studies the role of informational gaps related to students' own academic potential as a possible determinant of school choices. We notably explore whether youths' inaccurate self-perceptions about academic ability generate misallocations through inadequate high-school track choices. In Mexico, as well as in several other countries, track choices are crucial in shaping subsequent access to college and, consequently, labor market earnings. To the extent that students' preferences over tracks depend on expected performance therein, biased perceptions about own academic ability will distort perceived individual returns potentially leading to sub-optimal choices with detrimental longer-term consequences on educational and labor market trajectories.

The existing literature on the determinants of human capital investments has paid little attention to the role played by *perceived*, as opposed to realized, academic ability.[1] This is in part due to the notorious difficulties in interpreting relative rankings among agents' self-assessments of skills [Benoit and Dubra, 2011], but it is also a reflection of the inherent identification problems that plague the empirical relationship between beliefs and school choices - e.g. those holding higher perceptions about their own academic ability are likely to sort into more academic-oriented schools. Our study aims to overcome both empirical challenges in the relationship between perceived ability and educational choices. In the context of a centralized and merit-based high school admission system, we design an experimental

[1] A recent and growing body of literature shows that poor information about population-average characteristics such as the returns to education [Jensen, 2010, 2012; Nguyen, 2008], school quality [Hastings and Weinstein, 2008], and financial aid opportunities [Dinkelman and Martinez, 2014; Carrell and Sacerdote, 2013] can negatively affect educational choices and subsequent trajectories.

intervention that provides a subset of students with a mock version of the admission exam used to rank and place applicants. We elicit subjective probabilistic distributions about test performance as a proxy for perceived academic ability both before and after the exam and communicate individual scores to a randomly chosen group of students. By comparing the treatment group to the placebo group that only takes the mock exam, we are able to assess whether providing students with a salient signal about their own academic ability enables them to update beliefs and, if so, how this change affects revealed preferences over the available education modality (tracks) within the high school admission system.

More specifically, we nest our study within 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 peculiar features, this system offers an ideal setting for our purposes. First, most applicants apply to high schools without a clear idea of their own academic ability. Students 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, administrative records capture submitted individual rankings (portfolios) over a wide range of available high school alternatives, which allows us to accurately measure changes in applicants' revealed preferences due to the treatment. Third, the matching mechanism in place and the large portfolio size that can be submitted imply that the assignment mechanism is strategy-proof [Pathak, 2011]. Since admission probabilities to each school do not play a role when choosing an application portfolio, any effect on choices due to our intervention is enabled through changes in net expected benefits from each schooling option. Fourth, placement in the system solely depends on preferences and individual performance in the admission exam. Since we can measure any change in those two outcomes induced by the intervention, we are also able to track its effects on final outcomes such as school placement and high-school trajectories.

Before the intervention, large discrepancies are found between students' expected and realized test performance. The distribution of mean beliefs clearly dominates both distributions of exam scores (mock and placement), revealing the presence of overconfidence in our data. We also find that self-assessments are strong predictors of school track choices and placement outcomes within a control group of students who did not take the mock exam, even after controlling for individual performance in the placement exam. Providing feedback about realized performance in the mock exam substantially reduces the gap between expected and actual performance and reduces the variance in the individual beliefs' distributions. Consistent with Bayesian updating, respondents who receive negative (positive) feedback relative to the their expectations adjust downward (upward) their mean posterior beliefs, and this effect is more pronounced among those with greater initial gaps. On average, preferences for the academic-oriented track become (more) responsive to students' individual achievement, as measured by both realizations of test performance as well as the cumulative Grade Point Average (GPA) in middle school. 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. However, the effects on preferences and placement outcomes seem to be concentrated among students who receive positive performance feedback relative to their baseline expectations. Relative to their counterparts in the placebo group, these students record an average increase of 18 percentage points in the share of academic programs requested within the assignment system. Instead, the estimated average treatment effect on school choices among students who receive negative feedback relative to their baseline expectations is very small and statistically insignificant.

We argue that this evidence is consistent with the presence of "self-image" motives in an otherwise standard school-choice model with unobserved academic ability and uncertainty about individual academic performance. In this framework, students' incentives to learn about their academic ability depend on whether the expected gain from getting more accurate information outweighs the potential damage that new signals may cause to the consumption value attached to envisioning themselves as capable of succeeding in an academic-oriented school. The additional (and unsolicited) signal provided by the intervention generates asymmetric school choice responses depending on the underlying change in individual incentives to keep searching for signals *before* choices are made. In particular, those who receive a signal that is above their mean beliefs (right-updaters) are less likely to draw additional signals after the treatment and, accordingly, they are prone to increase their demand for the academic-oriented track. Instead, students provided with a signal that is below their mean beliefs (left-updaters) have an incentive to search for additional signals until they get a high enough draw that partially compensates the negative effect of the treatment on mean beliefs. Consequently, these students are less likely to align their school choices with their realized academic performance in response to the treatment. Further evidence exploiting random variations in the time elapsed between the provision of information about exam performance and the submission of school preferences indirectly confirms this hypothesis. Among left-updaters who only had one extra day between the feedback delivery and the submission of their preferences, the treatment effectively reduces the share of academic options selected by

34 percentage points vis-a-vis the mean in the placebo group.

This paper speaks and contributes to three different strands of literature. First, existing evidence on the role played by misperceptions about own academic ability as a determinant factor of school choices is still very scarce. Most studies focus on either the role of tastes versus beliefs about future earnings [Arcidiacono, 2004, 2005; Attanasio and Kaufmann, forthcoming; Kaufmann, forthcoming; Wiswall and Zafar, forthcoming] or the role of incomplete information about the educational system [Ajayi, 2011; Avery and Hoxby, 2012; Bettinger et al., 2012; Lai et al., 2009; Hoxby and Turner, 2014]. Few exceptions are Arcidiacono et al. [2012]; Stinebrickner and Stinebrickner [2012, 2014], who analyze the role of beliefs about future performance on college major choices and drop-out decisions, and Dizon-Ross [2014], who measures the effect of informing parents about their children's academic performance on remedial investments among primary-school students in Malawi.

Second, economists have traditionally sought to explain biased self-views in the context of non-bayesian models by either postulating some form of biased processing of information [Daniel et al., 1998; Rabin and Schrag, 1999] or by assuming that decision makers form beliefs without adequate information [Akerlof and Dickens, 1982; Brunnermeier and Parker, 2005]. This paper provides empirical evidence that is instead consistent with the notion that biased beliefs can follow from a rational Bayesian updating process where agents have hedonic motives [Köszegi, 2006].[2]

Third, in spite of a bulging experimental literature on the widespread presence of overconfidence about own attributes (see Dunning et al. [2004]; Moore and Healy [2008] for reviews), there is very little evidence to date on how it affects real-world decisions. This paper takes some steps in this direction by linking measures of the subjective distribution of beliefs about own academic ability to administrative high-stake data on school application preferences and outcomes.

The remainder of this paper proceeds as follows: Section 2 presents a simple model of school choice that lays out the intuition behind the main effects of the intervention. Section 3 discusses the COMIPEMS school assignment system, the experimental design, and the main features of the sample that we use in the empirical analysis. Section 4 presents beliefs' baseline patterns as well as evidence on the updating process induced by the intervention while Section 5 presents the effects of the treatment on school-track choices and placement,

[2] There are also several Bayesian models in which agents acquire biases in certain measures. See Bénabou and Tirole [2002]; Carrillo and Mariotti [2000] for the context of hyperbolic discounting; Zabojnik [2004] in the context of costly learning with type-dependent costs and Rabin [1995] in the context of moral constraints and self-serving biases. These theories, however, often predict pessimistic rather than optimistic biases.

and Section 6 concludes.

2 Conceptual Framework

In this section, we lay out a simple framework that speaks to the interplay between beliefs about own personal traits and choices based on them so as to provide an intuition for the expected effects of the intervention under study. In the model, agents do not observe a given personal trait that is the main determinant of future individual performance in a task. Performance is also influenced by randomness, which implies that even if agents perfectly observed all their traits they would still face uncertainty about the future.

We build upon Köszegi [2006] by assuming that agents care about and manage their self-image. In the model, agents have beliefs about their own academic ability which enter into the utility function both directly through an hedonic component and indirectly through an instrumental component. While the instrumental component captures the role of beliefs in the ex-ante evaluation of different alternatives, the hedonic portion reflects the consumption value that people attach to envisioning themselves as capable of succeeding in an ability-sensitive task.

Two different activities occur in two different periods: in period $t = 0$ agents gather information about their own academic ability and in period $t = 1$ choices are made. Learning about oneself in period $t = 0$ is assumed to follow Bayes' rule. The informational intervention under study is modeled as an additional signal during period $t = 0$, which affects choices through its potential effect on agents' beliefs at the end of that same period.

2.1 Setup

Agents do not observe their true level of academic ability q but they know that it distributes $N(\mu_q, \sigma_q^2)$ in the population.[3] In period $t = 0$, agents are able to observe an arbitrary (and possible infinite) number of costless signals about their own q. Signals are denoted by $s_0^j = q + \epsilon_0^j$, where $\epsilon_0^j \sim N(0, \sigma_s^2)$ and $j = 0, 1, ...$ labels the signal's order of drawing. After each s_0^j, the agent decides whether to keep drawing signals or to stop and move on to $t = 1$. Priors about q at the beginning of $t = 0$ are equal to its population distribution $N(\mu_q, \sigma_q^2)$.

[3] One may think of q as the sum of innate cognitive skills and a second component, which reflects the previously accumulated stock of knowledge to date. As it will become clear later, we do not need to explicitly define these two ability components in the model due to the structure that we impose on the payoff function (see equation 3).

Let F_0^j denote the cumulative distribution of beliefs in $t = 0$ after j signals and assume it is derived from F_0^{j-1} in a Bayesian way.

In $t = 1$, agents choose among two types of schools $k_1 = \{\alpha, \beta\}$ based on their updated beliefs F_1 (normally distributed with mean μ_1) after drawing an optimal number of signals in $t = 0$. Type-α schools reflect more academic-oriented curricular programs whereas type-β schools capture programs which place an emphasis on teaching a profession or a craft (e.g. vocational education). After school choices are made, a random performance outcome is realized: $s_1 = q + \epsilon_1$ and $\epsilon_1 \sim N(0, \sigma_1^2)$.[4]

Agents' beliefs-based utility function is given by

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

The first term in (1) corresponds to the hedonic component of the utility function which directly depends on F_1. In particular, agents are assumed to derive utility from thinking that they are good enough to perform well in type-α schools. Let $\underline{s}$ denote the minimum performance required to succeed in those schools. Hedonic utility is 1 if the agent believes she would get a higher payoff from $k_1 = \alpha$ than from $k_1 = \beta$ and zero otherwise. Under risk neutrality, $k_1 = \alpha$ is a better choice whenever $\Pr(s_1 > \underline{s}) \geq \frac{1}{2}$. With the distribution of s_1 centered around μ_1, $\Pr(s_1 > \underline{s}) \geq \frac{1}{2}$ if and only if $\mu_1 > \underline{s}$. Thus,

$$u(F_1) = \begin{cases} 1 & \text{if } \mu_1 > \underline{s} \\ 0 & \text{otherwise.} \end{cases} \tag{2}$$

The instrumental component of the utility function (1) is given by

$$x(s_1, k_1) = \begin{cases} 0 & \text{if } k_1 = \beta, \\ 1 & \text{if } k_1 = \alpha \text{ and } s_1 > \underline{s}, \\ -1 & \text{if } k_1 = \alpha \text{ and } s_1 \leq \underline{s}. \end{cases} \tag{3}$$

The step-functions underlying expressions (2)-(3) imply that payoffs in type-β schools are fixed and independent of performance. This is a rather extreme characterization of the decision problem under study, which is however not crucial for the results that we discuss

[4] Notice that signals do not affect future academic performance - i.e., agents cannot increase their q by learning about themselves. This also rules-out the presence of indirect effects of signals on academic learning through, say, effort choices. In this sense, the process through which the stock of human capital evolves over time is not relevant for the mechanism that we highlight in the model.

in the next section. In general, it is required that (i) the hedonic component of the utility function is concave for higher values of mean beliefs and convex for lower values of mean beliefs,[5] and that (ii) there is a tight link between hedonic and instrumental utility - i.e. students derive utility not only from believing they are good but that they are *good enough to be successful in a type-α school*.[6]

2.2 Testable Predictions

Agents' incentives to learn about their academic ability depend on whether the expected gain from getting more accurate information about the likelihood of being successful in an academic-oriented school outweighs the potential damage that new signals may cause to the hedonic component of the utility function in (1). This trade-off generates an optimal stopping rule in $t = 0$ for students holding relatively high expectations about their academic ability, whereas students with relatively low expectations about themselves will keep drawing signals both for hedonic and instrumental reasons. As a consequence, some of the agents with $\mu_1 > \underline{s}$ in fact have $q \leq \underline{s}$ while those with $\mu_1 \leq \underline{s}$ know their type with certainty ($j \to \infty$).

Result 1 *Even though agents are Bayesian and thus unbiased in their beliefs about own academic ability, the beliefs in the decision period are on average upwardly biased relative to the underlying distribution of academic abilities. In particular, agents with relatively high beliefs are overconfident on average whereas those with relatively low beliefs are unbiased on average.*

To understand the intuition for Result 1, consider an agent with $\mu_0^j > \underline{s}$. Sampling more information about q only affects her utility if she ends up with lower mean beliefs (otherwise, both her hedonic utility and her school choices remain the same). Being forced to conclude that $q \leq \underline{s}$ decreases the agent's hedonic utility from one to zero. However, it also increases

[5]Psychological evidence seems consistent with the notion that people are more averse to self-relevant information when they happen to hold high beliefs about themselves than when they happen to hold low beliefs. For example, Stahlberg et al. [1999] report that subjects show greater interest in further information after receiving negative feedback than after receiving positive feedback. Importantly, this is only true for dimensions along which the subject has uncertain self-knowledge. In another study, Wyer and Frey [1983] show that subjects who had been given negative feedback on an IQ test recalled more (positive and negative) information from subsequent articles about IQ tests than did those given positive feedback. Thus, people with a lowered self-esteem seem to search more for information.

[6]Of course, a person's hedonic utility can also depend on the variance of beliefs. That is, she might feel better if she is more certain that she would succeed in a type-α school. The thrust of the model would be very similar if these more complicated considerations were included.

her instrumental utility because the agent now makes a better school choice (i.e. $k_1 = \beta$). For high values of μ_0^j, the losses from discovering that she is worse than expected outweigh the instrumental benefits generated by the additional signal, and hence the agent optimally chooses not to collect any more information about q in $t = 0$.

The intervention provides an additional (and unsolicited) signal $s_0^\star$ about q at some point during period 0. Let μ_0^j and $\mu_0^\star$ denote respectively agents' mean beliefs before and after receiving $s_0^\star$. Assuming for simplicity that the learning process outlined above has converged by the time of the intervention, those with $\mu_0^j > \underline{s}$ choose $k_1 = \alpha$ while those with $\mu_0^j \leq \underline{s}$ choose $k_1 = \beta$. Absent the treatment, (upwardly) biased beliefs would systematically translate into biased school choices.

Result 2 *The share of agents who choose academic-oriented schools is greater than the share of agents in the population whose academic ability is high enough to be successful therein.*

On average, delivering $s_0^\star$ may better align school choices with performance in the mock test, though this effect depends on whether $s_0^\star \lesseqgtr \mu_0^j$. If $\mu_0^j < s_0^\star$, the treatment-induced shift in beliefs to the right translates into a change in school choices from $k_1 = \beta$ to $k_1 = \alpha$ for the (marginal) agents with $\mu_0^j \leq \underline{s}$ and $\mu_0^\star > \underline{s}$. Those agents are less likely to draw additional signals after $s_0^\star$: sampling more information about q may in fact decrease with positive probability both hedonic and instrumental utility. Among students with $s_0^\star < \mu_0^j$, those with $\mu_0^j > \underline{s}$ and $\mu_0^\star \leq \underline{s}$ have an incentive to search for additional signals until they get a high enough draw that undoes the negative effect of the treatment on posteriors. The effect of the treatment on school choices for those who are induced to update their beliefs to the left after receiving the performance feedback is thus confounded by the simultaneous increase in the incentive to search for further signals.

Result 3 *Agents receiving a signal which is below their mean priors are more likely to keep searching for more signals and hence are less likely to alter their school choices in response to the treatment. On the other hand, agents who receive a signal that is above their mean priors are likely to stop gathering signals about their academic ability. As a result, they are more prone to shift their preferences toward academic-oriented schools.*

Notice that, in the model, utility and choices are defined at the school type level. Thus, it is implicitly assumed that q equally affects the payoffs of all schooling alternatives within the same type and, accordingly, the change in beliefs induced by the signal is not expected to have any differential effect on choices within those broadly defined school types.

3 Context, Design and Data

3.1 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).[7] 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 first track includes traditional schools more focused on preparing the student with general knowledge to continue studying at a university. The second track partly covers the curriculum of general education programs but it also features additional courses allowing students 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 graduate from programs in general or technical education are automatically eligible to pursue a university degree. 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.[8] The most demanded institutions within the COMIPEMS assignment process are UNAM and IPN which sponsor educational options from the general and technical tracks, respectively. The high demand for these options is in part due to the fact these schools are university-affiliated. In particular, students that successfully graduate from a UNAM's high school are guaranteed direct

[7]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 additionally require a 7.0 cumulative grade point average (GPA) in junior high school.

[8]In 2010, almost half of the seats offered by the COMIPEMS corresponded to the general education track. About a third of the seats were from schools in the technical track while the remaining 20% corresponded to vocational programs.

admission into university.[9]

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 detailed calendar of all the activities related to the application process, a list of the schools available in the admission process as well as their basic characteristics (location, track, and specialties, if applicable). The COMIPEMS' website provides additional information such as past cut-off scores for each school-specialty in the previous three years. 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.[10] 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.

Unassigned students can search for a school by independently applying to COMIPEMS's schools with available seats at the end of the process or to schools with open admissions outside the system. Notice that placed students are only matched with one schooling alternative. If, for some reason, placed students are not happy with their assignment, they can only search for another option by independently applying to a school just like unplaced students do. By construction, the residual options available at the end of the allocation process are not included in the preference portfolios submitted by unplaced (or unhappy) students, with the exception of the few vacant seats that placed students may free up if they were not to enroll in their assigned option. Thus, the system itself eliminates any strategic incentive to remain unplaced within the centralized allocation process.

The COMIPEMS assignment mechanism makes students' rankings fully informative about their underlying preferences over the different schooling alternatives made available

[9]In our sample, 73% of the applicants that list a general education school as their highest ranked alternative choose an UNAM school while IPN is the first option for 54% of the students listing a technical program at the top.

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

through the centralized process. First, the matching mechanism used is equivalent to the deferred-acceptance algorithm originally proposed by Gale and Shapley [1962]. Second, even though students are allowed to rank up to twenty options, under 2% of the students in our sample fill up the entire preference sheet. These two features together imply that the COMIPEMS assignment mechanism is strategy-proof, since there is no penalty for ranking schools according to true preference and the size of the portfolio does not seem to constrain students' choices. In other words, admission probabilities to each school do not play a role when choosing an application portfolio and thus there is no strategic disadvantage in choosing a school for which the student has a low ex-ante probability of admission.

3.2 The Intervention

The intervention provides students with relevant information about their own academic potential in order to observe its effects on beliefs and, ultimately, on choices. The main challenge we face is identifying a performance measure that is highly correlated with academic ability and that is expressed in a scale that is familiar to ninth graders. Prospective applicants can be assumed to be sufficiently familiar with the structure and scale of the COMIPEMS exam, providing us with a suitable environment to elicit subjective expectations about academic 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. Throughout the paper, the group of students who took the mock exam without receiving any feedback on performance will be referred to as the placebo group. 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, which we denote as the control group. We further elicit subjective distributions about exam performance both before and after the mock exam. Notice the link to the model discussed in Section 2. By deriving mean expected performance in the exam, we are actually measuring the expected value of academic ability (q) at the individual level, provided that randomness in performance is mean-zero. Whenever we provide the score in the mock exam, the treatment group receives a signal that does include q as well as the realization of randomness in that particular measurement.

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 with no negative marking considered for wrong answers. It covers a wide range of subjects corresponding to the public middle school curriculum (Spanish, mathematics, and social and natural sciences) as well as mathematical and verbal aptitude sections. It is natural to assume, as we do, that due to the exam's contents, performance in the mock exam (and/or the real admission exam) is much more informative about students' future performance in the academic track than in other tracks.

Since the delivery of the signal took place in February, thirteen questions in the mock exam related to the curriculum covered between March and June of the last grade of middle school were excluded from the evaluation.[11] 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 other two bars show the average mock exam score of his classmates and the average score in the actual exam among the universe of applicants during the 2013 round, respectively. We do not attempt to back-out the role of each of these information nudges on students' behavior. Instead, by simultaneously providing applicants with these three pieces of information we intend to better contextualize their performance outcomes in the exam's scale. Thus, the treatment effect is attributed to the joint delivery of these data. Figure 1 shows a sample of the sheets used to deliver information to the applicants. For each school, sheets with the 2013 average score and the group average performance in the mock exam were pre-printed. During the delivery, the surveyors plotted the bar corresponding to the individual's score in the same sheet.[12]

Students are informed about the administration of the mock exam a couple of days in advance, but they are not given an exact date for the examination. This strategy intends to reduce biases due to the unexpected application of the exam without generating absenteeism. A potential concern is that the exam we deliver is low stakes and, as such, is not a good reflection of students' academic performance. We try to minimize this issue by i) informing, not only the students, but the school and the parents about the administration of the mock exam while making them all aware of the benefits of additional practice, ii) requesting the principal to send the person in charge of the discipline in the school and/or a teacher to proctor the exam. Given the hierarchical nature of Mexican schools, particularly in basic schooling levels, we believe that this last feature induced students to take the mock exam

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

[12] 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.

seriously.

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 academic potential.[13] 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 (more than 30).

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 2. Treatment assignment is randomized within each of the 12 resulting strata and at the school level. As a result, 44 schools are assigned to the treatment group in which we administer the mock-exam and provide face-to-face feedback on performance, 46 schools are assigned to the placebo group in which we administer the mock exam without informing about the test results and 28 schools are assigned to a control group in which we only collect follow up data. In each school in the sample, we randomly pick one section among the entire ninth grade cohort and interview the universe of students therein.

3.3 Subjective Expectations

The baseline survey and the mock exam took place on a rolling frame basis. The baseline survey was conducted over the last two weeks of January 2014. Two or three days after the 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 3 depicts the timing of the

[13]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.

application process and of the activities related to the intervention.

As mentioned above, the registration process requires students to fill out a socio-demographic survey which collects detailed individual information. We thus design our baseline and follow up surveys so as to complement the information contained 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, we 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 [Delavande et al., 2011]. 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 and 20 beans means that the students believes the event will occur for sure.[14]

In addition to the 20 beans, students were provided with a card divided in 6 discrete intervals of the score in the admission exam (see Table 1). Surveyors then elicit students' expected performance in the COMIPEMS exam by asking them to allocate the beans across the 6 intervals so as to represent the chances of getting a score in each bin.[15]

3.4 Sample Description

Among the 90 ninth-grade sections assigned to take the mock-exam, we interviewed 3001 students in the baseline survey, only 2790 were present the day of the exam, and a subset of 2544 were additionally present in the follow up survey. The overall attrition rate of 15% is orthogonal to treatment assignment. After augmenting this sample with 912 students who

[14] 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.

[15] During the pilot activities, we tested two versions of this exercise with 4 and 6 intervals and 10 and 20 beans, correspondingly. Students seemed to have no problems manipulating 20 beans across 6 intervals and hence we decided to keep this version for the final instruments.

belong to the control group, we end up with 3,456 observations. Of those, 3,100 (90%) students were identified in the official data collected within the COMIPEMS system allowing us to match our survey (GPA in middle-school, highest educational level aspired, study habits, among others) and mock exam records with detailed socio-demographic characteristics from the registration form (gender, age, household income, parental education and occupation, personality traits, among others). The administrative records also provided us with the full ranked list of schooling options requested by each student and placement outcomes (including score in the admission exam).[16]

Table 2 provides basic descriptive statistics and a balancing test of the randomization for the main variables that we employ in the empirical analysis. Consistent with the random treatment assignment, no significant differences are detected across groups. By the time baseline data was collected, about 30% of the students in our sample had 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 admission 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 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 realized 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 score in the placement exam is balanced, not only across treatment and placebo groups, but also relative to the control group.

[16]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 participation 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 our sample.

4 Beliefs

We first characterize the beliefs measured by our survey data. In general, the individual beliefs distributions that result from the elicitation process of subjective expectations about test performance (see Section 3) seem well-behaved. Only 6% of the respondents concentrate the 20 beans in one interval. The distribution of mean beliefs 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. This characterization thus confirms that the empirical belief distributions at the individual level are very much in line with a normal distribution so that the mean and standard deviation are sufficient summary statistics of the empirical densities.

4.1 Baseline Trends

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 assess the extent to which beliefs are biased in our sample.[17] The top panel of Figure 4 plots the empirical CDF of mean beliefs in the baseline as well as of the scores in the mock exam and in the COMIPEMS placement exam, respectively, for the sample of 2,293 students in the treatment and placebo groups. The distribution of beliefs clearly dominates both distributions of exam scores, although this pattern tends to fade out for high values of the score support. Notice that the distribution of the placement score is to the right of the distribution of the mock exam. 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 (end of January-early February), students are far from concluding their preparation for the exam. Relative to the mock exam, 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 shift upwards.

The support of the gap between expected and realized performance (as measured by

[17]The linear correlation between the two test performance measures remains quite high: 0.82; whereas the cumulative Grade Point Average (GPA) in middle school features a linear correlation of just 0.45 with both test performance measures.

the mock exam or the placement exam score) is quite large, fluctuating between -57 to 68 points. The median student overestimates his test performance in a range comprised between 12 and 15 points depending on the performance measure used. The bottom panel of Figure 4 displays the empirical relationship between the share of students with beliefs above their actual performance and mean prior beliefs at baseline. Irrespective of the performance measure used, there is a clear increasing relationship between the share of people with an upward bias in beliefs and their initial self perceptions.

These baseline trends are largely consistent with Result 1 of the model presented in Section 2. Beliefs about performance in the test are on average biased upwards, and more so for students with relatively high mean priors whereas those with relatively low priors seem on average unbiased in their predictions.

4.2 Beliefs' Updating

We first look at the effects of the intervention on students' expectations about their performance in the test. The top panel of Figure 5 depicts the CDF of the gaps between mean posteriors and placement score for the treatment, placebo and control groups. Relative to the placebo and the control groups, the gap between expected and realized performance is narrower among students in the treatment group. More importantly, the distributions of the gap for the placebo and 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 performance. The corresponding 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 control and by 4.33 points (std.err.=0.53) relative to the placebo group. There are no statistically significant differences in the absolute gap between the placebo and control groups.[18]

These patterns are confirmed by OLS estimates reported in Column 1 of Table 3, in which we compare the average absolute value of the gap between expected performance and the realized score in the mock exam across the treatment and the placebo samples. On average, the intervention induces students' to close their expectation gaps by 6.6 points, which is about a third of the mean in the placebo group. Column 2 further shows that providing students with information on their score in the mock exam reduces the average standard deviation of the perceived academic ability distributions by roughly 15% relative

[18]Results are available upon request.

to the average in the placebo group.

We next explore whether receiving positive or negative feedback relative to expected performance has different effects on beliefs' updating patterns. We create an indicator variable which takes the value of one if the score in the mock exam is greater than mean beliefs in the baseline (i.e. right-updaters) and zero otherwise (i.e. left-updaters), and allow our treatment indicator to vary across these two categories. Column 3 in Table 3 shows that the treatment generates a greater adjustment in absolute terms in the mean belief distribution observed among left-updaters. Relative to their counterparts in the placebo group, these students adjust downward their mean posteriors by 9.9 points whereas right-updaters only adjust upward their mean posterior by 2.8 points. The estimates reported in column 4 reveals that the treatment induces students in both groups to shrink the dispersion of their belief distributions, with a more pronounced effect for right-updaters (although we cannot reject equality of the two coefficients: p-value=0.12).

In our sample, students receiving negative feedback relative to their initial beliefs respond more than those receiving positive feedback. This seemingly asymmetric response is, however, in line with the Bayesian setting that we impose in the model discussed in Section 2 since students who are provided with a signal that is below their expectations are on average further away from their actual performance. The bottom panel of Figure 5 illustrates this point by plotting the average gaps between posterior mean beliefs and mock exam scores for the placebo and treatment groups as a function of the initial gap. The clock-wise rotation of the solid line shows that the intervention symmetrically tilts expectations towards realized performance for both right-updaters and left-updaters, and the empirical density reveals that the average gap among right-updaters in the baseline is much smaller.[19]

5 School Choices

In order to take the theoretical predictions to empirical scrutiny, we first need to clarify how we identify in the data the academic-oriented schools and the associated students' demands for school types. In what follows, we consider as main dependent variable the average share of schooling options that belong to the general education track in the COMIPEMS application portfolio of each applicant. Schools that belong to the technical and the vocational tracks are considered less oriented to academic training. The advantage of this measure is that it

[19]These updating patterns discard the presence of a confirmatory bias or a "good news-bad news" effect in our setting [Eil and Rao, 2011], which would imply differential processing of the information received depending on the location of the signal relative to the distribution of prior beliefs.

is objectively based on the curricular content of each high-school program made available through the centralized admission system. While there may be idiosyncratic variations across schools within the same track on the extent to which students' academic performance is rewarded, those are likely to being averaged out in our definition.[20]

5.1 Average Treatment Effects

We first restrict the analysis to the control group and regress the average share of schooling options from the academic-oriented track on the expected mean prior and the score in the placement exam. Column 1 of Table 4 reports the associated OLS estimates, where both explanatory variables have been normalized to zero mean and unitary variance in order to simplify the reading of the magnitudes of the estimated coefficients. The results show that mean beliefs have a positive and significant slope in the demand function for academic options. A one standard deviation increase in expected academic performance is associated with an average increase in the share of requested academic options of about 4.5 percentage points (the point estimate is 0.023 with a baseline value of 0.514). On the contrary, the estimated coefficient for academic performance, as measured by the score in the placement exam, is small and statistically insignificant.

When we repeat this exercise using the actual assignment in a given school track as the dependent variable, the estimates reveal a similar pattern. Column 2 in Table 4 shows an even greater effect of expected vis-a-vis realized performance: a one standard deviation in mean beliefs generates an increase of about 7.5 percentage points in the probability of being placed in the academic track. The lack of a significant correlation between placement probability in the academic track and the actual score highlights that the bias in perceptions reflected in the submitted preference lists translates into biased outcomes. Students who think they are good enough to go to academic options demand them relatively more and are thus more likely to get placed in one of such options.

Although merely descriptive, these trends lend support to the prediction of the model on the relationship between students' biased perceptions about academic ability and schooling decisions (see Result 2). The close link between school choices and final outcomes further sheds some light on the potential role of inaccurate perceptions about own academic ability

[20]Our data allow us to check for the presence of other effects of the intervention along a variety of school margins which vary within educational modality (e.g. teachers and school characteristics, type of sponsoring educational institution, degree of competition/selectivity in the assignment system, etc). None of those application outcomes seem to systematically respond to the treatment (the results are available upon request).

behind the mismatch between students' actual skills and educational trajectories.

We next evaluate whether and how high school track choices and placement outcomes change as a result of the treatment. The OLS estimates reported in Column 3 of Table 4 show that the intervention increased the sensitivity of students' demand for academic-oriented programs with respect to their actual academic performance, as measured by the mock exam score. Compared to students in the placebo group, a one standard deviation in the mock exam among treated students is associated with an increase in the share of requested academic options of 4.8 percentage points. In sum, the treatment better aligned academic performance with students' choices.

Results in column 4 show that a similar pattern emerges for the probability of assignment in a school that belongs to the academic-oriented track. Since the treatment did not have an indirect effect on performance in the actual placement exam (see Table 2), this result suggests that the observed effects on placement outcomes are mostly driven by the underlying changes in choices induced by the intervention.

We further assess the robustness of these findings by using two alternative measures of academic performance: the score in the placement exam and the cumulative Grade Point Average (GPA) at the end of middle school. The corresponding OLS estimates are reported in Table 5 and they are remarkably consistent across all specifications in terms of signs, magnitudes, and precision.

5.2 Heterogeneous Treatment Effects

The conceptual framework presented in Section 2 provides us with a rationale for the presence of differential impacts of the treatment depending on the implied direction of the updating (see Result 3). Accordingly, right-updaters are more prone to register a change in their choices since the positive feedback they receive makes them more likely to stop searching for additional signals. In turn, left-updaters are expected to undo the effects of the treatment on beliefs by searching for additional signals before submitting their preferences. The estimates reported in column 1 of Table 6 are very much in line with these predictions. Among the group of right-updaters, the treatment is associated with an average increase of 8.3 in the share of requested academic options. This is a large effect as it corresponds to 18 percent of the average in the placebo group. In turn, the large reductions in beliefs recorded among left-updaters as a consequence of the treatment (see column 3 in Table 3) do not seem to be long-lasting. The treatment effect on school choices estimated for these students is very small and statistically insignificant.

Due to budget and logistic limitations, we could not collect data on beliefs after the follow up survey in order to directly check that the treatment effect fades away among left-updaters while it persists among right-updaters. However, we can still provide auxiliary evidence on this prediction by taking advantage of the exogenous variation between the delivery of the mock exam score and the preference registration. 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 of origin, the number of days between the delivery of the score in the mock exam and the registration of school preferences is orthogonal to any determinant of school choice. Column 2 in Table 6 shows that left-updaters who only had one day between the delivery of the score in the mock exam and the registration of their schooling preferences in the assignment system do in fact respond to the treatment. These students had no time to search for additional signals after treatment delivery and thus they effectively reduce their share of academic options by 17.7 - i.e. 34 percent of the average in the placebo group. The treatment effect among this sub-sample of left-updaters is twice that of right-updaters (see column 1), which is entirely consistent with the larger response in mean beliefs induced by the information feedback (see columns 3-4 in Table 3).

We next explore the presence of non-linear treatment effects for right-updaters depending on the level of mean prior beliefs - as before, we normalize this variable in order to facilitate the interpretation of the associated interaction effect with the treatment. The estimates reported in column 3 provide evidence in favor of a convex relationship between perceptions about academic ability and the school choice responses to the intervention. Conditional on receiving a positive signal, a one standard deviation increase in mean priors implies an additional treatment effect of 4 points in the share of academic options requested.

We finally study the relationship between the share of academic options and students' mean beliefs after the intervention where the latter are instrumented using the random treatment assignment. 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 choices, (ii) students' beliefs respond to the treatment and (iii) the treatment affects beliefs in the same direction [Imbens and Angrist, 1994].[21] The corresponding local average

[21] One potential violation of the exclusion restriction may occur whenever the treatment triggers subsequent behavioral responses (e.g. study effort) that are correlated with future performance and that students incorporate in their current school choices. However, the above-mentioned result that the treatment is not correlated with performance in the subsequent placement exam (see Table 2) supports Assumption (i). Furthermore, the evidence reported in Table 3 confirms that Assumption (ii) holds. Assumption (iii) is not directly verifiable, but it is likely to hold in our setting provided that we split the sample between left-updaters and right-updaters. In fact, the resulting CDF of beliefs in the treatment and placebo groups do not cross with each other (the results are available upon request).

treatment effect (LATE) parameter allows us to quantify the average sensitivity of the individual demand functions for academic options with respect to treatment-induced changes in students' current perceptions about their academic ability.

The last column of Table 6 presents the IV-LATE results only for the sub-sample of right-updaters, who are the ones who in fact change their choices after the treatment. In this group, a one point increase in mean beliefs generated by the intervention is associated with an increase in the share of requested academic-options of 8 percentage points (the point estimate is 0.037 with a baseline value of 0.46). This is a very large effect, which is not directly comparable to the corresponding OLS effect of beliefs on school choices reported in column 1 of Table 4 (the latter is measured in standard deviation units), for two reasons. First, the LATE is usually identified for the sub-population of "compliers" (in this setting, the portion of right-updaters who alter their school track choices as a result of the beliefs' updating induced by the treatment), who may be different from the overall population of applicants in our sample along several dimensions. Second, the observed school choice responses are likely to reflect the specific nature of the signal provided with the intervention, which, as mentioned in Section 3, is geared towards revealing salient information about future performance in the academic track.

6 Conclusion

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 own 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. We have documented that the intervention helps students align expectations about their own academic skills with actual academic performance. The treatment also shifted track choices towards a positive assortative matching equilibrium in which better performers tend to choose (and be assigned to) more academic-oriented school. 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 academic performance did not alter their school-track decisions. On the contrary, those who receive positive feedback chose a significantly higher share of schools that belonged to the academic-oriented track.

The evidence presented is consistent with the presence of self-image concerns in an otherwise standard Bayesian framework in which agents learn about their own academic ability. The distribution of beliefs captured in the follow up survey is likely to keep changing for students who receive a bad signal, as the latter is detrimental for their egos. Thus, they keep on searching for additional information 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.

An emerging series of studies has been trying to develop interventions that help reduce inertia, change routine and/or simplify choices by providing students or parents with information about application procedures and costs, returns to education, and financial aid opportunities (see Lavecchia et al. [2014] for a recent review). Though these interventions have proved to have great potential to improve school choices and outcomes, our paper shows that that access to information may have unexpected asymmetric responses. In our setting, the information we provide to 9th graders is only incorporated into actions for a small group of students receiving good news about their academic performance. About 80 percent of the students did not take into account the information in the way the policy maker would have expected and this seems to stem from the fact that people care about their self-esteem. Students who are told they are not good enough to continue onto academic-oriented high schools keep searching for signals about their performance and thereby undo the effects of the information provided by the intervention on actions despite its initial effects on beliefs.

Our analysis includes some limitations. For instance, longitudinal information on students' beliefs after the treatment would allow us to directly test the key implication of the learning model on the presence of an asymmetric response to the provision of performance feedback. Yet, further evidence which employs exogenous 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.

While in Section 5 we speculate that the observed effects of the treatment may be persistent along the students' educational paths, we currently lack information on later outcomes. As we write, the students who took part to this intervention are just starting high school.

In future work, we plan to investigate the potential welfare consequences of the treatment in terms of high-school dropout and academic performance.

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Figures

Figure 1: The Informational Treatment: Sample Performance Delivery Sheet

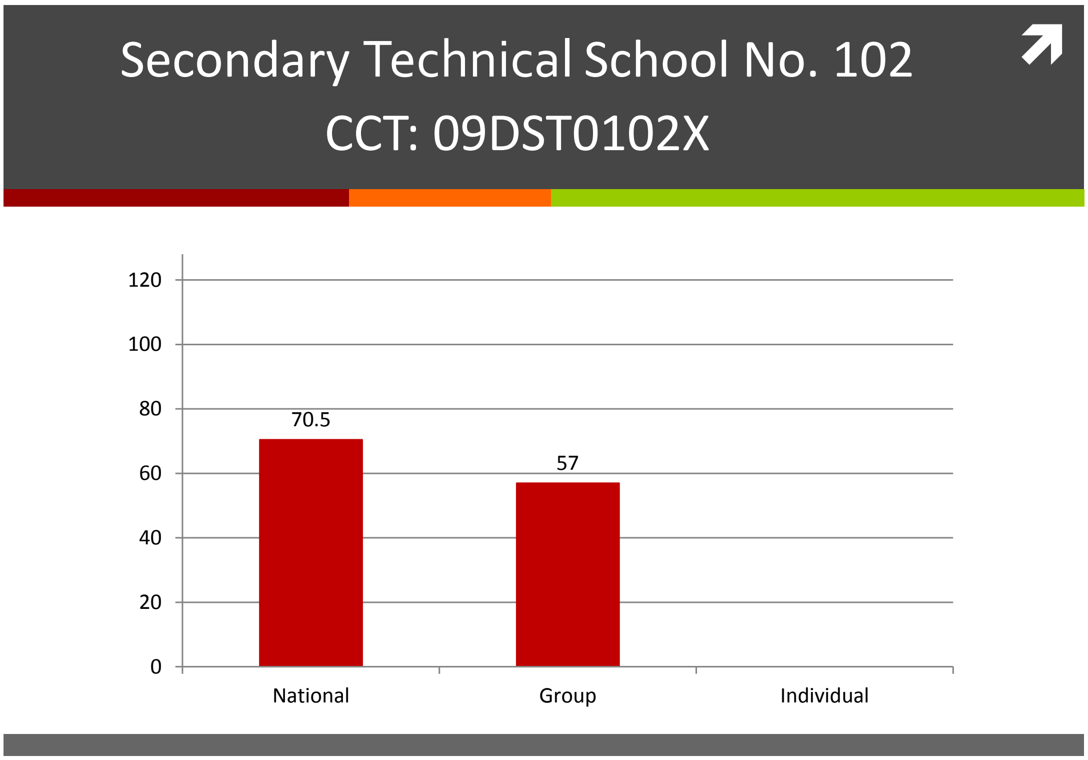


Figure 2: The Schools in the Sample within the Metropolitan Area of Mexico City

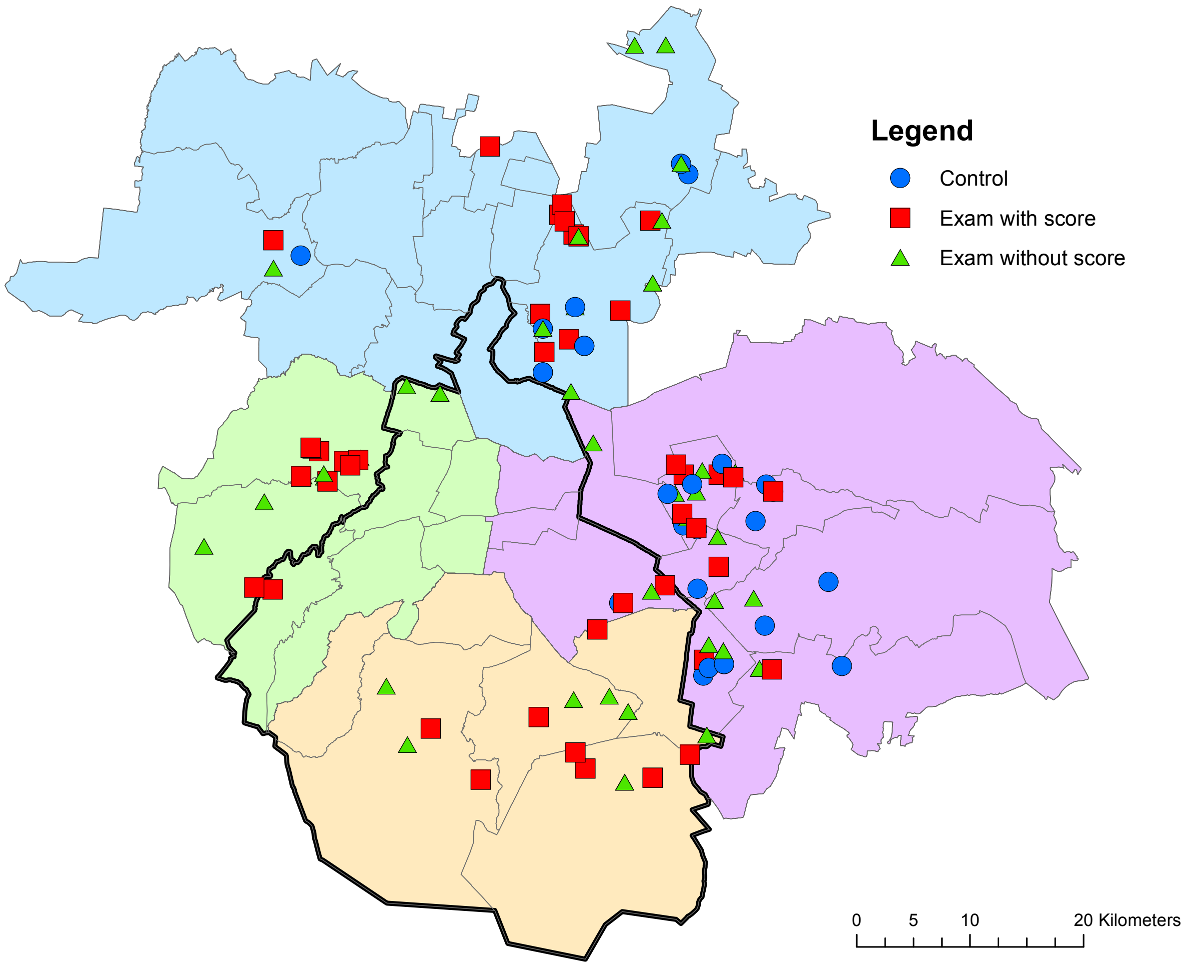


Figure 3: The COMIPEMS Process and the Intervention: Timeline of Events

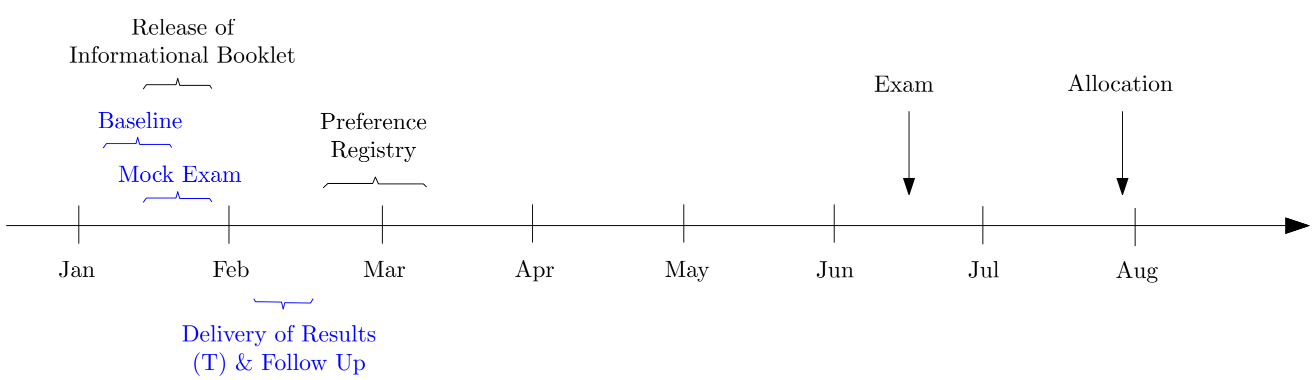
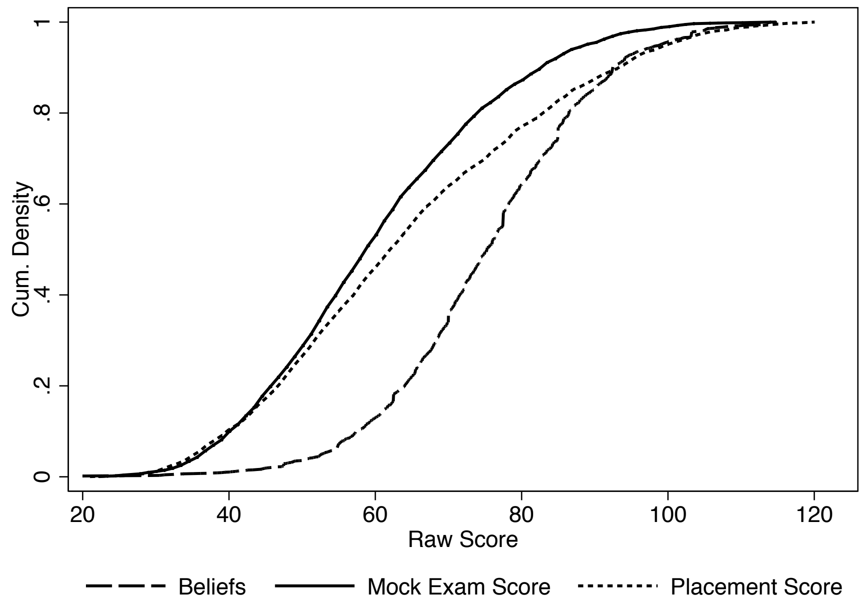
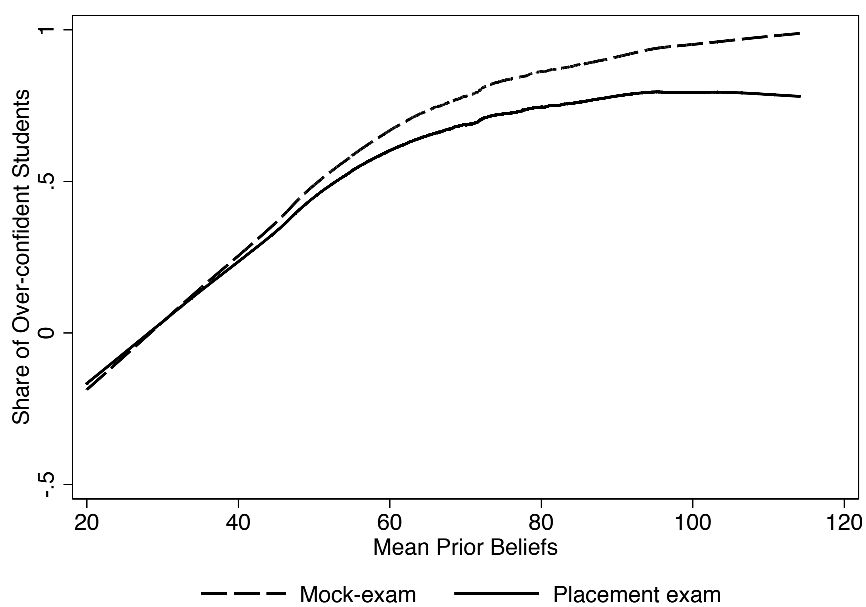


Figure 4: Baseline Beliefs and Realized Performance



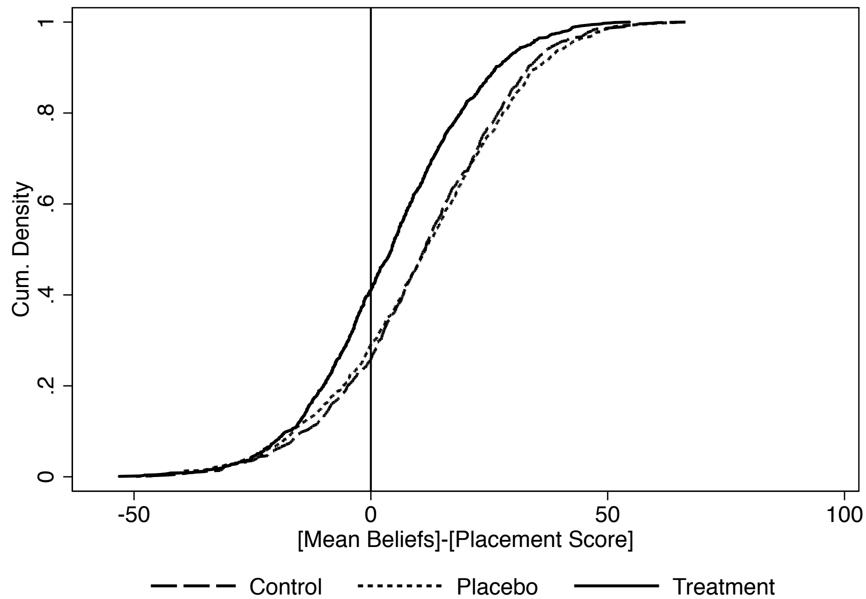
(a) CDF of Mean Belief Priors and Test Performances



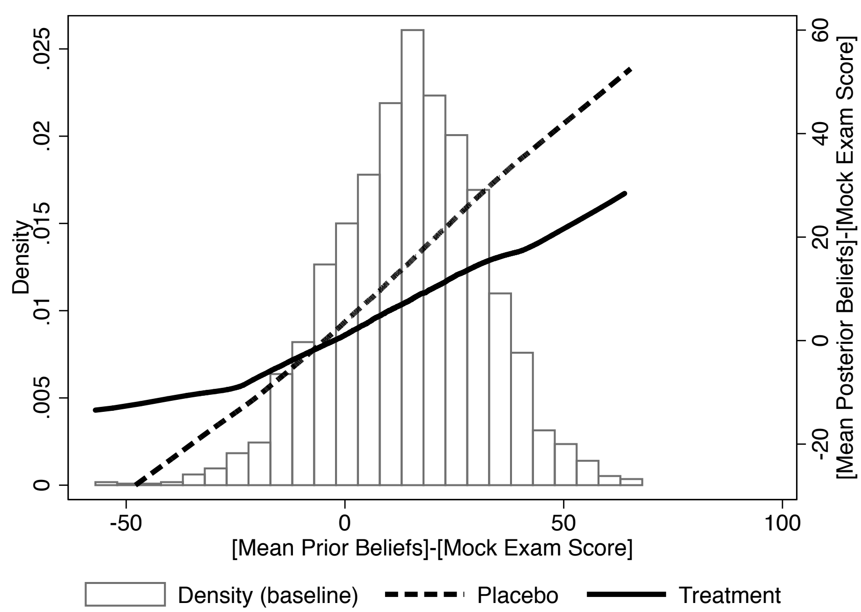
(b) Overconfidence and Mean Prior Beliefs

Note: The top panel shows empirical cumulative densities of mean beliefs and realized scores. The bottom panel shows non-parametric estimates (based on locally weighted regression smoothers) of the effect of mean prior beliefs on the probability that the value of those beliefs is greater than the realized value of the score in either the mock exam or the placement exam.

Figure 5: Beliefs' Updating



(a) Decomposition of Treatment Effects on Expectation Gaps



(b) Changes in Expectation Gaps with respect to Baseline Values

NOTE: The top panel shows empirical cumulative densities of the difference between expected and realized performance in the placement exam. The bottom panel shows the empirical density of the gap between expected and realized performance (left y-axis) as well as overlaid non-parametric estimates - based on locally weighted regression smoothers - of the effect of the gap between expected and realized performance in the baseline on the gap between expected and realized in the follow-up (right y-axis).

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 perform an exercise. Suppose that you take the COMIPEMS exam today, which has a maximum possible score of 128 and a minimum possible score of zero. How sure are you that your score would be...*

Table 2: Summary Statistics and Randomization Check

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

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 Expectations about Exam Performance

Sample	Treatment & Placebo			
Dependent Variable	Abs(Gap)	SD Beliefs	Mean Beliefs	SD Beliefs
	(1)	(2)	(3)	(4)
Treatment	-6.596***	-2.626***		
	(0.642)	(0.420)		
TreatXRight-Updater			2.786**	-3.623***
			(1.317)	(0.766)
TreatXLeft-Updater			-9.854***	-2.423***
			(0.915)	(0.428)
Right-Updater			-14.533***	3.104***
			(1.135)	(0.601)
Mean DepVar in Placebo	19.59	17.45	75.61	17.45
Number of Observations	2293	2293	2293	2293
R-squared	0.290	0.083	0.346	0.095
Number of Clusters	90	90	90	90

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%.
OLS estimates, standard errors clustered at the school level are reported in parenthesis. Sample of ninth graders in schools that belong to the treated and the placebo group. All specifications include a set of dummy variables which correspond to the randomization strata and the following set of individual and school characteristics (see Table 2 for details): mock exam score, 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: Preferences for and Placement in the Academic Track

Sample	Control		Treatment & Placebo	
Dependent Variable	Portfolio Share	Placement Dummy	Portfolio Share	Placement Dummy
	(1)	(2)	(3)	(4)
Mean Beliefs (z-score)	0.023*	0.032**		
	(0.013)	(0.015)		
Placement Exam Score (z-score)	-0.007	0.019		
	(0.011)	(0.021)		
TreatXMock Exam Score			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/Placebo	0.514	0.426	0.518	0.425
Number of Observations	786	786	2293	2293
R-squared	0.084	0.049	0.087	0.075
Number of Clusters	28	28	90	90

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%.
OLS estimates, standard errors clustered at the school level are reported in parenthesis. Sample of ninth graders in schools that belong to the control group (columns 1-2) and treated and placebo groups (columns 3-4). All specifications include a set of dummy variables which correspond to the randomization strata and the following set of individual and school characteristics (see Table 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: Treatment Impacts on School Choices. Alternative Performance Measures

Sample	Treatment & Placebo			
Dependent Variable	Portfolio Share	Placement Dummy	Portfolio Share	Placement Dummy
	(1)	(2)	(3)	(4)
TreatXPlacement Exam Score	0.031**	0.053*		
	(0.013)	(0.028)		
Placement Exam 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	-0.019	0.012	-0.020
	(0.017)	(0.024)	(0.017)	(0.025)
Mean DepVar in Control	0.520	0.433	0.519	0.428
Number of Observations	2253	2253	2276	2276
R-squared	0.086	0.081	0.083	0.071
Number of Clusters	90	90	90	90

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%.
OLS estimates, standard errors clustered at the school level are reported in parenthesis. Sample of ninth graders in schools that belong to the treated and placebo groups. All specifications include a set of dummy variables which correspond to the randomization strata and the following set of individual and school characteristics (see Table 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	Portfolio Share of Academic Schools			
Sample	All OLS (1)	Left-Updaters OLS (2)	Right-Updaters OLS (3)	Right-Updaters IV-LATE (4)
TreatXRight-Updater	0.083*** (0.029)			
TreatXLeft-Updater	-0.005 (0.017)			
Right-Updater	-0.057** (0.022)			
Treatment		-0.003 (0.017)	0.091*** (0.029)	
TreatXShort Delay		-0.177*** (0.060)		
Short Delay		0.083* (0.046)		
TreatXMeanPrior			0.043* (0.024)	
Mean Prior (z-score)			-0.001 (0.029)	
Mean Beliefs				0.037** (0.017)
Mean DepVar in Placebo	0.51	0.52	0.46	0.46
Number of Obs	2293	1851	441	441
R-squared/Cragg-Donald F	0.086	0.083	0.169	4.54
Number of Clusters	90	90	84	84

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%.
OLS (columns 1-3) and 2SLS (column 4) estimates, standard errors clustered at the school level are reported in parenthesis. Sample of ninth graders in schools that belong to the treated and placebo groups. Left-(right)updaters are defined as those with mean baseline beliefs that are lower (higher) than the realized value of the score in the mock exam. All specifications include a set of dummy variables which correspond to the randomization strata and the following set of 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.