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Protecting unsophisticated applicants in school choice through information disclosure

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Abstract: Unsophisticated applicants can be at a disadvantage under manipulable and hence strategically demanding school choice mechanisms. Disclosing information on applications in previous admission periods makes it easier to assess the chances of being admitted at a particular school, and hence may level the playing field between applicants who differ in their cognitive ability. We test this conjecture experimentally for the widely used Boston mechanism. Results show that, absent this information, there exists a substantial gap between subjects of higher and lower cognitive ability, resulting in significant differences in payoffs, and ability segregation across schools. The treatment is effective in improving applicants' strategic performance. However, because both lower and higher ability subjects improve when they have information about past demands, the gap between the two groups shrinks only marginally, and the instrument fails at levelling the playing field.

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1 Introduction

Many school districts operate school choice programs that aim to provide all students with equal access to good schools. Applicants are typically asked to report their preferences over multiple schools and a school choice mechanism is used to determine the eventual allocation of school seats.

If this mechanism is manipulable—i.e. if it may be advantageous for applicants to misrepresent their preferences—families that are less equipped to asses the consequences of various potential manipulations may find themselves at a disadvantage. In this paper, we study experimentally whether one can protect these applicants by providing additional information.

The argument that unsophisticated applicants are disadvantaged under the widely used but manipulable Boston mechanism (henceforth BOS, also known as Immediate Acceptance), has been at the core of many recent reforms that replaced it with strategy-proof mechanisms and in particular the Deferred Acceptance mechanism (henceforth DA).[1] For example, in 2005 Boston abandoned its old mechanism that still bears the city's name after observing that under BOS, "the need to strategize provides an advantage to families who have the time, resources and knowledge to conduct the necessary research" [Pathak and Sönmez, 2008].[2]

On the other hand, BOS may improve upon DA according to various ex-ante efficiency and welfare criteria [Abdulkadiroğlu et al., 2011; Miralles, 2009]. Hence, it would seem preferable not to abandon BOS, but to protect unsophisticated applicants *within* the mechanism by making it easier for them to identify equilibrium strategies.

Intuitively, BOS is able to generate these welfare gains because, in equilibrium, applicants would take into account not only how valuable a school is to them but also how likely they are to be admitted if they apply. Hence only students with a sufficiently high valuation would be willing to apply at (highly) oversubscribed schools, while others would demote these schools in their submitted preference lists. To help applicants identify schools that are likely to be oversubscribed, a school council might decide to disclose information on the

[1]For practical reasons many districts constrain the length of applicants' preference lists and apply mechanism that would be strategy-proof only absent this constraint. Nevertheless, Pathak and Sönmez [2013] confirm that a number of such constrained mechanisms—in particular constrained DA—are less manipulable then similarly constrained BOS.

[2]Similarly concerned over its strategic complexity, England banned the use of BOS in 2007 and many local authorities moved to DA. For an overview of recent reforms, see Pathak and Sönmez [2013].

number of applicants at various schools in previous years. Assuming that the distribution of applicants' preferences over the years is sufficiently stable, such information should be informative of the expected number of applicants in the current year and hence help applicants to coordinate on an equilibrium. If successful, this would eliminate the gap between formerly 'sophisticated' and 'naive' applicants while also allowing to reap the improvements in ex-ante welfare that BOS can in theory provide.

In a parallel paper [Basteck and Mantovani, 2016], we report experimental evidence confirming that—absent detailed information on previous applications—BOS increases the gap between subjects of different cognitive ability compared to DA. Subjects of higher ability fare better than their peers of lower ability: because they are less able to identify optimal strategies in BOS, the latter earn significantly less and are over-represented at the worst school, resulting in ability segregation across schools. Nevertheless, BOS is able to generate significant welfare improvements as sufficiently many subjects are able to identify optimal strategies.

Here we test experimentally whether providing information on previous applications can increase the welfare of subjects of lower cognitive ability within BOS, reduce the gap between subjects of higher and lower ability and help to avoid ability segregation across schools. Moreover, we test whether this information will further increase average expected payoffs in BOS.

Since the failure to anticipate others' strategies is one likely source of their strategic mistakes, enhanced information should benefit subjects of low ability. Yet information on others' strategies needs to be complemented with an understanding of their consequences for acceptance probabilities at schools at the different steps of the process. Hence, since subjects of higher cognitive ability may be better able to make use of the provided information, information provision may end up widening the gap between low and high ability subjects and could further disadvantage the former.

In our experiment, we classify participants' cognitive ability according to their score in a 36 question Raven test. We then let them play ten school allocation games under the Boston mechanism, using two different preference environments. For the information treatment, participants are informed of the number of applicants that listed each school first in the previous game where the distribution of preferences was identical. We compare their choices and outcomes to those obtained in a control treatment where information about past strategies is not provided.

In our first preference environment students are required, in equilibrium, to reveal truthfully the most preferred choice but manipulate by ranking a safe school second (*skipping-the-middle*). The cue to this strategy is to understand that both their first and second most preferred school will be oversubscribed in the first round, so that placing any of them second would lose them the chance of being admitted in the second round. In our second preference environment, a majority of students should instead *skip-the-top*—i.e. manipulate by ranking a less preferred school first. Here a student should anticipate that demand at his most preferred school will be so high that he would be better off by taking a safe seat at his second most preferred school. In both cases, knowing the demands in the first round of the past period may allow students to form a more precise prediction and use a more appropriate strategy in the current period.

Indeed, students that submit preferences truthfully tend to be worse off in both environments. Students that do not manipulate their second choice end up at their least preferred school whenever rejected in the first round. Students that do not manipulate their first choice when they should, have a low chance of being admitted in the first round, and have to compete for the possibly few remaining seats in further ones, while they could have secured a seat at their second preferred school. As documented in Basteck and Mantovani [2016], subjects of lower cognitive ability are more prone to all of these mistakes, as well as to disadvantageous manipulation of the first choice—i.e. over-cautiousness. As a consequence, they earn less and ability segregation emerges, as lower cognitive ability subjects are over-represented at the worst school. We ask whether information on past applications levels the playing field between subjects of higher and lower cognitive ability, while also improving efficiency as measured by average expected payoffs.

Answers to both questions are mixed. Strategies of both low and high ability subjects are significantly affected by the presence of information about past demands in the direction of more appropriate manipulations. The strategic gap between the two groups reduces but only marginally. While ability segregation is less pronounced with information, subjects of lower cognitive ability earn less than their peers both with and without information. The reduction in this gap induced by information is not significant. Thus, while the treatment is effective in affecting players' strategies, it fails at leveling the playing field. Similarly, better strategies translate into higher average expected payoffs, overall. However, this increase in welfare does not appear to be significant.

Revealing information about past strategies has been used as a tool to affect beliefs in a plethora of experimental studies—as, for instance, in coordination games [Devetag, 2003], oligopolistic competition [Huck et al., 1999], and to study boundedly rational equilibrium models [Huck et al., 2011]. Some experimental studies in school choice implement pure one-shot designs [Calsamiglia et al., 2010; Chen and Sönmez, 2006], or repeated rounds with no feedback [Klijn et al., 2013; Pais and Pintér, 2008]. Others use repeated rounds with feedback on the outcomes of each round to allow for learning [Chen and He, 2015; Chen et al., 2015; Chen and Kesten, 2012; Featherstone and Niederle, 2014]. Pais and Pintér [2008] compare treatments with different levels of information about others' preferences and find marked differences between them. Chen and He [2015] show subjects are willing to pay for information on both their own and others' preferences and highlight the role of information disclosure policies. We add to this literature by studying the role of information about past choices and its use by students of different ability. We argue that many parents search for this information to infer future demands at schools in the field. Most importantly, education authorities have this information available, contrary to that on applicants' preferences, so that making it available is a policy tool they can easily implement.

The paper is organized as follows. Section 2 introduces the school choice environments, experimental design and procedures. Hypotheses are found in Section 3. Results follow in Section 4. Section 5 concludes.

2 Experimental design and procedures

The decision environment and part of the treatments are borrowed from Basteck and Mantovani [2016]. We briefly summarize them, together with the presentation of the novel treatments, in this section. We refer to Basteck and Mantovani [2016] for a more detailed discussion of the common parts.

2.1 Set-up, matching mechanism and equilibria

There are 4 schools $s \in S = \{A, B, C, D\}$, with 4 seats each. Competing for these seats are 16 students $i \in I$, 4 of each type $t_i \in T = \{1, 2, 3, 4\}$. Each student i admitted to a school s receives a payoff $p(s, t_i)$ that depends on both the school s and her own type t_i.

Students report a ranking of schools $\succ_i$, i.e. a strict linear order on S. A centralized lottery draws a different number l_i between 1 and 16 for each student. Those numbers are used to break ties among applicants. An allocation

TABLE 1: PAYOFFS IN P1 (LEFT PANEL) AND P2 (RIGHT PANEL)

$p(s, t_i)$	School A	School B	School C	School D	School A	School B	School C	School D
Type 1	20	10	6	0	20	11	7	0
Type 2	16	17	6	0	16	15	7	0
Type 3	16	10	8	0	16	11	11	0
Type 4	16	10	6	0	16	11	7	0

Notes: each cell represents the payoff a student of type t_i obtains when admitted at school s in the relevant preference environment.

mechanism uses the submitted rankings and the lottery numbers to generate a matching between students and schools. The BOS mechanism proceeds as follows.

> ROUND 1. Each student applies at the school that she ranked first. If there are at most 4 students applying at a school, they are admitted. If there are more than 4 students applying, the school admits the 4 applicants with the lowest lottery number.
>
> ROUND $k > 1$. Each student who has not yet been admitted, applies at the school that she ranked at the k^{th} position. The school admits applicants in the order of their lottery numbers until either it has admitted 4 students in total (including previous rounds) or there are no more applicants who have ranked the school in k^{th} position.
>
> With as many seats as students, each student has been admitted to some school when the algorithm terminates after at most 4 rounds.

We study two different preference environments, henceforth P1 and P2. Payoffs are given in Table 1. Students agree that D is the worst school, and the associated payoff is always zero. Students of type 1, 2 and 3 earn a higher payoff than others respectively at school A, B and C. The associated equilibria are as follows (calculations can be found in Basteck and Mantovani [2016].):

Equilibria BOS-P1.

In every pure strategy Nash equilibrium of the game induced by BOS-P1 :

- *11 students report* $A \succ_i C \succ_i B, D$*: all type* 1 *and 7 out of the 8 type* 3 *and* 4
- *5 students reports* $B \succ_i C \succ_i A, D$*: all type* 2 *and 1 out of the 8 type* 3 *and* 4

Equilibria BOS-P2.

In every pure strategy Nash equilibrium of the game induced by BOS-P2 :

- *all students of type* 1 *and three of type* 4 *report* $A \succ_i C, B, D$

- *all students of type* 2 *and one of type* 4 *report* $B \succ_i C, A, D$
- *all students of type* 3 *report* $C \succ_i A, B, D$
- *some students of type* 1, 2, *and* 4 *rank* C *second.*

The intuition for the equilibrium in P1 is as follows. At least four students like schools A and B best, so both will be filled in the first round. Since the payoff from C is relatively low even for type 3 students, no one will initially apply at C. Since in the second round, only schools C and D have available seats, all initially rejected students should apply at C in the second round. Finally, one additional student besides the four students of type 2 will initially apply at B in order to avoid the heavily oversubscribed school A. This student will be either of type 3 or 4, as students of type 1 receive a higher payoff at school A.

If in the experiment some applicants are naive, in the sense that they always report truthfully—and hence out-of-equilibrium—we will still see A and B filled in the first round but C will be less oversubscribed in round 2. Moreover, a higher chance at C as a fall-back option will make sophisticated players who best respond more willing to initially apply at their true first choice A or B. Hence the main difference between naive and sophisticated players lies in the fact that the latter would use *skip-the-middle* strategies. For example, assume that there are two naive and two sophisticated players of each preference type. Then we get the following, unique,[3]

Pseudo-equilibrium BOS-P1.
Considering only sophisticated players,

- *6 students of type* 1, 3, *and* 4 *report* $A \succ_i C \succ_i B, D$*,*
- *2 students of type* 2 *report* $B \succ_i C \succ_i A, D$*.*

In P2, students of type 2 still have a very high valuation for B. In an equilibrium they would apply there first, avoiding the heavily oversubscribed school A that is everyone's favorite. Also, students of type 3 have a higher valuation of school C, making them too willing to apply at C in the first round. While in equilibrium students are indifferent about how to rank schools beyond the first, some must rank C second. Otherwise a student of type 3 would deviate, initially applying at A and ranking C second as a safe fall-back option. But this, in turn, would induce everyone else to rank C second, so that the type 3 student would no longer be willing to apply at A in the first round.

If again we consider truthful out-of-equilibrium reports by some players, A will become more competitive, which reduces the incentives of sophisticated players to apply there. However, as a countervailing effect, C would be

[3] Calculations to corroborate this claim are provided in Appendix A.

available as a fall-back option with positive probability in round 2, making sophisticated players more willing to initially apply at the most competitive school A. If again we assume that for each of the four types there are two naive and two sophisticated players, we get the following, unique,[4]

Pseudo-equilibrium BOS-P2.

Considering only sophisticated players,

- *4 students of type* 1 *and* 3 *report* $A \succ_i C \succ_i B, D$,
- *4 students of type* 2 *and* 4 *report* $B \succ_i C \succ_i A, D$.

Hence the main difference between naive and sophisticated players lies in the fact that the latter use both *skip-the-middle* and *skip-the-top* strategies.

2.2 Design

In each session, subjects face three different tasks.[5]

Raven test. Each session starts with a computerized version of Raven's Standard Progressive Matrices test. The Raven test is a leading non-verbal measure of analytic intelligence [Carpenter et al., 1990; Gray and Thompson, 2004].[6] Each question of the test asks to identify the missing element that completes a visual pattern from a list of candidates.[7] Out of five blocks of questions on the Raven test, we administer the three most difficult blocks (C, D, E) for a total of 36 questions. Subjects have 18 minutes to complete the test, 5 minutes for each of the blocks C and D, and 8 minutes for block E. Within each block, subjects can move back and forth between the questions, skipping some or changing their previous answers. Subjects earned 0.1 ECU for each correct answer.

Bomb risk elicitation task (BRET). Next, we administer the BRET, developed by Crosetto and Filippin [2013]. In this task, subjects have to decide how many out of 100 boxes to collect. One box selected at random with uniform probability contains a bomb, and the location of the bomb is unknown to subjects. Subjects earn 0.1 ECU for each collected box as long as they do not collect the bomb, in which case they receive zero. The more boxes a subject collects the less risk averse (or the more risk loving) she is; collecting 50 boxes corresponds to risk neutrality.

[4]Calculations to corroborate this claim are provided in Appendix A.

[5]Appendix B includes a screenshot of the decision screen for each task.

[6]Raven test scores are associated with the degree of sophistication in the beauty contest [Gill and Prowse, 2015], with the performance in Bayesian updating [Charness et al., 2011], and with more accurate beliefs [Burks et al., 2009].

[7]See Appendix B for an example.

School allocation game. As described in Section 2.1, subjects play as students applying at schools under the Boston mechanism. Overall there are 10 periods in which subjects apply; only one is selected randomly to determine payoffs. In each period, sixteen students, four for each preference type, are allocated seats at four schools with admission decisions depending on applicants submitted rank order lists and their lottery numbers, which are used to break ties. Subjects know their own preference type as well as the distribution of preferences when deciding on a rank order list to submit. Lottery numbers are drawn each period only after all subjects submit their lists. In each session subjects play five consecutive periods of the school choice game under each of the two preference environments, i.e. under a fixed distribution of preferences, for a total of ten games. That is, we vary the preference environment *within* subjects. We vary the order of preference environments P1 and P2 across sessions to control for order effects.

We vary *between* subjects the amount of feedback information they receive after each period of the school choice game. In sessions with No Information (NI), subjects are informed of their lottery draw, the school that they were admitted to, and the corresponding payoff in ECU. In sessions with Information (I) subjects are also informed on the number of applicants at each school in the first round of the mechanism—i.e. how many subjects ranked each school first. This information is also available to them when making a decision in the following period under the same preference environment.[8]

In each session, we label as 'High' the subjects whose Raven score is above the median of the distribution of scores in their session. Subjects below the median are labeled as 'Low'.[9] Subjects are not informed whether they are above or below the median. Instead we use this classification to assign two High and two Low subjects to each preference type in order to ensure that preferences and cognitive abilities are uncorrelated. Subject to this constraint, a new preference type is assigned randomly to each player in every new period.

2.3 Procedures

The computerized experiment was run at the WZB-TU Experimental Lab in Berlin between September 2015 and February 2016. It involved 192 subjects,

[8]Within the same project, we run sessions also under the Deferred Acceptance mechanism, with feedback identical to that of NI. Data are analyzed and reported in Basteck and Mantovani [2016].

[9]We break ties using the amount of time used to complete the Raven test. If ties still remain we break them at random.

TABLE 2: SESSIONS

Session	Date	Participants	Mechanism	Order	Info treatment
1	Sep 2015	16	BOS	P1-P2	NI
2	Sep 2015	16	BOS	P1-P2	NI
3	Sep 2015	16	BOS	P1-P2	NI
4	Nov 2015	16	BOS	P2-P1	NI
5	Nov 2015	16	BOS	P2-P1	NI
6	Nov 2015	16	BOS	P2-P1	NI
7	Feb 2016	16	BOS	P1-P2	I
8	Feb 2016	16	BOS	P1-P2	I
9	Feb 2016	16	BOS	P1-P2	I
10	Feb 2016	16	BOS	P2-P1	I
11	Feb 2016	16	BOS	P2-P1	I
12	Feb 2016	16	BOS	P2-P1	I

Notes: Order indicates whether the five rounds of preference environment 1 were run before (P1-P2) or after (P2-P1) preference environment 2. Info treat indicate whether the session was under No Information (NI) or Information (I). Six sessions with deferred acceptance as a mechanism (not reported here) where run between September and November 2015.

distributed over 12 experimental sessions, where each subject participated only in one session. Sessions took on average around 80 minutes. The computerized program was developed using Z-tree [Fischbacher, 2007]. Table 2 summarizes sessions' details.

All sessions followed an identical procedure. Subjects were randomly assigned to cubicles in the lab. Instructions were read aloud before each task.[10] To ensure everybody understood the tasks, subjects had to answer control questions before the BRET, and the school choice game. For the school choice game, this included an example where subjects had to find the allocation in a simple school choice problem, given submitted lists and lottery numbers. The tasks would only start after every subject had correctly answered all control questions. To get subjects used to the decision environment, we run a trial round of BRET where no ECU could be earned, before running a single payoff-relevant round.

At the end of the school choice game, subjects were asked to complete a questionnaire. We gathered qualitative information about their strategies and their opinions regarding school choice. We also collected data on whether they had faced the Raven or a similar test before and on whether they were used to

[10] An English version of the experimental instructions is available in Appendix B.

TABLE 3: CLASSIFICATION OF STRATEGIES

Type(s)	Truthful	Safe Naive	Skip-The-Top	Skip-The-Middle
2 in P1	$B \succ A \succ C \succ D$	–	–	$B \succ C \succ A, D$
3 in P2	$A \succ B \succ C \succ D$ $A \succ C \succ B \succ D$	$B \succ A \succ C, D$ $C \succ A \succ B, D$	$B, C \succ A, D$	–
all other	$A \succ B \succ C \succ D$	$B \succ A \succ C, D$	$B \succ C \succ A, D$	$A \succ C \succ B, D$

Notes: the table delineates the labels that we apply to strategies for different types.

playing mind puzzles.

Subjects were told they would be paid according to the ECU earned in the Raven test, in the BRET and in one round of the school choice game selected at random by the computer. To determine payoffs in Euro, we applied the exchange rate: 1ECU= .70€. Subjects could earn between 0 and 14 Euros from the school choice game, between 0 and 2.52 Euros from the Raven test, and between 0 and 6.93 Euros from the BRET. The average payment, including 5 Euros of show-up fee, was 15.45 Euros.

3 Hypotheses

For types whose true preferences over schools are $A \succ B \succ C \succ D$, let *Truthful* denote the strategy where the reported rank order list coincides with the true preferences, let *Skipping-The-Top* (STT) denote the strategy where the most preferred school is demoted, the second most preferred school is ranked first, and the third most preferred school second, and let *Skipping-The-Middle* (STM) denote the strategy where the most preferred school is listed first, and the third preferred school second. For type 2 in P1 and type 3 in P2 whose preferences differ slightly, we adjust these labels accordingly, see Table 3.

We hypothesize that information provision helps subjects to identify individually optimal strategies. In P1, both schools A and B will be filled in the first round, both in equilibrium and in a situation where some subjects are biased towards truth-telling (see Section 2.1 on the (pseudo-)equilibria of P1). Hence, it should be individually rational to rank C second and in particular choose to *Skip-The-Middle*.[11] In P2, schools A and B will similarly be filled in the first round so that it is a best response to rank C second (or, eventually, first

[11] We also find that ex-post, using the empirical distribution of strategies, STM is the best response for all types, see Section 4.

in the case type 3)—see Section 2.1 on the (pseudo-)equilibria of P2.[12] Hence, it should be individually rational to choose either to *Skip-The-Middle* or *Skip-The-Top*.[13]

Hypothesis 1. *In* P1*, the fraction of STM strategies is higher under* I *than under* NI*. In* P2*, the combined fraction of STM and STT strategies is higher under* I *than under* NI*.*

The average expected payoff in equilibrium—i.e. before lottery numbers are drawn—is between 9.9 and 9.95 in P1, and equal to 10.87 in P2.[14] If subjects fail to coordinate on an equilibrium, the average expected payoff is lower, as schools are no longer as likely to admit those types of students that have a higher valuation for that school. Hence, if information provision moves subjects strategies towards the equilibrium profile, it should increase average expected payoffs.

Hypothesis 2. *Subjects' average expected payoff is higher under* I *than under* NI *in both preference environments.*

We expect Low subjects to be more prone to strategic mistakes, and in particular biased towards truth-telling. In the extreme case all Low subjects report truthfully and High subjects best respond to them—applying STM in P1 and STM or STT in P2—so that the expected payoff for a Low subject is 8.58 in P1 and 6.58 in P2, while the expected payoff for a High subject is 11.66 in P1 and 12.27 in P2. The main source of payoff differences is the fact that Low subjects are more likely to be assigned to the worst school D, since they fail to rank C as a safe choice in round 2. Information may allow Low subjects to identify optimal strategies. When this is the case, treatment I should level the playing field: payoff differences between High and Low subjects decrease, as does ability segregation across schools.

Hypothesis 3. *The difference in the expected payoffs of High and Low subjects are lower under* I *than under* NI*, in both* P1 *and* P2*.*

Hypothesis 4. *In both* P1 *and* P2*, Low subjects are less likely than High subjects to be admitted to* C *and more likely to be admitted to school* D *under* NI*. Low and High subjects are equally likely to be admitted at any school under* I*.*

[12] Ranking C at least second might be ineffectual if, in equilibrium, C is filled in the first round as well. However, it is still a best response. Moreover, in the pseudo-equilibrium where some subjects are truth-telling, playing a best response *requires* to rank C at least second.

[13] Again, we find that against the empirical distribution of strategies, it is a best response to choose STM, STT or, for type 3, $A \succ C \succ B \succ D$, see Section 4.

[14] The expected payoff in P1 depends on the selected equilibrium, i.e. on the identity of the fifth applicant at B.

TABLE 4: SEAT ALLOCATION DYNAMICS

		NI-P1	I-P1	NI-P2	I-P2
School A	round 1	100%	100%	100%	100%
	round 2	–	–	–	–
School B	round 1	100%	100%	93%	92%
	round 2	–	–	100%	100%
School C	round 1	16%	48%	36%	74%
	round 2	74%	93%	72%	98%
School D	round 1	1%		2%	0%
	round 2	3%	1%	3%	0%

Notes: the table shows the (cumulative) percentage of seats of each school that are allocated in the first two rounds of the allocation procedure in the different treatments.

4 Results

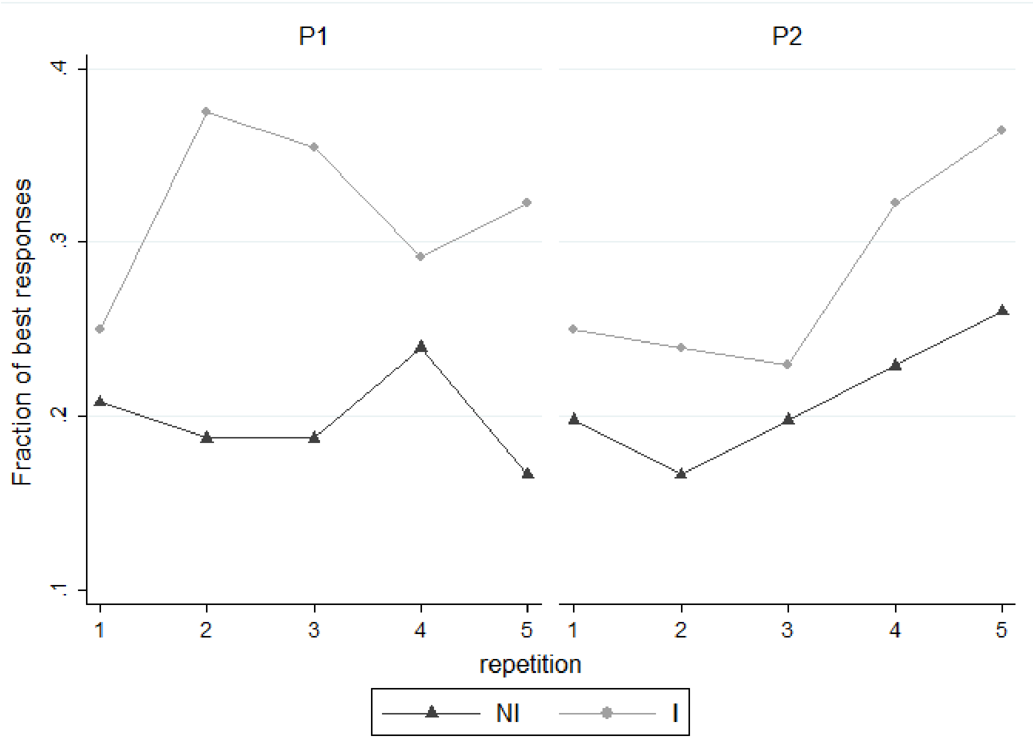
Table 4 shows in which round seats at the different schools are assigned to their final match. School A is always filled in the first round. The same holds for school B in P1; on average more than 90 percent of its seats are filled in P2 in the first round both under I and NI. School C is filled faster under I than under NI and faster in P2 than in P1.

We check that the strategies we identify as best responses in the pseudo-equilibria described in Section 2.1, are indeed best responses against the empirical distribution of opponents' strategies in one's period and treatment. STM turns out to be always the best response for each type in P1, except for type 4, for which it is a best response in all but one instance (i.e. 98 percent of the time) where the best response is STT. In P2, the best response is always STM for type 1, always STT for type 2. For type 3 submitting truthfully ($A \succ C \succ B \succ D$) is a best response 90 percent of the time, while ranking C first is a best response in the remeining cases. For type 4, the best response is either STT (23 percent of the time), or STM (77 percent of the time). These results support the approach of considering STM as the optimal strategy in P1, and STT and STM as the optimal strategies in P2.[15]

Figure 1 shows the evolution over periods of the fraction of best responses to the empirical distribution in one's treatment and period. A Wilkoxon rank-

[15]We cannot discriminate the sophistication of type 3 in P2, since both naive and sophisticated subjects will be truthful.

FIGURE 1: SOPHISTICATION OVER PERIODS



Notes: the figure shows the fraction of best responses to the empirical distribution in one's treatment and period. We average over the n^{th} repetition of each P1 and P2, independently on whetherP1 or P2 where run first—i.e. repetition 1 includes data on both period 1 and 6 of the same treatment.

sum test (WRS) finds no significant difference across treatments in period one in both P1 and P2 (P1: $z = -1.12$, P-val= .26; P2: $z = 0.67$, P-val= .50).[16] This supports the assumption that the increase in the difference that follows, and the resulting aggregate differences are due to the treatment, and not to differences in the samples.[17]

On aggregate, 18 percent of strategies are STM in NI-P1 and this fraction increases to 33 percent in I-P1; 16 (7) percent of strategies are STT (STM) in NI-P2, which increases to 29 (15) percent in I-P2. Non-parametric tests on differences across treatments are shown in Table 5. Every test is based on one observation per session. We find evidence that significantly fewer STM lists are submitted in NI with respect to I under both preference environments. Under NI-P2, subject use STT significantly less relative to I-P2. Conversely, the rate of naive truth-telling is always significantly higher under NI. Under

[16]Indeed, WRS test finds no difference for each of the variables listed in Table 5 in period one.

[17]We choose to keep all data in the analysis. We note that excluding period one does not affect our results.

TABLE 5: ACROSS TREATMENT DIFFERENCES

	Sample	Truthful		SN		STT		STM		Exp. Payoff	
		Z	P-val	Z	P-val	Z	P-val	Z	P-val	Z	P-val
NI vs I	P1	**2.49**	**.01**	1.53	.12	1.70	.09	**-2.25**	**.02**	-1.12	.26
	P2	**2.41**	**.02**	.966	.33	**-2.33**	**.02**	**-1.97**	**.05**	-.961	.34
P1 vs P2	NI	-.11	.92	**-2.20**	**.03**	**-2.20**	**.03**	**2.20**	**.03**		
	I	.314	.75	-1.69	.09	**-2.20**	**.03**	**2.20**	**.03**		

Notes: the table reports for each of the listed variables: the Wilkoxon rank-sum test, and corresponding P-value, on the difference between NI and I within each preference environment; the Wilkoxon signed-rank test for the differences between P1 and P2, within either NI or I. A positive statistic means a higher value for NI (P1). The statistic is computed using one observation per session. Expected payoff is computed using a recombinant strategies procedure with 1000 recombinations for each subject in each period, and an identical number of tie breakers. Bold indicates significance at the .05 level.

both NI and I, strategies shifts in the expected way between P1 and P2.

While players fall short of the equilibrium benchmark, disclosing information about past applications allows them to improve their strategic performance, supporting Hypothesis 1.

Result 1. *Under* I, *players use more STM strategies in* P1, *and more STM and STT strategies in* P2 *as compared to* NI. *We find evidence that these strategies are the optimal ones in all treatments.*

Since observed payoffs depend to a large extent on the random lottery numbers, and on the strategies used in that particular period by the other subjects, we use a recombinant estimation technique to estimate and compare expected payoffs across treatments. The procedure, which is standard in the literature [e.g. Chen and Sönmez, 2006], proceeds as follows. Start by picking the strategy of the first subject in the first period, and match it with fifteen strategies drawn at random among those used in the first period in all sessions of the same treatment, under the constraint that there are four players for each type in the resulting virtual game. Given these sixteen strategies, seats are assigned based on a new random lottery ordering. Repeat n times, always rematching the same strategy, and create n random samples, each with its own lottery ordering, and corresponding allocation. Implement this procedure for all subjects and all periods. We choose $n = 1000$. We consider each individual average payoff over recombinations as the expected payoff of the corresponding subject in that period.[18]

The average over these (estimated) expected payoffs is 9.49 under NI-P1, 9.55 under I-P1, and 9.9 under NI-P2, 10.1 under I-P2. In equilibrium, the

[18]We note that all of our results hold when we use the raw payoffs obtained in the experiment.

corresponding figures would be (approximately, see Section 3) 9.9 in P1, and 10.87 in P2. As the last columns in Table 5 show, the increase in ex-ante expected payoffs we observe between NI and I is not statistically significant. That is, a significant improvement in the strategies of subjects improve only marginally the average payoff. The reason for this goes as follows. As more subjects use optimal strategies, there is increased competition for the other sophisticated subjects, and worse prospects also for naive ones. Thus, when a player switches from naive to sophisticated, her payoff increases, but on aggregate this improvement is mitigated by the decrease in the payoffs of others. Indeed the average expected payoff of STM in P1 is 11.75 in NI and 11.49 in I, the average expected payoff of other strategies is 9.11 in NI and 8.88 in I. In P2 subjects playing as in the pseudo-equilibrium described in Section 2.1 earn on average 12.03 in NI and 11.21 in I; others earn on average 9.39 in NI, 9.29 in I.

Result 2. *Subjects' average expected payoff is higher under* I *than under* NI*, but the differences are not statistically significant, for both preference environments.*

Figure 2 reports the distribution of Raven scores and its median. We split the overall sample between High and Low subjects, rather than keep using the classification adopted to allocate them within each session. We use on the whole sample the same criterion adopted within each session: we break ties in the partial ordering induced by Raven scores using the amount of time used to complete the test, where faster subjects receive a higher rank. If ties survive to this procedure, we break them at random. The median happens to be at a Raven score of 30, where 17 minutes and 57 seconds are used to complete the test. Subjects that do strictly better than that are classified as High, others are classified as Low.[19] Figure 3 reports the distribution of choices in the risk elicitation task. We overimpose the kernel densities for Low and High subjects. We detect a small but significant positive correlation between Raven scores and risk attitudes.[20] This relation suggest it is important to control for risk aversion when analyzing differences between Low and High subjects.

Figures 4 and 5 show the distribution of strategies used by High and Low subjects in each treatment. STT strategies increase from NI to I for both Low

[19]In Basteck and Mantovani [2016] the threshold was slightly different, causing two subjects that were classified as High there to be classified as Low here. As such the statistics reported here for treatment NI will occasionally differ from the corresponding ones in Basteck and Mantovani [2016], despite the fact the data is exactly the same.

[20]Spearman's $\rho = .16$, P-val= .02. The sign and magnitude of this correlation replicates the findings of Dohmen et al. [2010]. However, if we consider jointly the data reported here and those in Basteck and Mantovani [2016] the Spearman correlation coefficient is .07 and is not significant.

FIGURE 2: DISTRIBUTION OF RAVEN SCORES

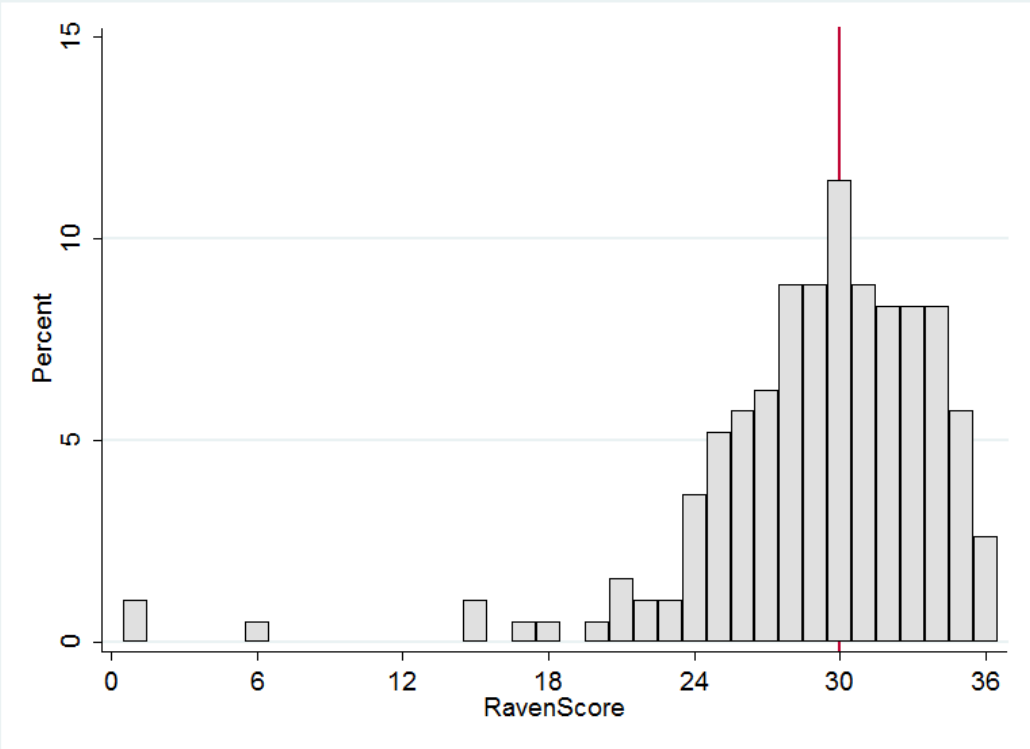


FIGURE 3: DISTRIBUTION OF RISK TASK CHOICES

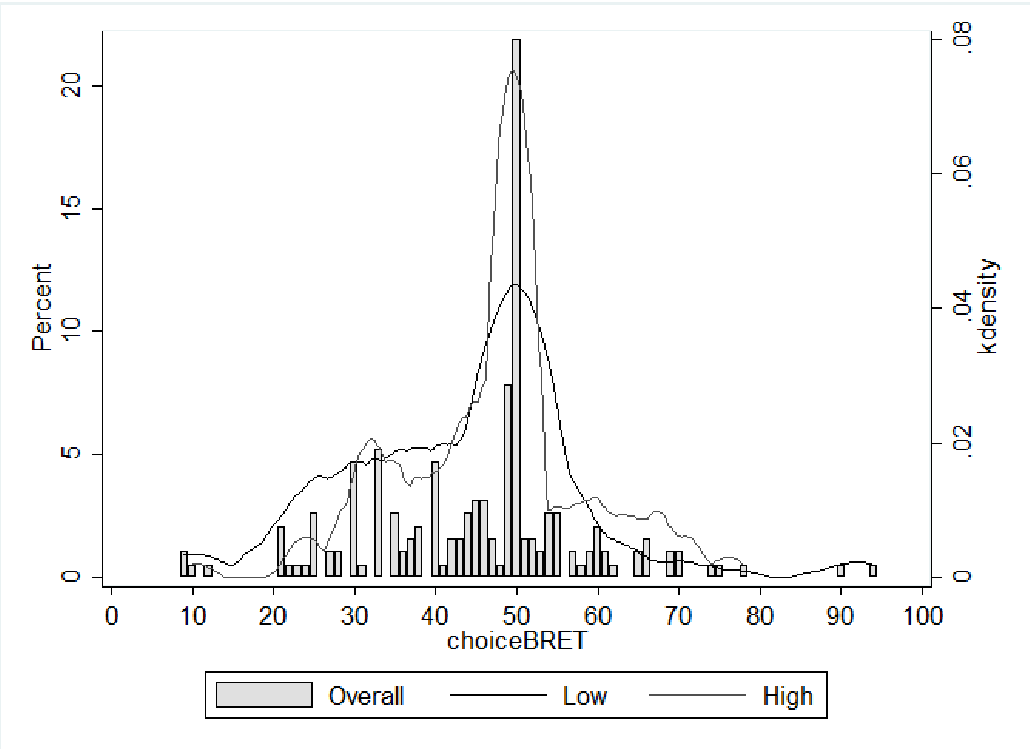


FIGURE 4: PLAYERS' STRATEGIES - P1

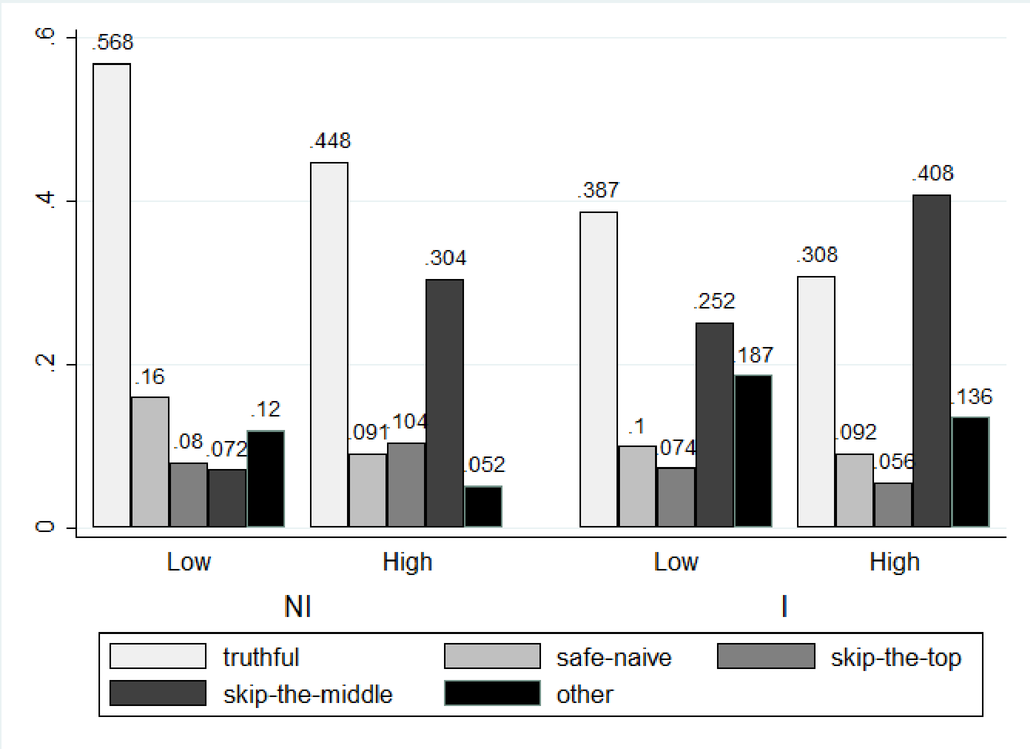


FIGURE 5: PLAYERS' STRATEGIES - P2

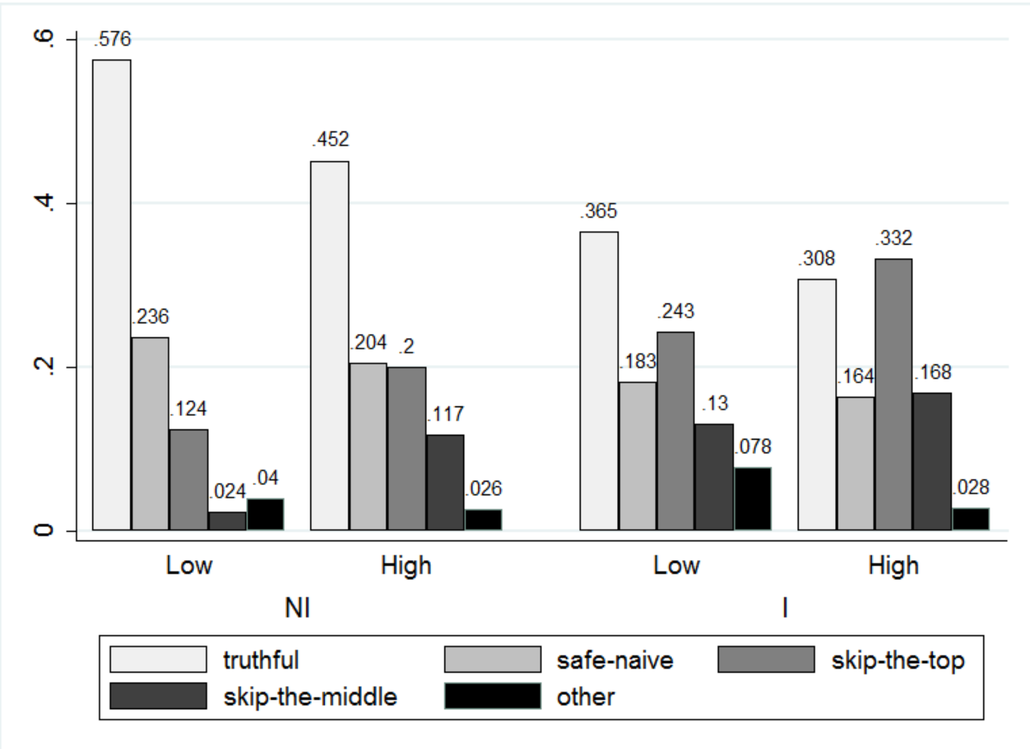


TABLE 6: DIFFERENCES BETWEEN HIGH AND LOW

Treatment	Truthful		SN		STT		STM		Exp. Payoff	
	Z	P-val	Z	P-val	Z	P-val	Z	P-val	Z	P-val
NI-P1	-1.15	.25	-1.15	.25	.314	.75	**2.20**	**.03**	**2.20**	**.03**
I-P1	-.734	.46	.314	.75	-.734	.46	**1.99**	**.04**	**2.20**	**.03**
NI-P2	-1.57	.11	-1.15	.25	**1.99**	**.04**	**1.99**	**.04**	**2.20**	**.03**
I-P2	-.420	.67	.105	.92	**1.99**	**.04**	.314	.75	**2.20**	**.03**

Notes: the table reports, for each of the listed variables, the Wilkoxon signed-rank test, and the corresponding P-value, on the difference between High and Low subjects within each treatment. A positive statistic means a higher value for High subjects. The statistic is computed using one observation per session. Expected payoff is computed using a recombinant strategies procedure with 1000 recombinations for each subject in each period, and an identical number of tie breakers. Bold indicates significance at the .05 level.

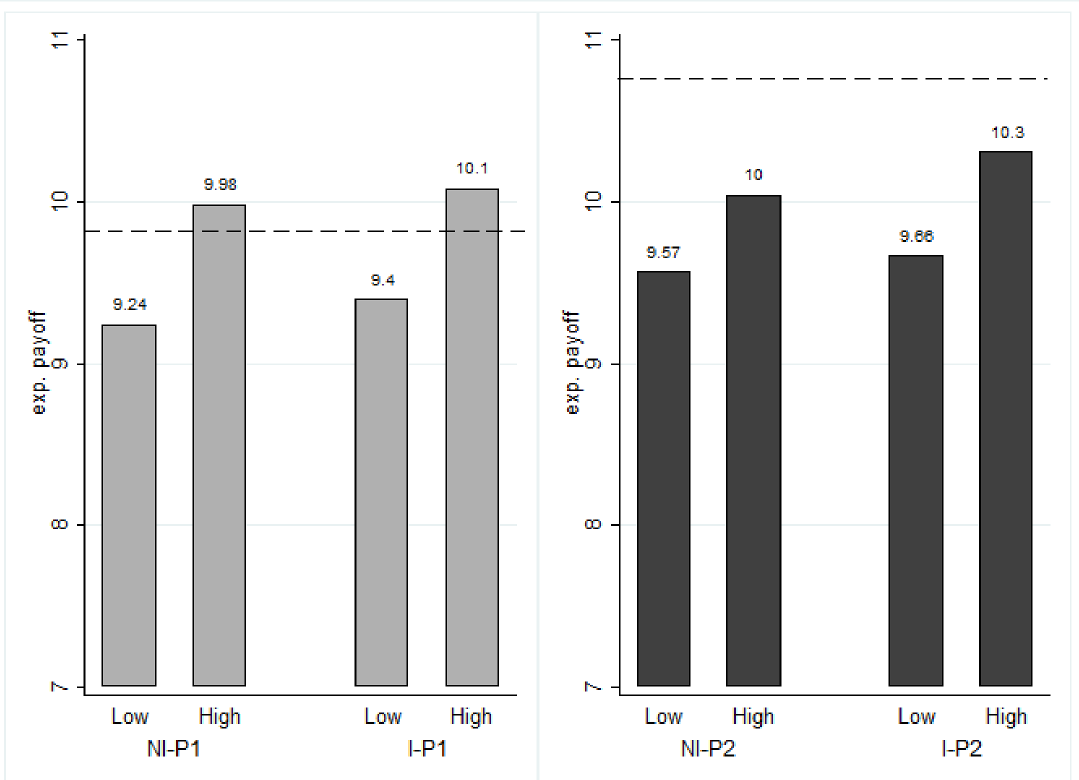
and High subjects in both preference environments. The same holds for STT strategies in P2. Conversely both Low and High subjects reduce their truth-telling rate under I.

Table 6 compares the strategies of Low and High subjects using non-parametric methods. Since we base the tests on one observation per session, the observations for Low and High subjects are matched, and we adopt the Wilkoxon signed-rank (WSR) test. The tests confirm that, in P1, High subjects use STM more frequently than Low subjects under both treatments. In NI-P2, they are more likely to use STT and STM, while in I-P2 the difference is only significant for STT. This strategic gap between the two groups is reflected by their average expected earnings (Figure 6). Low subjects earn less in all treatments. The differences in expected payoffs are found to be significant, as reported in the last columns of Table 6.

Table 7 also investigate differences between Low and High subjects using regression analysis. The approach is useful to control for other variables that may be related to both cognitive ability and strategic behavior. For instance it allows to control for risk preferences, as measured by subjects' choices in the BRET. All models are random-effects panel regressions, where standard errors are clustered at the session level. All previous findings are confirmed: I has a positive but non-significant effect on expected payoffs, for both Low and High subjects; High subjects earn significantly more than Low ones, and the reduction in the gap between them is not significant. As it may be expected, we find more risk averse subjects to earn significantly less in P1, but not in P2.

The top panels of Figure 7 represent these results. In the bottom panels, we show the linear relation between Raven score and predicted expected pay-

FIGURE 6: LOW AND HIGH SUBJECTS' PAYOFFS



Notes: average expected payoffs computed for top (High) and bottom (Low) half of the distribution of Raven scores, computed using a recombinant strategies procedure with 1000 recombinations for each subject in each period, and an identical number of tie breakers. Dashed lines = equilibrium payoffs.

off for each treatment. They are obtained from models similar to (3) and (6), except they estimate the interaction between the mechanism and the (continuous) Raven scores, rather than the dummy High/Low.[21] The exercise is useful to shed light on how information about demands affect outcomes over the full distribution of cognitive ability. Results confirm that there exists a positive relation between Raven scores and expected payoffs in the school choice game. Notably, the slopes for I are higher than those for NI, and significantly so in P2. This result is suggestive that proper use of information requires skills that not everyone has. As a consequences, while it lifts the prospects of many subjects—including many that we classified as Low—those in the left tail of the distribution of cognitive ability are left behind and have their situation worsened. Overall we do not find support for Hypothesis 3.

Result 3. *High subjects earn higher payoffs than Low ones under all treatments. The difference between the two groups does not decrease significantly between* NI *and* I*,*

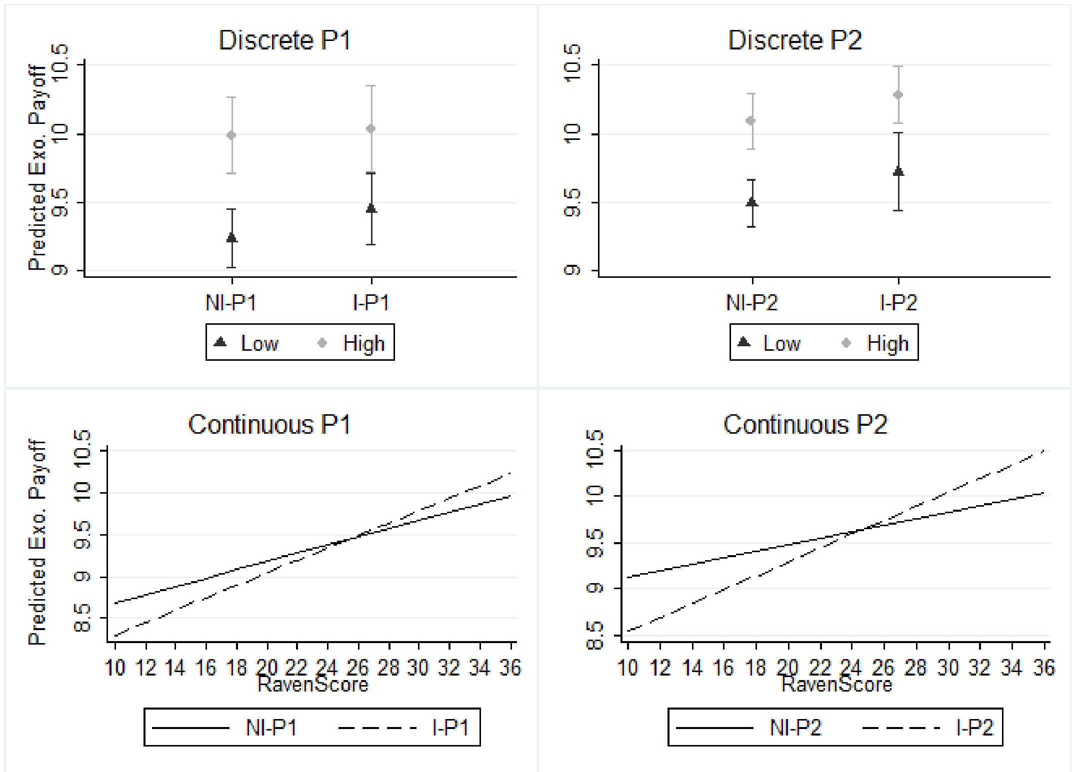
[21]See the full estimates in Appendix C

TABLE 7: TREATMENT AND COGNITIVE ABILITY: REGRESSION TABLES

	Dep. Var.: Expected payoff					
	P1			P2		
	(1)	(2)	(3)	(4)	(5)	(6)
I	0.128	0.130	0.211	0.181	0.212	0.232
	(0.158)	(0.139)	(0.177)	(0.160)	(0.141)	(0.181)
High	0.712***	0.665***	0.747***	0.557***	0.581***	0.601***
	(0.140)	(0.140)	(0.208)	(0.0903)	(0.0760)	(0.111)
I*High			-0.163			-0.0397
			(0.296)			(0.168)
age		-0.0282	-0.0279		-0.0261	-0.0260
		(0.0181)	(0.0187)		(0.0167)	(0.0166)
female		-0.305	-0.301		0.0164	0.0174
		(0.172)	(0.174)		(0.132)	(0.133)
period		0.00964	0.00964		0.0508	0.0508
		(0.0291)	(0.0291)		(0.0423)	(0.0424)
Type 2		1.581***	1.582***		-0.0708	-0.0707
		(0.317)	(0.316)		(0.252)	(0.252)
Type 3		-1.631***	-1.632***		-0.0721	-0.0717
		(0.154)	(0.155)		(0.284)	(0.284)
Type 4		-2.148***	-2.148***		-2.179***	-2.180***
		(0.172)	(0.173)		(0.179)	(0.179)
order		0.171	0.168		-0.266	-0.266
		(0.139)	(0.136)		(0.140)	(0.140)
BRET		0.0118	0.0117**		0.00858	0.00857
		(0.00605)	(0.00594)		(0.00624)	(0.00623)
_cons	9.254***	9.965***	9.919***	9.345***	10.07***	10.27***
	(0.132)	(0.458)	(0.506)	(0.154)	(0.590)	(0.576)
Obs. (groups)	960 (12)	960 (12)	960 (12)	960 (12)	960 (12)	960 (12)

Notes: the dependent variable is computed using recombinant strategies procedure with 1000 recombinations for each subject in each period, and an identical number of tie breakers. In parentheses we report robust standard errors, clustered at the session level. *,**,***: statistically significant at the 10%, 5% and 1% level, respectively.

FIGURE 7: MARGINAL EFFECTS



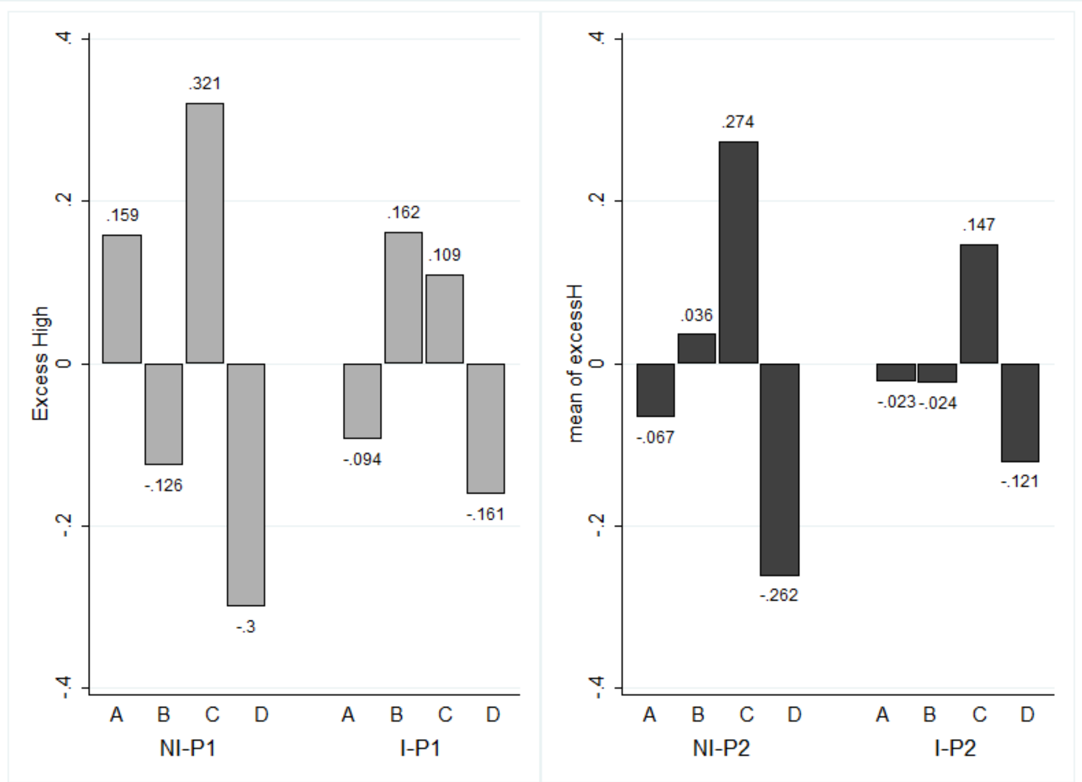
Notes: the figure shows marginal effects obtained from models (3) and (6) of Table 7 (top panels), and from models (3) and (6) of Table 9 (bottom panels).

in both preference environments.

Hypothesis 4 states that ability segregation should decrease in I relative to NI. While this hypothesis is related to the relative payoffs of High and Low, its rejection does not follow from Result 3. If High and Low subjects play in a similar way, and since types are uncorrelated with cognitive ability by design, one is expected to find two High and two Low subjects at each school. Figure 8 reports the excess High subjects one is expected to find at each school given the strategies used in the experiment. A positive value indicates that more High subjects are admitted at that school on average. A negative value indicates that more Low subjects are admitted at that school on average. To control for the effect of different lottery draws and different session compositions, we use the allocations obtained through the recombinant strategies technique.

The figure shows that under NI around 2.3 High subjects are admitted at school C, while only around 1.7 are admitted at school D. In other words, school C admits at least 30 percent more High subjects relative to school D under NI. The same figure drops to around 15 percent under I. Table 8 reports

FIGURE 8: EXPECTED NUMBER OF HIGH PEERS



Notes: if High and Low play in a similar way, one can expect to find exacltly 2 High students at each school. The figure shows the excess High subjects, relative to 2, that one is expected to find at each school, given the strategies used, in case exactly half of the subjects were High. The average is computed using a recombinant strategies procedure with 1000 recombinations for each subject in each period, and an identical number of tie breakers. The average is weighted to account for the different proportions of High subjects in different treatments.

the difference in the probability that High and Low subjects have of being admitted at each school, and the corresponding tests of significance, based on the marginal effects obtained from a multinomial logit model. Under NI, we find High subjects are significantly more likely than Low ones to be admitted at school C, and the converse holds for school D. The differences in these probabilities shrink under I. In particular, in both preference environments Low subjects are not significantly more likely to be admitted at school D. Thus, while ability segregation is not eliminated under I, it reduces relative to NI.

Result 4. *Low subjects are significantly more likely than High subjects to be admitted at school D under* NI, *but not under* I.

TABLE 8: DIFFERENCES IN PROBABILITY OF ASSIGNMENT AT EACH SCHOOL

		School A	School B	School C	School D
High vs Low	NI-P1	.014	-.025	**.086**	**-.074**
		(.036)	(.038)	**(.032)**	**(.038)**
	I-P1	-.037	-.042	.037	-.041
		(.039)	(.036)	(.038)	(.031)
	NI-P2	.016	.008	**.077**	**-.010**
		(.026)	(.048)	**(.048)**	**(.032)**
	I-P2	-.033	.015	**.050**	-.032
		(.046)	(.026)	**(.022)**	(.025)
NI-P1 vs I-P1	Low	.022	-.028	.016	-.010
		(.025)	(.025)	(0.28)	(.029)
	High	-.029	.040	-.033	.023
		(.031)	(.030)	(.026)	(.026)
NI-P2 vs I-P2	Low	.030	-.005	.010	.035
		(.029)	(.029)	(.023)	(.021)
	High	-.020	.002	-.017	.035
		(.025)	(.027)	(.031)	(.023)

Notes: each cell in the table can be interpreted as the estimated difference in the probability of being admitted at each school. Estimates come from a multinomial logit model. The top panel shows the difference between High and Low subjects within each treatment. The bottom panels show the difference between NI and I for Low and High subjects. Robust standard errors in parentheses. Bold indicates significance at the .05 level.

5 Conclusions

Being admitted to a good school may have substantial effects on the educational achievement of children and their life opportunities—or so many parents believe. Because of that, fairness and equality concerns play a central role in the choice of school allocation mechanisms. Theory suggests that the Boston mechanism can harm subjects who are less able to game the system. The experimental evidence reported here confirms this hypothesis and shows that subjects of lower cognitive ability fare worse and end up segregated in the worse school under the Boston mechanism.

Recently, a number of papers have proposed modifications to the Boston mechanism in order to make it less penalizing towards strategic mistakes [Dur, 2013; Mennle and Seuken, 2014; Miralles, 2009]. These methods entails some efficiency costs and also make the mechanism less simple to convey to a large and diverse group of applicants. A possible alternative consists of finding methods that help applicants to identify optimal strategies. One such method is to provide students with enhanced information. We show that, indeed, disclosing information about past applications at schools help students to improve their strategic performance. However, because students of both higher and lower ability improve, the gap between the two shrinks only marginally. In fact, enhanced information may even harm those at the very bottom of the distribution of cognitive ability, since they are less able to use the additional information and are left further behind.

Thus, making information about past demands more accessible can improve performance under the Boston mechanism. We show it could also reduce ability segregation across schools. However, it will probably not be sufficient in order to level the playing field between those who can and those who cannot strategize well.

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A Pseudo-equilibria

Pseudo-equilibrium BOS-P1. By assumption on the naive, there are 6 students of type 1, 3 and 4 who report $A \succ_i B \succ_i C \succ_i D$ and 2 of type 2 who report $B \succ_i A \succ_i C \succ_i D$. We claim, that a strategy profile where the 6 sophisticated students of type 1, 3 and 4 report $A \succ_i C \succ_i B, D$ while 2 sophisticated of type 2 report $B \succ_i C \succ_i A, D$ is the unique equilibrium for the sophisticated students, if we take the reports of naive students as given.

Note that A will be filled in round one as there are at least 4 applications by naive students alone. Moreover, in any equilibrium B will be filled in the first round – otherwise a sophisticated player would switch and apply at B. Then, all sophisticated should in equilibrium rank C second, as it is the best school that has seats available at this point.

The only question that remains, is how many students will apply at A and at B in the first round. Suppose $\#A = 12$, $\#B = 4$, with only types 2 applying at B. A type 2, who applies at B in our candidate profile, is admitted at B and earns her maximal possible payoff - any deviation makes her strictly worse off.

If a sophisticated player looses in the first round at A the number of applicants at C in round 2 can be between 2 (if on other sophisticated student was rejected at A) and 6 (if 5 others were rejected). The probability that one other sophisticated student is rejected at A (and 4 other accepted) is

$$\frac{7 \cdot 4 \cdot 3 \cdot 2 \cdot 1}{11 \cdot 10 \cdot 9 \cdot 8 \cdot 7} \cdot 5 = \frac{1}{66}.$$

The probability that two other sophisticated students are rejected is

$$\frac{7\cdot 6\cdot 4\cdot 3\cdot 2}{11\cdot 10\cdot 9\cdot 8\cdot 7}\cdot\frac{5\cdot 4}{2}=\frac{2}{11}.$$

The probability that three other sophisticated students are rejected is

$$\frac{7\cdot 6\cdot 5\cdot 4\cdot 3}{11\cdot 10\cdot 9\cdot 8\cdot 7}\cdot\frac{5\cdot 4}{2}=\frac{5}{11}.$$

The probability that four other sophisticated students are rejected is

$$\frac{7\cdot 6\cdot 5\cdot 4\cdot 4}{11\cdot 10\cdot 9\cdot 8\cdot 7}\cdot 5=\frac{10}{33}.$$

The probability that five other sophisticated students are rejected is

$$\frac{7\cdot 6\cdot 5\cdot 4\cdot 3}{11\cdot 10\cdot 9\cdot 8\cdot 7}=\frac{1}{22}.$$

Then the conditional probability of acceptance at C (after rejection at A) is

$$\frac{1}{66}+\frac{2}{11}+\frac{5}{11}+\frac{10}{33}\frac{4}{5}+\frac{1}{22}\frac{4}{6}=\frac{61}{66}$$

and the expected payoff for a sophisticated student of type 1, or 4 applying at A is at least

$$\frac{4}{12}p(4,A)+\frac{8}{12}\frac{61}{66}p(4,C)=9.0303,$$

and the expected payoff for a student of type 3 is

$$\frac{3}{12}p(4,A)+\frac{8}{12}\frac{61}{66}p(3,C)=10.26.$$

If i of type 1, 3 or 4 switches and applies at B, there will again be between 2 and 6 applicants at C in round two, depending on the number of rejected sophisticated types at A. The probability that 4 sophisticated students are rejected at A (and 1 other accepted) is, as above, equal to $\frac{10}{33}$. The probability that 5 sophisticated are rejected is also equal $\frac{1}{22}$. We will use these probabilities to derive lower bound on the probability that a student who switched to B ends up at D - this will yield an upper bound on the expected payoff and show that the deviation is not profitable.

Assume that $l_i = 16$, i.e. assume that i draws the lowest lottery number. Then she is rejected at B (where she is one out of now 5 applicants) and will also be rejected at C whenever there are 5 or 6 applicants in round two, i.e.

whenever 4 or 5 sophisticated types have been rejected at A.

Assume that $l_i = 15$. Then she will be rejected at B if all other applicants have lottery numbers between 1 and 14, i.e. with probability $\frac{14\cdot13\cdot12\cdot11}{15\cdot14\cdot13\cdot12} = \frac{11}{15}$. Note that if she is rejected, one of the 7 students rejected at A has lottery number 16. If then there are 5 applicants at C, she will be rejected if none of the 4 rejected sophisticated types that where rejected at A has lottery number 16. If then there are 6 applicants at C, she will be rejected if none of the 5 rejected sophisticated types that where rejected at A has lottery number 16.

Combining all cases, we get a lower bound on the probability that i ends up at D of

$$\frac{1}{16}\left(\frac{10}{33}+\frac{1}{22}\right)+\frac{1}{16}\left(\frac{11}{15}\left(\frac{3}{7}\frac{10}{33}+\frac{2}{7}\frac{1}{22}\right)\right)=\frac{23}{1056}+\frac{11}{1680}.$$

This yields an upper bound on the expected payoff of types 1 and 4 of

$$\frac{4}{5}p(4,B)+\left(\frac{1}{5}-\frac{23}{1056}-\frac{11}{1680}\right)p(4,C)=9.0300.$$

A type 3 who would switch to B would get at most

$$\frac{4}{5}p(3,B)+\left(\frac{1}{5}-\frac{23}{1056}-\frac{11}{1680}\right)p(3,C)=9.37.$$

Hence, for any i of type 1, 3 or 4, switching to B lowers her expected payoff.

In the same way, for any other profile where $\#A < 12$, and $\#B > 4$, sophisticated students of type 1, 3 and 4 would deviate and apply at A. The only thing left to show, is that there cannot be an equilibrium where $\#A = 12$, and $\#B = 4$ and some type 2 applies at A. There, she would earn $\frac{4}{12}16 + \frac{8}{12}\frac{61}{66}6 = 9.0303$ (as we calculated for type 4 above), while a switch to B would earn her at least $\frac{4}{5}p(2,B) = 13.6$. This completes the proof.

Pseudo-equilibrium BOS-P2. By assumption on the naive, there are 6 students who report $A \succ_i B \succ_i C \succ_i D$ and 2 students who report $A \succ_i B, C \succ_i D$. We claim, that a strategy profile where the 4 sophisticated students of type 1 and 4 report $A \succ_i C \succ_i B, D$ and 4 sophisticated of type 2 and 4 report $B \succ_i C \succ_i A, D$ is the unique equilibrium for the sophisticated students, if we take the reports of naive students as given.

Note that A will be filled in round one as there are at least 8 applicants by naive students alone.

Claim 1. *There is no equilibrium with* $\#A = 8$.

Proof of Claim: Assume $\#A = 8$, so that all sophisticated types apply at B or C. If $\#B \leqslant 5$, any type 1, 2, 4 who applies at C would switch and apply at B instead, as $\frac{4}{6} \cdot 11 > 7$. However, for $\#B \geqslant 6$, no type 1 would apply at B or C, as a switch to A yields at least $\frac{4}{9} \cdot 20 + \frac{5}{9}\frac{2}{4} \cdot 7 \approx 10.83$ while applying at B yields at most $\frac{4}{6} \cdot 11 + \frac{2}{6} \cdot 7 \approx 9.33$ and applying at C yields 7 – a contradiction. $\Diamond$

Claim 2. *There is no equilibrium with* $\#A = 9$.

Proof of Claim: Assume $\#A = 9$. If $\#B \leqslant 5$, any type 1, 2, 4 who applies at C would switch and apply at B instead, as $\frac{4}{6} \cdot 11 > 7$. However, for $\#B \geqslant 6$, no type 1 would apply at B or C, as a switch to A yields at least $\frac{4}{10} \cdot 20 + \frac{6}{10}\frac{2}{4} \cdot 7 = 10.1$ while applying at B yields at most $\frac{4}{6} \cdot 11 + \frac{2}{6} \cdot 7 \approx 9.33$ and applying at C yields 7 – a contradiction. $\Diamond$

Claim 3. *There is no equilibrium with* $\#A = 10$.

Proof of Claim: Assume $\#A = 10$. If $\#B = \#C = 3$, any type 1, 2, 4 who applies at C would switch and apply at B instead, as $11 > 7$. If $\#B = 5$, $\#C = 1$, and both naive students of type 3 rank C second, no type 1 or 3 would apply at B where they earn less than $\frac{4}{5} \cdot 11 + \frac{1}{5} \cdot 7 = 10.2$ and instead apply at A where they receive

$$\begin{aligned}
&\frac{4}{11} \cdot 20 + \frac{7}{11}\left(\frac{3}{5}\left(\frac{6}{10}\frac{5}{9}\frac{4}{8}\frac{3}{7}\right) + \frac{3}{4}\left(\frac{6}{10}\frac{5}{9}\frac{4}{8}\frac{4}{7} \cdot 4\right)\right. \\
&\qquad\qquad \left. + 1\left(1 - \frac{6}{10}\frac{5}{9}\frac{4}{8}\frac{3}{7} - \frac{6}{10}\frac{5}{9}\frac{4}{8}\frac{4}{7} \cdot 4\right)\right)7 \\
&= \frac{80}{11} + \frac{49}{11}\left(\frac{92}{105}\right) \approx 11.2
\end{aligned}$$

or, in case of type 3,

$$\frac{4}{11}16 + \frac{7}{11}\left(\frac{92}{105}\right)11 \approx 11.95.$$

If $\#B > 5$ or if less than two naives rank C second, this only increases the incentives to switch to A. Hence, any equilibrium with $\#A = 10$ would have $\#B = 4$ and $\#C = 2$.

But who would be willing to apply at C? No type 3 - if they would deviate and apply at A, they could earn 11.95 (see above). Similarly, any other type applying at C would deviate and receive at least

$$\frac{4}{11}16 + \frac{7}{11}\left(\frac{92}{105}\right)7 \approx 9.7 > 7.$$

Hence, there is no equilibrium with $\#A = 10$. $\Diamond$

Claim 4. *There is no equilibrium with* $\#A = 11$.

Proof of Claim: Assume $\#A = 11$. If $\#B < 4$, any type 1, 2, 4 who applies at C would switch and apply at B instead, as $11 > 7$. If $\#B = 5$, $\#C = 0$ and both naive type 3 rank C second, no type 1 or 3 would apply at B where they earn at most $\frac{4}{5} \cdot 11 + \frac{1}{5} \cdot 7 = 10.2$ and instead apply at A where they receive

$$\begin{aligned}
&\frac{4}{12}20 + \frac{8}{12}\left(\frac{4}{6}\left(\frac{7}{11}\frac{6}{10}\frac{5}{9}\frac{4}{8}\frac{3}{7}\right) + \frac{4}{5}\left(\frac{7}{11}\frac{6}{10}\frac{5}{9}\frac{4}{8}\frac{4}{7}\cdot 5\right)\right.\\
&\qquad \left. +1\left(1 - \frac{7}{11}\frac{6}{10}\frac{5}{9}\frac{4}{8}\frac{3}{7} - \frac{7}{11}\frac{6}{10}\frac{5}{9}\frac{4}{8}\frac{4}{7}\cdot 5\right)\right)7\\
&= \frac{80}{12} + \frac{56}{12}\left(\frac{61}{66}\right) \approx 10.98
\end{aligned}$$

or, in case of type 3,

$$\frac{4}{12}16 + \frac{8}{12}\left(\frac{61}{66}\right)11 \approx 12.1.$$

If less than two naives rank C second, this only increases the incentives to switch to A. We are left with the case $\#A = 11$, $\#B = 4$ and $\#C = 1$, but then no one would be willing to apply at C - not even type 3, who would receive

$$\frac{4}{12}16 + \frac{8}{12}\left(\frac{61}{66}\right)11 \approx 12.1$$

when switching to A. $\Diamond$

Now, let us consider our candidate profile where $\#A = 12$, $\#B = 4$. We just saw that type 1 and 3 are wiling to apply at A (rather than switch back to B or C). To see that type 2 and 4 are willing to apply at B, check that their expected payoff is 15 and 11 respectively. If they were to switch to A, they would receive at most $\frac{4}{13} \cdot 16 + \frac{9}{13} \cdot 7 = 9.8$ (ranking B second is even less profitable as there will be many naives at B in round 2). This establishes our candidate profile as an equilibrium.

Could there be an equilibrium with $\#A = 12$, $\#B = 4$ where some type 2 or 3 applies at A? No: If both naives rank C second, a type 2 or 3 at A would earn

$$\frac{4}{12}16 + \frac{8}{12}\left(\frac{61}{66}\right)7 \approx 9.65,$$

whereas a switch to B yields at least

$$\frac{4}{5}11+\frac{1}{5}\underbrace{\left(1-\frac{7}{11}\frac{6}{10}\frac{5}{9}\frac{4}{8}\frac{3}{7}-\frac{7}{11}\frac{6}{10}\frac{5}{9}\frac{4}{8}\frac{4}{7}\cdot 5\right)}_{\mathbb{P}(\leqslant 3 \text{ applicants from A at C in round two})}7$$
$$=\frac{44}{5}+\frac{7}{5}\left(\frac{43}{66}\right)\approx 9.71.$$

If less than two naives rank C second, applying at A yields at most $\frac{4}{12}16+\frac{8}{12}7=10$ whereas a switch to B yields at least

$$\frac{4}{5}11+\frac{1}{5}\left(1-\frac{7}{11}\frac{6}{10}\frac{5}{9}\frac{4}{8}\right)7=\frac{44}{5}+\frac{7}{5}\left(\frac{59}{66}\right)\approx 10.05.$$

Finally, could there be an equilibrium with $\#A > 12$? No. This would require that some sophisticated type 2 or 3 applies at A, but we just saw that there are not even willing to do this even when $\#A = 12$.

B Experimental materials

Instructions

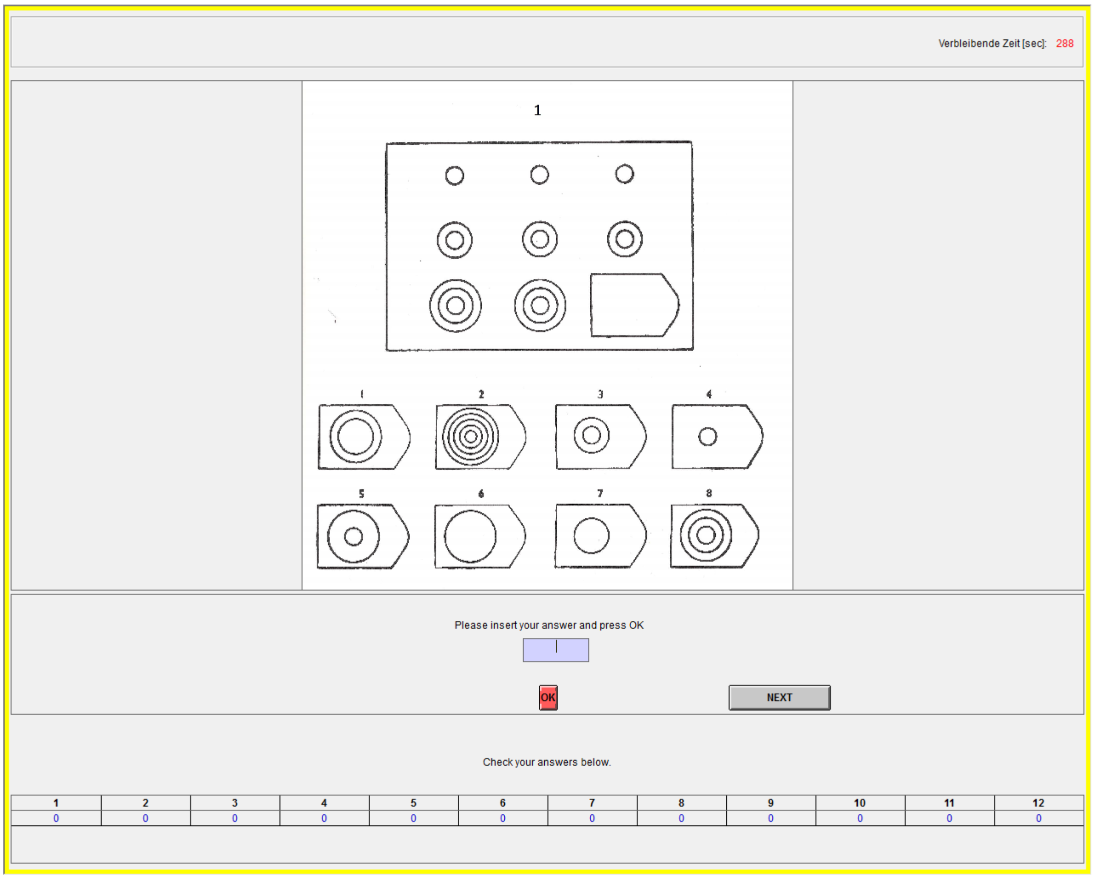
Welcome to this experiment in decision-making. You will receive 5 Euros as a show-up fee. Please, read carefully these instructions. The amount of money you earn depends on the decisions you and other participants make. In this experiment, on top of the show-up fee, you can earn between 0 and 26.30 Euro. In the experiment you will earn ECU (Experimental Currency Units). At the end of the experiment we will convert the ECU you have earned into Euro according to the rate: 1 ECU = 0.7 EURO. You will be paid your earnings privately and confidentially after the experiment. Throughout the experiment you are not allowed to communicate with other participants in any way. If you have a question please raise your hand. One of us will come to your desk to answer it.

TASK 1

On the sheet of paper on your desk you see a puzzle: a matrix with 8 graphic elements and an empty slot. There are eight possible numbered elements that could fill the empty slot. Only one is correct. Your task is to identify the element that correctly solves the puzzle. You choose the element you want by typing the corresponding number and pressing OK.

You will face 36 such puzzles, divided in three blocks of 12 puzzles each. Within each block, you can move back and forth through puzzles even without solving them, and change the answers you have given before. You have five minutes to complete blocks 1 and 2, and eight minutes to complete block 3. For each puzzle you correctly solve you earn 0.1 ECU. You will be informed about your score and earnings at the end of the experiment.

FIGURE 9: SCREENSHOT OF A QUESTION IN THE RAVEN TEST



TASK 2

On the sheet of paper on your desk you see a field composed of 100 numbered boxes. You earn 0.1 ECU for every box that is collected. Every second a box is collected, starting from the top-left corner. Once collected, the box disappears from the screen and your earnings are updated accordingly. At any moment you can see the amount earned up to that point.

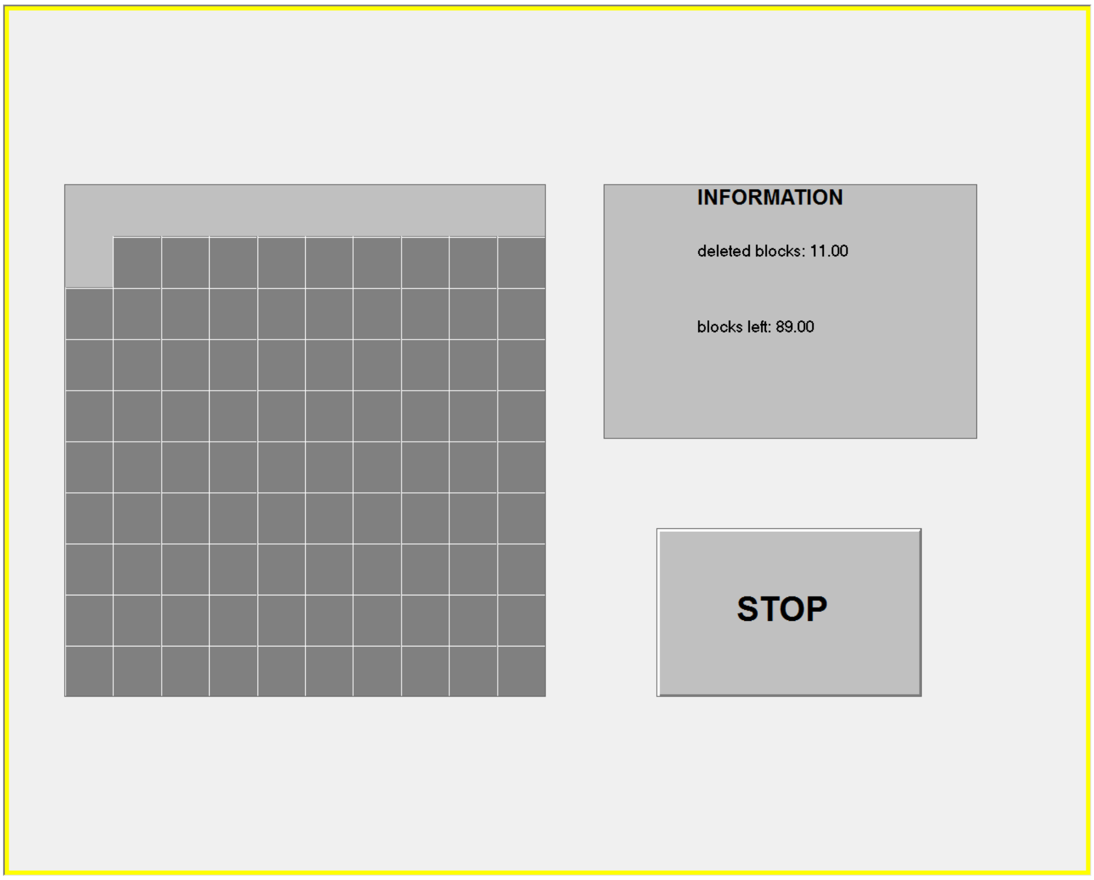
Such earnings are only potential, however, because behind one of these boxes hides a time bomb that destroys everything that has been collected.

You do not know where this time bomb lies. You only know that the time bomb can be in any place with equal probability: the computer will randomly determine the number of the box containing the time bomb. Moreover, even if you collect the time bomb, you will not know it until the end of the experiment.

Your task is to choose when to stop the collecting process. You do so by hitting 'Stop' at any time.

If you happen to have collected the box where the time bomb is located, you will earn zero. If the time bomb is located in a box that you did not collect you will earn the amount of money accumulated when hitting 'Stop'. We will start with a practice round. After that, the paying experiment starts.

FIGURE 10: SCREENSHOT OF THE BOMB RISK ELICITATION TASK



TASK 3

In this part of the experiment, we model a procedure to allocate seats at schools to students. Each student has to submit an application form to apply for a seat at a school. You and the other participants take the role of students. An assignment procedure that we will explain in detail below, decides, based on the application forms submitted by you and the other 15 participants, who receives a seat at which school.

There are 10 Rounds, in which you will apply anew for a seat at a school. All rounds are independent: where you are admitted, depends only on the application forms submitted in this round. Your chances in the current round are not influenced by your own decisions or the decisions of other participants in previous rounds. At the end of the experiment, one round is selected randomly. Your payoff for Task 3 depends on the school that you have been admitted to in that round.

Earnings: rounds 1-5

In each round, you and the remaining 15 participants apply for one of 16 seats. These are distributed over 4 schools – A, B, C, D – where each school has 4 seats. The earnings of a student admitted at a school depends on his type. There are 4 types of students – 1, 2, 3, 4 – with 4 students of each type. The type of a participant will be randomly drawn in each round. The earnings of a student, depending on his type and the school the he is admitted to, are summarized in the table below.

ECU for a seat at school	A	B	C	D
Type 1	20	10	6	0
Type 2	16	17	6	0
Type 3	16	10	8	0
Type 4	16	10	6	0

You can read the table as follows: in a round where you are a student of type 2 and are admitted at school C you earn 6 ECU - if this round is chosen to be paid out, this amount will be converted to Euros and paid out at the end of the experiment. In the same way, a student of type 3 that is admitted at school A receives a payoff of 16 ECU.

The payoffs above remain unchanged for the first 5 rounds. In rounds 6-10, there is different payoffs table, which you will see on the screen.

Available decisions

In each round, you have to submit an application form. To do so, you have to fill in under 'first choice', 'second choice', 'third choice' and 'fourth choice' the name of the respective school: 'A','B','C' or 'D'. This ranking determines the order with which your applications are sent to the schools, and, through the procedure outlined below, the school you are assigned to. You are free to choose the order in which you rank schools. When you are done, confirm your list by clicking 'submit'.

The assignment procedure

Once all application forms have been submitted, each student draws a lottery number from 1 to 16: each number is drawn once. Each student has the same chances. For the assignment, students with a lower lottery number receive preferential treatment over students with a higher lottery number.

The assignment of participants to available seats works as follows:

phase 1:

- Application by students. Each student applies at the school that he ranked as first choice on his application form.

- Admission. If at most 4 students listed a school as first choice, all of them receive a seat at that school. If more students listed a school as first choice, than the school has seats, the seats at that school are given to the students with the lowest lottery numbers. Students, who receive a seat in phase 1 are admitted for good; for them, the assignment procedure is over. Applicants that do not receive a seat move to the next phase.

phase 2:

- Application by students. Every student, who has not been assigned a seat in phase 1, applies at the school that he ranked as second choice on his application form.

- Admission. If in the second phase there are at most as many applicants as free seats at the school, all of them receive a seat at the school. If there are more applicants than free seats, the remaining free seats are given to the students with the lowest lottery numbers. If there are no free seats left, no applicant receives a seat at the school. Students, who receive a seat in phase 2 are admitted for good; for them, the assignment procedure is over. Applicants that do not receive a seat move to the next phase.

phase 3:

- Application by students. Every student, who has not been assigned a seat in phases 1 and 2, applies at the school that he ranked as third choice on his

application form.
- Admission. If in the third phase, there are at most as many applicants as free seats at the school, everyone receives a seat at the school. If there are more applicants in the third phase than free seats, the remaining free seats are given to the students with the lowest lottery number. If there are no free seats left, no applicant receives a seat at the school. Students, who receive a seat in phase 3 are admitted for good; for them, the assignment procedure is over. Applicants that do not receive a seat move to the next phase.

phase 4:
- Application by students. Every student, who has not been assigned a seat in phases 1, 2 and 3, applies at the school that he ranked as fourth choice on his application form.
- Admission. Since there are 16 applicants and 16 seats, there are as many free seats in phase 4 as applicants. Everyone receives a seat.

After every round you are informed about your lottery number and about the school where you received a seat. Then the next round starts.

Example

To illustrate the procedure described above, we consider an example. In this example, there are 8 students and 4 schools – V, W, X, Y – with 2 seats each to be assigned. Each Student draws a lottery number between 1 and 8.

student	Lottery number	First choice	Second choice	Third choice	Fourth choice
1	7	W	V	Y	X
2	5	V	W	X	Y
3	2	X	V	Y	W
4	8	V	X	Y	W
5	1	V	Y	W	X
6	3	X	W	Y	V
7	6	X	W	Y	V
8	4	V	Y	X	W

phase 1:
- Student number 1 applies at his first choice, school W. Since he is the only applicant there for two seats, he is accepted.
- Students number 2, 4, 5 and 8 apply at school V, that has only 2 seats available. The students with the two lowest lottery numbers (Student number 5

and 8) are accepted at school A. Students number 2 and 4 receive no seat in this phase.
- Students number 3, 6 and 7 apply at school X, that also has two available seats. Since there are more applicants than available seats, students with the lowest lottery numbers (students number 3 and 6) are accepted at X. Student number 7 receives no seat in this phase.
- The assignment procedure ends for students number 1, 3, 5, 6, and 8, who all received a seat at a school. Students number 2, 4 and 7 have received no seat in this phase and move to the next phase.

phase 2:
- Students number 2, 4 & 7 have no seat yet and apply at their second choice.
- Students number 2 and 7 apply at school W, where there is one free seat available. This is assigned to the student with the lowest lottery number (student number 2).
- Student number 4 applies at school X. There, there are no free seats.
- Students number 4 and 7 have received no seat in this phase and move to the next phase.

phase 3:
- Students number 4 and 7 apply at their third choice school, school Y, and are admitted, as school D has two free seats available. With this, the assignment procedure ends.

We arrive at the following assignment:

Student number	1	2	3	4	5	6	7	8
school	W	W	X	Y	V	X	Y	V

We start with a short quiz and an example. Then we begin with round 1.

C Further results

Table 9: Treatment and cognitive ability: regression tables (continuous Raven score)

	Dep. Var.: Expected payoff					
		P1			P2	
	(1)	(2)	(3)	(4)	(5)	(6)
I	0.106	-1.362**	-0.644	0.158	-1.019	-0.985
	(0.146)	(0.555)	(0.709)	(0.156)	(0.599)	(0.542)
Raven	0.0551***	0.0379***	0.0488***	0.0501***	0.0363**	0.0351***
	(0.0142)	(0.0146)	(0.0118)	(0.0129)	(0.0151)	(0.0124)
[1em] I*Raven		0.0503**	0.0255		0.0403**	0.0402**
		(0.0227)	(0.0262)		(0.0192)	(0.0172)
age			-0.0293			-0.0273
			(0.0180)			(0.0162)
female			-0.310			0.0160
			(0.185)			(0.133)
period			0.00964			0.0508
			(0.0291)			(0.0424)
Type 2			1.579***			-0.0715
			(0.316)			(0.246)
Type 3			-1.648***			-0.0719
			(0.152)			(0.282)
Type 4			-2.151***			-2.174***
			(0.167)			(0.179)
order			0.172			-0.256
			(0.114)			(0.132)
BRET			0.0104			0.00745
			(0.00616)			(0.00621)
_cons	7.914***	8.513***	8.987***	8.204***	8.756***	9.633***
	(0.333)	(0.324)	(0.667)	(0.388)	(0.429)	(0.604)
Obs. (groups)	960 (12)	960 (12)	960 (12)	960 (12)	960 (12)	960 (12)

Notes: the dependent variable is computed using recombinant strategies procedure with 1000 recombinations for each subject in each period, and an identical number of tie breakers. In parentheses we report robust standard errors, clustered at the session level. *,**,***: statistically significant at the 10%, 5% and 1% level, respectively.