REM WORKING PAPER SERIES

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REM Working Paper 098-2019

September 2019

REM – Research in Economics and Mathematics
Rua Miguel Lúpi 20,
1249-078 Lisboa,
Portugal

ISSN 2184-108X

Improving Schools through School Choice: An Experimental Study of Deferred Acceptance

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September 28, 2019

Abstract

In the context of school choice, we experimentally study the student-optimal stable mechanism where subjects take the role of students and schools are passive. Specifically, we study if a school can be better off when it unambiguously improves in the students' *true* preferences and its (theoretic) student-optimal stable match remains the same or gets worse. Using first-order stochastic dominance to evaluate the schools' distributions over their *actual* matches, we find that schools' welfare almost always changes in the same direction as the change of the student-optimal stable matching, i.e., incentives to improve school quality are nearly idle.

Keywords: school choice, matching, deferred acceptance, school quality, stability.

JEL–Numbers: C78, C91, C92, D78, I20.

1 Introduction

In many public school choice programs over the world children are assigned to public schools on the basis of (parents') preferences and the priorities of children for different schools (based on, e.g., walking distance, siblings, etc.). Abdulkadiroğlu and Sönmez (2003) advocate the use of centralized mechanisms, and in particular the student-optimal stable mechanism.[1] In this context,

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[1]This is the mechanism based on the deferred algorithm (Gale and Shapley, 1962) where students make proposals to schools. See also Roth (2008).

Hatfield et al. (2016) consider the incentives for schools to improve their quality. A mechanism is said to respect improvements of school quality if a school becomes better off whenever that school improves, i.e., becomes more preferred by students. Hatfield et al. (Proposition 1, 2016) show that there is no stable mechanism that respects improvements of school quality at every school preference profile.[2] Their analysis assumes that students reveal their preferences truthfully, i.e., report the ranking that coincides with their true preferences. This assumption seems quite reasonable for the student-optimal stable mechanism as it is known to be strategy-proof.

However, the experimental literature has shown that under the student-optimal stable mechanism subjects do not always realize that it is in their best interest to reveal their preferences truthfully. For instance, the reported truth-telling rates are between 57% and 58% in Calsamiglia et al. (2010), 64% in Chen and Sönmez (2006), between 44% and 65% in Klijn et al. (2013), and between 67% and 82% in Pais and Pintér (2008).[3] There is also literature that uses field data and makes this observation, e.g., Chen and Pereyra (2019) and Fack et al. (2019) on the Mexico city and Paris high-school match, respectively, Hassidim et al. (2018) on the admission process on graduate studies in psychology in Israel, Rees-Jones (2018) on the matching of medical students to residencies in the US, and Shorrer and Sóvágó (2017) on the Hungarian college admissions.[4] Consequently, theoretical findings on the student-optimal stable mechanism that hinge on its strategy-proofness may not carry over to real-life applications. So, laboratory and field experiments seem necessary to test theory and provide further important insights.

In view of the above, we focus on the student-optimal stable mechanism and study whether the negative finding of Hatfield et al. (Proposition 1, 2016) also holds in the laboratory. We construct related school choice problems such that some school improves in the students' *true* preferences yet the (theoretic) student-optimal stable match is worse or the same. Our main result is that this type of improvement in the students' true preferences can indeed decrease the quality of the school's *actual* match. In fact, the distribution of students that are matched to the school after its improvement in the students' true preferences is often first-order stochastically dominated by the initial distribution.

The rest of the paper is organized as follows. In Section 2, we describe the experimental design, hypotheses, and procedures. In Section 3, we present and discuss our experimental results. The detailed experimental instructions are relegated to the Online Appendix.

2 Laboratory experiment

Design and Hypotheses

The experiment is based on four matching problems where students i_1, i_2, i_3, and i_4 each seek to obtain a seat at schools s_1, s_2, s_3, and s_4. Each school offers exactly one seat. The preferences of the students and the priorities of the schools in the four problems are depicted in Table 1.

[2]Considering a sequence of random markets that are "regular" and "sufficiently thick" and grow *infinitely large*, Hatfield et al. (Theorem 1, 2016) show that all stable mechanisms strongly respect improvements of school quality.

[3]Moreover, in a recent experimental study, Guillen and Veszteg (2019) show that much of the observed truth-telling comes from confused decision-makers following a default, very focal strategy.

[4]Another strand of literature explores factors behind the play of dominated strategies in strategy-proof environments. See, e.g., Basteck and Mantovani (2018) and Rees-Jones and Skowronek (2018) for cognitive ability.

Problem **1**	Preferences				Priorities			
	i_1	i_2	i_3	i_4	s_1	s_2	s_3	s_4
Best match	s_2	$\underline{s_4}$	s_4	$\underline{s_2}$	i_2	i_2	i_4	i_4
Second best	$\underline{s_3}$	s_3	$\underline{s_1}$	s_1	i_1	i_3	i_3	i_1
Third best	s_4	s_1	s_2	s_3	$\underline{i_3}$	$\underline{i_4}$	$\underline{i_1}$	$\underline{i_2}$
Worst match	s_1	s_2	s_3	s_4	i_4	i_1	i_2	i_3

Problem **2**	Preferences				Priorities			
	i_1	i_2	i_3	i_4	s_1	s_2	s_3	s_4
Best match	$\underline{s_2}$	$\underline{\boldsymbol{s_3}}$	$\underline{s_4}$	$\underline{s_1}$	i_2	i_2	i_4	i_4
Second best	s_3	$\boldsymbol{s_4}$	s_1	s_2	i_1	i_3	i_3	i_1
Third best	s_4	s_1	s_2	s_3	i_3	i_4	i_1	i_2
Worst match	s_1	s_2	s_3	s_4	$\underline{i_4}$	$\underline{i_1}$	$\underline{i_2}$	$\underline{i_3}$

Problem **3**	Preferences				Priorities			
	i_1	i_2	i_3	i_4	s_1	s_2	s_3	s_4
Best match	s_2	s_4	s_4	s_2	$\underline{i_2}$	i_2	$\underline{i_4}$	i_4
Second best	$\underline{\boldsymbol{s_4}}$	$\underline{\boldsymbol{s_1}}$	s_1	s_1	i_1	$\underline{i_3}$	i_3	$\underline{i_1}$
Third best	$\boldsymbol{s_3}$	$\boldsymbol{s_3}$	$\underline{s_2}$	$\underline{s_3}$	i_3	i_4	i_1	i_2
Worst match	s_1	s_2	s_3	s_4	i_4	i_1	i_2	i_3

Problem **4**	Preferences				Priorities			
	i_1	i_2	i_3	i_4	s_1	s_2	s_3	s_4
Best match	s_2	$\underline{s_4}$	s_4	$\boldsymbol{s_1}$	i_2	i_2	i_4	i_4
Second best	$\underline{s_3}$	s_3	$\underline{s_1}$	$\underline{\boldsymbol{s_2}}$	i_1	i_3	i_3	i_1
Third best	s_4	s_1	s_2	s_3	$\underline{i_3}$	$\underline{i_4}$	$\underline{i_1}$	$\underline{i_2}$
Worst match	s_1	s_2	s_3	s_4	i_4	i_1	i_2	i_3

Table 1: Students' preferences over schools and schools' priorities over students. The underlined matches constitute the student-optimal stable matchings. The bold-faced entries highlight the differences between preference profiles P^1 and P^3 (indicated in P^3), P^1 and P^4 (indicated in P^4), and P^2 and P^4 (indicated in P^2).

The four problems were constructed in such a way that they satisfy the following three properties. First, the priorities are identical in all four problems, which simplifies the task for experimental subjects as they all take the role of students and it allows us to compare for each school outcomes across problems.

Second, the problems allow for a unanimous comparison of the student-optimal stable matchings by the schools, which we explain next. For each $k = 1, 2, 3, 4$ and each school s, let $\mu^k(s)$ denote the student-optimal stable match of school s at problem k.[5] The underlined matches in Table 1 depict the student-optimal stable matchings. We assume that the priorities of the schools reflect their preferences, i.e., obtaining a higher priority student is preferred to a lower priority student. For every school s, let $\succ_s$ ($\succeq_s$) denote the strict (associated weak) preference relation.

Observation I: The schools rank the four student-optimal stable matchings in the same order: for each school s, $\mu^3(s) \succ_s \mu^1(s) = \mu^4(s) \succ_s \mu^2(s)$.

Third, the problems only slightly differ in preferences to study the effect of school improvements, which we explain next. For $k = 1, 2, 3, 4$, let P^k denote the students' strict preferences in problem k. Let $k, l \in \{1, 2, 3, 4\}$, $k \neq l$. Following Hatfield et al. (2016), we say that a school s^* improves (in the students' true preferences) moving from P^k to P^l if

i1. for each student i and each school $s \neq s^*$, $s^* P^k_i s \Rightarrow s^* P^l_i s$;
i2. for each student i and any two schools $s, s' \neq s^*$, $s P^k_i s' \Leftrightarrow s P^l_i s'$.

The bold-faced entries in Table 1 and Observation I help to identify the differences between preference profiles and to verify the following observation.

[5]We refer to the Online Appendix, Gale and Shapley (1962), and Roth (2008) for a description of the deferred acceptance algorithm to calculate the student-optimal stable matchings.

Observation II: There are exactly four situations (s, P^k, P^l) where school s improves in the students' preferences moving from P^k to P^l but $\mu^k(s) \succeq_s \mu^l(s)$. These situations are

a. (s_3, P^3, P^1): school s_3 improves from P^3 to P^1 but $\mu^3(s_3) \succ_{s_3} \mu^1(s_3)$;
b. (s_1, P^1, P^4): school s_1 improves from P^1 to P^4 but $\mu^1(s_1) = \mu^4(s_1)$;
c. (s_2, P^4, P^1): school s_2 improves from P^4 to P^1 but $\mu^4(s_2) = \mu^1(s_2)$;
d. (s_3, P^4, P^2): school s_3 improves from P^4 to P^2 but $\mu^4(s_3) \succ_{s_3} \mu^2(s_3)$.

If subjects are mostly truth-telling (whether or not being aware of strategy-proofness), then we should expect the actual matchings to be "close" to the student-optimal stable matchings so that the school improvements identified in situations a.–d. in Observation II do not lead to better actual matches for the schools.

Hypothesis H_0: Each improvement of a school in the students' true preferences identified in Observation II does *not* lead to a better actual match for the school.

On the other hand, in view of evidence from the experimental literature on the student-optimal stable mechanism, we can expect a substantial number of subjects to not truthfully reveal their preferences. Then, given that the school improvements described in Observation II are minimal and straightforward in the sense that only one school improves, one can conjecture that the actual match of the school improves.

Hypothesis H_1: Each improvement of a school in the students' true preferences identified in Observation II leads to a better actual match for the school.

Procedures

The experiment was programmed within the z-Tree toolbox provided by Fischbacher (2007) and carried out at Lineex (www.lineex.es) hosted at the University of Valencia. In total, 96 undergraduates participated in the experiment. We ran two sessions with 48 subjects. At the beginning of each session, subjects were randomly assigned into groups of four. Within each group, one subject was assigned the role of student i_1, another subject the role of student i_2, and so forth. Groups and roles did not change over the course of the experiment.

Participants were told that the experiment would take a total of 24 rounds and that preferences and priorities would change every six rounds. In both sessions, rounds 1 to 6 used preferences and priorities of problem 1, rounds 7 to 12 used preferences and priorities of problem 2, rounds 13 to 18 used those of problem 3, and rounds 19 to 24 used those of problem 4.[6] Before the first round, subjects went individually over an illustrative example in order to get used to the matching procedure. Afterwards, we implemented a trial round that was not taken into account for payment and that helped subjects to get familiar with the computer software. The problem played in the trial round 0 was different from problems 1–4. At the beginning of each of the 24 rounds, the computer screen presented the preferences of all group members and the priorities of the four schools. Subjects took then their respective decisions.

At the end of each round, each subject got to know his/her match and corresponding payoff. No explicit information about the behavior or the outcome of the other group members was provided.

[6] Note that this avoids that any two pairs of problems that we compare (as discussed in the design section, namely $\{P^1, P^3\}$, $\{P^1, P^4\}$, and $\{P^2, P^4\}$) be played consecutively.

At the end of the experiment, one round was randomly selected for payment. Subjects received 24, 20, 16, and 12 experimental currency units (ECU) if they ended up in their most, second most, third most, and least preferred school. Each ECU was worth 1 Euro. A session lasted about 120 minutes and subjects earned on average 23.50 Euro for their participation including a 3 Euro show-up fee. The detailed experimental instructions are relegated to the Online Appendix.

3 Results

In previous laboratory experiments it has been observed that a substantial part of subjects do not play the weakly dominant strategy of truth-telling. As Figure 1 shows, this is also the case in our experiment.

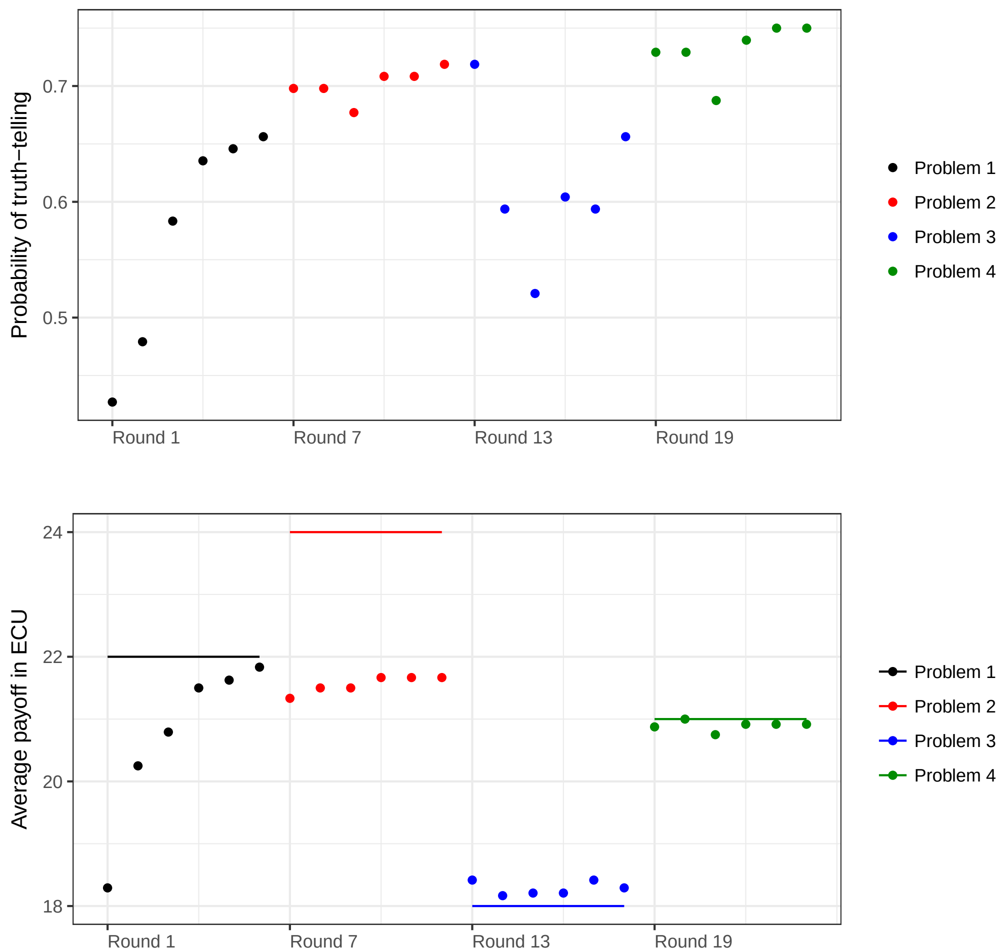

Figure 1: Truth-telling (upper panel) and average payoff (lower panel). The horizontal bars indicate the average payoff at the corresponding student-optimal stable matching.

More precisely, the average truth-telling rates are 57.12%, 70.13%, 41.45%, and 73.09% in problems 1, 2, 3, and 4, respectively. It can be observed that repetition leads to higher truth-telling rates in all problems but problem 3. Repetition also has an effect on the average payoff, which is depicted in the lower panel of Figure 1. In problem 1, the average payoff is initially well below the average payoff at the student-optimal stable matching, but repetition helps to close this gap. There are no visible learning effects for the other three problems. In comparison with the average payoff at the student-optimal stable matching, the average payoff in the experiment is far smaller in problem 2, slightly higher[7] in problem 3, and slightly smaller in problem 4.

We now turn to our hypotheses, which concern the effect of a school improvement (in the true students' preferences) on the actual student the school receives. Since the preferences of each school are fixed throughout, it is possible to make comparisons between the distributions over actual matches that the school receives at different problems. A distribution over matches can be conveniently described as follows. Given a school s, for $\ell = 1, 2, 3, 4$, let $q_s(\ell)$ denote the (cumulative) probability with which school s receives a match that is ranked ℓ-th or worse (below). For example, $q_{s_2}(3)$ is the probability that school s_2 receives student i_1 (ranked 4-th) or i_4 (ranked 3-rd). Note that for each school s, $q_s(1) = 1$. Then, a cumulative distribution function can be described by the vector $q_s = (q_s(4), q_s(3), q_s(2), 1)$, which consists of four weakly increasing probabilities. Figure 2 depicts in each panel the cumulative distribution functions obtained in the four problems for the corresponding school. The cumulative distribution function for school s in problem $k = 1, 2, 3, 4$, denoted q_s^k, is calculated from the actual matches of school s in all experimental groups and all rounds in which problem k was played. For instance, $q_{s_2}^1(3) \approx 0.85$.

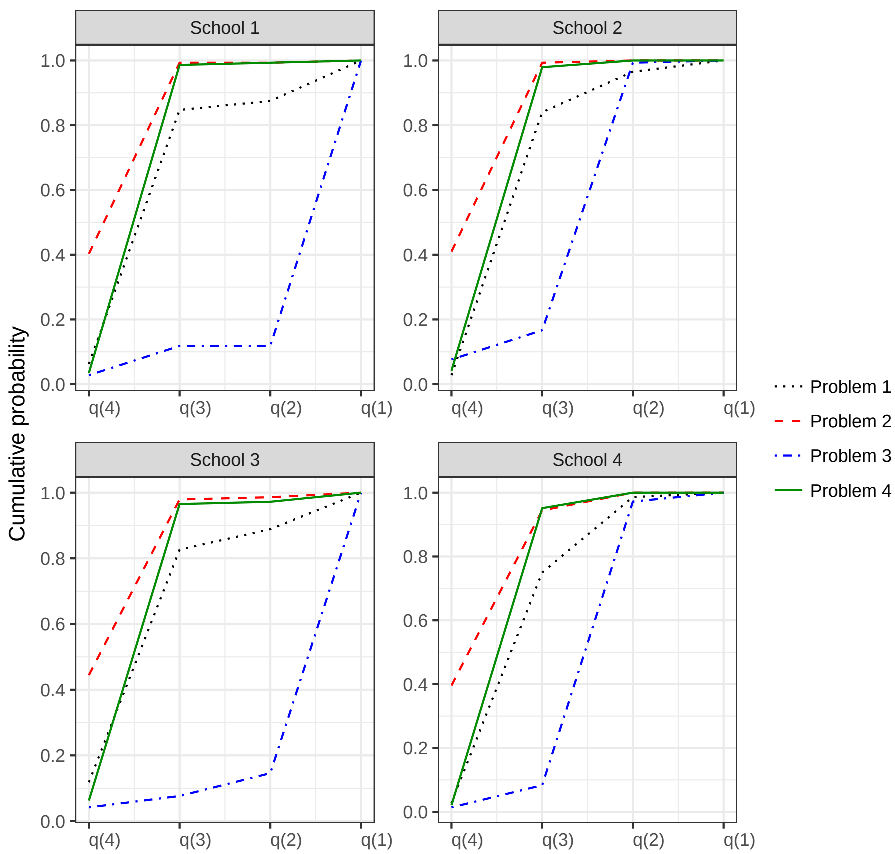

Figure 2: Cumulative distribution functions q_s^k for each school s and each problem $k = 1, 2, 3, 4$.

[7] This is possible because in problem 3 there are (unstable) matchings that Pareto-dominate the student-optimal stable matching.

To obtain the strongest possible conclusions concerning the evaluation of a school's matches, we do not make further assumptions on the intensity of the schools' preferences. In particular, we do not assume any particular utility functions for the schools. Then, to compare distributions over matches for a given school, a natural but demanding tool is that of first-order stochastic dominance. A cumulative distribution function q first-order stochastically dominates another cumulative distribution function $\tilde{q}$, denoted by $q \succ_{FOSD} \tilde{q}$, if for each $\ell = 1, 2, 3, 4$, $q(\ell) \leq \tilde{q}(\ell)$ and for some $\ell = 2, 3, 4$, $q(\ell) < \tilde{q}(\ell)$. In Figure 2, for any school and any two cumulative distribution functions q and $\tilde{q}$, $q \succ_{FOSD} \tilde{q}$ if and only if the graph of q lies below that of $\tilde{q}$. Our findings are as follows.

Result: With respect to the situations exhibited in Observation II,

a. $q^3_{s_3} \succ_{FOSD} q^1_{s_3}$;
b. $q^1_{s_1} \not\succ_{FOSD} q^4_{s_1}$ and $q^4_{s_1} \not\succ_{FOSD} q^1_{s_1}$;
c. $q^1_{s_2} \succ_{FOSD} q^4_{s_2}$;
d. $q^4_{s_3} \succ_{FOSD} q^2_{s_3}$.

In short, cases a, b, and d support the null hypothesis H_0, while case c supports the alternative hypothesis H_1. However, given that the order of play is 1, 2, 3, and 4, the finding in case c could be due to learning: if the decisions of the subjects are better at problem 4 than at problem 1, as suggested by the increased truth-telling rates in Figure 1, then schools could be expected to be worse off at problem 4.[8] All in all, we reject the alternative hypothesis H_1. So, in line with Hatfield et al. (Proposition 1, 2016), improvements of school quality under the student-optimal stable mechanism do not guarantee a better match for the school.

As a final remark, we note that even when a school improves in the students' true preferences *and* its student-optimal stable match gets better, the distribution of this school's matches does not get unambiguously better (in terms of first-order stochastic dominance): this is precisely what happens with school s_4 when we move from problem 2 to problem 4. Hence, even if in theory the match is better, it may not be so in the lab.

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