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Does disappointment aversion explain non-truthful reporting in strategy-proof mechanisms?

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Abstract

Disappointment aversion has been suggested as an explanation for non-truthful rankings in strategy-proof school-choice matching mechanisms. We test this hypothesis using a novel experimental design that eliminates important alternative causes of non-truthful rankings. The design uses a simple contingent choice task with only two possible outcomes. Between two treatments, we manipulate the possibility for disappointment aversion to have an effect on ranking. We find a small and statistically marginally significant treatment effect in the direction predicted by disappointment aversion. We therefore conclude that disappointment aversion is a minor contributor to non-truthful rankings in strategy-proof school-choice matching mechanisms.

Keywords Matching · Loss aversion · Strategy-proofness · Experiment · School choice · Market design

JEL Classification C72 · C91 · D47 · D91

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1 Introduction

Strategy-proof direct revelation mechanisms are hallmark achievements of mechanism design. Participants do not need to form beliefs about how other participants are going to act or what other participants' preferences (types) might be. Participants should simply report their preferences truthfully because they can do no better regardless of what the other participants do. That is, truthful reporting is a weakly dominant strategy. We focus on strategy-proof student-school (or analogous) matching mechanisms such as deferred acceptance (DA, Gale and Shapley, 1962), top trading cycles (TTC, Shapley and Scarf, 1974, originally proposed by Gale, see also Abdulkadiroğlu and Sönmez, 2003) or (random) serial dictatorship ((R)SD). In these mechanisms, students report their ordinal preferences by ranking the schools, while schools behave non-strategically by following their priorities over students. Strategy-proofness implies that we would expect students to submit rankings that coincide with their ordinal preferences.

There is growing evidence, however, that students frequently submit rankings that do not follow their ordinal preferences. This has been documented both in the lab (Chen and Sönmez, 2006; Pais and Pintér, 2008; Li, 2017) and in the field (Gross et al., 2015; Rees-Jones, 2017; Rees-Jones and Skowronek, 2018; Chen and Pereyra, 2019; Hassidim et al., 2021; Artemov et al., 2023; Shorrer and Sovago, 2023).[1] These non-truthful rankings present a serious challenge since they have the potential to undermine the desired properties of the resulting allocation. Moreover, they can introduce bias or noise to a policymaker's inference about the population distribution of preferences over schools, which can lead to a misallocation of resources across schools.

Hassidim et al. (2017) catalogue various explanations for non-truthful reporting in strategy-proof mechanisms, such as game-form misperception,[2] underestimation of the likelihood of obtaining favored schools, and "behavioral" preferences. It is important to understand what factors really lie behind non-truthful reporting in order to help us think about the directions in which to modify these mechanisms or their presentation in order to increase the incidence of truthful reporting.

This paper aims to contribute to this research program. In particular, we examine a prominent behavioral hypothesis that non-truthful reporting might be driven by "disappointment aversion" (Meisner and von Wangenheim, 2023). This hypothesis is based on the expectation-based loss aversion (EBLA) models of Köszegi and Rabin (2007, 2009). In this setting, each student cares not only about the particular school

[1] Hassidim et al. (2021), Artemov et al. (2023) and Shorrer and Sóvágó (2023) establish this observation relying purely on observational data on reported rankings, whereas Gross et al. (2015), Rees-Jones (2017), Rees-Jones and Skowronek (2018) and Chen and Pereyra (2019) complement the observational data with surveys to elicit true ordinal preferences. For recent surveys of this literature, see Hakimov and Kübler (2021) and Rees-Jones and Shorrer (2023). Table A.1 tabulates the rates of non-truthful ranking observed in the literature in strategy-proof mechanisms. Table A.2 tabulates analogous rates for non-strategy-proof mechanisms. Interestingly, some studies document that, under the same setting, non-strategy-proof mechanisms can result in higher rates of truthful ranking than strategy-proof mechanisms (e.g., Klijn et al., 2019; Bó and Hakimov, 2020; Cerrone et al., 2024).

[2] In general, game-form misperception is a failure to properly understand how players' actions map to their payoffs (Chou et al., 2009; Cason and Plott, 2014; Guillen and Veszteg, 2021).

they are assigned to ("consumption utility"), but also about how their realized assignment compares to a reference point ("gain-loss utility"). EBLA can explain why a student ranks a less-preferred school (in terms of consumption utility), where they have a higher probability of obtaining a seat, above a more preferred school, where they have a lower probability of obtaining a seat (e.g., "district school bias," Chen and Sönmez, 2006). By ranking the more preferred school higher, the student sets themselves up to expect to obtain a seat in that school with a higher probability than if they ranked this school lower. But because the student is still not very likely to obtain a seat in this school, they will more likely become disappointed in that their realized utility will fall short of their expected utility. Ranking the less-preferred school first is a way of endogenously reducing the probability and size of this disappointment. Formally, as shown by Meisner and von Wangenheim (2023) using the choice-acclimating personal equilibrium of Köszegi and Rabin (2007), a necessary, but not a sufficient, condition for optimality of a non-straightforward ranking is that students' preferences exhibit "loss dominance" (see Sect. 2).[3] Indeed, Dreyfuss et al. (2022) and Dreyfuss et al. (2023) document that EBLA explains observed data better than standard theory.

However, previous experiments have not been designed to distinguish the effect of disappointment aversion from other potential explanations of non-straightforward ranking. Our main contribution is in measuring the effect of disappointment aversion using a design that eliminates the impact of these alternative explanations, which we achieve by simplifying the underlying economic environment. We believe that such a simplification is necessary for proper identification, since more complex environments are susceptible to confounds, in particular game-form misperception. At the same time, our simple design allows us to maximize the effect of disappointment aversion on non-straightforward reporting within our setting. Moreover, we argue that the size of this effect can be used as an (approximate) upper bound for the size of the effect of disappointment aversion in more complex matching settings as well.

We simplify the matching environment in two ways. First, we restrict the environment to only two schools, one high-value and one low-value, with no possibility of remaining unmatched. Second, we reduce the mechanism to an equivalent contingent choice task. In doing so, we rely on the two-step framing of any strategy-proof matching mechanism proposed by Katuščák and Kittsteiner (2024). In the first step, based on the rankings submitted by other students and the non-strategic priorities of the schools, the mechanism determines which schools are obtainable for the applicant. This step does not use the ranking submitted by the applicant in any way. The set of obtainable schools is therefore exogenous to the applicant. In the second step, the ranking submitted by the applicant is used to pick

[3] In this context, (non-)straightforward ranking refers to ranking schools (not) according to their consumption utility. Consistent with other papers in the literature, we now switch to the "straightforward/non-straightforward" terminology rather than the "truthful/non-truthful" terminology because, in the presence of gain-loss utility, the format of the ranking report no longer allows a student to report all aspects of their preferences that would be necessary for a direct revelation mechanism to act in their best interest. In this constrained reporting environment, a non-straightforward ranking might be a more accurate reflection of the student's preferences than a straightforward ranking would be if the student cares sufficiently about the gain-loss utility and is sufficiently loss-averse (see Sect. 2). In a standard setting with only consumption utility, (non-)straightforward ranking coincides with (non-)truthful ranking.

the highest-ranked school from the set of obtainable schools. We simplify the decision situation by putting the applicant directly into the second step. In that step, the applicant faces one of three contingencies: either only one school is obtainable, or only the other, or both (none of them being obtainable is impossible). The probabilities of the three contingencies are given explicitly, without referring to notions of competing applicants, school priorities or acceptance thresholds. Without knowing which contingency is realized, the applicant submits one of two possible rankings of the two schools. If the top-ranked school happens to be obtainable, the applicant is assigned to that school. Otherwise they are assigned to the other school.

Our identification of the impact of disappointment aversion is based on a comparative statics prediction derived from the EBLA model of Köszegi and Rabin (2007) that the incidence of non-straightforward ranking increases with the difference between the probability of only the low-value school being obtainable and the probability of only the high-value school being obtainable, provided that this difference is positive. Based on this prediction, we use a between-subjects design with two treatments. In *Symmetric*, the two probabilities are similar, predicting no non-straightforward ranking due to disappointment aversion except for the most extremely loss-dominant students. In *Asymmetric*, the probability of only the low-value school being obtainable is much higher than the probability of only the high-value school being obtainable, predicting non-straightforward ranking due to disappointment aversion for all but the least loss-dominant students. Assuming that the incidence of noise and errors in ranking behavior is orthogonal to this probability variation, the difference in the rate of non-straightforward ranking between *Asymmetric* and *Symmetric* identifies the causal impact of disappointment aversion on non-straightforward ranking. Moreover, the treatment effect captures nearly the maximum extent of non-straightforward ranking that is possible due to disappointment aversion in any matching setting. It hence provides an approximate upper bound for the effect of disappointment aversion in both simple and complex matching settings.

To motivate the two-school simplification, note that in an environment with more than two schools, the data pattern that indicates disappointment aversion (a less-preferred school by consumption utility that is more obtainable is ranked above a more-preferred school that is less obtainable) can also be accounted for by the applicant "misperceiving" the mechanism. As a leading example, consider the "immediate acceptance" misperception under which the applicant believes they face the immediate acceptance, or Boston, mechanism. This can be justified by real-world experience from job, marital or dating markets that are all decentralized in nature and in which matching alternatives disappear over time unless pursued. Under this misperception, "chasing" a more-favored but lower-probability school instead of a less-favored but higher-probability school might be risky since the latter might fill up and leave the applicant at an even less-preferred school if the pursuit of the former fails. As a result, it might be optimal to rank these two schools in order of obtainability rather than consumption value, leading to non-straightforward ranking. By restricting the environment to only two schools, with no possibility of remaining unmatched, we eliminate the risk of "chasing" the more-preferred school since one is at worst guaranteed a seat in the other school.

The contingent choice task reduction simplifies the decision environment. It is aimed at further mitigation of mechanism misperception and other potential confounds of disappointment aversion. For example, by clearly communicating probabilities of various contingencies, we overcome the possible issue of an applicant believing that a low-priority or a highly-competitive school is impossible to obtain and hence, under an otherwise correct perception of strategy-proofness, that this school could without any loss be ranked anywhere or omitted from the ranking altogether (Chen and Pereyra, 2019). Also, by not using school priorities as sources of variation in the likelihood of obtainability of various schools, we avoid the possibility of an applicant having "reciprocal preferences," i.e., interpreting school priorities as proxies for how much each school likes them and modifying their own preferences over schools accordingly due to an update about an unobserved component of match quality or due to reciprocity (Opitz and Schwaiger, 2023a, b). Similarly, since the availability of seats is based on exogenous probabilities, we also avoid the possibility that subjects rank "less-competitive" schools above "more-competitive" ones so as to avoid a possible hit to "ego utility" that might result from a rejection from a more-competitive school (Köszegi, 2006; Moscariello, 2023).

To the extent that our identification is based on comparing rates of non-straightforward ranking in two different treatments, it might appear that our simplifications are not necessary since the impact of potential confounds would be differenced out. However, in a more complex environment, confounds such as game form misperception would interact with changes in obtainability of various schools in a way that would predict behavioral change in the same direction as disappointment aversion. This underlines why it is important to simplify the mechanism in order to eliminate or at least mitigate these confounds.[4]

We find an economically small and statistically marginally significant treatment effect in the direction predicted by disappointment aversion. The rate of non-straightforward ranking is 3.8% in *Symmetric* and 7.5% in *Asymmetric*.[5] The treatment effect of 3.72 percentage points is statistically marginally significant. The 95% confidence interval for the treatment effect is $(-0.72, 8.16)$ percentage points. In comparison, rates of non-straightforward ranking estimated in the experimental literature range from 14 to 75% (Table A.1).[6] Since the treatment effect provides an

[4] Another potential reason for non-straightforward reporting in strategy-proof mechanisms is failure of contingent thinking (see, for example, Esponda and Vespa, 2024). Under this hypothesis, a player has the correct perception of the mechanism, but cannot properly identify the contingencies they face or that there is a strategy that is a common best response to all of these contingencies. This explanation does not introduce a confound into our identification strategy since our two treatments are identical in terms of how easy is it to recognize the contingencies and find best responses to each of them.

[5] The experimental data and code for the analysis used to support the findings of this study have been placed in an open ICPSR repository (http://doi.org/10.3886/E189301V5) (Chen et al., 2023).

[6] This range is derived from Table A.1, which includes the following studies: Chen and Sönmez (2006), Pais and Pintér (2008), Calsamiglia et al. (2010), Klijn et al. (2013), Guillen and Hing (2014), Zhu (2015), Chen et al. (2016), Featherstone and Niederle (2016), Ding and Schotter (2017), Guillen and Hakimov (2017), Li (2017), Chen et al. (2018), Guillen and Hakimov (2018), Hakimov and Kesten (2018), Chen and Kesten (2019), Ding and Schotter (2019), Klijn et al. (2019), Bó and Hakimov (2020), Guillen and Veszteg (2021), Afacan et al. (2022), Cerrone et al. (2024). We exclude treatments in which subjects receive advice on how to play, in which subjects only receive information about their own (induced) preferences, in which

approximate upper bound on the extent of non-straightforward ranking due to disappointment aversion in any matching setting, we conclude that disappointment aversion in fact accounts for a small amount (approximately 3.72 percentage points) of the non-straightforward ranking typically observed in school-choice matching experiments (from 14 to 75%).

The rest of the paper is organized as follows. Section 2 introduces a theoretical framework that formally illustrates the comparative statics that the experimental design is based on. Section 3 describes details of the experimental design. Section 4 presents our empirical findings. Section 5 discusses the findings. Finally, Section 6 concludes.

2 Theory

Consider the following binary contingent choice situation: a student applicant wants to obtain a seat at one of two schools, A or B. The consumption utility, or valuation, of school A is larger than that of school B, i.e. $v_A > v_B$. The applicant knows the probabilities $p_A > 0, p_B > 0$, and $p_{AB} = 1 - p_A - p_B < 1$, which are, respectively, the probability of the contingency that only school A has an obtainable seat, only school B has an obtainable seat, or both schools have an obtainable seat. The applicant is asked to rank the two schools, either A over B, denoted by AB (the straightforward ranking) or B over A, denoted by BA (the non-straightforward ranking). If the top-ranked school is obtainable, the applicant obtains a seat in that school, otherwise they obtain a seat in the other school. Note that it is impossible for the applicant to end up with a seat in neither school. Therefore if the applicant maximizes their expected consumption utility and $p_{AB} > 0$, ranking AB is strictly dominant, regardless of p_A and p_B, and hence the mechanism is strategy-proof.

If the applicant's utility exhibits reference dependence, then following Kahneman and Tversky (1979), we assume that the utility of the applicant consists of the consumption utility from the actual assignment (either v_A or v_B) and a gain-loss component, which enters the utility function additively with a weight of $\eta > 0$. Moreover, it is assumed that, relative to positive surprises, negative surprises get a weight $\lambda > 1$ capturing the applicant's individual degree of loss aversion. Hence, if the applicant obtains a seat at school $s \in \{A, B\}$ and expects to obtain a seat at school $r \in \{A, B\}$, their utility is given by

$$u(s \mid r) = v_s + \begin{cases} \eta(v_s - v_r) & \text{if} \quad v_s \geq v_r, \\ \lambda\eta(v_s - v_r) & \text{if} \quad v_s < v_r. \end{cases} \tag{1}$$

Under reference-dependent preferences, it is key to model what the reference point is and how it is affected by the decision maker's choice. We follow Köszegi and Rabin (2007) in assuming that the reference point is stochastic and given by the lottery over the two schools that results from the chosen ranking (a so-called choice-acclimating

Footnote 6 continued
subjects did not face any uncertainty when they make their decisions, i.e., they face a "hot choice", or in which a truthful report of preferences is not a dominant strategy.

personal equilibrium). If the applicant ranks AB, the probability of obtaining a seat at school A is $q_A = p_A + p_{AB}$ and the probability of obtaining a seat at school B is $q_B = p_B$. Analogously, if the applicant ranks BA, the probability of obtaining a seat at school B is $q_B = p_B + p_{AB}$ and the probability of obtaining a seat at school A is $q_A = p_A$. Therefore the expected utility of ending up in school s is

$$E_r[u(s \mid r)] = v_s + q_{r \neq s} \begin{cases} \eta(v_s - v_r) & \text{if} \quad v_s \geq v_r, \\ \lambda\eta(v_s - v_r) & \text{if} \quad v_s < v_r. \end{cases} \tag{2}$$

As a result, the overall expected utility given the applicant's ranking choice is given by

$$\begin{aligned} E_s\{E_r[u(s|r)]\} &= q_A[v_A + q_B\eta(v_A - v_B)] + q_B[v_B - q_A\lambda\eta(v_A - v_B)] \\ &= \underbrace{q_A v_A + q_B v_B}_{\text{consumptionutilitycomponent}} - \underbrace{q_A q_B \Lambda(v_A - v_B)}_{\text{gain}-\text{losscomponent}}, \end{aligned} \tag{3}$$

where $\Lambda \equiv \eta(\lambda - 1)$ is referred to as the loss dominance parameter. Intuitively, for the gain-loss component, there is a positive payoff surprise $v_A - v_B$ if the applicant expects a seat in school B but ends up in A and there is a negative payoff surprise $-(v_A - v_B)$ if the applicant expects to obtain a seat in school A but ends up in B. The compound probability of each surprise is $q_A q_B$. Surprises are weighted by η relative to the consumption utility and negative surprises get an additional weight of λ, leading to the definition of Λ.

Note that having classical preferences corresponds to either $\eta = 0$ (no weight on gain-loss utility) or $\lambda = 1$ (no loss aversion). These two are isomorphic parametrizations. In fact, any pair (η, λ) that results in the same value of Λ constitutes an isomorphic representation of the very same EBLA preferences. In the jargon of empirical research, within the context of EBLA preferences, η and λ are not separately identified. This is because, under EBLA, the chosen lottery determines the reference point, so it is impossible to exogenously manipulate the reference point, which is necessary to separately identify η and λ. As a result, we carry on the analysis by using the composite preference parameter Λ only.

Ranking AB gives the expected utility

$$EU(AB) = (p_A + p_{AB})v_A + p_B v_B - (p_A + p_{AB})p_B\Lambda(v_A - v_B), \tag{4}$$

whereas ranking BA gives the expected utility

$$EU(BA) = p_A v_A + (p_B + p_{AB})v_B - p_A(p_B + p_{AB})\Lambda(v_A - v_B). \tag{5}$$

This means that the expected utility difference between the two rankings is

$$\begin{aligned} EU(AB) - EU(BA) &= \overbrace{p_{AB}(v_A - v_B)}^{\text{diff.consumptionutilitycomponent}} - \overbrace{\Lambda(p_B - p_A)p_{AB}(v_A - v_B)}^{\text{diff.gain}-\text{losscomponent}} \\ &= p_{AB}(v_A - v_B)[1 - \Lambda(p_B - p_A)]. \end{aligned} \tag{6}$$

This reveals that ranking BA is optimal if and only if $\Lambda(p_B - p_A) > 1$. A necessary condition is that $\Lambda > 1$, i.e. that the applicant exhibits "loss dominance."

The next section describes our experimental design based on this theoretical foundation.

3 Experimental design

We frame the choice situation as one where subjects try to obtain an employment position in one of two institutes instead of a seat in one of two schools. In comparison to a school matching framing, we deem this framing to be more fitting to a situation in which one obtains a monetary reward for a match. We run two between-subjects treatments in which each subject makes a single choice of ranking the two institutes and faces two possible outcomes: obtaining a position in institute A or obtaining a position in institute B. In the actual instructions (see part A of the Supplementary Material), the two institutes are labeled using neutral names "Circle" and "Square", with the two labels being randomly matched at the subject level to the labels A and B that we use in the paper. The two treatments differ in the probability distribution over the three contingencies. In *Symmetric*, the contingency probabilities are chosen such that disappointment aversion predicts that nearly all subjects will provide a straightforward ranking. In *Asymmetric*, the contingency probabilities are instead chosen such that almost everyone who is loss-dominant is predicted to provide a non-straightforward ranking due to disappointment aversion. As a result, we would expect to see a significantly higher rate of non-straightforward ranking in *Asymmetric* compared to *Symmetric* if disappointment aversion is an important factor behind non-straightforward rankings. On the other hand, if disappointment aversion is not an important factor behind non-straightforward rankings, we would expect to see little to no difference in the rate of non-straightforward ranking between the two treatments.

3.1 Parameter choices

If all subjects follow the theory presented in the previous section, then the contingency probabilities could in the limit be chosen such that all subjects would rank straightforwardly in *Symmetric*, whereas all subjects with $\Lambda > 1$ (loss dominance) would rank non-straightforwardly in *Asymmetric*. This could be achieved by setting $p_A \geq p_B$ in *Symmetric* and by letting $p_A \to 0$, $p_{AB} \to 0$ and $p_B \to 1$ in *Asymmetric*. This way, the treatment effect (*Asymmetric* minus *Symmetric*) on the rate of non-straightforward ranking would identify the total fraction of loss-dominant subjects. As shown by Masatlioglu and Raymond (2016) for a general choice environment with EBLA preferences and by Meisner and von Wangenheim (2023) specifically for school choice with any number of students and schools, a necessary condition for non-straightforward ranking is $\Lambda > 1$ (see (6)). As a result, in any choice environment with EBLA preferences, and in any school choice environment in particular, one cannot obtain a larger theoretical treatment effect of disappointment aversion than with the proposed limit parametrization. That is, we are in principle not losing any ability to identify the effect of disappointment aversion by reducing the school choice problem to the very simple setting that we use. In fact,

Table 1 Experimental design parametrization

	v_A	v_B	p_A	p_B	p_{AB}
Symmetric	€10	€4	0.4	0.5	0.1
Asymmetric	€10	€4	0.05	0.85	0.1

with the limit parametrization, the treatment effect provides an upper bound for the effect of disappointment aversion on non-straightforward ranking across the entire domain of matching environments.

However, we must move away from these theoretical limits for our experiment. First, in *Asymmetric*, the limit parametrization comes at the cost of weakening incentives: as $p_{AB} \to 0$, flipping the ranking makes almost no difference to the resulting outcome lottery. If (in the spirit of the random utility model) subjects' ranking behavior includes noise whose effect declines with the predicted expected utility difference, any realistic design requires choosing a non-negligible probability p_{AB}. Next, we aim to make *Symmetric* and *Asymmetric* as structurally similar to each other as possible. In order for the rankings to affect the outcome distributions identically, we use the same value of p_{AB} in both treatments. Also, in order to have three contingencies with positive probability in both treatments, we set $p_A > 0$ but small in *Asymmetric*. Furthermore, to avoid ranking differences between the treatments based solely on which singleton contingency is more likely, we set $p_A < p_B$ but close to each other in *Symmetric*. Table 1 displays the parameters used in each treatment. We set v_A to €10, v_B to €4 and $p_{AB} = 0.1$ in both treatments. In *Asymmetric*, we set $p_A = 0.05$ and $p_B = 0.85$. In *Symmetric*, we set $p_A = 0.4$ and $p_B = 0.5$.[7]

With these parameters, the theory predicts that anyone with $\Lambda > 1.25$ in *Asymmetric* and $\Lambda > 10$ in *Symmetric* will provide a non-straightforward ranking. Since estimates suggest that the fraction of individuals with $\Lambda > 10$ is negligible,[8] in terms of predicted ranking, this design comes very close to the theoretical limit discussed above. The only empirically relevant deviation from this limit comes from subjects with $\Lambda \in (1, 1.25]$, who are predicted to rank straightforwardly in *Asymmetric*. However, this small "slack" relative to the theoretical limit is unlikely to lead to an underestimate of the effect of disappointment aversion in comparison to many existing experimental settings since these settings are also not parametrized to reach that limit. Importantly, our parametrization is tighter than those of other papers that

[7] A practical issue that might arise with these parameters is that subjects' rankings, which might be costly to devise, only matter 10% of the time. This might make subjects more likely to choose a default action, which could be straightforward reporting. While this might increase overall levels of straightforward reporting, it would occur in both treatments equally, so the treatment effect is unaffected. We thank an anonymous referee for pointing this out.

[8] For example, Sprenger (2015) estimates that very few people have $\lambda > 11$, which, under his assumption that $\eta = 1$, is equivalent to $\Lambda > 10$. We do not run a separate loss aversion elicitation task ourselves because it would complicate the interpretation of subjects' decisions in the experiment. The EBLA model implies that the reference point would be determined by the combination of the two choices (in the ranking and in the elicitation task), making it difficult to control the available reference points in each task. This problem might be mitigated by revealing the outcome of the first choice before the second choice is made, but this might create wealth effects.

have investigated the impact of EBLA preferences on non-straightforward ranking (Dreyfuss et al., 2022, 2023). In these settings, non-straightforward ranking is predicted only for $\lambda > 3$, which, under the assumption that $\eta = 1$, is equivalent to $\Lambda > 2$ in our setting.

3.2 Quiz

We administer a two-question quiz after the instructions. The aim is to ensure understanding of the instructions. The first question asks: "Could it be that both institutes have an open position?" The second question asks: "Could it be that you are hired by neither institute?" The first question calls attention to the fact that the ranking choice affects the outcome with positive probability. The second question is designed to counteract a possible immediate acceptance misperception. After answering, subjects are given the correct answers with explanations (see part A of the Supplementary Material for a screenshot and part B of the Supplementary Material for the distribution of responses).

3.3 Sample size

Our goal is to examine whether disappointment aversion is an economically significant explanation of non-straightforward ranking. Given the number of possible alternative explanations (see Sect. 1), we deem disappointment aversion to be economically significant if it can account for a sufficiently large proportion of non-straightforward ranking, which we choose to be one third. Recall that the rates of non-straightforward ranking observed in strategy-proof mechanisms range from 14 to 75%. One third of the midpoint of this range is approximately 14%.

On this background, we aim to have a power of at least 0.8 for being able to discern a treatment effect of 14 percentage points from the null hypothesis of no effect. In regard to the needed sample size, assuming the highest possible standard error for the estimate of the treatment effect given its size,[9] this requires at least 200 subjects per treatment. In expectation of an imperfect show-up rate, we over-recruited subjects for both treatments and ended up with 209 subjects in *Symmetric* and 212 subjects in *Asymmetric*. These sample sizes give us a power of 0.8216 against the null hypothesis using the Pearson's χ^2 test.

3.4 Procedure

The experiment was organized as follows. Subjects received links to the experiment via email and proceeded with the experiment online after clicking on the link. The instructions and the decision drop-down menu were displayed on a single screen (see a screenshot in part A of the Supplementary Material) to focus subject attention. After reading the instructions, answering the two quiz questions, and being informed about the correct answers to these questions, subjects submitted their rankings.

9 With the treatment effect of size d, the theoretical standard error is maximized if the proportions of non-straightforward ranking in the two treatments are equal to $0.5 \pm 0.5d$.

Subjects were then presented with several debriefing questions designed to elicit the reasoning behind their decisions.[10] Next, they filled out a demographic questionnaire. Finally, subjects were told at which institutes they obtained positions and their resulting payoffs. Subjects were not told which contingency was realized, so they were not able to judge whether they would have obtained a position at the other institute had they submitted the other ranking. The type of feedback was pre-announced in order to prevent decision-making based on anticipated regret.

Three sessions were run online in February 2021 using *oTree* (Chen et al., 2016) with payment via an online transfer. The subjects were mostly students from RWTH Aachen University recruited from the subject pool of the AIXperiment laboratory using the *Online Recruitment System for Economic Experiments* (Greiner, 2015). Subjects who previously participated in experiments about strategy-proofness were excluded. The experiment was run in German.[11]

Table D.1 in the Supplementary Material displays a summary of the demographic variables. Of the 421 subjects, 42% were women and 58% were men. The age of the subjects ranged from 18 to 56 years, with a mean of 24.6 years. 52% of subjects were studying engineering, 28% were studying economics, business, social sciences, and law, 10% were studying mathematics and natural sciences, and the rest were in other fields. 96% of subjects reported being fluent in German or being native German speakers.

The average payoff was €6.11 (€7.22 in *Symmetric* and €5.02 in *Asymmetric*). There was no stated show-up fee, but there was a guaranteed payoff of €4 due to the design of the experiment. The average time spent on the experiment was 6.3 minutes.

4 Results

Table 2 displays the distribution of rankings in the two treatments. In *Symmetric*, 201 out of 209 subjects (96.2%) rank *AB*, whereas the other 8 subjects (3.8%) rank *BA*. In *Asymmetric*, 196 out of 212 subjects (92.5%) rank *AB*, whereas the other 16 subjects (7.5%) rank *BA*. The difference in the rate of non-straightforward ranking between *Asymmetric* and *Symmetric* of 3.72 percentage points is statistically marginally significant (Pearson's χ^2-test's $p = 0.10$; Fisher's exact test's $p = 0.14$). The 95% confidence interval for the treatment effect is $(-0.72, 8.16)$ percentage points.[12]

[10] The debriefing questions and the distributions of responses are presented in part C of the Supplementary Material. Note that these questions were unincentivized.

[11] Instructions in German and translated into English are presented in part A of the Supplementary Material.

[12] We also examined responses to the debriefing questions. Unfortunately, these were unhelpful for addressing our research question mainly for two reasons. First, few subjects (24 out of 421) ranked non-straightforwardly, making it difficult to infer reasons for such behavior. Second, among the subjects who ranked straightforwardly, a substantial number (39%) agreed to mutually exclusive statements (see Table C.7 in the Supplementary Material). For these reasons, we do not feel confident enough to draw conclusions from the given responses.

Table 2 Distribution of rankings by treatment

Ranking	*Symmetric*	*Asymmetric*
AB	96.2%	92.5%
	(201)	(196)
BA	3.8%	7.5%
	(8)	(16)
Subjects	209	212

Note: The number of subjects in each cell is presented in parentheses

5 Discussion

Our results suggest that the conclusion in Dreyfuss et al. (2022) and Dreyfuss et al. (2023) that EBLA can explain non-straightforward ranking, should not be interpreted as implying that EBLA is a major driver of non-straightforward ranking. Our results instead suggest that when other potential drivers of this behavior are eliminated, EBLA in fact accounts for a small amount (3.7 percentage points) of the level of non-straightforward ranking typically observed in school choice experiments (14% to 75%, Table A.1).

Our finding translates to more complex matching settings under the assumption that the effect of disappointment aversion does not interact with complexity of those settings per se, as is the case in the choice-acclimating personal equilibrium EBLA model of Kőszegi and Rabin (2007). In this model, whether disappointment aversion does or does not result in a non-straightforward ranking depends only on the fundamentals (payoffs and probabilities) of the underlying lottery choice problem and on the decision maker's Λ. Any complexity that goes beyond these fundamentals does not affect the ranking choice. As a result, even though we estimate the effect of disappointment aversion in a simple matching setting, the findings also extend to more complex matching settings.

We can imagine a more general theory that explicitly considers interactions between disappointment aversion and the complexity of the choice problem, but no such formal theory has been suggested, to the best of our knowledge. Even if such a theory existed, identifying the effect of disappointment aversion in more complex matching settings would be challenging. For example, complexity likely goes hand-in-hand with the extent of mechanism misperception. Hence if we were to find larger treatment effects in more complex environments, it would not be possible to determine whether the increase is driven by an interaction between disappointment aversion and complexity as opposed to being driven by the misperception.

There are also possible interpretations of the treatment effect that cannot be distinguished from disappointment aversion using our experimental design. One such interpretation is a model of report-dependent preferences under which the behavioral utility component increases with the rank of the assigned school in one's reported ranking (Meisner, 2023; Kloosterman and Troyan, 2023). Within our setting, this explanation is observationally equivalent to disappointment aversion, indicating that further experiments would be necessary to distinguish between these two interpretations. The treatment effect that we identify could therefore also reflect report-dependent preferences, implying that the effect of disappointment aversion on its own might be smaller than suggested by the treatment effect.

6 Conclusion

We experimentally examine the hypothesis that a significant driver of non-straightforward ranking (relative to the order of consumption utility) in strategy-proof student-school matching mechanisms is disappointment aversion. Disappointment aversion is a non-classical preference component that is analytically captured by expectation-based loss aversion. Under such preferences, a student might prefer to top-rank school *B* that is inferior to school *A* based on consumption utility in order to avoid the potential disappointment associated with top-ranking and hence having an increased hope of *A* but obtaining *B*.

To test the hypothesis, we run an online between-subjects experiment with a simple student-school assignment decision task. There are two schools, *A* and *B*, with *A* having a higher induced value than *B*, and two ranking choices. There are two treatments, *Symmetric* and *Asymmetric*, designed such that disappointment aversion should have a negligible effect on ranking in the former, while having nearly the largest possible effect in the latter.

Our first contribution is in designing an environment in which we eliminate many confounding explanations of the treatment effect, such as mechanism misperception, reciprocal preferences, or ego utility. Our second contribution is in providing an (approximate) upper bound for the size of the effect of disappointment aversion on non-straightforward reporting in any matching setting.

We find a small and statistically marginally significant treatment effect in the direction predicted by the disappointment aversion hypothesis. The rate of non-straightforward ranking is 3.8% in *Symmetric* and 7.5% in *Asymmetric*, with the treatment difference of 3.72 percentage points. In comparison, the non-straightforward ranking rate in strategy-proof matching mechanisms estimated in the experimental literature typically varies from 14% to 75% (Table A.1). This result suggests that disappointment aversion plays a minor role in explaining non-straightforward rankings observed in the literature.

More broadly, our study contributes to the examination of factors that might cause non-straightforward reporting in strategy-proof mechanisms. Our finding suggests that, at least in school choice, research attention should be focused on other potential drivers of non-straightforward ranking, such as those that we eliminate in our design. This research has the potential to help us improve real-world designs with the objective of making straightforward reporting not only theoretically optimal, but also empirically prevalent.

Appendix A Survey of Non-Truthtelling Rates in the Literature

Table A.1 Summary of strategy-proof mechanisms literature

Authors	Treatment	No. of students	No. of schools	No. of seats at each school	Length of ROL	Mechanism	Non-truthtelling rate in percentage	Information	Advice
Afacan et al. (2022)	Repetition Improvement	4	4	1; 1; 1; 1	4	DA	34	2	0
Afacan et al. (2022)	Strategic Improvement	4	4	1; 1; 1; 1	4	DA	43	2	0
Afacan et al. (2022)	BOTH	4	4	1; 1; 1; 1	4	DA	44	2	0
Afacan et al. (2022)	Repetition Improvement	4	4	1; 1; 1; 1	2	DecDA	43	2	0
Afacan et al. (2022)	Strategic Improvement	4	4	1; 1; 1; 1	2	DecDA	37	2	0
Afacan et al. (2022)	BOTH	4	4	1; 1; 1; 1	2	DecDA	53	2	0
Afacan et al. (2022)	Repetition Improvement	4	4	1; 1; 1; 1	2	DecDA2	40	2	0
Bó and Hakimov (2020)	DA	8	8	1; 1; 1; 1; 1; 1; 1; 1	8	DA	52	1	0
Calsamiglia et al. (2010)	Unconstrained Designed	36	7	3; 3; 6; 6; 6; 6; 6	7	DA	42	1	0
Calsamiglia et al. (2010)	Unconstrained Random	36	7	3; 3; 6; 6; 6; 6; 6	7	DA	43	1	0
Calsamiglia et al. (2010)	Unconstrained Designed	36	7	3; 3; 6; 6; 6; 6; 6	7	TTC	37	1	0
Calsamiglia et al. (2010)	Unconstrained Random	36	7	3; 3; 6; 6; 6; 6; 6	7	TTC	26	1	0
Calsamiglia et al. (2010)	Constrained Designed	36	7	3; 3; 6; 6; 6; 6; 6	0-3	DA	43	1	0
Calsamiglia et al. (2010)	Constrained Random	36	7	3; 3; 6; 6; 6; 6; 6	0-3	DA	64	1	0
Calsamiglia et al. (2010)	Constrained Designed	36	7	3; 3; 6; 6; 6; 6; 6	0-3	TTC	47	1	0
Calsamiglia et al. (2010)	Constrained Random	36	7	3; 3; 6; 6; 6; 6; 6	0-3	TTC	61	1	0
Cerrone et al. (2024)	Unmanipulable	5	5	1; 1; 1; 1; 1	5	DA	56	2	0
Cerrone et al. (2024)	Manipulable Market 1	5	5	1; 1; 1; 1; 1	5	DA	45	2	0
Cerrone et al. (2024)	Manipulable Market 2	5	5	1; 1; 1; 1; 1	5	DA	45	2	0
Chen and Kesten (2019)	4-school	4	4	1; 1; 1; 1	4	DA	25	2	0
Chen and Kesten (2019)	6-school	6	6	1; 1; 1; 1; 1; 1	6	DA	53	2	0
Chen and Kesten (2019)	4-school	4	4	1; 1; 1; 1	4	PA	29	2	0

Table A.1 continued

Authors	Treatment	No. of students	No. of schools	No. of seats at each school	Length of ROL	Mechanism	Non-truthtelling rate in percentage	Information	Advice
Chen and Sönmez (2006)	Designed	36	7	3; 3; 6; 6; 6; 6; 6	7	DA	28	1	0
Chen and Sönmez (2006)	Random	36	7	3; 3; 6; 6; 6; 6; 6	7	DA	44	1	0
Chen and Sönmez (2006)	Designed	36	7	3; 3; 6; 6; 6; 6; 6	7	TTC	50	1	0
Chen and Sönmez (2006)	Random	36	7	3; 3; 6; 6; 6; 6; 6	7	TTC	57	1	0
Chen et al. (2018)	DA-4 all human	4	4	1; 1; 1; 1	4	DA	40	2	0
Chen et al. (2018)	DA-40 all human	40	4	10; 10; 10; 10	4	DA	33	2	0
Chen et al. (2018)	DA-40 human-robot	40	4	10; 10; 10; 10	4	DA	38	2	0
Chen et al. (2018)	DA-4K human-robot	4000	4	1000; 1000; 1000; 1000	4	DA	34	2	0
Chen et al. (2016)	Designed complete	36	7	3; 3; 6; 6; 6; 6; 6	7	DA	46	2	0
Chen et al. (2016)	Designed complete	36	7	3; 3; 6; 6; 6; 6; 6	7	TTC	29	2	0
Ding and Schotter (2019)	Phase 1	5	3	2; 2; 1	3	DA	60	1	0
Ding and Schotter (2019)	Phase 2	5	3	2; 2; 1	3	DA	60	1	1
Ding and Schotter (2019)	Repeated	5	3	2; 2; 1	3	DA	35	1	0
Ding and Schotter (2019)	Intergenerational own	5	3	2; 2; 1	3	DA	28	1	1
Ding and Schotter (2019)	Intergenerational multiple	5	3	2; 2; 1	3	DA	40	1	1
Featherstone and Niederle (2016)	Aligned	5	3	2; 1; 1	3	DA	20	1	0
Featherstone and Niederle (2016)	Uncorrelated	5	4	1; 1; 1; 1	4	DA	34	1	0
Guillen and Hakimov (2017)	Baseline UMT	4	4	1; 1; 1; 1	4	TTC	30	2	0
Guillen and Hakimov (2017)	UMT	4	4	1; 1; 1; 1	4	TTC	59	2	0

Table A.1 continued

Authors	Treatment	No. of students	No. of schools	No. of seats at each school	Length of ROL	Mechanism	Non-truthtelling rate in percentage	Information	Advice
Guillen and Hakimov (2017)	Baseline CBMT	4	4	1; 1; 1; 1	4	TTC	38	2	0
Guillen and Hakimov (2017)	CBMT	4	4	1; 1; 1; 1	4	TTC	70	2	0
Guillen and Hakimov (2017)	Baseline CUMT	4	4	1; 1; 1; 1	4	TTC	44	2	0
Guillen and Hakimov (2017)	CUMT	4	4	1; 1; 1; 1	4	TTC	72	2	0
Guillen and Hakimov (2017)	Baseline UPT	4	4	1; 1; 1; 1	4	TTC	39	2	0
Guillen and Hakimov (2017)	UPT	4	4	1; 1; 1; 1	4	TTC	55	2	0
Hakimov and Kesten (2018)	MD	261	3	85; 93; 83	3	TTC	41	1	0
Hakimov and Kesten (2018)	PD	106	3	37; 40; 29	3	TTC	10	1	1
Hakimov and Kesten (2018)	MPD	113	3	35; 40; 38	3	TTC	24	1	1
Guillen and Hing (2014)	No advice (baseline)	4	4	1; 1; 1; 1	4	TTC	27	1	0
Guillen and Hing (2014)	Right advice	4	4	1; 1; 1; 1	4	TTC	50	1	1
Guillen and Hing (2014)	Wrong advice	4	4	1; 1; 1; 1	4	TTC	72	1	1
Guillen and Hing (2014)	Mixed advice	4	4	1; 1; 1; 1	4	TTC	58	1	1
Guillen and Veszteg (2021)	DA Baseline	4	4	1; 1; 1; 1	4	DA	32	1	0
Guillen and Veszteg (2021)	TTC Baseline	4	4	1; 1; 1; 1	4	TTC	54	1	0
Guillen and Veszteg (2021)	RDA	4	4	1; 1; 1; 1	4	RDA	69	1	0
Guillen and Veszteg (2021)	RTTC	4	4	1; 1; 1; 1	4	RTTC	78	1	0
Guillen and Hakimov (2018)	Designed	10	3	3; 3; 4	3	TTC	43	2	0
Guillen and Hakimov (2018)	Random-Correlated	10	5	2; 2; 2; 2; 2	5	TTC	70	2	0
Guillen and Hakimov (2018)	Random-Uncorrelated	10	4	2, 2; 3; 3	4	TTC	63	2	0
Guillen and Hakimov (2018)	Designed	10	3	3; 3; 4	3	ETTC	44	2	0
Guillen and Hakimov (2018)	Random-Correlated	10	5	2; 2; 2; 2; 2	5	ETTC	73	2	0

Table A.1 continued

Authors	Treatment	No. of students	No. of schools	No. of seats at each school	Length of ROL	Mechanism	Non-truthtelling rate in percentage	Information	Advice
Guillen and Hakimov (2018)	Random-Uncorrelated	10	4	2, 2; 3; 3	4	ETTC	60	2	0
Klijn et al. (2013)	GS_ u20: unconstrained	3	3	1; 1; 1	3	DA	50	2	0
Klijn et al. (2013)	GS_ u13: unconstrained	3	3	1; 1; 1	3	DA	35	2	0
Klijn et al. (2013)	GS_ u27: unconstrained	3	3	1; 1; 1	3	DA	56	2	0
Klijn et al. (2019)	CI Problem 1	4	4	1; 1; 1; 1	0-4	DA	58	2	0
Klijn et al. (2019)	CI Problem 2	4	4	1; 1; 1; 1	0-4	DA	51	2	0
Klijn et al. (2019)	CI Problem 3	4	4	1; 1; 1; 1	0-4	DA	53	2	0
Klijn et al. (2019)	CI Problem 4	4	4	1; 1; 1; 1	0-4	DA	48	2	0
Li (2017)	SP-RSD one shot	4	4	1; 1; 1; 1	4	RSD	40	1	0
Li (2017)	SP-RSD multiple rounds	4	4	1; 1; 1; 1	4	RSD	36	1	0
Li (2017)	OSP-RSD one shot	4	4	1; 1; 1; 1	1	OSP-RSD	8	1	0
Li (2017)	OSP-RSD multiple rounds	4	4	1; 1; 1; 1	1	OSP-RSD	7	1	0
Pais and Pintér (2008)	Zero information	5	3	2; 2; 1	3	DA	18	0	0
Pais and Pintér (2008)	Low information	5	3	2; 2; 1	3	DA	24	1	0
Pais and Pintér (2008)	Partial information	5	3	2; 2; 1	3	DA	33	1	0
Pais and Pintér (2008)	Full information	5	3	2; 2; 1	3	DA	33	2	0
Pais and Pintér (2008)	Zero information	5	3	2; 2; 1	3	TTC	4	0	0
Pais and Pintér (2008)	Low information	5	3	2; 2; 1	3	TTC	18	1	0
Pais and Pintér (2008)	Partial information	5	3	2; 2; 1	3	TTC	24	1	0
Pais and Pintér (2008)	Full information	5	3	2; 2; 1	3	TTC	14	2	0
Zhu (2015)	Baseline uncorrelated	3	3	1; 1; 1	3	DA	75	2	0
Zhu (2015)	Baseline correlated	3	3	1; 1; 1	3	DA	47	2	0

Table A.1 continued

Authors	Treatment	No. of students	No. of schools	No. of seats at each school	Length of ROL	Mechanism	Non-truthtelling rate in percentage	Information	Advice
Zhu (2015)	Advice uncorrelated	3	3	1; 1; 1	3	DA	19	2	1
Zhu (2015)	Advice correlated	3	3	1; 1; 1	3	DA	31	2	1
Zhu (2015)	Observation uncorrelated	3	3	1; 1; 1	3	DA	50	2	1
Zhu (2015)	Observation correlated	3	3	1; 1; 1	3	DA	58	2	1

Notes

Variables	Full description
Authors	Last name(s) of the author(s) (year of publication or upload in parentheses)
Treatment	Treatment name in the paper and short environment description
No. of students	Number of students applying to schools
No. of schools	Number of schools offering seats to students
No. of seats at each school	List of number of seats at each school (for example, 2;2;1 means the most-preferred and the second most-preferred schools have two seats each and the least-preferred school has one seat)
Length of ROL	Length of rank ordered list a student is allowed to submit
Mechanism	Matching mechanism used to allocate students to schools
Non-truthtelling rate in percentage	Average rate of non-truthful reporting in percentage; a truthful report is defined as ranking all schools in order of induced preferences; if a definition of truthtelling other than this is used, it is indicated under comments

Table A.1 continued

Notes

Variables	Full description
Information	Degree of information about preferences and priorities given to subjects before the mechanism is run; 0: Zero information, only own preferences are known; 1: Some information, own preferences and some information about the schools' priorities or the other students' preferences are known; 2: Full information, own and the other students' preferences and schools' priorities are known
Advice	Was advice in any form given to the students?; 0: No; 1: Yes

Abbreviations	Full description
DA	(Static student-proposing) Deferred Acceptance mechanism (see Gale and Shapley, 1962)
DecDA	Decentralized DA with a divided market
DecDA2	Decentralized DA with two iterations under parallel markets
ETTC	Equitable Top Trading Cycles mechanism (see Hakimov and Kesten, 2018)
OSP-RSD	Obviously Strategy-Proof RSD (see Li, 2017)
PA	Parallel Acceptance mechanism
RDA	Reverse DA (see Guillen and Veszteg, 2021)
RSD	Random Serial Dictatorship
RTTC	Reverse TTC (see Guillen and Veszteg, 2021)
TTC	Top Trading Cycles mechanism (see Shapley and Scarf, 1974; Abdulkadiroğlu and Sönmez, 2003)

Note: A more comprehensive and detailed version of the table is provided as an Excel file in the Supplementary Material

Table A.2 Summary of non-strategy-proof mechanisms literature

Authors	Treatment	No. of students	No. of schools	No. of seats at each school	Length of ROL	Mechanism	Non-truthtelling rate in percentage	Information	Advice
Afacan et al. (2022)	Strategic Improvement	4	4	1; 1; 1; 1	2	DecDA2	52	2	0
Afacan et al. (2022)	BOTH	4	4	1; 1; 1; 1	2	DecDA2	52	2	0
Bó and Hakimov (2020)	IDAM	8	8	1; 1; 1; 1; 1; 1; 1; 1	1-8	IDA	30	1	0
Bó and Hakimov (2020)	IDAM-NC	8	8	1; 1; 1; 1; 1; 1; 1; 1	1-8	IDA	36	1	0
Calsamiglia et al. (2010)	Unconstrained Designed	36	7	3; 3; 6; 6; 6; 6; 6	7	IA	82	1	0
Calsamiglia et al. (2010)	Unconstrained Random	36	7	3; 3; 6; 6; 6; 6; 6	7	IA	78	1	0
Calsamiglia et al. (2010)	Constrained Designed	36	7	3; 3; 6; 6; 6; 6; 6	0-3	IA	82	1	0
Calsamiglia et al. (2010)	Constrained Random	36	7	3; 3; 6; 6; 6; 6; 6	0-3	IA	92	1	0
Calsamiglia et al. (2010)	Constrained Designed	36	7	3; 3; 6; 6; 6; 6; 6	0-3	DA	98	1	0
Calsamiglia et al. (2010)	Constrained Random	36	7	3; 3; 6; 6; 6; 6; 6	0-3	DA	93	1	0
Calsamiglia et al. (2010)	Constrained Designed	36	7	3; 3; 6; 6; 6; 6; 6	0-3	TTC	100	1	0
Calsamiglia et al. (2010)	Constrained Random	36	7	3; 3; 6; 6; 6; 6; 6	0-3	TTC	93	1	0
Cerrone et al. (2024)	Unmanipulable	5	5	1; 1; 1; 1; 1	5	EADA	29	2	0
Cerrone et al. (2024)	Manipulable Market 1	5	5	1; 1; 1; 1; 1	5	EADA	30	2	0
Cerrone et al. (2024)	Manipulable Market 2	5	5	1; 1; 1; 1; 1	5	EADA	36	2	0
Cerrone et al. (2024)	Object Unmanipulable	5	5	1; 1; 1; 1; 1	5	EADA	32	2	0
Cerrone et al. (2024)	Enforced Unmanipulable	5	5	1; 1; 1; 1; 1	5	EADA	38	2	0
Chen and Kesten (2019)	4-school	6	6	1; 1; 1; 1; 1; 1	6	IA	54	2	0
Chen and Kesten (2019)	6-school	6	6	1; 1; 1; 1; 1; 1	6	IA	77	2	0
Chen and Kesten (2019)	6-school	6	6	1; 1; 1; 1; 1; 1	6	PA	74	2	0
Chen and Sönmez (2006)	Designed	36	7	3; 3; 6; 6; 6; 6; 6	7	IA	86	1	0
Chen and Sönmez (2006)	Random	36	7	3; 3; 6; 6; 6; 6; 6	7	IA	72	1	0
Chen et al. (2018)	BOS-4 all human	4	4	1; 1; 1; 1	4	IA	54	2	0

Table A.2 continued

Authors	Treatment	No. of students	No. of schools	No. of seats at each school	Length of ROL	Mechanism	Non-truthtelling rate in percentage	Information	Advice
Chen et al. (2018)	BOS-40 all human	40	4	10; 10; 10; 10	4	IA	62	2	0
Chen et al. (2018)	BOS-40 human-robot emp	40	4	10; 10; 10; 10	4	IA	55	2	0
Chen et al. (2018)	BOS-4K human-robot emp	4000	4	1000; 1000; 1000; 1000	4	IA	57	2	0
Chen et al. (2018)	BOS-40 human-robot truth	40	4	10; 10; 10; 10	4	IA	58	2	0
Chen et al. (2018)	BOS-4K human-robot truth	4000	4	1000; 1000; 1000; 1000	4	IA	52	2	0
Chen et al. (2016)	Designed complete	36	7	3; 3; 6; 6; 6; 6; 6	7	IA	81	2	0
Ding and Schotter (2019)	B-16 Phase 1	5	3	2; 2; 1	3	IA	63	1	0
Ding and Schotter (2019)	B-16 Phase 2	5	3	2; 2; 1	3	IA	66	1	1
Ding and Schotter (2019)	B-10 Phase 1	5	3	2; 2; 1	3	IA	48	1	0
Ding and Schotter (2019)	B-10 Phase 2	5	3	2; 2; 1	3	IA	57	1	1
Ding and Schotter (2019)	Repeated	5	3	2; 2; 1	3	IA	44	1	0
Ding and Schotter (2019)	Intergenerational own	5	3	2; 2; 1	3	IA	52	1	1
Featherstone and Niederle (2016)	Aligned	5	3	2; 1; 1	3	IA	94	1	0
Featherstone and Niederle (2016)	Uncorrelated	5	4	1; 1; 1; 1	4	IA	42	1	0
Klijn et al. (2013)	BOS_ u20: unconstrained	3	3	1; 1; 1	3	IA	60	2	0
Klijn et al. (2013)	BOS_ u13: unconstrained	3	3	1; 1; 1	3	IA	38	2	0

Table A.2 continued

Authors	Treatment	No. of students	No. of schools	No. of seats at each school	Length of ROL	Mechanism	Non-truthtelling rate in percentage	Information	Advice
Klijn et al. (2013)	BOS_ u27: unconstrained	3	3	1; 1; 1	3	IA	69	2	0
Klijn et al. (2013)	BOS_ c20: constrained	3	3	1; 1; 1	2	IA	73	2	0
Klijn et al. (2013)	BOS_ c13: constrained	3	3	1; 1; 1	2	IA	82	2	0
Klijn et al. (2013)	BOS_ c27: constrained	3	3	1; 1; 1	2	IA	86	2	0
Klijn et al. (2013)	GS_ c20: constrained	3	3	1; 1; 1	2	DA	76	2	0
Klijn et al. (2013)	GS_ c13: constrained	3	3	1; 1; 1	2	DA	83	2	0
Klijn et al. (2013)	GS_ c27: constrained	3	3	1; 1; 1	2	DA	79	2	0
Klijn et al. (2019)	CS Problem 1	4	4	1; 1; 1; 1	0-4	DA School	62	2	0
Klijn et al. (2019)	CS Problem 2	4	4	1; 1; 1; 1	0-4	DA School	51	2	0
Klijn et al. (2019)	CS Problem 3	4	4	1; 1; 1; 1	0-4	DA School	36	2	0
Klijn et al. (2019)	CS Problem 4	4	4	1; 1; 1; 1	0-4	DA School	44	2	0
Klijn et al. (2019)	DI Problem 1	4	4	1; 1; 1; 1	0-4	IDA	48	2	0
Klijn et al. (2019)	DI Problem 2	4	4	1; 1; 1; 1	0-4	IDA	37	2	0
Klijn et al. (2019)	DI Problem 3	4	4	1; 1; 1; 1	0-4	IDA	37	2	0
Klijn et al. (2019)	DI Problem 4	4	4	1; 1; 1; 1	0-4	IDA	57	2	0
Klijn et al. (2019)	DS Problem 1	4	4	1; 1; 1; 1	0-4	IDA School	6	2	0
Klijn et al. (2019)	DS Problem 2	4	4	1; 1; 1; 1	0-4	IDA School	6	2	0
Klijn et al. (2019)	DS Problem 3	4	4	1; 1; 1; 1	0-4	IDA School	1	2	0
Klijn et al. (2019)	DS Problem 4	4	4	1; 1; 1; 1	0-4	IDA School	1	2	0

Table A.2 continued

Authors	Treatment	No. of students	No. of schools	No. of seats at each school	Length of ROL	Mechanism	Non-truthtelling rate in percentage	Information	Advice
Pais and Pintér (2008)	Zero information	5	3	2; 2; 1	3	IA	13	0	0
Pais and Pintér (2008)	Low information	5	3	2; 2; 1	3	IA	38	1	0
Pais and Pintér (2008)	Partial information	5	3	2; 2; 1	3	IA	53	1	0
Pais and Pintér (2008)	Full information	5	3	2; 2; 1	3	IA	53	2	0

Notes

Variables	Full description
Authors	Last name(s) of the author(s) (year of publication or upload in parentheses)
Treatment	Treatment name in the paper and short environment description
No. of students	Number of students applying to schools
No. of schools	Number of schools offering seats to students
No. of seats at each school	List of number of seats at each school (for example, 2;2;1 means the most-preferred and the second most-preferred schools have two seats each and the least-preferred school has one seat)
Length of ROL	Length of rank ordered list a student is allowed to submit
Mechanism	Matching mechanism used to allocate students to schools
Non-truthtelling rate in percentage	Average rate of non-truthful reporting in percentage; a truthful report is defined as ranking all schools in order of induced preferences; if a definition of truthtelling other than this is used, it is indicated under comments

Table A.2 continued

Notes

Variables	Full description
Information	Degree of information about preferences and priorities given to subjects before the mechanism is run; 0: Zero information, only own preferences are known; 1: Some information, own preferences and some information about the schools' priorities or the other students' preferences are known; 2: Full information, own and the other students' preferences and schools' priorities are known
Advice	Was advice in any form given to the students?; 0: No; 1: Yes

Abbreviations	Full description
DA	(Static student-proposing) Deferred Acceptance mechanism (see Gale and Shapley, 1962)
DA School	(Static school-proposing) Deferred Acceptance mechanism (see Gale and Shapley, 1962)
DecDA	Decentralized DA with a divided market
DecDA2	Decentralized DA with two iterations under parallel markets
EADA	Efficiency Adjusted DA mechanism
IA	(Boston) Immediate Acceptance mechanism
IDA	Iterative Deferred Acceptance mechanism
IDA School	School-proposing Iterative Deferred Acceptance mechanism
PA	Parallel Acceptance mechanism
TTC	Top Trading Cycles mechanism (see Shapley and Scarf, 1974; Abdulkadiroğlu and Sönmez, 2003)

Note: A more comprehensive and detailed version of the table is provided as an Excel file in the Supplementary Material

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