



Rankings-dependent preferences: A real goods matching experiment



Andrew Kloosterman ^a, Peter Troyan ^{b,*}

^a ISO New England, USA

^b Department of Economics, University of Virginia, USA

ARTICLE INFO

Keywords:

Market design
Matching
School choice
Rankings-dependent preferences
Real goods experiment
Endowment effect
Strategy-proofness

ABSTRACT

We investigate whether preferences for objects received via a matching mechanism are influenced by how highly agents rank them in their reported rank order list. We hypothesize that all else equal, agents receive greater utility for the same object when they rank it higher. The addition of rankings-dependent utility implies that it may not be a dominant strategy to submit truthful preferences to a strategyproof mechanism, and that non-strategyproof mechanisms that give more agents objects they report as higher ranked may increase market welfare. We test these hypotheses with a matching experiment in a strategyproof mechanism, the Random Serial Dictatorship (RSD), and a non-strategyproof mechanism, the Boston mechanism. A novel feature of our experimental design is that the objects allocated in the matching markets are real goods, which allows us to directly measure rankings-dependence by eliciting values for goods both inside and outside of the mechanism. The experimental results are mixed, with stronger evidence for rankings-dependence in the RSD treatment than the Boston treatment. We find no differences between the two mechanisms for the rates of truth-telling and the final welfare.

1. Introduction

In strategyproof mechanisms, it is always an optimal strategy for agents to truthfully report their private information to the mechanism. This theoretical property is clearly appealing, as it gives a mechanism designer the ability to predict play and make meaningful statements about other criteria such as welfare. However, a growing body of empirical evidence has documented significant deviations from truthful reporting in such mechanisms. This issue is particularly important in matching markets, the focus in this paper, in which participants submit preference rankings of alternatives such as schools or medical residency programs to a centralized clearinghouse which determines the assignment. Evidence of non-truthful behavior in strategyproof matching mechanisms can be found both in the lab (Chen and Sönmez, 2006; Pais and Pintér, 2008; Li, 2017) and in high-stakes decisions in the field (Chen and Pereyra, 2019; Shorrer and Sóvágó, 2023; Hassidim et al., 2021).

Deviations from truthful reporting are harmful under the implicit assumption that agents' preferences are standard economic preferences in that the values for the objects they receive in the mechanism are determined solely by the characteristics of these objects. If this assumption holds, then, when we observe agents rank objects they value less above objects they value more in a strategyproof mechanism, we can claim that these deviations from truthful reporting are indeed "mistakes". But what if this underlying assumption is wrong? Then, these mistakes may not really be mistakes, but rather optimal behavior from agents with non-standard preferences.

* Corresponding author.

E-mail addresses: akloosterman@iso-ne.com (A. Kloosterman), troyan@virginia.edu (P. Troyan).

In this paper, we explore the possibility that the rankings agents submit to the mechanism influence their values. For instance, an agent may value an object higher when they rank it 2nd compared to a counterfactual in which they receive the same object but rank it 4th. In particular, we consider agents who get a utility from receiving an object x that takes the form:

$$u(x) = v(x) + \rho(\text{rank}(x)).$$

We call $v(x)$ the agent's *fundamental value* for object x ; this corresponds to the standard economic preferences assumed in typical matching models. The second term, $\rho(\text{rank}(x))$, is an additional *rankings-dependent utility* component that is determined by how highly the agent ranked x in their reported preferences. When $\rho(j) = 0$ for all j , the model reduces to a standard model of preferences. In this paper, we investigate the possibility that ρ may not be identically zero, and in particular, that it is a decreasing function; in other words, all else equal, agents receive more utility when they rank an object higher.¹

Investigating questions of rankings-dependent utility is important because if this is an extant phenomenon, there are consequences for real-world market design. Indeed, this paper was motivated by discussions with school district administrators who continue to prefer the Boston mechanism over strategyproof alternatives such as deferred acceptance, because Boston gives more students their reported first choices. The standard critique is that this data cannot be taken at face-value, because the Boston mechanism gives clear incentives for agents to manipulate their preferences, even in the absence of rankings dependence (see Dur et al. (2018) for evidence of such behavior in a real-world school choice environment). The response from administrators is that while they understand this argument in theory, it is missing an important issue in practice: *parents just do not like to get something they ranked low in their list* (Cambridge, MA School District, personal communication). If this is correct, then this should be incorporated into matching market models to better reflect how real-world agents make decisions in mechanisms and evaluate outcomes.

The main contribution of this paper is to test this hypothesis: we design and implement an experiment to explore the existence (or lack thereof) of rankings-dependent utility. This would be nearly impossible to do in a field setting, as people usually participate in a mechanism only once and it would be impossible to disentangle their fundamental value from any potential rankings-dependent utility. Thus, we use a laboratory experiment, which will allow us to do precisely that.

The mechanisms that we use for our experiment are the Random Serial Dictatorship (RSD) and the Boston mechanism. We chose these mechanisms because they are two canonical mechanisms that are widely used in practice.² Further, RSD is strategyproof, while the Boston mechanism is not, yet the Boston mechanism may result in agents receiving higher-ranked goods. This allows us to answer not only our main question of rankings-dependent utility, but also to test the hypothesis that a non-strategyproof mechanism may be welfare-enhancing once rankings-dependent utility is taken into account.

A novel and key feature of our experimental design is that we use real objects that are in the room at the time of the experiment and that the participants may take home with them. To determine whether utility is rankings-dependent, in Phase I of the experiment, we first elicit valuations for 20 common objects (backpacks, alarm clocks, phone chargers, etc.) with the multiple price list (MPL) elicitation method. In Phase II of the experiment, five of the objects (a Fjallraven backpack, a Hydroflask water bottle, a Moleskine notebook, a generic ceramic coffee mug, and a package of 4 ballpoint pens) were chosen and the participants were asked to submit a rank-order list of these five objects to a mechanism. The mechanism (either RSD or Boston, depending on the treatment) produces an allocation of one object to each participant. After the mechanism, we once again elicit each participant's valuation for the object that they were allocated in the mechanism. The Phase I valuation measures the fundamental value $v(x)$ independent of any mechanism, while the Phase II valuation measures $v(x) + \rho(j)$, the value for x when it is received in a mechanism in which it is ranked j^{th} . So, the difference—which we call Net Value (NV)—is a measure of ρ , the rankings-dependent utility term:

$$NV(j) = \text{Phase II value} - \text{Phase I value} = (v(x) + \rho(j)) - v(x) = \rho(j).$$

Under the hypothesis that utility is not rankings-dependent ($\rho = 0$), we should see no difference in valuations between the two phases; under the alternative hypothesis, ρ , and hence the difference in valuations, is a non-zero function that is decreasing in the rank of the object received.³

The use of real goods is a significant departure from the experimental literature on matching mechanisms which usually uses fictitious “goods” with induced monetary values (Hakimov and Kübler (2021) provide a recent survey of this literature). An induced values design is insufficient for our purposes because rankings-dependent preferences, by construction, are subjective so we need to elicit them. We use a standard elicitation mechanism, the multiple price list, that elicits this subjective utility in dollar terms. Monetary prizes would not work because we would be eliciting the value of money in dollar terms and it is almost certain that we would find that people value money at its monetary value. We would therefore conclude that $\rho(\text{rank}(x))$ would be zero because of

¹ This can be viewed as a rankings-dependent endowment effect, similar to the well-established standard endowment effect that participants value objects more when they own them than when they do not Ericson and Fuster (see 2014, for a survey). One of the leading explanations of the endowment effect is expectations-based reference dependence (EBRD, Kőszegi and Rabin, 2006; Kőszegi and Rabin, 2007; Kőszegi and Rabin, 2009). Recent work by Meisner and von Wangenheim (2023) and Dreyfuss et al. (2025) studies EBRD in the context of matching mechanisms. We discuss these papers and the relation to ours later in the Introduction. We also discuss the endowment effect further when describing the experimental design in Section 3.

² Deferred acceptance Gale and Shapley (DA, 1962) is another popular mechanism used in practice. RSD is a special case of DA where the priorities at each object are equivalent (and random). We chose RSD and Boston because these are simpler mechanisms to explain than DA.

³ It is important to emphasize that while MPL elicitation may itself be susceptible to certain behavioral “biases” (see, e.g., Andersen et al. (2006)), we use the *same* elicitation method in both phases and compare *differences* in valuations across phases to compute Net Value. Thus, nonzero Net Values cannot be attributed to the choice of elicitation method. We elaborate on this point in Section 3.

this improper elicitation. The use of real goods also more faithfully simulates real-world environments, where participants must form their own preferences rather than have them being induced.⁴

Our main hypothesis of interest is that the NV—which measures $\rho(\text{rank}(x))$ —should be non-zero, and in particular decreasing in the reported rank of the good received in the mechanism. For the RSD treatment, NV is nearly monotonically decreasing, from an average of +\$2.87 for participants who receive their top-ranked good to an average of -\$0.69 for participants who receive their fifth-ranked good (out of five). For the Boston treatment, on the other hand, there is a much smaller increase in NV for the first ranked good (only about +\$0.60), and, looking at the raw data averages, there is no clear evidence that NV is decreasing in rank. Non-parametric tests provide statistical support for these impressions. Moving to regression analysis, we find some support for the hypothesis in both treatments, with the rank being a statistically significant predictor of NV for both the RSD treatment and the Boston treatment separately, as well as for the pooled sample. Interestingly, both the Phase I value and a risk aversion regressor (measured using the Holt-Laury switching point) are strongly statistically significant in the Boston treatment, but not in the RSD treatment.

Beyond simply exploring the possible existence of rankings-dependent preferences, we also study some of the implications they may have in terms of truth-telling and welfare. With the introduction of rankings dependence into preferences, truth-telling is no longer an equilibrium of either RSD or Boston. This is because participants now have an incentive to rank less popular goods higher under both mechanisms, to achieve a higher rankings-dependent utility, and solving for an equilibrium becomes very complex. In Section 3, we provide some theoretical results under certain simplifying assumptions that are relevant for the experiment. In particular, we show that in equilibrium, relative to RSD, the Boston mechanism has (i) weakly less truthful reporting (Theorem 1), (ii) weakly more agents getting their top-ranked object (Theorem 2), and (iii) weakly higher welfare (Theorem 3).

In the experiment, we find no differences in the rates of truth-telling between the two treatments according to a wide range of measures. This is interesting given the plethora of experimental work going back to Chen and Sönmez (2006) that generally finds much less truth-telling in Boston than in deferred acceptance (which is equivalent to RSD in our setting). In our experiment, this is driven by much lower rates of truth-telling in RSD relative to the literature (as opposed to higher rates of truth-telling in Boston).⁵ Note that this is what would be expected if preferences are indeed rankings-dependent: reporting truthfully (i.e., according to elicited Phase I values) is no longer an optimal strategy, and so the truth-telling rates in RSD should be lower than in other experiments.⁶

To summarize, we find mixed evidence that utility is rankings-dependent in matching mechanisms. Data from the RSD treatment is consistent with the hypothesis, while evidence from the Boston treatment is less clear-cut, though we do find rank to be a significant predictor after controlling for subjects' risk aversion. We find no differences in truth-telling nor welfare in our experiment, but this will depend on the details of the preference environment: we are the first (to our knowledge) to show theoretically that in some settings, rankings-dependent utility can upend the standard welfare comparisons between mechanisms.⁷

A further contribution of our paper is the use of real goods in matching market experiments. This design choice was necessitated by the nature of the rankings-dependent preferences we are trying to study, but we think it may be useful for other researchers in the experimental matching literature, which as of now has almost exclusively used induced values designs. While the use of real goods certainly complicates some aspects of experimental design and analysis, it also more faithfully simulates real-world markets, where agents must form their preferences rather than having them given to them. To the extent that preference formation plays a role in how agents participate in matching mechanisms—whether via rankings-dependence as we model it, or other channels—real goods experiments will be important in understanding the outcomes of matching mechanisms. We provide some initial evidence of such a phenomenon in a simple setting (RSD), though much more work is needed to more thoroughly understand how agents form preferences in matching settings.

⁴ The only other example of real goods in a matching experiment we are aware of is Guillen and Hakimov (2018), who conduct a field experiment to study the impact of top-down advice on truth-telling rates in a classroom setting in which students were assigned to one of three term paper topics. Similar to us, they faced the issue of how to elicit subjects' true preferences outside of the mechanism, so they could compare to behavior inside of the mechanism. They solved this problem by first having students believe they could choose any of the three topics and asking them to indicate their favorite. Later in the semester, they "surprised" the students and told them that 1/3 of the class had to be assigned to each of the three topics, and the students were asked to submit a rank list. The final allocation was determined using the top trading cycles mechanism. Notice that this method elicited an ordinal preference for the top choice only.

⁵ These comparisons are not perfect, as most of this literature studies DA, while we study RSD, which is a special case of DA. A survey of matching experiments by Hakimov and Kübler (2021) reports truth-telling rates in the (arguably more complex) DA mechanism in the range of 55–85 %, though this varies with the details of the treatment. Li (2017) does study RSD specifically, and finds truth-telling rates of about 60 %, compared with 40 % for our experiment. All of these experiments use an induced-values design.

⁶ While the lower rates of truth-telling in RSD is an interesting result, and is consistent with our theory, the lack of differences across treatments is less significant, and could just be an artifact of the details of our experiment, and in particular the goods that were chosen. (Indeed, our theoretical results only say that RSD should have weakly higher rates of truth-telling, which is consistent with the data.) Further, the use of real goods also may complicate comparisons with previous studies: with induced values, if a participant does not understand the mechanism, a natural default may be to just rank the fictitious goods according to the values given, which would lead to higher rates of truth-telling in induced values environments. Thus, while induced values provide greater levels of control for the experimenter, the results from such experimental designs may not carry over to real-world settings where actual goods are being distributed.

⁷ In general, solving for the equilibrium is very difficult once rankings-dependent preferences are introduced, and our theoretical results hold only under particular assumptions on the preference environment. While we attempted to replicate this in the experiment as much as possible, the use of real goods instead of induced values inherently gives us less control over the preferences. Thus, the lack of differences given the specific objects and design of our experiment does not preclude differences in other environments.

1.0.1. Related literature

One of the main motivations for this paper is the growing body of evidence of “mistakes” in strategyproof mechanisms (see the first paragraph of the introduction for references). There are several ways one can proceed from these observations. Assuming that the underlying preference model is correct, and these mistakes are actually mistakes, one response is that the designer should simply invest more in communicating how the mechanism works and teaching the participants that truthful reporting is in their interest. [Rees-Jones and Skowronek \(2018\)](#) conduct a “lab-in-the-field” experiment in which they recruit medical students who have just gone through the National Resident Matching Program (NRMP) to participate in a related lab experiment, and find a significant fraction of participants did not report truthfully, despite having just participated in the same mechanism in a high stakes environment in which the NRMP invests heavily in tutorials describing how the mechanism works. If this approach is unsuccessful, another possibility is to design mechanisms that are more easily recognizable as strategyproof by the participants on their own. For instance, [Li \(2017\)](#) introduces the notion of obvious strategyproofness as a desideratum for mechanism design, with the idea being that if the mechanism is designed to satisfy this criterion, the players will be able to recognize themselves that truthful reporting is a dominant strategy, and will be more likely to report truthfully. Li’s paper has led to a rapidly expanding literature on obvious mechanism design as a way to limit mistakes by participants.⁸

We take a different approach in this paper, which is a reassessment of the assumption that agent preferences are determined solely by the characteristics of the goods they receive. We discuss a few other recent papers that have explored related ideas.

The closest paper to ours theoretically is [Meisner \(2023\)](#). He proposes an equivalent model of utility that consists of both a fundamental value plus a rankings-dependent component. He then focuses on strategyproof mechanisms, and proves that any non-truthful preference ranking can be rationalized as optimal for some beliefs over match probabilities (what he refers to as “attainability distributions”, which are determined by the mechanism itself combined with beliefs about the strategies of the other agents).

[Meisner and von Wangenheim \(2023\)](#), [Dreyfuss et al. \(2022\)](#), [Dreyfuss et al. \(2025\)](#), and [Chen et al. \(2024\)](#) all focus specifically on expectations-based reference-dependent preferences (EBRD preferences for short, also referred to as EBLA for expectations-based loss aversion; [Kőszegi and Rabin \(2006\)](#)) as a possible explanation for seemingly dominated choices in strategyproof mechanisms. [Meisner and von Wangenheim \(2023\)](#) introduce loss aversion into school choice problems and theoretically study its implications for submitting non-truthful rank order lists. [Dreyfuss et al. \(2022\)](#) re-evaluate the experimental data of [Li \(2017\)](#), who finds mistakes in the non-obviously strategyproof RSD mechanism, and find that EBRD preferences might explain this behavior.

[Dreyfuss et al. \(2025\)](#) build a matching model with EBRD preferences and conduct a lab experiment using four different implementations of the DA mechanism: static (student) proposing, static receiving, dynamic proposing, and dynamic receiving. Using a model of loss aversion, they derive predictions on the relative proportion of non-truthtelling behavior across these four mechanisms.⁹ They show that, for plausible values of the loss aversion parameter, the trends in the data qualitatively match those predicted by the theory; that is, they find more truthtelling in the treatments that are predicted to have higher rates of truthtelling according to the theory.

While similar in motivation, the paper by [Dreyfuss et al. \(2025\)](#) is very different from ours, from both a methodological and experimental design perspective. They consider a particular utility model (EBRD) and show that the trends across treatments in the data qualitatively match the trends predicted by EBRD. While these trends are suggestive evidence for EBRD preferences, in terms of overall levels, they also find much more non-truthtelling behavior than the EBRD predictions, and write that “the EBRD model, while explaining a lot of the observed data, appears to be an incomplete explanation”. For instance, another explanation could be that subjects misunderstand the game they are playing. Because of this potential confound, [Chen et al. \(2024\)](#) design a much simpler experimental environment to eliminate the possibility of game form misunderstanding, but which still predicts non-truthtelling behavior for loss-averse subjects. In this simplified environment, they find very high rates of truth-telling and a small and marginally statistically significant treatment effect. This leads [Chen et al. \(2024\)](#) to argue that loss aversion is likely not a major explanation for mistakes in matching mechanisms.¹⁰ Note that both [Dreyfuss et al. \(2025\)](#) and [Chen et al. \(2024\)](#) use induced values designs, so neither experiment would be able to capture other aspects of rankings-dependent preferences that would only be found with a real-goods design, as in our paper.

Our experiment, on the other hand, was designed to provide a direct measurement of non-standard preferences. While we are agnostic on the underlying source, we think this is a feature, rather than a bug, of our design: by taking a direct measurement of non-standard preferences, we avoid the potential confounds that arise in trying to indirectly infer their existence from qualitative trends across treatments that would be predicted by a particular theory, as discussed above. We think this makes our paper a useful complement to the other approaches that have been taken. Further, both [Dreyfuss et al. \(2025\)](#) and [Chen et al. \(2024\)](#) use induced values and reduce the game to an individual decision problem where participants are effectively asked to choose between objective lotteries. We use real goods, and have participants play in a multiplayer game with other real subjects. This is important to the extent that factors such as preference formation over objects and relative comparisons of one’s own outcome against others influence agent preferences, issues which are likely to be present in real-world settings. While the goal of the present paper was more modest—to

⁸ Theoretical explorations include [Ashlagi and Gonczarowski \(2018\)](#), [Troyan \(2019\)](#), [Pycia and Troyan \(2023\)](#), and [Bade and Gonczarowski \(2017\)](#). For lab experiments, see [Zhang and Levin \(2017\)](#) and [Bó and Hakimov \(2023\)](#).

⁹ What we call non-truthtelling behavior they refer to as “non-straightforward behavior”.

¹⁰ In addition to risk aversion, we also had our subjects complete a standard loss aversion task. We did not find loss aversion to be a significant predictor in any of our regressions.

simply evaluate the existence of non-standard preferences—we think that a natural direction for future work is deeper analysis of the microfoundations behind them.

2. Model

2.1. Preferences and mechanisms

There is a set $I = \{i_1, i_2, \dots, i_N\}$ of agents and a set $X = \{x_1, x_2, \dots, x_N\}$ of objects. A **matching** is a function $\mu : I \rightarrow X$ where $\mu(i) \in X$ is the object that is assigned to agent i . There are an equal number of agents and objects, and so we assume that all matchings assign a unique object to every agent, i.e., if $i \neq j$, then $\mu(i) \neq \mu(j)$. Let \mathcal{M} denote the set of matchings. Notice that we assume each object has capacity 1, an equal number of agents and objects, and the objects have no preferences or priorities over the agents. All these restrictions can easily be generalized to capture important features of real-world settings such as school choice. We make these assumptions because they are all that is needed to study our main phenomena of interest, and they ensure that the model corresponds as directly as possible to the experiment that we run.

Let \mathcal{P} be the set of all strict ordinal rankings over X . For any $P_i \in \mathcal{P}$, we write xP_iy to denote that agent i strictly prefers x to y . We use R_i for the corresponding weak relation, i.e., xR_iy if either xP_iy or $x = y$. A **mechanism** is a function $\psi : \mathcal{P}^N \rightarrow \mathcal{M}$. We write $\psi_i(P) \in X$ for the object allocated to i at preference profile $P = (P_1, \dots, P_N)$. Every mechanism induces a game in which the action space for each agent is \mathcal{P} .

In most of the matching literature, it is assumed that an agent's utility is determined solely by the object received. We deviate from this assumption by allowing an agent's utility to depend on both (i) the object received and (ii) the position in which they ranked the object in their preferences. Formally, agent i 's utility from submitting a reported preference ranking P_i and receiving object x is

$$u_i(x, P_i) = v_i(x) + \rho(j). \quad (1)$$

where $j = |\{x' \in X : x' R_i x\}|$ is the rank of object x in the reported preference list. We call $v_i(x)$ agent i 's **fundamental value** for object x , and $\rho(j)$ agent i 's **rankings-dependent utility** from receiving the object that she ranked in the j^{th} position. Note that in this formulation, the function $\rho(\cdot)$ is the same for all agents. This could be generalized to allow for heterogeneity, but given our experimental design, it is infeasible to measure a different $\rho(j)$ for each agent and each j , and so we omit this generalization in the model.

The main assumption of our model is that, all else equal, participants will prefer to get objects when they rank them higher in their submitted preferences. For example, receiving good x provides more utility when it is ranked 2nd than when it is ranked 4th. Hence, we assume the function ρ is such that $\rho(1) \geq \rho(2) \geq \dots \geq \rho(n)$. We discussed in the introduction possible microfoundations for rankings-dependent utility. In the experiment, we do not attempt to discern between these explanations, but seek simply to determine whether preferences do in fact depend on rankings.

For our experiment, we consider two mechanisms which we describe here: random serial dictatorship and the Boston mechanism.

Random Serial Dictatorship (RSD)

The random serial dictatorship mechanism works as follows. Each agent submits a strict ordinal ranking over all of the objects. The mechanism then draws an ordering of the agents randomly from the uniform distribution over all possible agent orderings, and proceeds in rounds as follows:

- Round 1: The first agent in the order is assigned their top-ranked object according to their submitted preference ranking.
- Round 2: The second agent in the order is assigned their top-ranked object that was not assigned in the first round.
- Round $k = 3, \dots, N$: The k^{th} agent in the order is assigned their top-ranked object that was not assigned in any earlier round $1, \dots, k - 1$.

Boston Mechanism

The Boston mechanism works as follows. Each agent submits a strict ordinal ranking over all of the objects. The mechanism draws an ordering of the agents randomly from the uniform distribution over all agent orderings, which will be used as a “tie-breaker” below. The mechanism then proceeds in rounds as follows:

- Round 1: The mechanism considers the top-ranked object of each agent. If only one agent has ranked an object first, the object is assigned to that agent. If more than one agent has ranked an object first, the mechanism assigns the object to the agent among them who was ranked highest in the random agent ordering drawn above. The agents and objects that were assigned leave the market. If all agents have been assigned an object, the mechanism ends. Otherwise, all agents and objects that were not assigned in this round proceed to the next round.
- Round 2: Only agents who were not assigned an object in the first round participate. The mechanism looks at the second-ranked choices of all such agents. If only one agent has ranked an object second, the object is assigned to that agent. If more than one agent has ranked an object second, the mechanism assigns the object to the agent among them who was ranked highest in the random agent ordering drawn above. The agents and objects that were assigned leave the market. If all agents have been assigned an object, the mechanism ends. Otherwise, all agents and objects that were not assigned in this round proceed to the next round.
- Round $k = 3, \dots, M$: The mechanism proceeds exactly as in round 2, except it now considers the k^{th} -ranked objects in the rank lists of the agents who participate in this round.

The definitions of RSD and Boston given above are simplifications of a more general class of mechanisms. It is easy to extend the mechanisms to incorporate features such as multiple copies of each object, outside options, and priority lists over the agents for each object, all of which are common in settings such as school choice (see, for instance, [Abdulkadiroğlu and Sönmez \(2003\)](#)). We use these definitions because they are sufficient to capture the properties of assignment mechanisms that are the focus of our investigation while still remaining parsimonious enough to design a lab experiment to cleanly test our hypotheses of rankings-dependent preferences.

2.2. Incentives and welfare

Two key concerns when designing any allocation mechanism are the incentives they provide participants with regard to reporting their preferences and the equilibrium welfare of the resulting allocation. In this section, we briefly discuss these issues for both RSD and Boston. After discussing the details of the experimental design, we will return to these issues with formal theorems and experimental hypotheses based on them.

Truth-telling

A common desideratum when designing a matching mechanism is strategyproofness, the property that it is a weakly dominant strategy in the mechanism-induced game for each agent to submit their true preference ranking to the mechanism. In the standard matching literature where utility is not rankings-dependent (i.e., $p(j) = 0$ for all j), this corresponds to submitting the preference ranking that lists objects in decreasing order of fundamental value. This strategy is, for obvious reasons, called the *truthful strategy*. Without rankings-dependent utility, RSD is well-known to be strategyproof while Boston is not. Intuitively, in Boston an agent with fundamental values $v_i(x) > v_i(y)$ may want to lie and report yP_ix , if x is likely to be very popular while y is almost as good and easier to get, because failing to get x might lead to getting an even worse object z . Indeed, [Troyan and Morrill \(2020\)](#) show that not only is Boston manipulable, it is *obviously* manipulable, and there is empirical evidence from school choice data that show families engaging in precisely this type of non-truth-telling behavior ([Dur et al., 2018](#)).

With the introduction of rankings-dependent utility in Eq. (1), the strategyproofness property is not well-defined because there is no one-to-one mapping of utility to a preference ranking of the objects. Nevertheless, it is straightforward to still define the truthful strategy with respect to fundamental values exactly as above as the strategy that submits a preference ranking that lists objects in decreasing order of fundamental value. Though this terminology is not as descriptive in this model where fundamental value does not reflect total utility, we continue to use it to maintain consistency with the previous literature. Due to the rankings-dependent utility, it is no longer the case that choosing the truthful strategy is weakly dominant in RSD. There is extensive empirical and experimental evidence (see the first paragraph in the introduction for references) that agents do not submit the truthful strategy to the mechanism in deferred acceptance, a mechanism that is equivalent to RSD in our setup. Indeed, this is the main motivation for incorporating and investigating rankings-dependent utility in the RSD mechanism.

Welfare

We introduce a small example to exhibit the welfare comparisons of RSD compared to Boston. Consider 3 agents and 3 goods, with the fundamental values given in the table below.

	$v_i(x)$	$v_i(y)$	$v_i(z)$
i_1	1	0.7	0
i_2	1	0.7	0
i_3	1	0.7	0

When there is no rankings-dependent utility ($\rho(j) = 0$ for all j), then, because agents have symmetric preferences, every allocation results in a sum of utilities that is equal to 1.8. Thus, from a total welfare perspective, RSD and Boston are equivalent (for any equilibrium).

Now, consider $\rho(1) = 0.1$ for all agents, and $\rho(2) = \rho(3) = 0$. It is easy to check that truth-telling with respect to fundamental values (i.e., all agents report ordinal preferences $xPyPz$) is the unique equilibrium of RSD. No matter the random ordering, all final allocations result in a sum of utilities that is equal to $W^{RSD} = (1 + 0.1) + 0.7 + 0 = 1.9$.¹¹

Next, consider Boston with the following strategies: agents i_1 and i_2 report truthfully with respect to fundamental values, while agent i_3 reports $yPzPx$. It can be checked that this is an equilibrium. In this equilibrium, one of the agents—agent i_3 in our example—reports her second-best good, good y , first. This ensures that she gets y for sure, but also leaves the remaining two agents with a higher chance of receiving the best good, x (in particular, agents i_1 and i_2 each have a 50/50 chance of receiving x and z). Since the preferences of i_1 and i_2 are symmetric, any final allocation results in a sum of utilities $W^B = (1 + 0.1) + (0.7 + 0.1) + 0 = 2 > W^{RSD}$.

¹¹ Note that this defines welfare as the sum of utilities, which includes the rankings-dependent component. It might be argued that rankings-dependent utility is a “mistake”, and so should not be included in the welfare calculation, which should instead be just the sum of fundamental values (we thank an anonymous referee for raising this issue). Based on our discussions with school administrators, who view rankings-dependence as a relevant phenomenon (see the Introduction), we have chosen to include the rankings-dependent utility in our primary definition of welfare. However, we acknowledge the alternative measure, and for the experiment, we also report results for welfare excluding the rankings-dependent component (see footnote 24).

Thus, Boston results in a greater overall total welfare. Intuitively, the reason is that Boston incentivizes some agents to rank the good with the second-highest fundamental value first. In equilibrium, this means that there will be two agents who are getting a good ranked first, and so, both of these agents will receive the rankings-dependent utility $\rho(1)$. This is our motivation for comparing the RSD and Boston mechanisms in our experiment. In the next section, we will provide formal theorems and corresponding experimental hypotheses that generalize these intuitions for our experimental environment.¹²

3. Experimental design

The experiment consists of two phases that participants complete in succession. We have two treatments that only differ in the matching mechanism run in Phase II. These treatments are *RSD* which runs the random serial dictatorship and *Boston* which runs the Boston mechanism. Hereafter, we will continue to use italics for the treatment names and regular text for the mechanisms. We will describe each phase here, and the full instructions are provided in Appendix D.

3.1. The two phase experiment

Phase I

Phase I is identical for both treatments. The participants complete 22 incentivized tasks. The first 20 tasks consist of valuing 20 different objects with the multiple price list elicitation method. See Table C.8 in the appendix for a description of each of the 20 objects including the monetary values (measured as the prices on Amazon.com where we purchased the objects) and average elicited values.

The multiple price lists are framed as willingness to accept (WTA) and consist of two screens. The first screen elicits the value of each object in dollar increments and the second screen elicits the value in two cent increments. In particular, for each of the 20 objects, the participants are told they have been allocated the object and then have the opportunity to exchange the object for various amounts of money. On the first screen, they see a list where each row represents keeping the object or exchanging it for an amount of money that ranges from \$1.00 to \$50.00. The participants choose the row that is the last row where they prefer keeping the object to exchanging it. We provide a screen shot in Fig. D.2(a) in Appendix D where the participant has selected the row with the dollar amount \$16.00. The participants can change their minds and click a different row, and then confirm their choices once they have come to a final decision. The second screen is presented identically to the first except that the amounts of money range from \$x.02 to \$x + 1 in \$0.02 increments where x is the value of the last row (the one they had selected) from the first screen. We provide a screen shot in Fig. D.2(b) in Appendix D where the participant has selected the dollar amount \$16.56.

In order to assist the participants in this valuation task, pictures of each object are provided on the screens as the objects are valued. We also had the physical objects at the front of the room and would bring them over for further inspection on request. All participants value the frisbee first, the set of picture frames second, the cable spirals third, and the final 17 objects in random order. The reason to fix the first three objects is to allow participants to gain familiarity with the elicitation method. These three objects are not relevant for Phase II and are not analyzed in the results section.

If one of these valuation tasks is selected for payment for a given participant, we pay the participant in the standard way for multiple price lists. We randomly and uniformly draw a number between 1 and 50 which corresponds to a dollar amount the participant will receive. The participant then receives the object if they had indicated that they preferred the object to that amount of money and the dollar amount if they had indicated that they preferred the amount of money to the object. In the case that the randomly drawn number is exactly equal to the dollar amount in the last row where they would keep the object (the row they had clicked on the first screen), then a second randomly and uniformly drawn number between 1 and 50 is generated. In this case, the number corresponds to the two cent increments on the second screen (1 was \$0.02, 2 was \$0.04, etc.) and the participant receives the object if they had indicated that they preferred the object to the amount of money and the dollar and cents if they had indicated the opposite.

The final two tasks are incentivized risk aversion and loss aversion elicitation tasks. The risk aversion task is the classic Holt and Laury (2002) price list task. To be consistent with the previous 20 tasks, there are 50 rows of lottery choices between Lottery A, high payoff of \$24.00 with x% chance and low payoff of \$20.00 with 100 – x% chance, and Lottery B, high payoff of \$38.00 with x% chance and low payoff of \$12.00 with 100 – x% chance. The chance of the high payoff, x, increases across rows from 2 % to 100 %. The participants select the last row where they preferred Lottery A. If this task is selected for payment for a given participant, we randomly and uniformly draw a number between 1 and 50 and run Lottery A if that number is less than or equal to the number of the last row where they preferred Lottery A, and we run Lottery B otherwise.

The loss aversion task compares risky choices with gains to risky choices with losses. Again, there are 50 rows of lottery choices. For this task, the participant chooses between Option A, \$20.00 for sure plus a 50 % chance of a \$10.00 bonus, and Option B, \$30.00 for sure plus a 50 % chance of losing \$x. The loss in Option B varies from \$20.00 to \$0 in increments of \$0.40 across the rows. The

¹² There is a recent strand of literature that emphasizes that non-strategyproof mechanisms may outperform strategyproof ones in equilibrium even with just standard preferences because the opportunity to misrepresent their preferences gives a channel by which agents can express some information on their cardinal utilities (Abdulkadiroğlu et al., 2011; Troyan, 2012; Abdulkadiroğlu et al., 2015; Fragiadakis and Troyan, 2019). This will happen when some agents have a stronger cardinal preference for the middle good y, as Boston gives them a channel through which they can express this preference (by ranking it first), while RSD does not. This will not be an equilibrium in the example here without rankings-dependence, but more generally, this channel in favor of Boston is still present. The additional inclusion of rankings-dependent utility will only serve to amplify the potential welfare gains of Boston.

participants select the last row where they prefer Option A. If this task is selected for payment for a given participant, we randomly and uniformly draw a number between 1 and 50 and run Option A if that number is less than or equal to the number of the last row where they preferred Option A, and we run Option B otherwise.

Finally, the participants complete an unincentivized questionnaire to conclude the phase. The questionnaire includes the standard cognitive reflection task questions as well as demographics and academic endeavors.

Phase II

After completing the 22 tasks and questionnaire in Phase I, the participants move on to Phase II. In this phase, the participants are randomly matched into groups of five to engage in a matching market with the five goods. We use the same five goods for Phase II in every session, and they are all taken from the set of twenty goods valued in Phase I. The goods for Phase II are:

1. Fjallraven backpack: A small 16L gray backpack from the popular brand Fjallraven with a monetary value of \$66.95.¹³
2. Hydroflask waterbottle: A light-blue reusable water bottle from the popular brand Hydroflask with a monetary value of \$32.96.
3. Moleskine notebook: A notebook with 192 pages and a high-quality black cover from the popular brand Moleskine with a monetary value of \$21.90.
4. Blue ceramic mug: A generic blue ceramic mug with a monetary value of \$11.99.
5. Set of 4 Uni-ball pens: A set of four fine-point black rollerball pens from the well-known brand Uni-ball with a monetary value of \$6.88.

Each participant engaged in a matching market corresponding to their treatment, *RSD* or *Boston*, just one time. They then value the good they received from the mechanism using the exact same multiple price list elicitation method used for the 20 goods valued in Phase I.

At the end of the experiment, participants were paid for a single task that was equally likely to be one of the tasks in Phase I or the unique task in Phase II. In the case a task from Phase I was selected, each of the 22 tasks was equally likely to be selected for payment. As there are more tasks in Phase I, each is less likely to be selected for payment than the Phase II task. We discuss the reasons for this payment structure in Remark 3 in the next subsection.

Remark 1. The participants only engage in a single matching market. We chose this method so that they would feel greater ownership of the good they received. In order to make sure the participants fully understood the procedure we presented them with instructions that included an example (with different goods than in the actual market), had them complete a quiz regarding a second example which required them to answer all the questions correctly before moving on, and provided them with an 8 minute practice period in which they could engage in as many markets as they wanted against robot players. We feel comfortable that they understood the mechanisms, because they almost all received 100 % on the quiz the first time, rarely used the full 8 minutes to practice against the robots, and because neither the Boston nor the RSD mechanisms are particularly complicated mechanisms.

3.2. Theoretical predictions and experimental hypotheses

Rankings-Dependent Utility

Our first and main hypothesis assesses whether preferences are indeed rankings-dependent.

Recall our model with rankings-dependent utility, Eq. (1):

$$u_i(x, P_i) = v_i(x) + \rho(j).$$

To test the hypothesis that preferences are rankings-dependent, we are interested in the term $\rho(j)$. Our experimental design allows us to recover exactly this. Suppose that, in the matching mechanism in Phase II, a participant i receives object x , which was reported as her j^{th} ranked good. The valuation elicitation at the end of Phase II measures $v_i(x) + \rho(j)$. In Phase I, the participant also valued object x , independent of any mechanism or ranking context. Thus, the Phase I valuation of object x is $v_i(x)$. So the net value, $NV(j)$, the difference in valuations for the same object x between Phase II and Phase I, is the rankings-dependent component $\rho(j)$:

$$NV(j) = \text{Phase II value} - \text{Phase I value} = (v_i(x) + \rho(j)) - v_i(x) = \rho(j)$$

If $\rho(j)$ is decreasing in j as assumed in Section 3, the same should be true for the net value between Phase I and Phase II. This gives rise to our first hypothesis.¹⁴

Hypothesis 1. Participants prefer to get objects they rank higher, and so $NV(j)$ will be decreasing in submitted rank j .

Remark 2. One might be concerned that in the course of the experiment subjects are asked to value one object twice, and a desire to appear “consistent” could bias the results. First, notice that such a desire actually works against finding an effect, as it will tend to compress Net Value towards 0; in other words, if we do find an effect, it will not be driven by the desire for consistency. Second,

¹³ The goods were purchased on Amazon.com. The prices on Amazon vary slightly from day to day so the monetary values given here are approximate.

¹⁴ Several commenters have mentioned that we might also want to consider the percentage change in Net Value—i.e., calculate Net Value = (Phase II value - Phase I value)/(Phase I value)—in addition to a linear formulation. A problem arises in that this does not allow us to cleanly disentangle the fundamental value from rankings dependence. This is because we would have $NV(j) = \rho(j)/v_i(x)$, and we would be unable to determine whether any changes in NV were due to changes in ρ or changes in v .

while impossible to eliminate completely, we designed the experiment to mitigate this concern as much as possible. By placing the questionnaire and explanation of the matching mechanisms in the middle of the experiment (so that, by the time they submitted their Phase II valuation, a lot of time had passed) and having them value 20 objects in Phase I (15 of which were not relevant for the experiment), we are confident that most subjects did not remember their initial valuation for the good that they received in Phase II.

Remark 3. [Endowment Effect] The “endowment effect” is the well-established finding that participants value objects more when they own them than when they do not (Ericson and Fuster, 2014). One of the leading explanations for the endowment effect is expectations-based loss aversion. Expectations-based loss aversion is also a potential explanation for rankings-dependent utility in matching mechanisms, as shown by Dreyfuss et al. (2025) (cf. the Introduction, where we discuss this in more detail). However, there are also two other sources of “endowment effects” that could be present in our experiment: the elicitation mechanism itself and the probability that a given task is selected for payment.

To the extent that rankings-dependence is due to expectations-based loss aversion, the first potential endowment effect falls under what we are trying to measure, while the other sources are not of interest to us. With this in mind, we designed the experiment specifically so that these latter two phenomena do not affect our hypotheses. First, we use the same WTA elicitation mechanism in both Phases so any endowment effect from the elicitation mechanism immediately cancels out when calculating Net Value. The probability of payment effect is a little trickier, but it is also not an issue for testing our hypotheses because they are statements about comparisons across Net Values for which an added constant is irrelevant.

To make this point about probability of payment more formally, we can augment Eq. 1 by adding a “probability of payment” endowment effect term to the utility function as follows:

$$u_i(x, P_i) = v_i(x) + \rho(j) + \xi_k$$

where ξ_k represents the endowment effect for phase $k = I, II$. Because the Phase II task has a higher probability of being chosen for payment, we expect $\xi_{II} > \xi_I$. Then, Net Value (Eq. 2) becomes

$$\begin{aligned} NV(j) &= \text{Phase II value} - \text{Phase I value} \\ &= (v_i(x) + \rho(j) + \xi_{II}) - (v_i(x) + \xi_I) \\ &= \rho(j) + \xi_{II} - \xi_I. \end{aligned}$$

Hence, it is true that Net Value entangles rankings-dependence with the probability-of-payment endowment effect so we cannot make any statements about the exact values we elicit for Net Value. Importantly, though, when we compare across rankings, the probability-of-payment endowment effect constant is irrelevant. Specifically, Hypothesis 1 predicts that Net Value is decreasing across rank which is still the case when we add a constant to all Net Values.

Truth-telling

Our next two hypotheses concern truth-telling and preference-reporting. Recall that, in the standard matching model without rankings-dependent utility, RSD is strategyproof while the Boston mechanism is not. In the case of Boston, agents may manipulate their preferences implied by their fundamental values by ranking popular objects lower, because they are likely to be taken in earlier rounds of the mechanism. But how do these properties extend to the case of rankings-dependent utility?

Truth-telling with respect to fundamental values will no longer be a dominant strategy of either mechanism, because agents may want to manipulate and rank objects that are less popular highly so that they receive a higher-ranked object and the corresponding increase in $\rho(j)$. In general, it is difficult to solve for an equilibrium with rankings-dependence. However, by putting more structure on the model, we will be able to show some results. In particular, we make the following assumption:

Assumption 1. For all $x_j \in X$, $v_i(x_j) = v_{i'}(x_j) := v_j$ for all $i, i' \in I$ and that these values are common knowledge. Further, $v_1 \geq v_2 \gg \bar{v} = v_3 = \dots = v_n$.

In words, we assume that there is complete information, a common ordinal preference, and that objects x_1 and x_2 are much better than the other objects.¹⁵ The motivation for this assumption is that goods 1 and 2 are strongly enough preferred by all agents to all other goods, so that the main strategic decision is whether to rank x_1 or x_2 first. The exact goods in the experiment were chosen with this model of valuations in mind: given the popularity of Fjallraven backpacks and Hydroflask water bottles to our participant pool of undergraduate students, we expected these two objects to be the most popular, and strongly preferred to the other three objects (a notebook, coffee mug, and pens). Indeed, 84% of the participants value the backpack and water bottle as the two best goods, as measured by the Phase I elicitation. Table 1 in the appendix shows that the average elicited values for the five objects used in the matching mechanism were \$28.24 (backpack), \$22.56 (water bottle), \$9.11 (notebook), \$6.53 (mug) and \$5.33 (pens).

Given the preference assumption, for the theory, we focus on equilibria that have the following structure. For RSD: n_1 of the agents rank x_1 first and x_2 second; the remaining $n - n_1$ agents rank x_2 first and x_1 second; for the remaining goods x_3, \dots, x_n , each agent draws a ranking of these goods uniformly at random from all possible rankings. For Boston: n_1 agents rank x_1 first; the remaining $n - n_1$ agents rank x_2 first; for the remaining goods x_3, \dots, x_n , as in RSD, each agent draws a ranking of these goods uniformly at

¹⁵ Though unlikely to ever hold exactly, common ordinal preferences is an assumption commonly made to approximate highly correlated preferences while still maintaining analytic tractability (see, e.g., Abdulkadiroğlu et al. (2011), Featherstone and Niederle (2016), and Fragiadakis and Troyan (2019)). As we explain in this paragraph, we think our preference assumption captures well the fundamental strategic tension we wanted to induce with our selection of objects, which is whether to rank the backpack or the water bottle first.

Table 1
Net Value Regressions.

Treatment	<i>RSD</i>		<i>Boston</i>		All
	(1)	(2)	(3)	(4)	(5)
rank	-0.8795*	-1.2358*	0.0979	-1.8063**	-1.5526***
initial value		-0.0570		-0.2768***	-0.1759***
risk aversion	0.0603		0.2676***		0.1679***
loss aversion	0.0015		-0.1331		-0.0769
CRT score	0.0222		-0.5717		-0.3484
female	-0.1220		-0.6279		-0.5844
Phase I order	0.1077		-0.1333		-0.0328
practice	-0.1485		-0.2194		-0.1477
truthful	-1.6938		-0.7317		-1.1834
_cons	3.6033***	4.5190	0.6334	10.4651**	8.1221**
No. of Obs.	100	100	100	100	200
R-Squared	0.03	0.11	0.00	0.27	0.16

random from all possible rankings. They rank these goods immediately below their top choice, and rank the remaining good that was not their top choice (either x_1 or x_2) at the bottom of their list.

In other words, in RSD, all agents rank x_1 and x_2 first and second, in some order. In Boston, each agent ranks either x_1 or x_2 first, and puts the other option not chosen last in their preferences. This is in line with our motivation that the main source of competition is over goods x_1 and x_2 . Thus, an agent's strategy boils down to whether to rank x_1 or x_2 first, and the key equilibrium object to solve for is the exact number of agents that choose to rank x_1 (the best object according to fundamental value) first in their reported preferences in RSD versus Boston. As we show in the proof of [Theorem 2](#), this number is uniquely determined for each mechanism.¹⁶

Theorem 1. *Suppose the preference assumption holds. If truth-telling with respect to the fundamental value is an equilibrium of the Boston mechanism, truth-telling is also an equilibrium of RSD.*

This theorem suggests that there should be more truth-telling in RSD compared to Boston. Intuitively, truth-telling is an equilibrium of the Boston mechanism when the fundamental value v_1 is so high that it is worth it for every agent to enter the round 1 lottery for x_1 , rather than deviating by ranking x_2 first and receiving payoff $v_2 + \rho(1)$ for sure. In RSD, this deviation is even less profitable, because it does not guarantee x_2 with certainty (though it does guarantee the deviating agent will not get x_1). Thus, if the deviation is not profitable in Boston, it will not be profitable in RSD, either, and so truth-telling with respect to fundamental values is an equilibrium of both mechanisms.

Our experimental design allows us to measure each participant's rankings with respect to fundamental value, because the fundamental values are elicited in Phase I. This gives our second testable hypothesis.

Hypothesis 2. Participants will submit preferences in Phase II that are truthful with respect to fundamental values as measured by the elicitation in Phase I weakly more in RSD than in Boston.

[Theorem 1](#) only discusses the truth-telling equilibrium. It may of course be that truth-telling is not an equilibrium of either Boston or RSD: Once rankings-dependent utility is introduced, even in RSD, an agent may want to deviate from truthful reporting in order to obtain the additional rankings-dependent utility term $\rho(1)$. Even in this case, however, we still expect more agents to misreport by ranking x_2 first in the Boston mechanism, for similar reasons as for [Theorem 1](#). This is formalized in the next theorem.

Theorem 2. *Suppose the preference assumption holds. Then, in any equilibrium of RSD, weakly more agents rank good x_1 first in their preference list than in any equilibrium of Boston.*

This theorem leads to our third hypothesis.

Hypothesis 3. In Phase II, weakly more participants will rank the good with the highest elicited Phase I value first in their preference list in RSD than in Boston.

Welfare

Our final hypothesis concerns the overall welfare of the two mechanisms. Arguably, this is the most important feature of any mechanism, because the ultimate goal of any allocation mechanism is to produce an outcome that maximizes participant satisfaction, taking into account all components of utility, including fundamental values and rankings-dependent components.

¹⁶ There is one caveat to the strategies defined above, which is that if all agents rank x_1 first in Boston ($n_1 = n$), then, given the preference assumption, it is optimal for everyone to rank x_2 second, rather than last, in their preference list. Essentially, this happens when $v_1 \gg v_2$ to the extent that it is worth it for every agent to enter the round 1 lottery for x_1 , in which case x_2 will still be available in round 2 of the mechanism, and Boston effectively becomes equivalent to RSD. The theorems stated below still hold regardless. See the proofs in [Appendix A](#) for details.

Theorem 3. Suppose the preference assumption holds. The equilibrium total welfare of the Boston mechanism is weakly higher than the equilibrium total welfare of RSD.

The intuition for this result is that, given our preference assumption, the sum of the fundamental value components of total welfare is the same for any allocation. Thus, the welfare comparison between any two mechanisms is determined by the sum of the rankings-dependent utility terms:¹⁷

$$W = \sum_{j=1}^n (\# \text{ agents who receive their } j^{\text{th}} \text{ ranked good}) \times \rho(j)$$

In the Boston mechanism, in equilibrium, at least one agent ranks good x_2 first and receives it for sure, and so there are two agents who receive rankings-dependent utility $\rho(1)$.¹⁸ In RSD, even if some agents do rank x_2 first, there is still a non-zero probability that the good will go to an agent who ranked it second, and who thus will receive rankings-dependent utility $\rho(2) < \rho(1)$, resulting in lower total welfare for RSD. The full details of the argument can be found in the appendix.

In the experiment, we measure the total welfare of a mechanism as the sum of the elicited values for the mechanism allocation in Phase II. This gives our final hypothesis.

Hypothesis 4. The sum of the elicited values in Phase II will be weakly higher in Boston than in RSD.

Each of the theorems and experimental hypotheses above compare RSD and Boston using weak comparisons. For instance, they suggest that total welfare under Boston will be weakly higher than under RSD. Although there are valuation vectors that make each comparison strict, for others, the outcomes will be equal. A limitation of our experimental design is that we cannot control the valuation vectors as is possible with an induced value design. For this reason, we consider Hypothesis 1—the one that doesn't rely on any such assumptions about the v 's—as the primary point of interest. At the same time, we still think [Theorems 1–3](#) and their corresponding hypotheses are valuable for providing novel insights into the effects of rankings-dependent preferences on incentives and welfare, and so include them in our results as well.

3.3. Procedures

All experiments were run at the University of Virginia VeconLab with undergraduate students recruited from the Darden BRAD lab recruitment pool. There were a total of 200 participants, 100 for each of the two treatments in a between-subjects design. No feedback was provided before the end of the experiment so each participant is treated as an independent observation for the results. The experiment was programmed and run with z-Tree ([Fischbacher, 2007](#)). For the participants who earned money the average earnings were \$33.59. For the participants who received goods, the average monetary values of these goods was \$36.69.¹⁹ Additionally, all participants received \$6 for showing up.

4. Results

In this section, we turn to the data to analyze the extent to which our experimental results provide support for our four hypotheses. For regressions we use stars to indicate significance at the usual levels (* if $p < .1$, ** if $p < .05$, and *** if $p < .01$).

Hypothesis 1: Net Value

Recall that Hypothesis 1 argues that net value, the elicited value in Phase II minus the elicited value in Phase I, will be decreasing in submitted rank j . As a first look, [Fig. 1](#) provides the average net values for each rank for *RSD*, *Boston*, and both treatments.

For *RSD*, the results presented in [Fig. 1](#) are consistent with the hypothesis. Net value is almost monotonically decreasing, with the exception of the 3rd and 4th rankings (n is only 13 for the latter case, as most people received one of their top 3 ranked goods). Indeed, a non-parametric test rejects the null of no difference among the ranks in favor of the alternative that net value is decreasing in submitted rank ([Jonckheere-Terpstra, \$p = .019\$](#)). On the other hand [Fig. 1](#) seems to show no relationship between net value and submitted rank in *Boston*, and the non-parametric test does not reject the null in this case ([Jonckheere-Terpstra, \$p = .618\$](#)).

To explore these results in more detail, we turn to regression analysis. The regressions are presented in [Table 1](#). For each treatment, we first regress net value on submitted rank alone (regressions (1) and (3)). We then add a number of potential influences on net value including Phase I elicited value (initial value), Holt-Laury switching point (risk aversion), the loss aversion task switching point (loss aversion), score out of three on the three cognitive reflection tasks (CRT score), a dummy for gender (female), the randomly selected order in which the good received in Phase II was valued in Phase I (Phase I order), the number of submitted preference orders in the practice session against robots (practice), and a dummy for whether the received good in Phase II was ranked higher than it would have been according to the implied fundamental values elicited in Phase I (truthful). These regressions are reported in columns (2) and (4), and the pooled regression with both treatments is presented in column (5).

¹⁷ Note that we define welfare to include the rankings-dependent utility term. See footnotes 11 and 24 for further discussion of this issue.

¹⁸ There is also the case that v_1 is so high that all agents rank x_1 first in Boston. However, in this case, all agents will also rank x_1 first in RSD (see [Theorem 2](#)) and the mechanisms become equivalent, so the welfare of the two mechanisms will be the same.

¹⁹ In total, 144 participants earned money and 56 participants received goods.

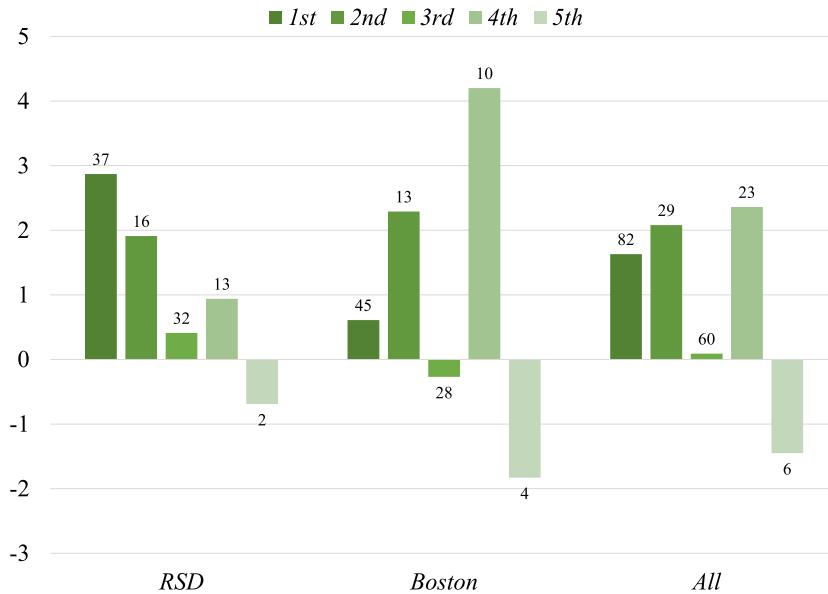


Fig. 1. Net Values with # of obs. on ends of bars.

In line with the non-parametric tests, [Table 1](#) provides support for the hypothesis that net value is decreasing in rank of received good for *RSD*. Net value decreases by \$0.88 or \$1.24 per ranking depending on whether controls are included or not, and the estimates are marginally significant. Surprisingly, in contrast to the non-parametric tests, [Table 1](#) also provides support for the hypothesis for *Boston* when controls are included. For *Boston*, net value decreases by \$1.81 per ranking when controls are included and the estimate is significant. Pooling the data, there is a strongly significant decline of \$1.55 per rank.

Additionally, in *Boston*, participants' initial values negatively impact net value while risk aversion positively impacts net value. The effect from initial value must be carefully evaluated, because initial value is part of the net value. Observe that the estimate -0.2768 means that an increase of \$1.00 in initial value decreases net value by \$0.28, but this can also be translated and interpreted as an increase of \$1.00 in initial value increases final value (the Phase II valuation) by \$0.72. Combined with the positive intercept in the regression, the estimates imply that small initial values are increased and large initial values are decreased. One interpretation of the result then is reversion to the mean; participants perhaps value objects with noise and so low Phase I valuations are more likely to be increased while high Phase I valuations are more likely to be decreased.²⁰

The effect of risk aversion is harder to understand. As we will discuss in the next section, risk aversion impacts truth-telling in *Boston*. Briefly, we show there that risk averse participants are more likely to highly rank a good that they do not like as much but is less popular. Here, this means that when they get this less popular good their net values increase. We are not as confident as to why this may be the case, but one possibility is utility from the relief that they do indeed obtain the good they moved up in their rankings.

Given the discrepancy between the raw data and the regression analysis (particularly with the difficult to interpret positive effect of risk aversion in *Boston*), we think it is prudent to dive a little deeper. As a check of robustness, we ran additional regressions with dummy variables for final rank and for the good received. The first set of dummies allows for a non-linear relationship between rank and net value, while the second allows for the possibility that there is something inherent about the good itself. In order to not have too many regressors relative to data, we only included the controls for initial value and risk aversion. The results are presented in [Table B.5](#) in the appendix.

These results show some similarities and some differences to [Table 1](#). Most notably, the support for the hypothesis goes away. In particular, in *RSD*, the estimates that are significant are for receiving the highest-monetary goods, the backpack and the waterbottle.²¹ The results for *Boston* indicate it is only initial value and risk aversion that matter. Putting them together, there is no evidence in support of the hypothesis in [Table B.5](#). So is [Table 1](#) or [Table B.5](#) correct? [Table B.5](#) has higher values of R^2 , but we also do not want to say it is more correct than [Table 1](#). Indeed, the reason we have it as a check of robustness is that there are two reasons to be wary of the results in [Table B.5](#). First, the dummy variables reduce the power of the estimates for the effects of rank (essentially only considering 20 data points at a time rather than the whole set of 100). Indeed the estimates for ranks in *Boston* are negative indicating that rank does decrease net value, just not significantly, which could be an issue of power. Second, there is clearly high collinearity between the goods received, the initial value, and the final ranks. For example, [Table 1](#) indicates that getting your 1st ranked good

²⁰ For example, a small initial value is more likely to come from a participant who under-valued the good in Phase I relative to their true fundamental value and is therefore more likely to increase the valuation in Phase II.

²¹ The mug is omitted. So the coefficients are interpreted with respect to the mug which has the smallest net value.

Table 2
Truth-telling Rates.

Measure	RSD	Boston	All
Exact			
All	0.40	0.37	0.39
Top Choice	0.78	0.79	0.79
Top 2 Choices	0.60	0.60	0.60
Up to \$2 differences			
All	0.58	0.53	0.56
Top Choice	0.82	0.83	0.83
Top 2 Choices	0.70	0.67	0.69

is best for net value while [Table B.5](#) indicates that getting the backpack is best for net value. Of course, these two outcomes are highly correlated (and backpacks also had the largest initial values so that is correlated as well). This is the reason we omit the goods dummies in our main regressions reported in [Table 1](#).

To conclude, we find mixed evidence for Hypothesis 1: the evidence seems stronger for *RSD* than for *Boston*, which may indicate that the choice of mechanism may influence preference formation in other more complex ways as well. This should be further explored in the future with alternative experimental designs. Part of the ambiguity may be due to the use of real goods, which may induce more noise in the data, making strong statistical inference more difficult. This is a necessary trade-off to answer our question of interest, because real goods allow us to directly identify rankings-dependence in ways that induced values would not.

Hypotheses 2 and 3: Truth-telling

We now move to assessing truth-telling (with respect to fundamental value) and Hypotheses 2 and 3. To assess Hypothesis 2, we begin with reporting the proportion of participants whose submitted rankings list in Phase II is truthful with respect to the fundamental values elicited in Phase I.²²

When testing Hypothesis 2, we classify an agent as truth-telling only if all five goods were ranked in the same order as the fundamental value elicited in Phase I. However, this could be an overly restrictive definition of truth-telling, because the goods were intentionally chosen such that two of them (the backpack and water bottle) were likely to be much better than the other three to most people. To the extent that the remaining three goods (notebook, mug, pen) were viewed as significantly worse than the top two, and similar in value amongst themselves, participants may have been more focused on the choice between the top two goods and submitted “noisy” preferences over the remaining three goods. There also is likely to be noise in the Phase I elicitation procedure itself, and since the notebook, mug, and pen were similar (and lower) in value, this could be a cause of apparently non-truthful reporting over all 5 goods in Phase II.²³

We deal with this issue in two ways. First, we also assess Hypothesis 3, which looks at the number of agents who report their top good truthfully. Second, we consider submitted rankings lists in Phase II that are truthful with respect to the fundamental values elicited in Phase I with some noise. In particular, we chose an ad-hoc cutoff of \$2 and classify preferences as truthful as long as they do not rank a good in Phase II higher than another good with an elicited value that is \$2 more. All the proportions are presented in [Table 2](#).

Of course, as the leniency allowed in truthful reporting increases, the proportions of truthful reporting increases. But the main result, regardless of how truth-telling is measured, is that there are no differences in truthful reporting between the two treatments. The non-parametric Wilcoxon ranksum test confirms this impression for all 6 pairwise comparisons between the treatments. We find no support that Hypotheses 2 and 3 hold strictly. We also find it interesting that even in the most lenient case, about 20% of participants are putting a good at the top of their submitted list that they valued more than \$2 less than their top-value good.

To provide a clearer picture of what impacts truth-telling, we ran probit regressions with a dependent variable equal to 1 for participants who chose the truth and the independent variables risk aversion, loss aversion, CRT score, female, and practice (all measured the same as in the net value regressions). We report the results for the case where truth-telling is measured as the top 2 choices in the main text ([Table 3](#)), because we think that misstating the bottom 3 goods is mostly noise and it is exactly these two goods for which there is an incentive to misrepresent one’s preferences in the Boston mechanism. The other two measures are presented in [Tables B.6](#) and [B.7](#) in the Appendix where we find estimate values are quite similar although the levels of significance vary somewhat.

The regressions indicate that while truth-telling rates are similar across the two treatments, this may be due to different reasons. The estimate for CRT score is positive and marginally significant in *RSD* (with the exact measure, regression (1)) indicating that deeper-thinking participants tell the truth more often. So perhaps the non truth-telling in *RSD* is due to mistakes made by participants with low CRT scores rather than driven by rankings-dependent utility. The estimate for risk aversion is negative and marginally significant in *Boston* (with the exact measure, regression (3)) indicating that risk averse participants are less likely to tell the truth, which is line with a model of standard preferences. Finally, the estimate for female is positive and significant in *Boston* which is consistent with the finding that females are more averse to lying ([Dreber and Johannesson, 2008](#); [Erat and Gneezy, 2012](#)).

²² The preferences elicited in Phase I are not always strict, in which case listing the indifferent goods in any order is classified as truthful.

²³ Indeed, the interpretation of the initial value coefficient in [Table 1](#) suggests that this is the case.

Table 3
Truth-telling Top 2 Choices Regressions.

Treatment	<i>RSD</i>		<i>Boston</i>		All	
	(1)	(2)	(3)	(4)	(5)	(6)
risk aversion	-0.0160	-0.0117	-0.0255*	-0.0196	-0.0183*	-0.0147
loss aversion	0.0051	-0.0053	-0.0060	-0.0013	-0.0004	-0.0029
CRT score	0.2477*	0.2093	0.1546	0.0880	0.2011**	0.1544
female	0.5353	0.3808	0.5626**	0.4994*	0.5189**	0.4426**
practice	-0.0135	-0.0166	0.0369	0.0255	0.0094	0.0034
_cons	-0.1425	0.5057	0.3021	0.4284	0.0348	0.4137
Measure	Exact	\$2 diff.	Exact	\$2 diff.	Exact	\$2 diff.
No. of Obs.	100	100	100	100	200	200

Table 4
Welfare.

<i>RSD</i>	<i>Boston</i>	All
90.29	89.97	90.13

Hypothesis 4: Welfare

Finally, we address Hypothesis 4 and welfare. Table 4 presents the results for welfare measured as the sum of participants elicited Phase II values for the goods they received in the matching mechanism.

As is clear in the table, there is no difference between the two treatments. This is confirmed with the Wilcoxon rank sum non-parametric test.²⁴

5. Conclusion

We investigate whether agent preferences over goods received via a matching market are influenced by how highly they ranked the object in their reported preference list. We design a laboratory experiment to test whether agents value a good more the higher that they rank it. A novel feature of our experiment is that we use real goods, rather than induced values. This more faithfully simulates real-world settings in which participants must form their own values over their potential outcomes, and allows us to directly measure for rankings-dependent utility by eliciting values for objects both inside and outside of the mechanism and taking the difference.

We find mixed evidence to support the hypothesis that valuations are influenced by reported rankings, with a clearer effect seen for RSD than for Boston. We also find much lower rates of truth-telling in RSD than previous matching experiments that use induced values designs, which is consistent with preferences being rankings-dependent. While we do not claim a “slam-dunk” for the hypothesis of rankings dependence, given their potential implications, we think the results of our experiment warrant further study of the issue of preference formation and its effect on reporting behavior in matching mechanisms, which has been largely ignored in the (vast) matching literature thus far. There could be other reasons for deviations from “truthful reporting” (according to fundamental values) that may not be captured by our experiment, such as a “joy of winning” effect (Cooper and Fang, 2008). Alternatively, rankings dependence may be stronger if, rather than being distributed randomly, priorities were based on a real-effort task, as then receiving a high-ranked object suggests the agent scored highly on this task and is thus of high ability.

We also hope that our paper inspires the use of more real-goods experimental designs as opposed to induced values. While induced values have the advantage of giving the experimenter more control, by construction, they are unable to say anything about preference formation. Using real goods will induce new challenges in experimental design, but with a significant benefit of more closely replicating and understanding the challenges faced by people in real-world matching markets.

6. Data availability

The data used in this article can be found at <https://doi.org/10.17632/nx38rwmsgv.1>.

Declaration of competing interest

The authors declare that they have no relevant or material financial interests that relate to the research described in this paper.

²⁴ As discussed in footnote 11, it may be argued that rankings-dependent utility is a “bias” that should not be included in the final welfare comparisons. In this case, a better measure of welfare would be the sum of the Phase I valuations. Under this measure, the average welfare is 82.25 for RSD and 85.75 for Boston. So, there is slightly higher welfare for Boston, but the difference is not significant.

Acknowledgements

A portion of this work was carried out while Kloosterman was a faculty member at the University of Virginia. For their comments, we would like to thank Vincent Meissner, Assaf Romm, Yingzhi Liang, Alex Rees-Jones, and audiences at the 12th Conference on Economic Design in Padova and the 2023 WZB Berlin Matching Market Design Conference. We gratefully acknowledge support from the UVA Buckner W. Clay Dean of Arts and Sciences, Vice President for Research, Quantitative Collaborative, and the Bankard Fund for Political Economy. This project was approved by the Institutional Review Board of the University of Virginia (Protocol Number 5421).

Appendix A. Proofs of Theorems

Proof of Theorem 1. By supposition of the theorem, assume that truth-telling is an equilibrium of the Boston mechanism. We will show that truth-telling is also an equilibrium of RSD by showing that if all $j \neq i$ report truthfully, then i 's best response is also to report truthfully.

Let all agents other than $j \neq i$ report a preference ranking that respects the fundamental values, $P_j : x_1, \dots, x_n$. Fixing these reports for $j \neq i$, let i 's expected utility from any report P'_i be $EU^{SD}(P'_i)$. If i also reports a ranking that respects the fundamental values, $P_i : x_1, \dots, x_n$, this becomes

$$EU_i^{SD}(P_i) = \frac{1}{n} \sum_{j=1}^n u_i(x_j, P_i) = \frac{1}{n} \sum_{j=1}^n (v_j + \rho(j)). \quad (\text{A.1})$$

Consider any alternative report for i , $P'_i \neq P_i$. Let k' be the lowest index such that object $x_{k'}$ is not ranked in the k' -th position, and let x_{k^*} be the object that is ranked k' -th, where by definition, $k^* > k'$. Notice that such a k^* exists, as otherwise $P'_i = P_i$. Under P'_i , agent i will never receive any object $x_{k'}, \dots, x_{k^*-1}$. To see this, let ℓ be i 's (randomly drawn) order in the serial dictatorship. If $\ell < k'$, then i will receive object x_ℓ ; if $k' \leq \ell \leq k^*$, object x_{k^*} is still available when it is i 's turn to choose, and thus i receives object k^* ; if $\ell > k^*$, then at i 's turn, all objects x_1, \dots, x_{k^*} have been taken by earlier agents, and thus i receives some object from the set x_{k^*+1}, \dots, x_n . This implies that i 's utility from reporting P'_i is bounded above by

$$EU^{SD}(P'_i) \leq \frac{1}{n} \sum_{j=1}^{k'-1} (v_j + \rho(j)) + \frac{n - k' + 1}{n} (v_{k^*} + \rho(k')) \quad (\text{A.2})$$

Now, consider the Boston mechanism, and the same report P'_i from above (continuing to assume that all other agents report truthfully in Boston). If i reports P'_i in the Boston mechanism, then her expected utility is

$$EU^B(P'_i) = \frac{1}{n} \sum_{j=1}^{k'-1} (v_j + \rho(j)) + \frac{n - k' + 1}{n} (v_{k^*} + \rho(k')) \quad (\text{A.3})$$

This is because, in Boston, if i is not assigned in one of the first $k' - 1$ rounds, she is assigned to x_{k^*} in round k' with certainty. In this case, she has ranked this object k' -th, so her total utility is the term in parentheses at the end of Eq. (A.3). This event occurs with probability $(n - k' + 1)/n$.

Notice that if i reports truthfully with respect to fundamental values in Boston, her payoff is

$$EU_i^B(P_i) = \frac{1}{n} \sum_{j=1}^n (v_j + \rho(j)), \quad (\text{A.4})$$

which is equivalent to the payoff from reporting P_i in SD; see Eq. (A.1). Finally, we have the following:

$$EU^{SD}(P'_i) \leq EU^B(P'_i) \leq EU^B(P_i) = EU^{SD}(P_i) \quad (\text{A.5})$$

where the first inequality follows from Eqs. (A.2) and (A.3), the second from the fact that truth-telling is assumed to be an equilibrium of Boston, and the last equality from Eqs. (A.1) and (A.4). Eq. (A.5) implies $EU^{SD}(P'_i) \leq EU^{SD}(P_i)$, i.e., truth-telling is optimal in SD. \square

Proof of Theorem 2. First, consider Boston, and let n_1^B be the number of agents who rank x_1 first. It is trivial to see that $n_1^B = 0$ is never an equilibrium, because if no agent is ranking x_1 first, then any agent can deviate to a strategy that does so and receive payoff $v_1 + \rho(1)$ for sure, which is the highest possible attainable payoff.

Next, consider the case that $n_1^B = n$ is an equilibrium of the Boston mechanism, i.e., all agents rank x_1 first. In this case, each agent will also rank x_2 second (see footnote 16), and so receives x_1 with probability $1/n$ and x_2 with probability $1/n$, for an equilibrium utility of $(1/n) \times (v_1 + \rho(1)) + 1/n \times (v_2 + \rho(2)) + ((n - 2)/n) \times (\bar{v} + \bar{\rho})$, where $\bar{\rho} = \frac{1}{n-2} \sum_{j=3}^n \rho(j)$. The last term, $\bar{v} + \bar{\rho}$, comes from the fact that for all agents not assigned in rounds 1 or 2, the assignment is just a random assignment of goods x_3, \dots, x_n , and so by symmetry, any individual agent's payoff is just the average, $\bar{v} + \bar{\rho}$; this event happens with probability $(n - 2)/n$. If any individual agent deviates to ranking x_2 first, she gets it for sure, with resulting utility $v_2 + \rho(1)$. Since this is an equilibrium, we conclude that $(1/n) \times (v_1 + \rho(1)) + 1/n \times (v_2 + \rho(2)) + ((n - 2)/n) \times (\bar{v} + \bar{\rho}) \geq v_2 + \rho(1)$. Note also if $n_1^B = n$ is an equilibrium, then it is the unique equilibrium. This follows because if there were some other equilibrium in which $n_1^B < n$, then some agent i is not ranking x_1 first. Agent i will therefore never receive x_1 , and so her payoff is bounded above by $v_2 + \rho(1)$. If she deviates to ranking x_1 first and x_2

second, then her payoff is bounded below by $(1/n) \times (v_1 + \rho(1)) + 1/n \times (v_2 + \rho(2)) + ((n-2)/n) \times (\bar{v} + \bar{\rho})$. As was just shown, the latter is greater than the former, and so this deviation is profitable.

Now, consider SD. If all n agents rank x_1 first and x_2 second, then they once again receive payoff $(1/n) \times (v_1 + \rho(1)) + 1/n \times (v_2 + \rho(2)) + ((n-2)/n) \times (\bar{v} + \bar{\rho})$. If any agent deviates to ranking anything other than x_1 first,²⁵ they will never receive x_1 , and so their payoff is bounded above by $v_2 + \rho(1)$, which, from the above calculation for Boston, is smaller than the expected payoff from ranking x_1 first and x_2 second. Thus, $n_1^{SD} = n$ is also an equilibrium of SD. Similar arguments as for the Boston case above show that this equilibrium is once again unique, and so $n_1^{SD} \geq n_1^B$, as required.

So, for the remainder of the proof, we restrict to $0 < n_1 < n$. Let $U^B(x_j, n_1)$ be the expected utility of an agent who ranks good x_j first when n_1 total agents (including this agent) rank x_1 first, and the remaining $n - n_1$ total agents rank x_2 first. Then, we can calculate:

$$\begin{aligned} U^B(x_1, n_1) &= \frac{1}{n_1}(v_1 + \rho(1)) + \frac{n_1-1}{n_1}\delta \\ U^B(x_2, n_1) &= \frac{1}{n-n_1}(v_2 + \rho(1)) + \frac{n-n_1-1}{n-n_1}\delta \end{aligned}$$

where $\delta = \bar{v} + (\rho(2) + \rho(3) + \dots + \rho(n-1))/(n-2)$.²⁶ The equations derive from the fact that if an agent ranks x_1 first in Boston, she has a $1/n_1$ chance of getting x_1 . If she does not, good x_2 will not be available in round 2, and so she will get some good x_3, \dots, x_n . There will always be exactly $n-2$ agents left after round 1 of the mechanism, and since all agents rank goods x_3, \dots, x_n the same, this becomes a random allocation of the remaining objects. The δ terms represent the total expected utility from this random allocation. Let

$$\Delta_{x_1 \rightarrow x_2}^B(n_1) = U^B(x_1, n_1) - U^B(x_2, n_1 - 1)$$

be the change in utility for an agent whose equilibrium strategy is to rank x_1 first, and who deviates to ranking x_2 first. Similarly, let

$$\Delta_{x_2 \rightarrow x_1}^B(n_1) = U^B(x_2, n_1) - U^B(x_1, n_1 + 1)$$

be the expected change for an agent whose equilibrium strategy is to rank x_2 first, but who deviates to ranking x_1 first.

For n_1^B to be an equilibrium, we need both $\Delta_{x_1 \rightarrow x_2}^B(n_1^B) \geq 0$ and $\Delta_{x_2 \rightarrow x_1}^B(n_1^B) \geq 0$, i.e., no agent has a profitable deviation. Algebra shows that these two equations reduce to

$$n_1^B \in \left[\frac{n(v_1 + \rho(1) - \delta)}{v_1 + v_2 + 2(\rho(1) - \delta)} - \frac{v_2 + \rho(1) - \delta}{v_1 + v_2 + 2(\rho(1) - \delta)}, \frac{n(v_1 + \rho(1) - \delta)}{v_1 + v_2 + 2(\rho(1) - \delta)} + \frac{v_1 + \rho(1) - \delta}{v_1 + v_2 + 2(\rho(1) - \delta)} \right]. \quad (\text{A.6})$$

Subtracting the left endpoint from the right endpoint gives a range of length exactly 1. So, there will be a unique integer n_1^B in this range, which corresponds to the unique equilibrium number of agents who rank x_1 first in Boston.

Let n_1^{SD} be the equilibrium number of agents who rank x_1 first in SD. We will show that $n_1^{SD} \geq n_1^B$. For SD, the analogous equations to the above are:

$$\begin{aligned} U^{SD}(x_1, n_1) &= \frac{1}{n}(v_1 + \rho(1)) + \frac{1}{n} \left(\frac{n_1-1}{n-1}(v_2 + \rho(2)) + \frac{n-n_1}{n-1}(v_1 + \rho(1)) \right) + \frac{n-2}{n}\delta' \\ U^{SD}(x_2, n_1) &= \frac{1}{n}(v_2 + \rho(1)) + \frac{1}{n} \left(\frac{n_1}{n-1}(v_2 + \rho(1)) + \frac{n-n_1-1}{n-1}(v_1 + \rho(2)) \right) + \frac{n-2}{n}\delta' \end{aligned}$$

In each equation above, there is a $1/n$ chance that the agent is ordered first, in which case she gets her first ranked good. If not, there is a $1/n$ chance she is ordered second. In the top equation, there is an $(n_1-1)/(n-1)$ chance the first agent was one of the remaining agents who ranked x_1 first, in which case agent i receives x_2 , and a $n_2/(n-1)$ chance the first agent was one of the agents who ranked x_2 first, in which case i gets x_1 again. If i is ordered third or higher in the SD ordering, then both goods x_1 and x_2 are gone at her turn, and, as for the Boston case above, i 's utility in this case is that from a random assignment of the remaining goods, represented by $\delta' = \bar{v} + \bar{\rho}$.²⁷

Define $\Delta_{x_1 \rightarrow x_2}^{SD}(n_1)$ and $\Delta_{x_2 \rightarrow x_1}^{SD}(n_1)$ analogously to the above, except replacing SD for Boston. Similarly as for Boston, for n_1^{SD} to be an equilibrium, we need $\Delta_{x_1 \rightarrow x_2}^{SD}(n_1^{SD}), \Delta_{x_2 \rightarrow x_1}^{SD}(n_1^{SD}) \geq 0$. Algebra shows that these equations reduce to

$$n_1^{SD} \in \left[\alpha + \frac{1}{2}, \alpha - \frac{1}{2} \right] \quad (\text{A.7})$$

where $\alpha = \frac{n}{2} + \frac{(n-1)}{2} \frac{v_1 - v_2}{\rho(1) - \rho(2)}$. Once again, this range has total length 1, and so there is a unique equilibrium number of agents n_1^{SD} that rank x_1 first.

²⁵ This follows because if the agent is chosen first in the RSD ordering, she receives her first-ranked object which is different from x_1 , while if she is not chosen first, then whoever is chosen first takes x_1 .

²⁶ This is slightly different than $\bar{v} + \bar{\rho}$ calculated above for the $n_1 = n$ case, because here, agents begin ranking goods x_3, \dots, x_n second in their list, and so the summation of the rankings-dependent utility terms run from $j = 2$ to $n-1$; they will never receive their n^{th} ranked object, which is the object among $\{x_1, x_2\}$ that they did not rank first.

²⁷ The term δ' here is slightly different from the δ term in the Boston equations above, because in RSD, agents rank x_1 and x_2 first and second, in some order, while in Boston, one of x_1 or x_2 is ranked last. However, for RSD, the δ' terms are less important, because they will cancel out when checking for profitable deviations below.

What remains to check is that $n_1^{SD} \geq n_1^B$. To show this, note first that the lower bounds in Eqs. (A.6) and (A.7) are determined by the equations $\Delta_{x_2 \rightarrow x_1}^B(n_1) \geq 0$ and $\Delta_{x_2 \rightarrow x_1}^{SD}(n_1) \geq 0$ that ensure that those who rank x_2 first do not want to deviate to ranking x_1 . Next, notice that

$$\frac{d\Delta_{x_2 \rightarrow x_1}^{SD}(n_1)}{dn_1} = \frac{2(\rho(1) - \rho(2))}{n(n-1)} > 0,$$

which implies that $\Delta_{x_2 \rightarrow x_1}^{SD}(n_1)$ is an increasing function. Now, evaluate $\Delta_{x_2 \rightarrow x_1}^{SD}(n_1)$ at $\xi = \frac{n(v_1 + \rho(1) - \delta)}{v_1 + v_2 + 2(\rho(1) - \delta)} - \frac{v_2 + \rho(1) - \delta}{v_1 + v_2 + 2(\rho(1) - \delta)}$, which is the lower bound of Eq. (A.6):

$$\Delta_{x_2 \rightarrow x_1}^{SD}(\xi) = -\frac{(v_1 - v_2)((n-3)\rho(1) + (n+1)\rho(2) + (n-1)(v_1 + v_2) - 2(n-1)\delta)}{n(n-1)(v_1 + v_2 + 2\rho(1) - 2\delta)}$$

Now, since $v < v_2$ and $\rho(j)$ is a decreasing function, we have

$$\delta = v + \frac{1}{n-2} \sum_{j=2}^{n-1} \rho(j) < v_2 + \rho(2)$$

It is then simple to check that both the numerator and denominator of the above equation are positive, which implies that $\Delta_{x_2 \rightarrow x_1}^{SD}(\xi) < 0$. Because the function is increasing, the crossover point that defines the lower bound of Eq. (A.7) must lie to the right of ξ . Thus, the range of Eq. (A.7) must be to the right of the range of Eq. (A.6) (they may overlap), which implies that $n_1^{SD} \geq n_1^B$. \square

Proof of Theorem 3. The overall welfare from any mechanism is just the sum total of the utilities. Recall that an agent's utility is the sum of the fundamental value of the object she receives, v_j , and a rankings-dependent component: $u_i(x, P_i) = v_i(x) + \rho(j)$. Because $v_i(x_j) = v_j$ for all i , the sum of the fundamental values will be the same for any assignment. This means that the overall welfare of any mechanism ψ is determined by the sum of the rankings-dependent utility components:

$$W^\psi = \sum_{j=1}^n (\# \text{ agents who receive their } j^{\text{th}} \text{ ranked good}) \times \rho(j)$$

There are two cases. First, if $n_1^B = n$ —that is, all agents choose to rank x_1 first in the Boston mechanism—then, by Theorem 2, $n_1^{SD} = n$ as well. The two mechanisms are then equivalent, and so $W^B = W^{SD}$.

Second, assume that $1 \leq n_1^B < n$ (recall that Theorem 2 shows that $n_1^B = 0$ is never an equilibrium of Boston). For Boston, we have

$$W^B = 2\rho(1) + \sum_{j=2}^{n-1} \rho(j).$$

The first term, $2\rho(1)$ comes from the fact that at least one agent is ranking x_1 first and at least one agent is ranking x_2 first, and so both of these agents are receiving their top-ranked object. Then, all remaining agents rank the remaining goods uniformly at random, and so, by symmetry, they are equally likely to get any of these goods. Notice that the summation starts at 2, because they place the good from $\{x_1, x_2\}$ that was not their top choice at the bottom of their rankings (and will never receive it).

For SD, the equation is

$$W^{SD} = \frac{n_1^{SD}}{n} \frac{n_1^{SD}-1}{n-1} (\rho(1) + \rho(2)) + 2 \times \frac{n_1^{SD}}{n} \frac{n-n_1^{SD}}{n-1} (\rho(1) + \rho(1)) \quad (\text{A.8})$$

$$+ \frac{n-n_1^{SD}}{n} \frac{n-n_1^{SD}-1}{n-1} (\rho(1) + \rho(2)) + \sum_{j=3}^n \rho(j) \quad (\text{A.9})$$

The first three terms derive from the probability that the first two agents are “ $x_1 P_i x_2$ ” agents or “ $x_2 P_i x_1$ ” agents. For instance, for the first term, there is an n_1^{SD}/n chance that the first agent in the order ranks $x_1 P_i x_2$ and, conditional on this, a $(n_1^{SD}-1)/(n-1)$ chance that the second agent has the same ranking. In this case, x_1 goes to an agent who ranked it first and x_2 goes to an agent who ranked it second, so the rankings-dependent component of welfare is $\rho(1) + \rho(2)$. The next two terms are calculated similarly, for all possible combinations of the preferences of the first two agents. Finally, the summation term at the end comes from the fact that all remaining agents rank x_3, \dots, x_n uniformly randomly, and so by symmetry, are equally likely to get any rank. Note that, unlike in Boston, the summation runs from $j = 3$ to n , because all agents rank x_1 and x_2 first in SD.

Now, notice that

$$\begin{aligned} W^{SD} &\leq \frac{n_1^{SD}}{n} \frac{n_1^{SD}-1}{n-1} (2\rho(1)) + 2 \times \frac{n_1^{SD}}{n} \frac{n-n_1^{SD}}{n-1} (2\rho(1)) + \frac{n-n_1^{SD}}{n} \frac{n-n_1^{SD}-1}{n-1} (2\rho(1)) + \sum_{j=3}^n \rho(j) \\ &= 2\rho(1) \left(\frac{n_1^{SD}}{n} \frac{n_1^{SD}-1}{n-1} + 2 \times \frac{n_1^{SD}}{n} \frac{n-n_1^{SD}}{n-1} + \frac{n-n_1^{SD}}{n} \frac{n-n_1^{SD}-1}{n-1} \right) + \sum_{j=3}^n \rho(j) \\ &= 2\rho(1) + \sum_{j=3}^n \rho(j) \\ &\leq W^B \end{aligned}$$

where the first line replaces $\rho(2)$ with $\rho(1) \geq \rho(2)$, the second factors out the $2\rho(1)$ terms, the third follows because the term in parentheses sums to 1, and the last follows from $\rho(j)$ being a decreasing function. \square

Appendix B. Robustness Regressions

Table B.5
Net Value Regressions with Dummies.

Treatment	RSD	Boston	All
	(1)	(2)	(3)
ranked 2nd	1.0655	-0.6298	1.2793
ranked 3rd	0.3480	-1.8964	-0.4721
ranked 4th	0.3046	-0.6471	0.4093
ranked 5th	-1.0711	-4.5459	-3.0656
backpack	10.3001***	5.4340	8.1776***
notebook	2.6014	-2.4099	-0.0261
waterbottle	7.5522***	4.2488	6.3798***
pens	1.7752	-4.0435*	-1.2110
initial value	0.2130***	-0.4093***	0.3186***
risk aversion	0.0654	0.1927**	0.1436***
_cons	-1.6011	2.3024	-0.3960
No. of Obs.	100	100	200
R-Squared	0.25	0.31	0.23

Table B.6
Truth-telling All Choices Regressions.

Treatment	RSD		Boston		All	
	(1)	(2)	(3)	(4)	(5)	(6)
risk aversion	-0.0224	-0.0142	-0.0211	-0.0270*	-0.0165	-0.0177*
loss aversion	0.0000	-0.0057	-0.0224	-0.0042	-0.0110	-0.0046
CRT score	0.1794	0.1956	0.2118	0.2475*	0.1863*	0.2250**
female	0.5718*	0.2012	0.4025	0.4182	0.4065*	0.3214
practice	-0.0701**	-0.0507*	0.0481*	0.0282	-0.0024	-0.0105
_cons	0.1589	0.6542	-0.0824	0.0730	-0.0909	0.2440
Measure	Exact	\$2 diff.	Exact	\$2 diff.	Exact	\$2 diff.
No. of Obs.	100	100	100	100	200	200

Table B.7
Truth-telling Top Choice Regressions.

Treatment	RSD		Boston		All	
	(1)	(2)	(3)	(4)	(5)	(6)
risk aversion	-0.0054	-0.0087	-0.0310*	-0.0225	-0.0150	-0.0138
loss aversion	0.0076	0.0034	-0.0060	-0.0047	0.0004	-0.0008
CRT score	0.2041	0.1716	0.0538	0.0787	0.1151	0.1197
female	0.4395	0.3006	-0.0074	0.0962	0.1824	0.1757
practice	-0.0201	-0.0226	-0.0021	-0.0043	-0.0154	-0.0157
_cons	0.1928	0.7298	1.8629**	1.6134**	1.0138**	1.1603**
Measure	Exact	\$2 diff.	Exact	\$2 diff.	Exact	\$2 diff.
No. of Obs.	100	100	100	100	200	200

Appendix C. Phase I goods

Table C.8
Phase I goods.

Good	Description	Amazon Price	Avg. Elic. Val.
Backpack	Fjallraven gray, 16L backpack	\$66.95	\$28.24
Alarm clock	Aisuo bluetooth alarm/speaker/nightlight	\$37.99	\$19.46
Water bottle	Hydroflask blue, 32 oz. water bottle	\$32.96	\$22.56
Shower speaker	Donerton bluetooth waterproof speaker	\$29.99	\$18.75
Laptop stand	Ergonomic universal laptop stand	\$26.99	\$15.99
Outdoor blanket	Bearz blue waterproof blanket	\$24.99	\$14.04
Cold brewer	Takeya 1 qt. carafe	\$21.00	\$15.04
Picture frames	Set of 4 white, wooden frames	\$20.99	\$9.99
Popcorn set	3 bags of popcorn plus flavors	\$22.00	\$8.72
Notebook	Moleskine 192 age, black notebook	\$21.90	\$9.11
Tile	Bluetooth chip to track item on phone	\$19.99	\$14.98
Phone mount	Gosenech phone holder with bracket	\$19.79	\$8.87
Popcorn popper	Silicone microwaveable popcorn maker	\$14.99	\$7.49
Charging pad	Anker phone-charging pad	\$13.99	\$14.57
Frisbee	Discraft yellow 175 g. Frisbee	\$13.76	\$6.60
Mug	Blue ceramic mug	\$11.99	\$6.53
Playing cards	Deck of black playing cards	\$7.99	\$4.93
Pens	Set of 4 Uni-ball rollerball pens	\$6.88	\$5.33
Keychain tool	Multi-tool that attaches to keys	\$6.64	\$5.11
Cable spirals	Set of 24 plastic, multicolor cable-protectors	\$6.29	\$5.03

Appendix D. Instructions and Screen Shots

Welcome and Phase I

Welcome. This is an experiment in decision making. Various research foundations and institutions have provided funding for this experiment and you will have the opportunity to make a considerable amount of money which will be paid to you at the end. Make sure you pay close attention to the instructions because the choices you make will influence the amount of money you will take home with you today. Please ask questions if any instructions are unclear.

Terminology: In the following instructions, we will say that the computer will make a **random** choice from a number of possibilities. This means that the computer will randomly select one of the possibilities with equal chance for each. If there are N possibilities, you can think of this as the computer rolling a die that has N sides, and choosing the possibility that comes up on the die.

This experiment will consist of two phases. We will hand out the instructions for each phase before you complete the phase. You will be paid for your choices in only one of the two phases which will be randomly selected by the computer. Your choices in each phase have no impact on later phases.

Your earnings may be an object or money, and at the end of the experiment, we will give you the object or the money to take home. You will see pictures of the object on your screen, but we have the actual objects here with us. If you want us to show you the actual object at any time, just raise your hand and we can bring it over. Everyone will also get \$6 for participating.

Phase I

For this phase, you will be allocated an object that will be shown to you on your screen. Remember that we have the actual objects with us, so feel free to raise your hand if you would like us to bring one over for closer inspection.

The Task

After you are allocated your object, you will have the opportunity to give up the object in exchange for a certain amount of money. Whether you keep the object or receive money will be determined as follows:

First, you will see a screen with 50 rows. Each row is a choice between keeping the object or exchanging it for \$1, \$2, ..., up to \$50. As we will explain carefully in the earnings section of the instructions below, if a row is selected for your earnings, you will take home the choice (object or money) that you selected in that row. Because it is time-consuming to have you click a button for every row, instead you only need to **click on the bullet for the object in the last row where you would keep the object over the specified number of dollars**. Because all rows above the one you select offer less money, we will fill in all of these rows with you selecting to keep the object. Similarly, because all rows below offer more money, we will fill in all of these rows with you selecting the money. After you click, you can change your mind by clicking on a different row and that row will become the last row where you keep the object. Click confirm when you have finalized your choice.

Next, you will see another screen with 50 rows. Each row is a choice between keeping the object or exchanging it for \$x.02, \$x.04, up to \$x + 1 where x is the dollar amount in the row you selected on the first screen. As for the first screen, you make just one choice. **Click on the bullet for the object in the last row where you would keep the object over the specified number of dollars**. As on the first screen, because all rows above have less money, we will automatically fill in all of these rows with you selecting the object over the money. Because all rows below contain more money, we will automatically fill in all these rows with you selecting

the money over the object. After you click, you can change your mind by clicking on a different row and that row will become the last row where you keep the object. Click confirm when you have finalized your choice.

Procedures

You will do this task for **20 objects**. After you complete the task for the 20 objects, we will ask you to complete 2 additional tasks unrelated to this task for a total of 22 tasks in Phase I. The instructions for those tasks will be presented on the screen when you do them. Finally, we will have you answer a short questionnaire.

Earnings

If this phase is selected for earnings, the computer will first randomly select 1 of the 22 tasks and you will receive earnings for that task. If the selection is one of the 20 tasks described above, your earnings will be determined as follows. The computer will randomly select one of the 50 rows from the first screen. **We will then implement your choice from that row. That is, you will get the object to take home with you if you selected to keep the object in this row, or the amount of money if you selected the money.**

There is one exception: If the randomly selected row from the first screen is the last row where you would keep the object, the computer will randomly select one of the 50 rows from the second screen and **we will then implement your choice from that row**. Keep in mind that any row from any task could be selected so it is in your best interest to select the object when you prefer the object to the money and to select the money when you prefer the money to the object.

For the 2 additional tasks, how your earnings would be determined if the computer selected one of them will be explained to you when you complete those tasks .

Phase II: RSD

Phase II

For this phase, you will be divided into groups of 5 people, and for each group there will be 5 objects. Each member of the group will end up with exactly one of the objects. The object you get will be determined by your preferences for the objects, along with the preferences of your group members. Each group member will provide a **preference ranking** of the objects. Because there is only one of each object available, if multiple people list a particular object first, we will use a random tie-breaking procedure to determine who gets what. The details of how this is done are described below.

The Task

You will first be shown the 5 objects. Your task for this phase is to **submit a list ranking the objects from most-preferred to least-preferred**. After collecting everyone's rankings, the computer will randomly select a picking ordering of the 5 group members. The computer will then allocate an object to each group member as follows.

- The group member who is randomly chosen to go first will be allocated the first object on their submitted list.
- The group member who is randomly chosen to go second will be allocated the highest-ranked object on their list that was not taken by the first group member.
- The group member who is randomly chosen to go third will be allocated the highest-ranked object on their list that was not taken by the first or second group member.
- The group member who is randomly chosen to go fourth will be allocated the highest-ranked object on their list that was not taken by the first, second, or third group member.
- The group member who is randomly chosen to go fifth will be allocated the highest-ranked object on their list that was not taken by the first, second, third, or fourth group member.

It is up to you how to rank the objects. Given the rules of the procedure, there is no way to manipulate your rankings to obtain an object that is ranked higher than the one you would have ended up with if you had just ranked the objects in the order you prefer them. This is because your ranking list does not affect what objects will be available when it is your turn in the picking order to receive an object, and when your turn arrives, the computer will give you the object you have ranked highest among those that are still available.

Example:

Let's go through an example. For our example we will use four objects that are not used in the experiment and a group of four people to make it simpler to understand and ensure we are making no suggestions about how you should rank the objects in the experiment. The example is purely to help you understand the procedure the computer will use.

We will use the names Ann, Bob, Carol, and Dave. The items are a Pizza, Chips, Soda, and Pretzels. Suppose our group members submit the following rankings:

	Ann	Bob	Carol	Dave
1st choice	Chips	Pizza	Pizza	Pretzels
2nd choice	Pizza	Chips	Pretzels	Soda
3rd choice	Soda	Soda	Chips	Chips
4th choice	Pretzels	Pretzels	Soda	Pizza

After submitting their rankings, the computer randomly determines a picking ordering of the group members. Say that the computer randomly orders Ann, Bob, Carol, and Dave as follows:

Object Allocation 1: First Choice

You have been allocated a **Discraft frisbee**.

For each of the 50 rows on the right you will either choose to "keep the object" or "exchange the object for an amount of dollars" where the amount of dollars is specified in each row.

Click anywhere in the box "I will keep the object" in the **last row where you want to keep the object**. We will fill in that you will keep the object for all rows above and exchange for the money for all rows below.

After you make a click, you will see your selections for all the rows in turquoise. If you wish, you can then change your choice by clicking in the box for "I will keep the object" in a different row. Once you have finalized your choice, click the **confirm** button.

If this task is selected for payment, the computer will randomly select one of the 50 rows and implement your choice from the row. You will receive the object if "keep the object" is selected or receive the specified dollar amount if "exchange the object" is selected. There is one exception: if the last row where you would keep the object is the randomly selected row then your earnings will be determined by your choice on the next screen.

1.	I will keep the object	I will exchange the object for \$1
2.	I will keep the object	I will exchange the object for \$2
3.	I will keep the object	I will exchange the object for \$3
4.	I will keep the object	I will exchange the object for \$4
5.	I will keep the object	I will exchange the object for \$5
6.	I will keep the object	I will exchange the object for \$6
7.	I will keep the object	I will exchange the object for \$7
8.	I will keep the object	I will exchange the object for \$8
9.	I will keep the object	I will exchange the object for \$9
10.	I will keep the object	I will exchange the object for \$10
11.	I will keep the object	I will exchange the object for \$11
12.	I will keep the object	I will exchange the object for \$12
13.	I will keep the object	I will exchange the object for \$13
14.	I will keep the object	I will exchange the object for \$14
15.	I will keep the object	I will exchange the object for \$15
16.	I will keep the object	I will exchange the object for \$16
17.	I will keep the object	I will exchange the object for \$17
18.	I will keep the object	I will exchange the object for \$18
19.	I will keep the object	I will exchange the object for \$19
20.	I will keep the object	I will exchange the object for \$20
21.	I will keep the object	I will exchange the object for \$21
22.	I will keep the object	I will exchange the object for \$22
23.	I will keep the object	I will exchange the object for \$23
24.	I will keep the object	I will exchange the object for \$24
25.	I will keep the object	I will exchange the object for \$25
26.	I will keep the object	I will exchange the object for \$26
27.	I will keep the object	I will exchange the object for \$27
28.	I will keep the object	I will exchange the object for \$28
29.	I will keep the object	I will exchange the object for \$29
30.	I will keep the object	I will exchange the object for \$30
31.	I will keep the object	I will exchange the object for \$31
32.	I will keep the object	I will exchange the object for \$32
33.	I will keep the object	I will exchange the object for \$33
34.	I will keep the object	I will exchange the object for \$34
35.	I will keep the object	I will exchange the object for \$35
36.	I will keep the object	I will exchange the object for \$36
37.	I will keep the object	I will exchange the object for \$37
38.	I will keep the object	I will exchange the object for \$38
39.	I will keep the object	I will exchange the object for \$39
40.	I will keep the object	I will exchange the object for \$40
41.	I will keep the object	I will exchange the object for \$41
42.	I will keep the object	I will exchange the object for \$42
43.	I will keep the object	I will exchange the object for \$43
44.	I will keep the object	I will exchange the object for \$44
45.	I will keep the object	I will exchange the object for \$45
46.	I will keep the object	I will exchange the object for \$46
47.	I will keep the object	I will exchange the object for \$47
48.	I will keep the object	I will exchange the object for \$48
49.	I will keep the object	I will exchange the object for \$49
50.	I will keep the object	I will exchange the object for \$50

Confirm



(a) First Screen

Object Allocation 1: Second Choice

You have been allocated a **Discraft frisbee**.

For each of the 50 rows on the right you will either choose to "keep the object" or "exchange the object for an amount of dollars and cents" where the amount of dollars and cents is specified in each row.

Click anywhere in the box "I will keep the object" in the **last row where you want to keep the object**. We will fill in that you will keep the object for all rows above and exchange for the money for all rows below.

After you make a click, you will see your selections for all the rows in turquoise. If you wish, you can then change your choice by clicking in the box for "I will keep the object" in a different row. Once you have finalized your choice, click the **confirm** button.

If this task is selected for payment and the computer randomly selected the last row where you keep the object on the previous screen, the computer will randomly select one of the 50 rows and implement your choice from the row. You will receive the object if "keep the object" is selected or receive the specified dollar amount if "exchange the object" is selected.

1.	I will keep the object	I will exchange the object for \$16.02
2.	I will keep the object	I will exchange the object for \$16.04
3.	I will keep the object	I will exchange the object for \$16.06
4.	I will keep the object	I will exchange the object for \$16.08
5.	I will keep the object	I will exchange the object for \$16.10
6.	I will keep the object	I will exchange the object for \$16.12
7.	I will keep the object	I will exchange the object for \$16.14
8.	I will keep the object	I will exchange the object for \$16.16
9.	I will keep the object	I will exchange the object for \$16.18
10.	I will keep the object	I will exchange the object for \$16.20
11.	I will keep the object	I will exchange the object for \$16.22
12.	I will keep the object	I will exchange the object for \$16.24
13.	I will keep the object	I will exchange the object for \$16.26
14.	I will keep the object	I will exchange the object for \$16.28
15.	I will keep the object	I will exchange the object for \$16.30
16.	I will keep the object	I will exchange the object for \$16.32
17.	I will keep the object	I will exchange the object for \$16.34
18.	I will keep the object	I will exchange the object for \$16.36
19.	I will keep the object	I will exchange the object for \$16.38
20.	I will keep the object	I will exchange the object for \$16.40
21.	I will keep the object	I will exchange the object for \$16.42
22.	I will keep the object	I will exchange the object for \$16.44
23.	I will keep the object	I will exchange the object for \$16.46
24.	I will keep the object	I will exchange the object for \$16.48
25.	I will keep the object	I will exchange the object for \$16.50
26.	I will keep the object	I will exchange the object for \$16.52
27.	I will keep the object	I will exchange the object for \$16.54
28.	I will keep the object	I will exchange the object for \$16.56
29.	I will keep the object	I will exchange the object for \$16.58
30.	I will keep the object	I will exchange the object for \$16.60
31.	I will keep the object	I will exchange the object for \$16.62
32.	I will keep the object	I will exchange the object for \$16.64
33.	I will keep the object	I will exchange the object for \$16.66
34.	I will keep the object	I will exchange the object for \$16.68
35.	I will keep the object	I will exchange the object for \$16.70
36.	I will keep the object	I will exchange the object for \$16.72
37.	I will keep the object	I will exchange the object for \$16.74
38.	I will keep the object	I will exchange the object for \$16.76
39.	I will keep the object	I will exchange the object for \$16.78
40.	I will keep the object	I will exchange the object for \$16.80
41.	I will keep the object	I will exchange the object for \$16.82
42.	I will keep the object	I will exchange the object for \$16.84
43.	I will keep the object	I will exchange the object for \$16.86
44.	I will keep the object	I will exchange the object for \$16.88
45.	I will keep the object	I will exchange the object for \$16.90
46.	I will keep the object	I will exchange the object for \$16.92
47.	I will keep the object	I will exchange the object for \$16.94
48.	I will keep the object	I will exchange the object for \$16.96
49.	I will keep the object	I will exchange the object for \$16.98
50.	I will keep the object	I will exchange the object for \$17.00

Confirm



(b) Second Screen

Fig. D.2. Value Elicitation Screen Shots.

1st	Bob
2nd	Carol
3rd	Dave
4th	Ann

The computer then determines the allocations using this ordering and the preferences of each group members using the following procedure

1. Bob was ordered first. His top-ranked object is the Pizza. Thus, he is given the Pizza. This indicated in **bold** in the table.

Bob	
1st choice	Pizza
2nd choice	Chips
3rd choice	Soda
4th choice	Pretzels

2. Carol was ordered next. The Pizza is gone, so the objects that remain are the Soda, the Chips, and the Pretzels. According to Carol's list, her top choice of these is the Pretzels, so Carol is given the Pretzels.

Carol	
1st choice	Pizza
2nd choice	Pretzels
3rd choice	Chips
4th choice	Soda

3. Dave was ordered third. The Pizza and the Pretzels are gone, so the objects that remain are the Soda and the Chips. According to Dave's list, his top choice between the Soda and the Chips is the Soda, so he is given the Soda.

Dave	
1st choice	Pretzels
2nd choice	Soda
3rd choice	Chips
4th choice	Pizza

4. Ann is ordered last. The Pizza, Pretzels, and Soda are gone, so Ann receives the Chips.

Ann	
1st choice	Chips
2nd choice	Pizza
3rd choice	Soda
4th choice	Pretzels

Procedures:

During the procedure, all you must do is submit **one list ranking all of the objects**. Note that you will not know your place in the picking order when you submit your rankings. The computer will take everyone's list and then determine the picking order. The picking order is entirely random, and is not influenced by the list you submit. As described above, when your turn comes, the computer will consider all of the objects that are still left, and give you the one that you ranked the highest.

After you are allocated your object, **we will ask whether you want to keep the object or exchange it for various amounts of money using the exact same procedures as in Phase I**.

Earnings:

If this phase is selected for payment the computer will randomly select rows just as in Phase I to determine if you will keep the object allocated to you by the procedure or the amount of money.

Practice:

We will now hand out a worksheet with another example of the procedure. Please work through the worksheet and raise your hand when you are finished. We will come over and check your work and help you if there are any mistakes.

Then, there will be an 8 minute practice period on the computer. The practice period will allow you to practice allocating 5 objects to 5 people. You can submit a ranking list and the computer has robots who submit the other 4 ranking lists. While you will not know the random ordering in the actual procedure when you submit your rank list, this practice will allow you to simulate what you would have gotten if you had submitted different lists. You can experiment with different rank lists as many times as you like for the randomly selected picking order. You can also generate a new random picking order, and experiment further.

For the practice round, we recommend that you first think about the order in which you would actually prefer the objects, and take note of the outcome you get for each possible list you try.

We are doing this practice because you will only submit your ranking list once for the actual experiment and we want to make sure everyone fully understands the procedure before we continue to the actual experiment. You do not earn anything for the practice.

Phase II: Boston

Phase II

For this phase, you will be divided into groups of 5 people, and for each group there will be 5 objects. Each member of the group will end up with exactly one of the objects. The object you get will be determined by your preferences for the object, along with the preference of your group members. Each group member will provide a **preference ranking** of the objects. Because there is only one of each object available, if multiple people list a particular object first, we will use a random tie-breaking procedure to determine who gets what. The details of how this is done are described below.

The Task

You will first be shown the 5 objects. Your task for this phase is to **submit a list ranking the objects from most-preferred to least-preferred**.

After collecting everyone's rankings, the computer will randomly assign each group member a number from 1 to 5 that will be used to break ties. Every group member will be assigned a different number. The computer will then allocate an object to each group member in rounds as follows.

- In the **first round**, the computer looks at everyone's **first choices**.
 - If only one person has an object as their first choice, that person is allocated the object.
 - If more than one person lists an object as their first choice, then the computer will give the object to the person who was assigned the **lowest number** 1–5.
- In the **second round**, only people who did not receive anything in the first round participate. In the second round, the computer looks at everyone's **second choices**.
 - If only one person has an object as their second choice, then that person is allocated the object.
 - If more than one person has an object as their second choice, then the computer will give the object to the person who was assigned the **lowest number** 1–5.

The procedure continues in rounds in the same manner, considering only agents who remain and their third choices in round 3, fourth choices in round 4, etc., until everyone is assigned an object.

It is up to you how to rank the objects. The random numbers 1–5 are not assigned until after everyone submits their rankings. They will only be used if the computer needs to break ties.

Example:

Let's go through an example. For our example we will use four objects that are not used in the experiment to make it simpler to understand and ensure we are making no suggestions about how you should rank the objects in the experiment. The example is purely to help you understand the procedure the computer will use.

We will use the names Ann, Bob, Carol, and Dave. The items are a Pizza, Chips, Soda, and Pretzels. Suppose our group members submit the following rankings:

	Ann	Bob	Carol	Dave
1st choice	Pizza	Pizza	Pizza	Pretzels
2nd choice	Pretzels	Chips	Chips	Soda
3rd choice	Chips	Soda	Pretzels	Chips
4th choice	Soda	Pretzels	Soda	Pizza

After collecting the rankings, the computer randomly assigns each group member a number, in this case 1 to 4 because there are 4 group members. Say that this random assignment resulted in the following:

Name	Random Number
Ann	2
Bob	1
Carol	3
Dave	4

Round 1

- The computer considers the first choice of every group member (indicated in gray in the table below).
- Only Dave has the pretzels as his first choice, so Dave gets the pretzels.
- Ann, Bob, and Carol have the pizza as their first choice.
- Bob has a lower random number (1) than Ann (2) or Carol (3). Bob gets the pizza.
- Ann and Carol get nothing in this round.
- The objects that are assigned are denoted in **bold** in the table below.

	Ann	Bob	Carol	Dave
1st choice	Pizza	Pizza	Pizza	Pretzels
2nd choice	Pretzels	Chips	Chips	Soda
3rd choice	Chips	Soda	Pretzels	Chips
4th choice	Soda	Pretzels	Soda	Pizza

Round 2

- Only Ann and Carol participate in Round 2. The remaining objects are the Chips and the Soda.
- The computer considers Ann and Carol's second choices.
- Carol's second choice is the chips. Carol receives the chips.
- Ann's second choice is the pretzels, but the pretzels were already taken by Dave in round 1.
- Ann does not receive anything in this round.

	Ann	Carol
1st choice	Pizza	Pizza
2nd choice	Pretzels	Chips
3rd choice	Chips	Pretzels
4th choice	Soda	Soda

Round 3

- Only Ann remains. The computer considers Ann's third choice.
- Ann's third choice is the chips, but the chips were already taken by Carol in Round 2.
- Ann receives nothing in this round.

	Ann
1st choice	Pizza
2nd choice	Pretzels
3rd choice	Chips
4th choice	Soda

Round 4

- Only Ann remains. The computer considers Ann's fourth choice.
- Ann's fourth choice is the soda.
- Ann receives the soda.

	Ann
1st choice	Pizza
2nd choice	Pretzels
3rd choice	Chips
4th choice	Soda

Procedures:

During the procedure, all you must do is submit **one list ranking all of the objects**. The computer will take everyone's list and determine assignments in each round based on the rank lists following the above procedure, breaking ties using the randomly assigned numbers.

After you are allocated your object, we will ask whether you want to keep the object or exchange it for various amounts of money using the exact same procedures as in Phase I.

Earnings:

If this phase is selected for payment the computer will randomly select rows just as in Phase I to determine if you will keep the object allocated to you by the procedure or the amount of money.

Practice:

We will now hand out a worksheet with another example of the procedure. Please work through the worksheet and raise your hand when you are finished. We will come over and check your work and help you if there are any mistakes.

Then, there will be an 8 minute practice period on the computer. The practice period will allow you to practice allocating 5 objects to 5 people. You can submit a ranking list and the computer has robots who submit the other 4 ranking lists. While you will not know the random ordering in the actual procedure when you submit your rank list, this practice will allow you to simulate what you would have gotten if you had submitted different lists. You can experiment with different rank lists as many times as you like for the randomly selected picking order. You can also generate a new random picking order, and experiment further.

For the practice round, we recommend that you first think about the order in which you would actually prefer the objects, and take note of the outcome you get for each possible list you try.

We are doing this practice because you will only submit your ranking list once for the actual experiment and we want to make sure everyone fully understands the procedure before we continue to the actual experiment. You do not earn anything for the practice.

Supplementary material

Supplementary material associated with this article can be found in the online version at [10.1016/j.geb.2025.10.008](https://doi.org/10.1016/j.geb.2025.10.008)

References

- Abdulkadiroğlu, A., Che, Y.-K., Yasuda, Y., 2015. Expanding “choice” in school choice. *Am. Econ. J.: Microecon.* 7 (1), 1–42.
- Abdulkadiroğlu, A., Che, Y.-K., Yasuda, Y., 2011. Resolving conflicting preferences in school choice: the “Boston” mechanism reconsidered. *Am. Econ. Rev.* 101, 399–410.
- Abdulkadiroğlu, A., Sönmez, T., 2003. School choice: a mechanism design approach. *Am. Econ. Rev.* 93 (3), 729–747.
- Andersen, S., Harrison, G.W., Lau, M.I., Rutström, E.E., 2006. Elicitation using multiple price list formats. *Exper. Econ.* 9, 383–405.
- Ashlagi, I., Gonczarowski, Y.A., 2018. Stable matching mechanisms are not obviously strategy-proof. *J. Econ. Theory* 177, 405–425.
- Bade, S., Gonczarowski, Y., 2017. Gibbard-satterthwaite success stories and obvious strategyproofness.
- Bó, I., Hakimov, R., 2023. Pick-an-object mechanisms. *Manage. Sci.* 70 (7), 4693–4721.
- Chen, L., Pereyra, J.S., 2019. Self-selection in school choice. *Game. Econ. Behav.* 117, 59–81.
- Chen, R., Katuščák, P., Kittsteiner, T., Küller, K., 2024. Does disappointment aversion explain non-truthful reporting in strategy-proof mechanisms? *Exper. Econ.* 27 (5), 1184–1210. <https://doi.org/10.1007/s10683-024-09847-9>
- Chen, Y., Sönmez, T., 2006. School choice: an experimental study. *J. Econ. Theory* 127 (1), 202–231.
- Cooper, D.J., Fang, H., 2008. Understanding overbidding in second price auctions: an experimental study. *Econ. J.* 118 (532), 1572–1595.
- Dreber, A., Johannesson, M., 2008. Gender differences in deception. *Econ. Lett.* 99 (1), 197–199.
- Dreyfuss, B., Glicksohn, O., Heffetz, O., Romm, A., 2025. Deferred acceptance with news utility. *Manage. Sci.* .
- Dreyfuss, B., Heffetz, O., Rabin, M., 2022. Expectations-based loss aversion may help explain seemingly dominated choices in strategy-proof mechanisms. *Am. Econ. J.: Microecon.* 14 (4), 515–555.
- Dur, U., Hammond, R.G., Morrill, T., 2018. Identifying the harm of manipulable school-choice mechanisms. *Am. Econ. J.: Econ. Policy* 10 (1), 187–213.
- Erat, S., Gneezy, U., 2012. White lies. *Manage. Sci.* 58 (4), 723–733.
- Ericson, K.M., Fuster, A., 2014. The endowment effect. *Annu. Rev. Econ.* 6 (1), 555–579.
- Featherstone, C.R., Niederle, M., 2016. Boston versus deferred acceptance in an interim setting: an experimental investigation. *Game. Econ. Behav.* 100, 353–375.
- Fischbacher, U., 2007. z-tree: Zurich toolbox for ready-made economic experiments. *Exper. Econ.* 10 (2), 171–178.
- Fragiadakis, D.E., Troyan, P., 2019. Designing mechanisms to focalize welfare-improving strategies. *Game. Econ. Behav.* 114, 232–252.
- Gale, D., Shapley, L.S., 1962. College admissions and the stability of marriage. *Am. Math. Month.* 69 (1), 9–15.
- Guillen, P., Hakimov, R., 2018. The effectiveness of top-down advice in strategy-proof mechanisms: a field experiment. *Eur. Econ. Rev.* 101, 505–511.
- Hakimov, R., Kübler, D., 2021. Experiments on centralized school choice and college admissions: a survey. *Exper. Econ.* 24 (2), 434–488.
- Hassidim, A., Romm, A., Shorrer, R.I., 2021. The limits of incentives in economic matching procedures. *Manage. Sci.* 67 (2), 951–963.
- Holt, C.A., Laury, S.K., 2002. Risk aversion and incentive effects. *Am. Econ. Rev.* 92 (5), 1644–1655.
- Kőszegi, B., Rabin, M., 2006. A model of reference-dependent preferences. *Q. J. Econ.* 121 (4), 1133–1165.
- Kőszegi, B., Rabin, M., 2009. Reference-dependent consumption plans. *Am. Econ. Rev.* 99 (3), 909–936.
- Kőszegi, B., Rabin, M., 2007. Reference-dependent risk attitudes. *Am. Econ. Rev.* 97 (4), 1047–1073. <https://doi.org/10.1257/aer.97.4.1047>
- Li, S., 2017. Obviously strategy-proof mechanisms. *Am. Econ. Rev.* 107 (11), 3257–3287.
- Meisner, V., 2023. Report-dependent utility and strategy-proofness. *Manage. Sci.* 69 (5), 2733–2745.
- Meisner, V., von Wangenheim, J., 2023. Loss aversion in strategy-proof school-choice mechanisms. *J. Econ. Theory* 207, 105588. <https://doi.org/10.1016/j.jet.2022.105588>
- Pais, J., Pintér, A., 2008. School choice and information: an experimental study on matching mechanisms. *Game. Econ. Behav.* 64, 303–328.
- Pycia, M., Troyan, P., 2023. A theory of simplicity in games and mechanism design. *Econometrica* 91 (4), 1495–1526.
- Rees-Jones, A., Skowronek, S., 2018. An experimental investigation of preference misrepresentation in the residency match. *Proceed. Natl. Acad. Sci.* 115 (45), 11471–11476.
- Shorrer, R.I., Sóvágó, S., 2023. Dominated choices in a strategically simple college admissions environment. *J. Politi. Econ. Microecon.* 1 (4), 781–807.
- Troyan, P., 2012. Comparing school choice mechanisms by interim and ex-ante welfare. *Game. Econ. Behav.* 75 (2), 936–947.
- Troyan, P., 2019. Obviously strategy-proof implementation of top trading cycles. *Int. Econ. Rev. (Philadelph.)* 60 (3).
- Troyan, P., Morrill, T., 2020. Obvious manipulations. *J. Econ. Theory* 185, 104970.
- Zhang, L., Levin, D., 2017. Partition obvious preference and mechanism design: theory and experiment.