School Choice with Incomplete Preferences: An Experimental Study

Abstract

Standard matching theory assumes students have complete strict preferences, yet empirical evidence documents substantial rates of incomplete preferences in various choice contexts including school choice decisions. This paper provides the first experiment on extensions of the Deferred Acceptance (DA) and the Top Trading Cycles (TTC) mechanisms with incomplete preferences. Three key findings emerge. First, DA elicits higher overall consistent reporting rates than TTC driven by subjects with complete preferences, while subjects with incomplete preferences maintain high consistency under both mechanisms. Second, individual consistent reporting has a strong influence on determining welfare outcomes though the mechanisms are not strategyproof, generating more efficient and fair assignments. Third, students with incomplete preferences could benefit their peers: complete-preference students achieve higher exchange-proof assignment rates in mixed groups than in groups composed entirely of complete-preference students. These findings suggest that accommodating incomplete preferences in matching mechanisms can enhance welfare outcomes for all participants.

1. Introduction

School choice mechanisms have become central to education policy worldwide, with centralized assignment systems now operating in major cities, including Boston, Chicago, New York, and many other cities[1]. The design of these mechanisms fundamentally affects educational

[1] Boston: https://www.bostonpublicschools.org/enrollment/welcome-services/registration
Charlotte-Mecklenburg: https://www.cmsk12.org/academics/school-choice
Chicago: https://www.cps.edu/gocps/elementary-school/explore/choice-programs/
Denver: https://schoolchoice.dpsk12.org/page/round-1/
New York: https://www.schools.nyc.gov/enrollment/enroll-grade-by-grade/how-students-get-offers-to-doe-public-schools

access, efficiency, and equity outcomes for millions of students and families. Two mechanisms have been widely studied: the Deferred Acceptance (DA) mechanism (Gale & Shapley, 1962), which admits no justified envy[2], and the Top Trading Cycles (TTC) mechanism (Shapley & Scarf, 1974), which guarantees efficiency when preferences are complete and truthful.

Literature on standard school assignment algorithms (Abdulkadiroğlu & Sönmez, 2003) provides clear theoretical guidance under the assumption that students have complete strict preference orderings over all available schools. Under this assumption, the DA mechanism produces envy-free matchings where no student-school pair would mutually prefer to be matched to each other over their current assignments. The TTC mechanism generates Pareto efficient outcomes where no student can be made better off without making another student worse off. Both mechanisms are strategyproof where truthful preference reporting is a weakly dominant strategy. These theoretical results have guided mechanism adoption and policy recommendations across educational contexts.

However, the assumption of complete preferences has been questioned for decades with the first mention of incompleteness appearing in von Neumann-Morgenstern (1947)[3]. Literature has been exploring incompleteness to develop utility theory (Aumann, 1962; Bewley, 1986; Ok, 2002; Dubra et al., 2004), showing that rationality does not require preference completeness. A growing body of empirical evidence challenges the complete preferences assumption. Survey data finds that students have difficulties in deciding which school they prefer (Dwenger & Weizsäcker, 2018), and experimental studies consistently documenting substantial rates of

[2] I use "no justified envy" rather than "stability" to emphasize that the focus is on fairness of individual assignments rather than coalition stability, while these concepts are equivalent in this paper's context.

[3] "If the general comparability assumption is not made, … It leads to what may be described as a many-dimensional vector concept of utility. This is a more complicated and less satisfactory set-up, but we do not propose to treat it systematically at this time" (von Neumann-Morgenstern, 1947, p. 29).

incomplete preferences across various choice contexts (Danan & Ziegelmeyer, 2006; Gerasimou, 2022; Nielsen & Rigotti, 2024). These theoretical and empirical developments suggest that many students participating in school choice systems may be unable to rank all available schools.

The implications of incomplete preferences for mechanism performance remain largely unexplored in experimental literature. While theoretical work by Kuvalekar (2023) and Guamán and Torres-Martínez (2023) has begun addressing incomplete preferences in matching contexts, no existing experimental studies examine how DA and TTC mechanisms perform with incomplete preferences.

This paper provides the first experiment on the performance of DA and TTC mechanisms when students can express incomplete preferences through pairwise choice comparisons. In a controlled laboratory experiment (with 264 subjects across 15 sessions), subjects could indicate indecision when they select "cannot compare" two schools, revealing preference incompleteness. When students express incomplete preferences, the computer constructs all ordered lists consistent with their submitted choices. Each list is then used to generate different matchings, and the best matching is selected as the final assignment.

The experimental findings reveal several patterns that challenge conventional predictions regarding the efficiency superiority of the TTC mechanism and the fairness superiority of the DA mechanism. First, DA generates higher overall consistent preference reporting rates than TTC, and this advantage is concentrated among subjects with complete preferences; subjects with incomplete preferences maintain high consistency (approximately 75%) regardless of mechanism. Second, efficiency and fairness outcomes depend primarily on individual consistent reporting behavior. Students whose submitted preferences are consistent with their true preferences are more likely to be assigned to non-dominated and envy-free schools[4]. Third, students with incomplete preferences could create positive spillovers for their peers: more students with complete preferences in mixed groups are assigned to exchange-proof schools than those in all-complete groups.

This paper makes several key contributions to school choice and mechanism design literature. First, it provides the first experimental comparison of DA and TTC mechanisms when students can express incomplete preferences, offering empirical evidence about mechanism performance in previously untested conditions. Second, it employs a pairwise choice methodology that allows students to express preference incompleteness while maintaining compatibility with existing centralized assignment systems. Third, it implements matching with incomplete preferences by selecting the welfare-maximizing allocation among all feasible matchings. Finally, the results offer insights and practical guidance for mechanism designers and policymakers pertaining to mechanism performances.

2. Related Literature

2.1. Theoretical Literature on School Matching Mechanisms

This research aims to experimentally explore the effects of incomplete preferences in the frameworks of the student-proposing deferred acceptance (DA) mechanism (Gale and Shapley, 1962) and the top trading cycle (TTC) mechanism (Shapley and Scarf, 1974; Abdulkadiroğlu and Sönmez, 2003). Both mechanisms are widely discussed in school choice models due to their desirable properties. The DA mechanism is known for no justified envy, meaning that no student

[4] An exchange-proof assignment is when no student can strictly improve their assignments by switching schools (see Section 5.2.1 for details). A non-dominated assignment is a school which is not dominated by any other school. An envy-free assignment is when no student has a higher priority than another student but prefers that student's assignment to her own (See Section 5.3.1 for details).

who is prioritized over another at some school will prefer that other student's assignment to her own. The TTC mechanism is Pareto efficient for students, meaning no student can improve their match without making another student worse off (Abdulkadiroğlu & Sönmez, 2003). Both the student proposing DA mechanism and the TTC mechanism are strategyproof for students, meaning students cannot achieve a better outcome by misrepresenting their preferences (Dubins & Freedman, 1981; Roth, 1982a; Roth 1982b). While classical mechanisms assume that students have complete preferences (able to rank all schools from most to least preferred), completeness is difficult to justify on theoretical grounds.

The incomplete preferences literature addresses limitations in classical matching theory's assumption of complete preference orderings. Several solution concepts for incomplete preferences in marriage markets have been introduced, including the concepts of compromise core and men-(women-) optimal core, to address stability issues when not all alternatives can be fully ranked (Kuvalekar, 2023)[5]. In one-to-one housing markets with incomplete preferences, two concepts of coalitional stability (core and strong core) have been developed, and the TTC mechanism remains core-selecting and group strategyproof on any complete preference extension (Guamán & Torres-Martínez, 2023)[6]. In addition to the theoretical discussions, empirical research suggests that the complete preferences assumption may not hold in practice.

2.2. Experimental Studies on Incomplete Preferences

---

[5] The main concepts are: weak core, compromise core, and strong core. The weak core consists of matchings where no coalition wants to deviate and form matches among themselves that improve the allocation of all agents in the coalition, but this set can be too large. The strong core consists of matchings where no coalition wants to deviate and form matches among themselves that improve the allocation of at least one agent without harming anyone in the coalition, but this set can be empty. The compromise core is therefore proposed as an intermediate solution that sits between the weak and strong core concepts, providing a non-empty refinement that avoids both the potential emptiness of the strong core and the large size of the weak core.

[6] The core may have multiple elements and the strong core may be empty. The complete preference extension refers to transforming agents' incomplete preferences into strict linear orders.

Several experimental studies report that incomplete preferences are common. Indecisiveness has been elicited by testing willingness to give up money to delay making choices. Evidence shows that 59% of subjects exhibit incomplete preferences, with 28% of these subjects being indecisive in at least one-quarter of choices between lotteries and sure payoffs. (Danan & Ziegelmeyer, 2006). With risk-free options[7], 21% of subjects exhibit choice patterns consistent with incomplete preferences (Gerasimou, 2022). New methodological approaches have been developed that allow people to directly report when they cannot decide between stochastic perspectives (lotteries), separating true indifference ("I like both equally") from incomplete preferences ("I don't know which I prefer") (Nielsen & Rigotti, 2024). Research on why people sometimes choose to randomize between lotteries suggests that while 50% of subjects chose randomization, half of these cases actually reflected incomplete preferences rather than preferences for randomization (Cettolin & Riedl, 2019).

2.3. Experimental Studies on Pairwise Comparison Methods

Pairwise choice comparison methods are often used to elicit preferences. In a survey on ordinal preference elicitation methods (choices and ranking) in health economics, it's argued that pairwise comparisons reduce cognitive complexity and measurement error compared to full ranking tasks, particularly for decisions involving many alternatives (Ali & Ronaldson, 2012).

An experiment on paired comparison methods for economic valuation found that median willingness-to-forego estimates closely matched actual market prices (within $2-22) across four private goods, eliciting 96.7% non-circular preference among participants without the cognitive burden of complete ranking tasks (Rosenberger et al., 2002). Simple pairwise comparison

---

[7] In their experiment, risk-free options consist of gift card combinations.

approaches yield higher completion rates than complex allocation tasks[8] (Skedgel et al., 2015). Comparing the ranking methods to choice methods[9], the ranking methods have an advantage in preference transitivity (Huls et al., 2022), but the choice method leaves the possibility to incorporate an explicit "cannot compare" option. However, reliability problems can arise when pairwise comparisons involve numerous questions (Maida et al., 2012). While students in real-life school choice systems often apply to more than three schools, my experimental design limits participants to three pairwise comparisons to maintain reliability and avoid cognitive overload. Future research could extend this methodology to larger choice sets, potentially using adaptive pairwise comparison designs that minimize the number of required comparisons while still eliciting complete preference structures over more alternatives.

2.4. Experimental Studies on School Choice Mechanisms

The experimental literature on school choice mechanisms assumes complete preference. Chen and Sönmez (2006) pioneered experimental comparisons of the Boston mechanism, DA mechanism, and TTC mechanism, establishing frameworks for testing mechanism performance in laboratory settings. Subsequent research has varied information conditions (Chen & Sönmez, 2016; Guillen & Hakimov, 2017; Pais & Pintér, 2008), mechanism property description (Guillen & Hakimov, 2018), and learning dynamics over repeated rounds (Bó & Hakimov, 2020; Chen & Kesten, 2019; Ding & Schotter, 2019). Several findings have emerged. Information effects on dominant strategies vary across mechanisms, but reliable information about mechanism

[8] The comparison approach in their experiment asks subjects to choose the most preferred option among many; while the allocation task asks subjects to allocate a fix-ed amount money between multiple alternatives.

[9] The two choice methods are best-worst and best-best. In the best-worst method, subjects are asked to choose the best choice among all choices, and then the worst choice among all remaining choices. This process continues by alternating between selecting the best and worst options from the remaining set until all choices are ranked. In the best-best method, subjects are asked to choose the best choice among all choices and then the best choice among all remaining choices until all choices are ranked.

properties helps participants make better decisions. Repeated decision-making leads to gradual improvement in truth-telling rates. Whereas social learning and third-party advice can undermine truth-telling behavior.

To the best of my knowledge, there are no studies examining how DA and TTC mechanisms perform with incomplete preferences, a significant gap given the theoretical discussions on matching properties under such conditions (Kuvalekar, 2023; Guamán & Torres-Martínez, 2023) and empirical evidence that incomplete preferences exist broadly and naturally among decision-makers (Danan & Ziegelmeyer, 2006; Cettolin & Riedl, 2019; Gerasimou, 2022). This paper addresses this gap by comparing DA and TTC mechanism performance when students can express incomplete preferences through pairwise choice comparisons, providing the first experimental evidence on whether and how preference incompleteness affects school assignment mechanism outcomes.

3. The Model

A school matching problem with students having incomplete preferences consists of a finite set of students I and a finite set of schools S. Each school $s \in S$ has q_s available seats. The priority of school s is represented by $\succ_s$ which is strict and complete on I. Each student $i \in I$ has a preference relation P_i consisting of strict preferences $\succ_i$ and incomparable preferences $\bowtie_i$. The relation $s_1 \succ_i s_2$ means i strictly prefers s_1 against s_2. The relation $s_1 \bowtie_i s_2$ means s_1 and s_2 are incomparable: neither $s_1 \succ_i s_2$ or $s_2 \succ_i s_1$ holds[10]. A student has incomplete preferences if the incomparability relation $\bowtie$ applies to at least one school pair[11].

A *matching* is $\mu: I \to S \cup I$, such that each student is assigned to one school or to herself. $\mu(i)$ is referred to as the assignment of student i under μ, $\mu(s)$ as the assignment of school s under μ. If $\mu(i) \notin S$, then $\mu(i) = i$, which means student i remains unmatched.

A matching μ is *individually rational* if there is no student such that $i \succ_i \mu(i)$. A matching μ *dominates* μ' if $\mu(i) \succ_i \mu'(i)$ or $\mu(i) = \mu'(i)$ for all i and there exists at least one student j such that $\mu(j) \succ_j \mu'(j)$. A matching is *efficient* if it is not dominated by any other matching(s). A matching μ is *non-wasteful* if there does not exist a student-school pair (i, s) such that $|\mu(s)| < q_s$ and $s \succ_i \mu(i)$. A matching μ is *fair* if there does not exist and student-school pair (i, s) such that $\mu(i) \neq s$ and $i \notin \mu(s)$, but $s \succ_i \mu(i)$ and $i \succ_s j \in \mu(s)$ for one j. In other words: no student prefers another assignment over her own and has a higher priority at that school than the student who received it. A matching μ is *justified envy free* if it is individually rational, non-wasteful and fair.

A mechanism φ maps every $(P_I, \succ_S)$ to a matching. $\varphi_i(P_I, \succ_S)$ is the school matched to student $i \in I$ and $\varphi_s(P_I, \succ_S)$ is the set of students matched to school $s \in S$. A mechanism is *strategyproof* if for every $i \in I$, and for every $(P_I, \succ_S)$, there does not exist an alternative preference relation P_i' such that $\varphi_i(P_i', P_{-i}, \succ_S) \succ_i \varphi_i(P_i, P_{-i}, \succ_S)$, where P_{-i} is the preference

[10] Eliaz and Ok (2006) define $s \bowtie s'$ as neither $s \succsim s'$ nor $s' \succsim s$. In this paper, since $\succ_i$ is a strict preference order, there is no such pair (s, s') that $s \sim s'$ when $s \neq s'$. Thus, it will not lose any information defining $s \bowtie s'$ as neither $s \succ s'$ nor $s' \succ s$.

[11] *Incomparability* $\bowtie_i$ is not transitive: there may exist s_1, s_2, s_3 such that $s_1 \bowtie_i s_2$, $s_2 \bowtie_i s_3$, and $s_1 \succ_i s_3$. This intransitivity distinguishes incomparability from indifference, which is transitive. The two concepts also differ in their economic intuition. Incomparability means an agent cannot compare two options, whereas indifference means the agent views the two options as equivalent.

relations in P_I except for i. A mechanism is efficient (fair) if it only selects an efficient (fair) matching.

3.1 . School Choice Mechanisms

School choice mechanisms assign students to schools based on students' reported preferences over schools and schools' priorities over students. The paper considers two renowned mechanisms: the student proposing deferred acceptance mechanism (DA) and the top trading cycle mechanism (TTC). The DA mechanism is justified envy free (fair) but not necessarily efficient as there may exist alternative matchings where some students could be made better off without making any other students worse off. TTC is efficient but not necessarily justified envy free (fair) as the resulting matching may contain justified envy students when another student with a lower priority is assigned to a more preferred school.

The DA mechanism operates through a series of rounds. Initially, each student applies to their most preferred school. Schools then tentatively accept applicants up to their capacity based on priority and reject the rest. In subsequent rounds, rejected students apply to their next preferred school, and each school reevaluates all applicants (both new and those previously held) again tentatively accepting the highest-priority students up to capacity. This process repeats until no student is rejected in a round, at which point the tentative assignments become final.

The TTC mechanism proceeds in rounds where, in each step, every student points to their most preferred remaining school, and each school points to the highest-priority student among those pointing to it. These links form one or more cycles, and each student in a cycle is immediately assigned to the school they point to. Both the assigned students and any schools that reach full capacity are removed from the process. This procedure repeats with the remaining students and schools until all students are assigned.

However, the traditional mechanisms assume students have complete preferences; now I propose an extension of the DA mechanism and the TTC mechanism to accommodate incomplete preferences.

First, each student's preference P_i is modeled as $P_i = \succ_i^a \cap \succ_i^b \cap \ldots \cap \succ_i^k$, in which $\succ_i^a, \succ_i^b$ ... are strict ordered lists consistent with the strict preference relations of P_i[12]. $|P_i|$ is the number of ordered lists of student i.

Second, each student contributes one ordered list from her collection each time, resulting in $C = \prod |P_{i \in I}|$ unique preference combinations. Run the classical mechanism with each preference combination, resulting in C matchings.

Third, choose one of C matchings as the final matching. In the DA mechanism, selects the matching that minimizes willingness to swap, where the fewest number of students can mutually benefit from exchanging their assignments. If multiple DA matchings tie for minimum willingness to swap, one is selected randomly. The TTC treatment with incomplete preferences selects the matching that minimizes justified envy. If multiple outcomes tie for minimum justified envy, one is selected randomly.

The DA mechanism with incomplete preferences is justified envy free and the TTC mechanism with incomplete preferences is efficient (see proofs in Appendix D), but neither is strategyproof. In the scope of this paper, I use a computational method to calculate the proportion

[12] For example, if i's preference is $s_1 \succ_i s_2$, $s_1 \succ_i s_3$, and $s_2 \bowtie_i s_3$, then $P_i = \succ_i^a \cap \succ_i^b$ where $\succ_i^a = s_1 \succ s_2 \succ s_3$ and $\succ_i^b = s_1 \succ s_3 \succ s_2$.

of truthful reporting preferences being the weakly dominant strategy. Truthful reporting preference P_i is a weakly dominant strategy if there does not exist an alternative preference relation P_i' such that $\varphi_i(P_i', P_{-i}, \succ_S) \succ_i \varphi_i(P_i, P_{-i}, \succ_S)$. The computational method compares student i's matching outcome under truthful reporting versus misreporting, identifying cases where truthful reporting is weakly dominated by an alternative report. This comparison is evaluated across all possible preference combinations of other students (more details in Appendix A). Results show that in the DA mechanism, truthful reporting is weakly dominant in 91.27% scenarios, and in the TTC treatment, truthful reporting is weakly dominant in 92.67% scenarios.

4. Experiment Design

The experiment is designed to get insights about the performance of two matching mechanisms: the DA and the TTC mechanisms when students' preferences can be incomplete. Within each mechanism, groups in which all students have complete preferences serve as the Baseline, while groups with at least one student holding incomplete preferences are the treatment. I then compare consistent reporting behavior, efficiency, and justified envy across mechanisms and preference types.

Each experiment session consists of two parts. In Part 1, subjects' preferences over "schools" are elicited using a pairwise choice method without any knowledge of matching in Part 2; In Part 2 one of the two matching mechanisms, DA or TTC, is implemented and subjects' preferences over "schools" are elicited again and used for matching. Each experiment session contains Part 1 and one school assignment treatment in Part 2.

4.1. Part 1: Preference Elicitation Tasks

Part 1 is the same in all experiment sessions, consisting of two rounds of pairwise comparisons of three schools. The purpose of part 1 is to elicit their preferences over "schools". In each round, subjects are asked to state their binary ranking over three stochastic school options. Subjects were shown a school option table, and then asked three school comparison questions. Each school option is represented by a lottery (Appendix B has the full parameter design). For example, the three options available are: School X involves drawing one ball from an urn of ten balls, where 4 balls are worth \$18 and 6 balls are worth \$8, that is the lottery: $\$18{,}0.4; \$8{,}0.6$. School Y is represented by the lottery: $\$18\ or\ \$8$ with unknown probabilities, and School Z is the lottery $\$15{,}0.7; \$8{,}0.3$. In each school comparison question, subjects can choose "better than" or "Cannot compare the two". The "better than" options are interpreted as complete strict preferences and the "cannot compare" option is interpreted as incomplete preferences[13].

4.2. Part 2: School Choice Decision Tasks

Each experiment session contains one mechanism treatment in Part 2, which includes twenty rounds. In the first ten rounds, each student's feasible school options are the same as in the first round in Part 1; in the last ten rounds, each student's school options are the same as in the second round in Part 1. In each round of twenty rounds of school assignments, students are randomly grouped into three. There are three schools and three students in each group. Schools' priority information is shared with all subjects in the group. Before making school choice

[13] If the elicited preference is $X \succ Y, Y \succ Z, Z \succ X$, the preference is interpreted as indifference; if the elicited preference is $X \succ Y$, $Y \succ Z$ and $X \bowtie Z$, the preference is interpreted as $X \succ Y \succ Z$ because this is the only consistent extension of the elicited preference.

decisions, a video is played to illustrate the matching algorithm[14]. Paper instructions are distributed after the video instructions for students' reference.

In Part 2, subjects are asked to make choices over schools as in Part 1 in each round. After collecting answers to the pairwise comparison questions, the algorithm constructs a set of ordered lists for each student based on their response in that round (for all possible scenarios, check Appendix E).

Scenario 1: If subjects' choices are: "X is better than Y", "X is better than Z", and "Y is better than Z", then the set of ordered list consistent with their choices is $(X \succ Y \succ Z)$.

Scenario 2: In cases with incomplete preferences, I use the extension method in Dushnik and Miller (1941) to construct the set, in which the intersection of the ordered lists is the preference elicited in pairwise comparisons. For example, if subjects' choices are: "X is better than Y", "X is better than Z", and "Cannot compare Y and Z", then the set of ordered lists consistent with their choices is $(X \succ Y \succ Z)$ and $(X \succ Z \succ Y)$.

If each constructed set of group members contains one ordered list, the algorithm runs as usual, generating one matching outcome. If there exists one or more group members whose constructed set has more than one ordered list, the matching mechanism will run multiple times to generate multiple matchings, and each time the algorithm takes one ordered list from each group member until all combinations are exhausted. Among the multiple matching outcomes, the algorithm applies different selection criteria depending on the treatment. In the DA treatment, the

[14] Links to the TTC recording: https://youtu.be/gqLIfcUKdF0, DA recording: https://youtu.be/P0hbC71T5_E

algorithm selects the outcome that minimizes willingness to swap, while in the TTC treatment, the algorithm selects the outcome that minimizes justified envy[15].

4.3. Payments

Each subject is paid by randomly selecting one round at the end of the experiment. If the selected round is in Part 1, the preference elicitation task, and the selected answer is “better than”, the subject gets paid based on the preferred school; if the selected answer is “cannot compare the two”, the subject may select another participant's answer to determine their payment. For example, if Subject #1 answers “cannot compare”, she is offered to select among participants who chose "is better" answers, and gets paid based on the school picked by that participant.

If the selected round is in Part 2, the school choice decision, the payment is determined by the matched school in that round.

In addition, each subject gets a $5 participation fee.

4.4. Experiment Sessions

I conducted 15 experiment sessions in Spring 2025 at the Experimental Economics Center at Georgia State University. Subjects are undergraduate students from Georgia State University, 264 subjects in total with 132 subjects in each treatment. Each session consists of one mechanism treatment and lasts from 60-90 minutes. Experiment instructions are included in Appendix C.

5. Results

[15] Appendix C Part 2 contains examples on both mechanisms.

Average earnings totaled $17.37 per participant, including a $5 participation fee. Average earnings across matching mechanisms showed minimal variation, with $17.52 in the Deferred Acceptance (DA) mechanism and $17.22 in the Top Trading Cycles (TTC) mechanism. Similarly, average earnings did not differ substantially between subjects with complete ($17.17) and incomplete preferences ($17.71).

I categorize subjects into four distinct types based on their responses to school comparison questions in Part 1. Complete preferences arise when all answers are strict and transitive. For example, if a subject states that School X is better than Y, School Y is better than Z, and School X is better than Z. Incomplete preferences contain "cannot compare" responses while maintaining transitivity in the strict preference portions. For example, School X is better than Y, School Z is better than Y, but the subject cannot compare Schools X and Z. Other preferences consist of cyclical rankings, such as School X is better than Y, School Y is better than Z, and School Z is better than X, and responses violating transitivity, for example, School X is better than Y, School Y is better than Z, but the subject cannot compare Schools X and Z.

Table 1 reports the sources of incomplete preferences in the experiment. Schools are represented by sure (certain payoff), risky (known probability and known payoff), and ambiguous (unknown probability but known payoff) lotteries. Incomplete preferences are most prevalent in ambiguous versus risky lottery pairs, where participants chose "Cannot compare the two" in 17.52% of cases[16]. Table 2 reports individual preference types by categorizing participants based on their preference patterns in both rounds. The results show that half of all participants exhibit complete preferences in both rounds (55.30% in DA and 50.76% in TTC),

[16] This number is lower than the results in Cettolin and Riedl (2019) because in their experiment, each subject made 21 comparisons between risky and ambiguous lotteries, whereas in Part 1 of this study, each subject made only 6 comparisons.

while roughly one-quarter display incomplete preferences in at least one round (25.76% in DA and 29.55% in TTC). The remaining participants showed other combinations of preference types across rounds. These statistics indicate that the sample is well-balanced across the two mechanism treatments[17].

Table 1. Sources of Incomplete Preferences

Lottery Pair Type	Incomplete preferences	Total Obs.
Ambiguous vs Ambiguous	9 (9.38)	96
Ambiguous vs Risky	136 (17.52)	776
Ambiguous vs Sure	7 (5.83)	120
Risky vs Risky	5 (1.60)	312
Risky vs Sure	4 (1.43)	280
All pairs	161 (10.16)	1584

Note: Table 1 reports the frequency of incomplete preferences across different lottery pair categories in Part 1, with row percentages in parentheses. Each subject made 3 choices in each of 2 rounds, generating 264 subjects*3*2=1584 obs.

Table 2. Frequency of Each Type of Subjects in Part 1

	DA	TTC
Always complete	73 (55.30)	67 (50.76)
Incomplete (1 or 2 incompletes)	34 (25.76)	39 (29.55)
Others	25 (18.94)	26 (19.70)
Total obs.	132	132

Note: Table 2 reports the frequency of subject types, categorizing subjects as: always exhibiting complete preferences across both rounds, having incomplete preferences in only one round, having incomplete preferences in both rounds, or other combinations. Column percentages are in parentheses.

The following analysis focuses on the final ten rounds (11-20) of Part 2 to allow for sufficient experience with the mechanisms.

5.1.2. Consistent reporting rate

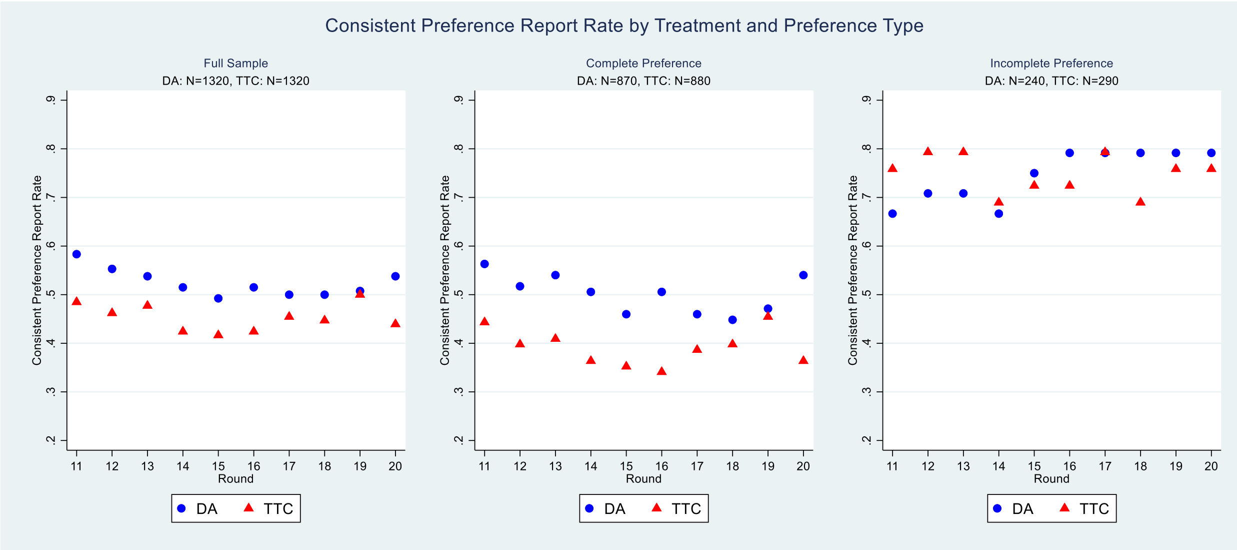
[17] A Pearson chi-square test confirms that the distribution of preference types does not differ significantly between mechanisms ($\chi^2(2) = 0.62$, $p = 0.734$).

The consistent preference reporting requires no direct contradictions of strict preference components between Part 1 and Part 2. Using elicited preferences from Part 1 as the baseline, a report in Part 2 is classified as inconsistent only when there is a direct reversal of strict preferences (e.g., reporting "X is better than Y" in Part 1 but "Y is better than X" in Part 2). For example, if a subject reports "X is better than Y" in Part 1 and later reports "Cannot compare X and Y" in Part 2 (or vice versa), this is considered a consistent reporting since there is no direct preference reversal. One special case involves subjects who exhibited indifferent preferences in Part 1. If a subject reports "X is better than Y", "Y is better than Z", and "Z is better than X" in Part 1, but later reports "Y is better than X", "Z is better than Y", and "X is better than Z" (or vice versa), this is considered a consistent preference reporting, regardless of the specific direction of the cycle[18].

The overall consistent reporting rate is about 50% percent, which is within the 30-80% range commonly documented in the literature (Hakimov & Kübler, 2021). Figure 1 displays the consistent reporting rate across rounds. First, the rate starts high, decreases slightly in the middle rounds, then increases slightly toward the end. Second, the patterns are visible that the DA mechanism elicits more consistent preferences (52.4%) compared to the TTC mechanism (45.3%). This difference is primarily driven by subjects with complete preferences, who constitute two-thirds of the sample. In both mechanisms, about 75% of subjects with incomplete preferences report consistent preferences: 74.58% in the DA mechanism and 74.83% in the TTC mechanism (Table 7 column 1).

Figure 1. Consistent Preference Reporting Rate across Rounds

[18] Such cases are rare, representing less than 1% of the total observations (approximately 2 cases per round).



For statistical inference, I use a random effects logistic model (Equation 1). The dependent variable is whether subject i reports consistent preferences in round t. DA is a binary variable of mechanism ($= 1$ for DA, $= 0$ for TTC). $Pref_i$ is a categorical variable of individual i's preference type, where individuals with complete preferences are the baseline, $Pref_{i1} = 1$ if the individual has incomplete preferences, and $Pref_{i2} = 1$ if the individual has other types of preferences. α_i is the unobserved subject-specific heterogeneity and is uncorrelated with each explanatory variable in all time periods, and ε_{it} is the idiosyncratic error term.

$$Y_{it} = \beta_0 + \beta_1 DA + \gamma_1 Pref_{i1} + \gamma_2 Pref_{i2} + \delta_1 DA * Pref_{i1} + \delta_2 DA * Pref_{i2} + \alpha_i + \varepsilon_{ij} \quad (1)$$

Table 3 presents random-effects logistic regression coefficients across different model specifications. In the full interaction model (column 2), the coefficient on the DA mechanism β_1 is 2.658 (p = 0.054), indicating that, holding preference type constant at complete preferences, subjects in the DA mechanism have 2.66 times the odds of reporting consistent preferences compared to those in the TTC mechanism. The coefficient on incomplete preferences γ_1 is 29.247 ($p < 0.01$), showing that within the TTC mechanism, students with incomplete preferences have much higher odds of reporting consistently compared to students with complete preferences. The interaction term δ_1 is 0.294 ($p = 0.242$), while not statistically significant, suggesting that the positive effect of incomplete preferences on consistent reporting is attenuated in the DA mechanism. The combined odds ratio for incomplete preferences in DA is approximately 8.60 (29.247 × 0.294), smaller than the effect observed in TTC, but still indicating higher odds of consistent reporting relative to complete preferences. These results support the patterns in Figure 1 that the DA mechanism elicits a higher consistent preference reporting rate and subjects with incomplete preferences naturally report preferences consistently.

Table 3. Random logistic model on consistent preference report

	Consistent preference reporting rate	
	(1)	(2)
DA	2.222*	2.658*
	(0.919)	(1.350)
Preference type		
Incomplete	16.732***	29.247***
	(8.717)	(20.684)
Other	0.226**	0.172*
	(0.141)	(0.170)
DA × *Incomplete*		0.294
		(0.308)
DA × *Other*		1.531
		(1.939)
N	2640	2640

Note: *** p<.01, ** p<.05, * p<.1. Table 3 reports whether a subject reports consistent preferences. The coefficients in the table are odds ratios, with standard errors in parentheses. Standard errors are clustered at the participant level.

Result 1a: The DA mechanism generates higher overall consistent reporting rates than the TTC mechanism.

Result 1b: Subjects with incomplete preferences achieve high consistency rates regardless of the mechanism.

5.2. Efficiency

5.2.1. Exchange-proof assignment

Efficiency is measured using two approaches. The first measurement is the number of students who are assigned to exchange-proof schools in each group. An exchange-proof assignment means students cannot strictly improve their outcome by swapping schools with another student without making that student worse off. Students are willing to swap schools when they can be strictly or weakly better off (but not when they cannot compare two schools). For example, if student A prefers school X than Y than Z, and student B prefers school Y than X than Z, and student A is assigned to school Y while student B is assigned to school X, then both students can be strictly better off by switching their assignments. Neither student has an exchange-proof assignment[19]. When agents have complete preferences, the TTC mechanism is theoretically exchange-proof, while the DA mechanism is not.

A random-effects ordered logit model (Equation 2) formally examines these patterns, where the dependent variable is the number of students who are assigned to an exchange-proof school in group g round t, DA is a binary variable on mechanism, $CtConsist$ is a categorical variable indicating number of students who report consistent preferences, $CtIncPref$ is a categorical variable indicating number of students who have incomplete preferences, and $CtOtherPref$ controls the number of students who have other types of preferences. $1\{\cdot\}$ is an

[19] Example 2 (Both students have exchange-proof assignments): If student A prefers school X than Y than Z, and student B prefers school Y than Z, X than Z, but cannot compare X and Y, and student A is assigned to school Y while student B is assigned to school X, then student A would benefit from swapping but student B is unwilling to swap (cannot determine if the trade helps or hurts). Since the swap would potentially harm student B, no Pareto-improving trade exists. Both students have exchange-proof assignments.
Example 3 (Student A does not have an exchange-proof assignment, Student B does): If student A prefers school X than Y than Z, and student B is indifferent between schools X, Y and Z, and student A is assigned to school Y while student B is assigned to school X, then student A would be strictly better off swapping while student B would be no worse off (indifferent). Student A does not have an exchange-proof assignment, but student B does.

indicator variable, equal to one when the condition in the bracket is satisfied, 0 otherwise. α_g is the unobserved group-specific heterogeneity, and ε_{gt} is the idiosyncratic error term.

$$Y_{gt} = \beta + \beta_1 DA + \sum_{a=1,2,3} \gamma_a 1\{CtConsist_{gt} = a\} + \sum_{b=1,2} \gamma_b 1\{CtIncPref_{gt} = b\}$$

$$+ \sum_{c=1,2} \gamma_c 1\{CtOtherPref_{gt} = c\} + \alpha_g + \varepsilon_{gt} \ (2)$$

Table 4. Ordered logistic model on the count of exchange-proof assignments

	Count of exchange-proof assignments
DA	1.223 (0.211)
Count consistent reporting	
1	1.348 (0.269)
2	2.905*** (0.648)
3	9.501*** (4.211)
Count incomplete preferences	
1	1.411** (0.244)
2 or more	2.335** (0.976)
Count other preferences	
1	0.644*** (0.102)
2 or more	0.787 (0.199)
N	880

Note: *** p<.01, ** p<.05, * p<.1. Table 4 reports the number of students who are assigned to exchange-proof schools in each group in each round. The coefficients in the table are odds ratios, with standard errors in parentheses. Standard errors are clustered at the participant level.

Table 4 shows how group composition affects exchange-proof assignment outcomes. Consistent preference reporting exhibits a strong dose-response relationship with exchange-proof assignments: while one consistent reporter increases the odds by 35% (not significant) comparing to no consistent reporter, having two consistent reporters increases the odds to 2.91 times those of the baseline ($p < 0.01$), and when all three students report consistently, the odds rise to 9.50 times the baseline level ($p < 0.01$). Groups with more students holding incomplete preferences achieve better exchange-proof outcomes due to the incomparable nature (OR=1.411 when one student has incomplete preferences, $p < 0.01$, and OR=2.335 when two or more students have incomplete preferences, $p < 0.01$). The mechanism type (DA vs. TTC) does not significantly affect exchange-proof assignments once group composition is controlled for (OR = 1.223, $p = 0.243$), though the point estimate suggests DA may slightly increase the odds relative to TTC.

Result 2a: Consistent reporting shows a strong dose-response effect in increasing group-level exchange-proof assignments.

Result 2b: A higher number of students with incomplete preferences improves the likelihood of exchange-proof assignments.

Result 2c: Though not statistically significant, the DA mechanism slightly increase the odds of being assigned to exchange-proof schools.

5.2.2. Non-dominated assignment: Complete-Preference Subsample Analysis

The other measure examines whether a student is assigned to a non-dominated school. For example, if a student has incomplete preferences between schools X and Y, and school Z is strictly dominated by both X and Y, then both X and Y are non-dominated assignments. The non-dominated assignment rate in Table 7 is calculated as the number of students assigned to non-dominated schools divided by the total number of students.

There is a natural difference between exchange-proof assignments and non-dominated assignments. Exchange-proof is a property involving potential trades between students, while non-dominated is an individual property based solely on one's own preference structure. If at least two students are assigned to non-dominated schools, then all students in the groups are assigned to exchange-proof schools. The summary statistics in Table 7 (columns 2 & 3) support this relationship: the proportion of non-dominated assignments is consistently larger than the proportion of exchange-proof assignments.

First, I restrict the sample to students with complete preferences and examine how their outcomes vary by group composition. I compare students in all-complete groups (where all group members have complete preferences) with those in mixed groups (at least one other group member has incomplete preferences).

Descriptive statistics show that complete-preference students achieve higher non-dominated assignment rates in mixed groups (44.0%, n = 647) compared to all-complete groups (40.9%, n = 1103). In the DA mechanism, 47.6% of students in mixed groups (n = 292) receive non-dominated assignments compared to 43.4% in all-complete groups (n = 578). Under TTC, the corresponding rates are 41.1% (n = 355) and 38.1% (n = 525), respectively.

For statistical inference, a random effect logistic model is estimated (model 2). The dependent variable is whether subject i is assigned to a non-dominated school in round t. DA is a binary variable of mechanism ($= 1$ for DA, $= 0$ for TTC). $Consist_{it}$ is a binary variable on whether subject i reported consistent preferences in Part 2 round t as in Part 1. $CtOtherConsist_{gt}$ counts the number of other group members who report consistent preferences, and $MixedGroup_{gt}$ is whether all group members have complete preferences ($= 1$

when at least one other group member has incomplete preferences, $= 0$ when all group members have complete preferences). Table 5 column 1 shows a non-significant 15% increase in the odds of non-dominated assignment under DA relative to TTC (OR = 1.146, p = 0.664). Individual consistent reporting shows a strong association with outcomes, with students who report consistent preferences having 5.6 times the odds of exchange-proof assignment (OR = 5.602, p < 0.01). The number of other consistent reporters in the group does not significantly affect individual outcomes (OR = 1.166, p = 0.232), suggesting that non-dominance is an individual property rather than a group-level phenomenon. Similarly, being in a mixed group with both complete and incomplete preference holders has only marginal effect on non-dominated assignments (OR = 1.043, p = 0.792).

$$Y_{it} = \beta_0 + \beta_1 DA + \beta_2 Consist_{it} + \beta_3 CtOtherConsist_{gt} + \beta_4 MixedGroup_{gt} + \alpha_i + \varepsilon_{it} \quad (2)$$

Table 5. Logistic model on welfare measurements (complete-preference subsample)

	Non-dominated (1)	Envy-free (2)
DA	1.146 (0.360)	1.667 (0.608)
Consistent report	5.602*** (1.416)	5.010*** (1.756)
Count other consistent report	1.166 (0.150)	0.997 (0.167)
Mixed group	1.043 (0.168)	1.303 (0.288)
N	1750	1750

Note: *** p<.01, ** p<.05, * p<.1. Table 5 restricts the samples to students with complete preferences. The coefficients in the table are odds ratios, with standard errors in parentheses. Standard errors are clustered at the participant level. Column 1 reports whether a subject is assigned to a non-dominated school, and column 2 reports whether a subject is envy-free.

Results 2d: Consistent reporting significantly increases the likelihood of being assigned to a non-dominated school for complete-preference subjects.

Results 2e: Mechanism choice, other group members' preference types, and other group members' consistent reporting behavior have positive yet insignificant effects on non-dominated assignments of complete-preference subjects.

5.2.2. Non-dominated assignment: Full Sample Analysis

The previous analysis restricted attention to students with complete preferences, as previous experimental studies assumed complete preferences. We now turn to the full sample, which includes all subjects regardless of preference types. This analysis addresses an additional question: Do subjects with incomplete preferences achieve non-dominated assignments at similar rates as those with complete preferences?

Table 7 presents full sample descriptive statistics. The patterns are consistent with the complete-preference subsample: DA shows a small advantage for non-dominated assignments. Table 6 presents random-effects logistic regression results for the full sample (N = 2,640). The dependent variable in column 1 is whether a student receives a non-dominated assignment. The model (Equation 3) includes preference type indicators, with complete preferences as the baseline category. $Pref_{i1} = 1$ if an individual has incomplete preference, and $Pref_{i2} = 1$ if an individual has other type of preference[20]. α_i is the unobserved subject-specific heterogeneity, and ε_{it} is the idiosyncratic error term.

[20] The incomplete preference category differs from both indifferent and intransitive categories. According to Auman (1962), “Indifference between two alternatives should not be confused with incomparable the former involves a positive decision that it is immaterial whether the one or the other alternative is chosen, whereas the latter means that no decision is reached” (p.446). Incomplete preferences preserve transitivity within their strict preference components, while intransitive preferences violate transitivity in these components. Indifference comes from the cyclical choices between three schools. These distinctions prevent pooling incomplete preferences with indifferent or intransitive preferences.

$$
\begin{aligned}
Y_{it} = \beta_0 + \beta_1 DA + \beta_2 Consist_{it} + \beta_3 CtOtherConsist_{gt} + \beta_4 CtOtherIncomp_{gt} \\
+\gamma_1 Pref_{i1} + \gamma_2 Pref_{i2} + \delta_1 DA * Pref_{i1} + \delta_2 DA * Pref_{i2} + \alpha_i + \varepsilon_{it}
\end{aligned} \tag{3}
$$

The DA mechanism slightly increases the likelihood of non-dominated assignments, with students under DA having 13.0% higher odds (OR = 1.130, p = 0.723) compared to TTC (Table 6 column 1). This effect of the DA mechanism is similar in magnitude to what we observed in the complete-preference subsample (OR = 1.146).

Students who report consistent preferences have 8.3 times the odds of a non-dominated assignment (OR = 8.334, p < 0.01). The strong association between consistent reporting and both exchange-proof and non-dominated assignments suggests that most students benefit from honest preference revelation. This aligns with the computational method, finding that truthful reporting is a dominant strategy in more than 90% of scenarios. While our mechanisms are not strategyproof, manipulation opportunities appear limited in practice, and students who attempt to manipulate are often worse off than those who report consistently.

Students with incomplete preferences achieve significantly higher non-dominated rates than those with complete preferences (OR = 5.002, p < 0.01). However, as the subsample analysis reveals, this likely reflects subjects with incomplete preferences having fewer schools in their choice set, mechanically reducing dominated alternatives. The interaction terms (DA × Incomplete) are not significant (OR = 0.622, p = 0.512), indicating no mechanism advantage for subjects with incomplete preferences.

Similar to the subsample results, the number of peers with incomplete preferences does not significantly affect individual welfare outcomes (OR = 1.036, p = 0.795). Likewise, the count of peers reporting consistently shows a non-significant positive effect, increasing the odds of non-dominated assignment by 14% (OR = 1.136, p = 0.265).

Table 6. Logistic model on efficiency measurements (full sample)

	Non-dominated assignments	Envy-free assignments
	(1)	(2)
DA	1.130 (0.389)	1.694 (0.624)
Consistent report	8.334*** (2.014)	7.221*** (2.167)
Preference type		
Incomplete	5.002*** (2.729)	2.004 (1.266)
DA × Preference type		
DA × *Incomplete*	0.622 (0.512)	0.928 (0.860)
Count other consistent report	1.136 (0.130)	0.853 (0.114)
Count other incomplete preferences	1.036 (0.141)	1.255 (0.204)
N	2640	2640

Note: *** p<.01, ** p<.05, * p<.1. The coefficients in the table are odds ratios, with standard errors in parentheses. Standard errors are clustered at the participant level. The dependent variable in column 1 is whether a subject is assigned to a non-dominated school; the dependent variable in column 2 is whether a subject is assigned to an envy-free school.

Results 2f: Consistent reporting significantly increases the likelihood of being assigned to a non-dominated school, while mechanism, other group members' preference types and reporting behavior have positive yet insignificant effects on non-dominated assignments for all subjects.

5.3. Justified Envy

Justified envy arises when a student has a higher priority than another student but prefers that student's assignment to her own, similar to the fairness concept in Balinski and Sönmez (1999). The variable of interest is the proportion of students who are envy-free (experience no justified envy). When agents have complete preferences, the DA mechanism is theoretically envy-free, while the TTC mechanism is not.

5.3.1. Complete-Preference Subsample Analysis

Students with complete preferences in mixed groups achieve higher envy-free rates (85.6%, n = 647) than those in all-complete groups (81.4%, n = 1,103), though this pattern varies by mechanism. In the DA mechanism, group composition has minimal impact: envy-free rates are 86.6% in mixed groups (n = 292) versus 84.8% in all-complete groups (n = 578). In contrast, TTC shows greater sensitivity to group composition: mixed groups achieve 84.8% envy-free rates (n = 355) compared to 77.7% in all-complete groups (n = 525). This suggests that the presence of incomplete-preference peers may improve fairness under TTC.

Table 5 column 2 presents logistic regression estimates for envy-free assignments. Individual consistent reporting has the strongest association with fair matchings, with students who report consistent preferences having 5.01 times the odds of envy-free assignment compared to those who do not (OR = 5.010, $p < 0.01$). The DA mechanism shows 67% higher odds than TTC (OR = 1.667, $p = 0.161$), though this difference is not statistically significant. Neither peers' consistent reporting (OR = 0.997, $p = 0.987$) nor mixed group composition (OR = 1.303, $p = 0.230$) significantly affects envy-free assignments among complete-preference students.

Extending to the full sample (Table 6), the main findings persist across all preference types. Individual consistent reporting remains the dominant factor (OR = 7.221, $p < 0.01$). DA is 69.4% more likely to assign a student to an envy-free school (OR = 1.694, $p = 0.152$), though not statistically significant. Students with incomplete preferences achieve higher envy-free rates than those with complete preferences (OR = 2.004, $p = 0.271$), likely because their inability to compare certain schools reduces potential justified envy. As in the subsample, peers' consistent reporting (OR = 0.853, $p = 0.233$) shows little effects, while the presence of incomplete-preference peers (OR = 1.255, $p = 0.161$) influences individual envy-free outcomes marginally.

Result 3a: Individual consistent reporting is the strongest predictor of envy-free assignments.

Result 3b: DA shows modestly higher odds of envy-free assignments than TTC.

Result 3c: Students with incomplete preferences achieve higher envy-free rates.

Result 3d: Being in a mixed group can marginally improve the odds of envy-free assignments.

When incomplete preferences are elicited, the differences between mechanisms become less prominent in efficiency and fairness measurements, contrasting with the complete preferences literature where mechanism choice has large effects on welfare outcomes (Chen and Sönmez, 2006; Chen and Sönmez, 2016; Pais and Pintér, 2008)[21].

Overall, DA shows a slight advantage in inducing consistent preferences, which further increases efficiency and reduces justified envy. Notably, although neither mechanism used in the experiment is strategy-proof, the empirical analysis of the data confirms that consistent reporting significantly increases welfare.

For a robustness check, I replicate the above analysis by replacing consistent preference reporting with identical preference reporting. The results remain largely consistent, with full details presented in Appendix F.

6. Conclusion

[21] Regarding stability (similar to justified envy) in this research, they find the matchings in the DA mechanism is 20-30% more stable than those in the TTC mechanism (Chen and Sönmez, 2006; Chen and Sönmez, 2016; Pais and Pintér, 2008). Regardig efficiency, the TTC mechanism is about 10-20% more efficient than the DA mechanism (Pais and Pintér, 2008).

Incomplete preferences are not necessarily a sign of confusion or insufficient information, but rather a natural feature of decision-making that may improve matching outcomes by reducing strategic complexity. This paper provides the first experimental examination of DA and TTC mechanisms when students can express incomplete preferences in school choice contexts. The findings challenge conventional predictions about mechanism superiority by revealing that performance depends critically on both preference reporting behavior and completeness of preferences.

The results establish three key empirical patterns. First, consistent preference reporting varies by both mechanism and preference type. DA induces higher overall consistency rates than TTC, driven by subjects with complete preferences. Among subjects with incomplete preferences, both mechanisms elicit similarly high consistency rates. Second, individual consistent reporting is the strongest predictor of both non-dominated and envy-free assignments, with consistent reporters having 5-8 times the odds of fair or efficient matchings across different specifications. Third, students with incomplete preferences achieve higher rates of both efficiency and fairness compared to those with complete preferences, likely because their inability to compare certain schools reduces dominated alternatives and potential justified envy. Fourth, incomplete preferences generate potential benefits without imposing costs on others. Moreover, the presence of incomplete-preference students may create positive spillovers: students with complete preferences in mixed groups achieve higher envy-free assignment rates than those in all-complete groups, though the result is not statistically significant. These findings suggest that incomplete preferences can enhance rather than diminish matching outcomes for all participants.

Approximately 9.77% (SD = 0.297) of students with incomplete preferences switch to reporting complete preferences, with 7.95% doing so under the DA mechanism and 11.6% under the TTC mechanism. Conversely, 7.23% (SD = 0.259) of students with complete preferences switch to reporting incomplete preferences, with 7.35% in the DA mechanism and 7.12% in the TTC mechanism. These results show a balanced rate of switching between complete and incomplete preferences, suggesting that the introduction of incomplete preferences does not make students with complete preferences more likely to misreport or attempt to game the system.

These findings offer practical guidance for mechanism designers and policymakers. The strong association between consistent reporting and favorable matching outcomes suggests that most students benefit from honest preference revelation, even though neither mechanism is fully strategyproof. The pairwise comparison methodology elicits incomplete preferences while maintaining compatibility with existing centralized assignment systems, offering a practical implementation approach for school choice markets. Importantly, incomplete preferences does not harm others' welfare outcomes and may even enhance exchange-proof assignments at the group level, suggesting no welfare costs from allowing preference incompleteness in school choice mechanisms.

Table 7. Summary Statistics (Rounds 11-20)

		(1) Consistent report rate	(2) Exchange-proof assignment	(3) Non-dominated assignment	(4) Envy-free students
Complete preference	DA	0.5011 (0.3937)	0.7483 (0.4343)	0.4483 (0.4976)	0.8540 (0.3533)
	N	87	870	870	870
	TTC	0.3909 (0.3762)	0.6773 (0.4678)	0.3932 (0.4887)	0.8057 (0.3959)
	N	88	880	880	880
Incomplete preference	DA	0.7458 (0.3635)	0.8375 (0.3670)	0.6583 (0.4753)	0.9208 (0.2706)
	N	24	240	240	240
	TTC	0.7483 (0.3491)	0.8621 (0.3454)	0.6966 (0.4605)	0.9069 (0.2911)
	N	29	290	290	290
Other preference (cyclical or intransitive)	DA	0.3667 (0.3526)	0.8762 (0.3302)	0.7619 (0.4269)	0.5571 (0.4979)
	N	21	210	210	210
	TTC	0.2467 (0.3739)	0.8533 (0.3550)	0.7133 (0.4537)	0.7200 (0.4505)
	N	15	150	150	150
All	DA	0.5242 (0.3966)	0.7848 (0.4112)	0.5364 (0.4989)	0.8189 (0.3852)
	N	132	1320	1320	1320
	TTC	0.4530 (0.4022)	0.7379 (0.4400)	0.4962 (0.5002)	0.8182 (0.3858)
	N	132	1320	1320	1320

Notes: Standard errors in parentheses. Table 7 shows the summary statistics of the full sample in rounds 11-20 in Part 2.

References

Abdulkadiroğlu, A., & Sönmez, T. (2003). School Choice: A Mechanism Design Approach. *American Economic Review*, 93(3), 729–747. https://doi.org/10.1257/000282803322157061

Ali, S., & Ronaldson, S. (2012). Ordinal preference elicitation methods in health economics and health services research: using discrete choice experiments and ranking methods. *British medical bulletin*, *103*(1), 21-44. https://doi.org/10.1093/bmb/lds020

Aumann, R. J. (1962). Utility theory without the completeness axiom. *Econometrica: Journal of the Econometric Society*, 445-462. https://doi.org/10.2307/1909888

Balinski, M., & Sönmez, T. (1999). A Tale of Two Mechanisms: Student Placement. Journal of Economic Theory, 84(1), 73–94. https://doi.org/10.1006/jeth.1998.2469

Bewley, T. F. (1986). Cowles Foundation Discussion Paper No. 807. *Knightian Decision Theory: Part*, *1*.

Bó, I., & Hakimov, R. (2020). Iterative versus standard deferred acceptance: Experimental evidence. *The Economic Journal*, 130(626), 356–392. https://doi.org/10.1093/ej/uez036

Chen, Y., & Kesten, O. (2019). Chinese college admissions and school choice reforms: An experimental study. *Games and Economic Behavior*, *115*, 83–100. https://doi.org/10.1016/j.geb.2019.02.003

Chen, Y., Liang, Y., & Sönmez, T. (2016). School choice under complete information: An experimental study. *Journal of Mechanism and Institution Design*, 1(1), 45–82. https://doi.org/10.22574/jmid.2016.12.002

Chen, Y., & Sönmez, T. (2006). School choice: An experimental study. *Journal of Economic Theory*, 127(1), 202–231. https://doi.org/10.1016/j.jet.2004.10.006

Danan, E., & Ziegelmeyer, A. (2006). Are preferences complete? An experimental measurement of indecisiveness under risk. Working paper.

Ding, T., & Schotter, A. (2019). Learning and mechanism design: An experimental test of school matching mechanisms with intergenerational advice. *The Economic Journal*, 129(623), 2779-2804. https://doi.org/10.1093/ej/uez024

Dubra, J., Maccheroni, F., & Ok, E. A. (2004). Expected utility theory without the completeness axiom. *Journal of Economic Theory*, *115*(1), 118–133. https://doi.org/10.1016/S0022-0531(03)00166-2

Dwenger, N., Kübler, D., & Weizsäcker, G. (2018). Flipping a coin: Evidence from university applications. Journal of Public Economics, 167, 240–250. https://doi.org/10.1016/j.jpubeco.2018.09.014

Gerasimou, G. (2022). Eliciting Weak or Incomplete Preferences in the Lab: A Model-Rich Approach. Working paper

Grenet, J., He, Y., & Kübler, D. (2022). Preference Discovery in University Admissions: The Case for Dynamic Multioffer Mechanisms. *Journal of Political Economy*, *130*(6), 1427–1476. https://doi.org/10.1086/718983

Guamán, E., & Torres-Martínez, J. (2023). On Housing Markets with Indecisive Agents by Emilio Guamán, Juan Pablo Torres-Martínez: SSRN. https://papers.ssrn.com/sol3/papers.cfm?abstract_id=4461572

Guillen, P., & Hakimov, R. (2017). Not quite the best response: Truth-telling, strategy-proof matching, and the manipulation of others. *Experimental Economics*, 20(3), 670–686. https://doi.org/10.1007/s10683-016-9505-0

Guillen, P., & Hakimov, R. (2018). The effectiveness of top-down advice in strategy-proof mechanisms: A field experiment. *European Economic Review*, *101*, 505–511. https://doi.org/10.1016/j.euroecorev.2017.10.020

Hakimov, R., & Kübler, D. (2021). Experiments on centralized school choice and college admissions: A survey. Experimental Economics, 24(2), 434–488. https://doi.org/10.1007/s10683-020-09667-7

Huls, S. P., Lancsar, E., Donkers, B., & Ride, J. (2022). Two for the price of one: If moving beyond traditional single‐best discrete choice experiments, should we use best‐worst, best‐best or ranking for preference elicitation? Health Economics, 31(12), 2630-2647. https://doi.org/10.1002/hec.4599

Kuvalekar, A. (2023). Matching with Incomplete Preferences (arXiv:2212.02613). *arXiv*. https://doi.org/10.48550/arXiv.2212.02613

Maida, M., Maier, K., & Obwegeser, N. (2012). Evaluation of techniques for structuring multi-criteria decision problems. CONF-IRM 2012 Proceedings. 20. https://aisel.aisnet.org/confirm2012/20

Ok, E. A. (2002). Utility Representation of an Incomplete Preference Relation. *Journal of Economic Theory*, *104*(2), 429–449. https://doi.org/10.1006/jeth.2001.2814

Pais, J., & Pintér, Á. (2008). School choice and information: An experimental study on matching mechanisms. *Games and Economic Behavior*, 64(1), 303–328. https://doi.org/10.1016/j.geb.2008.01.008

Rosenberger, R. S., Peterson, G. L., & Loomis, J. B. (2002). Applying a method of paired comparisons to measure economic values for multiple goods sets. *Journal of Agricultural and Applied Economics*, *34*(1), 215-229. https://doi.org/10.1017/S107407080000225X

Skedgel, C. D., Wailoo, A. J., & Akehurst, R. L. (2015). Choosing vs. allocating: discrete choice experiments and constant-sum paired comparisons for the elicitation of societal preferences. *Health Expectations*, 18(5), 1227-1240. https://doi.org/10.1111/hex.12098

Von Neumann, J., & Morgenstern, O. (1947). Theory of games and economic behavior (2nd rev. ed.). Princeton University Press.

Appendix A. Computational Method on Weakly Dominated Strategies

I use strictly truthful reporting (where the set of ordered lists are identical between true preferences and in reported preferences). The computational method doesn't account for indifference, because if a student is indifferent between all schools as the cyclical case in the experiment, truthful report is a weakly dominant strategy. And the method doesn't account for intransitive preferences (e.g., X>Y, Y>Z, cannot compare X and Z. Such cases are handled as X>Y>Z in the experiment). Each student has 19 unique preference list(s) from three comparisons between three schools.

The method works as follows:

1. Assume student a's true preference is [a1], and fix student b and student c's preferences at [b1][c1].

2. Compare the matching results of [a1][b1][c1] to [a2][b1][c1], [a3][b1][c1], ... [a19][b1][c1].

3. If matching [a2][b1][c1] assigns student a to a better school with higher probability, then mark [a1][b1][c1] as dominated by [a2][b1][c1].

4. Loop through all of students b and c's preference combinations from [b1][c1] to [b1][c2], ..., [b19][c19].

5. Repeat steps 1-4 for student a's true preferences [a2], ..., [a19].

sHere are two examples.

<u>Example 1: Using the DA mechanism</u>

School priorities:

X: acb

Y: bac

Z: cba

Student a's true and reported preference: a1: ZXY, a2: ZYX

Student b's reported preferences are b1:XYZ, b2: XZY, b3: ZXY

Student c's reported preference: XZY

The matching with a1 b1 c: (a-Z, b-Y, c-X), efficient regarding the reported preferences (no students can strictly improve their assignments by swapping schools)

The matching with a1 b2 c: (a-Z, b-Y, c-X), efficient

The matching with a1 b3 c: (a-X, b-Y, c-Z), inefficient, students a and c wat to swap schools

The matching with a2 b1 c: (a-Z, b-Y, c-X), efficient

The matching with a2 b2 c: (a-Y, b-Z, c-X), efficient

The matching with a2 b3 c: (a-Y, b-Z, c-X), efficient

The algorithm will randomly choose one from the two efficient matchings: (a-Z, b-Y, c-X) and (a-Y, b-Z, c-X).

The probability distribution for student a’s assignment is (Z: 0.5; Y:0.5).

Student a’s manipulation is a: ZXY

The matching with a1 b1 c: (a-Z, b-Y, c-X), efficient

The matching with a1 b2 c: (a-Z, b-Y, c-X), efficient

The matching with a1 b3 c: (a-X, b-Y, c-Z), inefficient, students a and c wat to swap schools

The algorithm will choose the efficien matching (a-Z, b-Y, c-X) as the final matching.

The probability distribution for student a’s assignment is (Z: 1).

(Z: 1) is better than (Z: 0.5; X:0.5) regarding a’s true preference.

As a result, manipulation dominates truthful reporting.

<u>Example 2: Using the TTC mechanism</u>

School priorities:

X: acb

Y: bac

Z: cba

Student a's true and reported preference: ZXY

Student b's reported preferences are b1:YZX, b2: ZYX

Student c's reported preference: YXZ

The matching with a b1 c: (a-Z, b-Y, c-X)

The matching with a b2 c: (a-X, b-Z, c-Y)

Both are justified envy free regarding the true preference.

The algorithm will randomly choose one of the two matchings.

The probability distribution for student a's assignment is (Z: 0.5; X:0.5).

Student a's manipulation is a1: YZX

The matching with a1 b1 c: (a-Z, b-Y, c-X),

The matching with a1 b2 c: (a-X, b-Z, c-Y), student a has justified envy on student c regarding the reported preference, therefore this assignment will not be chosen at the final matching.

The algorithm will choose the first matching as the final matching.

The probability distribution for student a's assignment is (Z: 1).

(Z: 1) is better than (Z: 0.5; X:0.5) regarding a's true preference.

As a result, manipulation dominates truthful reporting.

Appendix B. School Lottery Design

First set of parameters

Type 1 student

Round 1	Ball number	
	1--4	5--10
School X	$18 or $8	
School Y	$18	$8
School Z	$15	

Round 2	Ball number		
	1--4	5--7	8--10
School X	$15		
School Y	$15		$8
School Z	$18	$8	

Type 2 student

Round 1	Ball number	
	1--4	5--10
School X	$18	$8
School Y	$18 or $8	
School Z	$15	

Round 2	Ball number		
	1--4	5--7	8--10
School X	$18	$8	
School Y	$18 or $8		
School Z	$15		$8

Type 3 student

Round 1	Ball number		
	1--4	5--7	8--10
School X	$15		
School Y	$18	$8	
School Z	$15		$8

Round 2	Ball number	
	1--4	5--10
School X	$18 or $8	
School Y	$18	$8
School Z	$15	

Second set of parameters

Type 1 student

Round 1	Ball number	
	1--4	5--10
School X	$18 or $8	
School Y	$18	$8
School Z	$15	$10

Round 2	Ball number	
	1--4	5--10
School X	$15 or $10	
School Y	$15	$10
School Z	$18	$8

Type 2 student

Round 1	Ball number	
	1--4	5--10
School X	$18	$8
School Y	$18 or $8	
School Z	$15	$10

Round 2	Ball number	
	1--4	5--10
School X	$15	$10
School Y	$18 or $8	
School Z	$15 or $10	

Type 3 student

Round 1	Ball number	
	1--4	5--10
School X	$15 or $10	
School Y	$18	$8
School Z	$15	$10

Round 2	Ball number	
	1--4	5--10
School X	$15	$10
School Y	$18 or $8	
School Z	$15 or $10	

Appendix C. Experiment Instructions

Welcome

No Talking Allowed

This is an experiment of decision-making. You can make a considerable amount of money if you follow the instructions and make good decisions. We kindly ask you not to talk with other students. If you have any questions, please raise your hand and an experimenter will approach you and answer your questions in private.

Privacy

This experiment is structured so that no one, including the experimenters or the other participants, will ever know your personal decision. Your privacy is guaranteed because neither your name nor student ID number will be entered into the computer that records your decisions in the experiment. The only identifying information that will be used is the seat number.

Multiple Rounds

The experiment consists of two parts of questions, **Part 1 contains 2 rounds** decision tasks, and **Part 2 contains 20 rounds** decision tasks. **Each round is equally likely to be selected to determine your earnings at the end of the experiment**, so you should take each round seriously.

Cash Payoffs

You will get $5 base payment for participating, and additional earnings based on ONE randomly selected round. You will be paid in US dollars upon finishing the experiment.

Part 1 Decision Tasks (2 rounds)

What You'll Do

In each round, you'll be offered three school options (X, Y, and Z). Each option offers different chances to win money. You'll be asked to compare them by answering **three questions**.

How a School Option Pays

- For each school there is an container with 10 balls inside
- Each ball has a $ amount written on it
- ONE ball will be randomly drawn from the school's container
- The payoff is the $ amount on the drawn ball

An Example of School Options

<table>
<tr><th rowspan="2">School</th><th colspan="3">Ball number</th></tr>
<tr><th>1--4</th><th>5--7</th><th>8--10</th></tr>
<tr><td>X</td><td>$15</td><td colspan="2">$5</td></tr>
<tr><td>Y</td><td colspan="3">$15 or $5</td></tr>
<tr><td>Z</td><td colspan="2">$12</td><td>$5</td></tr>
</table>

- School X: 4 balls pay $15 each, 6 balls pay $5 each
- School Y: each ball pays $15 or $5, but the mix is not shown
- School Z: 7 balls pay $12 each, 3 balls pay $5 each

How You Get Paid

If you chose "is better", and this decision is randomly selected for payment:

- You will be paid based on the school you identified as better.
- Example: if the chosen answer is "Y is better", you will be paid based on school Y.

If you chose "cannot compare", and this decision is randomly selected for payment:

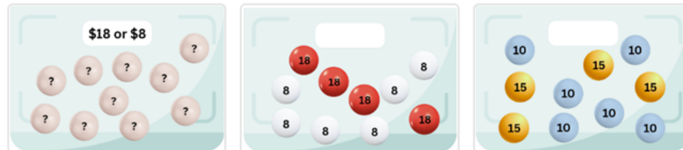
- **At the end of the experiment** you will be offered to select among participants who chose "is better" answers.
- Your will be paid based on the school picked by the participant you choose.

Figure B1. Decision page in Part 1

Part 1 Round 2

School	Ball number	
	1-4	5-10
X	$18 or $8	
Y	$18	$8
Z	$15	$10

School X School Y School Z



Your Decisions

Compare Schools X vs Y:
- ◯ X is better
- ◯ Y is better
- ◯ Cannot compare the two

Compare Schools X vs Z:
- ◯ X is better
- ◯ Z is better
- ◯ Cannot compare the two

Compare Schools Y vs Z:
- ◯ Y is better
- ◯ Z is better
- ◯ Cannot compare the two

Submit

Part 2: School Assignment (20 rounds)

What You'll Do

- In each round, you'll be asked the same three questions from Part 1: comparing pairs of schools (X, Y, and Z).

What's New

- Each school has a priority order for admitting students.
- You'll be randomly grouped with two other participants in each round.
- **Each school values differently to each group member.**
- Choices of all group members and schools' priority orders will be used to assign group members to schools.

How You Get Paid

If the randomly chosen round is from Part 2, your payment depends on **the school you are assigned to** in that round.

Details of School Assignment

A video will be played to explain how students are assigned to schools.

Example of School Assignment in treatment DA-P (same as the one in the video)

Schools' priority orders over students (unchanged in all rounds)

- School X: a-c-b
- School Y: b-a-c
- School Z: c-b-a

Step 1/4: Create Rankings (shown on your screen)

Assume students' rankings over schools are

Student	Rankings
a	Rank 1: Z-Y-X Rank 2: Z-X-Y
b	Z-X-Y
c	X-Z-Y

Step 2/4: Find an Assignment

Student	Rankings used
a	Rank 1: Z-Y-X
b	Z-X-Y
c	X-Z-Y

Each student's application is sent to their favorite school. Student a's application is sent to school Z, student b's application is sent to school Z, student c's application is sent to school X:

School X	School Y	School Z
c		~~a~~ b

School Z rejects student a because a has a lower priority than b. Student a's application is sent to school Y:

School X	School Y	School Z
c	a	~~a~~ b

Student a is assigned to school Y, student b is assigned to school Z, and student c is assigned to school X.

Step 3/4: Try All Rankings

Student	Rankings used
a	Rank 2: Z-X-Y
b	Z-X-Y
c	X-Z-Y

Student a's application is sent to school Z, student b's application is sent to school Z, student c's application is sent to school X:

School X	School Y	School Z
c		a b

School Z rejects student a because a has a lower priority than b. Student a's application is sent to school X:

School X	School Y	School Z
c a		~~a~~ b

School X rejects student c. Student c's application is sent to school Z:

School X	School Y	School Z
~~c~~ a		~~a~~ b c

School Z rejects student b. Student b's application is sent to school X:

School X	School Y	School Z
~~c~~ a b		~~a~~ ~~b~~ c

School X rejects student b. Student b's application is sent to school Y:

School X	School Y	School Z
~~c~~ a ~~b~~	b	~~a~~ ~~b~~ c

Student a is assigned to school X, student b is assigned to school Y, and student c is assigned to school Z.

Step 4/4: Select the Final Assignment

In **assignment 1**, no students want to switch their schools:

Student	Rankings used
a	Rank1: Z-Y-X
b	Z-X-Y
c	X-Z-Y

In **assignment 2**, student a and student c want to switch their schools:

Student	Rankings used
a	Rank 2: Z-X-Y
b	Z-X-Y
c	X-Z-Y

Assignment 1 is chosen as the final assignment.

Figure B2. Decision page in Part 2

Part 2 Round 1

You are Student a.

Schools' priority orders over students (click to collapse) -

Schools rank students from the highest priority to the lowest priority.

School X: a-c-b

School Y: b-a-c

School Z: c-b-a

Each group member's answers in Part 1 Round 1 (click to collapse) -

School pairs	Student a (You)	Student b	Student c
X vs Y	**X is better**	Cannot compare the two	Y is better
X vs Z	**Cannot compare the two**	Z is better	Z is better
Y vs Z	**Cannot compare the two**	Z is better	Z is better

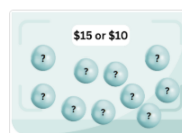
School Options

School	Ball number	
	1-4	5-10
X	$15	$10
Y	$18 or $8	
Z	$15 or $10	

School X

School Y

School Z



Your Decisions

The ranking(s) will automatically update on the right side as you make your choices.

Compare Schools X vs Y:
- X is better
- Y is better
- Cannot compare the two

Compare Schools X vs Z:
- X is better
- Z is better
- Cannot compare the two

Compare Schools Y vs Z:
- Y is better
- Z is better
- Cannot compare the two

Ranking(s) used in the assignment (favorite - 2nd favorite - 3rd favorite):

? - ? - ?

Example of School Assignment treatment TTC-P (same as the one in the video)

Schools' priority orders over students (unchanged in all rounds)

- School X: a-c-b
- School Y: b-a-c
- School Z: c-b-a

Step 1/4: Create Rankings (shown on your screen)

Assume students' rankings over schools are

Student	Rankings
a	Rank1: Y-Z-X Rank 2: Z-Y-X
b	Z-X-Y
c	X-Y-Z

Step 2/4: Find an Assignment

Student	Rankings used
a	Rank1: Y-Z-X
b	Z-X-Y
c	X-Y-Z

Each student points to their favorite school, and each school points to their highest-priority student to form cycles.

Student a → School Y → Student b → School Z → Student c → School X

Here we find a cycle. Each student in the cycle is assigned to the school they're pointing to.

Student a is assigned to School Y, Student b is assigned to School Z, and Student c is assigned to School X.

Step 3/4: Try All Rankings

Student	Rankings used
a	Rank 2: Z-Y-X
b	Z-X-Y
c	X-Y-Z

Student b → School Z → Student c → School X → Student a

Here we find a cycle. Student a is assigned to School Z, Student c is assigned to School X.

Student b is unassigned and points to the next available school, Y.

Student b → School Y

Here we find a cycle. Student b is assigned to School Y.

Step 4/4: Select the Final Assignment

In **assignment 1**, each student is assigned to their favorite school and none of them prefers any other school (the table below).

Student	Rankings used
a	Rank1: Y-Z-X
b	Z-X-Y
c	X-Y-Z

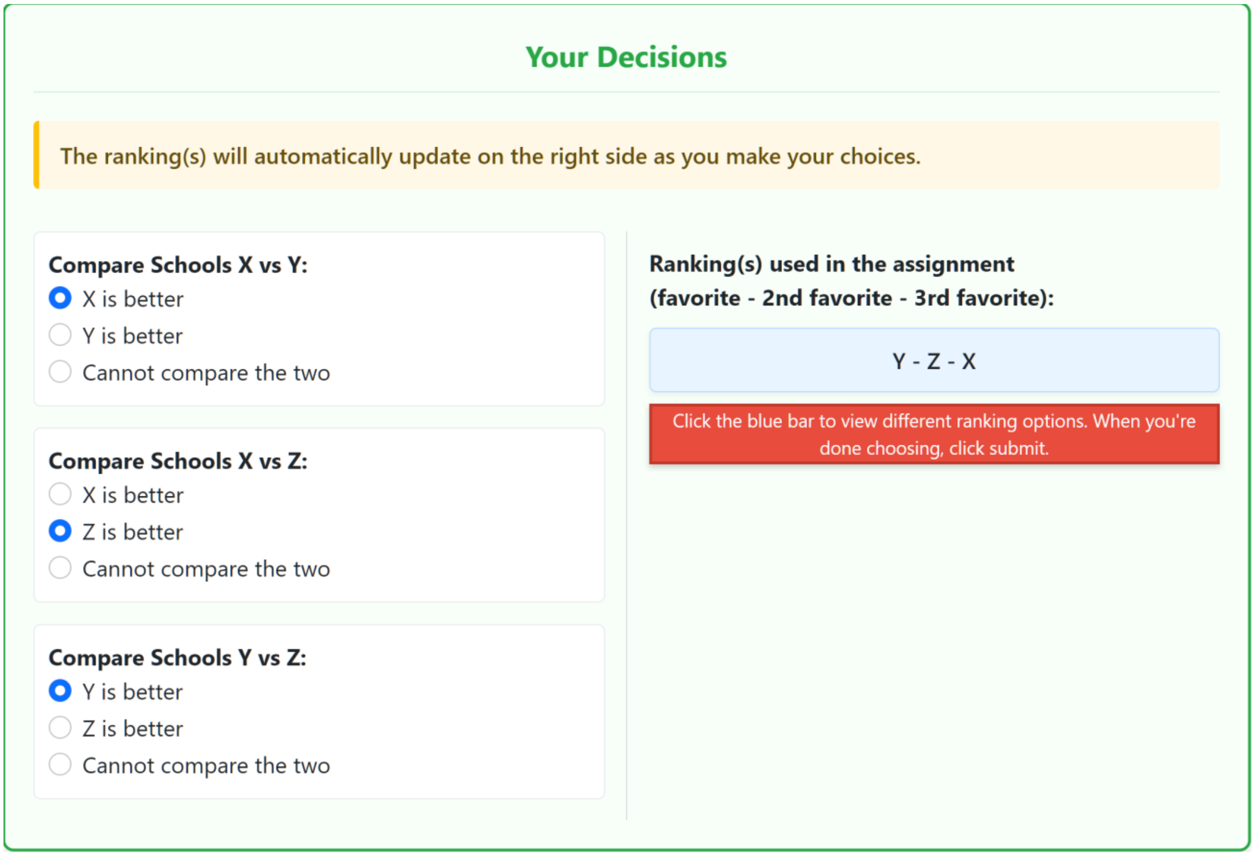
In **assignment 2**, student b prefers School Z than the current assignment, and school Z prefers student b than the current student (as circled below).

Student	Rankings used
a	Rank 2: Z-Y-X
b	Z-X-Y
c	X-Y-Z

- School X: a-c-b
- School Y: b-a-c
- School Z: c-b-a

Assignment 1 is chosen as the final assignment because there doesn't exist a student and a school pair which prefers each other than their current assignment.

Figure B3. Decision page for cyclical preference



Appendix D. Proofs

Proposition 1: Every one of the C matchings in the extended DA mechanism is stable.

Proof: Assume that matching μ is one of the C matchings and it's unstable with student i and school s forming a blocking pair, in which $s \succ_i \mu(i)$. Since $s \succ_i \mu(i)$ holds for all ordered lists extended from P_i, it holds for the ordered lists used to generate matching μ. This contradicts to the fact that each matching μ is stable regarding the ordered lists used to generate it (because the standard DA matching is stable). Therefore there doesn't exist an unstable matching μ.

Proposition 1: Every one of the C matchings in the extended TTC mechanism is efficient.

Proof: Assume that matching μ is one of the C matchings and it's inefficient, in which there exists two students i and j such that $\mu(j) \succ_i \mu(i)$ and $\mu(i) \succ_j \mu(j)$. Since $\mu(j) \succ_i \mu(i)$ and $\mu(i) \succ_j \mu(j)$ hold for all ordered lists extended from P_i, they hold for the ordered lists used to generate matching μ. This contradicts to the fact that each matching μ is efficient regarding the ordered lists used to generate it (because the standard TTC matching is efficient). Therefore any matching μ is efficient.

Appendix E. All extension scenarios.

Scenario 3: If subjects' choices are: "X is better than Y", "Cannot compare X and Z", and "Cannot compare Y and Z", then the set of ordered lists consistent with their choices is $(X \succ Y \succ Z), (X \succ Z \succ Y),$ and $(Z \succ X \succ Y)$.

Scenario 4: If subjects' choices are: "Cannot compare X and Y", "Cannot compare X and Z", and "Cannot compare Y and Z", then the set of ordered lists consistent with their choices is $(X \succ Y \succ Z), (X \succ Z \succ Y), (Z \succ X \succ Y), (Z \succ Y \succ X), (Y \succ Z \succ X)$, and $(Y \succ X \succ Z)$.

Scenario 5: If subjects' choices are "X is better than Y", "Z is better than X", and "Y is better than Z", the student is asked to choose one ordered list from $(X \succ Y \succ Z), (X \succ Z \succ Y), (Z \succ X \succ Y), (Z \succ Y \succ X), (Y \succ Z \succ X), (Y \succ X \succ Z)$ and submit (FigureB3).

Scenario 6: If subjects' choices are "X is better than Y", and "Y is better than Z", "Cannot compare X and Z", the set of ordered list consistent with their choices is $(X \succ Y \succ Z)$.

Appendix F. Robustness check with identical preference reporting

AF.1. Identical (Truthful) Preference reporting rate

The identical preference reporting rate is calculated as the ratio of identical preference reports to total reports. I construct this variable by comparing subjects' responses across Part 1 and Part 2 of the experiment. In Part 1, subjects' answers to the school comparison questions establish their baseline preferences. A subject is classified as engaging in identical preference reporting if they provide the same answers in Part 2 as in Part 1. Any deviation between Part 1 and Part 2 responses is categorized as non-identical reporting.

The overall identical preference reporting rate is around 30% percent. Figure AF.1 displays the identical reporting rate by rounds. In the full sample, the DA mechanism elicits more identical preferences (41.8 %) compared to the TTC mechanism (33.4%). This aggregate difference is primarily driven by subjects with complete preferences. In both mechanisms, about 20% of subjects with incomplete preferences report identical preferences. More statistics can be found in Table AF.1.

Table AF.2 presents random-effects logistic regression coefficients across different model specifications. In the full interaction model (column 2), the coefficient on the DA mechanism β_1 is 2.379 (p = 0.103), indicating that, holding preference type constant at complete preferences, subjects in the DA mechanism have 2.38 times the odds of reporting identical preferences compared to those in the TTC mechanism. The coefficient on incomplete preferences γ_1 is 0.345 (p = 0.126), showing that within the TTC mechanism, students with incomplete preferences have much lower odds of identical preference reporting compared to students with complete preferences. The interaction term δ_1 is 0.095 (p = 0.023), suggesting that the effect of incomplete preferences on identical reporting is even stronger under the DA mechanism. This pattern likely reflects the greater instability of incomplete preferences relative to complete ones. These results support the patterns in Figure AF.1 that the DA mechanism elicits a lower consistent preference reporting rate and subjects with incomplete preferences.

Figure AF.1. Truthful Reporting Rate across Rounds

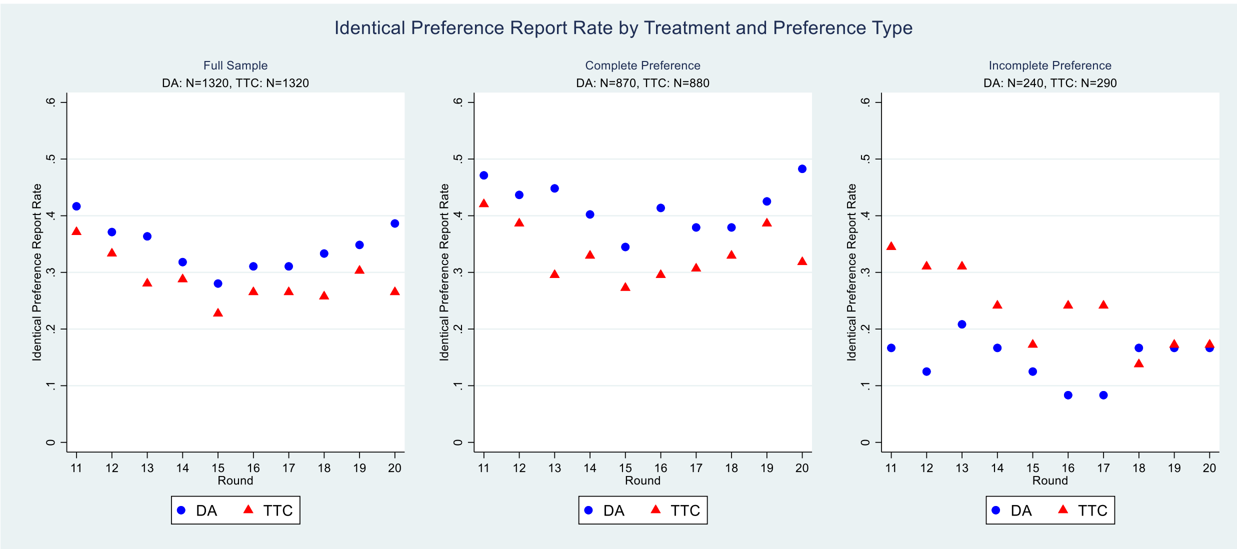


Table AF.1. Summary Statistics (Rounds 11-20)

		Truthful report rate
Complete preference	DA	0.4184 (0.3987)
	N	87
	TTC	0.3341 (0.3664)
	N	88
Incomplete preference	DA	0.1458 (0.2553)
	N	24
	TTC	0.2345 (0.3177)
	N	29
Other preference	DA	0.2619 (0.3584)
	N	21

(cyclical or intransitive)	TTC	0.1000 (0.2591)
	N	15
All	DA	0.3439 (0.3839)
	N	132
	TTC	0.2856 (0.3521)
	N	132

Notes: Standard errors in parentheses. Table 7 shows the summary statistics of the full sample in rounds 11-20 in Part 2.

Table AF.2. Random logistic model on identical preference report

	Identical preference reporting rate	
	(1)	(2)
DA	1.846 (0.779)	2.379 (1.266)
Preference type		
Incomplete	0.121*** (0.065)	0.345 (0.240)
Other	0.095*** (0.061)	0.033*** (0.034)
DA × *Incomplete*		0.095** (0.098)
DA × *Other*		5.088 (6.750)
N	2640	2640

Note: *** p<.01, ** p<.05, * p<.1. Table 3 reports whether a subject reports consistent preferences. The coefficients in the table are odds ratios, with standard errors in parentheses. Standard errors are clustered at the participant level.

Results AF.1a: Among students with complete preferences, the DA mechanism generates higher identical reporting rates than the TTC mechanism. However, this pattern is reversed for students with incomplete preferences.

Results AF.1b: Students with incomplete preferences are less likely to report identical preferences than those with complete preferences.

Compared to the consistent preference reporting results, the overall identical preference reporting rate is lower, since identical reporting is a subset of consistent reporting. The decline is more pronounced among students with incomplete preferences than among those with complete preferences.

AF.2. Efficiency

AF.2.1. Exchange-proof assignment

Efficiency is measured using two approaches. The first measurement is the number of students who are assigned to exchange-proof schools in each group. A random-effects ordered logit model (Equation 2) examines the patterns, where the dependent variable is the number of students who are assigned to an exchange-proof school in group g round t, DA is a binary variable on mechanism, $CtId$ is a categorical variable indicating number of students who report identical preferences, $CtIncPref$ is a categorical variable indicating number of students who have incomplete preferences, and $CtOtherPref$ controls the number of students who have other types of preferences. $1\{\cdot\}$ is an indicator variable, equal to one when the condition in the bracket is satisfied, 0 otherwise. α_g is the unobserved group-specific heterogeneity, and ε_{gt} is the idiosyncratic error term.

$$Y_{gt} = \beta + \beta_1 DA + \sum_{a=1,2,3} \gamma_c 1\{CtId_{gt} = a\} + \sum_{b=1,2} \gamma_c 1\{CtIncPref_{gt} = b\} + \sum_{c=1,2} \gamma_c 1\{CtOtherPref_{gt} = c\} + \alpha_g + \varepsilon_{gt} \quad (2)$$

Table AF.3. Ordered logistic model on the count of exchange-proof assignments

	Count of exchange-proof assignments
DA	1.285 (0.220)
Count identical reporting	
1	1.629*** (0.262)
2 or more	3.842*** (0.975)
Count incomplete preferences	
1	1.963** (0.329)
2 or more	3.926** (1.661)
Count other preferences	
1	0.653*** (0.106)
2 or more	0.820 (0.198)
N	880

Note: *** p<.01, ** p<.05, * p<.1. Table 4 reports the number of students who are assigned to exchange-proof schools in each group in each round. The coefficients in the table are odds ratios, with standard errors in parentheses. Standard errors are clustered at the participant level.

The ordered logistic regression results in Table AF.3 show how group composition affects exchange-proof assignment outcomes. Identical preference reporting exhibits a strong dose-response relationship with exchange-proof assignments: having one identical preference reporter increases the odds by 62.9% ($p < 0.01$) relative to having none, and having two or more identical preference reporters raises the odds to 3.84 times the baseline ($p < 0.01$). Groups with more students holding incomplete preferences achieve better exchange-proof outcomes due to the incomparable nature of rankings (OR = 1.963 when one student has incomplete preferences, $p < 0.01$, and OR = 3.926 when two or more students have incomplete preferences, $p < 0.01$). The mechanism type (DA vs. TTC) does not significantly affect exchange-proof assignments once group composition is controlled for (OR = 1.285, $p = 0.143$), though the point estimate suggests DA may slightly increase the odds relative to TTC.

Result AF.2a: Identical reporting shows a strong dose-response effect in increasing group-level exchange-proof assignments.

Result AF.2b: A higher number of students with incomplete preferences improves the likelihood of exchange-proof assignments.

Result AF.2c: Mechanism choice does not significantly affect exchange-proof assignments.

These results are very similar to those obtained using the consistent preference measure.

AF.2.2. Non-dominated assignment: Complete-Preference Subsample Analysis

The other measure examines whether a student is assigned to a non-dominated school.

First, I restrict the sample to students with complete preferences and examine how their outcomes vary by group composition. I compare students in all-complete groups (where all group members have complete preferences) with those in mixed groups (at least one other group member has incomplete preferences).

A random effect logistic model is estimated (model 2) to examine the factors affecting non-dominated assignments. The dependent variable is whether subject i is assigned to an non-dominated school in round t. DA is a binary variable of mechanism ($=1$ for DA, $=0$ for TTC). Id_{it} is a binary variable on whether subject i reported identical preferences in Part 2 round t as in Part 1. $CtOtherId_{gt}$ counts the number of other group members who report identical preferences, and $MixedGroup_{gt}$ is whether all group members have complete preferences ($=1$ when at least one other group member has incomplete preferences , $=0$ when all group members have complete preferences).

$$Y_{it} = \beta_0 + \beta_1 DA + \beta_2 Id_{it} + \beta_3 CtOtherId_{gt} + \beta_4 MixedGroup_{gt} + \alpha_i + \varepsilon_{it} \quad (2)$$

Table AF.4. logistic model on efficiency measurements (complete-preference subsample)

	Non-dominated (1)	Envy-free (2)
DA	1.204 (0.374)	1.760 (0.640)
Identical report	5.394*** (1.452)	8.526*** (3.542)
Count other identical report	1.100 (0.155)	0.911 (0.166)
Mixed group	1.165 (0.181)	1.311 (0.268)
N	1750	1750

Note: *** p<.01, ** p<.05, * p<.1. Table 5 restricts the samples to students with complete preferences. The coefficients in the table are odds ratios, with standard errors in parenthesis. Standard errors are clustered at the participant level. Column 1 reports whether a subject is assigned to a non-dominated school, and column 2 reports whether a subject is envy-free.

Table AF.4 shows a non-significant 20.4% increase in the odds of non-dominated assignment under DA relative to TTC (OR = 1.204, p = 0.550). Individual identical preference reporting shows a strong association with outcomes, with students who report identical preferences 5.39 times the odds of non-dominated assignment (OR = 5.394, p < 0.01). The number of other identical preference reporters in the group does not significantly affect individual outcomes (OR = 1.100, p > 0.502), suggesting that non-dominance is an individual property rather than a group-level phenomenon. Similarly, being in a mixed group with both complete and incomplete preference holders has no significant effect on non-dominated assignments (OR = 1.165, p > 0.181).

Results AF.2d: Identical preference reporting significantly increases the likelihood of being assigned to a non-dominated school for complete-preference subjects.

Results AF.2e: Mechanism choice, other group members' preference types, and other group members' identical reporting behavior have positive yet insignificant effects on non-dominated assignments of complete-preference subjects.

These results are very similar to those obtained using the consistent preference measure.

AF.2.3. Full Sample Analysis

Table AF.5 presents random-effects logistic regression results for the full sample (N = 2,640). The dependent variable in columns 1-3 is whether a student receives an exchange-proof assignment, while columns 4-6 examine non-dominated assignments. The model (Equation 3) includes preference type indicators, with complete preferences as the baseline category. $Pref_{i1} = 1$ if an individual has incomplete preference, and $Pref_{i2} = 1$ if an individual has other type of preference[22]. α_i is the unobserved subject-specific heterogeneity, and ε_{it} is the idiosyncratic error term.

$$\begin{aligned} Y_{it} &= \beta_0 + \beta_1 DA + \beta_2 Id_{it} + \beta_3 CtOtherId_{gt} + \beta_4 CtOtherIncomp_{gt} \\ &+ \gamma_1 Pref_{i1} + \gamma_2 Pref_{i2} + \delta_1 DA * Pref_{i1} + \delta_2 DA * Pref_{i2} + \alpha_i + \varepsilon_{it} \end{aligned} \quad (3)$$

The DA mechanism slightly increases the likelihood of non-dominated assignments, with students under DA having 21.6% higher odds (OR = 1.216, p = 0.560) compared to TTC (Table AF.5). This effect of the DA mechanism is similar in magnitude to what we observed in the complete-preference subsample (OR = 1.204).

Students who report identical preferences have 5.7 times the odds of a non-dominated assignment (OR = 5.653, p < 0.01). The strong association between identical preference

[22] The incomplete preference category differs from both indifferent and intransitive categories. Incomplete preferences preserve transitivity within their strict preference components, while intransitive preferences violate transitivity in these components. Indifference comes from the cyclical choices between three schools. These distinctions prevent pooling incomplete preferences with indifferent or intransitive preferences.

reporting and both exchange-proof and non-dominated assignments suggests that most students benefit from honest preference revelation, aligning with the results in the computational method that both mechanisms are strategyproof in more than 90% scenarios.

Students with incomplete preferences achieve significantly higher non-dominated rates than those with complete preferences (OR = 11.622, $p < 0.01$). However, as the subsample analysis reveals, this likely reflects subjects with incomplete preferences having fewer schools in their choice set, mechanically reducing dominated alternatives. The interaction terms (DA × Incomplete) are not significant (OR = 0.714, $p = 0.613$), indicating no mechanism advantage for subjects with incomplete preferences.

Similar to the subsample results, the number of peers with incomplete preferences does not significantly affect individual welfare outcomes (OR = 1.112, $p = 0.149$). Likewise, the count of peers reporting identical preferences shows a non-significant positive effect, increasing the odds of non-dominated assignment by 8.2% (OR = 1.082, $p = 0.137$).

Table AF.5. Logistic model on efficiency measurements (full sample)

	Non-dominated assignments	Envy-free assignments
	(1)	(2)
DA	1.216 (0.407)	1.810 (0.672)
Identical report	5.653*** (1.517)	8.442*** (2.966)
Preference type		
Incomplete	11.622*** (6.873)	4.760** (3.139)
DA × Preference type		
DA × *Incomplete*	0.714 (0.613)	1.048 (1.033)
Count other identical report	1.082 (0.137)	0.809 (0.121)
Count other incomplete preferences	1.112 (0.149)	1.175 (0.180)
N	2640	2640

Note: *** p<.01, ** p<.05, * p<.1. The coefficients in the table are odds ratios, with standard errors in parentheses. Standard errors are clustered at the participant level. The dependent variable in column 1 is whether a subject is assigned to a non-dominated school; the dependent variable in column 2 is whether a subject is assigned to an envy-free school.

Results AF.2f: Identical preference reporting significantly increases the likelihood of being assigned to a non-dominated school, while mechanism, other group members' preference types and reporting behavior have positive yet insignificant effects on non-dominated assignments.

These results are very similar to those obtained using the consistent preference measure.

AF.3. Justified Envy

AF.3.1. Complete-Preference Subsample Analysis

Table AF.4 presents logistic regression estimates for envy-free assignments. Individual identical preference reporting has the strongest association with fair matchings, with students who report truthfully having 8.53 times the odds of envy-free assignment compared to those who do not (OR = 8.526, $p < 0.01$). The DA mechanism shows 76% higher odds than TTC (OR = 1.760, $p = 0.120$), though this difference is not statistically significant. Neither peers' consistent reporting (OR = 0.911, $p = 0.608$) nor mixed group composition (OR = 1.311, $p = 0.185$) significantly affects envy-free assignments among complete-preference students.

Extending to the full sample (Table AF.5), the main findings persist across all preference types. Individual identical preference reporting remains the dominant factor (OR = 8.442, $p < 0.01$). DA is 81.0% more likely to assign a student to an envy-free school (OR = 1.810, $p = 0.110$), though not statistically significant. Students with incomplete preferences achieve higher envy-free rates than those with complete preferences (OR = 4.760, $p = 0.018$), likely because their inability to compare certain schools reduces potential justified envy. As in the subsample, group composition shows no significant effects: neither peers' consistent reporting (OR = 0.809, $p = 0.157$) nor the presence of incomplete-preference peers (OR = 1.175, $p = 0.292$) influences individual envy-free outcomes.

Result AF.3a: Individual consistent reporting is the strongest predictor of envy-free assignments.

Result AF.3b: DA shows modestly higher odds of envy-free assignments than TTC.

Result AF.3c: Students with incomplete preferences achieve higher envy-free rates.

Result AF.3d: Being in a mixed group can marginally improve the odds of envy-free assignments.

These results are very similar to those obtained using the consistent preference measure.