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Abstract

Preferences are central to matching markets, yet experiments typically rely on induced preferences that may not reflect real-world decision-making. We examine how induced versus non-induced preferences shape behavior in matching experiments, extending Chen & Sönmez (2006). Using the most frequently used school choice mechanisms (Boston, Deferred Acceptance, and Top Trading Cycles), we supplement monetary incentives with participants' own preferences. Our results show that preference induction systematically affects truthful reporting and comprehension of mechanisms. These findings underscore that experimental design choices matter for the validity of behavioral insights and have direct implications for policy evaluation.

Keywords: Non-induced Preferences, Experiments, Matching, School Choice

JEL: C78, D47

1 Introduction

Economists have shifted their focus in recent decades from price-regulated markets to allocation problems without monetary transfers, such as school choice, college admissions, kidney exchanges, social housing, and neighborhood help (Roth, 1984; Roth & Rothblum, 1999; Crawford & Knoer, 1981; Roth et al., 2004; Abdulkadiroğlu & Sönmez, 2003). Matching theory provides formal mechanisms for these settings and experimental methods offer valuable insights into how individuals behave under such mechanisms. These experiments assess participants' understanding, strategic manipulation, and whether theoretical properties—such as efficiency and stability—are preserved in practice[1].

All of these matching settings share a fundamental feature: allocations are determined by preferences or priorities rather than prices. In experiments, participants' payoffs are typically derived from their outcomes and reported preferences. As standard models rely on preferences, experimentalists often induce them to simulate realistic scenarios, such as school choice or housing. Surprisingly, whether induced preferences affect participants' choices and understanding differently from non-induced (i.e., authentic) ones remains largely unexplored, despite preferences being central to allocation decisions.

In this study, we investigate how the use of non-induced preferences influences participants' behavior in matching experiments, compared to the commonly used induced preferences. Building on the seminal work of Chen & Sönmez (2006), who examined strategic behavior in school choice using induced preferences to generate payoffs, we extend their framework by introducing a setting with non-induced preferences: Participants are matched to departments within a chocolate company, each associated with a distinct chocolate variant. Payoffs are based on a monetary reward and a chocolate bar from the assigned department. To elicit genuine preferences, participants were asked to make pairwise comparisons of chocolate types. A control group receives induced preferences, allowing for a direct comparison. Our central

[1] For a survey of experimental research on school choice and college admissions, see Hakimov & Kübler (2021)

contribution lies in assessing whether and how induced versus non-induced preferences affect behavior under different matching mechanisms — particularly with respect to participants' truth-telling, strategic consistency, and outcome efficiency. To ensure comparability with existing results, we implement the same three mechanisms studied by Chen & Sönmez (2006): the Boston mechanism (BOS) (Abdulkadiroğlu & Sönmez, 2003), the Gale–Shapley deferred acceptance mechanism (DA) (Gale & Shapley, 1962), and the Top Trading Cycles mechanism (TTC) which was introduced by Shapley & Scarf (1974) and adopted to school choice problems by Abdulkadiroğlu & Sönmez (2003). These were chosen for their contrasting theoretical properties — most notably with respect to strategy-proofness, efficiency, and stability — and their relevance in real-world school choice implementations. DA is strategy-proof for students and guarantees a stable matching, though it does not always yield Pareto efficient outcomes (Roth, 1984). In contrast, TTC ensures efficiency and is also strategy-proof, but it may produce unstable matchings (Ma, 1994). BOS, while simple to understand and commonly used in practice, is neither strategy-proof nor stable, and its efficiency depends on truthful reporting—which is not incentive-compatible (Roth, 2008; Azevedo & Budish, 2019). Thus, DA and TTC offer stronger theoretical guarantees, while BOS trades off those guarantees for simplicity. For a detailed comparison on school choice markets of these mechanisms, see (Abdulkadiroğlu & Sönmez, 2003; Roth & Sotomayor, 1990). Lab experiments support the theoretical results that the BOS leads to inferior outcomes. For a detailed overview see Hakimov & Kübler (2021).

Our findings indicate that participants with non-induced preferences behave more rationally and are more likely to report their true preferences when it is a dominant strategy. By this, the efficiency loss of not reporting true preferences is significantly higher in the control group compared to the treatment group. This is primarily driven by DA.

Following Chen & Sönmez (2006), a growing body of experimental research has examined school choice mechanisms, focusing on participants' truth-telling behavior, outcome efficiency, and the effects of varying information and transparency (Hakimov & Kübler, 2021; Guillen & Hakimov, 2017; Koutout et al., 2021; Chen et al., 2016). Some studies simulate real-world dynamics by incorporating advice interventions, such as peer-to-peer or policy-based guidance, which elicit heterogeneous responses. However, Guillen & Veszteg (2021) note that truthful reporting may reflect confusion rather than rational strategy. Participants' understanding can also shape decision-making, as shown by Bó & Hakimov (2020) and Klijn et al. (2019), who study how an iterative version of the DA algorithm affects behavior. Furthermore, there is evidence that participants are less likely to tell the truth as the number of schools increases (Chen & Kesten, 2019; Hakimov & Kübler, 2021). This already holds for relatively small variations in the number of schools. Chen & Kesten (2019) show that truth-telling rates were significantly lower in a scenario with six schools compared to a scenario with four schools. Overall, studies following Chen & Sönmez (2006) suggest that even subtle changes in experimental design can influence behavior, complicating replication. For instance, truth-telling may decrease as more information is provided (Guillen & Hakimov, 2017)[2]. Chen & Sönmez (2006) include a district school in their experimental design to reflect real-world school choice, where students typically receive higher priority at their local school. This feature, however, may encourage strategic misrepresentation, as participants might list the district school higher as a safer choice. Indeed, nearly two-thirds of misrepresentations in their study align with the district school bias. Supporting this, Pais & Pintér (2008) report that 80.2% of misreports in their experiment were similarly influenced. To avoid this confound, we exclude district schools from our design, allowing us to isolate the effects of preference induction and mechanism choice. A detailed description of the experimental design is provided in Section 3. Motivated by the considerations presented above, we focus on control groups commonly used in such experiments to investigate whether induced preferences systematically influence behavior. Laboratory settings are particularly advantageous, as both true and reported preferences can be observed—unlike in most field studies. Only a few field experiments have been able to elicit participants' true preferences (Guillen & Hakimov, 2018; He & Magnac, 2018; Hoyer & Stroh-Maraun, 2020). To our knowledge, we are the first to incorporate non-induced preferences into a controlled laboratory experiment, enabling a direct

[2] Also shown in other experiments surveyed by Hakimov & Kübler (2021).

assessment of how preference induction affects behavior, particularly truth-telling. Furthermore, if deviations from true preferences reduce efficiency—as suggested by Chen & Sönmez (2006)—we examine whether efficiency differs between treatments.

The paper proceeds as follows: We start our further analysis in section 2 with the model that our experiment is based on. We introduce the experimental design in section 3 as well as an explanation of the used mechanisms. The theoretical predictions as well as our hypotheses are stated. Afterwards, we present our results in section 4 and conclude in section 5.

2 School choice problems

Our experiment builds on the school choice framework introduced by Abdulkadiroğlu & Sönmez (2003), who model the assignment of students to seats at schools. We rephrase the model by using interns and departments instead of students and schools, respectively. After outlining the basic model, we review key properties and mechanisms that guide our analysis.

2.1 Model

Following Abdulkadiroğlu & Sönmez (2003) and Abdulkadiroğlu & Andersson (2023), a department choice problem consists of two disjoint, finite sets of interns $I = \{i_1, \ldots, i_n\}$ and departments $S = \{s_1, \ldots, s_m\}$, where each department $s \in S$ has a quota q_s. Each intern $i \in I$ has strict preferences P_i over $S \cup \{i\}$, where i denotes the possibility of remaining unmatched. Interns' preferences are summarized as $P = (P_{i_1}, \ldots, P_{i_n})$. Similarly, each department $s \in S$ has a strict priority ordering $\succ_s$ over $I \cup \{s\}$, where s denotes the option of leaving a seat empty. Let $\succ = (\succ_{s_1}, \ldots, \succ_{s_m})$ summarize departments' priorities.

The distinction between preferences and prorities plays an important role here. In contrast to two-sided matching markets like the market of college admissions (Gale & Shapley, 1962), departments do not act strategically. Thus, instead of expressing preferences over interns, their rankings or priorities are based on objective criteria. Therefore, while our market is one-sided and focuses on the interns' interests, it also features some characteristics of a two-sided market. Unlike in a housing markets (Shapley & Scarf, 1974), where houses are objects that do not care whose moving in, the departments in our setting actually express some sort of ranking.

A department choice problem is denoted $M = (I, S, P, \succ)$. A matching μ assigns each intern to a department and each department to no more interns than its capacities. Thus, if an intern is assigned to a department, then this department is also assigned to this intern and vice versa. $\mu(i)$ refers to the assignment of intern i under matching μ.

Definition 1. *More formally, a department choice matching is a mapping $\mu : I \rightarrow S \cup I$ such that*
$\mu(s) \subseteq I$ *for all* $s \in S$ *and* $|\mu(s)| \leq q_s$,
$\mu(i) \in S \cup \{i\}$ *for all* $i \in I$,
$\mu(i) = s \Leftrightarrow i \in \mu(s)$.

A particular matching in such a market may exhibit different properties. Because the department choice market features characteristics of one- and two-sided matching markets, properties commonly studied in both one-sided and two-sided markets may not hold. Moreover, the mechanisms used in our experimental design inherit different properties, which will be important later for comparing mechanisms and analyzing participants' behavior. Let us begin by considering the interns' side only. The objective is to improve an intern's allocation without worsening any other intern's match. Abdulkadiroğlu et al. (2020) define **Pareto efficiency** as follows: A matching μ Pareto dominates ν if $\mu(i) R_i \nu(i)$ for all $i \in I$ and $\mu(i) P_i \nu(i)$ for some $i \in I$. A matching is Pareto efficient if it is not Pareto dominated by any other. In other words, a matching is Pareto efficient if it is not possible to improve an intern's allocation without worsening another intern's situation.

If not only interns' preferences are considered but also departments' priorities, we may focus on stability and the absence of justified envy by ensuring that there is no intern-department pair that would block the allocation. Based on the work by Gale & Shapley (1962), (Abdulkadiroğlu & Sönmez, 2003) define a matching to be **stable** if no intern-department pair (i, s) exists such that $sP_i\mu(i)$ and $i \succ_s j$ for some $j \in \mu(s)$. A stable matching eliminates justified envy meaning that no intern prefers a department they are not matched to over their current match while the department also gives higher priority to this intern compared to at least one who is currently matched to it.

To find a matching, we may apply different mechanisms. To ensure that the resulting matching actually fulfills a certain property, truth-telling plays a crucial role. We define the department choice problem as a preference revelation game induced by a particular mechanism φ. A mechanism selects a matching for each economy, and the matching selected by φ is denoted $\varphi(P, \succ)$. Since our experiments aim to determine whether truth-telling differs between treatment groups, it is important to establish whether telling the truth is a dominant strategy under the different mechanisms. Thus, by (Abdulkadiroğlu et al., 2020), a mechanism φ is **strategy-proof** if truthful reporting is a dominant strategy for all $i \in I$, i.e., $\varphi(P, \succ)(i) \, R_i \, \varphi(P'_i, P_{-i}, \succ)(i)$.

2.2 Mechanisms

Our experiment focuses on the three most widely used direct matching mechanisms: BOS, DA and TTC. We adapted the descriptions from Chen & Sönmez (2006) to fit the intern–department matching problem. We will present here the mechanisms as explained in the experimental instructions and look at their properties. These properties give us a theoretical prediction of what behavior to expect from participants theoretically. The first two steps are the same for each mechanism[3]:

- Each intern submits a preference ranking over the departments.
- A priority ordering is determined randomly for all departments. It is the same ordering for each department.

Afterwards an assignment is established based on one of the following procedures:

[3] Examples of the mechanisms can be found in the instructions of the experiment in the appendix and in the experimental instructions by Chen & Sönmez (2006).

The Boston school choice mechanism (BOS)

Round 1: Each intern applies to their first choice. Each department accepts interns according to their priorities until all seats are filled or all interns have been accepted. Accepted interns are assigned to the given department and removed from the market. Remaining interns for the respective department are rejected.

Round 2: Rejected interns will be sent to the respective second most preferred department. If a department still has vacancies from the first round, it will accept interns with a higher priority until all vacancies are filled. The remaining interns will be rejected.

In general at

Round k: Rejected interns will be sent to the respective k^{th} most preferred department. If a department still has vacancies from the previous rounds, it will accept interns with a higher priority until all vacancies are filled. The remaining interns will be rejected. This procedure continues until either there are no seats left or there is no intern left who has listed it as their k^{th} choice.

The Deferred Acceptance mechanism (DA) - intern optimal

Round 1: Each intern applies to their first choice. Throughout the allocation process, a department can hold no more interns than its number of available seats. If a department receives more applications than its capacity, it rejects the interns with lowest priority. The remaining interns are retained provisionally.

In general at

Round k: Whenever an intern is rejected at a department, their application is sent to the next highest ranked department according to the preferences submitted. Whenever a department receives new applications, these interns are considered together with the provisionally retained interns for that department. Among the retained and new interns, the lowest priority ones in excess of the number of the seats available are rejected, while remaining interns are provisionally retained.

The allocation is finalized when no more interns can be rejected. Each intern is assigned a seat at the department that has provisionally retained their application at the end of the process.

The DA with one common priority list for all departments is a special case of the original DA by Gale & Shapley (1962).

The Top Trading Cycles mechanism (TTC)

We use a tentative assignment that is drawn randomly for the TTC. This random draw is independent from the draw done as described as a first step. In addition to adhering closely to Chen & Sönmez (2006), using a tentative assignment alters the allocation compared to the outcome produced by the DA. Otherwise, DA, Serial Dictatorship (SD), and TTC result in the same outcome due to a common priority list (Morrill & Roth, 2024; De Haan et al., 2023). Thus, the TTC is applied in the following way in our experiment.

The goal of the allocation process is to determine mutually beneficial exchanges between two or more applicants. The order in which these exchanges are considered is determined by the fair lottery stated before, i.e., the departments' priorities. Each intern is first tentatively assigned to a random department based on a second lottery. At first, no allocation is final. All seats are available for allocation. All interns are ordered in a queue based on the order in the first lottery.

Next, an application to the highest ranked department is submitted for the intern at the top of the queue. If the application is submitted to the same department that was originally tentatively assigned as a result of the second lottery, then this tentative assignment is finalized. The intern is allocated a seat at this department and removed from subsequent allocations. The seat becomes unavailable for other interns. The process continues with the next intern in line. If the application is submitted to another department, then the first intern in the queue who tentatively holds a seat at this department is moved to the top of the queue directly in front of the requester. Whenever the queue is modified, the process continues in the same way with the new intern at top. This way, each intern is guaranteed an assignment which is at least as good as their tentatively assigned department. A mutually beneficial exchange is obtained when a cycle of applications are made in sequence, which benefits all affected interns[4]. In this case, the exchange is completed and the interns as well as their assignments are removed from subsequent allocations. The process continues with the next intern in line. The process continues until all interns are assigned a seat at a department.

As the BOS is easy to understand, it is used frequently in practice (Abdulkadiroğlu & Sönmez, 2003; Hoyer & Stroh-Maraun, 2020; Roth, 2008). Under complete information, equilibrium strategies always result in a stable allocation (Ergin & Sönmez, 2006). It is Pareto efficient with truthful preference revelation but not when reported preferences deviate from true.
The original DA as well as our adaptation is strategy-proof for interns and a resulting matching is stable. The DA considers interns' preferences as well as departments' priorities. Be aware that the stability refers only to the reported preferences (stage 2 of the experimental design in section 3).
The DA as described here possesses additional features that the original DA does not possess. The DA with common priority list results in the same matching as the SD when the same ordering is used, already noticed by De Haan et al. (2023), Pathak & Sönmez (2013) and Hakimov & Kübler (2021).

By this the DA with a common list is the only mechanism that satisfies all three properties: strategy-proofness, Pareto efficiency and stability. Without a common list no mechanism exists that fulfills all three properties (Alcalde & Barbera, 1994). Abdulkadiroğlu & Sönmez (1998) show that for some ordering of the students any Pareto efficient allocation can be obtained.

The same holds for the TTC. In a school choice market with one common priority ranking for all schools, the TTC produces the same matching as the SD (Morrill & Roth, 2024). As the TTC requires an ex ante random endowment

[4]e.g., Anna applies to Ben's tentatively assigned department, Ben applies to Carl's tentatively assigned department, and Carl applies to Anna's tentatively assigned department.

and then finds a core allocation, the Random Serial Dictatorship and Core from random endownment, yield the same distribution of final allocations (Abdulkadiroğlu & Sönmez, 1998). The outcome of the TTC is Pareto optimal for all interns. Further, the mechanism is strategy-proof.

3 Experimental Design and Hypotheses

We design our experiment to compare the behavioral effects of using induced versus non-induced preferences in three standard matching mechanisms: BOS, DA, and TTC. Special attention is given to truthful preference revelation and efficiency comparisons. To create an environment where non-induced as well as induced preferences can be inquired, we adopt the school choice model to a scenario in which interns (students) are allocated to departments of a company (schools). In line with the school choice model, these allocations are based on interns' preferences and departments' priorities. We implement a 3×2 design: two treatments for each mechanism in which one control group is assigned induced preferences, and one treatment group reports non-induced preferences based on their own evaluations. This structure allows us to isolate the impact of preference formation while holding the matching environment constant.

We consider a chocolate-producing company that offers seven internship positions across four departments[5]. Each department specializes in a different type of chocolate: Dark Chocolate (D), Hazelnut Chocolate (H), Milk Chocolate (M), and White Chocolate (W). The company must assign each of the seven interns to one of these departments, and it follows an internal policy that allocations should respect interns' stated preferences. While there are exactly seven available internship slots, these four departments differ in size. Table 1 below lists the number of internship positions available in each department.

A laboratory experiment was designed to simulate this allocation problem. The allocation procedure of the experiment is closely aligned with that of Chen

[5]Experiments have shown that participants are less likely to tell the truth as the number of schools increases (Chen & Kesten, 2019; Hakimov & Kübler, 2021). We follow Chen & Kesten (2019) by using 4 departments.

Chocolate department	Dark	Hazelnut	Milk	White
Number of slots	3	2	1	1

Table 1: Number of slots per department

& Sönmez (2006), with one key difference: unlike in their design, participants are not prioritized by default by a district school. This eliminates strategic biases stemming from save choices (compare Chen & Sönmez, 2006; Guillen & Veszteg, 2021). Additionally, we adapted the design to fit our context by considering departments instead of schools and reduced the number of departments to four, in line with recent research on market size. All experimental instructions were formulated in German. The experiment was implemented using the z-Tree software by Fischbacher (2007) and Duch et al. (2020).

In November 2023, Bachelor's and Master's students from various faculties at Paderborn University were invited to participate in the experiment. A total of 252 students volunteered, recruited through the online platform ORSEE (Greiner, 2015). The experiment was conducted in the Behavioral and Experimental Economics Research Laboratory (BaER-Lab). In total, 14 sessions were run with 7-28 participants divided into groups of 7, each lasting approximately 35 minutes, depending on the complexity of the mechanism implemented. On average, each student received 8€ (including a show-up fee).

The experiment consisted of two stages: first, the elicitation or assignment of preferences over chocolate types; and second, the formation of the preference lists over chocolate departments to be used for the allocation of the interns to department. Participants were initially informed via on-screen instructions that the experiment was structured in two stages. However, the instructions for the second stage were not revealed until the first stage had been completed. As a result, participants completed the first stage without knowing the precise nature of the subsequent stage. To allow participants to revisit the instructions at any time, a printed copy of the first-stage instructions was also placed on each desk. The second-stage instructions were distributed only after all participants had completed the first stage.

In the first stage of the control group, each participant was assigned a randomly generated preference list over four types of chocolate. These preference lists were displayed individually on participants' screens and served as their true preferences throughout the experiment. Participants were instructed to treat these assigned rankings as their own for the remainder of the session. They were also informed that the lists were randomly generated and therefore likely to differ from those of other participants. In contrast, participants in the treatment group constructed their own preferences through a series of six pairwise comparisons involving the same four chocolate types. In each comparison, participants selected their preferred chocolate or indicated indifference[6]. If indifferences were detected, they were shown a predefined ranking. In some cases, the comparisons were repeated due to non transitive preferences or random assignment[7]. The resulting preference list over types was displayed at the end of the stage. To motivate participants, one participant per session drew four lottery numbers corresponding to seat assignments, and these selected participants received a chocolate bar corresponding to their top-ranked chocolate from the comparison task. Henceforth, the list from the first stage will be referred to as the true preference list for both groups.

In the second stage, all participants—regardless of treatment group—were randomly assigned to groups of seven individuals. Within each group, each participant was also randomly assigned a position in a priority order from 1 to 7 which serving as a priority ranking used by all departments, mimicking real-world scenarios in which departments assign applicants priorities. The earlier a participant appeared in this list, the higher they were prioritized by all departments. This ensured that each participant had an equal chance of being ranked first through seventh. Participants were informed of their position in the priority order only after the allocation process was completed.

Within each group, participants competed for seven available positions across the four departments: D, H, M, and W. As mentioned earlier, the

[6]See Thurstone (1927) and Davidson & Farquhar (1976) for further information on pairwise comparisons. Indifferences were included to increase the information content of the experiment (Gridgeman, 1959).

[7]Allowing some repetitions to occur by random assignment ensures that all participants are treated equally and prevents methodological bias that could arise if only certain individuals were required to repeat tasks.

departments differed in capacity (Table 1). Participants were informed about the specific matching mechanism—either BOS, DA, or TTC—that would be used for the allocation. They then submitted their department rankings which is used for the allocation. Final allocations were determined according to these reported preferences over departments and the department priorities derived from the random order.

Importantly, the preference lists from stage I—whether induced or self-constructed—determined participants' monetary payoffs. Being matched to a department that corresponded to a more-preferred chocolate type resulted in a higher payoff, while being allocated to a less-preferred type yielded a lower payoff. Thus, being assigned to the department producing one's top-ranked chocolate yielded the highest possible monetary reward, whereas an assignment aligned with the least-preferred type resulted in the lowest. In addition to the monetary reward and 2.50€ show-up fee, participants received a chocolate bar from the department to which they were assigned. The chocolate bar served as a tangible, non-monetary component of the payment structure, reinforcing the link between reported preferences and real outcomes. The specific payoff values in the experimental currency Thaler associated with each preference rank are shown in Table 2[8]. To align with the lab rules, the average payoff for a participant was set equal to the hourly student assistant wage at Paderborn University. The numbers were chosen to ensure that participants have a genuine financial incentive to participate. The differences in payoffs across outcomes are large enough that receiving a participant's top choice provides a substantially higher reward than receiving their least-preferred choice.

Chocolate	favorite	second favorite	third favorite	least favorite
Thaler	36	27	21	18

Table 2: Payoff table of participants

Furthermore, after Stage II, participants completed a questionnaire that captured their descriptive characteristics as well as their reasoning and assumptions about other participants.

[8]On a participant's screen, the chocolate types in the first row of the table are replaced with their true stage I preferences.

As we have seen, the three mechanisms BOS, DA and TTC have different strength and weaknesses. In theory, truthful preference revelation is incentivized by the chosen mechanism. Thus, rational players strategize in line with the given mechanism. But how do participants react in a controlled lab experiment? In experiments with induced preferences, truth-telling rates are significantly different from 100% and also differ between algorithms. A summary over truth-telling rates in different experiments for the three mechanisms can be found in section 4 Figure 1.

Most of the experiments confirm the theoretical assumption that truth-telling rates are lower for the BOS than the other mechanisms (e.g. Chen & Sönmez, 2006). However, Pais & Pintér (2008) find higher truth-telling rates for the BOS under zero information.

From a theoretical perspective, whether preferences are induced or non-induced does not influence strategic behavior like truth-telling. However, many experiments have shown that even small changes like the information provided or advice given may affect the behavior of participants. Guillen & Veszteg (2021) discover that the understanding is crucial to form rational choices. We expect that the formation of own preferences in stage 1 helps the participants to understand the origin of the preferences and thus, makes them more aware in the following process. By that we can also expect to see differences between treatments. As the truthful preference revelation under the BOS might not be a dominant strategy anyway, we expect to see lower truthful preference revelation in both BOS treatments.

Hypothesis 1 (Truth-telling)**.** *Truth-telling rates are higher for the DA and TTC mechanisms with non-induced preferences in comparison to induced preferences.*

Per se, BOS is not Pareto efficient, and efficiency losses may be amplified when participants misrepresent their preferences. Surprisingly, Chen & Sönmez (2006) found that DA exhibited lower efficiency losses than TTC in a controlled laboratory experiment. The efficiency of the BOS, in comparison with the others, varies across environments. Our second hypothesis follows directly from the first, as efficiency losses are expected to be closely linked to lower truth-telling rates.

Hypothesis 2 (Efficiency)**.** *Efficiency is higher for non-induced preferences when controlling for the three mechanisms.*

4 Results

To analyze the observed experimental data, we begin by examining the extent of truthful preference revelation among participants before we have a closer look at efficiency.

4.1 Truth-telling

Truth-telling is defined as the complete and accurate reporting of one's preference ordering, such that reported preferences are identical to true preferences. Any deviation, even by a single rank, is classified as a misrepresentation. Overall, 73.02% of participants with non-induced preferences and 28.57% of those with induced preferences revealed their preferences truthfully. Within the treatment group with non-induced preferences, the highest rate of truth-telling was observed under DA, followed by BOS and subsequently TTC. In the control group, however, BOS elicited the highest rate of truthful preference revelation, followed by DA and TTC. Taken together, only 50.79% of all participants reported their preferences truthfully.

Figure 1 (left) presents the proportions of truthful preference revelation across mechanisms for both the treatment and control groups. The figure also provides an overview of truth-telling behavior observed in other experimental studies. In comparison to Chen & Sönmez (2006), our results appear less differentiated across mechanisms. Nonetheless, truth-telling rates vary substantially across experiments. As emphasized by Hakimov & Kübler (2021) in their survey, such variation is strongly context- and setting-dependent. Accordingly, these contextual differences must be taken into account when comparing experimental findings.

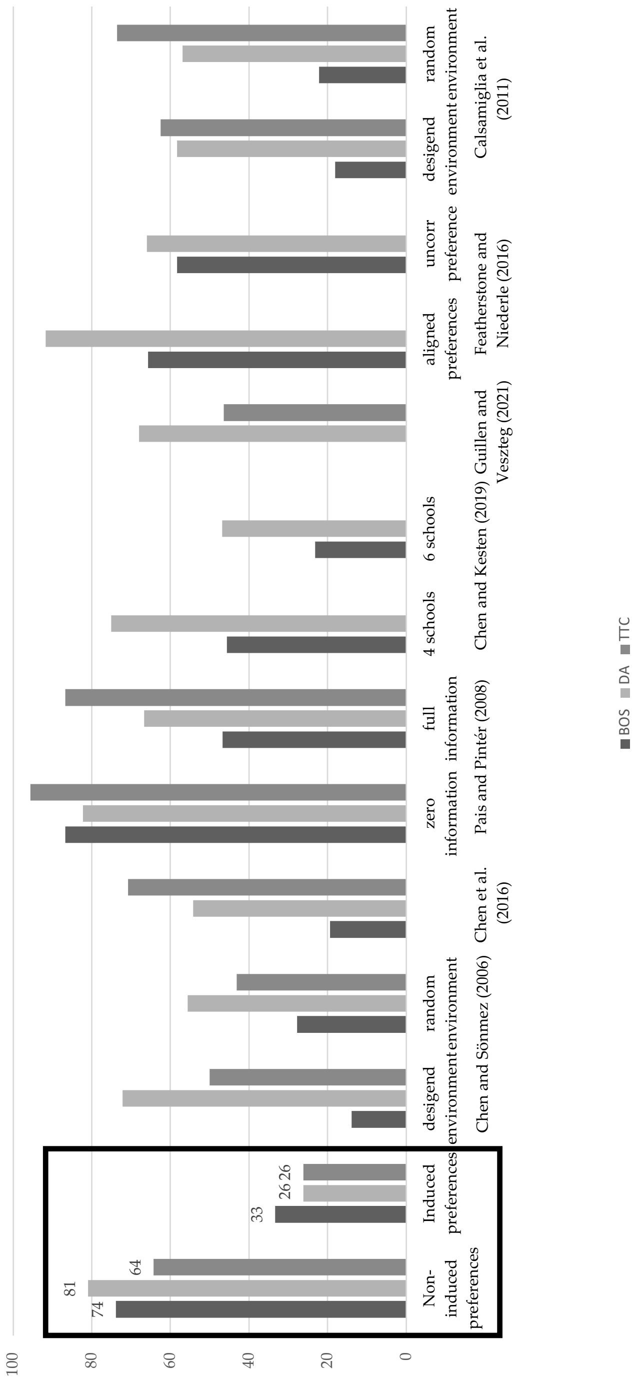

Figure 1: Proportion of truthful preference revelation by mechanism - Own depictions, extending Hakimov & Kübler (2021)

Result 1. *Non-induced preferences significantly increase the likelihood of a participant reporting true preferences for all three mechanisms.*

Support. This result is obtained based on a two-sample z-test, which indicates that the mean rate of truth-telling is significantly higher in the treatment group (non-induced preferences) compared to the control group (induced preferences), with $z = -7.056$ and $p < 0.001$.

To further confirm the relationship between preference type and truth-telling, we estimate a logit model with the binary dependent variable *truth-telling*. Table 3 reports the regression results. Being in the treatment group significantly increases the likelihood of reporting true preferences, thereby providing further support for Hypothesis 1.

	BOS (1)	DA (2)	TTC (3)
truth			
treatment	1.729***	2.483***	1.624***
	(0.480)	(0.527)	(0.476)
_cons	-0.693**	-1.036***	-1.036***
	(0.327)	(0.351)	(0.351)
N	84	84	84

Standard errors are in parentheses. Significance levels: *0.1 **0.05 ***0.01

Table 3: Logit model - Revelation of true preferences

□

These effects hold not only for DA and TTC but also for BOS. Since participants do not have full information about the preferences of other group members, strategizing under BOS may be risky. We also test for mean differences in truth-telling rates across the three mechanisms and find no statistically significant differences overall, failing to reject the null hypothesis. However, within the treatment group with non-induced preferences, the proportion of truth-telling is significantly higher for DA compared to TTC ($z = 1.713$, $p = 0.043$).

Understanding participants' decisions requires examining why they may fail to act rationally and how they misrepresent their preferences. Participants who do not follow a dominant strategy may lack full understanding of the mechanism or may be choosing randomly. Table 4 summarizes both truthful preference revelation and various patterns of misrepresentation for each group. These different patterns of misrepresentation are described in the following paragraphs.Across all mechanisms, a dominant strategy is to rank the least preferred chocolate last, as every participant is guaranteed a spot. Misreporting this last preference indicates either substantial misunderstanding of the mechanism or random reporting. Regression analysis, presented in Table 5, further reveals that being in the treatment group significantly increases the likelihood of truthfully reporting the fourth preference (also compare with Table 4). This finding suggests that participants' understanding or tendency toward random behavior differs between the control and treatment groups. Importantly, excluding these participants from the analysis does not alter the significance of our main results.

The fourth preference, however, is not the only one being misrepresented. As shown in Table 5, misrepresentation occurs at different positions in the ranking. We find that non-induced preferences significantly increase the likelihood of participants reporting the true second, third, and fourth preferences, whereas no significant effect is observed for the first preference. This pattern aligns with the Minneapolis strategy described by Chen & Sönmez (2006) for BOS, which recommends stating the true first preference and then strategically misrepresenting the second and third preferences. Among participants who engage in misrepresentation, 67.78% of the control group and 41.18% of the treatment group follow this strategy. Table 4 provides further details, presenting the percentages for each of the three mechanisms within the treatment and control groups.

As observed in other school choice experiments, department sizes do effect choices. In the questionnaire, several participants indicated that department size played a role in their decision-making. This, of course, is not consistent with the dominant strategy of truth-telling under DA and TTC. Whenever a small department (milk or white chocolate) is ranked as the top choice, we might expect participants to consider reporting non-truthful preferences. We define a *Small Department Bias* as having a small department as a top

Treatment		Truth	Misrepresentation of preferences			
			2&3 (Minneapolis)	4^{th} not true pref.	Small Dep. Bias	Other
treatment	BOS	0.738	0.119	0.024	0.214	0.119
control	BOS	0.333	0.476	0.119	0.357	0.071
treatment	DA	0.810	0.071	0.048	0.119	0.071
control	DA	0.262	0.476	0.143	0.333	0.095
treatment	TTC	0.643	0.143	0.048	0.286	0.119
control	TTC	0.262	0.500	0.143	0.405	0.071

Table 4: Proportion of truthful preferences revelation and misrepresentation

	true first	true second	true third	true fourth
main				
treatment	0.066	1.880***	2.197***	1.328**
	(0.364)	(0.281)	(0.303)	(0.526)
_cons	1.792***	-0.802***	-0.588***	1.858***
	(0.255)	(0.193)	(0.186)	(0.261)
N	252	252	252	252

Standard errors are in parentheses. Significance levels: *0.1 **0.05 ***0.01

Table 5: Truth-telling per rank

choice while failing to report preferences truthfully. Table 4 summarizes the proportion of participants exhibiting this bias. We further test this effect using a logit model:

$$ln(truth) = \beta_0 + \beta_1\text{treatment} + \beta_2\text{firstdark} + \beta_3\text{firsthazel} + \beta_4\text{firstwhite} + \varepsilon$$

Participants are grouped according to their most preferred chocolate, with milk chocolate serving as the reference category. According to the questionnaire, milk chocolate is the top preference for 42.86% of participants, and many participants reported awareness of this fact. Participants in the treatment group who most prefer milk chocolate are significantly more likely to report their top preference truthfully. For BOS, we observe significant differences between chocolate types, whereas no such differences appear for DA or TTC (Table 6). Specifically, for BOS, having hazel or dark chocolate—the largest departments—as the top preference significantly increases the likelihood of truth-telling compared to milk chocolate. This finding supports the notion that choosing a small, highly competitive department as a top preference incentivizes participants to strategize and misrepresent their preferences.

Participants in the treatment group, who were not assigned induced preferences, could indicate indifferences in Stage I, with 29.37% reporting such indifferences. No participant reported being indifferent across all pairwise comparisons. We do not find a significant effect of stating indifferences on truth-telling behavior. Notably, 6.35% of treatment participants did not report

	BOS	DA	TTC
truth			
treatment	2.203***	2.262***	1.495***
	(0.582)	(0.571)	(0.494)
firstdark	0.837***	-1.042	-0.603
	(0.731)	(0.862)	(0.819)
firsthazel	2.223***	-0.677	0.210
	(0.769)	(0.735)	(0.635)
firstwhite	0.956	-1.149	-0.454
	(0.755)	(0.703)	(0.600)
_cons	-1.791***	-0.305	-0.810
	(0.609)	(0.592)	(0.494)
N	84	84	84

Standard errors are in parentheses.
Significance levels: *0.1 **0.05 ***0.01

Table 6: Reported list equal true preferences

the same preferences in Stage I as in the subsequent questionnaire. Failing to report consistent preferences across the two stages has a significant negative effect on truth-telling within the treatment group. Importantly, our main results remain unchanged when these inconsistent participants are excluded from the analysis[9]. The only exception is a minor finding: the proportion of truth-telling is no longer significantly higher under DA compared to TTC in the treatment group.

4.2 Efficiency

Until now we have observed that misrepresentation varies across groups but is present in all of them. We now turn to analyzing whether participants, through such misrepresentation, negatively affect their own outcomes or those of their group members. In other words, does misrepresentation increase efficiency loss, and are there differences between treatments in this regard?

Table 7 compares the outcomes observed in the treatment groups with the

[9] Although some percentages and point estimates change slightly, the significance levels of our main findings remain stable.

true rank (payoff)	BOS				DA				TTC			
	control		treatment		control		treatment		control		treatment	
	reported	true	reported	true	reported	true	reported	true	reported	true	reported	true
1 (36)	26	26	24	24	23	29	12	14	22	25	15	19
2 (27)	3	8	8	10	7	8	18	15	10	9	15	12
3 (21)	8	2	6	5	8	2	7	8	7	5	5	4
4 (18)	5	6	4	3	4	3	5	5	4	3	7	7
mean rank	1.810	1.714	1.762	1.690	1.833	1.5	2.120	2.095	1.786	1.667	2.095	1.976
mean payoff	30.36	31	30.43	30.79	29.93	32.29	27.5	27.79	29.86	31	28	29
rank if DA		1.714		1.738						1.714		2.024
payoff if DA		30.71		30.43						30.86		29.14

Table 7: Number of participants receiving a certain rank under reported and true preferences (Rank 1 indicates top choice)

counterfactual scenario in which all participants had reported their preferences truthfully. Here a lower rank implies being matched to a more preferred choice. The table shows via the column *reported* the number of participants receiving each rank and the associated payoff for each mechanism. Across all mechanisms, participants in the treatment and control groups were, on average, assigned to less favorable (higher-ranked) options and received lower payoffs than they would have if everyone had reported their preferences truthfully (the column *true*). In particular, for DA and TTC, fewer participants obtained their top choice due to misrepresentation. These results indicate a clear efficiency loss resulting from participants' strategic misrepresentation.

To investigate efficiency losses or gains, we begin by considering several benchmark cases. First, we examine the expected per capita rank across all possible lottery outcomes[10], assuming full truthful preference revelation. Table 8a presents the expected rank under each mechanism, providing insight into the best achievable average ranks. Since the outcome of DA is equivalent to that of SD, the average per capita rank under DA serves as a reference for Pareto-efficient allocations (Table 8b).

	BOS	DA	TTC
treatment	1.736	2.107	2.020
control	1.669	1.574	1.677

(a) Original mechanism

	BOS	DA	TTC
treatment	1.778	2.107	2.020
control	1.687	1.574	1.677

(b) DA applied among all groups

Table 8: Expected per capita ranks across mechanisms and treatments
(Rank 1 indicates top choice)

TTC and DA yield Pareto-efficient allocations for all lotteries, and the average received ranks are identical for both mechanisms within each group. However, on average, DA produces slightly less-preferred outcomes than BOS, because BOS more frequently assigns participants to their top choices. While participants in the DA control and BOS, DA, and TTC treatment groups are slightly better off on average, participants in the BOS and TTC control groups are not.

[10] There are $7! = 5040$ different lottery outcomes for each group. Preallocations for TTC are the same as in the experiment.

As the outcome for each participant highly depends on their group members, we analyses the regrouping of participants. To compute mean efficiencies for the treatment and control group given true preferences, we use the recombinant estimator by Mullin & Reiley (2006). We calculate the outcomes and mean efficiencies when players' would have been grouped differently within their treatment or control group given each mechanism. In the six groups we could form $\frac{42!}{35!7!}$ different groups. In the actual estimation, we randomly generated 10 000 recombinations per participant following Calsamiglia et al. (2011) in their comment on the experiment by Chen & Sönmez (2006). Table 9 for true preferences and Table 10 for reported preferences show the mean, variance and standard deviation of the expected per capita rank and payoff (payoff in parentheses).

We conclude from the recombinant estimator, that in general in the treatment group a lower average rank is achieved than the control group. Thus, the environments have a different competitiveness level. Further, we see a tendency that the rank received under true preferences is lower than the rank under reported preferences. Thus, we further compare the average efficiency loss between the treatments using the new recombinations. We define efficiency loss in the following way:

$$\text{Efficiently loss} = \frac{\text{per capita received choice under } \textit{reported} \text{ preferences}}{\text{per capita received choice under } \textit{true} \text{ preferences}}$$

Accordingly, we define efficiency with respect to allocations based on fully revealed preferences.

Result 2. *For the DA, the efficiency loss of not reporting true preferences is significantly higher in the control group compared to the treatment group using the recombinant estimator. However, for the BOS, TTC and all mechanisms combined, we do not find significant results.*

Support. For the DA, the two-sample z-test shows that the mean rate of efficiency loss is significantly higher in the control group compared to the treatment group with z=2.100 and p=0.018. For the BOS, TTC and all mechanisms combined we do not find significant results (BOS: z=0.779, p=0.782;

rank (payoff)		Mean $\hat{\mu}$	Variance σ^2	Standard Deviation	Asym. Variance $var(\hat{\mu})$
BOS	control	1.454 (32.643)	0.734 (34.508)	0.857 (5.874)	0.000 (0.000)
	treatment	1.741 (30.709)	1.112 (47.489)	1.055 (6.891)	0.000 (0.000)
DA	control	1.596 (31.540)	0.843 (40.109)	0.918 (6.333)	0.000 (0.000)
	treatment	2.135 (27.796)	1.186 (49.571)	1.089 (7.041)	0.000 (0.000)
TTC	control	1.676 (30.924)	0.890 (42.335)	0.943 (6.507)	0.000 (0.000)
	treatment	2.042 (28.603)	1.279 (52.782)	1.131 (7.265)	0.000 (0.000)

Table 9: Recombinant Estimator of Expected per capita rank (and payoff) - true preferences

rank (payoff)		Mean $\hat{\mu}$	Variance σ^2	Standard Deviation	Asym. Variance $var(\hat{\mu})$
BOS	control	1.581 (31.715)	0.873 (41.423)	0.935 (6.436)	0.000 (0.000)
	treatment	1.766 (30.494)	1.105 (47.489)	1.051 (6.865)	0.000 (0.000)
DA	control	1.821 (29.992)	1.070 (49.389)	1.034 (7.028)	0.000 (0.000)
	treatment	2.136 (27.761)	1.168 (48.803)	1.081 (6.986)	0.000 (0.000)
TTC	control	1.771 (30.285)	0.992 (47.212)	0.996 (6.871)	0.000 (0.000)
	treatment	2.068 (28.278)	1.189 (49.589)	1.091 (7.042)	0.000 (0.000)

Table 10: Recombinant Estimator of Expected per capita rank (and payoff) - reported preferences

TTC: z=0.410, p=0.341; mechanisms combined: z=1.109, p=0.1337). □

We will further compare the efficiency in more detail for the given groups in our experiment. To analyze the effects of truthful preference revelation on efficiency within one of the six groups, the composition of preferences and competitiveness did not play a role. Competitiveness in this context is a post-treatment variable, since the process of preference induction or random assignment can produce groups with systematically different levels of competitiveness. Therefore, comparisons of efficiency between the groups should be interpreted as comparisons of relative efficiency losses, acknowledging that differences in competitiveness are themselves a consequence of the treatment.

To analyze the competitiveness of our groups let us start by comparing the groups in our experiment. We can differentiate between the three treatment and three control groups for each mechanism. The composition of groups is random, however, the preferences differ. While the likelihood of liking a certain chocolate is equally distributed for the control group, this is not the case for the treatment group. By that, preferences in the treatment group can be more heterogeneous than in the control group. As no information about the preferences of other participants is given for the treatment group, participants can only make assumptions over the most preferred chocolate types. For all participants, we see that they assume that milk chocolate is the most preferred chocolate. We can support their believes. In the questionnaire 42.86% of participants state that they prefer milk chocolate the most. In stage 1 for the treatment group 43.24% state that milk chocolate is their favorite chocolate. Thus, our treatment group is a good representation of the whole group.

By that, preferences are not equally distributed and preferences within a group are on average more heterogeneous. Thus, for our further analysis we compare the competitiveness of groups. We use the same competitiveness indices as Chen & Sönmez (2006). First, we compute the *first choice accommodation index* which represents the proportion of top choice receivers for each of the six subgroups, defined by the combination of mechanism and experimental group (treatment or control). It divides the average number of participants who receive their first choice under the Pareto efficient allocation[11] and truthful preference revelation by the number of participants in each subgroup (here 42). We find that for all mechanisms, the value is lower for the treatment group than for the control group (Table 11a). Thus, less participants receive their top choice in the treatment than in the control group which is also visible in the received payoff.

Second, we use the *competitive index*. It uses the following inputs: x - the mean value of the sum of ranks of the efficient allocation, min - the environment where every participant receives their top choice, max - the environment where every participant has the same preferences with $M \succ W \succ H \succ D$ (the biggest department is the least preferred). The index is then calculated by $\frac{\mathrm{x} - \max}{\max - \min}$. Here we find that the value is lower for the treatment group of all mechanisms, which shows that the treatment group environment is more competitive than the control group environment (Table 11b). This is in line with the first choice accommodation index as a less competitive environment results in a better average rank.

	BOS	DA	TTC
Treatment	0.548	0.333	0.524
Control	0.571	0.690	0.595

(a) First choice accommodation index

	BOS	DA	TTC
Treatment	0.369	0.548	0.512
Control	0.357	0.25	0.357

(b) Competitiveness index

Table 11: Competitiveness of treatments

Result 3. *The environment of the treatment group is more competitive than the environment of the control group.*

Accordingly, any efficiency comparison between groups should be understood as reflecting relative efficiency losses, given that variations in competitiveness arise from the treatment itself.

Looking at the different mechanisms in more details, supports what theory predicts about the dominant strategy of revealing true preferences for the TTC

[11] Remember the allocation under the DA is Pareto efficient for our implementation. The same lottery outcome is used as in the experiment.

and DA.

Result 4. *The mean of the received rank is significantly lower (i.e. better) for the true preference than the reported preferences for the DA and TTC as well as the treatment and control group.*

Support. There is a statistically significant difference between the received true choice under true and reported preferences, as indicated by the Wilcoxon signed-rank test for the DA $z = 2.53$ ($p = 0.011$) and the TTC $z = 2.323$ ($p = 0.028$). This suggests that the distributions of the two variables differ. For the treatment and control group statistics are z=2.008, p=0.045 and z=2.795, p=0.004, respectively. □

Similarly this holds for the payoff: A paired t-test shows that payoffs under true preferences are significantly higher than under reported preferences for both the DA (t=-2.77, p=0.004) and TTC (t=-2.29, p=0.012). Additionally, we see a difference for the DA control. The distribution is statistically different for the DA control group with z=2.566 (p=0.010). However, we do not find significant results for the other groups. For the payoff comparison, we likewise find significant differences for the DA control group (t-test: t=-2.707, p=0.010) and the TTC treatment group (t-test: t=-1.894, p=0.065).

For the BOS this phenomenon is only visible for the second rank but in general their means are not statistically different from each other (H_0 cannot be rejected).

Until now we considered the outcome for the whole group while strategy-proofness looks at the single participant. Not only the average rank of the group is increased and the payoff lowered but individuals who misrepresent harm themselves. How many participants harm themselves by misrepresenting their preferences while the reported preferences of the their group members are used? Theoretically, improvement is only possible for the BOS. However, the participants have no full information and thus, have to make assumptions when misrepresenting their preferences. In total, only one participant managed to improve their allocation. However, as this participant put the least preferred department not at the last position we assume that this participant did not act sophisticated. 13.49% of the control group and 6.35% of the treatment group harm themselves by misrepresenting, that is 18.89% and 25.53% of the once who misrepresent, respectively. However, we do not find significant result that being in the treatment group has a different effect on the own harm than being in the control group.

We further analyze the efficiency loss between groups. We define efficiency loss as the relation between the received rank under reported and true preferences:

$$\text{Relative efficiently loss} = \frac{\text{received rank under } \textit{true} \text{ preferences - received rank under } \textit{reported} \text{ preferences}}{\text{received rank under } \textit{true} \text{ preferences}}$$

By this we are able to make a comparison between groups as values are put into context.

Result 5. *The relative efficiency loss of not reporting true preferences is significantly higher in the control group compared to the treatment group. This is primarily driven by the DA.*

Support. Overall the relative efficiency loss is higher for the control group than the treatment group also when separating between mechanisms. We further

	BOS	DA	TTC
treatment	-0.050	-0.040	-0.119
control	-0.113	-0.321	-0.131

Table 12: Mean efficiency loss per treatment and mechanism

support the statistical significance as follows: While we find a significant difference between treatment and control groups overall ($p < 0.039$), separate t-tests for each mechanism reveal that this effect is mainly attributable to the DA ($p < 0.023$), with no significant effects detected for the TTC and BOS. A regression analysis supports these results (Table 13): Being in the treatment group decreases the efficiency loss significantly for the DA. □

In this way, we support the findings of the recombinant estimator analysis. However, Hypothesis 2 is only partially supported, as the results for BOS and TTC are not significant.

	all	BOS (1)	DA (2)	TTC (3)
relative efficiency loss				
treatment	0.119**	0.063	0.282**	0.012
	(0.057)	(0.079)	(0.122)	(0.092)
_cons	-0.188***	-0.113**	-0.321***	-0.131**
	(0.041)	(0.056)	(0.086)	(0.065)
N	84	84	84	

Standard errors are in parentheses. Significance levels: *0.1 **0.05 ***0.01

Table 13: Regression model - Relative efficiency loss

5 Conclusion

This paper highlights the importance of how preferences are elicited in matching experiments. Our findings demonstrate that using non-induced (i.e., naturally formed) preferences significantly increases the likelihood that participants truthfully report their preferences across all three tested mechanisms. Moreover, allowing participants to use their own preferences appears to improve their understanding of the mechanisms, particularly regarding strategic behavior.

We find that non-induced preferences lead to higher rates of truth-telling, especially in the second and third positions of the submitted preference lists. This, in turn, reduces the relative efficiency loss typically associated with misrepresentation. This is primarily driven by the DA.

These results call for a careful reassessment of past experiments that used induced preferences and provide guidance for future experimental designs. By showing that the origin of preferences can substantially shape participant behavior and outcomes, our study contributes to a deeper understanding of strategic interaction in matching markets and the external validity of experimental findings.

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6 Appendix

6.1 Appendix: Additional information

Chocolate	favorite	second favorite	third favorite	least favorite
Payoff	8,50€	7,00€	6,00€	5,50€

Table 14: A representative example of the monetary payoffs including the show-up fee.

6.2 Appendix: Instructions

Instructions were originally provided in German. We present the translated version here. They are structured as follows: On the first page, general information is given to all participants, thus they do not differ between groups. Afterwards participants receive instructions on Stage I depending on their group (treatment or control group). Stage II is provided for a participant from the control group under the Boston mechanism. Instructions are identical for all other groups, except the transfer rate which may vary (1:4, 1:5 or 1:6) and the Mechanism and Example sections. Instructions for the DA and TTC are provided subsequently.

Instructions

General Information

- Please read these instructions carefully first and then answer the questions on your computer screen.
- You can view the instructions at any time during the experiment.
- During the experiment, all amounts will be given in the fictitious currency "thaler".
- At the end of the experiment, we will ask you to fill out a questionnaire. You will receive further information about this as soon as the experiment is finished.

Please note:

- No communication is allowed.
- All cell phones must be turned off for the duration of the experiment.
- The payout and all decisions you make during this experiment will be anonymous, i.e. none of the other participants will know the identity of the person who made a certain decision.
- Please stay in your seat until the end of the experiment, you will be called for the payout by your assigned seat number.

Design of the experiment

- This experiment consists of two stages: Stage I and Stage II.
- The experiment deals with assignments. Examples of assignments are school place assignments in the U.S., donor organ assignments, or thesis assignments at UPB's Faculty of Business Administration and Economics.
- You will receive the instructions for Stage II after Stage I.

Good luck and thank you for your participation in this experiment!

Treatment Group

Experiment Stage I

Procedure Stage I

- Your decisions in this stage are independent of the decisions of the other participants.
- You will be shown six pairwise comparisons of chocolate types (milk chocolate, dark chocolate, white chocolate and hazelnut chocolate). Please always choose your preferred chocolate type. If you find both equally good/bad, please select the corresponding answer option "I find both equally". If you do not like or cannot tolerate chocolate, please select "I find both the same" in all six pairwise comparisons.
- It may happen that you have to do the six pairwise comparisons a second time. This can happen by chance or in case of not clearly identifiable preferences.
- Your preferences will be shown on the screen at the end of Stage I. If no clear preferences are apparent for some chocolate types, you will be shown a predefined overview.
- Note: There are no right or wrong answers, only your preferences are decisive.

Payment Stage I

- At the end of Stage I, one participant in the room is asked, on behalf of everyone, to draw four lottery numbers from an urn. Each ticket number in this urn corresponds to a seat number. The participants with the drawn seat numbers receive a bar of their favorite chocolate resulting from the six pairwise comparisons.

<u>Control Group</u>

Experiment Stage I

You will receive a randomly generated table with chocolate varieties. The chocolate types are "Dark chocolate", "Hazelnut chocolate", "Milk chocolate" and "White chocolate". This table is displayed on the screen and describes your preferences towards the mentioned chocolate types in this experiment. As the experiment progresses, assume that you have these preferences over the four types of chocolate. The other participants will also receive such a randomly generated table. Thus, the table may differ from the tables of the other participants due to the preferences assigned to you.

Experiment Stage II

Procedure Stage II

This is an experiment of decision making in economics. Depending on your decisions, you can earn money and chocolate. Your decisions at this stage depend on the decisions of the other participants.

In this experiment, we simulate a procedure for assigning internship applicants to departments in a chocolate company. The procedure, payoff rules, and method of assigning applicants are described below. Do not communicate with each other during the experiment. If you have questions at any time during the experiment, raise your hand and the experiment leader will help you.

Procedure

- Imagine that you are applying for an internship in a chocolate company. Internships are available in the departments of "Dark Chocolate (**D**)", "Hazelnut Chocolate (**H**)", "Milk Chocolate (**M**)" and "White Chocolate (**W**)".
- Each participant receives an internship in a department.
- Each participant will be randomly assigned to a group of seven applicants. Each group thus consists of you and six other participants. This means that in this experiment you are in an application process with six participants.
- In total there are seven internships available in the four departments (D, H, M, W). Department D has three places, H has two places, M and W have one place each.
- You will receive a **monetary payout** at the end of the experiment. This payout depends on the internship you receive. In addition, you will receive a bar of chocolate in the flavor of the department in which you are completing your final internship. The payouts are shown in the table below:

Internship opportunity obtained in the department with the

favorite chocolate	second favorite chocolate	third favorite chocolate	least favorite chocolate
36 thaler	27 thaler	21 thaler	18 thaler

The table can be explained as follows:

- If you are assigned to the department with your favorite chocolate, you will receive a reward of 36 thaler as well as 1 bar of this chocolate.
- If you are assigned to the department with your third favorite chocolate, you will receive a reward of 27 thaler as well as 1 bar of this chocolate.
- If you are assigned to the department with your second favorite chocolate, you will receive a reward of 21 thaler as well as 1 bar of that chocolate.
- If you are assigned to the department with your least favorite chocolate, you will receive a reward of 18 thaler as well as 1 bar of this chocolate.

Note: Other participants may have different payout tables. Therefore, the payouts may vary by department.

- As part of the application process, all participants submit an order of the departments for which they are applying.
- After all participants have made a decision, the experimenters begin the allocation procedure, which is explained below.

Method of assignment of applicants

- The departments have priorities over the applicants. For this purpose, after all applications have been submitted and before the assignment takes place, a lottery is held to generate a common order among the applicants. This order reflects the priorities of the departments: The earlier you appear in this order, the higher a department prioritizes you. This means that all applicants have the same chance to be first in line, second in line, ..., and last in line. You will be informed about your place in the order on your computer screen after the assignment has taken place.
- Once assignments have been determined, the experimenters will inform all participants of their assignment/internship placement and the corresponding payout. The internship assignment itself is carried out based on the following assignment method.
- After reading the explanation of the assignment mechanism, please indicate an order for the departments. Start with the department you want to apply to first and end with the department you want to apply to last.

Payment Stage II

- The payout of the experiment will be exchanged at an exchange rate of 1 euro per 6 thalers and paid out to you in cash together with the show-up fee of 2.50 euros. In addition, at this stage you will receive a bar of chocolate according to your assigned department.

Mechanism B

- The assignment process is explained below.
- Once all orders have been submitted and the lottery has been run, the internships will be assigned in four rounds.

Round 1:

a. An application is sent by each person applying to their first-choice department.
b. Each department accepts applicants according to their priorities until all internship positions are filled or all applicants have been accepted. Accepted applicants are assigned an internship position and their applications will be removed from the system. Remaining applications for the respective department are rejected.

Round 2:

a. Rejected applications will be sent to the respective second-choice department.
b. If a department still has open positions from the first round, it accepts applicants with a higher priority until all open positions are filled. The remaining applications are rejected.

Round 3:

a. Rejected applications will be sent to the respective third-choice department.
b. If a department still has open positions, it accepts applicants with a higher priority until all open positions are filled. The remaining applications are rejected.

Round 4:

The remaining applicants receive an internship at the respective fourth-placed department.

<u>Example B</u>

We will go through a simple example to show how the assignment method works.

Applicants and departments: Let's consider an application process at a juice manufacturer. In this example, there are six applicants, 1-6, and four departments named Apple, Orange, Grape and Pineapple.

Applicant ID Numbers: 1, 2, 3, 4, 5, 6	Departments: Apple, Orange, Grape, Pineapple

Internships: There are two internships each in the Apple and Orange departments and one each in the Grape and Pineapple departments.

Department	Internship 1	Internship 2
Apple	☐	☐
Orange	☐	☐
Grape	☐	
Pineapple	☐	

Submitted order over departments: Applicants submit the following order of departments:

	1st Choice	2nd Choice	3rd Choice	Last Choice
Applicant 1	Grape	Apple	Pineapple	Orange
Applicant 2	Grape	Pineapple	Apple	Orange
Applicant 3	Pineapple	Apple	Orange	Grape
Applicant 4	Grape	Apple	Pineapple	Orange
Applicant 5	Pineapple	Grape	Apple	Orange
Applicant 6	Apple	Orange	Pineapple	Grape

Lottery: The order of priority from the lottery is as follows:

1 – 2 – 3 – 4 – 5 – 6

The assignment: This assignment method consists of the following rounds:

Round 1: Each applicant applies to her first choice: applicants 1, 2, and 4 apply to the Grape department, applicants 3 and 5 apply to the Pineapple department, and applicant 6 applies to the Apple department.

- The Apple department accepts applicant 6.
- The Grape department accepts applicant 1 and rejects applicants 2 and 4.
- The Pineapple department accepts applicant 3 and rejects applicant 5.

Applicants		Department		Accept	Reject		Internship 1	Internship 2
6	→	Apple	→	6		→	$\boxed{6}$	$\boxed{}$
	→	Orange	→			→	$\boxed{}$	$\boxed{}$
1, 2, 4	→	Grape	→	1	2, 4	→	$\boxed{1}$	–
3, 5	→	Pineapple	→	3	5	→	$\boxed{3}$	–

Accepted applicants are removed from the further process.

Round 2: Each applicant who was rejected in round 1 then applies to her second choice. Applicant 2 applies to the Pineapple department, Applicant 4 applies to the Apple department, and Applicant 5 applies to the Grape department.

- There is no more space available in the Pineapple department, so applicant 2 is rejected.
- The Apple department accepts applicant 4 for its last internship position.
- There is no more space available in the Grape department, so applicant 5 is rejected.

Applicants		Department		Accept	Reject		Internship 1	Internship 2
4	→	Apple	→	4		→	$\boxed{6}$	$\boxed{4}$
	→	Orange	→			→	$\boxed{}$	$\boxed{}$
5	→	Grape	→		5	→	$\boxed{1}$	–
2	→	Pineapple	→		2	→	$\boxed{3}$	–

Round 3: Each applicant who was rejected in rounds 1-2 applies to her third choice. Applicants 2 and 5 apply to the Apple department.

- There is no place left in the Apple department, therefore the applicants 2 and 5 are rejected.

Applicants		Department		Accept	Reject		Internship 1	Internship 2
2, 5	→	Apple	→		2, 5	→	$\boxed{6}$	$\boxed{4}$
	→	Orange	→			→	$\boxed{}$	$\boxed{}$
	→	Grape	→			→	$\boxed{1}$	–
	→	Pineapple	→			→	$\boxed{3}$	–

Round 4: Each remaining candidate receives an internship at their last-choice department: applicants 2 and 5 will receive a spot in the Orange department.

Applicants		Department		Accept	Reject		Internship 1	Internship 2
	→	Apple	→			→	$\boxed{6}$	$\boxed{4}$
2, 5	→	Orange	→	2, 5		→	$\boxed{2}$	$\boxed{5}$
	→	Grape	→			→	$\boxed{1}$	–
	→	Pineapple	→			→	$\boxed{3}$	–

Based on this method, the final assignments are determined:

Applicant	1	2	3	4	5	6
Department	Grape	Orange	Pineapple	Apple	Orange	Apple

Mechanism D

- The specific assignment process is explained below.
- Once all orders have been submitted and the lottery has been conducted, the internships are assigned as follows:
  - An application is sent by all applicants to their first ranked department.
  - Throughout the assignment process, a department can hold no more applications than its number of available internships.
    If a department receives more applications than its capacity, it rejects the applicants with lowest priority. The remaining applications are tentatively accepted.
  - If an application is rejected by a department, it is forwarded to the next department according to the applicant's submitted order.
  - Whenever a department receives new applications, these applications are considered together with the tentatively accepted applications for that department.
  - Among the tentatively accepted and new applications, those applications that exceed the number of available internship positions and have the lowest priority are rejected. The remaining applications are or remain tentatively accepted.
  - The assignment is finalized when no more applications are rejected.
  - Each applicant is assigned a slot at the department that has provisionally retained his/her application at the end of the process.

Example D

We will go through a simple example to illustrate how the assignment method works.

Applicants and departments: We will look at an application process at a juice producer. In this example, there are six applicants, 1-6, and four departments, Apple, Orange, Grape and Pineapple.

Applicant ID Numbers: 1, 2, 3, 4, 5, 6	Departments: Apple, Orange, Grape, Pineapple

Internships: There are two internships each in the Apple and Orange departments, and one each in departments Grape and Pineapple departments.

Department	Internship 1	Internship 2
Apple	☐	☐
Orange	☐	☐
Grape	☐	
Pineapple	☐	

Submitted orders over departments: The applicants submit the following order of departments:

	1st Choice	2nd Choice	3rd Choice	Last Choice
Applicant 1	Grape	Apple	Pineapple	Orange
Applicant 2	Grape	Pineapple	Apple	Orange
Applicant 3	Pineapple	Apple	Orange	Grape
Applicant 4	Grape	Apple	Pineapple	Orange
Applicant 5	Pineapple	Grape	Apple	Orange

Applicant 6	Apple	Orange	Pineapple	Grape

Lottery: The lottery produces the following order:

$\boxed{1 - 2 - 3 - 4 - 5 - 6}$

The assignment method consists of the following steps:

Step 1: Each applicant applies to his/her first choice: applicants 1, 2 and 4 apply to the Grape department, applicants 3 and 5 apply to the Pineapple department, and applicant 6 applies to the Apple department.

- The Apple department tentatively accepts the application of applicant 6.
- The Grape department tentatively accepts the application of applicant 1 and rejects applicants 2 and 4.
- The Pineapple department tentatively accepts the application of applicant 3 and rejects applicant 5.

Applicants		Departments		Hold	Reject
6	→	Apple	→	$\boxed{6}\ \square$	
	→	Orange	→	$\square\ \square$	
1, 2, 4	→	Grape	→	$\boxed{1}$ –	2, 4
3, 5	→	Pineapple	→	$\boxed{3}$ –	5

Step 2: Each applicant rejected in Step 1 applies to his/her next choice: applicant 2 applies to the Pineapple department, applicant 4 applies to the Apple department, and applicant 5 applies to the Grape department.

- The Apple department considers the application of applicant 4 together with the application of applicant 6, which was tentatively accepted. It tentatively accepts both applications.
- The Grape department considers the application of applicant 5 together with the application of applicant 1, which was tentatively accepted. It tentatively accepts the application of applicant 1 and rejects applicant 5.
  The Pineapple department considers the application of applicant 2 together with the application of applicant 3, which was tentatively accepted. It tentatively accepts the application of applicant 2 and rejects applicant 3.

Hold	New Applicants		Departments		Hold	Reject
$\boxed{6}\ \square$	4	→	Apple	→	$\boxed{6}\ \boxed{4}$	
$\square\ \square$		→	Orange	→	$\square\ \square$	
$\boxed{1}$ –	5	→	Grape	→	$\boxed{1}$ –	5
$\boxed{3}$ –	2	→	Pineapple	→	$\boxed{2}$ –	3

Step 3: Each applicant rejected in Step 2 applies to his/her next choice: Applicants 5 and 3 apply to the Apple department.

– The Apple department considers the applications of applicants 3 and 5 together with the applications of applicants 4 and 6, which were tentatively accepted. It accepts the applications of applicants 3 and 4 tentatively and rejects applicants 5 and 6.

Hold	New Applicants		Departments		Hold	Reject
$\boxed{6}\ \boxed{4}$	3, 5	→	Apple	→	$\boxed{3}\ \boxed{4}$	5, 6
$\boxed{}\ \boxed{}$		→	Orange	→	$\boxed{}\ \boxed{}$	
$\boxed{1}$ –		→	Grape	→	$\boxed{1}$ –	
$\boxed{2}$ –		→	Pineapple	→	$\boxed{2}$ –	

Step 4: Each applicant rejected in Step 3 applies to his/her next choice: Applicant 5 and 6 apply to the Orange department.

– The Orange department tentatively accepts the applications of applicants 5 and 6.

Hold	New Applicants		Departments		Hold	Reject
$\boxed{3}\ \boxed{4}$		→	Apple	→	$\boxed{3}\ \boxed{4}$	
$\boxed{}\ \boxed{}$	5, 6	→	Orange	→	$\boxed{5}\ \boxed{6}$	
$\boxed{1}$ –		→	Grape	→	$\boxed{1}$ –	
$\boxed{2}$ –		→	Pineapple	→	$\boxed{2}$ –	

No application is rejected in Step 4. Based on this method, the final assignments are:

Applicant	1	2	3	4	5	6
Department	Grape	Pineapple	Apple	Apple	Orange	Orange

Mechanism T

- The specific assignment process is explained below.
- The goal of the assignment process is to initiate mutually beneficial exchanges between two or more applicants. The order in which these exchanges are considered is determined by a fair lottery.
- Once all orders have been submitted and the lottery has been conducted, the internships are assigned as follows:
    - Each applicant is first tentatively assigned to a random department based on another lottery.
    - At first, no assignment is final. All internships are available for assignments.
    - All applicants are ordered in a queue based on the order in the first lottery.
    - The application of the first applicant in the queue is sent to their first-choice department
        - If the application is submitted to the same department to which the applicant was tentatively assigned in the previous lottery, that tentative assignment is finalized. The applicant is assigned an internship in this department and removed from subsequent assignments. The internship becomes unavailable for other applicants. The process continues with the next applicant in the queue.
        - If the application is submitted to another department, then the first applicant in the queue who tentatively holds a slot at this department is moved to the top of the queue.
    - Whenever the queue is modified, the process continues in a similar manner: An application is submitted to the highest ranked department with available opportunities for the applicant at the top of the queue.
        - If the application is submitted to the same department to which the applicant was tentatively assigned in the previous lottery, that tentative assignment is finalized. The applicant is assigned an internship in this department and removed from subsequent assignments. The internship becomes unavailable for other applicants. The process continues with the next applicant in the queue.
        - If the application is submitted to another department, the first applicant in the queue who tentatively holds a slot at this department is moved to the top of the queue. This way, each participant is guaranteed an assignment which is at least as good as his/her tentatively assigned department, they were originally assigned to at the start of the process, based on their submitted priorities.
    - If a cycle is created due to the repeated modification of the queue, then each applicant in this cycle receives an internship position at their highest-ranked department that still has open positions. This constitutes a mutually beneficial exchange, in which every applicant in the cycle receives an internship position that they prefer to their tentatively assigned department. All applicants involved are then removed from the rest of the process. Their newly assigned internship positions become unavailable for other applicants. For example, such an exchange would be the following: Peter applies to John‘s tentatively assigned department, John applies to Paul's tentatively assigned department, and Paul applies to Peter's tentatively assigned department. In this case, the exchange is completed

and the applicants as well as their assignments are removed from subsequent assignments.
The process continues with the next participant in line.

- The process continues until all participants are assigned an internship.

Example T

We will go through a simple example to illustrate how the assignment method works.

Applicants and Departments: We will look at an application process at a juice producer. In this example, there are six applicants, 1-6, and four departments, Apple, Orange, Grape and Pineapple.

Applicant ID Numbers: 1, 2, 3, 4, 5, 6 Departments: Apple, Orange, Grape, Pineapple

Internship Opportunities: There are two internship opportunities each in the Apple and Orange departments, and one each in the Grape and Pineapple departments.

Department	Internship 1	Internship 2
Apple	☐	☐
Orange	☐	☐
Grape	☐	
Pineapple	☐	

Tentative assignments: Applicants are tentatively assigned to random departments.

Department	Internship 1	Internship 2
Apple	1	2
Orange	3	4
Grape	5	
Pineapple	6	

Submitted Department Rankings: The applicants submit the following department rankings:

	1st Choice	2nd Choice	3rd Choice	Last Choice
Applicant 1	Grape	Apple	Pineapple	Orange
Applicant 2	Grape	Pineapple	Apple	Orange
Applicant 3	Pineapple	Apple	Orange	Grape
Applicant 4	Grape	Apple	Pineapple	Orange
Applicant 5	Pineapple	Grape	Apple	Orange
Applicant 6	Apple	Orange	Pineapple	Grape

Lottery: The lottery produces the following order.

1 – 2 – 3 – 4 – 5 – 6

This assignment method consists of the following steps:

Step 1: A fair lottery determines the following applicant queue order: 1-2-3-4-5-6. Applicant 1 has ranked the Grape department as her top choice. However, the only available internship in the Grape department is tentatively held by applicant 5. So applicant 5 is moved to the top of the queue.

Step 2: The modified queue is now 5-1-2-3-4-6. Applicant 5 has ranked department Pineapple as her top choice. However, the only internship is tentatively held by applicant 6. Thus, applicant 6 is moved to the top of the queue.

Step 3: The modified queue is now 6-5-1-2-3-4. Applicant 6 has ranked the Apple department as her top choice. The two internships in the Apple department are tentatively held by applicants 1 and 2. Between these two, applicant 1 is ahead in the queue. So applicant 1 is moved to the top of the queue.

Step 4: The modified queue is now 1-6-5-2-3-4. Applicant 1 has ranked the Grape department as her top choice. A cycle of applications has now formed during the last three steps: applicant 1 applied for the internship tentatively held by applicant 5, applicant 5 applied for the internship tentatively held by applicant 6, and applicant 6 applied for the internship tentatively held by applicant 1. The exchanges can now be carried out: applicant 1 is assigned an internship in the Grape department, applicant 5 is assigned an internship in the Pineapple department, and applicant 6 is assigned an internship in the Apple department. These applicants as well as their assigned internships are removed from the system.

Step 5: The modified queue is now 2-3-4. There is one internship left in the Apple department and two in the Orange department. Applicant 2 applies to the Apple department, which is her top choice between these remaining two departments. Since applicant 2 tentatively holds an internship at the Apple department, her tentative assignment is finalized. Applicant 2 and her assignment are removed from the system.

Step 6: The modified queue is now 3-4. There are two internships left in the Orange department. Applicant 3 applies to the Orange department, which is the only department with available internships. Since applicant 3 tentatively holds an internship in the Orange department, her tentative assignment is finalized. Applicant 3 and her assignment are removed from the system.

Step 7: The only remaining applicant is applicant 4. There is one internship left in the Orange department. Applicant 4 applies to the Orange department for the last available internship. Since applicant 4 tentatively holds an internship in department, her tentative assignment is finalized. Applicant 4 and her assignment are removed from the system.

Final Assignment: Based on this method, the final assignments are:

Applicant	1	2	3	4	5	6
Department	Grape	Apple	Orange	Orange	Pineapple	Apple

Illustration

	Queue	Available internship opportunities	The top student in the queue applies to a department.	At the end of the step
Step 1	1 – 2 – 3 – 4 – 5 – 6	Apple Apple Orange Orange Grape Pineapple	1 applies to her 1st choice department Grape, which is tentatively assigned to 5.	5 moves to the top. 1 2 3 4 **5** 6
Step 2	5 – 1 – 2 – 3 – 4 – 6	Apple Apple Orange Orange Grape Pineapple	5 applies to her 1st choice department Pineapple, which is tentatively assigned to 6.	6 moves to the top. 5 1 2 3 4 **6**
Step 3	6 – 5 – 1 – 2 – 3 – 4	Apple Apple Orange Orange Grape Pineapple	6 applies to her 1st choice department Apple, which is tentatively assigned to 1 and 2.	1 moves to the top. 6 5 **1** 2 3 4
Step 4	1 – 6 – 5 – 2 – 3 – 4	Apple Apple Orange Orange Grape Pineapple	A cycle occured in the last 3 steps.	1 gets an internship in department Grape. 5 gets an internship in department Pineapple. 6 gets an internship in department Apple.
Step 5	2 – 3 – 4	Apple Orange Orange	2 applies to her 3rd choice department Apple, because her 1st and 2nd choices (departments Grape and Pineapple) are no longer available.	2 gets an internship in department Apple, because she is tentatively assigned to department Apple.
Step 6	3 – 4	Orange Orange	3 applies to department Orange, which is still available.	3 gets an internship in department Orange, because she is tentatively assigned to department Orange.
Step 7	4	Orange	4 applies to department Orange.	4 gets an internship in department Orange, because she is tentatively assigned to department Orange.

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