Fairness in Matching Markets: Experimental Evidence*

Tobias König Dorothea Kübler
Lydia Mechtenberg Renke Schmacker

May 3, 2021

Abstract

The fairness of allocation procedures has received considerable attention in the literature on matching markets. We consider fairness as the absence of justified envy, that is, an allocation is unfair if an agent who has priority over another agent prefers that agent's outcome to her own. We ask whether this fairness criterion plays a role for preferences over mechanisms. In two laboratory experiments, we let participants vote between the envy-free assortative matching and the Boston mechanism where some subjects are sincere by design while others can submit their rank-order lists strategically, leading to justified envy. To assess fairness preferences, we rely on veil-of-ignorance and spectator designs. We find that individuals have an aversion to justified envy, mainly in environments where the priorities at schools are based on earned entitlements. However, a persistently high share of individuals is willing to accept justified envy by voting for the Boston mechanism. Our findings indicate that many individuals believe that clever strategic behavior creates an entitlement of its own.

JEL-codes: D47; C92; I24; D72
Keywords: Matching markets; school choice; voting; Boston mechanism; naive agents; justified envy

*Financial support by the Deutsche Forschungsgemeinschaft through CRC TRR 190 ('Rationality and Competition') and Cluster of Excellence 'Contestations of the Liberal Script' (EXC 2055) is gratefully acknowledged. The paper has benefited from discussions with participants of the WZB MC seminar, the Berlin-Munich Seminar, and the Lausanne Matching and Market Design Workshop 2020.

Tobias König: Linnaeus University, tobias.koenig[at]lnu[dot]se
Dorothea Kübler: Technical University Berlin and WZB Berlin Social Science Center, dorothea.kuebler[at]wzb[dot]eu
Lydia Mechtenberg: University Hamburg, lydia.mechtenberg[at]uni-hamburg[dot]de
Renke Schmacker: University of Lausanne, renke.schmacker[at]unil[dot]ch

1 Introduction

In many important domains, matching procedures are employed to allocate goods and services. Examples include the allocation of seats at schools and universities, of public housing, and of appointments at public offices. Often individuals are required to state their preferences for the available options. By taking into account the applicants' priority, which can be based on test scores, lottery draws, or waiting times, the clearinghouse creates a matching outcome.

The theoretical matching literature assesses matching mechanisms with respect to certain desirable properties. Among these, the absence of justified envy is often regarded as the main criterion for a fair matching outcome. In the school-choice context, a matching is considered free of justified envy if there is no individual who prefers another school over her school and is entitled to it according to that school's priorities (Abdulkadiroğlu and Sönmez, 2003; Balinski and Sönmez, 1999).

In this paper, we empirically investigate the role of absence of justified envy (or envy freeness) for preferences over matching procedures. We focus on the widely used Boston mechanism. The Boston mechanism allows sophisticated individuals, who act strategically, to benefit at the expense of individuals who submit their true preferences (Pathak and Sönmez, 2008). Thereby, it can lead to outcomes characterized by justified envy. However, policy-makers and economists have argued that the extent to which individuals are able to make clever strategic choices should not affect the allocation of goods and services they are otherwise entitled to (Abdulkadiroğlu *et al.*, 2006). This argument was decisive for abandoning the Boston mechanism in the city of Boston.[1]

While the concept of fairness as envy freeness plays a central role in the theoretical literature on matching markets, there is no empirical evidence about whether individuals exhibit such a fairness preference over allocation procedures. In this paper, we take a first step toward closing this gap and let subjects vote over matching procedures. We focus on two procedures: the Boston mechanism and a matching procedure that directly implements the envy-free matching outcome. To isolate fairness preferences, we vary the subjects' degree of personal involvement. In particular, we observe their preferences both

[1]In a memo to the Boston School Committee on May 25th 2005, former Boston Public Schools superintendent Thomas Payzant described the rationale for switching from a manipulable school-choice mechanism to a strategy-proof mechanism as follows: "A strategy-proof algorithm *levels the playing field* by diminishing the harm done to parents who do not strategize or do not strategize well." (Abdulkadiroğlu *et al.*, 2006)

when they have a stake in the mechanisms and when they do not know their role or do not participate. Our results reveal that many individuals indeed care about the fairness of the allocation procedures. Some individuals prefer the mechanism that ensures envy freeness, in particular when matching priorities are based on earned entitlements. But there is also a considerable share of individuals who prefer the Boston mechanism, although it entails justified envy. Our explanation is that these individuals consider the Boston mechanism to be fair, since it rewards strategic behavior and cleverness, thereby creating entitlements on its own. We conclude that envy freeness is one, but not the only empirically relevant concept individuals employ when judging the fairness of allocation procedures.

We elicit individuals' fairness preferences over matching procedures in the context of school choice. We designed the school choice game to be as simple and transparent as possible: students have fully correlated preferences over schools, and the priorities of students at schools are identical, with enough seats for all students. Some students are sincere, i.e., subjects in their role are forced to submit their true preference ranking over schools. The remaining students are free to submit any preference ranking, i.e., they can be strategic. This setup ensures that there is common knowledge about the existence of heterogeneity in the individuals' ability to strategize, akin to the model setup in Pathak and Sönmez (2008). It guarantees that the Boston mechanism produces a unique, non-envy free matching outcome in equilibrium, allowing us to study voter's preferences for (or against) envy freeness.

In the baseline mechanism, students are assigned to the schools according to their predetermined rank: the highest-ranked student is matched to the most-preferred school, the second-ranked student to the second-most-preferred school, and so on. Hence, this allocation procedure directly implements the assortative matching that is free of justified envy. The second mechanism is the Boston mechanism, for which students submit preference lists over schools. Subjects can thus decide whether they want students to be assigned according to a simple mechanism yielding the assortative (envy-free) matching, or whether they want the Boston mechanism to determine the matching in the experiment. In the Nash equilibrium of the Boston mechanism, the strategic students manipulate their preference lists, and the sincere students are matched to the lowest-ranked schools as a result. This benefits all students with a lower priority than the sincere students, since these low-priority students are matched to better schools than in the assortative matching.

We conducted two experiments. In both of them, we study the preferences over the mechanisms of subjects in the role of students, denoted as *stakeholders*. Their voting decisions can be driven by selfish motives and by other-regarding concerns. To isolate fairness preferences, we also investigate how subjects vote when (i) they are behind a *veil of ignorance* and do not know their rank or their ability to submit their rank-order lists freely (Experiment 1), and (ii) when they are fully detached *spectators* who vote for a mechanism that will be implemented for another group of subjects (Experiment 2).

In Experiment 1, we elicit the preferences over mechanisms from stakeholders and from subjects who are behind a veil of ignorance. Subjects are randomly assigned a rank that determines their priority at schools, and two students (those who are ranked second and third) are sincere. Voting for the mechanisms is incentivized as follows: subjects have to pay a small fee to be able to vote, and the choice of a randomly picked subject is implemented for the group. Using a veil of ignorance in one treatment, the experiment varies whether or not the subjects know about their priority rank and thus their ability to manipulate their rank-order list in the Boston mechanism when voting for one of the mechanisms.

The Veil of Ignorance treatment allows us to investigate fairness preferences, abstracting from the individuals' role in the matching procedure. The concept of veil of ignorance was made prominent by the seminal contribution of Rawls (1971), and it has been frequently employed in experiments to identify impartial preferences for institutions (e.g. Frohlich *et al.*, 1987; Johansson-Stenman *et al.*, 2002; Schildberg-Hörisch, 2010). Note that in equilibrium with payoff-maximizing players, subjects are predicted to abstain from voting behind the veil of ignorance. The reason is that the distribution of payoffs is fixed, such that the random allocation of ranks makes every payoff equally likely, both in the matching resulting from the Boston mechanism and in the assortative matching.

The results of Experiment 1 reveal that most stakeholders engage in costly voting (80 percent of subjects), and that they condition their choice of a matching procedure on their ability to manipulate their rank-order list: students who can submit any rank-order list vote predominantly for the Boston mechanism, while most sincere students vote for the assortative matching. In the Veil of Ignorance treatment, where subjects are predicted to abstain in equilibrium, we still observe a large share of individuals (almost 60 percent) participating in costly voting. Furthermore, only a small share of subjects (13 percent) vote for the envy-free assortative matching—the great majority of voters (46 percent of subjects) prefer the Boston mechanism instead. On the one hand, the fact that subjects behind the veil of ignorance pay to vote suggests that their preferences over matching mechanisms are not only driven by their own payoffs. On the other hand, voters behind the veil of ignorance do not exhibit a clear preference for an allocation that is free of justified envy. This raises the question of why many individuals prefer the Boston mechanism over the assortative matching.

It can be argued that our setting in Experiment 1 is less conducive to aversion against justified envy than many real-world contexts, since the matching priorities of students are determined randomly. Moreover, the veil-of-ignorance design cannot fully exclude that participants expect out-of-equilibrium behavior from others (although no such evidence can be inferred from the elicited beliefs). We therefore conduct a second experiment, in which subjects vote for a mechanism that is implemented for another group of subjects. With this spectator design, we shut down all elements of self-interest and personal involvement (Konow, 2000; Cappelen *et al.*, 2013). Moreover, we let subjects work on a real-effort task that determines their priorities at schools. This is likely to create a stronger sense of entitlement to obtain a seat according to one's priority.

Our findings from Experiment 2 show that 80 percent of spectators engage in costly voting. The share of spectators voting for the envy-free matching is considerable (38 percent), suggesting that preferences for envy freeness are relevant when priorities are based on earned entitlements. However, there is still a large share of spectators (46 percent) who vote for the Boston mechanism. Hence, the preference for the Boston mechanism turns out to be remarkably robust, and persists even when priorities at schools are based on performance in a real-effort task.

One reason for the polarized voting pattern is that fairness ideals differ. This emerges from free-form answers in the post-experimental questionnaire: subjects who vote for the assortative matching consider the envy-free matching to be fair, while subjects who vote for the Boston mechanism state that the Boston mechanism leads to a fair outcome. Moreover, those who vote for the assortative matching tend to state that they want to prevent the sincere players from being sent to lower-ranked school in the Boston mechanism. In contrast, some subjects who vote for the Boston mechanism state that they want to reward smart behavior. These qualitative findings are in line with our interpretation that there is heterogeneity in fairness preferences over matching procedures: some individuals value envy freeness—others, however, ascribe fairness to a strategic environment.

Our experiment contributes to a growing literature on the behavioral aspects of matching markets.[2] Pathak and Sönmez (2008) show that sophisticated players benefit from a non-strategy-proof matching algorithm, since it allows them to exploit sincere players who

[2]For a survey of matching experiments see Hakimov and Kübler (2020).

do not act strategically. We present the first empirical test of this idea: using a controlled lab experiment, we provide evidence that the strategic players understand the incentives of the Boston mechanism, and prefer it over a matching procedure that directly implements the assortative matching outcome. At the same time, our work goes beyond the ideas of Pathak and Sönmez (2008). To understand fairness preferences regarding matching procedures and to test the empirical relevance of envy freeness, we elicit the preferences over matching mechanisms of subjects behind the veil of ignorance and of spectators who do not participate in the mechanisms. We find that a considerable share of individuals vote for the Boston mechanism since they perceive an outcome based on strategic choices as fair. This finding provides a novel explanation for the widespread use of the Boston mechanism.

The heterogeneity of people's ability to strategize has received some attention in the empirical matching literature. For instance, Dur *et al.* (2018) document that there is variation in students' ability to understand the incentives of the Boston mechanism to avoid over-demanded schools, using US field data. Moreover, a number of studies find that individuals hold incorrect beliefs about their admission chances, which prevents them from choosing optimally in the Boston mechanism (Kapor *et al.*, 2020; de Haan *et al.*, 2018; He, 2017). Basteck and Mantovani (2018) experimentally show that differences in cognitive skills translate into differences in matching outcomes in the Boston mechanism, with this link being less pronounced in a strategy-proof mechanism. In contrast, we exogenously vary the ability to strategize and let subjects vote over matching mechanisms when some agents are known to be sincere. We thus add a political economy perspective to the literature on behavioral market design.

In practice, absence of justified envy has proven to be an important criterion for school-choice mechanisms. For example, after the Boston School Committee abolished the manipulable Boston mechanism, a decision had to be made between the Pareto inefficient deferred-acceptance mechanism and the efficient top-trading cycles mechanism (TTC) (Abdulkadiroğlu and Sönmez, 2003). While TTC was initially preferred on grounds of efficiency, concerns about justified envy arising from trading priorities in TTC led to the adoption of DA (Abdulkadiroğlu *et al.*, 2006). It was also discussed whether some priorities—e.g., lottery priorities—are more legitimate to be traded than others—e.g., sibling priority. Our paper contributes to this discussion by studying the acceptability of justified envy in environments with priorities based on a lottery (Experiment 1) and in environments with merit-based priorities (Experiment 2).

Finally, our paper relates to a literature that uses incentivized experiments to study distributional preferences. One strand of research uses veil-of-ignorance designs, in which subjects decide on a redistribution scheme before they know whether they are net payers or receivers (e.g. Krawczyk, 2010; Schildberg-Hörisch, 2010; Durante *et al.*, 2014; Deffains *et al.*, 2016; Gerber *et al.*, 2019). These designs capture both distributional preferences and risk preferences regarding one's position in society. Another strand of research uses spectator designs where subjects decide on the distribution of resources for another group of subjects (e.g. Konow, 2000; Cappelen *et al.*, 2013; Durante *et al.*, 2014; Mollerstrom *et al.*, 2015; Deffains *et al.*, 2016; Cettolin and Riedl, 2017; Almås *et al.*, 2020). In our paper, we employ both approaches. However, the payoff distribution is held constant in our experiments. Instead, we let subjects decide on the allocation procedure that assigns the payoffs.

2 Matching markets: basic concepts

In this section, we introduce some concepts of matching theory that will be used to characterize the experimental school-choice problem. There are students who are assigned to schools with limited capacity. The students have strict preferences over schools, and schools have priorities according to which they order the students. We consider a setup where the number of students is equal to the number of seats.

A matching is considered *envy free* if the following condition is satisfied: There is no student who prefers another school to her match and has a higher priority at this school than an admitted student. This concept of envy freeness can be interpreted as fairness (Balinski and Sönmez, 1999; Abdulkadiroğlu and Sönmez, 2003).

In our setup, all students rank schools in the same way, i.e., they agree on the best school, the second-best school, etc. At the same time, all schools rank students based either on their performance rank or based on a single lottery. Thus, there exists an *assortative* matching outcome. A matching outcome is assortative if the highest-ranked students are matched to the most-preferred school, the next highest-ranked students are matched to the second most-preferred school, and so on. Thus, the assortative matching outcome is free from justified envy.

Our experiments employ the *Boston mechanism*, which works as follows:

All students submit a rank-order list of schools. Students are ranked according to their performance ranks or according to a lottery. The student assignment mechanism iterates the following steps:

Step 1: Each student applies to the school that she ranks first. Each school admits students up to its capacity, in the order of their priorities. These assignments are final. The remaining students are rejected.

Step k, k≥2: Each student who was rejected in the previous step applies to the next school in the rank-order list. Each school admits students up to its remaining capacity, according to their priority. These assignments are final. The remaining students are rejected.

The algorithm terminates when no student is rejected, or all schools have filled their capacity.

The Boston mechanism is not strategy-proof: truthful preference revelation is not a (weakly) dominant strategy for agents. For example, it is straightforward to see that it can be optimal for a student to skip her most preferred school if she has no chance of being admitted. In Nash equilibrium, the outcome of the Boston mechanism is stable (Ergin and Sönmez, 2006), which means that no blocking pair of a student and a school can profitably deviate. In the context of school choice, this implies that the matching is free of justified envy (Abdulkadiroğlu and Sönmez, 2003). However, the equilibrium requires strategic play by the students. If, in contrast, some students do not strategize, unstable outcomes can occur where a student prefers a seat at a school where she has a higher priority than a student who has been admitted (Pathak and Sönmez, 2008). This is the scenario that we focus on in our experiments.

3 Experiments

The two experiments are designed to study the role of envy freeness when voting over matching procedures. In both experiments, subjects can decide between a mechanism that guarantees envy freeness and the Boston mechanism, which potentially violates it.

We choose a simple matching market with fully correlated student preferences and school priorities. As a consequence, there is a unique envy-free allocation, namely the assortative matching. Subjects can vote for a matching procedure that directly implements the envy-free matching outcome.

As noted earlier, a property of the Boston mechanism is that strategic players can gain at the expense of those who do not manipulate their preferences, which creates justified envy. To capture this in our laboratory environment, we exogenously impose being sincere on a subset of agents when playing the Boston mechanism: for these students, the induced preferences are automatically submitted to the mechanism. The remaining students can submit their preferences freely. This design feature ensures common knowledge about the existence of individuals who fail to strategize.[3] Moreover, by forcing some agents to be sincere and by choosing the simple market with fully correlated preferences, we ensure existence of a unique (Nash) equilibrium outcome in the Boston mechanism, where the sincere students are matched to the worst schools. Therefore, we remove any strategic uncertainty that would occur when voting for a matching procedure with multiple equilibrium outcomes.

3.1 Experiment 1

The first experiment studies voting decisions of subjects who know their performance rank, called stakeholders, and of those who do not know their rank. We say that the latter subjects are placed behind a veil of ignorance. Whether a subject takes on the role of a stakeholder, who is informed of his rank, or is placed behind a veil of ignorance, is implemented using a between-subject design, randomized at the session level.

3.1.1 Design of Experiment 1

Priorities of students At the beginning of the experiment, the students are randomly assigned their rank. These ranks determine the priority of students at schools where a lower rank number means a higher priority (rank 1 is best, rank 2 second best, etc.). In the experiment, we use neutral wording and refer to the subjects in different ranks as student 1, student 2, etc.

Matching market The experimental matching problem consists of eight students and five schools. Schools A and B have one seat each while schools C, D, and E have two seats each. Students have identical preferences over schools. The priority of students at

[3] We chose this design feature since our interest is *not* in the question whether in practice agents play sincerely in the Boston mechanism and are exploited by strategic agents - this we take for granted, based on the existing literature. Instead, our interest is in the question under which conditions agents would prefer an alternative mechanism that prohibits justified envy.

schools is determined by their rank, and is therefore also perfectly correlated. Students participate in a school-admission game. Every participant earns 12 Euros if she receives a seat at school A, 10 Euros at school B, 8 Euros at school C, 6 Euros at school D, and 4 Euros at school E. The assignment is either based on the Boston mechanism, or the stable assortative matching is implemented. The assortative matching implies that the student in rank 1 is admitted by school A, the student in rank 2 is admitted by school B, etc.

$$\text{(Assortative)} \qquad ((1, A), (2, B), (3, C), (4, C), (5, D), (6, D), (7, E), (8, E)).$$

Nash equilibrium There is a unique Nash equilibrium outcome of the game induced by the Boston mechanism if all eight students can submit their preference lists strategically. This equilibrium outcome is the assortative matching. To see this, assume that students who know their rank list the school first that they would be matched to in the assortative matching. This is an equilibrium since no profitable deviation exists.[4]

If students 2 and 3 are forced to submit their preferences truthfully, student 4 can get into school B by listing it first, students 5 and 6 can get into school C by listing it first, and students 7 and 8 get into school D as long as they list it first, second, or third. Students 2 and 3 end up at the least attractive school E. The resulting matching is

$$\text{(Boston Nash Equil.)} \qquad ((1, A), (2, E), (3, E), (4, B), (5, C), (6, C), (7, D), (8, D)).$$

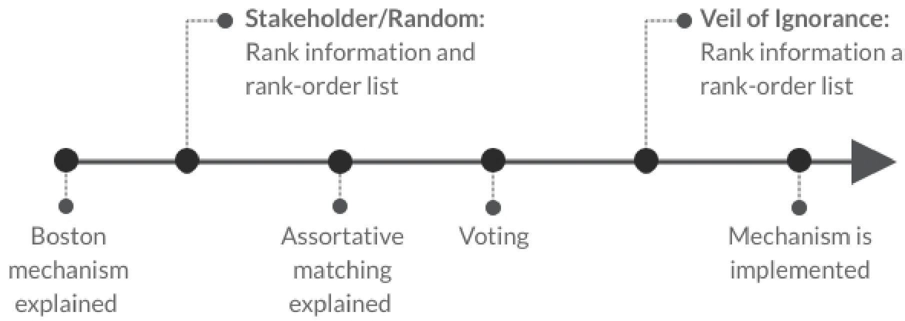
Analogous equilibrium outcomes occur if students 3 and 4, or 4 and 5 are sincere. All students in ranks below the sincere students move up to a better school while the sincere students are admitted to school E.

Experimental procedures Figure 1 presents a schematic overview of Experiment 1. At the beginning of the experiment, the Boston mechanism is introduced and explained to the subjects.[5] Subjects are informed that the students in ranks 2 and 3 cannot choose freely, but are forced to submit their true rank-order list. We require these students to be

[4]Note that there are multiple equilibrium strategies, since the ranking of schools after the first does not matter. Moreover, students 7 and 8 can submit any ranking as long as school E is on their list. Despite multiple equilibrium strategies, the equilibrium outcome is unique since the congested schools (A, B, C, and D) are filled in the first round of the Boston mechanism based on students' first ranked school.

[5]In the instructions of Experiment 1, we refer to the Boston mechanism as Procedure 1 and to the procedure that implements the assortative matching as Procedure 2. In contrast, in Experiment 2, we balanced at the session level which mechanism was Procedure 1 and 2, respectively.

Figure 1: Overview of Experiment 1



sincere in order to maximize the number of students with a predicted difference in outcomes between the two mechanisms. For the student in rank 1 only, the equilibrium outcome in the Boston mechanism is equivalent to her assortative matching outcome.

The subjects can explore the Boston mechanism with the help of an on-screen tool that lets them simulate the outcomes for different rank-order lists. After this trial phase, subjects work on a quiz to make sure they have understood the Boston mechanism. The students are asked to submit their rank-order lists of the schools for the Boston mechanism. Afterwards, we elicit their beliefs about the first choice of all other students. These beliefs are compared to the actual first choices. If the beliefs are correct, subjects earn an additional 2 Euros. The elicited beliefs permit us to study whether subjects expect others to play the equilibrium strategy or to deviate.

Subjects can vote on the mechanism that is used to allocate students to schools. The voting outcome is determined by the random dictator rule. The randomly chosen subject whose voting decision is implemented pays 0.10 Euros. If this subject has decided to abstain from voting, she pays nothing and a random draw determines which mechanism is applied. Note that independent of the voting outcome, all subjects submit their rank-order list for the Boston mechanism. This removes the possible motive to vote for the Boston mechanism in order to be able to play it afterwards.

Treatments and hypotheses There are two treatments that differ with respect to when subjects learn about their ranks and submit their preference lists. In Stakeholder/Random, subjects vote after their rank is announced and after they have submitted their rank-order list. In the Veil of Ignorance treatment, subjects vote and submit their preference list before the rank is announced.[6] Hence, in the latter treatment, they vote behind a veil of ignorance, not knowing their rank or whether or not they can submit a rank-order list that differs from their true preferences.

In **Stakeholder/Random**, subjects have full information about their rank when voting. Following Pathak and Sönmez (2008), we hypothesize that they vote for the mechanism that yields a higher payoff for them. Thus, the subject in rank 1 is indifferent, since she earns the same expected payoffs in both mechanisms. All other subjects have a strict preference.

Hypothesis 1. *In the Stakeholder/Random treatment, sincere subjects vote for the assortative matching while subjects in ranks below the sincere subjects vote for the Boston mechanism.*

In the **Veil of Ignorance** treatment, subjects vote *before* they know their rank and their ability to strategize. As ranks are determined randomly, both mechanisms have the same distribution of payoffs *ex ante*, but voting is costly. Therefore, assuming standard neoclassical preferences and beliefs implies that subjects abstain from voting. However, if subjects have fairness preferences, we conjecture that they prefer a matching that is free of justified envy. Hence, our hypothesis for the Veil of Ignorance treatment allows for both payoff-maximizing abstention and for votes that express a preference for no justified envy.

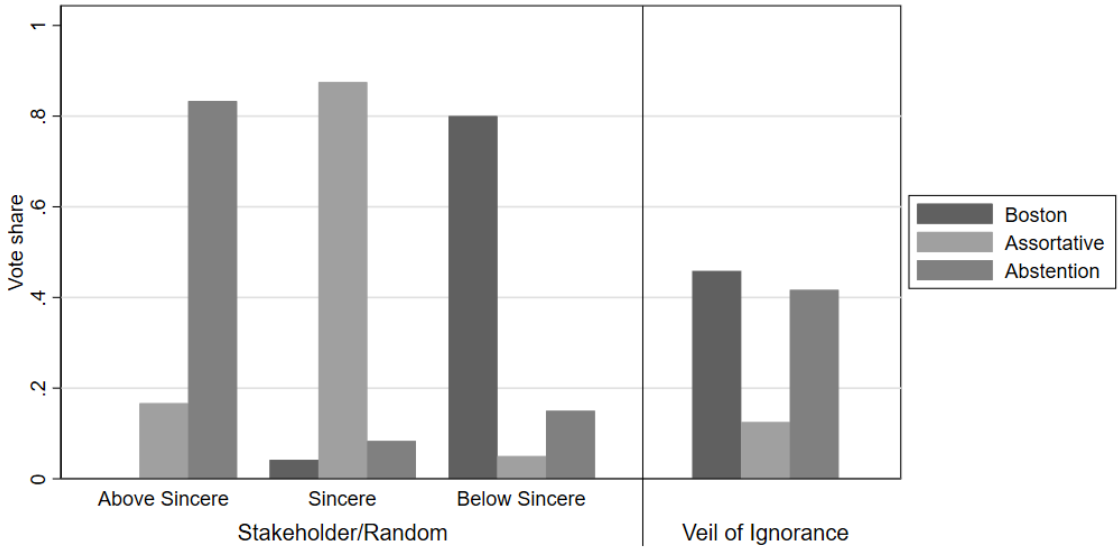
Hypothesis 2. *(a) [Payoff maximization] Subjects in the Veil of Ignorance treatment abstain from voting. (b) [Fairness] If subjects vote in the Veil of Ignorance treatment, they vote for the assortative matching.*

Overall, 96 subjects participated in the Stakeholder/Random treatment and 48 subjects in the Veil of Ignorance treatment.[7] Subjects received a 5 Euro show-up fee. Voting costs were 10 cents. Subjects earned on average 12.28 Euros with the sessions lasting on average 60 minutes. The experimental software was programmed using z-Tree (Fischbacher, 2007) and subjects were recruited using ORSEE (Greiner, 2015).

[6] In the Veil of Ignorance treatment, we had to ask for the rank-order list after the voting decision, since the rank-order list can only be elicited if the rank is known.

[7] Table A.1 in the Appendix presents descriptive statistics of the sample.

Figure 2: Vote shares in Stakeholder/Random and Veil of Ignorance (Experiment 1)



Notes: The figure shows the vote share for the respective procedure. In treatment Stakeholder/Random we divide the sample into ranks above sincere (n=12), sincere (n=24), and below sincere (n=60), and for Veil of Ignorance we show the full sample (n=48).

3.1.2 Results of Experiment 1: Voting

In Figure 2, we show the vote shares in both the Stakeholder/Random and the Veil of Ignorance treatment. Since in Stakeholder/Random the incentives to vote depend on the rank, we show the vote shares separately for subjects who have to submit their true preference lists (sincere, i.e., ranks 2 and 3), and for subjects in the ranks above and below the sincere students.

In Stakeholder/Random, we observe that subjects vote predominantly in line with their self-interest. Of the subjects in the first rank who are indifferent ("above sincere"), 83.3% abstain from voting and do not pay the voting costs as predicted. Moreover, 87.5% of sincere students who benefit from the assortative matching compared to the equilibrium payoff of the Boston mechanism vote for the assortative matching. Finally, 80.0% of subjects who benefit from the Boston mechanism because they have ranks below the sincere students ("below sincere") vote for the Boston mechanism. Fisher's exact tests confirm that the vote shares between the three groups differ significantly from each other (in all tests $p < 0.001$). These results support Hypothesis 1:

Result 1. *A majority of subjects in Stakeholder/Random vote for the mechanism with the higher individual payoff. Thus, most sincere subjects vote for the assortative matching while most subjects in the ranks below the sincere subjects vote for the Boston mechanism.*

Next we turn to the Veil of Ignorance treatment. The majority of subjects are willing to participate in costly voting: only 41.7% of subjects abstain. Moreover, 45.8% of subjects (78.6% of voters) vote for the Boston mechanism, while only 12.5% (21.4% of voters) vote for the assortative matching. A one-sided binomial test rejects the hypothesis that more subjects vote for the envy-free assortative matching outcome than for the Boston mechanism ($p < 0.001$).[8] Thus, in contrast to Hypotheses 2a and 2b, we find:

Result 2. *In contrast to prediction (a), 58.3% of subjects in the Veil of Ignorance treatment do not abstain from voting. In contrast to prediction (b), 45.8% vote for the Boston mechanism while only 12.5% vote for the assortative matching.*

Result 2 is in line with the view that individuals have preferences over matching procedures, even when they are impartial due to the veil of ignorance. However, only few subjects exhibit a preference for a mechanism that implements the envy-free matching. Instead, many subjects vote for the Boston mechanism that generates allocations with justified envy.

3.1.3 Results of Experiment 1: Preference lists and outcomes of Boston mechanism

It is possible that subjects behind the veil of ignorance vote for the Boston mechanism because they perceive it as fairer than the assortative matching. Alternatively, subjects may believe that they are smarter than others in playing the Boston mechanism, allowing them to earn higher payoffs than in the assortative matching. To investigate this alternative explanation, we analyze how subjects play the Boston mechanism and what they believe about other subjects' choices.

Table 1 shows how many students choose the equilibrium strategy.[9] We observe that a large fraction of subjects exhibit equilibrium behavior: in Stakeholder/Random the share

[8] Fisher's exact tests show that the distribution of votes in Veil of Ignorance is significantly different from the Stakeholder/Random Treatment (in all tests $p < 0.01$).

[9] Since students in ranks 2 and 3 are forced to submit their true preferences, it is the best response by the subject in rank 1 to list A first, in rank 4 to list B first, in ranks 5 and 6 to list C first, and in ranks 7 and 8 to list D at the first, second, or third position on the list.

Table 1: Equilibrium preference submission decisions in Stakeholder/Random and Veil of Ignorance (Experiment 1)

Rank	Stakeholder/Random	Veil of Ignorance	Fisher's exact test
Above Sincere (Rank 1)	12/12 (100.0%)	5/6 (83.3%)	p=0.333
Below Sincere (Rank 4-6)	27/36 (75.0%)	12/18 (67.0%)	p=0.536
Below Sincere (Rank 7-8)	21/24 (87.5%)	11/12 (91.7%)	p=1.000
Total	60/96 (83.3%)	28/36 (77.8%)	p=0.600

Notes: This table shows the fraction of subjects who play the equilibrium strategy in the Boston mechanism by treatment. The best response requires students in rank 1 to list school A as first choice, for students in rank 4 to list school B first, for students in ranks 5 and 6 to put school C first, and for students in ranks 7 and 8 to list school C as their first, second, or third choice.

is 83.3% and in Veil of Ignorance it is 77.8%, with no significant differences between the treatments. Hence, subjects seem to understand the mechanism well.

Next, we study the expectations about others' behavior in the Boston mechanism. If subjects believe that others make mistakes and do not play the equilibrium strategy and believe that they will be able to exploit this, the high vote share for the Boston mechanism behind the veil of ignorance could be explained by self-interest. Table 2 displays the fraction of subjects who expect other students to list the first choice predicted in equilibrium.[10] The majority of subjects in both treatments expect other students to do so, ranging from 90.5% to 76.2% for the Veil of Ignorance treatment. In the third column, we only consider those subjects who play the equilibrium strategy themselves and who are therefore likely to understand what the equilibrium outcome is ("Own Equilibrium Strategy"). Among those subjects, an even larger share expects others to choose the equilibrium strategy. These elicited beliefs limit the scope for explaining the high vote share for the Boston mechanism in the Veil of Ignorance treatment as resulting from expectations that others deviate from equilibrium play.

[10]To keep the task simple, we have only elicited expectations about the first choice of other students. For students in ranks 1 to 6 the first choice fully determines equilibrium behavior, hence, we show these ranks in Table 2. To determine if subjects in ranks 7 and 8 are expected to exhibit equilibrium behavior, we would need the expectations about their second and third choice.

Table 2: Expectations of other subjects' equilibrium choices

		Veil of Ignorance	
Rank	Stakeholder/Random	Total	Own Equilibrium Strategy
Above Sincere (Rank 1)	75/84 (89.3%)	38/42 (90.5%)	22/23 (95.7%)
Below Sincere (Rank 4)	63/84 (75.0%)	32/42 (76.2%)	22/24 (91.7%)
Below Sincere (Rank 5)	58/84 (69.0%)	32/42 (76.2%)	22/25 (88.0%)
Below Sincere (Rank 6)	57/84 (67.9%)	32/42 (76.2%)	21/23 (91.3%)

Notes: This table shows the reported expectations about other subjects' equilibrium behavior regarding their first choice. This means students in rank 1 list school A first, students in rank 4 list school B first, and students in ranks 5 and 6 list school C first. The last column is based on subjects in the Veil of Ignorance treatment who play the equilibrium strategy themselves. Note that students 7 and 8 list school D first, second, or third in equilibrium but we did not elicit beliefs about second or third choices.

3.1.4 Open Questions after Experiment 1

In Experiment 1, we find that some subjects prefer the asssortative matching outcome that is free of justified envy. However, more subjects vote for the Boston mechanism instead. We cannot rule out that this finding is evoked by two design features of the experiment.

First, the veil-of-ignorance approach of Experiment 1 does not fully mute self-interest in voting over matching procedures. Subjects can expect most others to deviate from the equilibrium strategy in the Boston mechanism, and may expect to earn higher payoffs as a result.

Second, it is possible that the student ranks in Experiment 1, which were determined randomly, are not perceived as creating strong entitlements. The theoretical concept of envy freeness is silent about what the priorities are based on. It could be that no justified envy is only considered to be important when the priorities are based on merit of some kind—which could then explain the low share of votes for the assortative matching in Experiment 1.

Hence, the question arises whether the findings of Experiment 1 hold up when self-interest can be excluded and stronger entitlements are created by basing the priorities on individual merit. To address this, we conduct another experiment.

3.2 Experiment 2

To exclude the possibility that material self-interest influences the voting decision, we let subjects vote over matching mechanisms to be implemented for *another* group of subjects. The voting decision of these spectators, who do not participate in the matching procedure, does not affect their own payoffs. Hence, the voting behavior of the spectators yields an alternative and direct measure of fairness preferences over matching mechanisms. Moreover, in Experiment 2, the matching priorities are determined by a real-effort task, instead of being based on luck.

3.2.1 Design of Experiment 2

Matching market We implement the same matching market and procedures as in Experiment 1. As before, there are eight students in every group who apply for seats at five schools. In the Boston mechanism, two students are sincere, i.e., they have to submit their true preferences.[11]

Priorities of students The relative performance of the students in a real-effort task determines their rank and thereby their priority at schools. Students have to count zeros in matrices consisting of zeros and ones for 10 minutes in the first part of the experiment. The task is based on Abeler *et al.* (2011), and a screenshot is provided in Figure B.9 in the Appendix. During these 10 minutes, subjects are allowed to browse the Internet by clicking an on-screen button. This outside option was meant to make the ranks even more legitimate, since subjects had an attractive alternative to working. Responses in the post-experimental questionnaire indicate that subjects consider higher payoffs to students with higher ranks in Experiment 2 as more merit-based than in Experiment 1.[12]

When performing the real-effort task, subjects do not know that they will participate in a school-choice game later in the experiment. They are only told that a better performance will, on average, pay out in the experiment. We ensure this by randomizing in a between-

[11]Due to a change in the lab rules with regards to the payment of experimental subjects between Experiment 1 and Experiment 2, the matching payoffs in Experiment 2 were slightly increased (see Appendix *B*.3.1).

[12]As illustrated in Figure A.1, subjects in Experiment 2 ("Effort") are more likely to agree with the statement "In the assortative matching everyone receives what she deserves" than in Experiment 1 ("Random"). According to a Wilcoxon rank-sum test, the difference in approval between Effort and Random is strongly significant ($z = 8.376, p < 0.001$).

group design which ranks are forced to be sincere: ranks 2 and 3, 3 and 4, or 4 and 5

Treatments There are two treatments in which we elicit preferences over mechanisms: Stakeholder/Effort and Spectator. In Stakeholder/Effort, subjects vote on the mechanism that will assign them to schools, similarly to subjects in treatment Stakeholder/Random in Experiment 1. In the Spectator treatment, subjects vote on the mechanism that will assign *other* subjects to schools in later sessions. These later sessions are called *Player* sessions.

Stakeholder/Effort Subjects first perform the counting-zeros task that determines their ranks. After the task, subjects receive feedback about their performance rank and are introduced to the school-choice problem. The information about who is sincere is given to the students after the explanation of the matching procedures.

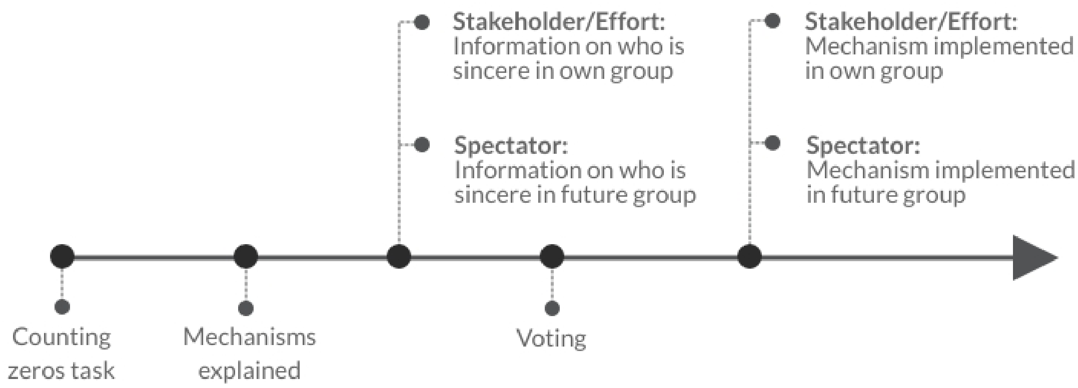
Afterwards, subjects are asked to vote whether the Boston mechanism or the assortative matching should be applied to their group. Voting costs 0.05 Euros, while subjects pay nothing if they abstain. This low cost of voting prevents those subjects who are indifferent between the two options from voting. The computer randomly picks one of the subjects and implements her choice. If a subject is picked who abstained, a random draw selects the procedure to be implemented. If the Boston mechanism is selected, subjects in the respective group are asked to submit their preference lists for the Boston mechanism. Finally, participants are informed about their payoffs.

Spectator In the Spectator treatment, subjects vote on the matching procedures used for school assignments in the future Player sessions. Hence, in this treatment we remove any personal stakes in the outcome of the vote and elicit preferences from fully impartial subjects.

The experimental design of the Player sessions is exactly the same as for subjects in Stakeholder/Effort. The only difference is that the players cannot vote on the procedures: the procedure that applies to their school-choice problem is determined by subjects in the Spectator sessions that take place before.

The spectators receive the original instructions of the participants in the Player sessions. They are told at which time the respective Player session will take place to enhance the spectators' trust that their voting decision is a real choice. Spectators are given the

Figure 3: Overview of Experiment 2



opportunity to try out the counting-zeros task (without performance feedback) and to solve the same comprehension questions as the players. To incentivize spectators to read the instructions carefully, they are paid for correctly answering the comprehension questions.[13]

After having read the instructions for the Player sessions, spectators are invited to vote for one of the matching procedures. For every group of eight players, there is a group of eight spectators. Each group of spectators is asked to vote for the implementation of the matching procedure for one group of players, after receiving information on who is sincere in this group (either players in ranks 2 and 3, 3 and 4, or 4 and 5). The voting procedure is the same as in Stakeholder/Effort: One spectator is randomly selected, and her voting choice of the mechanism will be implemented if she has decided to pay the 0.05 Euro voting fee. Thus, a spectator is pivotal with the same probability, namely ($\frac{1}{8}$), as the voters in the Stakeholder/Effort treatment. Figure 3 presents a schematic overview of Experiment 2.

Hypotheses For treatment Stakeholder/Effort, we expect payoff-maximizing voting decisions by the stakeholders, as in Experiment 1. Again, subjects in rank 1 are indifferent whereas all others are strictly better off under one of the two mechanisms.

[13]In addition to the show-up fee, spectators receive 5 Euros for answering comprehension questions about the matching procedures and 2 Euros for answering comprehension questions about the voting stage. If subjects do not give the correct answer on the first attempt, they can try another time or ask the experimenter for clarification.

Hypothesis 3. *In Stakeholder/Effort, sincere subjects vote for the assortative matching while subjects in ranks below the sincere subjects vote for the Boston mechanism.*

We now turn to the spectators. Since it is not payoff-relevant for spectators which mechanism is selected and since voting is costly, subjects with standard preferences are expected to abstain from voting. In contrast, if subjects have fairness preferences, we conjecture that they prefer the assortative matching, which ensures that the outcome is envy free and the payoffs reward performance in the real-effort task. Our next hypothesis formulates both possibilities, payoff-maximizing abstention and votes that express a fairness preference for the assortative matching:

Hypothesis 4. *(a) [Payoff maximization] Subjects in the Spectator treatment abstain from voting. (b) [Fairness] If subjects vote in the Spectator treatment, they vote for the assortative matching.*

There are 96 subjects each in the Stakeholder/Effort, the Spectator, and the Player sessions.[14] Participants received a show-up fee of 8.05 Euros in the Stakeholder/Effort treatment, and 10.05 Euros in the Spectator treatment. The experimental sessions of both treatments lasted on average 60 minutes. Average earnings (including the show-up fee) were 16.92 Euros in the Stakeholder/Effort and 17.05 Euros in the Spectator treatments.[15]

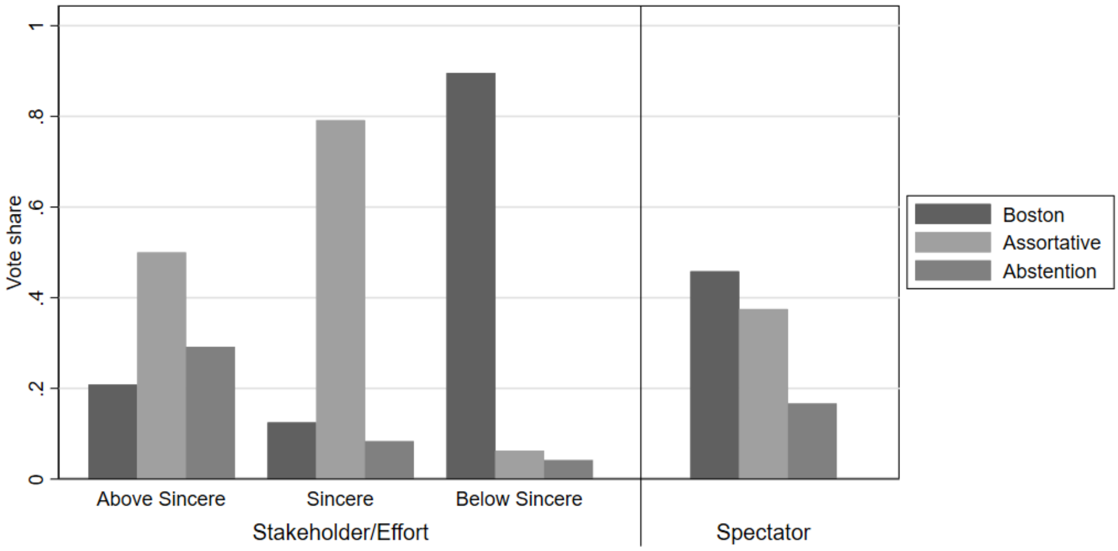
3.2.2 Results of Experiment 2: Voting

The share of votes for the Boston mechanism and the assortative matching as well as the share of abstentions are displayed in Figure 4. Similar to Experiment 1, most stakeholders vote for the mechanism that maximizes their payoff. While 79.2% of those who are forced to be sincere vote for the assortative matching, 89.6% of those who are in the lower ranks vote for the Boston mechanism. Only 29.2% of students with better ranks than the sincere students ("above sincere") abstain, although their equilibrium payoffs are the same under both mechanisms. Among them, 50.0% vote for the assortative matching while 20.8% vote for the Boston mechanism. Results from Fisher's exact tests show that the vote shares of "below sincere" students are significantly different from "above sincere" ($p < 0.001$) and "sincere" ($p < 0.001$), but that the vote shares of "sincere" and "above sincere" are

[14] Table A.1 in the Appendix presents descriptive statistics of the sample.

[15] In the Player treatment, subjects received a show-up fee of 8.00 Euros and the average earnings were 16.88 Euros.

Figure 4: Vote shares in Stakeholder/Effort and Spectator



Notes: The figure shows the vote share for the respective procedure. In Stakeholder/Effort we divide the sample into ranks above sincere (n=24), sincere (n=24), and below sincere (n=48), and for Spectator we show the total sample (n=96).

not significantly different from each other at conventional levels ($p = 0.092$). In sum, our findings support Hypothesis 3:

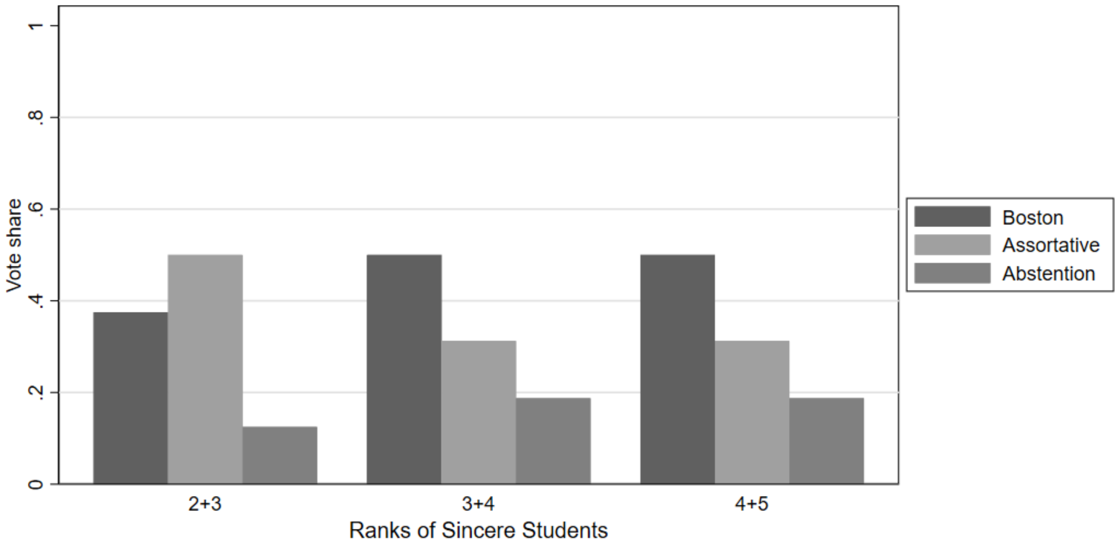
Result 3. *In line with the prediction, a large majority of subjects in Stakeholder/Effort vote for the mechanism that maximizes their payoff. Thus, most sincere subjects vote for the assortative matching while most subjects on ranks below the sincere students vote for the Boston mechanism.*

In the Spectator treatment, subjects do not have any personal stakes in the voting outcome. Nevertheless, only 16.7% abstain and thereby save on the voting costs. Hence, we reject Hypothesis 4a that subjects are purely motivated by self-interest. Instead, their voting behavior can be understood as an expression of fairness concerns.

Spectators favor the Boston mechanism and the assortative matching in similar proportions. Overall, 45.8% of spectators (55.0% of voters) vote for the Boston mechanism while 37.5% (45.0% of voters) vote for the assortative matching.[16] Thus, in contrast to

[16] According to Fisher's exact tests the voting pattern in Spectator is significantly different from that of the "sincere" ($p = 0.001$) and "below sincere" ($p < 0.001$) students in treatment Stakeholder/Effort but there is only a marginally significant difference from the subjects ranked "above sincere" ($p = 0.062$).

Figure 5: Vote shares in Spectator by ranks of sincere students



Notes: The figure shows the vote share for the respective procedure. The sample is divided into three subgroups depending on which ranks are sincere (n=32 each).

Hypothesis 4b, we do not find that more spectators vote for the assortative matching than for the Boston mechanism (one-sided binomial test, $p < 0.05$). If anything, a larger fraction of subjects favor the Boston mechanism.

Result 4. *In contrast to prediction (a), 83.3% of spectators do not abstain from voting. Moreover, contradicting prediction (b), 45.8% of spectators vote for the Boston mechanism, and 37.5% vote for the assortative matching.*

In Experiment 2, we find that a non-negligible share of subjects vote for the envy-free matching, suggesting that a preference for envy freeness is strengthened by the performance-based priorities. However, the share of subjects that vote for the Boston mechanism is still substantial, and in fact is almost identical as in Experiment 1, pointing toward a persistent preference for the Boston mechanism.[17]

Since we varied which subjects are forced to be sincere, we can study the spectators' reaction to this. Figure 5 shows suggestive evidence that spectators are more likely to vote

[17] In Table A.2 in the Appendix, we report results on subjects' rank-order lists if the Boston mechanism was selected (ten groups in the Stakeholder/Effort treatment and six groups in the Spectator treatment). Table A.3 displays that most subjects expect others to submit their first choice in line with the Nash equilibrium.

for the assortative matching when sincere students have higher ranks. In the treatment where sincere students have ranks 2 and 3, the proportion of spectators voting for the assortative matching is 50%, compared to only 31% when sincere students have ranks 3 and 4, or 4 and 5. The share of spectators voting for the Boston mechanism is 37.5% when sincere students are in ranks 2 and 3, which is 12.5 percentage points lower than when sincere students have ranks 3 and 4, or 4 and 5. This is consistent with the interpretation that fairness preferences over matching mechanisms depend on whether the students who experience justified envy have earned higher or lower priorities. However, due to the small number of observations in the corresponding cells, we do not have enough statistical power to precisely estimate these treatment differences.[18]

3.2.3 Results of Experiment 2: Reasons for voting decisions

We have found that spectators and top-ranked students engage in costly voting, even though payoff maximization would imply abstention. This finding suggests that, in addition to material concerns, individuals have fairness preferences over matching procedures. Moreover, there is heterogeneity in fairness views: some individuals prefer the assortative matching, but a considerable share of individuals prefer the Boston mechanism instead. In this section, we provide qualitative evidence from our post-experimental questionnaire supporting the interpretation that fairness concerns play a role for voting decisions. We also shed light on the specific motives underlying these particular fairness concerns.

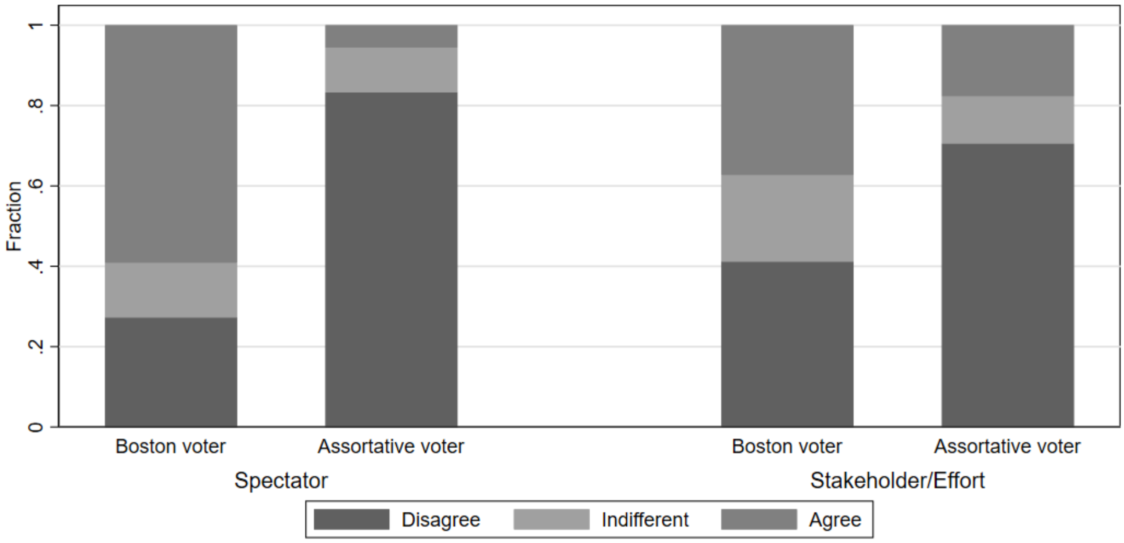
We asked subjects whether they agree that the Boston mechanism is fairer than the assortative matching.[19] Figure 6 displays the results differentiated by treatment and by the procedure the subject voted for. In the Spectator treatment, we find that those who vote for the Boston mechanism consider it to be fairer than the assortative matching, while the opposite is true for those who vote for the assortative matching. In the Stakeholder/Effort treatment, the heterogeneity is less apparent, since fairness considerations can interfere with individual payoff maximization. Nevertheless, subjects find the matching system they voted for fairer than the alternative system.

While a preference for the assortative matching is intuitive since the latter is free of justified envy, our robust finding of a preference for the Boston mechanism is puzzling

[18]For instance, when testing for differences in vote shares for the assortative matching with the sincere students in ranks 2 and 3 versus in ranks 3 and 4 or 4 and 5, Fisher's exact test (two-sided) yields a p-value of 0.117.

[19]The wording of the statement is "I find Procedure 1 fairer than Procedure 2." It is balanced at the session level whether Procedure 1 describes the Boston mechanism or the assortative matching.

Figure 6: Agreement that Boston mechanism is fairer than assortative matching



Notes: The figure shows subjects' approval with the statement in the post-experimental questionnaire by treatment and vote decision. In the experiment, the wording of the statement is "I find Procedure 1 fairer than Procedure 2," and it is balanced on the session level whether Procedure 1 refers to the Boston mechanism or the assortative matching. "Disagree" means that subjects chose "Fully disagree" or "Disagree" and "Agree" means that subjects chose "Fully agree" or "Agree."

at first sight. Why do individuals consider the Boston mechanism to be fair, even as it violates envy freeness? In the free-text part of the questionnaire, we asked spectators to explain their voting decisions. Those who voted for the assortative matching state that it is fairer than the Boston mechanism where sincere students do not get what they deserve.[20] On the other hand, spectators vote for the Boston mechanism because they believe that performance in the real-effort task should not be the sole criterion, and that smart choices should also be rewarded.[21] While only suggestive, the answers from the free-text questionnaire support the interpretation that people hold different views regarding the fairness of matching procedures: some people value freeness of justified envy, whereas others believe that clever strategic behavior should be rewarded.

4 Conclusions

We conducted two experiments to study preferences over matching procedures when some students are sincere. In both experiments, stakeholders vote in line with their self-interest, irrespective of whether their ranks are determined randomly or in a real-effort task. This supports the idea of Pathak and Sönmez (2008) that strategically sophisticated parents support the Boston mechanism.

We also study individuals who do not know their exact role or who are not directly affected by the matching procedure. Their preferences are important to understand, not least because such individuals are often part of the political decision-making process. We study these preferences by placing subjects behind a veil of ignorance and with the help of a spectator design. We find that most of the subjects are willing to pay a cost to influence which mechanism is implemented. While only few impartial subjects vote for the envy-free assortative matching when priorities are lottery-based, the vote share is larger when priorities are based on earned entitlements. However, the absence of justified envy is not

[20] For example, subjects state that they voted for the assortative matching because "the allocation is based on merit and those who deserve it have a 100% chance to get into the school they deserve," "because in the other mechanism two random students are disadvantaged," or "when subjects in lower ranks act strategically, Students 3 and 4 end up in School E, which I do not consider fair." All statements were translated from German.

[21] They state "even with a bad result in the preceding test, a smart approach to the second part can get you into a better school," "it is fairer than letting the allocation only depend on performance in the first part," "low-ranked students can arrive at a better school with a clever choice," "performance in the first part depends on certain capabilities and some people find it easier than others," or "the best always gets the highest payout; the two randomly picked subjects are outperformed by those below, if they give smart responses."All statements were translated from German.

the only fairness criterion that individuals employ. A persistently high share of individuals votes for the Boston mechanism that does not eliminate justified envy.

The experiment identifies a novel motive for why people support matching mechanisms that lead to justified envy. In spite of the presence of sincere students who are harmed by the Boston mechanism, the majority of subjects behind a veil of ignorance and of spectators display a preference for the Boston mechanism over the assortative matching. They consider the strategic environment to be fair, despite the justified envy it generates, since they tend to believe that clever strategy choices merit higher payoffs.

References

Abdulkadiroğlu, A., Pathak, P. A., Roth, A. E. and Sönmez, T. (2006). Changing the Boston school choice mechanism. *NBER Working Paper Series*, 11965.

— and Sönmez, T. (2003). School choice: A mechanism design approach. *American Economic Review*, **93** (3), 729–747.

Abeler, J., Falk, A., Goette, L. and Huffman, D. (2011). Reference points and effort provision. *American Economic Review*, **101** (2), 470–492.

Almås, I., Cappelen, A. W. and Tungodden, B. (2020). Cutthroat capitalism versus cuddly socialism: Are Americans more meritocratic and efficiency-seeking than Scandinavians? *Journal of Political Economy*, **128** (5), 1753—-1788.

Balinski, M. and Sönmez, T. (1999). A tale of two mechanisms: Student placement. *Journal of Economic Theory*, **84** (1), 73–94.

Basteck, C. and Mantovani, M. (2018). Cognitive ability and games of school choice. *Games and Economic Behavior*, **109** (1), 156–183.

Cappelen, A. W., Konow, J., Sørensen, E. Ø. and Tungodden, B. (2013). Just luck: An experimental study of risk-taking and fairness. *American Economic Review*, **103** (4), 1398–1413.

Cettolin, E. and Riedl, A. (2017). Justice under uncertainty. *Management Science*, **63** (11), 3739–3759.

de Haan, M., Gautier, P., Oosterbeek, H. and van der Klaauw, B. (2018). The performance of school assignment mechanisms in practice, `https://www.dropbox.com/s/psk2wnckh7s9wef/deHaanGautierOosterbeekVanderKlaauw2018feb.pdf` (retrieved 08/24/20).

Deffains, B., Espinosa, R. and Thöni, C. (2016). Political self-serving bias and redistribution. *Journal of Public Economics*, **134**, 67–74.

Dur, U., Hammond, R. G. and Morrill, T. (2018). Identifying the harm of manipulable school-choice mechanisms. *American Economic Journal: Economic Policy*, **10** (1), 187–213.

Durante, R., Putterman, L. and van der Weele, J. (2014). Preferences for redistribution and perception of fairness: an experimental study. *Journal of the European Economic Association*, **12**, 1059–1086.

Ergin, H. and Sönmez, T. (2006). Games of school choice under the Boston mechanism. *Journal of Public Economics*, **90** (1-2), 215–237.

Fischbacher, U. (2007). z-Tree: Zurich toolbox for ready-made economic experiments. *Experimental Economics*, **10** (2), 171–178.

Frohlich, N., Oppenheimer, J. A. and Eavey, C. L. (1987). Laboratory results on Rawls's distributive justice. *British Journal of Political Science*, **17** (1), 1–21.

Gerber, A., Nicklisch, A. and Voigt, S. (2019). The role of ignorance in the emergence of redistribution. *Journal of Economic Behavior & Organization*, **163**, 239–261.

Greiner, B. (2015). Subject pool recruitment procedures: Organizing experiments with ORSEE. *Journal of the Economic Science Association*, **1** (1), 114–125.

Hakimov, R. and Kübler, D. (2020). Experiments in matching markets: A survey. *Experimental Economics*.

He, Y. (2017). Gaming the Boston school choice mechanism in Beijing, `https://sites.google.com/site/yinghuahe/files/BM.pdf` (retrieved 04/24/20).

Johansson-Stenman, O., Carlsson, F. and Daruvala, D. (2002). Measuring future grandparents' preferences for equality and relative standing. *The Economic Journal*, **112** (479), 362–383.

Kapor, A. J., Neilson, C. A. and Zimmerman, S. D. (2020). Heterogeneous beliefs and school choice mechanisms. *American Economic Review*, **110** (5), 1274–1315.

Konow, J. (2000). Fair shares: Accountability and cognitive dissonance in allocation decisions. *American Economic Review*, **90** (4), 1072–1091.

Krawczyk, M. (2010). A glimpse through the veil of ignorance: Equality of opportunity and support for redistribution. *Journal of Public Economics*, **94**, 131–141.

Mollerstrom, J., Reme, B.-A. and Sørensen, E. Ø. (2015). Luck, choice and responsibility. An experimental study of fairness views. *Journal of Public Economics*, **131**, 33–40.

Pathak, P. A. and Sönmez, T. (2008). Leveling the playing field: Sincere and sophisticated players in the Boston mechanism. *American Economic Review*, **98** (4), 1636–1652.

Rawls, J. (1971). *A Theory of Justice*. Oxford: Oxford University Press.

Schildberg-Hörisch, H. (2010). Is the veil of ignorance only a concept about risk? An experiment. *Journal of Public Economics*, **94**, 1062–1066.

Online Appendix

A Additional Tables and Figures

Table A.1: Descriptive Statistics

	Stakeholder/ Random	Veil of Ignorance	Stakeholder/ Effort	Spectator	Player
Age (Mean)	23.708	22.854	23.094	22.802	22.646
	(3.854)	(4.946)	(4.496)	(3.272)	(3.836)
Female (Share)	0.521	0.604	0.552	0.500	0.500
Studying (Share)	0.948	0.917	0.927	0.958	0.969
Observations	96	48	96	96	96

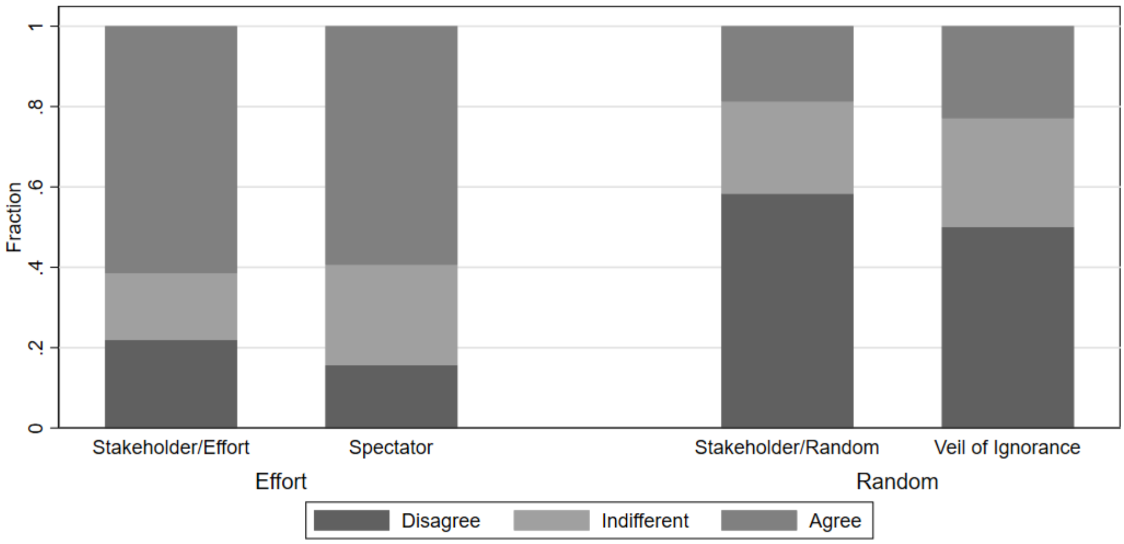
Notes: Table shows descriptive statistics of the experimental dataset. Standard deviations for continuous variables are in parentheses.

Table A.2: Equilibrium choices in Stakeholder/Effort and Player (Experiment 2)

Rank	Stakeholder/Effort	Player	Fisher's exact test
Above Sincere	18/20 (90.0%)	10/11 (90.9%)	p=1.000
Below Sincere	33/40 (82.5%)	18/25 (72.0%)	p=0.363
Total	51/60 (85.0%)	28/36 (77.8%)	p=0.415

Notes: Table shows the fraction of subjects who play the equilibrium strategy in the Boston mechanism by treatment. Best response depends partly on who are the sincere students. In all cases, students in rank 1 list school A first and students in ranks 7 and 8 list school C first, second or third. With students in ranks 2 and 3 being sincere, students in rank 4 list school B first, while students in ranks 5 and 6 list school C first. With students in ranks 3 and 4 being sincere, students in rank 2 list school B first while students in ranks 5 and 6 list school C first. With students in ranks 4 and 5 being sincere, students in rank 2 list school B first while students in ranks 3 and 6 list school C first.

Figure A.1: Approval to statement “In the assortative matching everyone receives what she deserves”



Notes: The figure shows subjects approval to the statement in the post-experimental questionnaire by treatment and vote decision. In the experiment, the wording of the statement is “In Procedure 2 everyone receives what she deserves”. “Disagree” means that subjects respond “Fully disagree” or “Disagree” and “Agree” means that subjects respond “Fully agree” and “Agree”.

Table A.3: Expectations of other subjects' equilibrium choices (Experiment 2)

Rank	Stakeholder/Effort	Player	Fisher's exact test
Above Sincere (Rank 1)	62/70 (88.6%)	37/42 (88.1%)	p=1.000
Above Sincere (Rank 2)	40/49 (81.6%)	19/21 (90.5%)	p=0.485
Above Sincere (Rank 3)	13/21 (61.2%)	12/14 (85.7%)	p=0.252
Below Sincere (Rank 4)	16/21 (76.2%)	18/21 (85.7%)	p=0.697
Below Sincere (Rank 5)	34/49 (69.4%)	21/28 (75.0%)	p=0.794
Below Sincere (Rank 6)	52/70 (74.3%)	28/42 (66.7%)	p=0.397

Notes: This table shows the reported expectations about other subjects' equilibrium behavior regarding their first choice. The equilibrium strategy depends partly on who is sincere. In all cases, students in rank 1 list school A first and in rank 6 list school C first. With students in ranks 2 and 3 being sincere, students in rank 4 list school B first, while students in ranks 5 list school C first. With students in ranks 3 and 4 being sincere, students in rank 2 list school B first, while students in ranks 5 list school C first. With students in ranks 4 and 5 sincere, students in rank 2 list school B first, while students in rank 3 list school C first. Note that students 7 and 8 list school D first, second, or third in equilibrium but we did not elicit beliefs about second or third choices.

B Instructions and Screenshots

In this section, we provide paper instructions (translated from German) and screenshots from the experiment. They are presented in the same order as during the actual experiments. In the paper instructions, treatment-specific instructions are enclosed in square brackets. Whenever screenshots are similar across treatments, only one example is shown and deviations are described in the annotations.

B.1 Experiment 1 (Paper Instructions)

The following experiment is designed to analyze decision-making behavior. For your participation, you will be paid a show-up fee of 5 Euro. During the experiment, you will be able to earn some additional money based on your choices and choices of other participants. At the end of the experiment, your earnings will be paid out in cash privately. It is therefore very important that you read these instructions carefully. Please do not talk to the other participants. If you have a question, please raise your hand. We will then come to you and answer your question in silence. Please do not ask your questions out loud.

The experiment consists of two parts. Your choice situation in the first part of the experiment is explained below. The instructions for the second part will be handed out after the first part of the experiment is completed. The instructions are the same for all participants.

For each part, you will find short comprehension questions at the end of the instructions that we ask you to answer. Afterwards, one of the experimenters will come to you to check the comprehension questions and, if necessary, to clarify open questions.

Part 1

General Information

In this part of the experiment, we emulate a procedure that allocates seats at schools to students. In this procedure, students have to apply to a central clearinghouse for a seat at a school. You and the other participants take on the role of students in the experiment.

At the beginning of the experiment, you will be divided into groups of eight participants. Each participant in a group is randomly assigned a number from 1 to 8, representing students 1 to 8. All students in a group apply for a total of eight seats at secondary schools. There are five schools A, B, C, D and E. Schools A and B each have one, and schools C, D and E each have two seats available.

Payouts

Your payout depends on which of the five schools you get a seat at. The payouts you get at a respective school are summarized in the following table:

	School A	School B	School C	School D	School E
Payout	12 Euro	10 Euro	8 Euro	6 Euro	4 Euro

Example: Suppose you get a seat at school A. Your resulting payout is 12 Euro. If you get a seat at school E, you will receive 4 Euro.

Rank-order lists

Applications for school seats are based on rank-order lists. You have to enter on-screen which school is your first, second, third, fourth and fifth choice.

The following sample screen shows you how to enter your rank-order list.

Example: Submission of rank-order list

	Erstwunsch	Zweitwunsch	Drittwunsch	Viertwunsch	Fünftwunsch
Schüler 1					
Schüler 2					
Schüler 3					
Schüler 4					
Schüler 5					
Schüler 6					
Schüler 7					
Schüler 8					

In the example, you were randomly assigned the role of Student 5. You can click in the row of that respective student on each column field and type in one of the letters A, B, C, D or E, which represent the corresponding schools (upper and lower case are not important). Please note that you can only use each letter once. You must fill in each of the fields in your row.

You can rank the schools according to their payout, i.e. the rank-order list "first: A, second: B, third: C, fourth: D, fifth: E" or choose any other ranking.

For those participants who are randomly assigned the roles of students 2 and 3, the rank-order lists are automatically created according to their payout amount: “first: A, second: B, third: C, fourth: D, fifth: E.”

The allocation procedure

Admission to the five schools is based on the submitted rank-order lists and the student numbers. The allocation procedure is as follows:

Round 1:

- Application: Each of the students 1 to 8 applies to the school that they have named as their first choice on their rank-order list.
- Admission: If the same number or fewer students apply to a school than there are seats available, all of them receive a seat. If more students have applied to a school than the seats that the school offers, the applicants with the lowest student numbers will be admitted first. For the applicants who have received a seat, the allocation procedure is over.

Round 2:

- Application: Each of the students 1 to 8, who did not get a seat in round 1, applies to the school they have named on their rank-order list as second choice.
- Admission: A school only admits participants in round 2 if there are still free seats after round 1. If the same number or fewer students apply to a school than there are seats available, they will all receive a seat. If more students apply to a school than that school's available seats, the applicants with the lowest student numbers will be admitted first. For the applicants who have received a seat, the allocation procedure is over.

Round 3

- Application: Each of the students 1 to 8, who did not get a seat in round 2, applies to the school they have named on their rank-order list as a third choice.
- Admission: A school only admits participants in round 3 if there are still free seats after round 2. If the same number or fewer students apply to a school than there are seats available, they will all receive a seat. If more students apply to a school than that school's available seats, the applicants with the lowest student numbers will be admitted first. For the applicants who have received a seat, the allocation procedure is over.

etc.

The process ends when all eight school seats have been allocated.

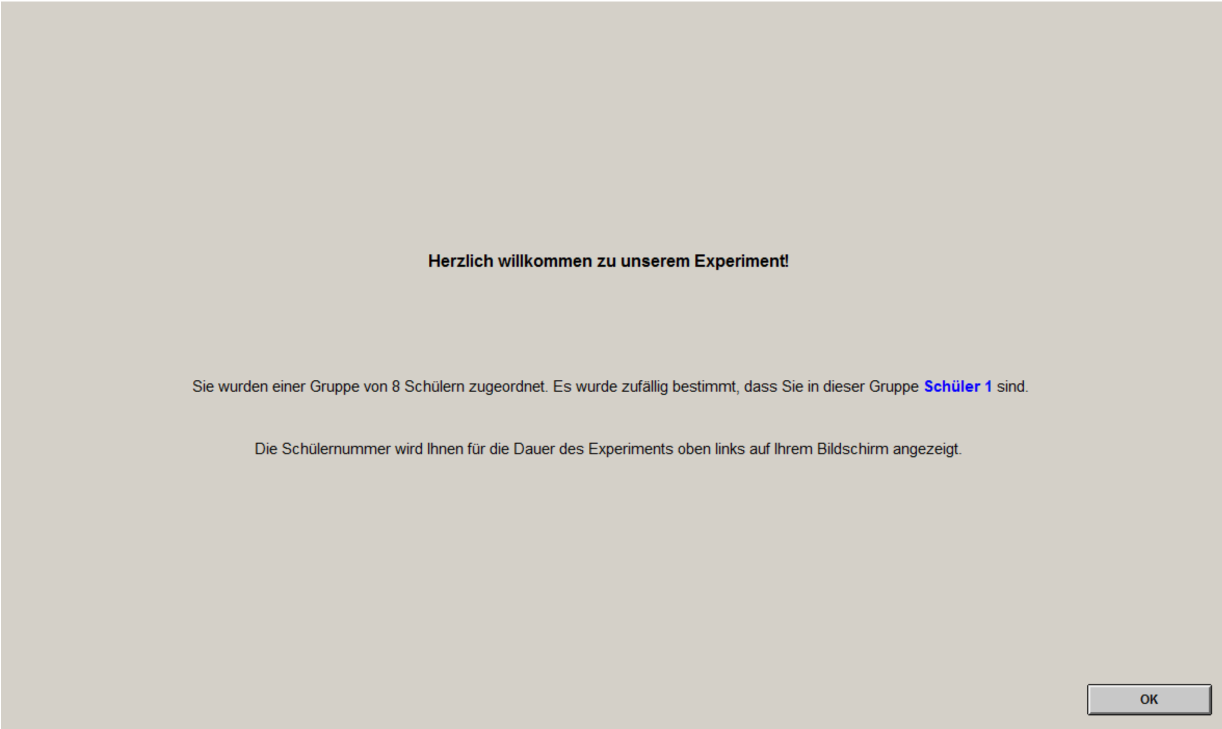
Trial Period

Before you submit your rank-order list and the school seats are allocated, you will have the opportunity to try out the allocation procedure on-screen. You can test how different rank-order lists of the participants affect the allocation of school seats. The way the trial period works will be explained later on the screen.

Comprehension Questions

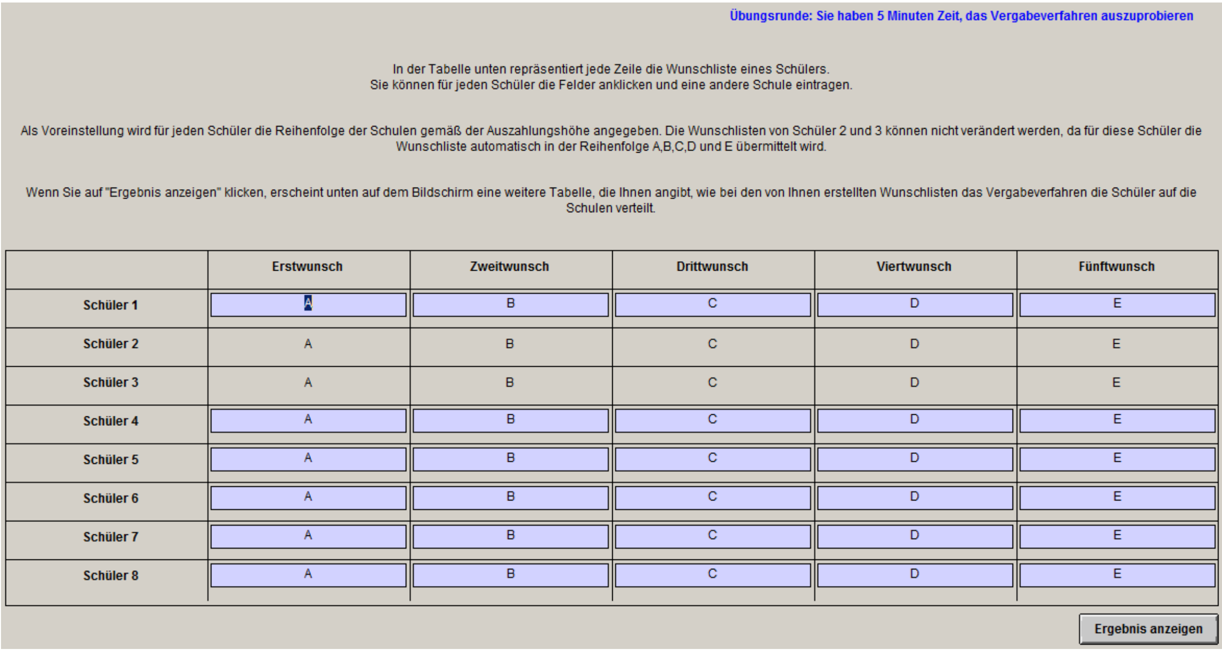
1. How many seats do schools A and B have to offer each?
2. How many seats are there at schools C, D, and E?
3. Assume that student 4 specifies school A as the first choice; students 1, 5, 6, 7 and 8 indicate a school other than School A as their first choice. Does student 4 get a seat at school A? Yes / No / Can not say
4. Assume that student 1 specifies school A as the first choice, while students 4 and 5 specify school C as the first choice. Do both get a seat at school C? Yes / No / Can not say
5. Assume that student 1, student 2, and student 3 submit school A as their first choice. Does student 4 get a higher payout if she submits school A or school B as her first choice? School A / School B

Figure B.1: Screenshot of welcome screen



Notes: The screenshot displays the welcome screen in the Stakeholder/Random treatment. Subjects are informed about their rank as determined by the random draw (here, student 1) and that the rank will be displayed on-screen for the remainder of the experiment. In contrast, the welcome screen in the Veil of Ignorance treatment informs the subjects that their rank will be revealed at a later stage.

Figure B.2: Screenshot of Trial Period



Übungsrunde: Sie haben 5 Minuten Zeit, das Vergabeverfahren auszuprobieren

In der Tabelle unten repräsentiert jede Zeile die Wunschliste eines Schülers.
Sie können für jeden Schüler die Felder anklicken und eine andere Schule eintragen.

Als Voreinstellung wird für jeden Schüler die Reihenfolge der Schulen gemäß der Auszahlungshöhe angegeben. Die Wunschlisten von Schüler 2 und 3 können nicht verändert werden, da für diese Schüler die Wunschliste automatisch in der Reihenfolge A,B,C,D und E übermittelt wird.

Wenn Sie auf "Ergebnis anzeigen" klicken, erscheint unten auf dem Bildschirm eine weitere Tabelle, die Ihnen angibt, wie bei den von Ihnen erstellten Wunschlisten das Vergabeverfahren die Schüler auf die Schulen verteilt.

	Erstwunsch	Zweitwunsch	Drittwunsch	Viertwunsch	Fünftwunsch
Schüler 1	A	B	C	D	E
Schüler 2	A	B	C	D	E
Schüler 3	A	B	C	D	E
Schüler 4	A	B	C	D	E
Schüler 5	A	B	C	D	E
Schüler 6	A	B	C	D	E
Schüler 7	A	B	C	D	E
Schüler 8	A	B	C	D	E

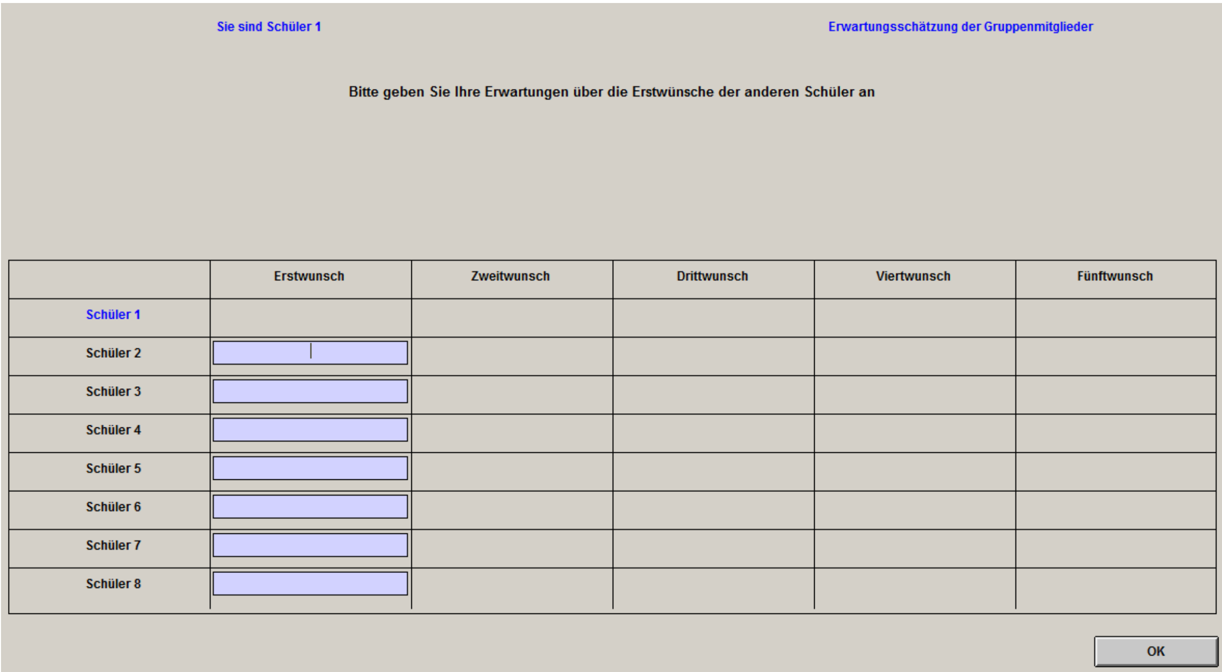
Ergebnis anzeigen

Notes: The screenshot displays the trial period in the Veil of Ignorance treatment. Subjects have five minutes to try out different rank-order lists for non-sincere students. They can change the ranking of schools in the fields with a blue background. The rank-order lists of students 2 and 3 are fixed. When clicking the button the resulting matching is shown. In treatment Stakeholder/Random, subjects see the same screen except that their rank is displayed in the top left corner of the screen.

Figure B.3: Screenshot of belief elicitation instructions (Stakeholder/Random)

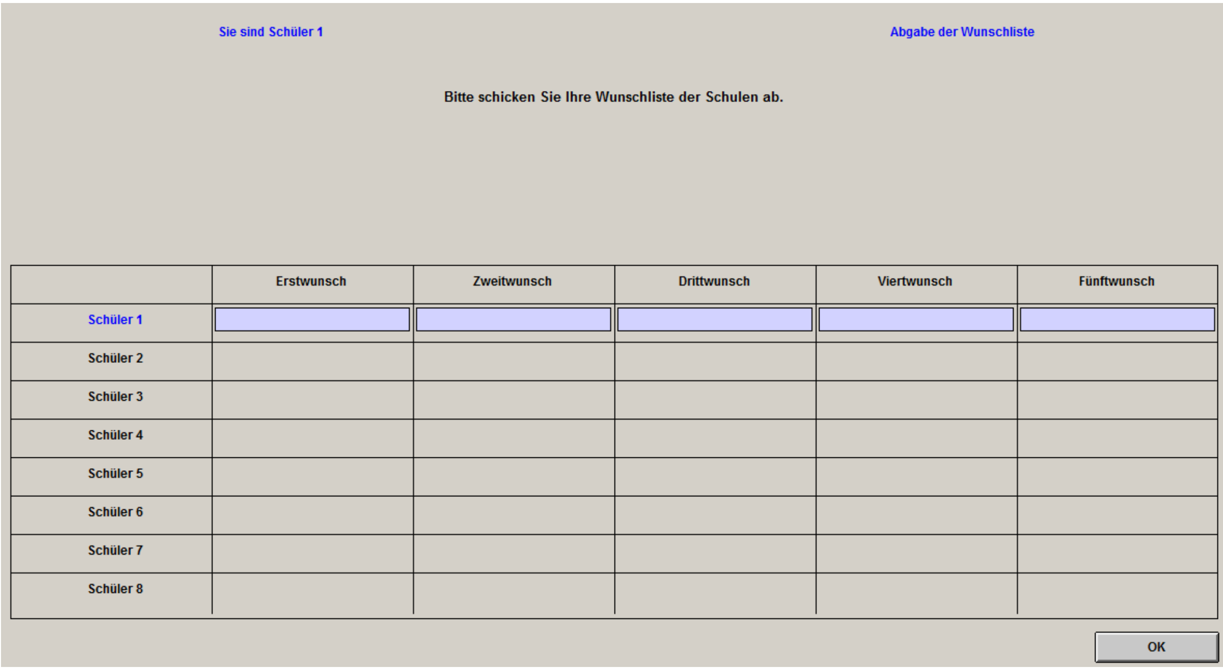
Notes: On this screen, subjects in the Stakeholder/Random treatment are informed that their expectations about other subjects' first choices in Procedure 1 (the Boston mechanism) are elicited on the next screen. If their expectations are correct, they will receive an additional 2 Euros. In the Veil of Ignorance treatment, the belief elicitation is conducted after the ranks are revealed.

Figure B.4: Screenshot of belief elicitation (Stakeholder/Random)



Notes: On this screen, the expectations about other subjects' first choices in Procedure 1 (the Boston mechanism) are elicited. In the Veil of Ignorance treatment, these expectations are elicited at a later stage.

Figure B.5: Screenshot of submission of rank order lists (Stakeholder/Random)



Notes: On this screen, subjects in the Stakeholder/Random treatment submit their rank-order lists for the Boston mechanism. The screen shows the choice situation for a subject in rank 1. In the Veil of Ignorance treatment, the submission or rank-order lists happens at a later stage.

Part 2 (Paper Instructions)

Voting

In the following, the allocation procedure presented in Part I is referred to as Procedure 1.

[Treatment Stakeholder/Random: Before you learn the result of Procedure 1, ... *]*

[Treatment Veil of Ignorance: Before you learn the draw of your student number and also before Procedure 1 is carried out, ... *]*

...you have the opportunity to vote on whether the payments for this experiment are actually made in accordance with Procedure 1, or whether the result of an alternative procedure is used, which is referred to in the following as Procedure 2. Procedure 2 leads to the following result:

Table: Result of Procedure 2

	Seat at school	Payout
Student 1	A	12 Euro
Student 2	B	10 Euro
Student 3	C	8 Euro
Student 4	C	8 Euro
Student 5	D	6 Euro
Student 6	D	6 Euro
Student 7	E	4 Euro
Student 8	E	4 Euro

The table can be read as follows: Student 1 receives a seat at School A and, thus, a payment of 12 Euro, Student 2 receives a seat at School B and 10 Euro, etc.

The voting works as follows. Each participant can decide on the screen whether to take part in the vote or to abstain. Whoever takes part in the vote can indicate whether the result of Procedure 1 or Procedure 2 should be applied.

Then, one student out of the eight students is selected at random. If this student has voted for one of the procedures, the result of the allocation procedure chosen by her is implemented for her entire group and her (and only her) payout is reduced by 10 Cent. If the randomly selected student has decided not to take part in the vote, it will be determined at random whether the result of Procedure 1 or the result of Procedure 2 will be implemented for her group. In this case of abstention, the randomly selected participant does not have to pay the 10 Cent.

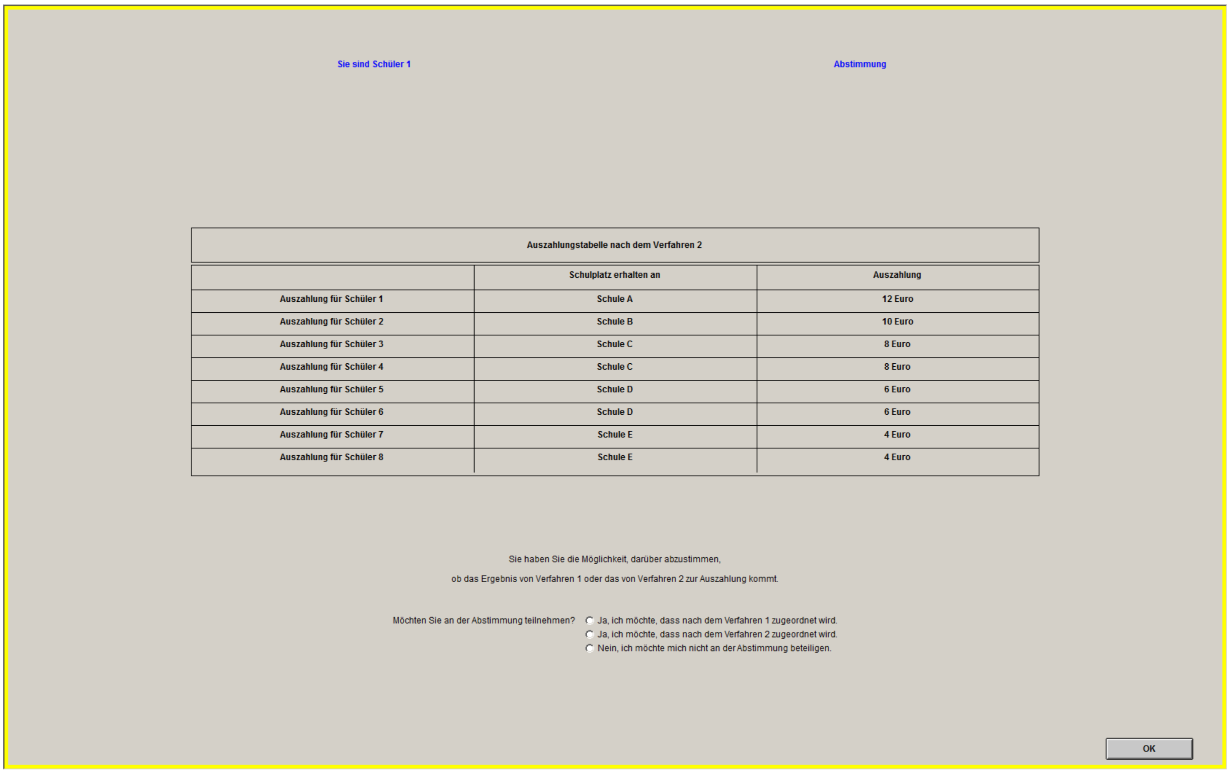
[Treatment Veil of Ignorance: Before the result of your group's voting is announced, the student numbers from 1 to 8 will be drawn. You and all other students will then be asked to enter your rank-order lists for Procedure 1 on the screen. The submission of the rank-order lists cannot be changed later, and is binding in the event that Procedure 1 is selected in your group.*]*

Afterwards, you will be informed about the voting result of your group. If the vote for your group determined that Procedure 1 should be implemented, Procedure 1 will be applied and the result of this procedure will be paid out. If Procedure 2 is chosen in your group, each student will be paid out according to Procedure 2.

Comprehension Questions

1. Assume you have voted for one of the two allocation procedures. Another participant voted for the other allocation procedure. You are chosen at random. Which allocation procedure is implemented for the group? The procedure I voted for. / The procedure the other participant voted for. / Will be decided randomly.
2. Do you have to pay 10 Cent in the example just mentioned? Yes / No
   [Treatment Veil of Ignorance:
3. The participants only submit their rank-order lists for Procedure 1 if Procedure 1 is selected in the vote. True / False
4. Rank-order lists must be submitted for Procedure 1, but not for Procedure 2. True / False
5. I choose Procedure 1 or 2 before I get my student number. True / False*]*

Figure B.6: Screenshot of voting decision



Notes: On this screen, subjects in the Stakeholder/Random treatment vote whether the payout is according to Procedure 1 (the Boston mechanism) or Procedure 2 (the assortative matching that is displayed on the screen). Subjects are asked whether they would like to participate in the vote and the options are "Yes, I would like the matching to be according to Procedure 1", "Yes, I would like the matching to be according to Procedure 2", or "No, I do not want to participate in the vote". In the Veil of Ignorance treatment, the choice situation is the same but the rank is not displayed in the top left corner of the screen.

Figure B.7: Screenshot of announcement of ranks (only Veil of Ignorance)

Notes: On this screen, subjects in the Veil of Ignorance treatment are informed about their rank as determined by the random draw (here, rank 1) and that the rank will be displayed on-screen for the remainder of the experiment. After this screen, subjects in the treatment Veil of Ignorance see the screenshots in Figures B.3 to B.5.

B.2 Experiment 2 (Paper Instructions)

Welcome to our experiment. For your participation in this experiment you will receive an amount of *[Treatment Stakeholder/Effort:* 8.05 Euro, *Treatment Spectator:* 10.05 Euro, *Treatment Player:* 8.00 Euro*]*. You can earn additional money in this experiment. It is very important that you read these instructions carefully and thoroughly. The instructions are the same for all participants.

Please do not talk to the other participants. If you have a question, please raise your hand. We will then come to you and answer your question. Please do not ask your question(s) out loud under any circumstances.

[Treatment Stakeholder/Effort & Player: The experiment consists of two parts. In the following, your choice situation for the first part of the experiment will be explained. Instructions for the second part will be handed out after the first part is completed.

In the course of the experiment you have to answer comprehension questions. Once all participants have answered the comprehension questions correctly, the experiment can continue. If you are finished answering the comprehension questions or if you have a question, please raise your hand. Someone will then come to you to clarify your question.*]*

[Spectator: In this experiment, we emulate two procedures by which school seats are allocated to students. Your task is to carefully understand these allocation procedures. At the end of the experiment, you will vote over which of these two procedures should be used for a group of future experimental subjects. In other words, in the future we will invite participants who will take on the role of students. You will decide today which allocation procedure will be used to allocate these students to hypothetical schools.

The two allocation procedures are explained in the instructions of the future experimental subjects. Next, we will hand out these instructions and ask you to read them carefully. You will be asked to answer the future participants' comprehension questions. If you answer all the comprehension questions correctly, you can earn up to 7 Euro extra. If you have a question, please raise your hand. Someone will then come to you to help.

The experiment of the future participants consists of two parts. We will first give you the instructions for the first part, in which the participants will be given a work task. Afterwards we will hand out the instructions for the second part of the experiment, in which the school seat allocation procedures are explained.

At the end of today's session, you will vote over which of the allocation procedures will be implemented for the future participants. The future participants' session will take place today in the afternoon.]

Part 1

In today's experiment, you will be divided into groups of eight participants. The composition of the groups stays the same for the entire course of the experiment.

Work Task

In this part of the experiment you have to complete a work task. This work task determines your rank from 1 to 8 in your group. The rank in your group is important for the further course of the experiment: on average, participants with a better rank receive a higher payout. It is therefore worthwhile to provide effort in the work task.

The work task is a counting task. You will be presented a series of tables with zeros and ones for a duration of 10 minutes. Your task is to count the number of zeros in each table. If you enter the correct number of zeros, you will receive a point. If you give the wrong number of zeros, you will be deducted one point. Whoever has the most points will be in rank 1; whoever has the second most points will be in rank 2; whoever has the third most points will be in rank 3, etc. After the counting task, you will be told which rank you have achieved in your group.

During the counting task, you can open the internet browser by clicking on the red button at the bottom of the screen and surf the internet in the remaining time. With the key combination Alt+Tab on the keyboard you can return to the work task at any time.

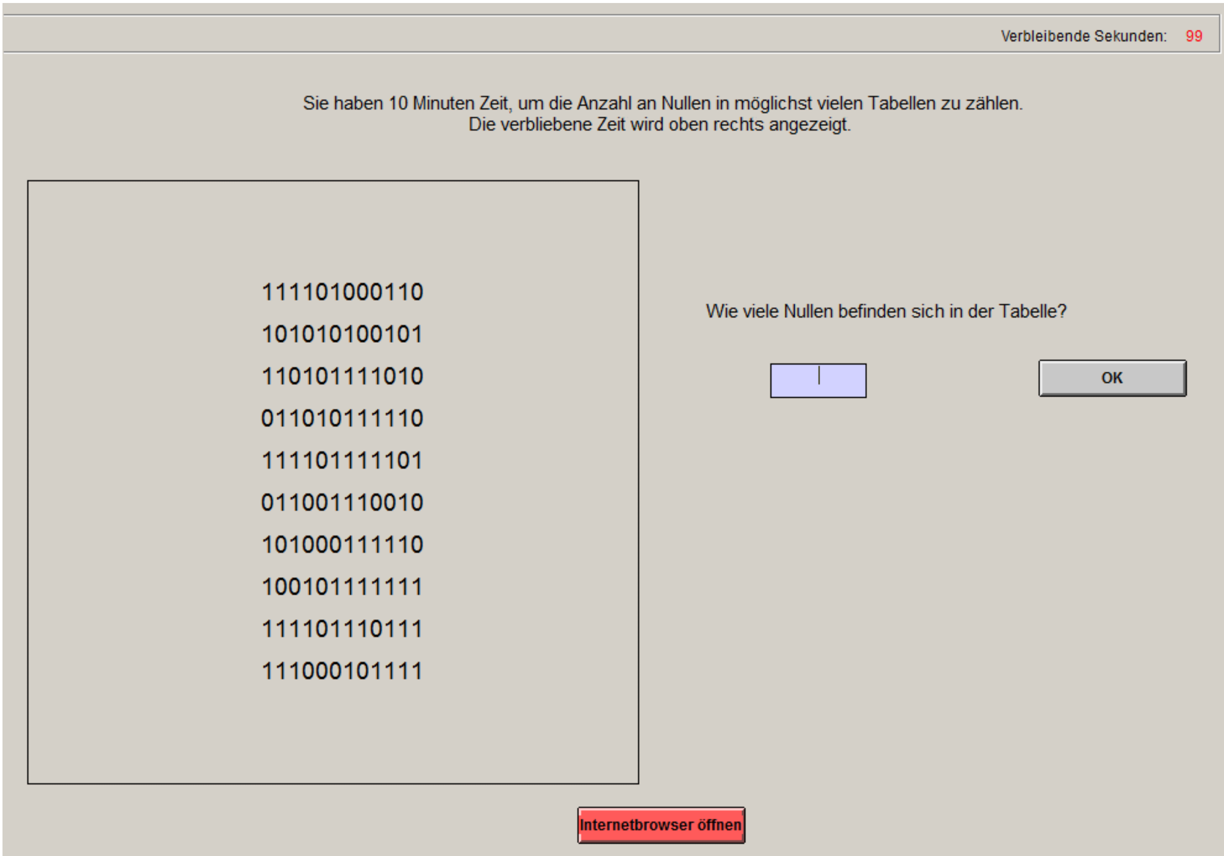
The figure below[22] illustrates the choice situation. Please wait until the work task is started on the screen.

[22]The printed instructions contain a screenshot that is the same as Figure B.9.

Figure B.8: Screenshot of welcome screen

Notes: The screenshot displays the welcome screen in the Spectator treatment. Subject are informed about the real effort task on the next screen. In particular, they are told that they have to count the number of zeros in as many tables as possible within 10 minutes. In the top left corner of the screen, subjects in the Spectator treatment are informed that this is the original screen that subjects in the Player session will see. This note is omitted in the Stakeholder/Effort and Player treatments.

Figure B.9: Screenshot of real effort task



Notes: The screenshot displays the real effort task. Subjects in the Stakeholder/Effort and Player treatments are supposed to count the number of zeros in the table on the left. (For subjects in the Spectator treatment the screen is only for illustrative purposes and their performance is without consequence.) After submitting the counted number, a new table is generated. The red button at the bottom of the screen opens a web browser, in which subjects can browse the internet. The browser is closed automatically after the time has run up.

Figure B.10: Screenshot of real effort task feedback

Notes: On this screen, subjects in the Spectator/Effort and Player treatments receive feedback on how many tables they counted correctly and which rank they achieved. In the Spectator treatment, the same screen is displayed for illustrative purposes, but instead of the number of tables and the rank three dots ("...") are displayed. Moreover, subjects are told to read the instructions for the second part of the experiment, which are handed out by the experimenter. After everyone finished reading, subjects need to enter a number announced by the experimenter to proceed.

B.3 Part 2 (Paper Instructions)

B.3.1 General Information

In this part of the experiment, we emulate two procedures that allocate seats at schools to students. You and your group from part 1 of the experiment take on the role of students. Your task is to carefully understand these allocation procedures.

[Treatment Stakeholder/Effort: At the end of the experiment, you will vote over which of these two procedures should be used for your group of participants. We will first present the procedures to you. We will then explain how the vote over the procedures works.*]*

[Treatment Player: At the end of the experiment you will learn which of the two procedures is used for your group of participants. Your payment for this experiment will be determined by the school at which you are placed.*]*

Scenario and Payout

The scenario for the two procedures is as follows. Each member of your group is assigned a number from 1 to 8, representing student 1 to student 8. Your assigned number corresponds to the rank you achieved in the first part of the experiment.

Example: If you have achieved rank 1 in the work task, your student number is 1; if you have achieved rank 8, your student number is 8.

There are five schools A, B, C, D and E, of which schools A and B each have one and schools C, D and E each have two seats to allocate. Hence, there are eight seats in total.

Your payout depends on which of the five schools you get a seat at. The payouts you receive at a respective school are summarized in the following table:

	School A	School B	School C	School D	School E
Payout	16 Euro	13 Euro	10 Euro	7 Euro	4 Euro

Example: Suppose you get a seat at school A. Your resulting payout is 16 Euro. If you get a seat at school E, you will receive 4 Euro.

The payout table is the same for all students, i.e. each student receives the highest payout at School A and the lowest payout at School E.

Procedure 1

In Procedure 1,[23] students are allocated to schools according to their rank. The allocation is as follows:

Table: Result of Procedure 1

	Seat at school	Payout
Student 1	A	16 Euro
Student 2	B	13 Euro
Student 3	C	10 Euro
Student 4	C	10 Euro
Student 5	D	7 Euro
Student 6	D	7 Euro
Student 7	E	4 Euro
Student 8	E	4 Euro

The table can be read as follows: Student 1 receives a seat at School A and, thus, a payment of 16 Euro, Student 2 receives a seat at School B and 13 Euro, etc.

Procedure 2

In Procedure 2, seats at school are allocated as follows. First, you have to enter on-screen which school is your first, second, third, fourth and fifth choice.

The following sample screen shows you how to enter your rank-order list.

Example: Submission of rank-order list

	Erstwunsch	Zweitwunsch	Drittwunsch	Viertwunsch	Fünftwunsch
Schüler 1					
Schüler 2					
Schüler 3					
Schüler 4					
Schüler 5					
Schüler 6					
Schüler 7					
Schüler 8					

In the example, you are student 1. You can click in the row of that respective student on each column field and type in one of the letters A, B, C, D or E, which represent

[23] Which procedure is Procedure 1 varies on the session level.

the corresponding schools. You can rank the schools according to their payout, i.e. the rank-order list “first: A, second: B, third: C, fourth: D, fifth: E” or choose any other ranking.

For two randomly selected students on ranks 2 to 5, rank-order lists are generated automatically according to payout, i.e. “first: A, second: B, third: C, fourth: D, fifth: E”. These students do not need to enter anything on the screen; the computer will automatically generate their rank-order list. You will be informed later on the screen for which students the rank-order list will be created automatically according to the payout amount.

Admission to the five schools is based on the submitted rank-order lists and the student numbers. The allocation procedure is as follows:

Round 1:

- Application: Each of the students 1 to 8 applies to the school that they have named as their first choice on their rank-order list.
- Admission: If the same number or fewer students apply to a school than there are seats available, all of them receive a seat. If more students have applied to a school than the seats that the school offers, the applicants with the lowest student numbers will be admitted first. For the applicants who have received a seat, the allocation procedure is over.

Round 2:

- Application: Each of the students 1 to 8, who did not get a seat in round 1, applies to the school they have named on their rank-order list as second choice.
- Admission: A school only admits participants in round 2 if there are still free seats after round 1. If the same number or fewer students apply to a school than there are seats available, they will all receive a seat. If more students apply to a school than that school’s available seats, the applicants with the lowest student numbers will be admitted first. For the applicants who have received a seat, the allocation procedure is over.

Round 3

- Application: Each of the students 1 to 8, who did not get a seat in round 2, applies to the school they have named on their rank-order list as a third choice.
- Admission: A school only admits participants in round 3 if there are still free seats after round 2. If the same number or fewer students apply to a school than there are seats available, they will all receive a seat. If more students apply to a school than

that school's available seats, the applicants with the lowest student numbers will be admitted first. For the applicants who have received a seat, the allocation procedure is over.

etc.

The process ends when all eight school seats have been allocated.

Further Steps

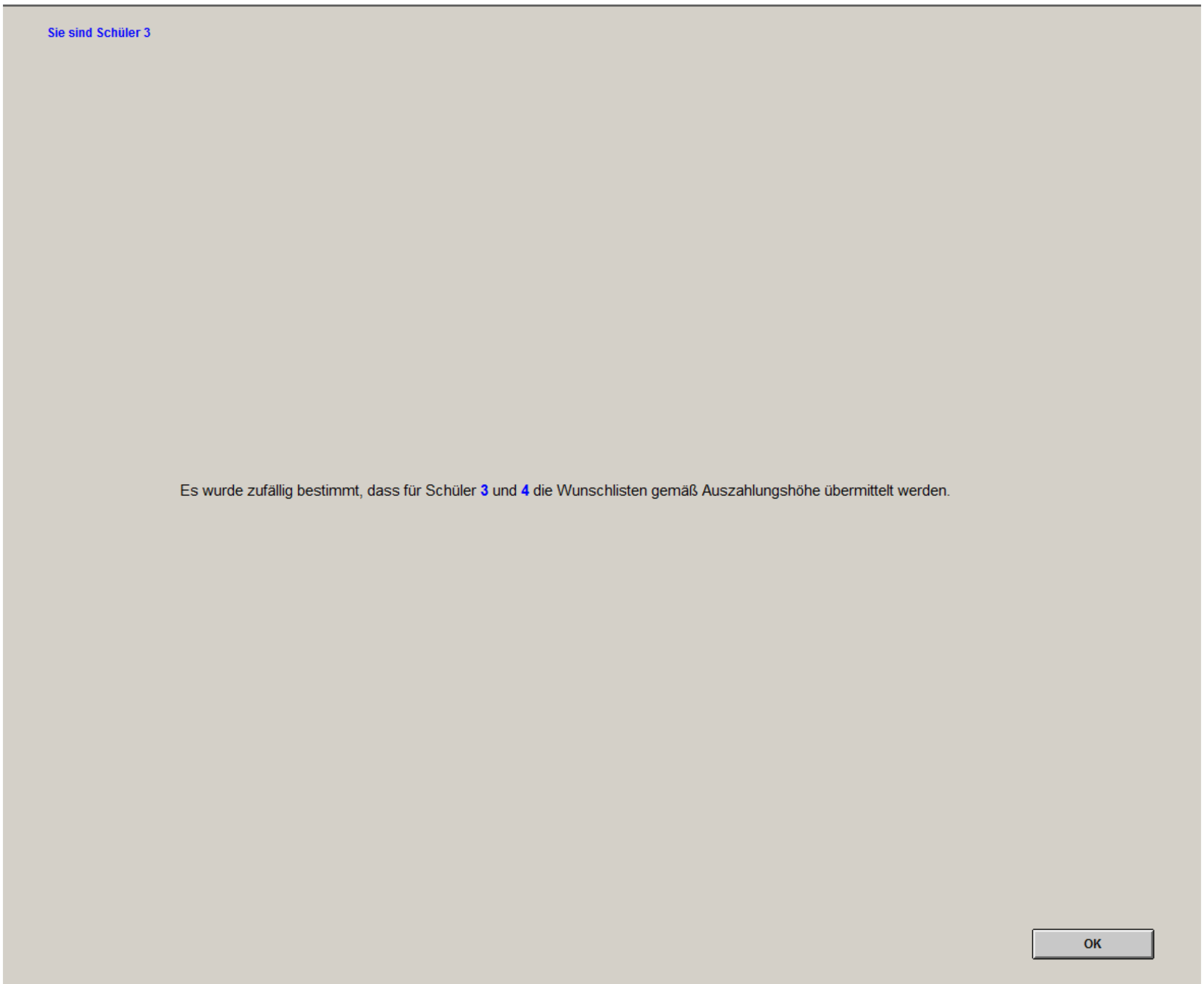
First, you will be informed on-screen for which of the two randomly selected participants the rank-order lists will be generated automatically, in case Procedure 2 is applied. Then you will have the opportunity to try out Procedure 2 on the screen. Afterwards, you will be able to vote over the allocation procedure to be used. This procedure will then be implemented for your group and your payment will depend on which school you are allocated to.

Before the experiment continues on-screen, please answer the following comprehension questions.

Comprehension Questions

1. How many seats do schools A and B have to offer each?
2. How many seats are there at schools C, D, and E?
3. What is your payment if you are allocated to school A?
4. What is your payment if you are allocated to school E?
5. In Procedure 1, schools are allocated solely on the basis of the rank achieved in the work task. True / False
6. Consider Procedure 2. Suppose students 1 and 2 give their rank-order list according to payout ("first: A, second: B, third: C, fourth: D, fifth: E"). At which school does student 3 end up if he submits "first: B, second: A, third: C, fourth: D, fifth: E" as his rank-order list?

Figure B.11: Screenshot of announcement of ranks

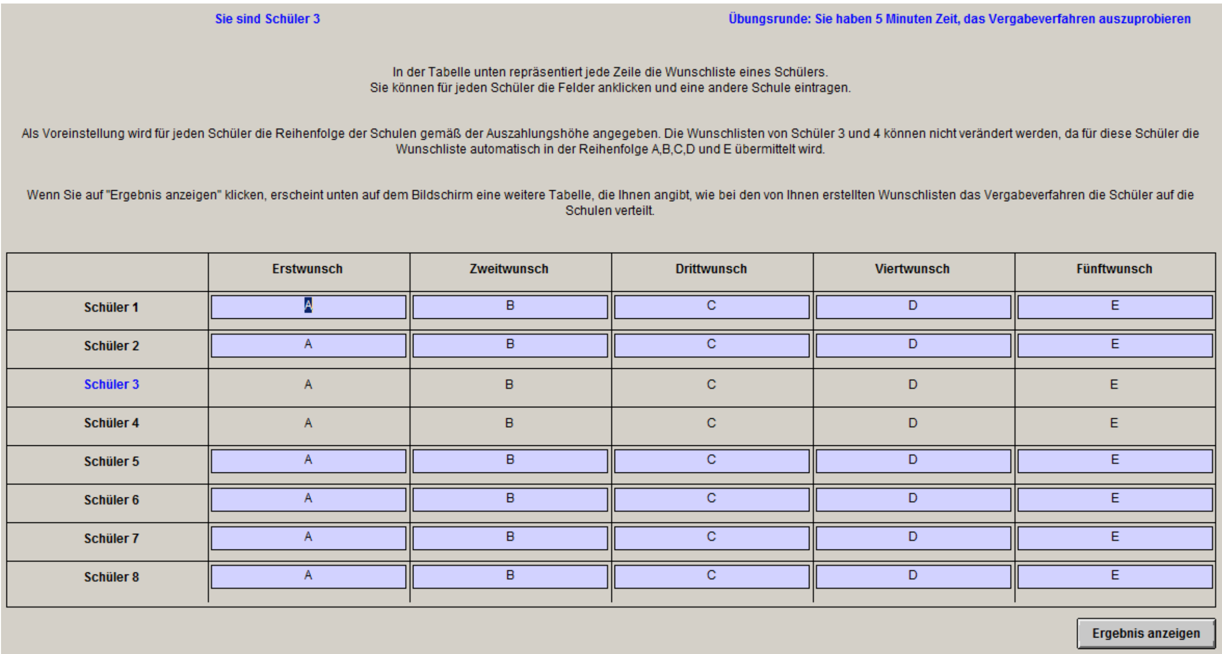


Notes: On this screen, subjects are informed about the students who will automatically submit the rank order lists according to payout amount (i.e., the sincere students). In the Spectator treatment, the subject's rank in the top left corner of the screen is omitted.

Figure B.12: Screenshot of comprehension quiz

Notes: On this screen, subjects (in the Spectator treatment) have to answer a comprehension quiz about the two procedures (in this example, procedure 1 is the assortative matching and procedure 2 is the Boston mechanism). The comprehension quiz asks for outcomes in different scenarios conditional on two students (in this example, ranks 2 and 3) submitting their sincere rank order list. In the top right corner of the screen, subjects in the Spectator treatment are informed that they receive an additional 5 Euros for answering the quiz correctly (omitted in the Stakeholder/Effort and Player treatments). In the top left corner, subjects are informed that this is the original screen that subjects in the Player treatment will see.

Figure B.13: Screenshot of Trial Period



Notes: The screenshot displays the trial period (in the Stakeholder/Effort and Player treatments). In the example, the subject is in rank 3 and is in a group, in which student 3 and 4 have to submit their true preferences. Subjects have five minutes to try out different rank-order lists for non-sincere students. When clicking the button the resulting matching is shown. Subjects in the Spectator treatment see the same screen but without a subject's rank displayed in the top left corner and without her rank being colored in the table.

On-screen instructions about voting stage (only Stakeholder/Effort and Spectator)

[Spectator: Now you can decide which school matching procedure will be applied for the future subjects.*]*

[Stakeholder/Effort: Now you can decide which school matching procedure will be applied.*]*

The vote works as follows. All subjects in today's session decide on-screen whether they would like to participate in the vote or abstain. Those who take part in the vote can indicate whether Procedure 1 or Procedure 2 is applied *[Spectator:* for the future subjects.*]*

[Spectator: In a future session, there will be a group of subjects, in which subjects *[2 and 3; 3 and 4; 4 and 5]* automatically submit their rank-order list. This corresponds to the case that you have seen on the previous screens. In today's session you and seven other subjects will take a vote on whether Procedure 1 or Procedure 2 will be applied for this future group of subjects.*]*

In the vote you or one of the other seven subjects are selected at random. If the selected subject voted for one of the procedures, the result of the allocation procedure that he chose will be applied *[Spectator:* for the future group of subject*]*. His (and only his) payout is reduced by 5 Cent. If the randomly selected subject decided to abstain from the vote, it is determined randomly whether Procedure 1 or Procedure 2 is applied *[Spectator:* for the future subjects*]*. In this case of abstention, the randomly selected participant does not have to pay the 5 Cent.

When all participants are finished reading the instructions, the experiment continues with a comprehension quiz.

On-screen instructions about voting stage (only Player)

In a previous session there was a vote on which procedure is applied for you and the other subjects.

On the next screen you will be informed which procedure is applied.

Figure B.14: Screenshot of comprehension quiz about voting (only Spectator and Stakeholder/Effort)

Notes: On this screen, subjects in the Spectator treatment answer a comprehension quiz about the voting stage. The quiz tests comprehension of the random dictator rule and the voting costs of 5 Cents. In the top right corner of the screen, subjects in the Spectator treatment are informed that they can earn an additional 2 Euros for answering the questions correctly. This note is omitted in the Stakeholder/Effort treatment.

Figure B.15: Screenshot of voting decision (only Spectator and Stakeholder/Effort)

Notes: On this screen, subjects (in the Spectator treatment) vote whether the payout for the future subjects is according to Procedure 1 or Procedure 2. Subjects are asked whether they would like to participate in the vote and the options are "Yes, I would like the matching to be according to Procedure 1", "Yes, I would like the matching to be according to Procedure 2", or "No, I do not want to participate in the vote". In treatment Stakeholder/Effort, the subjects vote instead which procedure is applied in their own session.

Figure B.16: Screenshot of payout screen

Notes: On this screen, subjects in the Spectator treatment are informed which mechanism was selected for the future Player session. In addition, they are informed that they receive 10.05 Euros show-up fee and 7.00 Euros for answering all comprehension questions correctly. If the Boston mechanism is selected, subjects in the Stakeholder/Effort and Player treatments have to submit their rank-order lists (Figure B.5) and report their beliefs about other's first choices (Figure B.3 and B.4) before seeing the payout screen. If the assortative matching is selected, subjects in the Stakeholder/Effort and Player treatments directly proceed to the payout screen.