Matching and Chatting: An Experimental Study of the Impact of Network Communication on School-Matching Mechanisms*

Tingting Ding and Andrew Schotter†

February 2, 2016

Abstract

While, in theory, the school matching problem is a static non-cooperative one-shot game, in reality the "matching game" is played by parents who choose their strategies after consulting or chatting with other parents in their social networks. In this paper we compare the performance of the Boston and the Gale-Shapley mechanisms in the presence of chatting through social networks. Our results indicate that allowing subjects to chat has an important impact on the likelihood that subjects change their strategies and also on the welfare and stability of the outcomes determined by the mechanism.

Key Words: School Choice, Matching, Mechanism Design, Networks, Chat

JEL Classification: C78, C91, C72

*Support for this research was funded by the National Science Foundation grant number 1123045. We would like to thank the Associate Editor, anonymous referees, Tayfun Sönmez, Leeat Yariv and Tim Salmon for their valuable comments, and thank the participants of the 2012 International ESA conference, the 2013 North American ESA Conference, the University of Texas at Dallas Economics seminar, and the Santa Barbara Conference on Behavioral Economics for helpful discussion. We would also like to thank the Center for Experimental Social Science at New York University for their lab support as well as Anwar Ruff for his programming assistance.

†Ding: School of Economics; Key Laboratory of Mathematical Economics, Shanghai University of Finance and Economics, 111 Wu Chuan Road, Shanghai 200433, P. R. China (email: tingting.ding@mail.shufe.edu.cn); Schotter: Department of Economics; Center for Experimental Social Science, New York University, 19 West 4th St., New York, NY, 100012 (email: andrew.schotter@nyu.edu).

In recent years there has been a great deal of interest in designing matching mechanisms that can be used to match public school students to schools (the student matching problem).[1] The design of matching mechanisms relies on a combination of economic theory and common sense, and these attempts have proven extremely useful in helping organizations solve this complicated problem.[2] The premise of this paper is that when testing mechanisms we must do so in the environment in which they are used in the real world rather than in the environment envisioned by theory. More precisely, in theory the school matching problem is a static one-shot game played by parents of children seeking places in a finite number of schools and played non-cooperatively without any form of communication or commitment between parents. However, in the real world, the school choice program is played out in a different manner. Typically parents choose their strategies after consulting with other parents in their social networks and exchanging advice on both the quality of schools and the proper way they should play the "school-matching game." In addition, parents who have engaged in the matching mechanism in the past may also communicate with parents currently in the match and offer their words of wisdom. We can call these two systems of advice the "horizontal" and the "vertical" advice systems. This paper focuses on horizontal advice networks and a companion paper (Ding and Schotter, 2015) focuses on vertical advice. The question we ask here is whether chat between parents (similar to naive advice defined in Schotter (2003)) affects the strategies they choose, and if so, whether it does so in a welfare-increasing or decreasing manner.[3]

In this paper we compare the performance of the Boston and the Gale-Shapley mechanisms, two often used school matching mechanisms, in the presence of chatting through social networks. In each mechanism subjects must submit rankings over three objects whose values to them are either $24, $16, and $4. We allow subjects in our experiments to play the "matching game" twice, once before (Phase 1) and once after chatting (Phase 2).[4] Because the Boston mechanism is not strategy-proof, we would also like to examine how subjects using the Boston mechanism are influenced by preference intensity. We

[1]See Balinski and Sönmez (1999); Abdulkadiroğlu and Sönmez (2003); Ergin and Sönmez (2006); Erdic and Ergin (2008); Abdulkadiroğlu et al. (2009); Pathak and Sethuraman (2011); Kesten and Ünver (2013); Abdulkadiroğlu et al. (2011); Haeringer and Klijn (2009) for some of the central theoretical contributions, Chen and Sönmez (2006); Pais and Pintér (2008); Calsamiglia et al. (2010); Featherstone and Niederle (2014); Klijn et al. (2013); Chen and Kesten (2013) for experimental studies, and Abdulkadiroğlu et al. (2005a) and Abdulkadiroğlu et al. (2005b) for the summary of school choice reforms in New York City and Boston.

[2]See Toch and Aldeman (2009) for a news article praising the New York City matching scheme, as well as Herszenhorn (2003) "Revised Admissions for High School," New York Times.

[3]In other work (Schotter and Sopher, 2003, 2007; Nyarko et al., 2006; Iyengar and Schotter, 2008), advice received by chatting has proven to have a very powerful influence on decision makers in the sense that advice tends not only to be followed but typically has a welfare increasing consequence.

[4]For the subjects who are not allowed to chat, we ask them to write down the logic of their strategies by introspection when the others chat.

then test a variant of the Boston mechanism, in which the values associated with subjects' three choices are $24, $10, and $4, respectively. In the text below, we call our two variants of the Boston treatments the Boston 16 and the Boston 10 mechanisms for the ease of exposition, though these two treatments indeed deploy the same matching mechanism. The Boston 16 and the Gale-Shapley mechanisms serve as our dual baselines.

Given our design, the behavior in Phase 1 is a test of the static mechanisms, while the difference between Phase 1 and Phase 2 defines the impact of chat. Among the questions we attempt to answer are:

1. In the absence of chat (Phase 1), is the performance of our baseline Boston 16 and Gale-Shapley mechanisms equivalent? I.e., are the strategies employed across subject types and the welfare of the mechanisms the same?
2. Do subjects who chat change their Phase-1 submitted rankings more than those who are isolated and do not chat?
3. How are the behavior and welfare of subjects using the Boston mechanism influenced by the preference intensity?
4. Is chat stability increasing? I.e., does chat lead to an increase in stable outcomes?
5. If chatting is beneficial, does it matter to whom you chat? I.e., does it matter whether your social network is populated by people like you or different from you?
6. Does chatting influence welfare differently when subjects have priority rights or not? (In school matching programs some students (subjects) have priority for admission to some schools (objects).)
7. Does the content of chat change as we look across mechanisms and subject types?

While we will not discuss the answers to these questions in detail in this introduction, as a preview we find that chatting has many beneficial characteristics. More precisely, over all, allowing people to chat not only increases their welfare but also the stability of the matches created. This is interesting since we find that there is no difference in subject behavior or subject welfare between our baseline Boston 16 and Gale-Shapley mechanisms in the absence of chat in Phase 1, which may lead to the conclusion that these two mechanisms are equivalent.[5] After chatting is introduced, however, behavior

[5]Previous experimental papers on school matching present mixed evidence on the performance of various mechanisms. In terms of efficiency, for example, Pais and Pintér (2008) and Calsamiglia et al. (2010), find no difference across all of their treatments. Other papers have mixed results. Chen and Sönmez (2006) find in their "designed environment" that the efficiency of the Gale-Shapley mechanism is greater than that of the Boston mechanism, though in their "random environment" no such difference is found. Featherstone and Niederle (2014) and Klijn et al. (2013) find under some circumstances the efficiency in the Boston mechanism is greater than that in the Gale-Shapely mechanism. The results on

and welfare diverge across these mechanisms. The implication, therefore, is that including chat into an experimental design on matching enhances its external validity since in the real world chatting is ubiquitous.

If chatting is beneficial, it might make sense to ask if it matters with whom you chat. We find that, those subjects who communicate with people of their own experimental type (i.e., who have the same induced preferences and priorities) tend to change their submitted preference rankings more between phases but their payoffs increase less, indicating that sometimes the beneficial aspect of chatting is to persuade subjects who are already using good strategies not to change.

With respect to welfare, chatting appears to influence welfare differently between subjects with priority rights and those without. Though there is little welfare change among subjects with priority rights between phases, chatting does significantly change the welfare of those without priority rights. If one equates subjects without priority rights with people living in disadvantaged areas (where they may have priority rights but in the least desirable schools), our results indicate that allowing better communication between parents in those areas may improve their welfare. In addition, our results show that among subjects who chat, those chatting with others of different types appear to increase their payoffs between phases more significantly than those isolated. Chat also increases the fraction of stable outcomes in the Gale-Shapley and the Boston 10 mechanisms.

While this paper is the first to study the performance of school matching mechanisms (or, perhaps, any economic mechanism) in the presence of chatting via social networks amongst peers, there are a few other papers that study what you might call top-down advice given to people by those who administer the mechanism in which they suggest various strategies. In the experiments conducted by Braun et al. (2014), subjects receive strategic coaching before submitting their preferences: in strategy-proof mechanisms subjects are told to submit truthful preferences, while in the non-strategy-proof mechanism subjects are offered a suggested manipulation. They find that subjects in their experiment are likely to follow advice. The results of Guillen and Hing (2014), however, are mixed. They deploy the top trading cycles mechanism and have three advice treatments:

strategy differences are also unclear. Klijn et al. (2013) find differences in strategic behavior in all their treatments. Pais and Pintér (2008) find that when subjects have zero information (only know of their own preference rankings) there is no difference in the fraction of subjects submitting truthful preferences while, when subjects have more information about school priorities and others' preferences, they are more likely to tell the truth in the Gale-Shapley mechanism. Featherstone and Niederle (2014) find that while in their "uncorrelated preference environment" there is no difference in truth-telling across the Boston and Gale-Shapley mechanisms, in their "aligned preference environment" subjects are more likely to tell the truth in the Gale-Shapley. In contrast, Chen and Sönmez (2006) and Calsamiglia et al. (2010) show in all their treatments the fraction of truth-telling in the Gale-Shapley mechanism is greater than that in the Boston mechanism. Because the designs of these experiments are so different and the number of subjects in a typical session varies so widely, it is hard to compare these results or reach any definite conclusions.

right advice recommending truth-telling, wrong advice suggesting misrepresentation and mixed advice that offer both right and wrong advice. They find that the fraction of subjects telling the truth in all three advice treatments is significantly lower than that in the control group where no advice is provided.

It is important to point out that the object of chat in our experiment is the strategies of subjects but not the quality of schools or objects. While in the world outside of the lab parents are very likely to talk about the quality of the schools which they might send their children to or the fit between the schools and their children, in our experiment, because subjects know the values of the objects to them precisely, there is no scope for such discussion. We have done this intentionally to focus the attention of subjects on strategic issues.

The remainder of the paper is organized as follows. In Section 1 we provide a quick overview of the Boston and the Gale-Shapley mechanisms and their theoretical properties. In Section 2 we show our experimental design, while in Section 3 we present our results by answering the questions stated above and several more. Finally, Section 4 offers some conclusions.

1 The School Choice Matching Problem

In this section we will closely follow Pathak (2011) in describing the school choice problem. A school choice model with I students and N schools consists of a triple (P, q, π) where

1. $P = (P_1; \cdots ; P_I)$ are the preferences of students,
2. $q = (q_1; \cdots ; q_n)$ are the school capacities, and
3. $\pi = (\pi_1; \cdots ; \pi_n)$ are the school priorities.

The preferences of the students express their rankings over schools, the school capacities state the number of seats in each school, while the school priorities express information about how applicants are ordered at schools. In this paper we investigate only the school choice problem in which students are free to choose their strategies while schools are constrained to accept students on the basis of their exogenously defined priorities. Hence we will be looking at the "one-sided" version of the matching problem.

The outcome of a school choice problem is a student assignment, or a matching $\mu : I \to S$, where $\mu(i)$ indicates the school assignment of student i. A matching is **Pareto efficient** if there is no way to improve the allocation of a student without making another student worse off. (Note that only the welfare of students is considered in this definition.) A matching is **stable** if there is no student-school pair $(i; s)$ such that (a) student i prefers school s to her assignment $\mu(i)$, and (b) there is another student j with lower priority than student i assigned to s under μ, or school s has slots unfilled. Finally, a mechanism is **strategy-proof** if truth-telling is a dominant strategy for all students.

1.1 Boston Mechanism

The Boston mechanism works as follows:

Step 1) Only the first choices of the students are considered. For each school, consider the students who have listed it as their first choice, and assign the seats in the school to these students one at a time following the school's priority order until either there are no seats left or there are no students left who have listed it as their first choice.

In general, at

Step k) Consider the remaining students. Only the kth choices of these students are considered. For each school with still available seats, consider the students who have listed it as their kth choice, and assign the remaining seats to these students one at a time according to priority until either there are no seats left or there are no students left who have listed it as their kth choice.

This mechanism, while widely used, is not strategy-proof.

1.2 Gale-Shapley Deferred Acceptance Mechanism

The Gale-Shapley deferred acceptance mechanism works as follows:

Step 1) Each student proposes to her first choice. Each school tentatively assigns its seats to its proposers one at a time following its priority order until either there are no seats left or there are no students left who have listed it as their first choice.

In general, at

Step k) Each student who was rejected in the previous step proposes to her next choice. Each school considers the students who has been held together with its new proposers, and tentatively assigns its seats to these students one at a time according to priority, until either there are no students left who have proposed or all seats are exhausted. In the latter case, any remaining proposers beyond the capacity are rejected. The mechanism terminates either when there are no new proposals, or when all rejected students have exhausted their preference lists.

The Gale-Shapley mechanism is strategy-proof. Though it generally does not guarantee Pareto-optimal results, it does determine student-optimal stable matches when students and schools have strict preferences (Dubins and Freedman, 1981; Roth, 1982), *i.e.*, no other stable assignment Pareto dominates the outcome produced by the Gale-Shapley mechanism, although this outcome might be Pareto dominated by some unstable assignments. However, when schools have coarse priorities, in the sense of not being able to express a strict order over students, the welfare consequences change as the Gale-Shapley mechanism may not always produce student-optimal stable matches. In our experimental design we consider coarse priorities.

Our experiments are not aimed at investigating which mechanisms are "best." Such work has already been done by Chen and Sönmez (2006), Pais and Pintér (2008), Calsamiglia et al. (2010) and Featherstone and Niederle (2014). Instead, we are interested in the impact of advice and network communication on behavior and mechanism performance. As mentioned above, we are therefore more interested in mechanisms that are *not* strategy-proof like the Boston mechanism, since such mechanisms leave a large amount of room for strategic dissembling and hence are more likely to exhibit strategic diversity across subnetworks when chatting is allowed.

2 Experimental Design

All our experiments were conducted in the experimental laboratory of the Center for Experimental Social Science at New York University. Five hundred and ten students were recruited from the general undergraduate population of the university using the CESS recruitment software. The experiment was programmed using the z-tree programming software (Fischbacher, 2007). The typical experiment lasted about an hour with average earnings of $23.52. Subjects were paid in an experimental currency called Experimental Currency Units (ECU's) and these units were converted into U.S. dollars at a rate specified in the instructions. To standardize the presentation of the instructions, instead of reading the instructions, after the students had looked the instructions over, we showed a pre-recorded video which read them out loud and simultaneously projected the written text on a screen in front of the room. The video for one treatment can be downloaded at https://goo.gl/dmuuuJ, while the printed instructions are available in Appendix A.

2.1 Structure of Experiment

In the experiment subjects participated in three distinct decision tasks, and the monetary payoffs they received were the sum of their payoffs in each task. The first task was the matching experiment to be described below, while the second task was one play of the $2/3^{rd}$'s Beauty Contest and the third was the Holt and Laury (2002) risk aversion task. We used the beauty contest game as a diagnostic tool to evaluate their strategic sophistication, and we had subjects perform the Holt-Laury task for the obvious reason of eliciting their attitudes of risk aversion. For the sake of brevity we relegate the results of our risk aversion exercise to the Appendix and do not report our beauty contest results since they proved to be inconsequential.

2.2 The Matching Problem

Our experiments are designed with an eye toward integrating the school choice mechanisms with social networks. To evaluate the impact of chat we ran a static school choice matching experiment twice, once before (Phase 1) and once after (Phase 2) allowing chatting via networks. Subjects were paid for either Phase 1 or Phase 2, but not for both. At the end of the experiment, the computer randomly determined which payoff, Phase 1 or Phase 2, would be paid. Subjects did not receive any feedback about their decisions or the decisions of other participants until the very end of the experiment, so the results of Phase 1 were not known until after the experiment. We used neutral language so schools were called "objects" and students were designated "subjects."

2.2.1 Phase 1

In both phases of the experiment there are 20 subjects. At the beginning of Phase 1 we randomly assign subjects to types in the sense that among these 20 subjects four are designated Type 1, four Type 2, four Type 3, four Type 4 and four Type 5. In addition to these 20 subjects there are also a set of 20 objects grouped into three types which are called Object A, Object B, and Object C. In total there are 8 units of Object A, 8 units of Object B and 4 units of Object C.[6]

Table 1 presents the full preference matrix of our subject types. These preferences are the same for all our experimental sessions, although the cardinal utilities associated with these objects vary across treatments.

Table 1: Preferences over Schools

	Student Preferences				
Type	1	2	3	4	5
1^{st} choice	C	C	C	A	A
2^{nd}choice	A	A	B	B	C
3^{rd}choice	B	B	A	C	B

In terms of payoffs, in six of our nine treatments that use either what we call the Boston 16 or the Gale-Shapley Mechanism, if a subject is matched to her first-best she will receive 24 ECU, if she is matched to her second-best she will receive 16 ECU, and if she is matched to her third-best she will receive only 4 ECU. In the remaining three treatments using what we call the Boston 10 Mechanism, we lower the value of the second-best object to each subject from 16 ECU to 10 ECU (see Section 2.2.3 for details).

[6]The numbers of subjects in the isolated network treatment, defined in Section 2.2.3, vary from 10 to 20. In each session the number is a multiple of 5, and then the number of subjects for each type and objects change accordingly. For example, when there are 15 subjects in one session, we have 3 subjects for each type, 6 units of Object A, 6 units of Object B and 3 units of Object C.

In all treatments, it is common knowledge that Types 1 and 2 are given priority for Object A, while Type 3 is given priority for Object B and Types 4 and 5 are not given priority for any objects. When the number of subjects of equal priority applying for an object is greater than the number of objects available, the mechanism employs a lottery to break ties.[7] Note that Types 1 and 2 are identical with respect to both their preferences and priorities.

Subjects are told their own types and matching payoffs for each object as well as a priority table, but they do not know the types or the object payoffs of any subjects other than themselves. They are then required to state their rankings over objects. Based on the information subjects provide, one of the matching mechanisms (either the Boston or the Gale-Shapley mechanism) determines the allocation outcome. Each subject is matched to one and only one object.

There are six possible preference orderings or rankings that subjects can submit in either Phase 1 or Phase 2: 1-2-3, 1-3-2, 2-1-3, 2-3-1, 3-1-2 and 3-2-1, where the numbers in each ranking are the subject's first-best, second-best and third-best objects. So strategy 1-2-3 is a truth-telling strategy since the subject submits her first-best object first, her second-best object second, and her third-best object last.

2.2.2 Phase 2

To measure the impact of chat on matching, we run Phase 2 which allows subjects to submit their preference rankings after they communicate with each other via chat boxes. By comparing the submitted preference rankings of subjects in Phase 2 to those of the same subjects in Phase 1, we are able to observe the impact of chatting on subject behavior and welfare.

At Phase 2, subjects face the same matching problem as at Phase 1. Their types and matching payoffs for each object do not change. However, at Phase 2, before subjects enter their rankings, they are assigned to some subnetwork and allowed to talk with other subjects in their subnetwork for five minutes via chat boxes. The size of subnetworks range from 1 to 5 subjects. If a subject is assigned to a subnetwork with only one subject, she can not talk to anyone but is asked to enter into the chat box what factors influence her decision in Phase 2. In other words, we ask her to write in the chat box what she thinks of as she contemplates her ranking.[8] All communication goes via chat boxes. After five minutes all chat boxes become inactive and subjects have to submit their rankings again, i.e., they have to enter into the computers which objects they rank first, second and third, just as they did in Phase 1. The exact types of subnetworks used will be described in

[7] In our experiment we used a single lottery instead of object specific lotteries to break ties.

[8] See Cooper and Kagel (2012) for a similar "self advice" feature.

Section 2.2.3.

Payoffs in Phase 2 are determined in the same way as they were in Phase 1. At the end of the experiment, the computer randomly determines which payoff, Phase 1 or Phase 2, will be paid to subjects.

Several points about our design are worth noticing. First, the preferences and priorities are chosen to maximize competition among subjects, and hence subjects are likely to think strategically and exchange their thoughts with others when chatting. For example, in our design no subject has priority over her first-best object. In fact those with priority rights always have priority for their second-best objects. In addition, the competition for Object C is intense because there are only 4 units of Object C available but all subjects with priority rights, 12 subjects in total, have it as their first-best. Hence those subject with priority rights may consider stating their second-best objects first, which is a strategic move. Furthermore, for subjects without priority rights, their first-best object is Object A, which both Types 1 and 2 have priority over. Hence, their ability to obtain their first-best object is determined not only by their own behavior but also by the behavior of Types 1 and 2. Without having a priority school to use as a target, Types 4 and 5 are more uncertain about their "best" strategies and therefore might be more likely to be influenced by chat.

Second, our matching game has comparatively large groups of subjects in order to mimic the real world where participants have no ability to coordinate. Since our experiment allows subjects to chat, we were afraid that, if we used only a small number of subjects in each group and allowed them all to chat with each other, they might collude. To avoid such collusion we recruited 20 subjects in each session and allowed them to chat via subnetworks of at most five subjects. This made it impossible (or at least extremely hard) for subjects to coordinate.[9]

Third, our design has an incomplete information structure to make the experimental environment closer to the real world. Note, however, as subjects know neither the preferences of others nor the distribution from which their preferences are drawn, when the Boston mechanism is used, there is no way to calculate the equilibrium. It is not a problem for us, because we are primarily interested in the changes of submitted preference rankings with the impact of chat and these changes can be easily measured without the knowledge of equilibrium.

Fourth, one could argue that the same experiment could be conducted using an across-subject design where some subjects play the matching game without chatting and other groups are allowed to chat. We decided on a within-subject design because we were

[9]As there is no collusion concern in the isolated treatment, sometimes we have less subjects per session.

more interested in how subjects changed their strategies in the face of advice rather than comparing the aggregate distributions of strategies across the chat and no chat treatments. In addition, the chat treatment of such an across-subjects design would, in essence, be identical to our current within-subjects design because one could imagine a subject reading the instructions, forming an opinion of what she might want to do, and then chatting. Our design allows us to see the first thoughts of subjects after they read the instructions but before they chat, and hence allows us to learn how chat influenced their thinking.

2.2.3 Treatments

Using the experimental procedures described above, we run a set of 9 treatments. These treatments differ by changing the matching mechanisms used while holding preferences and priorities constant, or changing the preference intensities while holding the mechanisms and priorities constant, or changing the network structures while holding all else constant. Below we describe these treatments one by one.

Preferences and Matching Mechanisms In terms of preference intensities we have two treatments, the Boston 16 and the Boston 10 treatments, which differ by the payoff received when subjects are allocated to their second-best objects. In the Boston 10 treatment, subjects are paid 24 ECU if they receive their first-best objects, 10 ECU if they receive their second-best objects, and 4 ECU if they receive their third-best objects, while for the Boston 16 treatment the payoffs are 24, 16 and 4 ECU's, respectively. (In the Gale-Shapley mechanism, the values are also 24, 16 and 4 ECU's, respectively.) The decrease for the second-best object from 16 ECU to 10 ECU was instituted to make subjects using the Boston 10 mechanism have more at stake in getting their first-best objects, since their second-best objects are less desirable. We believe this would increase competition and thereby increase the incidence of strategizing.

Because, in the Gale-Shapley mechanism, subjects have a dominant strategy while the equilibrium of the Boston mechanism is indeterminate, we suspected that the change of preference intensities would have a greater impact on the Boston mechanism. Therefore, we only reduced the value of the second-best object to 10 ECU in the Boston mechanism, and have the Boston 16 mechanism and the Gale-Shapley mechanism (with a value of 16 ECU for the second-best object) as our dual baselines.

Networks In investigating whether chatting impacts the behavior of subjects, it is important to ask with whom a subject chats. Does she talk to someone who has similar preferences and priorities, or someone quite different? While the subjects in our experiment, when being placed into subnetworks, do not know the types of others with whom they are networked, this information could be revealed during the chat (and our chat records show that it indeed is). In addition, subjects do not know the global network structure in the sense that they do not know the types of subnetworks other subjects are allocated to. We designed three different types of networks which we call Networks 1, 2, and 3, and these three networks are illustrated in Figure 1.

Figure 1 here.

Both Networks 1 and 2 are composed of four distinct 5-person subnetworks, some of which are complete and some incomplete. More precisely, the first subnetwork on the left in these two treatments is complete in the sense that each subject can send and receive messages from all the others, while the other subnetworks are incomplete. The subnetwork on the far right is "isolated" because no subject can communicate with any other. When chatting is allowed, these subjects can only talk to themselves. Note that subjects in the complete subnetwork of Network 2 have the same preferences and priorities (Types 1 and 2) while those in the incomplete subnetworks are all of different types, while in Network 1 the opposite is true. In Network 3, all subjects are isolated. Our design is summarized in Table 2.

Table 2: Experimental Design

Treatment	Pref. Intensity	Network	Mechanism	# of Sessions	# of Subjects
1	24-16-4	1	Boston	4	80
2	24-16-4	2	Boston	4	80
3	24-16-4	3	Boston	2	40
4	24-16-4	1	Gale-Shapley	4	80
5	24-16-4	2	Gale-Shapley	4	80
6	24-16-4	3	Gale-Shapley	3	45
7	24-10-4	1	Boston	2	40
8	24-10-4	2	Boston	2	40
9	24-10-4	3	Boston	2	25
				Total: 27	Total: 510

3 Results

We will present our results by stating a set of questions that we will attempt to answer using the data generated by our experiment. By comparing the behavior of subjects before and after chat, we are able to investigate whether chatting affects the strategies that subjects use and their matching outcomes. We first consider the baseline comparison between the Boston 16 and the Gale-Shapley mechanisms. We then discuss the Boston 10 mechanism separately in order to investigate the impact of changing preference intensity. Our discussion starts from Phase 1 behavior and then proceeds to Phase 2.

3.1 Phase 1 Results

If the behavior of our subjects and the performance of our dual baseline mechanisms were identical in Phase 1, one might be tempted to conclude that these mechanisms were equivalent and hence interchangeable. Such a conclusion could be misleading if chat changes behavior, since, in the real world, chatting via networks is the norm. To properly examine the impact of chatting on mechanism performance and subject behavior, we will first examine how our dual baseline mechanisms performed in Phase 1 where there was no chatting to see if we conclude that they were equivalent. We will then proceed to Phase 2 and investigate the impact of chatting on changes in behavior and welfare in order to make what we consider to be the most telling comparisons.

To investigate Phase 1 we ask the following question:

Question 1 – Baseline Mechanism Equivalence:

Is there a difference, in aggregate or subject type by subject type, in the stated preference rankings of subjects in our dual baseline (the Boston 16 and the Gale-Shapley) mechanisms in Phase 1? In addition, given the stated preferences of subjects in Phase 1, is there a difference between the welfare of subjects across these two mechanisms?

Table 3 presents the percentages of stated preference rankings in Phase 1 aggregated over all subjects and type by type. It is worth pointing out that the definitions we use to classify strategies in Table 3 depart slightly from the conventional ones. In our analysis we classify a subject's strategy on the basis of what she places first in her submitted ranking. For example, we classify a strategy as "truthful" if a subject submits her first-best choice first, "strategic" if she submits her second-best choice first, and "irrational" if she submits her third-best choice first.

We use this classification for three reasons. First, the main goal of this paper is to investigate how subjects change their submitted preference rankings as a result of chat. If we use a very disaggregated way to categorize strategies, each subject would have 6 possible strategies and the transition matrix of strategy changes, across Phases 1 and 2, would contain 36 cells. Such transition matrices, given our data, would contain many cells with few observations which would make statistical comparisons difficult. In contrast, under our definitions the transition matrix is 3×3 and is much easier to handle. Second, we believe that this aggregation has a cognitive justification since we consider what a subject states as her first choice (among three objects) as more indicative of her strategy than how she ranks other objects. A subject who ranks her second-best object first is clearly strategizing, while a strategy which places the third-best first is irrational since there is no scenario, in either mechanism, where that strategy can be beneficial. If a subject had to rank more than three objects it might be myopic to look only at her first choice, but, for a three-object world, little is lost since there are not many manipulations one can do after the first choice is fixed. There are relatively few subjects using strategies such as 1-3-2 and 2-3-1.[10]

Table 3 here.

When the Gale-Shapley mechanism is used one might suggest that all strategies other than truth-telling are irrational. For consistency sake, however, across treatments we will continue to use "irrational" to categorize strategies 3-2-1 and 3-1-2. To ensure our strategy classification does not create a bias in our results, we also do our analysis using the more conventional definition with six strategies in which the truthful strategy is defined as only submitting preference 1-2-3, and then rerun our main regressions concerning strategy changes across Phases 1 and Phase 2. These new regressions are presented in Appendix B where we find no statistical differences between the results using either classification.

Returning to Table 3, we see that there is little difference in submitted preferences between the Boston 16 and the Gale-Shapley mechanisms. While there are 37.00%, 58.00% and 5.00% of subjects submitting truthful, strategic, and irrational preference rankings respectively in the Boston 16 mechanism, in the Gale-Shapley mechanism these percentages are 39.51%, 53.66% and 6.38%, respectively. These percentages are not significantly different (using a set of t-tests) either when we pool subjects across types or when we compare the strategies chosen subject type by subject type except for the use of the irrational strategy by Type 1 subjects.[11]

To compare the welfare of subjects across our dual baseline treatments, we can not simply compare the outcomes determined at the end of the experiment since those outcomes are many times the result of the lottery used to break ties. If the lottery had turned out differently, we would have had a different outcome. To avoid this bias we simulate the expected matches of our subjects by randomly drawing a large number (2,500) of lottery orders while holding the stated preference rankings of subjects constant at their submitted preferences. We will refer to the results of this simulation often especially when we consider our results on welfare.

Table 4 presents the average percentage of times in our simulation that subjects receive their first, second, and third-best objects given their submitted preferences and the randomly drawn lottery orders. Our results show that there are no statistical differences in

[10]In our data only a small fraction of subjects use strategies 1-3-2 and 2-3-1. Among all treatments in Phase 1 there are 2.75% of subjects playing 1-3-2 while 8.63% playing 2-3-1. In Phase 2 the respective fractions are 2.16% and 10.00%. The fractions of subjects using irrational strategies are also small, 6.67% in Phase 1 and 4.17% in Phase 2. Therefore, if we use the disaggregative way to categorize strategies, some cells of the transition matrices would contain few observations.

[11]The high frequency use of irrational strategy in the Gale-Shapley, though, is hard to explain given truth-telling is the dominant strategy.

the expected matching outcomes across the dual baseline mechanisms either in aggregate or type by type. In each simulation iteration we run a χ^2 test with the null hypothesis that the outcomes across the two mechanisms are identical. In the last column of Table 4 we report the fraction of times when the null hypothesis is not rejected.

Table 4 here.

With these simulated outcomes, we also calculate the fraction of the first-best surplus that is captured by our subjects. Our results show that in the Boston 16 mechanism the subjects are able to capture 84.77% of the potential payoffs available to them, while in the Gale-Shapley mechanism they capture 85.62%. These fractions are not statistically different (the p-value is 0.6898).

As stated previously, in our experimental design we have a Boston 10 treatment where the second-best objects of subjects decrease in value from 16 ECU to 10 ECU. By increasing the opportunity cost of losing one's first-best object, we expect that competition for the first-best objects will increase and subjects will behave differently than they do in our dual baseline mechanisms. These considerations yield Question 2.

Question 2 – Preference Intensity and Behavior:

Compared to subjects using the Boston 16 and the Gale-Shapley mechanisms, do subjects using the Boston 10 mechanism submit preferences that differ, in Phase 1, from those submitted in our dual baseline experiments?

The short answer here is yes. Subject behavior in Phase 1 of our Boston 10 mechanism differs from that of both the Boston 16 and Gale-Shapley mechanisms. This can be seen most easily in Figure 2 where we present the use of truth-telling, strategic and irrational strategies across our three mechanisms in Phase 1.

Figure 2 here.

Looking at Figure 2 , notice that while there is little difference between the submitted rankings across the Boston 16 and Gale-Shapley mechanisms, there is a marked increase in the use of truthful strategies in the Boston 10 mechanism as well as an increase in the use of irrational strategies.

Table 3 presents a more disaggregated view of the data. When we compare the stated preference rankings of subjects in the Boston 10 mechanism with those in our dual baseline mechanisms, we find that there is a signifiant increase in the submission of truthful preferences for all subjects who have priority rights and for the pooled set of subjects.[12] Meanwhile we find a decrease in the use of the strategic strategy for all

[12] A similar impact of preference intensity is found in Klijn et al. (2013).

types when comparing the Boston 10 to our dual baseline mechanisms, and the decrease is significant not only for the subjects who have priority rights but also those who do not. Finally, similar to what we find in the Boston 16 mechanism, in the Boston 10 mechanism there is no subjects with priority rights use irrational strategy (i.e., stating their third-best objects as their first choices), while a non-negligible fraction of subjects without priority rights do so.

Given our answer to Question 2, one question that arises is whether the behavioral differences observed between the Boston 10 and our dual baseline mechanisms translate into differences in achieved welfare. This raises Question 3.

Question 3 – Welfare and Preference Intensity:

Compared to subjects in the Boston 16 and the Gale-Shapley mechanisms, are subjects in the Boston 10 mechanism more or less likely to receive their first, second or third best objects?

As we do in answering Question 1, we draw 2,500 lottery orders and compute the expected matches given the preference rankings submitted in the Boston 10 mechanism. The simulation results are reported in Table 4 along the expected allocation outcomes in our dual baseline mechanisms. In aggregate, subjects using the Boston 10 mechanism are more likely to receive their first-best, less likely to receive their second-best and more likely to receive their third-best. Using a set of χ^2 tests, the null hypothesis that the allocations are identical is rejected in 86.40% of the 2,500 simulation runs when we compare across the Boston 10 and Boston 16 mechanisms, and in 93.88% of the simulation runs when we compare the Boston 10 and the Gale-Shapley mechanisms.

3.2 Impact of Chat: Differences Between Phase-2 and Phase-1

To investigate the impact of chat, we examine the behavior of our subjects and the performance of our mechanisms in Phase 2 and compare it to Phase 1. In particular, we check whether the changes in behavior and matching outcomes of the non-isolated subjects are different from those of the isolated subjects. In addition, we are interested in how chat influences the changes in submitted preference rankings when we condition on whether subjects have priority rights and on the preferences they stated in Phase 1.

Note that our focus is on the changes in submitted preferences and outcomes across Phases 1 and Phase 2 and between subjects who chat and who do not chat, but not on the aggregate distribution of preferences or outcomes in these two phases. Comparing the distribution of submitted preferences across Phases 1 and 2, but not their change, may mask what is really going on since preference changes may cancel each other out and leave the impression that chatting has no impact. For example, say that in Phase

1 half the subjects submitted truthful preference rankings and half submitted strategic rankings. After chat, say all those who told the truth in Phase 1 submit strategic rankings in Phase 2, while all those who submitted strategic rankings in Phase 1 tell the truth in Phase 2. Furthermore, say for the subjects who are isolated, half of them chose to tell the truth and the other half entered strategic preferences in Phase 1, and no one changes in Phase 2. Then, while the Phase 1 and Phase 2 distributions would remain 50-50 for both the isolated and the non-isolated subjects, we could not claim that chatting has no impact, since it leads all non-isolated subjects to change but simply in a counter balancing direction. As we will see later, while our data does exhibit this cancelling-out property to some extent, these cancellations do not negate the impact of chat on the performance of the mechanisms.

3.2.1 Changes in Submitted Rankings

Our first question concerns the frequency of changes in submitted preference rankings across our two baseline mechanisms among subjects who chat (i.e., the non-isolated subjects) as compared to isolated subjects who do not. Later we will investigate the direction of those changes, and will condition on the subjects' priority rights and what strategies they used in Phase 1. To start, we examine unconditional aggregated changes.

Question 4 – Chat and Strategy Changes:

Do subjects who chat change their Phase-1 submitted rankings more than those who are isolated and do not chat? Do subjects change their submitted preference rankings more when they talk to people who are like them or different from them?

To answer Question 4 we first examine the fractions of subjects who change their submitted rankings from Phase 1 to Phase 2 across our three mechanisms, and check whether chat leads non-isolated subjects to change their preference rankings differently from those who are isolated.[13] The answer to these questions can be found in Table 5.

Table 5 here.

As Table 5 indicates, a substantial fraction of subjects change their preference rankings in Phase 2. In the Gale-Shapley mechanism while 40.83% of the non-isolated subjects change their submitted rankings, only 27.06% of the isolated subjects do so. For the Boston 16 mechanism the percentages are 25.83% and 30.00% respectively, and for the Boston 10 mechanism the fractions are 46.67% and 24.44%, respectively. A set of t-tests show that the non-isolated subjects change their strategies more often than those

[13] Here we aggregate over subjects who are isolated in Treatments 1 and 2 and those in Treatment 3 where all subjects are isolated.

isolated in the Gale-Shapley and the Boston 10 mechanisms (the p-values are 0.0387 and 0.0172, respectively). However, in the Boston 16 mechanism, there is no significant difference in strategy changes between subjects who chat and who do not (the p-value is 0.5245). The result of the Boston 16 mechanism may not be surprising if chatting convinces subjects that they have made the right choices in Phase 1 and so they should not change. For example, in Phase 1 a non-isolated subject in the Boston 16 mechanism may have submitted strategic preference ranking and, during chatting, become convinced that she need not change. We will, in fact, see that this is indeed the case from our chat records in Section 3.4.

It is also interesting to note that even when some subjects are not allowed to chat, they still change their submitted preference rankings in Phase 2.[14] As shown in our chat records, these isolated subjects change their strategies possibly because they realize their mistakes in Phase 1 or they believe the subjects who chat will change their strategies in Phase 2 so they should also change as a response (Similar phenomenon is found in Guillen and Hakimov (2014) which finds that subjects are more likely to deviate from truth-telling, the dominant strategy in the top trading cycle mechanism, when they learn that other players misrepresent their preference rankings.) When comparing across the mechanisms, we find that there is no statistical difference in the fraction of subjects changing their submitted preferences when chat is absent. In contrast, when subjects are allowed to chat, the fractions of subjects changing their strategies are significantly greater in the Gale-Shapley and the Boston 10 mechanisms when compared to that in the Boston 16 mechanism (the p-values are 0.0136 and 0.0075, respectively). This indicates that chat has a differential impact on behavior conditional on the mechanism used or the preference intensities within a mechanism.

We then run four logit regressions to examine the impact of chat as well as the network type. In these regressions we not only investigate whether chat has a differential impact on strategy changes but also whether whom you chat with (i.e., people like yourself or different than yourself) matters. In all four regressions, the dependent variable is a binary {0,1} variable indicating whether a subject changes her strategy across phases. The dependent variables include dummy variables indicating a subject's type, whether she is allowed to chat (or to whom she chat), and the mechanism used. The coefficients

[14] The fact that subjects who are isolated change their strategies indicates that there are two sources of learning taking place amongst our subjects: introspective and social. Introspective learning (see Rick and Weber, 2010) is that learning that takes place amongst subjects where the experiment they are engaged in is repeated without feedback (as between Phase 1 and Phase 2 in our study). Here subjects learn by introspection and second thinking. When chatting takes place, however, learning is social. Our results indicate that while both types of learning take place, social learning seems to have a larger impact on subjects in the sense that there is a significant difference in the fraction of subjects who change their strategies between those who chat and those who do not. Still, introspective learning is non-negligible. It is not the case that those who do not chat keep their Phase-1 preference in Phase 2.

measure the changes away from the default situation, which is an isolated subject of Type 5 in Specifications 1 and 3, and an isolated subject of Type 5 using the Boston 16 mechanism in Specifications 2 and 4.

Table 6 here.

The regression results substantiate what we stated above. As we see in Specifications 1 and 2, the presence of chat increases the likelihood that subjects change their submitted preference rankings. Furthermore, subjects who use the Gale-Shapley and the Boston 10 mechanisms are more likely to change their strategies after chat than those using the Boston 16 mechanism. In Specifications 3 and 4 where we divide the non-isolated subjects into homogeneous and heterogenous groups, we find that talking to subjects via heterogeneous subnetworks appears not to increase the incidence of strategy changes above that of isolated subjects. However, subjects communicating in homogeneous subnetworks, tend to change their Phase-1 submitted preference rankings more than isolated subjects.[15]

The results just stated are unconditional results. Since some of our subjects have priority rights for objects (namely Types 1, 2 and 3) and some do not (Types 4 and 5), we would like to investigate whether chat has a differential impact on the fraction of subjects changing their preference rankings across phases conditional on whether a subject has priority or not and also on the strategies they used in Phase 1.

Question 5 – Chat, Priority and Directed Strategy Changes:

Does chatting have a differential impact on the propensity of subjects to change their strategies across Phase 1 and Phase 2 depending on whether they have priority or not? Are subjects more or less likely to change their preference rankings after chat conditional on the strategy they submitted in Phase 1?

To answer Question 5, we aggregate subjects of Types 1-3 into a priority group and subjects of Type 4-5 into a non-priority group, and run two logit regressions in which Specification 5 examines the subset of subjects who submitted strategic preference rankings in Phase 1 and the dependent variable is a dummy variable indicating whether a subject switches from a strategic to a truthful preference submission, while Specification 6 investigates whether subjects who initially submitted truthful rankings switch to strategic preference rankings.[16] In both the specifications, the independent variables in-

[15]To ensure our results are not sensitive to the way we classify strategies, we rerun the above four regressions using a disaggregated definition, which allows 6 possible strategies of preference rankings and 36 possible changes between Phase 1 and Phase 2. The results, presented in Table 6′ in Appendix B, are qualitatively identical.

[16]Here we do not examine the changes from (to) the irrational strategy because only few subjects use the irrational strategy in our experiment.

clude an indicator of whether a subject has priority right and the interaction terms across mechanism and an indicator of whether a subject is allowed to chat.

Table 7 here.

When examining subjects who initially submitted strategic preference rankings, we find that subjects without priority rights are less likely to switch to truth-telling in Phase 2 than those with priority rights. However, when we look into the subset of subjects who initially submitted truthful preference rankings, the behavior difference with regard to priority rights disappears. In addition, our regressions show that the chat effect that we find from the pooled data only remains significant if subjects initially submitted strategic preference ranking in Phase 1, but it is not significant if subjects initially report their preferences truthfully.[17] The changes from the truthful to the strategic preference ranking, as we show in Appendix C, are more likely to be influenced by a subject's level of risk aversion.

As we mentioned at the start of this subsection, it is possible that these transitions, at the aggregate level, cancel each other out leaving the set of submitted preferences largely unchanged across phases. This is true to some extent in our data. As a comparison of Table 3 (presenting the distribution of strategies in Phase 1) and Table 8 (presenting the distribution of strategies in Phase 2) indicates, however, there are differences as well. For example, in the Boston 10 mechanism, while only 14.29% of Type 4 subjects in Phase 1 submit strategic preferences, this percentage increases to 42.86% in Phase 2. Similarly, the percentage of Type 5 subjects using the Gale-Shapley mechanisms and reporting strategic preferences increases from 53.66% to 65.85% across Phases 1 and Phase 2. Despite such differences, in many cases these changes tend to cancel each other out. Take the example of the Gale-Shapley mechanism. Although the fraction of subjects with priority who report truthfully increase from 38.21% in Phase 1 to 42.28% in Phase 2, the fraction of subjects who have no priority and tell the truth decrease from 41.46% in to 37.80%. As a result there is no significant difference in the the use of the truthful strategy across phases.

Table 8 here.

However, this observation does not negate our conclusion that chat influences behavior and matching outcomes which we will present below. Welfare and stability are influenced

[17]As we did above, we check whether the above results are sensitive to our classification of submitted preference rankings. More precisely, we examine whether subjects are more likely to change their strategies conditional on whether they submitted Strategy 1-2-3 in Phase 1. The results are presented in Table 7′ in Appendix B, and we again find no qualitative differences in these results.

more by which type of subjects change their behavior but not on the average across all types. For example, when examining the Gale-Shapley mechanism, we find that chat has a differential impact on subjects who have no priority than those who have. Among the subjects who chat, the fraction of those with priority telling the truth increases from 34.72% in Phase 1 to 44.44% in Phase 2, while the fraction of those without priority reporting truthfully remains the same across phases. In contrast, among the subjects who do not chat, it is the subjects without priority who are less likely to tell the truth. (The fraction of those using the truthful strategy decreases from 44.18% in Phase 1 to 35.29% in Phase 2). While these changes appear to cancel out, the fact that these changes are made by different types of subjects makes a big difference when it comes to stability and welfare. In addition, our aggregate results are conditional on the uniform distribution of types in our experiment. If the distribution of types is non-uniform, the same transitions between strategies could generate different aggregate results. This is why we have concentrated on the impact of chatting on changes but not absolute levels. Still, the cancellation of these changes at the aggregate level is an important phenomenon.

Question 6– Chat and Stability:

Does the percentage of stable matches increase from Phase 1 to Phase 2?

Given the preferences and priorities of our subjects, an outcome will be stable as long as no subjects of Types 1-3 are awarded their third-best objects.[18] Using this criterion to search for stable outcomes we find that in the Boston 16 mechanism 56.48% of the Phase 1 matches are stable while 56.99% of the Phase 2 matches are stable. The corresponding numbers for the Gale-Shapley mechanism are 68.75% and 85.45% respectively (p-value is 0.0000), while for the Boston 10 mechanism they are 17.16% and 49.65% respectively (p-value is 0.0000).

Several points are of note here. We find no increase in the stability of matches

[18]This can easily be established. The claim is that a match is stable if and only if no subjects of Types 1-3 are awarded their third-best objects.

Proof: (i) Stability $\Rightarrow$ No subjects of Types 1-3 are awarded their third-best objects.

Suppose a subject with priority right is allocated into her third-best. For the ease of exposition, say this subject is of Type 1 who has priority for Object A. Then (a) this subject prefers Object A to her current assignment, and (b) another subject who has a lower priority order than this subject is currently awarded Object A (because there are enough Objects A to satisfy the demand of Type 1) , which violates the condition of stability.

(ii) Stability $\Leftarrow$ No subjects of Types 1-3 are awarded their third-best objects.

Suppose the match is not stable. Then there exists a subject-object pair (i, s) such that (a) subject i prefers Object s to her current assignment; and (b) there is another subject with lower priority ranking than this subject assigned to Object s. Notice given our design subject i cannot be of Type 4 or Type 5 who have no priority for any objects because condition (b) cannot be satisfied. Furthermore, subject i cannot be of Type 1, 2 or 3 because we assume no subjects of Types 1-3 are awarded their third-best objects. Suppose subject i is Type 1 (or 2 or 3) and she is not allocated into her first-best object, condition (b) cannot be satisfied because no one has priority for Object C, the first-best school for Types 1-3.

across phases for the Boston 16 mechanism probably because, as we know, there was no significant change in the submitted preferences of subjects across phases. Second, notice that for the Gale-Shapley and Boston 10 mechanisms the frequency of stable matches increases considerably across phases. What is interesting is how few stable matches were created in Phase 1 of the Boston 10 mechanism and how dramatically they have increased there. This is probably the result of the fact that subjects in Phase 1 of the Boston 10 mechanisms submitted a large number of truthful preferences and those submissions lead many subjects to receive their third-best objects – exactly the circumstances where instability is likely to occur. Chat appears to have rectified this situation to some extent.

3.3 Welfare Changes

A standard welfare measure for experimental markets (of which matching is one example) calculates the fraction of the available surplus (i.e., the first-best payoff sum) that is captured by subjects in an experimental session. In our experiment, however, we care about the impact of chat on welfare and hence would like to compare the welfare of subjects who chat to those who do not, as well as the change in welfare across phases. This is not easily accomplished using the surplus measure, since that measure is an aggregate market measure encompassing both the isolated and the non-isolated subjects and there is no way to impute the changes in aggregate market welfare across phases separately to each group. We can, however, compare how the payoffs of the isolated and the non-isolated subjects change across phases, which we will do below.

To do this, we employ two different payoff measures and run regressions that investigate the impact of chatting on these payoff measures. The first measure, Payoff Difference, is simply the mean payoff difference of subjects between Phase 1 and Phase 2, while the second, Payoff Difference Dummy, is a discrete measure which assigns a -1 for any subject whose payoff decreases from Phase 1 to Phase 2 , a 0 for any subject whose payoff stays the same, and a $+1$ for any subject whose payoff increases. These differences are calculated for each subject using the mean payoff determined by our simulation, which takes account of the possibility that some outcomes are determined randomly by lottery draws.

The impact of chat on welfare may be influenced by a number of factors, such as whom one chats with (heterogeneous vs. homogeneous subnetwork), whether a subject chatting has a priority, and the type of matching mechanism used. Our next two questions concern the welfare impact of chatting under all of these circumstances.

Question 7 – Heterogeneity, Chat, and Payoff Increases:

Does allowing subjects to chat increase their payoffs across phases and do payoffs increase more when subjects chat with people like them or different from them, i.e., when they chat via heterogeneous or homogeneous networks?

We find that allowing subjects to chat either increases their payoffs across phases or, as is true in the Boston 10 mechanism, when payoffs decrease, they decrease less among those who chat. In addition, when we condition on whom the subjects chat with, we find that only when subjects chat with subjects unlike them, i.e., in heterogenous networks, do their payoffs increase.

To support this conclusion let us start by pointing out that across our three mechanisms since while the payoffs among those who chat increase by 0.7229 and 0.3128 in moving from Phase 1 to Phase 2 in the Boston 16 and the Gale-Shapley mechanisms, they decrease for those who do not (the payoff differences are −0.2097 and −0.6392, respectively). In the Boston 10 mechanism, payoffs decrease for both subjects who chat and who do not by −0.4974 and −0.7376, respectively. Note, however, that while the payoffs for subjects who chat decrease in the Boston 10 mechanism, the decrease is greater for those who do not chat. These mean payoff differences across subjects who chat and those who do not are significant for subjects using the Boston 16 and the Gale-Shapley mechanisms (p-values are 0.0159 and 0.0289,) but insignificant for the Boston 10 mechanism (p-value is 0.4140).

We further substantiate these results in a regressions analysis where we use our two payoff measures (Payoff Difference and Payoff Difference Dummy) as dependent variables and employ our usual set of right hand dummy variables such as a subject's type, a dummy designating whether she is allowed to chat, and the mechanisms used. For the payoff difference regression we use an OLS regression, while for the discrete payoff difference dummy variable we use an ordered logit regression.

Table 9 demonstrates several interesting facts. First, the chat variable is significantly positive in all regressions where it appears. That is, chat increases the average payoffs across Phase 1 and Phase 2, and also the likelihood that one's payoff will increase. Second, when we break the chat variable down by checking to whom subjects chat, it appears that only those who chat with people different from them increase their payoffs significantly. This result is interesting because we find that subjects in the homogeneous subnetworks are more likely to change their Phase-1 strategies. Hence, while speaking to people of one's own type may lead a subject to change her preference ranking, such changes do not necessarily lead to payoff increases. This might imply that there are some circumstances where not changing one's submitted preferences is advantageous and listening to people of a similar type may be damaging.

Table 9 here.

Question 8 - Welfare Changes, Mechanisms, Priority Rights and Chat: *Does chatting influence subject payoffs differently depending on whether they have priority rights or not?*

To answer Question 8, we run a simple OLS regression to examine the payoff difference across Phases 1 and Phase 2 for subjects with and without priority rights. We do this by running three separate regressions, one for each mechanism, where the dependent variable is the average payoff differences of subjects across phases, and the independent variables are interaction terms across an indicator of whether a subject has priority and an indicator of whether she is allowed to chat. The default situation is a subject with priority who is allowed to chat.

Table 10 here.

As Table 10 indicates, the impact of priority on the payoff differences varies across mechanisms. Among subjects who are allowed to chat in Boston 16 mechanism, the subjects without priority appear to experience a greater increase in their payoffs than those with priority. However, among subjects who are allowed to chat in the Gale-Shapley or the Boston 10 mechanism, there is no significant payoff difference between subjects who have priority and those who do not. Rather, in these two mechanisms, the chat has an impact when subjects have no priority and are isolated, in which case their payoffs decrease.

This result may be of interest for policy makers since if we consider subjects without priority (or with priority in sub-standard schools) to basically be those who inhabit lower income areas, it would appear that, if the city they lived in used the Gale-Shapley mechanism, they would benefit from being included in a better social network with others who could discuss the strategic elements of the mechanism they were using or a consumer education program that could advise them on what to do.

3.4 The Content of Chat

In this section of the paper we describe the chat that transpired between Phases 1 and Phase 2. In addition, we examine how the content of chat varies as we change the mechanism used, or condition on whether the chat is between subjects with (or without) priority, or condition on the strategies that subjects employed in Phase 1.

To do our analysis, we recorded all the messages exchanged among subjects during chat periods, and had two independent research assistants code the records. There were nine different categories that the chat text could be classified by. More precisely, a statement was coded with a T (Truthful) if it mentioned revealing one's truthful preference and a T- if it urged against doing so. Likewise, a message was coded as S (Strategic) if it encouraged listing one's second-best object first, and S- if the statement urged against being strategic. An I (Irrational) statement suggested an irrational submission, such as placing one's third-best first, and an I- statement warned against doing so. If a statement mentioned the lottery it was coded as L, and if a statement mentioned the scarcity of the objects in comparison to the possible demand it was coded as SC. Finally, a statement was coded with a C if subjects simply mentioned that chat might have an impact on the results. We allowed chat statements to be categorized by as many of the nine categories as was appropriate. When a subject repeated the same opinion several times, we counted every statement.

After our two research assistants coded each statement with as many labels as they saw fit, their codings were given to a third assistant, the judge, who compared them and, when they were in conflict, the judge decided which codings were best. There are surprisingly few conflicts between the coders.

Table 11 here.

Table 11 presents the percentage of each message type sent by those subjects who chat and those who are isolated across mechanisms. We find that in the Boston 16 mechanism, subjects discuss strategic preference submission almost twice as often as truthful preference rankings. For example, chat statements coded as S and T- constitute 45.8% of the meaningful chat statements while those coded as T and S- constitute only 26.0%. (Here we lump S and T- (T and S-) together, since an S- statement warning against being strategic is almost equivalent to suggesting that a subject report truthfully and the same is true for T- and S.) For the isolated subjects these percentages are 41.6% and 22.0%, respectively. This may explain why subjects in the Boston 16 mechanism chose not to change their strategies especially when they listed their second-best objects first in Phase 1, since chat served to reinforce the efficacy of submitting strategically. In contrast, subjects who chat in the Gale-Shapley mechanism talk about truthful and strategic preference submission with almost equal frequency (31.8% and 35.9%, respectively), while, in the Boston 10 mechanism, these percentages are 29.6% and 26.0%, respectively.

The differences between our dual baseline mechanisms are accentuated when we examine chat statements among subjects who communicate through homogeneous, heterogeneous and isolated subnetworks. For example, when subjects using the Gale-Shapley mechanism talk through homogeneous subnetworks they discuss truth-telling (strategic) preference ranking 34.2% (26.7%) of the time while, when they talk to others via a heterogeneous subnetwork, they discuss truth-telling (strategic preferences) 23.3% (37.9%) of the time. Finally, it is worthy of note that even when we pool all chat massages across all mechanisms and all types of subjects, we find that subjects who chat, whether in a homogeneous or heterogeneous subnetwork, chat in a different manner than those who are isolated (The p-values are 0.0000 for the comparison between both the heterogenous and homogeneous subnetworks and the isolated subnetworks).

What a subject chats about may, of course, depend on whether she has priority or not. We find that for each of our three mechanisms there is a significant difference in the distribution of chat messages sent by subjects with and without priority. These differences vary across mechanisms as we see in Figures 3(a)-(c).[19]

Figure 3 here.

A few more aspects of these distributions are of interest. First, note that subjects appear more to concern about the possibility of facing the lottery when the Gale-Shapley and Boston 10 mechanisms are used than when the Boston 16 mechanism is used. This is particularly true in the Boston 10 mechanism, where about 21.3% (20.6%) of the messages of subjects who chat concern the lottery for subjects with (without) priority.[20] Also, when scarcity is mentioned it is more likely to be mentioned by subjects without priority, since they are the ones who cannot guarantee themselves an object by acting strategically and hence have to worry about being frozen out of a preferred item. The dramatic difference across priority and non-priority subjects in their reference to irrational strategies, i.e., submitting one's least preferred object first, is puzzling but consistently higher for subjects without priority than those with it. For example, while in the Boston 16 mechanism 7.7% of the messages of non-priority subjects mention irrational preferences, only 3.1% of the messages of subject with priority do so. For the Gale-Shapley and the Boston 10 mechanisms, the percentages are 1.9% vs 14.6% and 4.0% vs 23.5%, respectively. It is hard to explain the use of irrational strategies and the differential reference to them by subjects with and without priority. One explanation is that since subjects without priority could not guarantee themselves their second-best objects by being strategic, they entertained exotic (and irrational) strategies.

Another relevant question is whether the messages that people send are related to the strategies that they used in Phase 1. In other words, do people who submitted truthfully in Phase 1 tend to suggest truth-telling and do those submitted strategic preferences suggest that strategy to their cohorts? The answer is yes as Figure 4 suggests.

[19] The p-values associated with the χ^2 tests under the null hypothesis that there are no differences in the distribution of chat statements across our 9 categories for subjects with and without priority are 0.023, 0.000 and 0.000 for the Boston 16, Gale-Shapley, and Boston 10 mechanisms, respectively.

[20] Remember that there are 12 subjects who have object C as their first choice and only 4 such objects available. Hence, if many subjects were to report truthfully, there would be a great scarcity of object C and many subjects would face a lottery since no subjects have priority for that object. This finding may explain why the fraction of subjects telling the truth significantly decreases after chat in the Boston 10 mechanism.

Figure 4 here.

As we see in Figure 4 , while the norm among chatters seems to offer advice that is identical to the strategy one just used, this is by no means the only advice offered. For example, in the Gale-Shapley mechanism, among those subjects who reported truthfully in Phase 1 almost 46% of the chat statements were coded as suggesting truth-telling in Phase 2. This rises to almost 50% if one adds the advice S-, which is warning against being strategic. The next largest category is for statements that urge strategic play in Phase 2 (15.8%). After these two categories, the rest of the advice is scattered over the remaining categories including a remarkable 12.9% urging the submission of the irrational strategies.[21] Similar results are found for those who played strategically in Phase 1, where, in the Gale-Shapley mechanism, 34.4% suggest using that strategy in Phase 2. Comparable results are found for the other treatments where the Boston mechanisms are used.

Finally one might ask what the rationale is that subjects use when urging the various strategies they do. For example, do those who suggest truth-telling in the Gale-Shapley mechanism do so because they have discovered it is a dominating strategy? Do subjects urging playing in a strategic manner understand that if one has priority then submitting one's second choice first guarantees getting one's second choice? The answer is basically that only a few subjects who urge truth-telling do so because it is dominant. Across all mechanisms, the rationale for being honest seems to be one of striving to get one's first-best choice despite the risks involved (this is especially true in the Boston 10 mechanism), while playing strategically is more associated with settling for one's second choice or playing it safe. This fact should not imply that subjects did not understand the mechanisms, they offer substantial evidence that many of them do, but despite this fact their strategies appear to be influenced by their desire to get their first-best objects or to settle for their second-best objects.

4 Conclusion

In this paper we have investigated the impact of communication on the performance of two frequently used school matching mechanisms, the Boston and the Gale-Shapley mechanisms. We study chat since, in the real world, parents are embedded in a set of social networks through which they communicate and discuss their preference submission

[21] Remember, these percentages are calculated based upon chatting statements that fall into these categories. It is possible that one person may have uttered several of these statements so this is not equivalent to saying that of the subjects who submitted truthfully in Phase 1 x% urged truth-telling in Phase 2.

strategies. By allowing chat in the lab we feel we are testing these mechanisms in a context closer to the way they are used.

What we find is interesting and we hope informative. First, chatting increases the likelihood that subjects will change their submitted preferences compared to those subjects who are isolated and do not chat. Second, chatting leads to an increase in the fraction of stable outcomes which is important from a policy point of view. With respect to welfare, chatting appears to influence welfare differently for subjects with and without priority rights. Though there is little welfare change among subjects with priority rights between phases, chatting does significantly change the welfare of those without priority rights. Furthermore, our results show that among subjects who chat, those chatting with others of a different type appear to increase their payoffs between phases more than those chatting with others like themselves. Combined with the result that subjects that chat with others like themselves tend to change their submitted preference rankings more between phases, this suggests that some of those preference changes are not beneficial. (Sometimes chat is beneficial if it persuades people not to change their already beneficial strategies.)

In terms of policy, our results indicate that the advice received through chatting via social networks can influence a subject's behavior and also the performance of the mechanism used. It is of more interest that being isolated when using the Gale-Shapley mechanism leads to a decrease in average payoffs when that isolation is combined with a lack of priority. As we say in the paper, this result may be of interest for policy makers since if we consider subjects without priority (or with priority in sub-standard schools) to basically be those who inhabit lower income areas, it would appear that, if the city they lived in used the Gale-Shapley mechanism, they would benefit from being included in a better social network with others who could discuss the strategic elements of the mechanism they were using or a consumer education program that could advise them on what to do.

References

Abdulkadiroğlu, A., Y.-K. Che, and Y. Yasuda (2011): "Resolving Conflicting Preferences in School Choice: The Boston Mechanism Reconsidered," *American Ecnomic Review*, 101(1), 399–410.

Abdulkadiroğlu, A., P. A. Pathak, and A. E. Roth (2005a): "The New York City High School Match," *American Economic Review, Papers and Proceedings*, 95, 364–367.

——— (2009): "Strategy-proofness versus Efficiency in Matching with Indifferences: Redesigning the NYC High School Match," *American Economic Review*, 99(5), 1954–1978.

Abdulkadiroğlu, A., P. A. Pathak, A. E. Roth, and T. Sönmez (2005b): "The Boston Public School Match," *American Economic Review, Papers and Proceedings*, 95, 368–371.

Abdulkadiroğlu, A. and T. Sönmez (2003): "School Choice: A Mechanism Design Approach," *American Economic Review*, 93(3), 729–747.

Balinski, M. and T. Sönmez (1999): "A Tale of Two Mechanisms: Student Placement," *Journal of Economic Theory*, 84, 73–94.

Braun, S., N. Dwenger, D. Kübler, and A. Westkamp (2014): "Implementing Quotas in University Admissions: An Experimental Analysis," *Games and Economic Behavior*, 85, 232–251.

Calsamiglia, C., G. Haeringer, and F. Klijn (2010): "Constrained School Choice: An Experimental Study," *American Economic Review*, 100(4), 1860–74.

Chen, Y. and O. Kesten (2013): "From Boston to Chinese Parallel to Deferred Acceptance: Theory and Experiments on a Family of School Choice Mechanisms," *Working Paper*.

Chen, Y. and T. Sönmez (2006): "School Choice: An Experimental Studay," *Jouranl of Economic Theory*, 127(1), 202–231.

Cooper, D. C. and J. H. Kagel (2012): "Failure to Communicate: An Experimental Investigation of the Effects of Advice on Strategic Play," *Working Paper*.

Ding, T. and A. Schotter (2015): "Learning and Mechanism Design: An Experimental Test of School Matching Mechanisms With Inter-generational Advice," *Working Paper*.

Dubins, L. E. and D. A. Freedman (1981): "Machiavelli and the Gale-Shapley algorithm," *American Mathematical Monthly*, 88, 485–494.

Erdic, A. and H. Ergin (2008): "What's the Matter with Tie-breaking? Improving Efficiency in School Choice," *American Economic Review*, 98, 669–689.

Ergin, H. I. and T. Sönmez (2006): "Games of School Choice Under the Boston Mechanism," *Journal of Public Economics*, 90, 215–237.

Featherstone, C. and M. Niederle (2014): "Improving on Strategy-proof School Choice Mechanisms: An Experimental Investigation," *Working Paper*.

Fischbacher, U. (2007): "z-Tree: Zurich Toolbox for Ready-made Economic Experiments," *Experimental Economics*, 10(2), 171–178.

Guillen, P. and R. Hakimov (2014): "Monkey See, Monkey Do: Truth-telling in Matching Algorithms and the Manipulation of Others," *Working Papers, University of Sydney*.

Guillen, P. and A. Hing (2014): "Lying Through Their Teeth: Third Party Advice and Truth Telling in a Strategy Proof Mechanism," *European Economic Review*, 70, 178–185.

Haeringer, G. and F. Klijn (2009): "Constrained School Choice," *Journal of Economic Theory*, 144(5), 1921–47.

Herszenhorn, D. (2003): "Revised Admissions for High School," *New York Times*.

Holt, C. A. and S. K. Laury (2002): "Risk Aversion and Incentive Effects," *American Economic Review*, 92(5), 1644–1655.

Iyengar, R. and A. Schotter (2008): "Learning Under Supervision: An Experimental Study," *Experimental Economics*, 11(2), 154–173.

Kesten, O. and M. U. Ünver (2013): "A Theory of School Choice Lotteries: Why Ties Should not be Broken Randomly," *Working Paper*.

Klijn, F., J. Pais, and M. Vorsatz (2013): "Preference Intensities and Risk Aversion in School Choice: A Laboratory Experiment," *Experimental Economics*, 16(1), 1–22.

Nyarko, Y., A. Schotter, and B. Sopher (2006): "On the Informational Content of Advice: A Theoretical and Experimental Study," *Economic Theory*, 29(2), 433–452.

Pais, J. and A. Pintér (2008): "School Choice and Information: An Experimental Study on Matching Nechanisms," *Games and Economic Behavior*, 64, 303–328.

Pathak, P. A. (2011): "The Mechanism Design Approach to Student Assignment," *Annual Review of Economics*, 3(c), 513–536.

Pathak, P. A. and J. Sethuraman (2011): "Lotteries in Student Assignment: An Equivalence Result," *Theoretical Economics*, 1 – 17.

Rick, S. and R. A. Weber (2010): "Meaningful Learning and Transfer of Learning in Games Played Repeatedly Without Feedback," *Game and Economic Behavior*, 68, 716–730.

Roth, A. E. (1982): "The Economics of Matching: Stability and Incentives," *Mathematics of Operation Research*, 7, 617–628.

Schotter, A. (2003): "Decision Making with Naive Advice," *American Economic Review*, 93(2), 196–201.

Schotter, A. and B. Sopher (2003): "Social Learning and Coordination Conventions in Intergenerational Games: An Experimental Study," *Journal of Political Economy*, 111, 498–529.

——— (2007): "Advice and Behavior in Intergenerational Ultimatum Games: An Experimental Approach," *Game and Economic Behavior*, 58(2), 365–393.

Toch, T. and C. Aldeman (2009): "Matchmaking: Enabling Mandatory Public School Choice in New York and Boston," *Education Sector*.

Table 3: Stated Preference in All Three Mechanisms: Phase 1 (Percent)

	Truthful				Strategic				Irrational			
	Boston 16	GS	Boston 10	p-value GS v.Boston 16 Boston 16 v.10 GS v.Boston 10	Boston 16	GS	Boston 10	p-value GS v.Boston 16 Boston 16 v.10 GS v.Boston 10	Boston 16	GS	Boston 10	p-value GS v.Boston 16 Boston 16 v.10 GS v.Boston 10
All	37.00	39.51	52.38	0.6040 0.0107 0.0324	58.00	53.66	38.10	0.3803 0.0009 0.0009	5.00	6.83	9.52	0.4361 0.1680 0.4260
Type 1	35.00	31.71	47.62	0.7570 0.3568 0.2416	65.00	58.54	52.38	0.5552 0.3568 0.6537	0.00	9.76	0.00	0.0440 N/A 0.0440
Type 2	40.00	48.78	61.90	0.4328 0.1097 0.3342	60.00	51.22	38.10	0.4328 0.1097 0.3342	0.00	0.00	0.00	N/A N/A N/A
Type 3	47.50	34.15	66.67	0.2268 0.1548 0.0160	52.50	60.98	33.33	0.4478 0.1548 0.0404	0.00	4.88	0.00	0.1598 N/A 0.1598
Type 4	27.50	48.78	57.14	0.0496 0.0305 0.5420	60.00	43.90	14.29	0.1509 0.0001 0.0100	12.50	7.32	28.57	0.4407 0.1687 0.0619
Type 5	35.00	34.15	28.57	0.9366 0.6143 0.6599	52.50	53.66	52.38	0.9181 0.9931 0.9260	12.50	12.20	19.05	0.9673 0.5273 0.5059
Priority	40.83	38.21	58.73	0.6775 0.0218 0.0083	59.17	56.91	41.27	0.7229 0.0218 0.0442	0.00	4.88	0.00	0.0137 N/A 0.0137
Non-Priority	31.25	41.46	42.86	0.1786 0.2169 0.8834	56.25	48.78	33.33	0.3442 0.0150 0.0975	12.50	9.76	23.81	0.5818 0.1425 0.0630

Table 4: Outcome Distributions in All Three Mechanisms: Phase 1 (Percent)

	Boston 16			Gale-Shapley			Boston 10			% H_0 not rejected by a χ^2 test
	First-best	Second-best	Third-best	First-best	Second-best	Third-best	First-best	Second-best	Third-best	Boston 16 vs.GS Boston 16 vs.10 GS vs. Boston 10
All	23.08	63.83	13.09	22.57	65.63	11.79	31.10	48.09	20.80	0.9928 0.1360 0.0612
Type 1	18.93	77.16	3.91	16.84	73.43	9.73	18.25	64.72	17.04	0.9452 0.6028 0.9624
Type 2	19.38	72.80	7.81	27.40	72.60	0.00	25.13	48.61	26.26	0.6348 0.3628 0.0032
Type 3	27.86	71.33	0.81	16.89	80.86	2.25	25.47	74.53	0.00	0.8432 0.9712 0.9140
Type 4	22.90	70.56	6.54	27.86	67.28	4.86	57.15	32.15	10.70	0.9904 0.0044 0.1140
Type 5	26.33	27.29	46.38	23.87	33.99	42.13	29.53	20.45	50.02	0.9732 0.9760 0.9132

Table 5: Fraction of Strategy Changes

	Boston 16	GS	Boston 10	Difference Boston 16 - GS Boston 16 - 10 Boston 10 - GS	p-value
All	0.2750	0.3512	0.3714	−0.0762	0.0985
	(0.4476)	(0.4785)	(0.4855)	−0.0964	0.0922
				−0.0202	0.7278
Isolated	0.3000	0.2706	0.2444	0.0294	0.6782
	(0.4611)	(0.4469)	(0.4346)	0.0556	0.5039
				−0.0262	0.7474
Non-isolated	0.2583	0.4083	0.4667	−0.1500	0.0136
	(0.4396)	(0.4936)	(0.5031)	−0.6694	0.0075
				0.2084	0.4620
Difference	0.0417	−0.1377	−0.2223		
(p-value)	(0.5245)	(0.0387)	(0.0172)		

Table 6: Strategy Change, Chat and Network Type

Variables	Specification 1 Coef. (Std. Err)	Specification 2 Coef. (Std. Err)	Specification 3 Coef. (Std. Err.)	Specification 4 Coef. (Std. Err.)
Type 1	−0.3413	−0.0358	−0.1468	−0.1497
	(0.3091)	(0.3104)	(0.3204)	(0.3218)
Type 2	0.2258	0.2285	0.2096	0.2122
	(0.3044)	(0.3056)	(0.3049)	(0.3062)
Type 3	0.1809	0.1832	0.1646	0.1668
	(0.3058)	(0.3067)	(0.3060)	(0.3073)
Type 4	0.3575	0.3605	0.3217	0.3245
	(0.3000)	(0.3012)	(0.3015)	(0.3029)
Chat	0.4274**	0.4395**	–	–
	(0.2008)	(0.2017)		
GS	–	0.3665*	–	0.3683*
		(0.2170)		(0.2175)
B10	–	0.4614*	–	0.4637
		(0.2585)		(0.2591)
Homo	–	–	0.6171***	0.6313***
			(0.2411)	0.2423
Hetero	–	–	0.2627	(0.2731)
			(0.2342)	0.2352
Constant	−1.1361***	−1.3937***	−1.1097***	−1.3684***
	(0.2524)	(0.2863)	(0.2529)	(0.2860)

$*$ = significant at 10%, $**$ = significant at 5%, $***$ =significant at 1%

Table 7: Strategy Change, Chat and Priority Rights

	Specification 5 Strategic → Truthful	Specification 6 Truthful → Strategic
Variable	Coeff. (Std. Err)	Coeff. (Std. Err)
Non-Priority	−0.6343*	−0.3407
	(0.3324)	(0.3126)
B16 × Chat	−0.1038	−0.3451
	(0.4994)	(0.5027)
GS × Chat	0.9708**	−0.0492
	(0.4211)	(0.4503)
B10 × Chat	0.9756*	0.4350
	(0.5526)	(0.4729)
GS × Iso	−0.1518	−0.3749
	(0.5575)	(0.5280)
B10 × Iso	−0.5302	−0.9582
	(0.8423)	(0.6432)
Constant	−1.2947***	−0.2260
	(0.3796)	(0.4151)

$*$ = significant at 10%, $**$ = significant at 5%, $***$ =significant at 1%

Table 8: Stated Preference in All Three Mechanisms: Phase 2 (Percent)

	Truthful				**Strategic**				**Irrational**			
	Boston 16	GS	Boston 10	p-value GS v.Boston 16 Boston 16 v.10 GS v.Boston 10	Boston 16	GS	Boston 10	p-value GS v.Boston 16 Boston 16 v.10 GS v.Boston 10	Boston 16	GS	Boston 10	p-value GS v.Boston 16 Boston 16 v.10 GS v.Boston 10
All	33.50	40.49	42.86	0.1459 0.1140 0.6907	62.50	55.61	49.52	0.1593 0.0313 0.3125	4.00	3.90	7.62	0.9599 0.2215 0.2070
Type 1	37.50	39.02	47.62	0.8895 0.4611 0.5302	62.50	58.54	52.38	0.7193 0.4611 0.6537	0.00	2.44	0.00	0.3233 N/A 0.3233
Type 2	42.50	39.02	47.62	0.7540 0.7104 0.5302	57.50	60.98	52.38	0.7540 0.7104 0.5302	0.00	0.00	0.00	N/A N/A N/A
Type 3	32.50	48.78	57.14	0.1391 0.0730 0.5420	67.50	51.22	42.86	0.1391 0.0730 0.5420	0.00	0.00	0.00	N/A N/A N/A
Type 4	25.00	46.34	33.33	0.0455 0.5130 0.3287	65.00	41.46	42.86	0.0340 0.1076 0.9185	10.00	12.20	23.81	0.7567 0.2052 0.2919
Type 5	30.00	29.27	28.57	0.9434 0.9095 0.9555	60.00	65.85	57.14	0.5911 0.8342 0.5185	10.00	4.88	14.29	0.3874 0.6435 0.2797
Priority	37.50	42.28	50.79	0.4491 0.0887 0.2749	62.50	56.91	49.21	0.3765 0.0887 0.3235	0.00	0.81	0.00	0.3193 N/A 0.3193
Non-Priority	27.50	37.80	30.95	0.1638 0.6957 0.4489	62.50	53.66	50.00	0.2568 0.1929 0.7034	10.00	8.54	19.05	0.7501 0.2007 0.1313

Table 9: Chatting and Payoff Differences

	Specification 7 Difference	**Specification 8** Difference	**Specification 9** Difference	**Specification 10** Difference Dummy	**Specification 11** Difference Dummy	**Specification 12** Difference Dummy
Variable	Coeff. (Std. Err)	Coeff. (Std. Err)	Coeff. (Std. Err)	Coeff. (Std. Err)	Coeff. (Std. Err)	Coeff. (Std. Err)
Type 1	−0.0508	−0.5048	−0.4541	−0.6955	−0.0688	0.0438
	(0.5861)	(0.5852)	(0.6047)	(0.2694)	(0.2969)	(0.2789)
Type 2	−0.9576	−0.9591	−0.9516	−0.4053	−0.4030	−0.3855
	(0.5824)	(0.5816)	(0.5825)	(0.2712)	(0.2707)	(0.2709)
Type 3	−0.7440	−0.7455	−0.7380	−0.1229	−0.1182	−0.1108
	(0.5824)	(0.5816)	(0.5825)	(0.2706)	(0.2707)	(0.2706)
Type 4	−2.002***	−2.000***	−1.983***	−0.3961	−0.4107	−0.3731
	(0.5812)	(0.5804)	(0.5829)	(0.2730)	(0.2732)	(0.2740)
Chat	0.7559**	0.7398**	-	0.4432***	0.4398***	–
	(0.3827)	(0.3822)		(0.1692)	(0.1694)	
GS	–	−0.4209	−0.4210	–	−0.3070*	−0.3059*
		(0.4119)	(0.4123)		(0.1830)	(0.1831)
B10	–	−0.9289*	−0.9291*	–	−0.1303	0.1310
		(0.4996)	(0.5000)		(0.2245)	(0.2249)
Homo	–	–	0.6489	–	–	0.2466
			(0.4681)			(0.2090)
Hetero	–	–	0.8168*	–	–	0.6023***
			(0.4454)			(0.1989)
Constant	0.3779	0.7478	0.7355	–	–	–
	(0.4686)	(0.5227)	(0.5244)			

∗ = significant at 10%, ∗∗ = significant at 5%, ∗ ∗ ∗ = significant at 1%

Table 10: Payoff Difference, Chat, Mechanism and Priority Rights

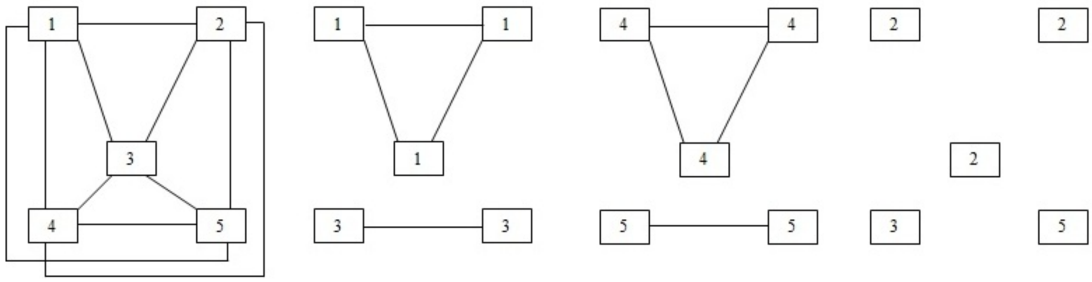
	Boston 16 Specification 13	Gale-Shapley Specification 14	Boston 10 Specification 15
Variable	Coeff. (Std. Err)	Coeff. (Std. Err)	Coeff. (Std. Err)
Non-Priority × Chat	2.1623***	−0.4761	−2.0551
	(0.7248)	(0.6541)	(1.4560)
Non-Priority × Iso	0.0122	−1.8594**	−2.6960*
	(0.8264)	(0.7305)	(1.5950)
Priority × Iso	−0.1207	−0.6645	0.0269
	(0.7248)	(0.6425)	(1.4067)
Constant	−0.1421	0.5032	0.3247
	(0.4584)	(0.4137)	(0.9209)

∗ = significant at 10%, ∗∗ = significant at 5%, ∗ ∗ ∗ =significant at 1%

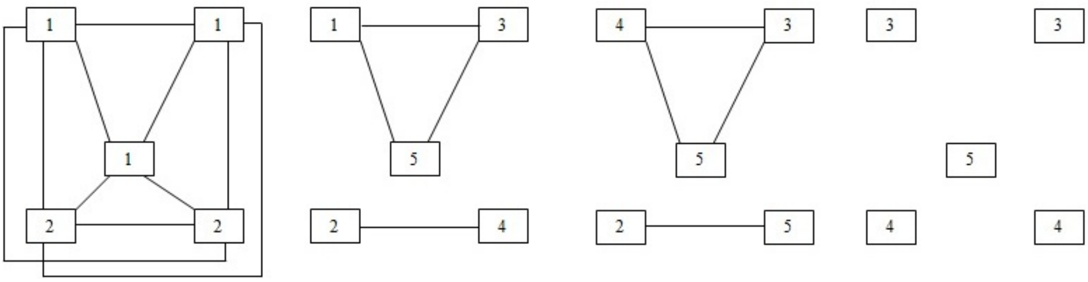
Table 11: Chat Records: Message Type

	Boston 16		**Gale-Shapley**		**Boston 10**	
	Chat	Isolated	Chat	Isolated	Chat	Isolated
T	0.2418	0.1883	0.2915	0.2184	0.2606	0.2091
T-	0.0293	0.0779	0.0404	0.0825	0.0493	0.0455
S	0.4286	0.3377	0.3184	0.2573	0.2113	0.1727
S-	0.0183	0.0325	0.0224	0.0388	0.0352	0.0545
I	0.0440	0.0584	0.0717	0.0631	0.1197	0.1182
I-	0.0110	0.0260	0.0224	0.0728	0.0141	0.0455
SC	0.0769	0.0909	0.0538	0.1359	0.0423	0.2000
L	0.0879	0.1753	0.1525	0.1311	0.2535	0.1545
C	0.0623	0.0130	0.0269	0.0000	0.0141	0.0000

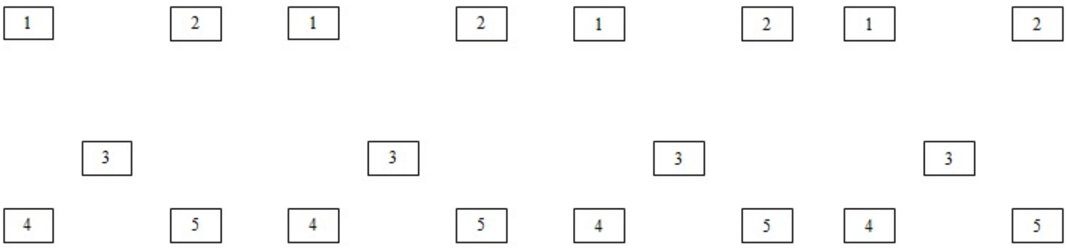
Figure 1: Network Structures



(a) Network 1: Heterogeneous Complete Subnetwork; Homogeneous Incomplete Subnetworks



(b) Network 2: Homogeneous Complete Subnetwork; Heterogeneous Incomplete Subnetworks



(c) Network 3: All Subjects Are Isolated

Figure 2: Phase 1 Submitted Preference Rankings

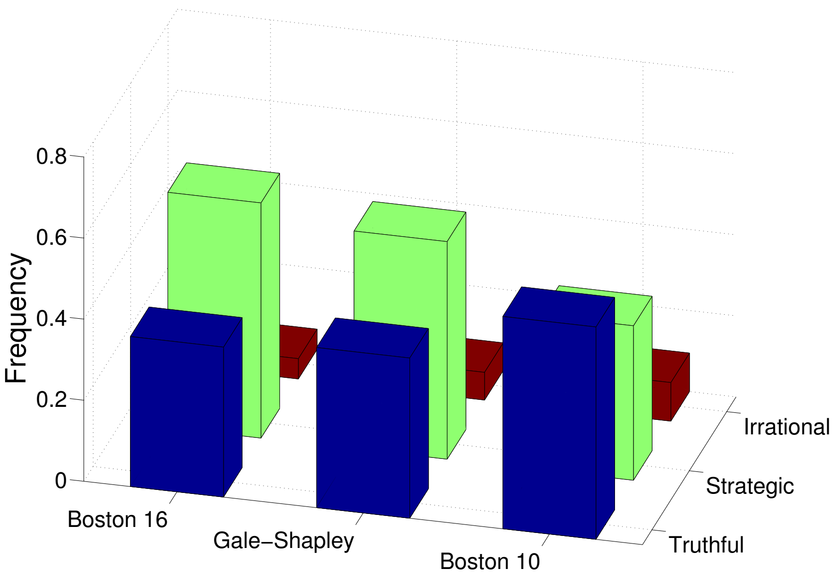
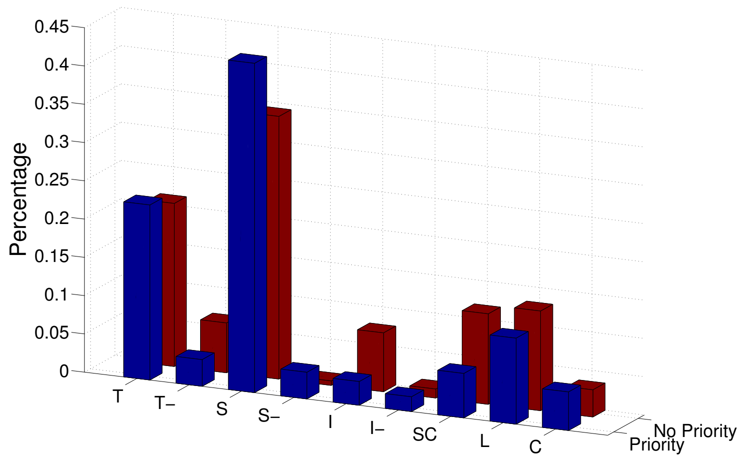
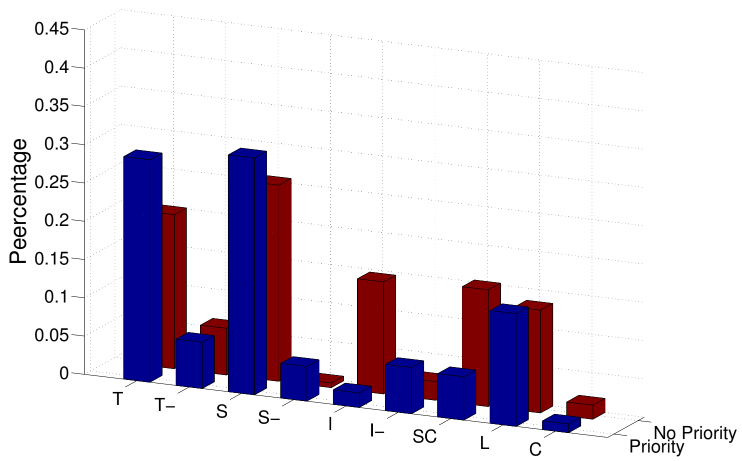


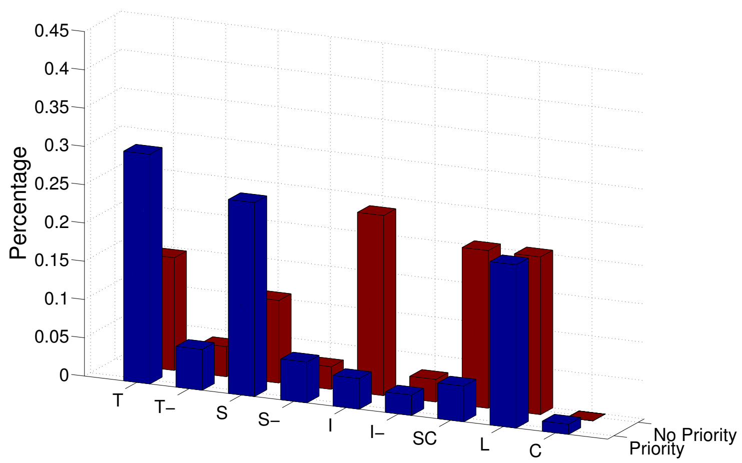
Figure 3: Distribution of Chat: Priority vs. Non-Priority



(a) Boston 16 Mechanism

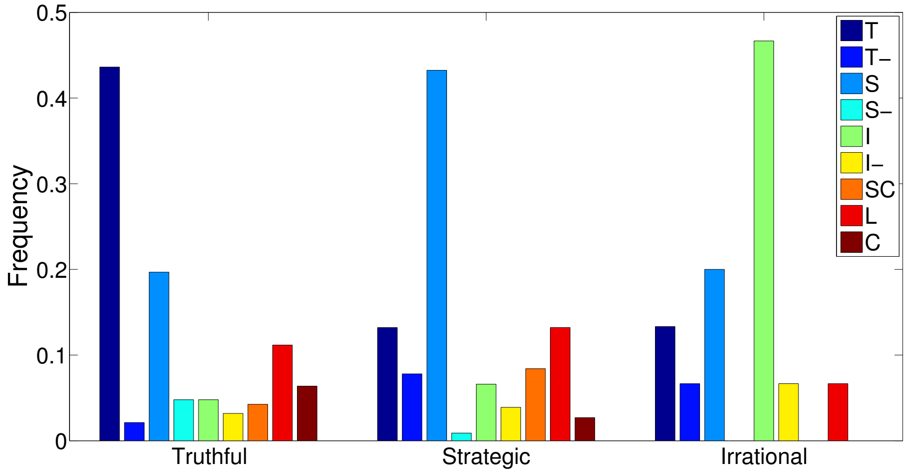


(b) Gale-Shapley Mechanism

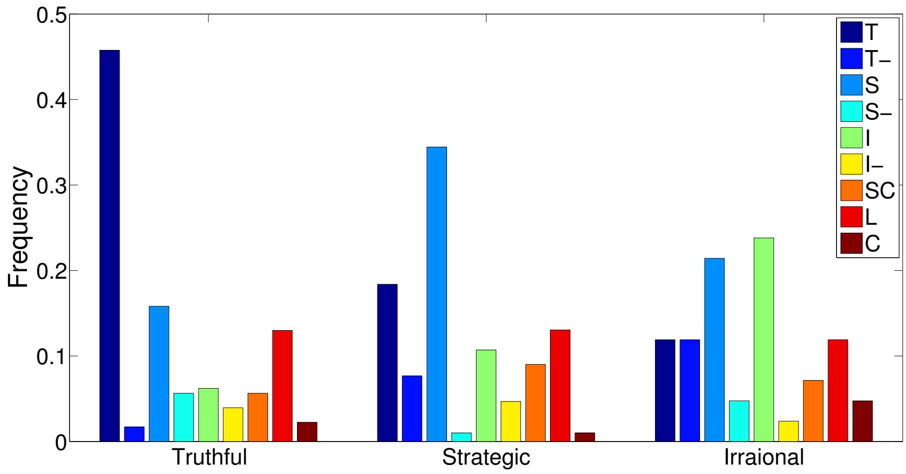


(c) Boston 10 Mechanism

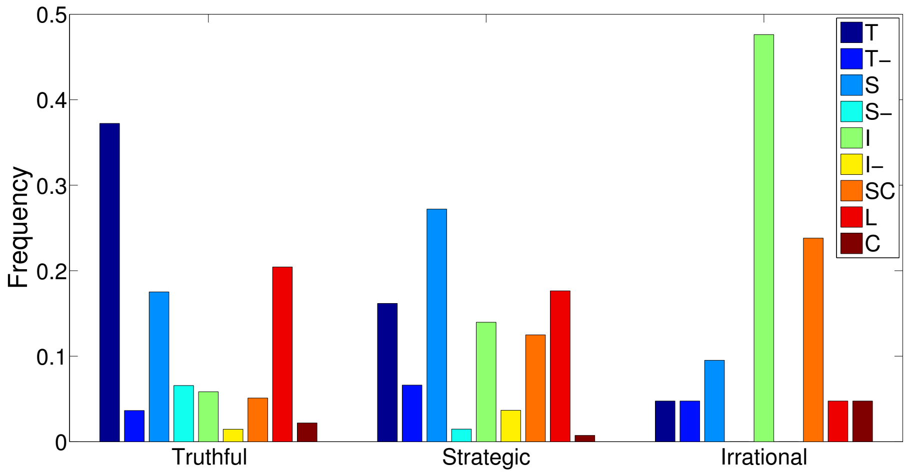
Figure 4: Messages Condition on Strategies used in Phase 1



(a) Boston 16 Mechanism



(b) Gale-Shapley Mechanism



(c) Boston 10 Mechanism