An Experiment on a Core Controversy*

Huibin Yan† Daniel Friedman‡

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†Department of Economics, University of California, Santa Cruz, CA 95064, hyan@ucsc.edu

‡Department of Economics, University of California, Santa Cruz, CA 95064, dan@ucsc.edu

Abstract

A longstanding criticism of the core is its sensitivity to small changes in player numbers, embodied in a well known example where one extra seller (resp. buyer) causes the entire surplus to go to the buyer's (seller's) side. We test this example in the lab, using different trading institutions that are designed to facilitate collusion. We find that successful collusion is infrequent even with the best collusion opportunities and, consistent with theory, the lion's share of the surplus typically goes to the less numerous side. Our study also sheds light on the boundaries of competitive equilibrium, the behavioral asymmetries between buyers and sellers, and the comparison of trading institutions.

Keywords: core, collusion, experiment, fairness, market

JEL codes: C71, C78, C92, D43

"The notion of the core has attracted the interest of economists for more than 100 years. Much of the core's appeal stems from the intuitive and natural story behind it, the story that first motivated F. Y. Edgeworth in 1881. It runs as follows. An allocation which is not the core is regarded as unstable. It is always the case, given such an allocation, that some individuals will be able to form a coalition and obtain an allocation that each strictly prefers." –Perry and Reny, 1994, p. 795

1 Introduction

The concept of the core is generally regarded as the most important contribution to economics made by cooperative game theory. Among other things, it provides a game theoretic foundation for the competitive equilibrium (CE). A well known critique of the core, however, is its seemingly excessive sensitivity to small changes in the numbers of players (e.g., Perry and Reny, 1994). The standard example is a market game in which each buyer and each seller wants to buy or sell one and only one indivisible unit of a good. All buyers have the same value v and all sellers have the same cost $c < v$. The core predicts a unique outcome when there are unequal numbers of buyers and sellers, namely that all the trade surplus goes to the less numerous side (called the short side hereafter) of the market. In particular, one extra trader on one side shifts the entire surplus to the other side.

The critique holds that the long side traders (the more numerous group, slated to receive zero payoff) may find a way to collude and seize some of the surplus. And as suggested by recent work in behavioral economics, fairness norms might cause traders on either side to reject these extreme allocations. If both forces work in tandem, we might even see fairness-motivated collusion.

In this paper we take the controversy to the laboratory. To give collusion its best shot, we examine small number cases, with two or three players on the long side and exactly one fewer player on the short side. Core theory doesn't specify the trading institution. Informed by recent advances in both theory and laboratory experiments, we choose to augment the standard computerized continuous double auction (CDA henceforth) with collusion opportunities. The first augmented version (DA-Chat) allows the subjects to have free preplay communication in computer chatrooms. The other augmented version is a novel DA-based bargaining environment (DA-Barg) that facilitates and implements collusive profit sharing agreements in addition to having an open chatroom throughout the trading period.

Section 2 reviews relevant literature, including key theoretical articles as well as previous laboratory studies. The most closely related laboratory studies are some pioneering experiments conducted in the 70's, and there are also related experiments in political science and market competitivity. Section 3 reviews both the cooperative and the noncooperative theories of the core, and notes their implications for laboratory studies. Section 4 lays out our experimental design and the testable implications. Section 5 presents the laboratory results. Following a concluding discussion, Appendix A presents supplementary regressions and robustness checks, Appendix B explains details in the coding of chatroom dialogues and collusion activities, and Appendix C reproduces sample instructions and quiz for the subjects.

2 Related Literature

The core was introduced formally in Shapley (1952) and Gillies (1953), though its conceptual roots go back to Edgeworth (1881). A more recent theoretical literature following Selten (1981) builds noncooperative foundations for the core through constructing noncooperative bargaining games from which core and only core outcomes emerge in equilibrium; see Perry and Reny (1994) for a prominent example, and see the introduction in Yan (2003) for a brief survey. Insights from this recent literature inform our implementation, as noted in section 3.2.

In a pioneering series of experiments, Murnighan et al. (1977, 1978, 1980) tested the core prediction against other cooperative solution concepts in the "left-shoe-right-shoe" context with just one short sider and two to eleven long siders. Murnighan et al. (1977, 1980) in particular covered the case of one short sider and two long siders. Using bargaining institutions far different from ours (characterized by sequences of simultaneous bilateral offers)[1]

[1] Specifically, one long sider might offer to buy the other long sider's shoe first and if successful then offer

and fixed matchings and volunteer subjects or subjects participating for course credits, they found that better information and communication opportunities fostered more effective collusion among long siders. With two long siders, their short siders on average kept only 55% to 60% of the total surplus under information and communication conditions closest to our DA-Barg, and even in their minimal information and communication treatments the short siders averaged no more than 70% to 83% of the surplus.

About the same time, Fiorina and Plott (1978) launched a laboratory investigation of the core (and other cooperative solution concepts) in voting games. They found the core did a good job of predicting the outcome of majority rule voting, although Eavey and Miller (1984) found that "fair" alternatives outside the core did even better.

Because the core generally contains the CE, the unique core allocation in our example coincides with the CE, and we will discuss how to view the example with respect to the two theories in our theoretical section. We examine exceptionally thin markets with unusual collusion opportunities, so our study also contributes to the so-called "boundary studies" of the CE. A major difference between our study and the experimental CE literature is that we make the demand and supply parameters public information as suggested by core theory, while this literature typically keeps them private information, so subjects may not be aware of their market power. Even with this difference put aside, the related boundary studies give mixed signals of what to expect in our setting. Smith and Williams (1990) report early computerized market settings, including several sessions with identical valuations and identical costs. These sessions featured a CDA with 4 multi-unit buyers and 4 multi-unit sellers and sizeable imbalances (either 16 units demanded and 11 units supplied, or the reverse). They found convergence to near the CE, with over 90% of surplus typically going to the short side after 3 or 4 periods. Also imputing identical valuations and identical costs, Friedman and Ostroy (1995) run 4-buyer-4-seller (4x4 hereafter) standard CDA markets with slightly imbalanced demand and supply parameters but with essentially divisible goods, and they found slow convergence towards the CE price.[2] Isaac et al. (1984) allowed one side of a

to buy the short sider's shoe.

[2]A few other studies may come to the reader's mind that are not as closely related to our studies. As a counterpoint of their finding of cross-country differences in ultimatum game outcomes, Roth et al. (1991)

4x4 market to collude, in the form of a one-time face-to-face free communication for up to 4 minutes when the session was paused, and still found rather competitive outcomes in the CDA. Joyce (1984) studied collusion in a hybrid market institution with some CDA and some Call Market features and reported that buyers were better than sellers at colluding and extracting surplus.

3 Theoretical Background

Intuitively, the core consists of allocations that are stable in the sense that no subgroup of players can break away and achieve better payoffs for all its members.

To formalize, let N denote the set of all players. Any nonempty subset $T \subseteq N$ of players is called a *coalition*, and $|T| \geq 1$ denotes the number of players in T. The *grand coalition* consists of all players, $T = N$. A *characteristic function* is a mapping v from the set of all coalitions into $\Re_+$. The number $v(T)$ is called the *worth* of coalition T; it may be interpreted as the size of the surplus available to the members of the coalition T. The pair (v, N) defines a cooperative game with transferable utility.

An *allocation* for coalition T is an element in $\Re_+^{|T|}$, written as $\omega = (\omega_i)_{i \in T}$. It is *feasible* if $\sum_{i \in T} \omega_i \leq v(T)$. A coalition T is said to *block* an allocation for the grand coalition $\omega = (\omega_i)_{i \in N}$ if $v(T) > \sum_{i \in T} \omega_i$. The *core* is the set of all the feasible allocations for the grand coalition that cannot be blocked by any coalition.

used 1-seller-versus-9-buyer market games for control, showing that in all the countries the market games lead to the same all-surplus-going-to-the-seller outcome as predicted by the competitive equilibrium. Their results are not directly comparable to ours because in their setting an individual buyer has no impact on the equilibrium price. Cason and Noussair (2007) also studied very thin markets, with only 2 single unit buyers and 3 single unit sellers. But they investigated price dispersion when there were search frictions in Posted Offer markets, an issue almost polar to ours.

3.1 The Buyer-Seller Example as a Cooperative Game

Now let N be the union of two disjoint player subsets, B and S, called buyers and sellers respectively. Let the worth of any coalition of one buyer and one seller be 1, interpreted as the total trade surplus in one transaction. Then set the worth of any coalition T equal to the number of buyer-seller pairs in T, that is, $v(T) = min\{|B \cap T|, |S \cap T|\}$. The game is called a *buyers' market*, and B is called the *short side*, if $|B| < |S|$. The opposite case, $|B| > |S|$, is called a *sellers' market* because now S is the short side. The *long side*, of course, is the other subset of players: S in a buyers' market and B in a sellers' market.[3]

It is straightforward to show that the core in such games is unique, non-empty, and extreme: the entire surplus goes to the short side players, who divide it equally. While this result is well known, we find no published proof, so we provide one here.

Proposition 1 *A buyers' (sellers') market has a unique core allocation ω. We have $\omega_i = 1$ if player i is a short sider and otherwise $\omega_i = 0$.*

Proof: Consider a buyers' market. We will first show that, fixing any seller i, he must get zero payoff in any core allocation ω. Consider the coalition of the remaining players $T = N \backslash \{i\}$. By the definition of buyers' market, T still contains at least as many sellers as buyers. Hence the specification of v ensures that T has the same worth as the grand coalition, i.e., $v(T) = v(N)$. Hence T blocks any allocation that gives a positive payoff to i. Therefore, any core allocation ω must satisfy $\omega_i = 0$.

We now show that each buyer gets 1. Remember that any coalition consisting of one buyer and one seller has worth 1, so for any $i \in S$ and $j \in B$, we must have $\omega_i + \omega_j \geq 1$. Since $\omega_i = 0$, we must have $\omega_j \geq 1$. But with each buyer getting at least 1, the only feasible allocation is that each buyer gets exactly $\omega_j = 1$.

It is easy to see that no subcoalition blocks ω: for any $T \subseteq N$, we have $\sum_{i \in T} \omega_i = |B \cap T| \geq min\{|B \cap T|, |S \cap T|\} = v(T)$. So the core allocation is indeed unique and as specified.

[3] The same characteristic function is sometimes interpreted in terms of the sets $B = L$ and $S = R$ of owners of left shoes and right shoes, or other Leontief-complementary pairs of goods.

The sellers' market is symmetric. $\diamond$

The intuition is transparent in a market with one buyer and two sellers. Any proposed allocation that gives either seller a positive payoff leaves a joint payoff of less than one to the buyer and the other seller, who can form a coalition that blocks the proposed allocation.

In a typical market transaction, no transfer (i.e. profit sharing) is possible between traders on the same side, so a trader unable to trade must receive zero. This can be captured by restricting feasible allocations to those assigning zero payoffs to redundant traders. In a market with one buyer and two sellers for example, at least one seller receives zero in any allocation in this restricted game. And it is easy to see that the above intuition, and in fact the proof of Proposition 1 and hence the unique core prediction holds for such a restricted (partially transferable utility) game as well.

The critique holds that collusion among long siders may cause the core prediction to fail. It is useful at this point to clarify that even though the word "collusion" has a connotation of forming a coalition, in core theory the long siders by themselves can only form zero-worth coalitions, which obviously do not bring positive returns to the participants. So how should we define collusion in this context? Recall that it is the blocking of noncore allocations that lead to the core outcome, so we can define collusion based on the absence of blocking when the blocking would hurt at least one other same sider. In the typical market setting for example, the relevant blocking takes the form of undercutting a same sider's price (since if successful this would make, in a 1x2 game for example, both the short sider and the undercutting long sider better off), so we can define collusion as failing to undercut a same sider's price when it is possible. When transfers are possible among same siders, any proposed allocation whereby a trader failing to trade is still to obtain a positive payoff can be blocked such as through giving zero payoff to this trader and dividing his original payoff among the rest of the players. So in such a setting collusion also includes failure to block such an allocation.

As mentioned in the introduction, the unique core allocation coincides with the CE in this example. To see this, consider a market where all sellers have the same opportunity cost $c \geq 0$ and all buyers have the same reservation value $r = c + 1$. Then the surplus (potential gains from trade) within any coalition is as specified above. The CE is by definition a price

p and post-trade allocation such that desired sales at p equal desired purchases. It is easy to see that the unique CE price is $p = r$ in a sellers' market and is $p = c$ in a buyers' market. For example, in the 2x3 game in Figure 1, all sellers have cost \$55 and all buyers have value \$89, resulting in a CE price of 55.

For example, the 2x3 game where a seller has cost \$55 and a buyer has value \$89 has a (flat) demand and supply configuration as in Figure 1.

—Insert Figure 1—

So is our study a test of the core or of the CE? Because of the dramatic market power possessed by individual long siders in this example and the collusion opportunities we provide, the CE theory does not assert the extreme prediction as forcefully as the core, and perhaps it is for this reason that the critique embodied in the example has always been known to concern the core. Moreover, full implementation of core theory requires that transfers among same siders be allowed, and such agreements would be slightly contrived for the typical CE testing environment. Therefore we regard our study primarily as a test of the core, and secondarily as a stress test of the CE as we present it with unusual challenges.

3.2 Implementation Issues

As can be seen from the above, the (cooperative) theory of the core places few constraints on how players interact. We will seek guidance from recent advances in both theoretical and experimental economics. We want to have a procedure that gives collusion a good chance from a behavioral view point while still compatible with the theories, both cooperative and noncooperative. In the discussion below we use the word "equilibrium" when viewing a procedure as a noncooperative game. On the theory side, recall that the literature on the noncooperative foundations of the core seeks to construct noncooperative bargaining games that generate core and only core outcomes in equilibrium. The models in this literature typically share the following features: complete information, binding agreements allowed only for allocations prescribed by the characteristic function, and one-shot interactions. We shall also obey these conditions and the intuition is easy to see, as described below.

To see why we need complete information, recall the blocking coalition described right after the proof of the proposition. That buyer and the other seller must be aware that they can form the coalition and split its worth of 1 in a way that leaves both better off than in the proposed allocation. To ensure such awareness, all buyer values and all seller costs should be public information. Another way to see this is note that, with private values and costs the core outcome would never obtain in equilibrium. This is because the core outcome requires full surplus extraction by one side of the market, and this is commonly known to be impossible in the mechanism design literature.[4]

It is well known in noncooperative game theory that access to commitment devices, and to binding agreements in particular, often drastically changes the outcome of a game. The noncooperative foundations literature typically allows binding agreements only for outcomes prescribed by the characteristic function. This restriction rules out, in particular, an irrevocable agreement between long siders where one side sells his trading right. If such agreements were allowed, the procedure would be unlikely to produce the core outcome: the core outcome would obtain in equilibrium only if after one long sider sells his trading right to the other, the latter could only obtain zero payoff in the ensuing (bilateral in the case of 1x2 games) bargaining with a short sider, because if the active long sider obtains any positive payoff in the ensuing bargaining, backward induction would lead him to entice the first long sider into an agreement, and the first long sider has no reason to reject a positive offer when zero is his equilibrium payoff.

Finally, the core pertains to one-shot interactions— a coalition blocks only if it can split its current worth to mutual immediate benefit. All sorts of outcomes are possible in repeated interaction, in which players sacrifice now in the hope of getting greater future gains. Such arrangements, however, are outside the scope of core theory. Thus, in testing the core prediction, we shall avoid repeatedly matching the same individual buyers and sellers.

[4] The intuition is that with values and costs believed to be randomly drawn from some probability distributions, a long sider, say a buyer, (rationally) believes that he can always (except when his is the lowest possible value) bid a bit below his true value and still have positive probability of transacting and hence earn positive expected payoff, assuming that the other buyers bid their true values.

A question that may come to the reader's mind is why we do not choose a noncooperative model from the noncooperative foundations literature and implement it in the lab. There are two reasons. One is that this literature seeks to support only core outcomes in equilibrium, and this is more than we need in the sense that our results will be stronger if the core outcome emerges from a procedure that allows noncore equilibrium outcomes as well. The second reason is behavioral. Bargaining in these models typically proceeds one proposal at a time, and each proposal specifies a feasible allocation for some coalition whose members immediately vote on it. It is implemented if all members vote in favor, but if even one member opposes it then it is discarded and someone can make a new proposal. In our context this means that the short sider can veto any long sider proposal involving the short sider, and since any feasible allocation involving positive payoffs to long siders necessarily involves at least one short sider, the short sider could immediately take off the table any long side proposal attempting collusion. It is easy to see that behaviorally this would not give collusion a fair shot.

For both these reasons we decide to employ natural trading institutions in which a trader's bid or offer remains valid until retracted by himself. Specifically, decades of market experiments show that CDA reliably produces convergence in the time frame allowed in a lab experiment, and it is used widely in practice (e.g. in the Chicago trading pits, the NYSE, and new electronic exchanges). Furthermore, it is neutral, in the sense that *a priori* it does not favor either side of the market. We will investigate three CDA-based trading institutions: the standard CDA (DA-Std), the CDA augmented with free preplay communication (DA-Chat), and the CDA augmented with bargaining features (DA-Barg) that allows collusive profit sharing agreements. The core prediction applies in all three institutions even though collusion opportunities differ.

Finally, we choose to implement a near perfect information procedure (with the few exceptions noted in the paper) whereby a player is fully informed of what all the other players have said and done before making his own move. Murnighan et al. (1977, 1978, 1980) consistently find that communication and information availability (when they make a difference) hurts short siders' payoffs through enhancing collusion effectiveness. Therefore this feature is consistent with our general spirit of facilitating collusion. In particular, one might wonder whether collusion could be sustained if we allow players, especially the long siders, to keep the existence and/or terms of their collusion agreements secret, but note that the agreement can be blocked by the long siders themselves through undercutting and/or retraction of profit sharing agreements[5].

How many buyers and sellers should we use? To challenge the core prediction, we will use just one extra long side trader, that is, $n \times (n+1)$ market games. While the core prediction may seem more striking for larger numbers of players, collusion is harder to sustain when more people are involved. There is well documented evidence in this regard from the experimental IO literature; see, for example, Dufwenberg and Gneezy (2000), Huck et al. (2004), Issac et al. (1984), and Harrison and Mckee (1985). Therefore we will use the minimal numbers of traders, with $n = 1, 2$ as the number of short siders. Our results (regression 5 of Table 5, among other results) confirm that indeed collusion is harder in $2x3$ than in $1x2$ markets.

4 The Experiment

4.1 The Trading Institutions

In our standard CDA, sellers and buyers could submit bids and asks at any time, simply by dragging a slider or typing in the desired price, and clicking a Bid or Ask button. These offers were displayed on the screens of every trader in the marketplace, as in Figure 2. Transactions occured immediately at the older price whenever a bid and ask overlapped, or when a trader selected an offer from the other side of the market and clicked the Buy or Sell button. CDA trading lasted 60 (for 1x2 sessions) or 95 (for 2x3 sessions) seconds each period. See Appendix C for details.

—Insert Figure 2—

In DA-Chat sessions, we begin with two standard CDA periods (excluded from main data

[5]Since any bid or ask represents a better deal for a stand-alone trader than for one having to share the profit, the same bid or ask after retraction constitutes a blocking proposal.

analysis) as it rarely occured to our pilot subjects to use the chat facility for collusion in the first periods.[6] We enable the chat facility in period three and thereafter each trading period is preceded by 2 (for 1x2 markets) or 3 (for 2x3 markets) minutes' preplay communication. Subjects enter a computerized chatroom where they may type messages seen by everyone in their marketplace. Such talk is cheap in that there is no enforcement mechanism. The bids and asks in the CDA are shown without revealing the trader's identity, e.g. seller A or B, in order to prevent the short sider(s) from punishing the long sider who initiates collusion in the chatroom.

—Insert Figure 3—

Unlike DA-Std and DA-Chat, the third institution, DA-Barg permits transfers among long siders. Adapting a MarketLink feature, DA-Barg opens a parallel collusion market, referred to as "Market 2" on traders' screens, in addition to leaving the chatroom open throughout each 3-minute trading period. In Market 2, as shown in Figure 3, long side traders can negotiate contracts to collude. They click tabs to switch between this screen and "Market 1", the standard MarketLink CDA supplemented with a bulletin board that reports all activities in both markets. In a 1x2 sellers' market, for example, the two buyers negotiate in Market 2 to have one buyer give up the right to trade in Market 1 in return for some percentage of the profit of the other buyer.[7] The screens reveal the participant labels, e.g., Buyer A or B, and the seller can view their negotiation in the bulletin board. The upper left area of the collusion screen is where they send proposals and counter proposals negotiating the percentages and who should give up trading. The computer enforces any agreement active at the time of a transaction. The chatroom is open to all players throughout a period.

The collusion agreement is retractable unilaterally by either long side trader without penalty

[6] We found no evidence of anchoring effects from having standard CDA in the first two periods; see Appendix A.1. Skipping the chatroom in these periods saves about 10 minutes in total and keeps the session within 2 hours, which is particularly relevant for DA-Chat as the experiment activity is simple and pilot subjects often complained of boredom.

[7] DA-Chat allows a limited form of profit splitting: if both long siders in a 1x2 market stick to a preplay collusion agreement to hold the same price, then they each on average have 50% chance of being chosen by the buyer for a trade.

prior to a transaction. As soon as either colluding trader clicks a Retract button in market 2, both may resume normal CDA trading, and the retraction is reported in the bulletin board for all to see. Thus the collusion agreement is revocable but is not cheap talk, because DA-Barg enforces the profit sharing if neither trader retracts.

How are the different trading institutions related to core theory? For both DA-Std and DA-Chat, no transfer is possible between same siders, so they correspond to the restricted game where a trader failing to trade receives zero payoff. As noted in section 3.1, the core prediction still applies. DA-Barg allows same side transfers, hence it corresponds to the full game. Regarding collusion, as discussed in section 3.1, in DA-Std and DA-Chat it simply refers to the failure to undercut a same siders price; in DA-Barg, collusion also includes agreeing to (without retracting) same side profit sharing agreements.

4.2 Procedures

We recruited 144 subjects at random from the LEEPS subject pool of over 1000 volunteers, most of them UCSC undergraduates majoring in Economics, Natural Science or Engineering. None had previous experience in our market game. On arrival at the laboratory, subjects received printed instructions, reproduced in Appendix C. They then listened to an oral summary with the user interfaces displayed on a wall screen. This was followed by a quiz and a few practice rounds. Over the next 1-2 hours they played 16 to 24 periods of the market games described above. Most subjects were paid between \$15 and \$25, according to profits earned during the session; average take-home pay was \$21.99, including a \$5 showup fee.

Subjects were randomly rematched each period to avoid repeated game effects. Some sessions had 2 separate marketplaces each with a 2x3 trader configuration each period, and the other sessions had 4 marketplaces each with a 1x2 trader configuration. To maintain anonymity, the messages on screen assigned a label to each participant, e.g. buyer or seller A, and the labels were shuffled every period. The matching scheme guaranteed that the same group of long siders meet at most twice (also, when $n = 2$, that short siders meet a minimal number of times) and that they had no way of knowing when they do meet again.

Consistent with our implementation discussion, subjects were fully informed both of the market parameters and of the other subjects' activities (including communication). The written and public oral instructions stated the number of participants in each market, e.g., 2 buyers and 3 sellers in a 2×3 buyers' market, and stated that all buyers had the same value (always 89 lab dollars) and all sellers had the same cost (55 lab dollars). The screen displays made all activities (bids, asks, transactions, proposals, agreements, chat messages, retractions, and so on) public information within the same marketplace. At the end of every period, each subject was shown the transaction prices in all the parallel marketplaces.

Table 1 summarizes the laboratory sessions. Due to software limitations, DA-Barg is implemented only for 1x2 markets. We have run 7 sessions of 1x2 markets with a total of 84 subjects in 56 markets, each lasting 10 periods (in DA-Barg) or 12 periods (in DA-Chat, without the chatroom for the first two periods), as well as 6 sessions of 2x3 markets with a total of 60 subjects in 24 markets of 8 periods each. The 2x3 markets were run for fewer periods than the 1x2 because the former, involving more traders, were designed to have longer periods. We run two sessions for each treatment unless the data exhibit large variance, in which case we run an extra session.

—Insert Table 1—

In each session we run either two 2x3 or four 1x2 markets simultaneously. The periods in the first half, called segment 1, of each session are all buyers' markets (or all sellers' markets). At the halfway point (that is, after 10 or 12 periods in a 1x2 market or 8 periods in a 2x3 market) we switch the role of half (in 1x2 sessions) or one third (in 2x3 sessions) of the long siders, so the switchers stay on the long side throughout the session. These subjects are given double showup fees; the instructions say that this is "for their role change." Of course, of the other subjects those who are long (short) siders for segment 1 of the session become short (long) siders for segment 2. Pilot sessions convinced us that subjects could adapt to the strategic environment more readily with this design—a single switch from buyers' markets to sellers' markets (or the reverse) midway through the session—than with alternatives such as simultaneous buyers' and sellers' markets with more rapid rotation of subjects from short side to long side.

4.3 Testable Hypotheses

Most market experiments analyze transaction price as well as surplus, but in our experiment, surplus subsumes price since all buyers have the same induced value and all sellers have the same induced cost. We therefore focus on a sufficient statistic for each transaction, Short Sider Surplus Shares (SSS) = the fraction of the surplus obtained by the short sider. The experiment allows us to test the following specific hypotheses.

1. The core predicts that short side traders get a disproportionate share of gains from trade. By contrast, fairness and collusion suggest division closer to an equal split. Normalizing per-transaction surplus ($89 - 55 = 34$ lab dollars) to 1, the equal split benchmark in a $n \times (n+1)$ market is $n/(2n+1)$ since there are n transactions possible, while the unique core prediction is 1.0. The core prediction is supported when the median SSS significantly exceeds the midpoint $\frac{3n+1}{4n+2}$ between equal split and 1.0.

2. The stylized fact that it is harder for a larger number of players to collude implies (a) long siders will collude more successfully in 1x2 than in 2x3 markets, and accordingly (b) SSS will be lower in 1x2 than in 2x3 markets.

3. Although the core prediction applies to all three institutions, we constructed DA-Chat and DA-Barg to provide increasing collusion opportunities relative to DA-Std. Hence we expect the core prediction to fare best (i.e., SSS will be largest) in DA-Std and worst (SSS smallest) in DA-Barg.

4. Given the opportunity, subjects will try to collude, but core theory predicts that such attempts will fail. Therefore we predict that later in the laboratory session (a) collusion rates will decline and (b) SSS will increase.

5 Results

To give the reader a general impression of the data, we begin with time graphs of three typical sessions. We then turn to direct tests of the main hypotheses. The rest of the section presents a series of regressions that provide more definitive results. Supplementary regressions are collected in Appendix A.

—Insert Figure 4—

5.1 Sample Session Graphs

Figure 4 graphically summarizes the SSS in three sessions. Panel A shows a 2x3 DA-Std session in which ten subjects rotated through two simultaneous markets. The first 8 periods had two buyers in each market, and the four buyers got the lion's share of the surplus of every transaction, e.g., 94 to 98% (the transaction price is this percentage times 34 plus the seller's cost) in period 8, always far above the equal-split line. The buyer shares were more dispersed in the first 4 periods (the first half of the segment) but all were at least 75%. In period 9, two subjects switched from sellers to buyers, so in periods 9-16 the sellers were short siders and the graph therefore shows sellers' shares of the surplus. These shares again become more tightly bunched above 90% in the last 4 periods (the "second half"). Only two potential trades were missed during the entire session, indicated by the arrows in periods 5 and 9.

Panel B shows a 1x2 DA-Chat session with twelve subjects rotating through four markets. Sellers were the short side in the first 12 periods, and four subjects were switched from buyers to sellers in the remaining 12 periods. As indicated by "No Chat" on the horizontal axis, chat was disabled in two initial periods of each segment. The markets are numbered according to short sider ID and sorted by profitability. It appears that individual short sider's shares are serially correlated, even though different long siders rotated through all four markets in each segment.

Panel C shows a 1x2 DA-Barg session. In period 11, four sellers became buyers, so sellers are the short siders from then onwards. One sees more equal divisions in this treatment, most of them involving buyers 1 and 2 in the first segment. The chatroom transcript suggests that a few short siders offered concessions out of a sense of fairness,[8] and in general the bargaining style of the individual short sider seems to plays an important role. One sees more (approximately) equal splits in this treatment, but there are also many splits that heavily favor the short side. The willing eye may see an upward trend in the SSS in the "second half" of both the buyers' markets and sellers' markets in this session.

Unless otherwise noted, the data analysis to follow will be restricted to "second half" data. As is common for tests of equilibrium theories, the "first half" of each segment (i.e., the first and third quarters of each session) is set aside because it usually is noisier and seems to reflect more mistakes and explorations. Appendix A verifies that the inferences from the entire data set are often weaker but generally agree with the inferences presented below.

5.2 Direct Tests of Hypotheses

Hypothesis tests are complicated by three kinds of potential correlation across data points—the two trades in the same 2x3 market might be correlated, or trades of the same short sider across periods, or even trades within the same session. The direct hypothesis tests in this subsection provide upper and lower significance bounds by (a) treating all data points as independent or alternatively (b) using the very conservative convention of using only one observation per session. For more refined inferences, we turn to regressions in the next subsection. As it turns out, the results (the significance levels in particular) of the (a) type direct tests are all confirmed by regressions.

Consistent with Hypothesis 1, the SSS in the three sample sessions were generally much closer to 1.0 than to the red line marking equal division. Table 2 describes the SSS over all realized transactions.

[8] E.g., in period 12, one seller wrote, "because all the sellers are selling at such a high price that sellers are getting alot more than buyers, which is kinda unfair", and another short sider in Session 2 wrote "if we all did that we should all get fair money[.] That is all I am trying to do[,] make it fair."

—Insert Table 2—

In each treatment cell, the median SSS exceeds the midpoint $\frac{3n+1}{4n+2}$ between equal split and 1.0, significant by the sign test at the 1% confidence level or better when all data points are taken as independent. Using only one summary observation per session, we note that the median exceeds the midpoint for buyers' markets in 11 of 13 sessions and for sellers' markets in 12 of 13 sessions, and the standard sign test has p-values lower than 0.01 for both buyers' and sellers' markets. We conclude that the data strongly support the core prediction.

Direct tests find support for Hypothesis 2a in DA-Chat sessions. Successful collusion is more than twice as frequent in 1x2 DA-Chat as in 2x3 DA-Chat markets, observed in 6.2% and 2.9% of the markets respectively, although the difference is not significant. However, significant difference is found in the incidence of honored collusion agreements (which did not always result in a trade due to short sider rejections), found in 11.3% of the 1x2 DA-Chat markets and 2.9% of the 2x3 markets, resulting in a one-sided p-value of .0755. We have only minimal evidence for possible tacit collusion in DA-Std sessions, as discussed briefly in Appendix A.2, and it is consistent with Hypothesis 2a. As noted earlier, our software does not permit 2x3 DA-Barg sessions. For Hypothesis 2b, no significant difference is found in SSS, based on Mann-Whitney tests, in either DA-Std or DA-Chat treatment, likely due to the overall low collusion rates.

Hypothesis 3 is supported by the fact that median SSS in DA-Std (0.912 for markets of either size) is considerably larger than in either DA-Chat or DA-Barg (those medians are 0.706 and 0.735 for 1x2 and 2x3 DA-Chat, and 0.735 for DA-Barg). The differences in distribution are significant for either comparison at better than the 0.1% level according to the Mann-Whitney tests. Two-sample median tests give similarly overwhelming results. Regarding the difference between DA-Barg and DA-Chat, DA-Barg sees twice as much successful collusion (in 12.8% of the markets) than 1x2 DA-Chat, and the difference has a one-sided p-value of .067 based on Chi-square test. This difference in collusion did not translate into significantly higher SSS in DA-Barg: median SSS is slightly (but insignificantly) higher in DA-Barg than in DA-Chat.

For Hypothesis 4a, direct tests find that the rate of successful collusion indeed declines significantly (one-sided p=.0465 by the Chi-square test) from the first to the second segment of each session, even though we find no trend within each segment. Consistent with Hypothesis 4b, SSS tends to have higher medians in the "second half" data. As shown in Appendix A, the increase is significant at the 1% level in DA-Std sessions, insignificant in DA-Chat sessions, and significant at the one-sided 10% (p=.056) level for DA-Barg sessions.

5.3 SSS Regressions

We now report regressions of the SSS that impose random session effects to account for the third type of correlation. Appendix A.3 reports results of the most conservative regressions, namely those with random effects for the individual short sider while clustering the standard errors at the session level, and with one observation randomly dropped from each 2x3 market. We shall note below the few cases when those more complex specifications alter the conclusions.

Table 3 reports (random session effects) regressions of SSS in each completed transaction on institutional dummies, supplemented by dummies for whether the observations come from the second segment (Seg2 = 1), for whether there are 3 long siders (D2x3 = 1) or just 2, and for whether the short side is sellers (Smarket=1) or buyers. We focus on regressions 1 and 2. Regressions 3 and 4 are supplementary, and regression 5 will be discussed in the next subsection.

–Insert Table 3–

Regression 1 shows that, compared to DA-Std, including either a chatroom or a bargaining facility significantly reduces the SSS, consistent with Hypothesis 3. The impact is also economically significant: the enhanced collusion opportunities transfer about 15% of profits to the long siders. Confirming the impression from Table 2, the coefficients for DA-Chat and DA-Barg are almost identical. The Seg2 coefficient estimate is significantly positive, consistent with Hypothesis 4, and suggests the transfer of an additional 5% of surplus to short siders in the second half of each session. Perhaps surprisingly in view of Joyce (1984), short side sellers tend to extract about 4% more surplus than buyers, other things equal, again significant at the 5% level.

Regression 2 includes a trend variable that measures the effect of the number of periods that has elapsed from the beginning of the segment. It is shown to be positive and significant even in the second halves, again supporting Hypothesis 4b.

Regression 3 shows that a more symmetrically designed pilot session, session 1, where subjects were switched between long and short sides frequently, indeed exhibits significantly higher SSS; in fact there was not a single successful collusion in this session. Regression 4 encompasses all periods and as expected there is more noise and subjects' learning is discernible from comparing it with the second-half-only regression 2.

Table 3 presents only the direct effects. Appendix A reports regressions including all second order interaction terms. They reveal some nuances—for example, that the buyer-seller asymmetry tends to vanish with experience—but don't alter the basic conclusions.

5.4 Collusion

For DA-Chat and DA-Barg sessions, we had two research assistants go through the chatroom and collusion market logs and independently classify, for each market of each period, whether collusion was proposed, whether fairness was discussed, whether there was agreement on a proposal, whether it was bilateral or trilateral, etc. Collusion was deemed successful when there was a transaction and the price conformed to the agreement. See Appendix B for details. Not surprisingly, no collusion attempt was found on the short side, and all the results below pertain to the long side. Appendix A.2 reports the evidence (quite minimal) on tacit collusion.

—Insert Table 4—

Table 4 gives the summary of the incidence of collusion and fairness discussion. The overall rate of successful collusion is low, observed in only 8.7% of the markets. The table also shows the underlying conditional probabilities. Slightly more than half of the time not a single long sider proposes collusion. When a proposal is made, the long siders may disagree on the collusion price or the profit sharing percentage; more than one third of the time, colluders are unable to reach agreement. When they do, nearly two thirds of the time the agreement fails.[9] Of the honored agreements, nearly one sixth were rejected by the short siders and resulted in no trade. Indeed, chatroom conversation shows that many subjects anticipate the problems: "that [collusion] never works", "someone will always crack and the other two will scramble for what is left", "this is gross[,] watching people sell each other out"; and one subject in DA-Barg wrote in post-experiment questionnaire that what was on his mind during the experiment was "how not to be screwed by others." This helps to explain the low proposal rate, which is remarkable given the ease of making a proposal, especially with our DA-Barg facilities.

Turning back to Table 3, regression 5 shows that all degrees of collusion are highly significantly associated with lower SSS. In particular even entirely unsuccessful attempts at collusion, namely collusion proposals that did not result in an agreement (as captured by the regressor "collusion proposal only") and collusion agreements that were cheated upon are associated with significantly lower SSS. Averaged over all successful collusions in DA-Chat and DA-Barg sessions, the SSS is .46, and 95% of the time it is between .35 and .65. One may also notice that fairness discussion alone reduces SSS by 3.3% (significant at one-sided 5% level), an order of magnitude less than the impact of having an honored collusion agreement (the sum of the coefficients on collusion agreement and honored agreement) and about half the impact of having only a collusion proposal; moreover, this estimate becomes highly insignificant after intra-short-sider correlations are controlled for, as reported in regression 5 of Table A4 in Appendix A.3.

—Insert Table 5—

Table 5 reports logit regressions that further dissect the collusion process. It includes random session effects to control for the fact that long siders encourage one another to collude[10]

[9]Probably the conditional failure rate (or rate of cheating) would be even higher if long siders were required to propose collusion. Chat logs confirm that many subjects refuse to entertain collusion proposals because they anticipate cheating.

[10]The chat log shows that, for instance, one long sider in session 12 advocated collusion with every fellow long sider he met and another long sider in the same session even asked other long siders to "spread the

and are rematched every period. Consistent with Hypothesis 3, there are more successful collusions in DA-Barg than in DA-Chat and the relevant coefficient in regression 1 is significant at one-sided 10% level. Notice that Seg2 has a negative impact on log odds of $-.726$, translating into more than 50% decrease in collusion rate, and this is statistically significant at one-sided 10% level, supporting Hypothesis 4a. Again, see Appendix A for a finer-grained comparison.

An important harbinger of success is that the chat mentions fairness. The gross counts noted in the penultimate row of Table 4 are reinforced by regression 2 of Table 5. It reports a highly significant fairness coefficient of 2.5, meaning that the odds of success more than quadruples. Regression 5 of Table 5 supports Hypothesis 2a by showing that D2x3 reduces the log odds of an honored collusion agreement by 1.54, with a one-sided p-value of .079.

The theory of the core implicitly assumes that a trade opportunity is always realized. On the other hand, it is well known in the lab that even with complete information some inefficiency is inevitable. Regressions 3 and 4 of Table 5 examine the incidence of missed trades, treating each potential trade as a unit of observation.[11] Recall from Table 2 that the efficiency levels are fairly high in each treatment; overall, 95.1% of potential trades are realized. Regression 4 indicates that in DA-Chat, honored collusion agreements are the main reason for missed trades: the coefficient estimate is 2.40, with a p-value of .011 and, after controlling for the various collusion and fairness effects, DA-Chat no longer produces significantly more missed trades than DA-Std. On the other hand, as suggested by the highly negative and highly insignificant interaction term Barg*CollusionHonored in regression 4, collusion does not contribute to missed trades in DA-Barg, and in fact (as shown in Table 4) honored collusion agreements in DA-Barg always succeeded. After controlling for the various collusion and fairness effects DA-Barg still produces several times more missed trades than DA-Std, as captured by the coefficient estimate 1.67 on Barg, which is significant at 5%.

Another piece of evidence might shed some light on the short sider's role in unsuccessful

word."

[11]The collusion dummies accordingly take values for each potential trade rather than for the market as a whole, although the difference exists only in a couple of observations in 2x3 markets.

collusion and missed trades. In DA-Chat sessions for which we have transaction time data, the mean transaction time (54% of the entire trading period) of the successful collusion is no greater than that of the overall average transaction time (55% of the entire trading period). This suggests that the short sider in a successful collusion does not generally try to hold out for the collapse of the collusion.

In Appendix A.2 we take full advantage of the data and study, for instance, the interesting question of what affect collusion success conditional upon the existence of a collusion agreement, and we further discuss DA-Barg v.s. DA-Chat and 2x3 DA-Chat v.s. 1x2 DA-Chat.

As alluded to earlier, at the end of each session we gave the subjects a questionnaire (not included in the paper) asking a few questions regarding their experiences, including whether there was anything unclear in the instructions, what went through their minds during the experiment, whether the trading period was too long or too short, and how the experiment might be improved. While many subjects shared their thoughts during the experiment, and a few wrote "too long" about the period length and some would like the design to be "improved" so that they might be on the short side more often, no subject complained of unclear instructions. Therefore we are fairly satisfied that the general lack of collusion was not due to a lack of understanding of the collusion opportunities.

5.5 Trend Extrapolation

Another perspective on the core prediction comes from fitted values and trends. We focus on the periods in segment 2 of each session, since they reflect behavior of subjects familiar with the general environment but facing a new situation (e.g., sellers' markets after previously seeing buyers' markets.) The fitted values come from regressions reported in Table A1 in Appendix A.

Tests based on regression 1 show that the buyer-seller asymmetry becomes insignificant in segment 2 except in DA-Barg markets. We therefore pool 1x2 markets with 2x3 markets and sellers' markets with buyers' markets in regression 2 to generate the summary fitted values and trends for the three institutions. The fitted values indicate that short siders obtain over

92% of the surplus in the last period of the DA-Std treatments, and that they are on track to obtain 100% after another 3 periods. Similarly in the DA-Barg treatment, the last period fitted SSS is 83% and they are on track to obtain the rest in 9 periods.[12] DA-Chat is the exception: the fitted value is just over 75% in the last period, and there is no significant trend, even though the SSS improves significantly from segment 1 to segment 2 as noted in Appendix A.

5.6 Summary

Overall, our laboratory experiment strongly supports the core prediction. In every treatment, short siders get the lion's share of the gains from trade. In two of the three market institutions (DA-Std and DA-Barg), their share seems to be headed towards 100%. Moreover, successful collusion is rare, even in a trading institution, DA-Barg, designed to facilitate collusion.

The presence of fairness discussion appears to impact SSS only through collusion— long siders attempting to collude are more likely to succeed when there is also fairness discussion, and but in SSS regressions the coefficient estimate for fairness discussion loses significance after intra-short-sider correlations are controlled for, and it is about an order of magnitude less than the impact on SSS from an honored collusion agreement.

We detect a tendency of sellers to retain more surplus than buyers, but the asymmetry fades with experience in segment 2, a finding potentially useful to other experiments where such asymmetry matters.

While theory remains silent on efficiency, we find that neither DA-Chat nor DA-Barg is as efficient as the standard continuous double auction: more mutually beneficial trades are missed in these new institutions even though the subjects had several times more time to interact each period. This is true for DA-Barg even after all the collusion and fairness factors are controlled for, and this finding perhaps reflects the inherent efficiency difference between

[12]The trend is significantly different from zero for DA-Std at 5% level, and the trend for DA-Barg while only borderline significant (p=.106) based on the pooled regression 1 is shown to be significant at 5% or 1% in the finer-grained regressions in Tables A1 and A5.

bargaining and market trading.

6 Conclusion

What do our results tell us about the conventional critique of the core? In our lab experiment, the extreme sensitivity to one extra player turns out to be quite realistic after all. Indeed the example is a credit to the core as it successfully predicts a counter-intuitive outcome.

Due to the inherent connection between the core and the competitive equilibrium, our results also contribute to the literature on the boundaries of CE. In particular they demonstrate robustness of CE in very thin markets with extreme supply-demand configurations and unusual collusion opportunities.

Our study opens the way to future experimental work. For example one could use our environment to examine other market institutions such as the call market, in which bids and asks are submitted simultaneously and then a market-clearing price is generated to determine all the transactions. Generally the call market converges less quickly and is somewhat less efficient than DA-Std, but it is not clear how it would fare in our environment. Another possibility for further study is to try reducing or eliminating the sense of unfairness in the core allocation, for example by taxing short siders' profits. Finally, it surely would be interesting to extend the so-far quite limited experimental literature on noncooperative foundations of the core.

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Appendix A: Supplementary Regressions and Tests

A.1 More on SSS

Table A1 shows regressions of SSS that include one or more of the following: i) some or all the second order interaction terms; ii) all periods; iii) trend and trend-related interaction terms that are useful in fine-grained extrapolation.

—Insert Table A1—

Tests based on regression 1 of Table A1, which include all the second order interaction terms, show that the buyer-seller asymmetry in SSS diminishes and becomes insignificant in segment 2 in all but one setting, the DA-Barg markets.[13] These tests are not included and are available upon request.

The following result provides more nuanced support for Hypothesis 4: the overall significant direct effect of segment 2 as reported in regression 1 of Table 3, when broken down into the three institutional treatments in regression 6 of Table A1, is shown to come from the more collusion-friendly DA-Barg and DA-Chat: the effect is insignificant for DA-Std, but .124 in magnitude for DA-Barg with a two-sided p-value of .000 and .046 for DA-Chat with the p-value being .02.[14]

Regressions 5 and 6 of Table A1 use all periods data. Regression 5 has trend variables and is comparable to the second half regression, regression 4, and regression 6 has no trend variables and is comparable to the second halves regression, regression 1. One can see that the all periods estimates are in general consistent with those based on the second halves, in the sense that there is not any coefficient whose all periods estimate has the opposite sign of that of the second halves estimate and both estimates are significant. Furthermore, for those functions of coefficients of interest to us (e.g. the sum of the coefficients of Seg2 and Bgn*S2, measuring the effect of Segment 2 in DA-Barg), the related tests yield consistent inferences,

[13]Finer-grained regressions including all the higher order interaction terms show that the 2x3 DA-Chat markets instead of the DA-Barg markets are where the asymmetry remains significant. However, the trend extrapolation using these regressions leads to the same conclusions as reported in the text.

[14]This .02 p-value increases to .079 after randomly dropping one observation from each 2x3 market, and all other results in this statement are robust.

with the only notable exception of the trend related estimates as one would expect from the learning patterns shown in the session graphs.

Table A2 shows the medians of the first halves and substantiates the claim in the text about the significant increases in SSS from the first to the second halves.

—Insert Table A2—

Finally, in DA-Chat we begin with two standard CDA periods, and we find no evidence of any anchoring effects. In particular the two-sample Mann-Whitney test comparing the first two periods' SSS of a DA-Chat session (namely our session 1, the exclusion of which session for an unrelated reason is explained in the paper) where the chat facility start from period 1 with periods 3 and 4's SSS of the delayed-chat sessions show one-sided p-value far from significant (p=.456). And the medians are very close, .75 and .735 respectively.

A.2 Collusion, Fairness and Missed Trades

We have run probit regressions as robustness checks for the logit regressions results reported in the paper, and the latter all prove robust.

Table A3 below shows logit regressions on the incidence of successful collusion i) in DA-Chat with only those observations where the long siders have reached a collusion agreement, and ii) in both DA-Barg and DA-Chat.

—Insert Table A3—

What affects the success of a collusion agreement? Our data allows us to probe into collusion success rate when there is a collusion agreement. For DA-Chat sessions, regression 2 in Table A3 reveals that the planned collusion profit, defined as the difference between the agreement price and the cost (or value) of the long side sellers (or buyers), affects success significantly negatively at 5% level. This means that greedy prices are less likely to succeed, both because there is greater temptation to cheat just as one would expect from game theoretical reasoning, and because the short sider is less likely to agree. For the reader's information, 72 is the modal collusion price, but it happened only 26.3 percent of time and there is a wide spread of the collusion price in general.[15] For DA-Barg sessions, regressions (not reported here but available upon request) reveal no significant role played by the collusion profit sharing percentage, apparently due to the fact that it is very narrowly concentrated–it is 87 percent concentrated between 40% to 50%, with 50% taking up two thirds of all agreements.

We have also done more careful comparisons of the conditional probabilities in Table 4, finding consistent results using both random session effects regressions and non-parametric tests, and here we report only the later to save space. The last row of Table 4 shows that the successful collusion rate in DA-Barg is twice as high as in 1x2 DA-Chat, which in turn has twice as high a collusion rate as that in 2x3 DA-Chat. The first of these differences is shown to be statistically significant at one-sided 10% level in regression 1 of Table 5. The second, however, is not, and this is easily explained if we trace the probabilities leading to collusion success: while there are even greater (and highly significant, with one-sided $p < .01$ by Fisher's exact tests) differences between the (both conditional and unconditional) rates of honored collusion, and in fact there is only one honored collusion in all the 2x3 markets, this one honored collusion was accommodated by the short siders, whereas nearly half of the honored collusions in 1x2 markets were rejected by the short siders.

We can also trace the probabilities leading to the difference in collusion success between DA-Barg and 1x2 DA-Chat. We see that they have similar unconditional rates of collusion agreements, but half of the collusion agreements in DA-Barg are honored, compared to one third approximately in 1x2 DA-Chat, and this difference is shown to have a two-sided p-value of .075 by Fisher's exact test. This has several explanations: the chat window is open throughout a trading period in DA-Barg, so a cheating subject in DA-Barg may suffer some disutility from seeing criticism in the chat window from the collusion partner; there is also some possible framing effect of our DA-Barg facility, which gives a collusion agreement a more formal aura. Continuing tracing the probabilities we see that an honored agreement always results in a trade in DA-Barg whereas nearly half of the honored agreements result in no trade in 1x2 DA-Chat. One explanation is probably that in the DA-Chat setting, without

[15] There is a specific price most of the time while on rare occasions the long siders simply agreed to "hold the same price".

the framing of collusion as an exchange of trading rights in the bargaining setting, the short sider is less willing to accept the bid or ask coming from a collusion agreement. We also see that fairness discussion appears twice as often in 1x2 DA-Chat than in 1x2 DA-Barg, and this difference is significant at 1% level by Fisher's exact test. It is plausible to conjecture that the presence of the collusion facility in DA-Barg alleviates the disadvantage felt by the long siders.

And corresponding to the vanishing of buyer-seller asymmetry in SSS in segment 2, we find that the asymmetry in the rate of successful collusion as reflected in regression 1 of Table 5 vanishes as well in every specification based on Fisher's exact tests (available upon request).

How about tacit collusion? Although we find a couple of clearcut cases of tacit collusion in 1x2 DA-Std markets[16], tacit collusion is hard to define in general. The following incidence of relatively low SSS in DA-Std may give a rough idea: in the second halves 8.5 percent of all DA-Std markets see SSS below three quarters, 3.4 percent below two thirds, and none below one half. These percentages for 1x2 DA-Std markets are all slightly and insignificantly higher than those for 2x3 DA-Std markets, consistent with Hypothesis 2a. For reference, the average SSS after a successful collusion in a DA-Chat or DA-Barg session is .44. It is easy to see that tacit collusion was not nearly as profitable as explicit collusion.

Appendix A.3: SSS Regressions with Random Short Sider Effects

As it turns out, randomly dropping one observation from each 2x3 market does not change the sign or significance level of any estimate we note except a secondary issue pointed out in footnote 14. Below we only report random short sider effects regressions while clustering the standard errors at the session level. Compared to regression results with random session effects, we see a general increase in the proportion of the variance explained by random effects, but little changes in the values of interest to us, with the few exceptions noted below. For example, the trend in DA-Barg that we use for extrapolation is shown to be highly significant (p=.000) and none of the fitted values changes by more than one percentage point. Below we list these regressions in Tables A4 and A5 as the counterparts to Tables 3

[16]There were two cases where the short sider received less than 2 thirds of the surplus and the transaction was made in the last ten seconds.

and A1.

—Insert Table A4 —

We now see that the pilot session, session 1, is highly significantly different—with a p-value of .000—from the rest of the communication sessions. On the other hand, regression 5 shows that with the collusion factors controlled for fairness discussion does not significantly reduce SSS.

—Insert Table A5 —

All the conclusions based on the session effects regressions in Table A1 are still valid.

Appendix B: Coding Collusion and Fairness Discussion

As mentioned before, we had two research assistants independently code the records of the chatrooms and the collusion markets of DA-Barg. They were highly consistent in their coding. Cohen's (1960) Kappa, the coefficient measuring inter-rater agreement, exceeded .88 for all variables, with 1 being perfect agreement. For our analysis we chose either's classification with equal probability when they coded differently.

Below we describe in greater details how they classify various degrees of collusion and the incidence of fairness discussion.

Fairness discussion is coded as 1 if subjects did any of the following or otherwise made comments obviously related to fairness: i) advocating equal split, including "how about 72?" without any discussion; ii) arguing that any outcome of the market must be fair; iii) making simple comment such as "this (experiment) is so unfair!" iv) arguing that current long siders also had chances of being on the short side.

The following are the main collusion dummies and we explain when they take value 1:

1. "collusion proposal only"

Only collusion proposals were sent and no collusion agreement was reached (e.g. due to disagreement about collusion price, or because only one person proposed and the other person did not respond). There are a few special cases worth mentioning. One is that subjects sometimes proposed that they hold the same price until the last 10 seconds. We did not consider this as a collusion proposal since price competition would still be possible in that 10 seconds. Another rare case is that two long siders first agree to hold the same price without specifying the price, and then one says how about 72, the other says how about 70, and chatroom ends. Such cases are coded as absence of an agreement.

2. "collusion agreement" and "nonspecific collusion price"

We code 1 if there is either a bilateral or a trilateral agreement. An agreement is considered reached in DA-Chat if they agree on a specific collusion price or price range, or leave it unspecified (e.g. when they just say "let's hold the same price") and without obvious disagreement over the price, in which case we let the dummy "nonspecific collusion price" take value 1. In particular the following, rare dialogue is considered an agreement without a specific collusion price: "Let's hold the same price." "Ok[.]" "How about 72?" and then the chatroom ends. It is natural to feel some arbitrariness about the coding of this example when comparing it to a similar example above, which we interpret as no agreement due to its ending on a counter proposal of "How about 70?". But such cases are so rare that coding them one way or the other does not affect our general results.

3. "honored agreement"

There are two cases, the bilateral and the trilateral agreements. Consider bilateral agreements first. For DA-Chat, when there is a specific collusion price or price range, we consider the agreement honored if nobody posts a price less favorable to the long siders than the agreed price or range. When the subjects only say "let's hold the same price", we consider it a failure if there is any price competition during the trading stage. For DA-Barg, we code 1 if the last collusion agreement of a marketplace is not retracted by either long sider.

In the case of a trilateral agreement, it turns out that in our sessions it was honored by either all three or none.

4. "successful collusion"

We code 1 for successful collusion if an honored agreement leads to at least one transaction. There was only one trilaterally-abided-by agreement in our sessions, and it led to two transactions.

Appendix C: Sample Instructions and Quiz of a DA-Barg Session

GENERAL INSTRUCTIONS

Please read the following instructions carefully. When you are done, you may ask questions. There will also be some practice periods to give you a better idea about how the game works. It is important that you understand how to play the game, so do not hesitate to ask questions after reading the instructions or during the practice periods.

In this experiment you will be interacting anonymously with other subjects as buyers and sellers in a market. You will also be able to "talk" to other traders in your market in a computerized chat room. A trader, say a seller, can also delegate her trading right to another trader on the same side of the market, a seller in this case, for a negotiated percentage of the profits earned by the second seller. Either seller may decide to terminate such a delegation contract at any time.

After you read the instructions, there will be 3 practice runs , then the real game begins. There will be 4 separate markets running simultaneously. For the first half of the session each market has 1 seller and 2 buyers, and for the second half each market has 1 buyer and 2 sellers. Each period you will be assigned to one of the 4 markets. Assignments switch each period so you will face a different set of buyers and sellers each time.

Each period lasts 3 minutes and the remaining time for each period is shown on the top right of you screen. Between periods there is a short interval for review, when you will see the profits earned in all the markets for that period.

Each period each seller and each buyer may sell or buy one and only one unit of a fictitious commodity. The commodity is worth 89 lab dollars to a buyer and it costs 55 lab dollars to a seller. The prices at which you buy or sell in double auction will determine your earnings. The buyer's profit is equal to 89 minus transaction price, and the seller's profit is transaction price minus 55. Buyers and sellers who don't trade earn a profit of 0 that period.

You can keep track of your earnings by jogging down your profit every period. Your total earnings over all periods will be converted into U.S. dollars (1 lab dollar = 0.06 real dollar) and paid to you, privately, in cash at the end of today's session. These earnings are in addition to a $5 show-up fee.

We want to emphasize that your responses throughout today's session will be completely anonymous to other subjects and will be kept confidential, and in fact we will not keep your names on our data.

The following instructions explain trading and earnings for *both* sellers *and* buyers. However, today only some of you will play both roles and the rest of you will be ***either*** a buyer ***or*** a seller for the entire session. Those playing both roles will be a seller in the first half and a buyer in the second half, and to prepare for it they will have a role change halfway through the practice periods. They will also get a double show-up fee at the end of the experiment.

You will be assigned a public ID, or “nickname” that will help remind you of your own activities during each period. A buyer (seller) will be called (unimaginatively) “buyer (seller)” as her public ID if she is the only buyer (seller) in the market, and “buyer A (seller A)” or “buyer B (seller B)” if there are two buyers (sellers). Whether someone is called “buyer (seller) A or B” is completely random and changes from period to period, and it does not affect her performance in any way.

When the experiment begins, your computer screen will tell you which role you play, how many buyers and sellers there are in your market, and the public ID/nickname you are given for each period.

Please use your real name to log in. We will use it to pay you, and we will keep it confidential.

Below we detail the 3 components of this experiment: market 1, for the commodity trade, market 2, for the delegation negotiation, and the chat room, where all players may post messages. You can switch between Market 1 and Market 2 by clicking on the “Market 1” and “Market 2” tabs. The chat room is located in the same window as Market 2. So if you are in Market 1, you need to switch to Market 2 to see or post messages in the chat room. There is a message box located in Market 1 that alerts everybody when there is any activity in either market: e.g. when there is a message in the chat room, a delegation proposal or agreement between two traders, a retraction of the delegation, a price posted by a trader in Market 1, and so on.

Market 1: commodity trade

INSTRUCTIONS FOR SELLERS

How do sellers make money?

Sellers earn money by selling units at prices that are above the cost.
The profit on each unit sold is its transaction price minus the cost. For example since each unit of the fictitious commodity costs 55, if the price Seller A sold it for was 75, her profit was 75 – 55 = 20 if she was the only seller or if she did not have delegation from the other seller, and her profit would be 20 times the agreed-upon percentage if she did have the delegation.

One very important point: **a seller does not incur the cost for a unit until it is sold**. Thus, the profit is zero for a seller who has not sold his unit.

Sellers' Screen (see screenshots at the end)

Who you are
The top of your screen shows your private login name (Alessandra for example), your public ID/nickname, which also indicates your role, a buyer or a seller.

the Market Panel
This panel contains a box for buyers' **Bids**, and a box for sellers' **Asks.** You cannot see the other traders' nicknames, which are replaced with "??". You can see your own nickname next to your ask, as a reminder of your ask price. The **Transactions** box, located on the upper right corner, shows the transaction(s) taking place in your market.

Choose-Your-Price Panel

A seller can choose an asking price by typing it in the blank space next to "price", and then clicking the **Ask** button below. You are allowed to ask the same price as asked by the other seller. For example, after seller A asks 59, seller B may also ask 59. Each period a seller may sell only one unit.

Profit Information box
This box is in the right half of the screen, and it shows the (gross) profit (i.e. price – 55) earned on the one unit sold in the current period. This profit is gross when you had delegation from the other seller because you need to pay, out of this money, the agreed percentage to the other seller.

Trading rules

At any time during the period, each seller (who has not already sold one unit or delegated her trading right) may submit an **ask** to sell a unit at a specified price. To replace an existing ask with a new one the seller needs to retract her existing ask first. To make an ask, the seller types in the price and clicks the button **Ask**. For instance, if the seller wants to accept the best bid, say

73, she can type 73 and click **Ask** , and then transaction will take place at price 73. Everyone in the market will see her bid (but not her nickname). To retract her ask, she only needs to highlight her ask in the transaction box and hit the **Retract** button.

INSTRUCTIONS FOR BUYERS

How do buyers make money?

Buyers earn money by buying units at prices that are below 89.
The experimenter will redeem each unit purchased at 89 lab dollars. For example, if the price the buyer paid was 75, the profit on the purchased unit was 89 - 75 = 14. This would be the buyer's profit if she was the only buyer or if she did not have delegation from the other buyer, and her profit would be 14 times the agreed-upon percentage if she did have delegation.

One very important point: a buyer does not receive the redemption value for a unit until the unit is bought. Thus, the profit is zero for a buyer with no purchase. Second, you earn negative profits (i.e., lose money) whenever you buy a unit above its value.

Buyers' Screen (see screenshots at the end)

Who you are

The top of your screen shows your private login name (Tai for example), your public ID/nickname, which also indicates your role, a buyer or a seller.

the Market Panel
This panel contains a box for buyers' **Bids**, and a box for sellers' **Asks.** You cannot see the other traders' nicknames, which are replaced with "??". You can see your own nickname next to your bid, as a reminder of your bid price. The **Transactions** box, located on the upper right corner, shows the transaction(s) taking place in your market.

Choose-Your-Price Panel

A buyer can choose a bidding price by typing it in the blank space next to "price", and then clicking the **Bid** button below. You are allowed to choose the same bid as submitted by the other buyer. For example, after buyer A bids 59, buyer B may also bid 59. Each period a buyer may buy only one unit.

Profit Information box

This box is in the right half of the screen, and it shows the (gross) profit (i.e. 89- price) earned on the one unit sold in the current period. This profit is gross when you had delegation from the other buyer because you need to pay, out of this money, the agreed percentage to the other buyer.

Trading rules

At any time during the period, each buyer (who has not already bought one unit or delegated her trading right) may make a **bid** to buy a unit at a specified price. To replace an existing bid with a new bid the buyer needs to retract her existing bid first. To make a bid, the buyer types in the price and clicks the button **Bid**. For instance, if the buyer wants to accept the best ask price, say 73, she can type 73 and click **Bid**, and then transaction will take place at price 73. Everyone in the market will see his bid (but not his nickname). To retract her bid, she only needs to highlight her bid in the transaction box and hit the **Retract** button.

Market 2: trading rights delegation

In this market two traders on the same side of the market may negotiate to have one trader delegate his trading right to the other. The delegating party will suspend his trading activities in market 1, and in return she will get the negotiated percentage of the profit when the delegated party makes a trade with the other side of the market. Either party may terminate the delegation unilaterally at any time before a trade takes place. Once a trade takes place, however, both the trade and the delegation agreement become irreversible.

For example, suppose there are two sellers and one buyer. If seller 1 wants to delegate his trading right to seller 2, she can type in the percentage she wants for himself, say 55%, then hit the "Send" button. This proposal will then be subjected to seller 2's approval, showing up on seller 2's screen as "I (seller 2) get 45% and you get 55%". After seller 2 hits "Accept", a confirmation message will appear in the chat room window as well as in the message box in Market 1, and at the same time the delegator will have all his market 1 buttons frozen. There will be a "Retract" button when there is an outstanding delegation agreement, and hitting the button will result in the termination of the agreement, at which point the delegator may resume his activities in Market 1. The buyer can view all these activities either directly by opening his Market 2 window or by reading the reports from the message box in Market 1.

Profit distribution when there is a trade is shown in the right half of the window. The delegator gets the agreed percentage of the trade profit and this amount shows up as a negative figure on the delegatee's screen because it will be subtracted from the trade profit when the computer calculates the delegatee's profit for that period.

Chat Room Instructions

It is similar to typical internet chat rooms (see screenshots at the end). You can "talk" with the other two traders in your market by typing messages and sending them, and your messages will be seen instantaneously by all traders in your market and they will also see your public ID/nickname.

You may not write any messages in an attempt to identify yourself or other subjects. Doing so will be grounds for dismissal and exclusion from this and future LEEPS experiments. With this only exception, you may say anything related to playing the game, and you may say nothing. Your earnings will not be affected by what you say or don't say in the chat room.

Frequently Asked Questions:

1. Is it true that if everybody acts fast and transacts early then you can all go home early?

Answer: No. Each period lasts 3 minutes regardless of how fast you transact.

2. Can you correct mistakenly typed bids and asks after they are already accepted by other players?

Answer: No. All trades are final.

Please take the following quiz to test your understanding of the game rules. We will announce the correct answers shortly.

Quiz

1. Your computer screen shows that you are a seller. You submit an ask of 55, and a buyer accepts it,

a. how much profit do you make?

b. Do the same exercise
i) if your ask is 77;
ii) if you are a buyer and you have bid 89 and 68.

2. You find yourself to be buyer A again after being buyer A in the last period. Can you assume that the buyer B this period is also the same person as in the last period?

You can still ask questions during the practice periods. But PLEASE KEEP QUIET DURING THE REAL GAME!

Thank you in advance for your cooperation.

Are there any questions?

Figure 1: Demand, Supply and Competitive Equilibrium (CE) in a 2 x 3 Buyers' Market

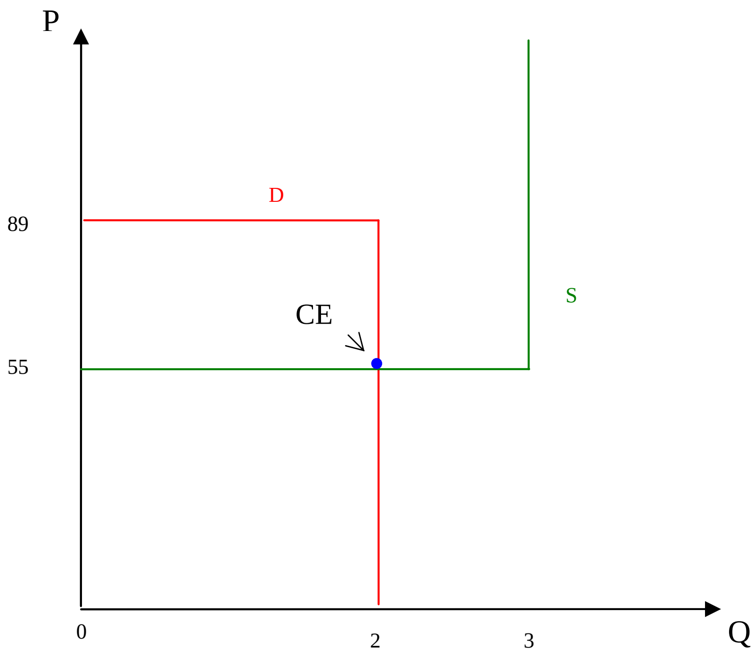
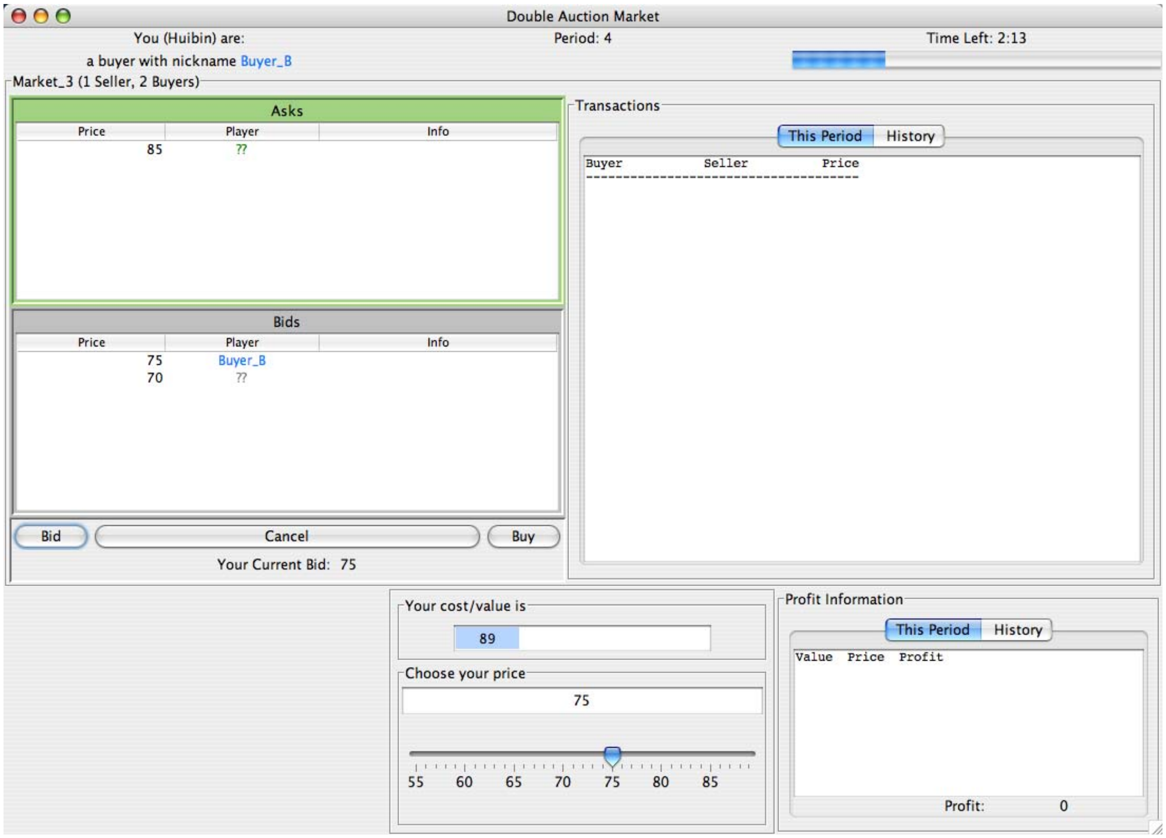
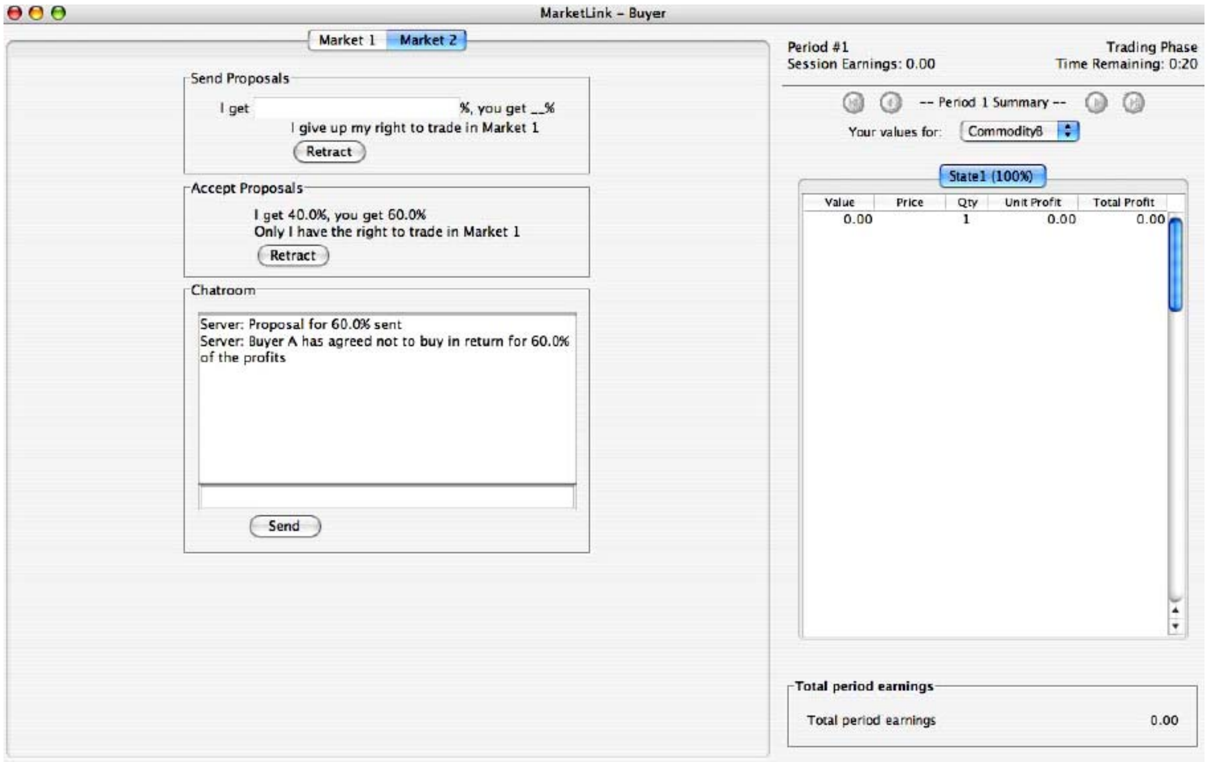


Figure 2: DA-Std User Interface



DA-Std trading screen is shown for a buyer (ID code Buyer_B, actual name Huibin). She can post a bid by either typing the desired price or by dragging the slider to the desired value in the "Choose your price" box and then clicking the Bid button. The value of 89 is shown nearby against a blue shaded background. The large box at upper left, labeled "Market 3 (1 Seller, 2 Buyers)," shows all current bids by both buyers and asks by the seller. The buyer transacts by either clicking on an ask or by waiting for the seller to click on her bid. The "Transactions" box shows the current period transactions in her local market; by clicking on the History tab the previous period's transactions are shown. The "Profit Information" box at the bottom right of the screen shows Buyer_B's previous transactions and profit. The History tab details trading profits and transactions in earlier periods. Sellers' trading screens are similar, with Ask and Sell buttons in the center box, costs instead of values, etc.

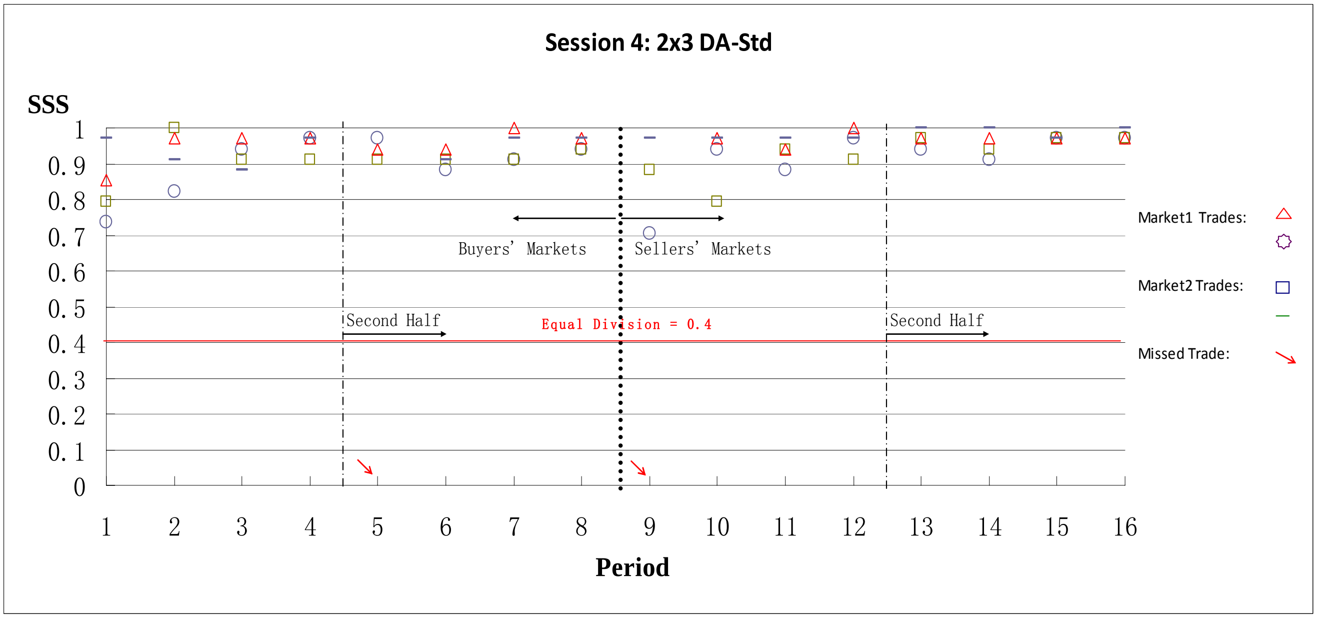
Figure 3: DA-Barg User Interface



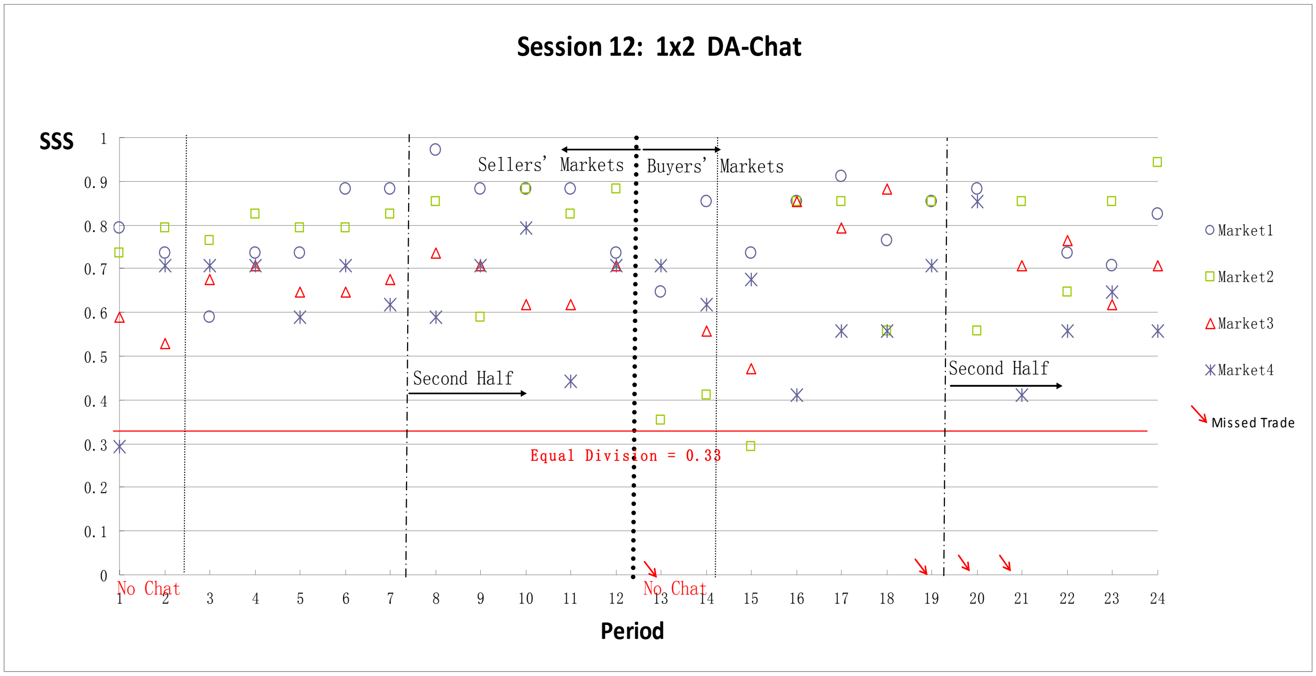
DA-Barg trading screen, tabbed to Market 2, is shown for Buyer_B. The box at upper left allows the buyer to propose (and to retract) an offer to award sole negotiation rights to the other buyer in exchange for a specified share of the profit. Just below is the “Accept Proposals” box, displaying an offer from the other buyer to give up his rights to buy in exchange for 60% of the profit; Buyer_B has accepted that offer but can retract by clicking the button. The Chatroom box below contains automatically generated messages from the server as well as messages sent by the traders (none at present). Earnings are shown on the right side of the screen.

Figure 4: Short Sider Surplus Shares (SSS) in Three Sessions

Panel A



Panel B



Session 12: 1x2 DA-Chat
SSS
Sellers' Markets
Buyers' Markets
Second Half
Second Half
Equal Division = 0.33
No Chat
No Chat
Period
Market1
Market2
Market3
Market4
Missed Trade

Panel C

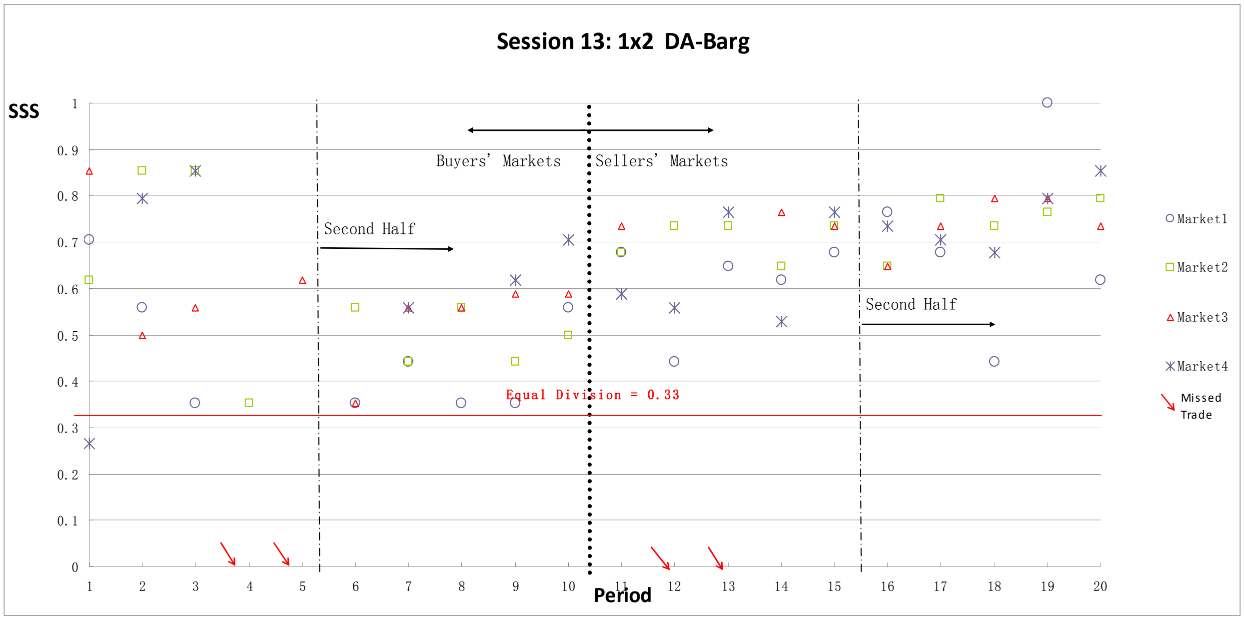

Session 13: 1x2 DA-Barg
SSS
Buyers' Markets
Sellers' Markets
Second Half
Second Half
Equal Division = 0.33
Period
Market1
Market2
Market3
Market4
Missed Trade
0
0.1
0.2
0.3
0.4
0.5
0.6
0.7
0.8
0.9
1
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
16
17
18
19
20

Table 1: Sessions

	DA-Std	**DA-Chat**	**DA-Barg**
1x2: 4 markets per session	2 sessions: B10S10, S10B10	2 sessions: B(2+10)S(2+10), S(2+10)B(2+10)	3 sessions: B10S10, B10S10, S10B10
2x3: 2 markets per session	3 sessions: B8S8, B8S8, S8B8	3 sessions: B(2+6)S(2+6), B(2+6)S(2+6), S(2+6)B(2+6)	No sessions due to programming limitation

Note: Notation such as B8S8 indicates that the session included 8 buyers' market periods followed by 8 sellers' market periods, and B(2+10) indicates that 2 periods of ordinary CDA preceded 10 augmented periods (with the Chat facility).

Table 2: **Median SSS and Efficiency Levels**

		DA-Std	**DA-Chat**	**DA-Barg**
1x2 **midpoint= .670**	Median (one-sided sign test p-value)	.912 (.000)	.706 (0.004)	.735(.001)
	Efficiency	97.5%	92.5%	93.3%
2x3 **midpoint= .700**	Median (one-sided sign test p-value)	.912 (.000)	.735 (.000)	
	Efficiency	99.0%	93.1%	

Table 3: SSS Regressions (Random Session Effects GLS)

Specification	1	2	3	4	5
Constant	.827***±.047	.757***±.062	.831***±.047	.702***±.049	.847***±.026
Chat	-.155***±.050	-.164***±.052	-.155***±.049	-.112**±.051	-.105***±.028
Barg	-.152**±.064	-.149**±.067	-.152**±.064	-.100±.066	-.087**±.036
Seg2	.054***±.012	.054***±.012	.056***±.012	.025**±.010	.044***±.011
Smarket	.046***±.012	.046***±.012	.035***±.012	.024**±.010	.017±.011
D2x3	.017±.050	.035±.054	.017±.050	.029±.052	.015±.028
Trend		.008*±.005		.015***±.002	
Session1			.158*±.092		
Collusion proposal only[1]					-.063***±.019
Collusion agreement[2]					-.073***±.019
Honored agreement[3]					-.203***±.028
Fairness discussion[4]					-.033*±.018
Data Range	Second Half	Second Half	Second Half (including session 1)	All Periods	Second Half
R-sq (overall):	.309	.314	.299	.163	.490
Rho	.286	.306	.284	.244	.119
Number of Observations	422	422	446	846	422

Note: The treatment dummies (Chat, Barg, D2x3, Seg2 and Smarket) are defined in the text. Trend is the number of periods that have lapsed from the beginning of each segment. Precise definitions of the collusion variables in regression 4 can be found in the Appendix. ***, ** and * indicate significance at 1%, 5% and 10% levels respectively and the numbers before and after ± are the coefficient estimate and its standard error respectively. Notes 1, 2, 3 and 4 correspond to the same in Table 5 and are explained there.

Table 4: Collusion and Fairness Appeals Summary

		1x2 DA-Barg		1x2 DA-Chat		2x3 DA-Chat		Overall	
Number of Markets		117		80		34		231	
Collusion Proposed	Fairness Discussed	59 50.4%	18 15.4%	34 42.5%	28 35.0%	15 44.1%	13 38.2%	108 46.8%	59 25.5%
Agreed		30 50.9%		26 76.5%		11(including 9 trilateral agreements) 73.3%		67 62.1%	
Honored		15 50.0%		9 34.6%		1(including 1 trilateral agreement)† 9.1% (11.1% of trilateral agreements)		25 37.3%	
Success		15 100.0%		5 55.6%		1 100.0%		21 84.0%	
Success∩ Fairness	Rate Conditional on Fairness	10	55.6%	4	14.3%	0	.00%	14	23.7%
Success Rate (Unconditional)		12.82%		6.25%		2.94%		8.71%	

Note: Unless noted otherwise, all rates are conditional on the counts in the row above.

† The only honored agreement was a trilateral one and it was honored by all three long siders. No trilateral agreement was honored by only two long siders.

Table 5: Logit Regressions for Collusion and Efficiency (with Random Session Effects)

Regression number	1	2	3	4	5
Dependent Variable	Successful Collusion	Successful Collusion	Missed Trade	Missed Trade	Honored Agreement
Constant	-1.816***±.513	-3.348***±.724	-3.893***±.746	-4.083±.776	-1.408***.425
Chat			1.523**±.720	.840±.798	
Barg	.803†±.555	1.537**±.647	1.409*±.807	1.674**±.842	.158±.462
Seg2	-.726†±.525	-.881±.589	-.097±.439	-.183±.466	-.228±.451
Smarket	-1.851***±.651	-1.458**±.695	-.097±.439	.074±.477	-1.535***±.529
D2x3	-.938±1.13	-1.290±1.18	-.294±.635	.059±.653	-1.532†±1.085
Collusion proposal only[1]				.061±.673	
Collusion agreement[2]				-.270±.744	
Honored agreement[3]				2.398**±.944	
Fairness discussion[4]		2.509***±.595		.831±.617	
Barg*CollusionHonored				-37.779±>1000	
Data Range	DA-Barg and DA-Chat	DA-Barg and DA-Chat	All Treatments	All Treatments	DA-Barg and DA-Chat
Log likelihood	-60.141	-49.721	-85.946	-79.572	-71.6711
Rho	.000	.000	.047	.027	2.53E-07
Number of observations	231	231	445	445	231

Note: all regressions use second half data.

[1] This dummy takes value 1 where collusion proposals were sent but did not result in an agreement.
[2] This dummy takes value 1 when a collusion agreement, either bilateral or trilateral, was reached. See Appendix B for more details.
[3] This dummy takes value 1 when there was a collusion agreement and the long siders did not deviate from the agreement price. See Appendix B for more details.
[4] This dummy takes value when there were some statements regarding fairness. See Appendix B for more details.
† indicates significance at one-sided 10% level.

Table A1: Regressions of SSS (Random Session Effects GLS)

Specification	1	2	3	4	5	6
Constant	.829***±.054	.894***±.074	.788***±.103	.646***±.029	.783***±.084	.847***±.042
Chat	-.194***±.070	-.093±.113	-.081±.147	.032±.045	-.126±.11	-.169***±.046
Barg	-.220***±.065	-.431***±.13	-.327**±.148	.043±.042	-.124±.101	-.206***±.059
D2x3	.069±.064		.081±.085	.068***±.022	.047±.101	.017±.045
Seg2	.094**±.045	-.155*±.09	.002±.118	.017±.039	.072±.066	.018±.018
Smarket	.023±.045		.023±.056	.008±.022	.014±.066	.042***±.012
Bgn*S2	.020±.033	.339**±.163	.103±.169	-.13**±.056	-.033±.029	.106***±.029
Bgn*Smkt	.121***±.033		.118***±.032	.054*±.029	.061**±.029	
Cht*D2x3	-.001±.088		-.038±.119	-.001±.024	.022±.14	
Cht*S2	.010±.026	.123±.128	.095±.125	-.012±.057	-.001±.023	.028±.027
Cht*Smkt	.069***±.026		.067**±.026	.044*±.023	.042*±.023	
S2*D2x3	-.080***±.026		-.071**±.034	-.072***±.024	-.084***±.023	
S2*Smkt	-.043±.078		-.042±.104	-.005±.02	-.016±.126	
Smkt*D2x3	-.016±.026		-.016±.026	-.018±.023	-.016±.023	
Trend		-.003±.009	.005±.01	.026***±.004		
Trd*Bgn		.026*±.015	.013±.015	-.03***±.006		
Trd*Cht		-.009±.013	-.012±.013	-.028***±.006		
Trd*S2		.025**±.012	.012±.013	.008±.006		
Trd*Bgn*S2		-.03±.021	-.01±.021	.018**±.009		
Trd*Cht*S2		-.015±.016	-.012±.016	0±.009		
Range	Second Half	Second Half	Second Half	All Periods	All Periods	Second Half
R-square (overall)	.365	.326	.376	.246	.127	.327
Rho	.245	.217	.376	0	.363	.239
Number of Observations	422	422	422	846	846	422

Table A2: Mean SSS in the First Halves

	DA-Std	DA-Chat	DA-Barg
1x2	.767***±.018	.731±.013	.702†±.018
2x3	.739***±.020	.731±.013	

*** indicates significant difference from second halves at 1% level.
† indicates that the mean is significantly below second half data at one-sided 10% level.

Table A3: Logit Regressions on Successful Collusion (with Random Session Effects)

Specification	1	2
Constant	-1.187*±.585	-38.744±>1000
Barg	1.693**±.681	
Seg2	-.019±.711	-38.744±>1000
Smarket	-1.574*±.850	-.735±.896
D2x3	-.744±1.195	3.348±2.045
Nonspecific collusion price[†]		-42.215±>1000
Planned collusion profit[††]		-7.513**±3.323
Trilaterality of agreement		-1.328±1.250
Data Range	DA-Barg and DA-Chat	DA-Chat only, with collusion agreements
Log likelihood	-34.268	-11.059
Rho	0.000	0.000
Number of Observations	67	37

†This dummy takes value 1 when there was no specific collusion price and the long siders simply agreed to, for instance, ``hold the same price". See Appendix B for details.
††When the long siders agree on a specific collusion price, the planned collusion profit is defined as the difference between this price and the cost (or value) of a long side seller (buyer).

Table A4: SSS Regressions with Random Short Sider Effects and Clustering at the Session Level

Specification	1	2	3	4	5
Constant	.831***±.038	.755***±.056	.836***±.038	.705***±.033	.849***±.035
Chat	-.153***±.038	-.163***±.037	-.153***±.038	-.112***±.037	-.101***±.036
Barg	-.149***±.054	-.146***±.054	-.149***±.054	-.099**±.042	-.086*±.047
D2x3	.017±.035	.037±.037	.017±.035	.029±.032	.017±.032
Seg2	.050**±.024	.050**±.024	.052**±.024	.021±.019	.040**±.021
Smarket	.042*±.024	.042*±.024	.029±.026	.022±.019	.016±.021
Trend		.009**±.004		.015***±.005	
Session 1			.144***±.026		
Collusion proposal only					-.061***±.022
Collusion agreement					-.075***±.011
Honored agreement					-.179***±.033
Fairness discussion					-.042±.033
Data Range	Second Half	Second Half	Second Half	All Periods	Second Half
R-sq (overall):	.309	.314	.298	.163	.491
Rho	.425	.433	.437	.285	.409
Number of Observations	422	422	422	846	422

Table A5: More SSS Regressions with Random Short Sider Effects and Clustering at the Session Level

Specification	1	2	3	4	5	6
Constant	.833***±.045	.836***±.053	.767***±.06	.644***±.036	.787***±.021	.85***±.041
Chat	-.192***±.047	-.015±.085	-.011±.131	.047±.05	-.126***±.023	-.169***±.043
Barg	-.217***±.061	-.314**±.152	-.277*±.159	.043±.061	-.125**±.050	-.201***±.075
D2x3	.064±.052		.069±.046	.066±.045	.044±.050	.017±.035
Seg2	.085**±.038	-.033±.062	.036±.048	.024±.041	.065*±.038	.014±.045
Smarket	.019±.052		.017±.054	.012±.044	.012±.041	.039±.025
Bgn*S2	.023±.030	.132±.118	.023±.127	-.14±.101	-.030±.031	.104±.064
Bgn*Smkt	.119***±.029		.119***±.034	.063*±.033	.064**±.030	
Cht*D2x3	-.001±.074		-.031±.083	-.004±.067	.022±.069	
Cht*S2	.017±.038	-.058±.126	-.072±.132	-.04±.05	.007±.032	.031±.049
Cht*Smkt	.060±.038		.064±.042	.04±.034	.037±.032	
S2*D2x3	-.072**±.036		-.048±.047	-.063±.034	-.078**±.031	
S2*Smkt	-.036±.071		-.035±.071	-.016±.069	-.012±.068	
Smkt*D2x3	-.014±.036		-.014±.042	-.016±.034	-.015±.031	
Trend		.006±.009	.009±.008	.027***±.009		
Trd*Bgn		.011±.017	.006±.016	-.031***±.012		
Trd*Cht		-.019±.012	-.021±.015	-.031***±.01		
Trd*S2		.008±.008	.005±.007	.007**±.003		
Trd*Bgn*S2		-.003±.015	.002±.015	.021±.016		
Trd*Cht*S2		.01±.016	.01±.016	.006±.007		
Data Range	Second Half	Second Half	Second Half	All Periods	All Periods	Second Half
R-square (overall)	.365	.321	.374	.245	.127	.326
Rho	.404	.411	.414	.311	.256	.418
Number of Observations	422	422	422	846	846	422