The Opportunity for Conspiracy in Asset Markets Organized with Dealer Intermediaries

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This article reports an asset market experiment in which asymmetrically informed traders transact through competing dealers. Dealers face a classic adverse selection problem, because some traders have private information regarding the asset value while other traders are uninformed. When dealers cannot communicate outside the market, they price the asset competitively and the market is generally informationally efficient. When dealers communicate privately between periods, they collude successfully to widen spreads and increase profit. Another treatment permits traders to post limit orders, while still allowing dealers to communicate. Limit orders restore informational efficiency and narrow spreads but cause dealers to earn negative trading profits.

> Market Maker 1: Hey, alright, uh, we're still goofing around with this MCAWA. I just went down an eighth on the bid.
> Market Maker 2: Okay.
> Market Maker 1: And that let me do that. So I told [Market Maker 3] to go down an eighth. . .
> Market Maker 2: If that's what you guys want me to do, I'll do it.
> Market Maker 1: Try it and then I'm going to try and go down another eighth, you know what I mean, and get it, get it back to $3/4 spread.
> —Phone conversation between two market makers for McCaw Cellular stock, April 8, 1994 (SEC, 1996b, p. 16).

In the past several years U.S. antitrust authorities and securities market regulators have taken several policy actions that are likely to dramatically influence price competition on the Nasdaq market. First, in July 1996 the Justice Department settled a civil action against the NASD and a long list of market makers to restrain violations of Section 1 of the Sherman Act. This case focused on illegal communication between market makers to set wide spreads and the long-standing Nasdaq quoting convention (*U.S. v. Alex.*

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Brown & Sons Inc. et al.; see note 1). In early August 1996 the Securities and Exchange Commission (SEC) settled a related but more wide-ranging enforcement action against the NASD concerning collusion among market makers and inadequate self-regulation. These investigations stemmed in part from research findings reported in Christie and Schultz (1994a, b) that documented avoidance of odd-eighth quotes without an obvious explanation other than collusion. Second, in late August 1996 the SEC adopted a rule requiring the public display of customer limit orders on the Nasdaq as well as the exchanges. This rule was phased in during 1997 and represents a major change for the handling of limit orders in the Nasdaq market, where dealers had not typically displayed such orders (SEC, 1996a).

Theoretical and empirical market microstructure research is useful for identifying potential implications of these new regulations that affect the specific rules governing exchange (e.g., O'Hara, 1995). Experimental economic methods are also valuable to assess the impact of these new regulations, and for many years researchers have used this methodology to characterize some of the behavioral properties of asset and nonasset markets (e.g., Sunder, 1995). Until recently, however, most laboratory asset market research has not been closely related to the market microstructure literature that studies intermediation, because the trading mechanisms employed in the laboratory rarely employ strategic intermediaries. Instead, traders usually trade directly with each other in a continuous double auction market.

The laboratory markets presented here add to a rapidly growing experimental literature that studies intermediation by allowing only a subset of subjects (the *dealers*) to make a market by posting public bid and ask prices [e.g., Bagnoli and Watts (1997), Lamoureux and Schnitzlein (1997a), Bloomfield and O'Hara (1998, 1999), Flood et al. (1999)]. In two of the three experimental treatments the remaining subjects (the *traders*) can only accept these public quotes, and limit orders are prohibited. These institutional features represent, for example, the privileged position of Nasdaq dealers relative to small traders who lack the ability (or incentive) to negotiate individual trades. The experimental design addresses three primary research objectives. The first research objective concerns the informational efficiency of these markets organized by dealer intermediaries. The asset traded in these laboratory markets pays the same state-contingent dividend to all subjects, but one set of traders has private information regarding the asset value while another set of traders is uninformed and must satisfy an exogenous and random liquidity demand. Dealers are never informed of the asset value ex ante, so they face a classic adverse selection problem when pricing the asset. The results indicate that when nonmarket communication is prohibited, dealers price the asset competitively and the market is informationally efficient in all but one session. In this environment the overall market performance with dealer intermediaries is similar to the performance of the double auction market organized without intermediaries.

The second research objective is to evaluate the ability of dealers to implement explicitly collusive agreements in this setting. The design introduces conspiratorial opportunities in some sessions by permitting dealers to communicate verbally and privately between periods. Dealer opportunities for nonmarket communication are greater in the experiment compared to markets in the field, and as discussed in Section 2.2 the communication structure avoids constraints on collusion imposed by within-period renegotiation and subgroup communication. The form of communication implemented here therefore provides very favorable conditions for collusion, but this is appropriate given my goal to determine if the trading rules of this institution permit collusive agreements to succeed. If collusion is unsuccessful even in this favorable environment, then this provides evidence that collusive asset pricing allegations in field markets should be viewed with skepticism. Successful collusion in this trading institution is not a foregone conclusion because previous laboratory research has demonstrated that the market institution plays an important role in determining the success of explicit conspiracies. For example, Isaac and Plott (1981) found that explicit conspiracies (also implemented through verbal communication between periods for a subset of market participants) were difficult to enforce when trading was organized through the *continuous double auction* institution. In contrast, Isaac, Ramey, and Williams (1984) found that sellers could enforce explicit conspiracies much more successfully when *posted offer* rules governed trading. The dealer market institution studied here shares features of both of these institutions, so the feasibility of conspiracies in this setting is an open research question.

The results show that when dealers can communicate they generally successfully collude to dramatically widen spreads and increase profit. This successful collusion typically leads to completely uninformative asset prices, and it occurs even though dealers have a continuous "temptation to defect" from the agreement [Clauser and Plott (1993)]. It seems likely that the screen-based trading system employed in the experiment facilitated collusion, because it allows immediate detection of cheating dealers. Notably, this nearly continuous information regarding rival dealer quotes is also available in many dealer markets in the field.

The third research objective is to evaluate the impact of trader limit orders on dealer competition and market performance. Nasdaq market makers have recently been required to begin publicly displaying trader limit orders. This allows traders to compete directly with dealers to supply liquidity. To highlight the greatest potential impact of this competition, in the limit orders treatment dealers are still permitted to communicate verbally between periods to discuss collusive agreements. In spite of this dealer communication, asset prices in this limit orders treatment are informationally efficient and spreads tighten to levels observed in the treatment without dealer communication opportunities. Aggregate dealer trading profits are negative, however, in every session with limit orders permitted.

These three treatments have a natural interpretation that corresponds to asset market regulation in the field. The treatment with no limit orders but nonmarket dealer communication corresponds to a setting with poor antitrust enforcement and dealer collusion through mechanisms such as the Nasdaq quoting convention.[1] The treatment without dealer communication corresponds to an environment with more active and stringent antitrust enforcement, while the treatment with limit orders and continued dealer communication corresponds to lax antitrust enforcement but quote competition from traders. The experimental results indicate that the stringent antitrust enforcement and limit orders both increase informational efficiency and tighten spreads, and the treatments are statistically indistinguishable on these market performance measures. Dealer profits, however, are significantly lower with limit orders than with antitrust enforcement. This suggests that the recently adopted limit order display rules *may* have an adverse impact on liquidity and immediacy if these rules result in a substantial reduction in the supply of market-making services.

Section 1 presents the experimental environment, and Section 2 reviews the dealers' adverse selection problem and additional theoretical considerations affecting collusion. Section 3 collects the results, and Section 4 concludes. At this point it is worth discussing several previous laboratory asset market studies that are relevant to the proposed research because they explicitly study intermediation. Lamoureux and Schnitzlein (1997b) implement a market with competing dealers, and traders who can simply accept the dealers' posted quotes to execute transactions. They focus on the role of an "off-floor" search market [similar to that first implemented by Campbell et al. (1992)] in disciplining dealer bid-ask spreads. As predicted, the search market reduces dealer profits and tightens the bid-ask spread, but surprisingly the search market appears to provide better price discovery. A treatment examined in Friedman (1993) also explicitly examines intermediation. In this treatment some traders were privileged market makers who could enter bids and asks to the market. Other traders could only accept these quotes. Friedman finds that this privilege was extremely profitable—with market makers earning up to three times the profit earned by other traders—and that market efficiency suffered as a result.

Bloomfield (1996) and Bloomfield and O'Hara (1998, 1999) also use laboratory experiments to study dealer intermediation, but in an environment with a series of discrete dealer auctions, rather than with continuous trading

[1] The quoting convention, which had existed for at least three decades on Nasdaq, consisted of two parts. Under the first part, if a market maker's dealer spread was 3/4 or wider, the convention required the market maker to post bid and ask prices in even-eighth increments. This ensured that the inside spread in those stocks was at least 1/4. Under the second part, market makers could only quote bid and ask prices in odd-eighth increments if they had a dealer spread less than 3/4. This deterred market makers from quoting bid and ask prices in odd-eighth increments because this was likely to result in a narrower spread. Market makers used a variety of mechanisms to enforce this convention, including non-market communication (U.S. Department of Justice, 1996).

as in the present study. Bloomfield and O'Hara (1998) study order preferencing in a setting without strategic traders. Their results indicate that when there are only two market makers, preferencing has a negative impact on market performance. With three market makers, however, preferencing does not have a significant impact on market outcomes. Bloomfield and O'Hara (1999) study the impact of market transparency by limiting the disclosure of price and quote information in different treatments. They find that disclosure benefits market makers but harms traders. Flood et al. (1999) also examine the impact of quote disclosure, but with continuous auctions (as in the present experiment). Their experiment identifies a distinct trade-off between liquidity and informational efficiency. In opaque markets, opening spreads are wider due to higher search costs; however, the higher search costs lead to more aggressive pricing and better price discovery. Bagnoli and Watts (1997) use batched orders rather than continuous trading, and they compare order- and quote-driven laboratory markets. They find that asset prices are more accurate in later rounds in quote-driven markets.

1. Experimental Design

In most laboratory studies, (1) the asset has a one- or two-period life and traders are reendowed each period with new assets and capital; (2) traders are typically divided into two or more "types" of investors; and (3) to simplify uncertainty the environment uses a small number of discrete states of the world [Sunder (1995, p. 492)]. Corresponding to these three design elements, in the present experiment (1) the asset expires after one period; (2) traders are divided into two investor types; and (3) the environment implements two dividend states.

Consistent with much of the theoretical market microstructure literature, one set of traders was *informed* and one set was *uninformed* of the true dividend value. Each session included five traders and three dealers, except for the two double auction sessions that did not include any dealers. The three uninformed traders were assigned end-of-period required positions to provide liquidity demand and supply. Following Lamoureux and Schnitzlein (1997b), I determined each uninformed trader's required net trades by an independent draw from a uniform distribution over $[-4, 4]$, excluding 0. (Lamoureux and Schnitzlein employ a wider distribution $[-5, 5]$.) The experimenter drew these required positions from a bingo cage at the beginning of each period. Traders paid a penalty of 1000 experimental "francs" for every unit that their actual position differed from the required position at the end of the period. The two informed traders learned the exact dividend paid for each unit of the asset (either 200 or 500 francs) before trading opened for the period. The two dividend states were equally likely, and a coin flip determined the state before the period began. The identity of the informed traders changed randomly between periods, as determined by rolling a die.[2] The experimenter delivered the dividend information or required position privately to each trader prior to opening the period for trading, using an e-mail feature built into the Multiple Unit Double Auction (MUDA) trading software used in the experiment [Plott (1991)]. The three dealers never learned the dividend value until the end of the period. Therefore only 2 of the 8 subjects knew the dividend state at the start of trading each period. Each session included 11 five-minute trading periods.[3]

All subjects observed a trading screen that continuously displayed public information—the bid and ask quotes chosen by the three dealers. Each subject also had private information—her own inventory position as well as her required position or the dividend value. When limit orders were prohibited, only dealers could post bids and asks, and traders could execute transactions only by accepting the dealer quotes. All dealers' bid and ask quotes were continuously displayed to all subjects. I deliberately chose an environment in which dealers could easily observe each other's quotes, because this makes it easy to police tacitly or explicitly collusive agreements.[4] When limit orders were permitted, traders could post bids and asks that were automatically displayed publicly and could be accepted by other traders (or dealers). I did not include a limit order book for traders to enter bids or asks that did not improve the currently available best terms of trade; to be accepted by the trading program for display, bids or asks had to strictly improve upon existing bids or asks. Therefore if a dealer and a limit order had the same quote, only the first quote was recognized by the trading program and displayed for possible acceptance (i.e., time priority). Subjects could submit a depth of one or two shares along with their price quotations, and a particular spread remained active following a transaction only if the depth remained positive (i.e., following a single-share transaction when the pretransaction depth was two shares). Subjects could not direct orders specifically to a dealer or to a limit order. Traders were endowed with four units of the asset and dealers were endowed with two units at the start of each period. There were no restrictions on short sales, and subjects had unlimited access to trading capital. Subjects who did not cover their short position by the end of the trading period paid the dividend value for each unit held short.

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[2] Using random devices in full view of subjects in this way increases credibility and subject understanding of the random processes. This design feature comes at a cost, however, as different sessions had different realizations of the random variables.

[3] Because of a network crash, session 307 included only 10 usable trading periods. Subjects did not know the exact number of periods, although the experiment used nearly all of the two-hour period for which they were recruited. Therefore, most subjects probably knew that the 11th period was either the last period or the next-to-last period.

[4] As mentioned previously, this quote information is available to dealers in field markets. Dealers apparently used this quote information to enforce the quoting convention on Nasdaq, as the Department of Justice identified numerous instances in which dealers telephoned each other to secure compliance with the quoting convention (U.S. Department of Justice, 1996). Often these were "friendly" calls to remind another market maker to stop (inadvertently) violating the convention.

I recruited subjects from undergraduate economics classes at USC, so most were majoring in economics or business. In the sessions reported here, all subjects were experienced, in the sense that they had previous training and experience trading in laboratory asset markets organized with dealer intermediaries. In particular, subjects were required to participate in a two-hour training session that included the following: (1) computerized instruction in trading procedures for the MUDA software; (2) specific instructions regarding the asset market environment and accounting procedures, which were read aloud by the experimenter while the subjects followed along on their own copy (the instructions are available from the author); and (3) four periods of trading. Subjects earned salient rewards in these trading periods. Salient earnings for the subsequent two-hour "data" sessions reported here ranged between $0 and $43.50, with an average of $24.50.

Some readers might consider this two-state environment to be excessively simple for investigating asset pricing. Many previous laboratory asset markets are more complex, some with longer-lived assets or more states. My goal, however, was to implement a simple environment in which outcomes can permit precise inferences regarding (a) the competitiveness of dealers and (b) the informational efficiency of asset prices. This simplified design also permits an evaluation of specific theoretical predictions from the market microstructure literature, some of which are derived in this kind of simplified, two-state environment [e.g., Pagano and Roell (1992)]. The guiding design philosophy is that in simpler environments the researcher is more likely to understand what is being observed. A natural extension would be to use a more complex and realistic environment (perhaps with a continuous asset distribution), but a necessary first step is a thorough study in this simple environment.

I report 17 data sessions, summarized in Table 1. The first digit of the session name indicates the number of dealers, and the second two digits indicate the sequence in which the sessions were conducted. A *c* in the session name indicates that dealers could communicate between periods and trader limit orders were prohibited, and an *L* in the session name indicates that traders could post limit orders. Each subject typically participated in two data sessions; an *x* at the end of the session name indicates the sessions with subjects who were more experienced.[5] The four treatments differed only in the number of dealers, the communication opportunities available to dealers, or the ability of traders to enter limit orders. In the *no communication* treatment, three dealers competed for order flow from the traders without

[5] The notes below Table 1 indicate the session in which subjects gained their experience. Subjects were randomly assigned to trader and dealer roles in each session, so that dealers in an experienced session sometimes were dealers in an earlier inexperienced session; in particular, 7 of the 24 dealers in the more experienced sessions had been dealers in a less experienced session.

Table 1
Summary of Laboratory Sessions

Session name	Number of dealers	Trader limit orders	Communication between dealers
Three dealers, no communication permitted (no communication)			
Session 301	3	Not permitted	Not permitted
Session 305x	3	Not permitted	Not permitted
Session 307	3	Not permitted	Not permitted
Session 309	3	Not permitted	Not permitted
Session 313x	3	Not permitted	Not permitted
Three dealers, nonmarket communication permitted (communication)			
Session 3c03	3	Not permitted	Permitted
Session 3c06x	3	Not permitted	Permitted
Session 3c08x	3	Not permitted	Permitted
Session 3c10	3	Not permitted	Permitted
Session 3c12x	3	Not permitted	Permitted
Three dealers, nonmarket communication and limit orders permitted (limit order)			
Session 3L15	3	Permitted	Permitted
Session 3L17	3	Permitted	Permitted
Session 3L18x	3	Permitted	Permitted
Session 3L19x	3	Permitted	Permitted
Session 3L20x	3	Permitted	Permitted
No dealers (double auction)			
Session 014x	0		—
Session 016	0		—

Each session included 11 trading periods, 3 uninformed traders, and 2 informed traders. The asset value in each period was 200 or 500 experimental francs, with equal probability. The two informed traders learned the exact asset value before trading opened, and all other subjects received no exogenous asset value information in that period. The three informed traders were required to meet a random required position at the end of the period (see text). Traders were endowed with four assets and dealers were endowed with two assets at the start of the period. There were no restrictions on short sales, and subjects were provided with unlimited access to trading capital.

Subjects in session 305x gained experience in session 3c03; those in 313x gained experience in 3c10; those in 3c06x gained experience in 301; those in 3c08x gained experience in 307; those in 3c12x gained experience in 303; most subjects in 3L18x gained experience in 016; those in 3L19x gained experience in 3L17; those in 3L20x gained experience in 3L15; and those in 014x gained experience in an unreported session with one (monopoly) dealer, session 111.

any nonmarket communication opportunities.[6,7] In the *communicating dealers* treatment, conditions were identical except that the three dealers were excused into the hallway before each period to "discuss all aspects of the market fully" as long as they did not "discuss side payments or make physical threats." These communication rules are the same as those employed by Isaac and Plott (1981) and Isaac, Ramey, and Williams (1984), discussed in

[6] Many market microstructure models assume that when at least two competing market makers exist, Bertrand-like competition will generate competitive asset pricing. The number of dealers necessary for competition in this laboratory environment is an empirical question. Bloomfield and O'Hara (1998) find that three dealers are sufficient (and two dealers insufficient) for competitive asset pricing.

[7] In the experiment, dealers compete for order flow on the basis of price. The experiment does not include preferencing, which can diminish the incentive for dealers to narrow the quoted spread [Godek (1996), Kandel and Marx (1997)]. Bloomfield and O'Hara (1998), however, find that preferencing has no effect on informational efficiency or the distribution of profits when there are three dealers.

the introduction. Some theoretical implications of this type of communication arrangement are discussed in Section 2.2. Although traders could not observe these dealer discussions, a research assistant openly monitored the discussions.

In the *limit order* treatment, dealers could communicate between periods as in the communicating dealers treatment, but traders could enter limit orders to the market as described above. Finally, I report two sessions with no dealer intermediaries. In these *double auction* sessions all traders could post bid and ask prices. This is the standard procedure for this well-studied institution. Consistent with previous research (usually in a more complex environment), asset prices were informationally efficient in these benchmark control sessions. Since results were unsurprising and research has studied this continuous double auction institution extensively, I chose to devote most of the scarce subject payment budget to the dealer market institution.

2. Theoretical Considerations

2.1 Competitive dealers

An advantage of this simplified environment is that it permits a direct application of theoretical market microstructure models. Recall that dealers never learn the dividend value until the end of the period. Consequently, they face an adverse selection risk when posting bid and ask prices because they do not know whether an informed or liquidity trader will take the other side of a transaction. Dealers must therefore post a wide bid-ask spread in early trading (before transaction prices reveal the dividend value) to avoid a negative expected profit.

Glosten and Milgrom (1985) develop a model to study this adverse selection risk. They calculate zero expected profit price quotes in the presence of informed and liquidity traders, and the example they present in their Section 3 is consistent with the present experimental design. Although initially uninformed, rational dealers obtain information regarding the private information of the informed traders based on transaction prices. In the Glosten and Milgrom model competitive asset prices and spreads evolve based on Bayesian updating of the dealer beliefs in response to trader purchases and sales. As trading activity reveals information about the dividend state, competition and dealers' increased confidence that they have better inferred the state causes spreads to tighten across transactions. Moreover, dealers increase their belief that the value is high following buy orders, so that the competitive bid and ask rise following buy orders. The converse is true following sell orders.

In this way transactions disseminate information to the market so that quotes posted by dealers better reflect the fundamental value of the asset. Through this mechanism, in the later part of each trading period the market can be informationally efficient. Dealers, however, may not compete as aggressively as is often assumed in theoretical models. If dealers compete less aggressively and widen spreads, each transaction may reveal less (private, trader) information. Taken to an extreme in this experimental environment, if dealers collude to post asks above the high dividend of 500 and post bids below the low dividend of 200, then only liquidity traders will transact and prices will be completely uninformative.

I should emphasize that the range of behavior possible for these human subjects is considerably richer than usually considered in theoretical models. For example, although uninformed traders' net asset demand is exogenous, the timing within a period of these transactions is endogenous. Like dealers, the uninformed traders can observe transactions to infer the dividend state, so some uninformed traders may choose to transact later in a period. As Lamoureux and Schnitzlein (1997b) also emphasize, this endogenous timing of trading complicates the application of standard theoretical models. Bulow and Klemperer (1994) provide some theoretical progress along this line, but in a different trading environment. If dealers recognize differences between informed and uninformed trader strategies, then prices can reflect inside information much more quickly.

2.2 Collusive dealers

Inspired by recent collusion allegations on the Nasdaq, several researchers have developed theoretical models to better understand collusion incentives in dealer markets. Dutta and Madhavan (1997) employ a dynamic model with repeated dealer interaction to derive noncooperative equilibria with strictly positive dealer profits. Kandel and Marx (1997) explicitly account for institutional features of the Nasdaq market (such as the fixed tick size, preferencing, and the excess spread policy) and derive noncooperative equilibria that can explain the empirical phenomenon of odd-eighths avoidance identified by Christie and Schultz (1994b). These models suggest that nonmarket communication is not necessary for dealers to collude.

In the laboratory markets with dealer communication reported here, dealers have a "first mover" advantage to select (possibly equilibrium) strategies that lead to substantial trading profits. Unfortunately one cannot refine the set of equilibria by relying on backward induction and focusing on the single-shot (stage game) Nash equilibria, because there is no last instant at which orders can be submitted.[8] The set of Nash equilibria is therefore large and probably contains collusive outcomes. These collusive equilibria can be supported by trigger strategy punishments, even in the no communication treatment.[9]

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[8] I am grateful to an anonymous referee for pointing out this problem of using backward induction in this setting. I am also grateful to this same referee for drawing my attention to the importance of renegotiation and subgroup communication discussed below. Friedman (1984) sidesteps the problem of an ill-defined final period in his theoretical study of the efficiency properties of double auction institution. He assumes a hypothetical final chance to trade when defining his "no-congestion" equilibrium (an "extra-chance game").

[9] The laboratory subjects have lives outside the experiment, so they could employ additional punishments beyond competitive pricing—just as dealers in the field have opportunities to exercise punishments outside the market.

If one focuses on self-enforcing (Nash equilibrium) agreements between dealers, two additional considerations arise: renegotiation and subgroup communication. As explained momentarily, the present communication structure biases results toward obtaining more collusive outcomes. This is consistent with my goal to give dealers a "best shot" at colluding in the communication treatment, and then to determine the impacts of allowing for trader limit orders or prohibiting dealer communication.

First consider the impact of prohibiting dealer renegotiation within a trading period, taking the perspective that each trading period is a complete game. Sustaining subgame-perfect collusive agreements between dealers typically requires that dealers agree to switch to single-shot competitive strategies in response to cheating. If dealers were allowed to communicate again within the trading period, however, these agreements may not be renegotiation-proof. After a dealer has cheated, he could argue that carrying out the (off-equilibrium) punishment hurts all dealers, and he could propose instead a continuation of the original agreement. This leads to the well-known "problem of renegotiation": opportunities for renegotiation raise the dilemma that threats lose credibility, so that many collusive outcomes are no longer sustainable equilibria [e.g., see Bergin and MacLeod (1993) or McCutcheon (1997) for a recent application]. Opportunities to renegotiate within a period could therefore reduce the sustainable level of collusion. The communication structure in the experiment may facilitate collusion because it prohibits within-period communication.

Next consider the impact of permitting communication only among all three dealers. There is no opportunity for subgroup communication, in which two dealers could meet to "double cross" the other. If this opportunity were available, some collusive agreements struck by dealers unanimously may not be coalition-proof [Bernheim, Peleg, and Whinston (1987)], in the sense of being self-enforcing with respect to potential subgroup deviations. The format of group communication therefore determines the set of self-enforcing Nash equilibria [Milgrom and Roberts (1996)], and the experiment may facilitate collusion because it does not permit dealer subgroups to communicate.

It seems reasonable to allow subgroups of dealers to communicate privately in another experimental treatment, particularly because in the field one typically observes subgroups of communicating dealers rather than all dealers communicating. It would be straightforward to investigate the importance of coalitions in this environment by allowing only a subset of the dealers to communicate, or by allowing a subset of the dealers to communicate following the discussion of the entire group of dealers. But we leave this study of coalition formation and coalition-proof equilibria for future research, given that the goals of this article are to create an environment conducive to collusion and to evaluate alternative mechanisms to reduce collusion. It might be wise to study coalition-proof equilibria, as well as renegotiation, in a simpler and more theoretically tractable environment.

2.3 Limit versus market orders

Introducing the public display of trader limit orders fundamentally changes the way that the asset is traded. Several researchers have recently examined traders' strategic choice between limit and market orders, in the context of auction markets (such as the NYSE) where limit orders play a central role in price formation. For example, Handa and Schwartz (1996) show that short-run price fluctuations due to liquidity events can make limit order trading profitable. The model presented by Harris (1998) is supported empirically by Harris and Hasbrouck (1996), and is particularly relevant for the present laboratory study because it highlights differences between liquidity and informed traders' strategies. Early in the trading period—particularly when spreads are wide—liquidity traders initially submit limit orders. If these orders do not fill, later in the period (as the deadline approaches) they submit market orders to ensure execution. By contrast, informed traders try to profitably trade on their information; consequently they will tend to use market orders early in the period to trade quickly before their information becomes public. Under certain conditions, informed traders then submit limit orders as the trading deadline approaches. Although the economic environment studied in Harris (1998) differs in several important ways from this laboratory environment, my results generally support his characterization of liquidity and informed trader strategies.

3. Results

I present the results in five subsections. Section 3.1 presents the sessions with no communication opportunities, as well as the no dealer (double auction) sessions. Section 3.2 presents the sessions in the communicating dealers treatment. Section 3.3 presents the sessions with trader limit orders permitted, and examines the traders' order submission strategies. Section 3.4 compares the treatments' average quantitative measures of market performance. Section 3.5 studies the dynamics of the price formation process and (partially) accounts for nonstationarities that arise because the performance measures depend on elapsed time via transactions.

3.1 Competing dealers with no communication opportunities

Transaction prices quickly reflect traders' private information in four of the five sessions without nonmarket communication between dealers. In session 305x, however, prices more poorly reflect the asset value. In this session dealers tacitly colluded, which indicates that communication is not necessary for dealer collusion.[10] The first two columns of Table 2 indicate the overall informational efficiency of each session: column 1 displays the RMSE of the

[10] In this more experienced session, one of the dealers had been a dealer in session 3c03 and the other two dealers had been traders in session 3c03.

Table 2
Informational Efficiency and Spread Measures

	Session	RMSE (transaction price) (1)	RMSE (spread midpoint) (2)	Mean spread (francs) (3)	Mean spread (percent) (4)	Effective spread (percent) (5)
Three dealers, no communication permitted						
1	Session 301	74 (77)	81 (77)	105 (91)	32 (29)	33 (30)
2	Session 305x	278 (257)	147 (144)	458 (410)	137 (123)	138 (124)
3	Session 307	109 (81)	103 (91)	80 (81)	22 (21)	29 (28)
4	Session 309	133 (138)	125 (122)	196 (203)	60 (57)	62 (58)
5	Session 313x	94 (96)	100 (93)	149 (108)	47 (35)	50 (39)
Three dealers, nonmarket communication permitted						
6	Session 3c03	324 (367)	185 (110)	535 (480)	119 (105)	129 (124)
7	Session 3c06x	491 (430)	185 (212)	907 (922)	198 (198)	198 (198)
8	Session 3c08x	297 (272)	182 (224)	439 (466)	91 (96)	95 (96)
9	Session 3c10	154 (190)	132 (149)	253 (316)	74 (90)	76 (90)
10	Session 3c12x	579 (640)	258 (250)	1017 (1100)	190 (198)	190 (198)
Three dealers, nonmarket communication and limit orders permitted						
11	Session 3L15	163 (144)	113 (84)	125 (130)	39 (38)	55 (57)
12	Session 3L17	147 (89)	179 (138)	119 (143)	37 (46)	40 (49)
13	Session 3L18x	116 (92)	112 (109)	109 (136)	34 (40)	37 (41)
14	Session 3L19x	119 (152)	102 (127)	75 (118)	25 (39)	28 (41)
15	Session 3L20x	114 (152)	111 (138)	94 (107)	27 (27)	29 (30)
No dealers (double auction)						
16	Session 014x	120 (118)	94 (112)	83 (79)	24 (22)	24 (22)
17	Session 016	85 (115)	78 (105)	92 (147)	27 (43)	27 (43)

The root mean squared error (RMSE) in columns (1) and (2) measures informational efficiency, with lower numbers representing more accurate transaction prices and higher informational efficiency. The spread measures in columns (3)–(5) represent transaction costs. A calculation based only on the final 5 periods in each session is shown in parentheses.

RMSE (transaction price) is the root mean squared error of transaction price relative to the realized asset value.

RMSE (spread midpoint) is the root mean squared error of 0.5*(inside ask + inside bid) relative to the realized asset value.

Mean spread (francs) is the mean of (inside ask − inside bid), calculated at the time of the transaction.

Mean spread (percent) is the mean of 100*(inside ask − inside bid)/(0.5*(inside ask + inside bid), calculated at the time of the transaction.

Effective spread (percent) is the mean of 2*100*{|transaction price − [0.5*(inside ask + inside bid)]|}/(inside ask + inside bid), calculated at the time of the transaction.

transaction price from the asset's fundamental value across all transactions, and column 2 displays the RMSE of the spread midpoint from fundamental value. For all calculations in Table 2, the figure shown in parentheses presents an alternative calculation based only on the final five periods in each session.[11] Rows 1–5 contain results of the no communication sessions. The mean spread realized at each transaction is a useful summary of the level of dealer competitiveness. As shown in column 3 of Table 2, the four

[11] The calculations in Table 2 as well as the subsequent analyses remove obvious typographical errors, but this has no impact on the conclusions. Out of a total of 3062 transactions, 8 transactions were affected by these obvious typographical errors (0.3%). Five of these transactions were in the no communication treatment and three were in the limit order treatment.

competitive sessions had mean spreads ranging between 80 and 196 francs, while the tacitly collusive session 305x had a mean spread of 458 francs.

Rows 16 and 17 of Table 2 present these statistics for the no dealer (double auction) sessions. The impressive performance of this institution is not surprising, since previous laboratory asset market research using the double-auction institution in more complex environments has generally supported the informationally efficient (rational expectations) equilibrium over alternatives [e.g., Plott and Sunder (1982) and DeJong et al. (1992)]. What is perhaps surprising is the comparable performance of the double-auction and the competitive dealer markets. The RMSE of the competitive dealer sessions compares favorably to the RMSE of the two double-auction sessions.

3.2 The impact of dealer communication opportunities

Dealer communication opportunities dramatically reduced dealer competition. Rows 6–10 of Table 2 present the RMSE (informational efficiency) and spread statistics for the sessions with communication opportunities and no trader limit orders. In four of these five sessions dealers posted wide spreads in most periods, and these four sessions were informationally inefficient. Nonmarket dealer communication was not sufficient to generate effective collusion, however, as dealers were relatively competitive (and asset prices informative) in session 3c10.

The groups of conspiring dealers employed different mechanisms to coordinate prices. Dealers in session 3c03 had some difficulty coming to an agreement and suffered periodic breakdowns in collusion. By the end of this session, however, the dealers agreed on and successfully adopted a "follow-the-leader" strategy—selecting one dealer each period to lead any price changes of the group. Dealers in session 3c06x and session 3c12x held firm to some rather extreme bid and ask prices, and even agreed to raise the ask prices toward the trading deadline in some periods in an attempt to "train" the liquidity traders to execute transactions early in the period. It was not until period 4 that the dealers in session 3c08x discussed a specific collusive strategy. For many periods thereafter they adopted a strategy of pricing competitively at the start of each period, apparently in an attempt to infer the dividend state from the informed traders before widening the spread later in the period. The unsuccessful communicating dealers in session 3c10 never agreed on a collusive strategy, although in some periods they discussed in vague terms the "need to keep spreads wide."

I conclude from this treatment that dealers can usually recognize the value of colluding to widen the bid-ask spread; that dealers are able to implement a collusive agreement in this version of a dealer market trading institution; and that this collusion has a substantial negative impact on informational efficiency. Section 3.4 presents a formal comparison of the treatments.

Table 3
Number of Market Orders and Accepted Limit Orders Submitted by Informed and Uninformed Traders, for Limit Order and Double-Auction Treatments

Time period	Trader type	Trades through market orders	Trades through limit orders
First half (150 seconds) of period	Uninformed (liquidity) traders	309	382
First half (150 seconds) of period	Informed traders	334	223
Second half (150 seconds) of period	Uninformed (liquidity) traders	227	204
Second half (150 seconds) of period	Informed traders	238	252

3.3 The impact of trader limit orders

The limit order treatment permitted dealers to communicate between periods to determine if the public display of trader limit orders was sufficient to constrain dealers' ability to collude. Rows 11–15 of Table 2 summarize the five limit order sessions. This quote competition by traders is effective in combating dealer collusion. In all sessions, prices typically corresponded closely to the asset value after the first few transactions of each period. In no cases were dealers successful in keeping spreads wide, and this inability to control spreads was evident in the (frustrated) dealer discussions between periods.

Recall from the discussion of Harris (1998) that the differing information and incentives of liquidity and informed traders imply different order submission strategies. In particular, early in the period informed traders will tend to submit market orders to trade quickly before their private information is revealed in asset prices, while liquidity traders are initially patient and submit limit orders if early spreads are wide. As the trade deadline approaches, liquidity traders will submit market orders to ensure execution, while informed traders may submit limit orders.

Table 3 indicates that the limit order and double-auction treatments generally support these qualitative predictions. In the first half of the trading period in these treatments, liquidity traders more commonly transact through limit orders than through market orders. The reverse is true for informed traders in the first half of the trading period.[12] By contrast, in the second half of the trading period, liquidity traders transact through market orders on a (slight) majority of their trades, and a minority of informed trader transactions occur through market orders.

3.4 A comparison of the treatments

The different spread measures shown in columns 3–5 of Table 2 represent transaction costs differently, but across sessions they all support the same

[12] The total number of limit orders and market orders are not equal in this table because dealers sometimes take one side of the transaction.

qualitative conclusions.[13] Consider first the impact of prohibiting dealer communication, when trader limit orders are not allowed. Except for the tacitly collusive session 305x and the competitive session 3c10 with communication, spreads are tighter in the sessions without communication than in the sessions with communication opportunities. This also holds when looking only at the final five periods. A similar ordering holds for the RMSE informational efficiency measures shown in columns 1 and 2, for all periods or only the final five periods; the lowest efficiency occurs in the treatment with dealer communication and no limit orders. The nonparametric Mann–Whitney test indicates that the RMSE is significantly lower in the no communication treatment than in the communication treatment without limit orders: $U = 1$ or 2 depending on the column and whether one considers all periods or only the final five periods (the 5% critical value with sample size $n = m = 5$ is $U^* = 4$).[14] This test also indicates that spreads are wider in the communication treatment without limit orders than in the no communication treatment: U ranges between 1 and 4 for the tests based on data in columns 3–5. Notably, the tacitly collusive session 305x is most similar to the sessions with dealer communication opportunities according to these measures; and the RMSE and spreads in the relatively competitive session 3c10 with communication opportunities only marginally exceed the competitive sessions without communication. As indicated previously, this leads to the conclusion that nonmarket communication between dealers is neither necessary nor sufficient for collusion to occur.

When dealer communication is permitted, allowing limit orders significantly increased informational efficiency (U ranges between 0 and 3 for both the RMSE measures in columns 1 and 2). Spreads are also significantly wider without limit orders; for every column 3–5, the spread measure for all five sessions without limit orders exceeds the spread measure for all five sessions with limit orders. This implies that $U = 0$. Finally, consider the comparison between the treatment without dealer communication and the treatment with dealer communication and limit orders. Market performance is not significantly different in these two treatments. The Mann–Whitney test fails to reject the null hypothesis that the mean RMSE informational efficiency measures

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[13] The effective spread in column 5 often slightly exceeds the mean percentage spread in column 4 because traders occasionally fail to execute a trade at an inside quote. Recall that all quotes are publicly displayed and are all acceptable, and due to the often rapid quote revision, traders at times mistakenly hit a noninside bid or ask.

[14] In the Mann–Whitney U test, one first combines the elements of both samples into one set and ranks them in ascending order, though with the samples still retaining their separate identities. For each element of each sample, one then determines how many elements of the other sample precede it in the ascending order. One then adds together these figures for each sample, and the U statistic is the lower of the two sums (one for each sample). For example, consider the transaction price RMSE comparison between the no communication and communication treatments (column 1 of Table 2). The four lowest figures are in the no communication treatment, so four zeros are in the U summation. The highest RMSE in the no communication treatment (278) is greater than one RMSE figure in the communication treatment (154). Therefore the U statistic is $0 + 0 + 0 + 0 + 1 = 1$ [Neave (1988)].

are equal in these two treatments (U ranges between 8 and 10). Likewise, the spread measures shown in columns 3–5 are not significantly different in these two treatments (U ranges between 4 and 12). From this I conclude that prohibiting dealer communication or allowing trader limit orders equally improve these measures of market performance.

These "policy remedies" have different impacts on dealer profits, however. Table 4 summarizes the per-period trading profits earned by the dealers, informed traders, and uninformed traders in each session. By design an inverse relationship exists between dealer and trader trading profits, because trading in this environment is a zero sum game.[15] In contrast to the statistically indistinguishable informational efficiency and spread performance of the no communication and limit order treatments, in these two treatments dealer profits are statistically and economically different. Dealer profits are always negative in the limit order sessions, while dealer profits were positive in all no communication sessions except session 307.[16,17] This difference across treatments is statistically significant; the Mann–Whitney U = 1 based on all periods, and U = 2 based on only the final five periods. Also, compared to the treatment without dealer communication opportunities, dealer profits were significantly higher in the communicating dealer treatment without limit orders (U = 1 for all periods, and U = 0 for the final five periods).

Trader profits also differ significantly across treatments. Informed traders benefit most from the introduction of limit orders, earning significantly more than informed traders with communication and no limit orders (U = 0) and significantly more than informed traders with no dealer communication opportunities (U = 1 for all periods and U = 2 for the final five periods).

[15] Recall that both dealers and traders receive an asset endowment at the beginning of each period, so their gross profits differ from these trading profits. Although this is a zero sum game, the rows in Table 4 usually fail to sum to zero because of fines paid by uninformed traders when they failed to buy or sell sufficient assets to meet their required position. These fines are shown in column 4 of Table 4, and they occurred because of simple accounting mistakes (especially in the first several trading periods) or because of deliberate demand or supply withholding due to wide spreads. Overall, traders could have incurred up to about 1400 fines (17 sessions*11 periods*3 uninformed traders*2.5 average trades to meet the required position), and they incurred 56. There was no evidence that traders misunderstood the trading rules. Note that fines were most common in the sessions with dealer communication.

[16] One dealer in session 3L15 was confused and repeatedly lost money on his trades. He was the only subject in this study who misunderstood the market incentives. This one dealer caused most of the substantially negative dealer trading profit for this session.

[17] The negative dealer trading profits in the limit order treatment cannot be an equilibrium phenomenon, and the late dealer profits shown in parentheses in Table 4 indicate that these trading losses declined toward the end of several sessions in this treatment. The differences are more pronounced in the earliest and latest periods: over all five sessions in the limit order treatment, per-period dealer trading profits were −867 francs in the first three periods, but were only −297 in the final three trading periods. Nevertheless, results reported in Lamoureux and Schnitzlein (1997a) suggest that dealer losses may not be extinguished even with extensive experience. Due to slow learning in a fragmented (search) market, Lamoureux and Schnitzlein find that dealer losses persist into the fifth session of experience. But learning in the present environment is presumably faster because dealers can observe other dealers' trades (and losses), unlike in Lamoureux and Schnitzlein's fragmented market. In Flood et al. (1999), three of the seven market makers earned negative trading profits, even though all were professional market makers.

Table 4
Per-Period Realized Trading Profits for Dealers, Informed Traders and Uninformed Traders, by Session

Session	Aggregate dealer profit (1)	Aggregate informed trader profit (2)	Aggregate uninformed trader profit (3)	Number of fines paid by uninformed traders (4)
Three dealers, no communication permitted				
Session 301	387 (477)	53 (127)	−531 (−604)	1 (0)
Session 305x	1842 (1460)	−37 (10)	−1987 (−1670)	2 (1)
Session 307	−89 (−274)	582 (338)	−675 (−64)	2 (0)
Session 309	636 (526)	168 (190)	−1168 (−716)	4 (0)
Session 313x	459 (81)	80 (284)	−540 (−365)	0 (0)
Three dealers, nonmarket communication permitted				
Session 3c03	2167 (2821)	92 (142)	−2622 (−3563)	4 (3)
Session 3c06x	3024 (2443)	0 (0)	−3478 (−2643)	5 (1)
Session 3c08x	1914 (1772)	99 (6)	−2558 (−2378)	6 (3)
Session 3c10	1214 (1900)	22 (38)	−1238 (−1938)	0 (0)
Session 3c12x	3023 (2626)	0 (0)	−3841 (−3606)	9 (5)
Three dealers, nonmarket communication and limit orders permitted				
Session 3L15	−1815 (−841)	1567 (833)	−480 (−392)	8 (2)
Session 3L17	−425 (−67)	688 (228)	−536 (−161)	3 (0)
Session 3L18x	−767 (−409)	1113 (670)	−437 (−261)	1 (0)
Session 3L19x	−607 (−977)	727 (1067)	−392 (−90)	3 (0)
Session 3L20x	−72 (−199)	323 (381)	−615 (−462)	4 (1)
No dealers (double auction)				
Session 014x	—	388 (426)	−660 (−626)	3 (1)
Session 016	—	101 (78)	−192 (−78)	1 (0)

This table presents the net trading profits per period for each class of subjects, in experimental francs. Gross profits exceed this level due to the initial endowment of the asset each period (four units for traders, two units for dealers). All profit calculations are adjusted to reflect subject fines for failure to meet a required position. A calculation based only on the final five periods in each session is shown in parentheses.

Informed trader earnings are not significantly different between the communicating dealers treatment and the no communication treatment for all periods (U = 9), but these traders earn significantly more in the no communication treatment during the final five periods (U = 3). Uninformed traders lose less compared to the communicating dealers treatment, when either limit orders are permitted or when communication is prohibited (U ranges between 0 and 1). Finally, uninformed trader earnings are significantly higher in the limit order than the no communication treatment over all periods (U = 3), but not during the final five periods (U = 7).

3.5 The evolution of spreads and informational efficiency

Models of adverse selection risk such as the Glosten and Milgrom (1985) model have the qualitative implication that dealer uncertainty declines across transactions except when uninformed trades occur on the "wrong" side of the market (e.g., an unprofitable liquidity buy order when the asset value is low). Consequently, the RMSE of the spread midpoint and spreads should tend to fall over time.

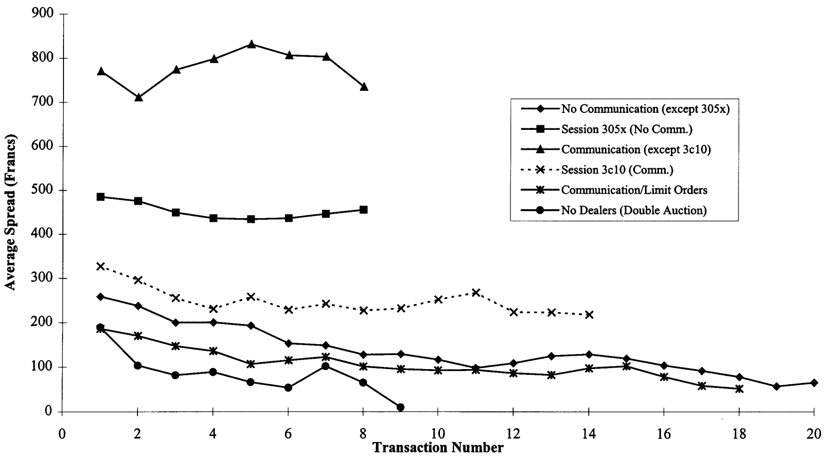

Figure 1
Average spread (in francs), by transaction.

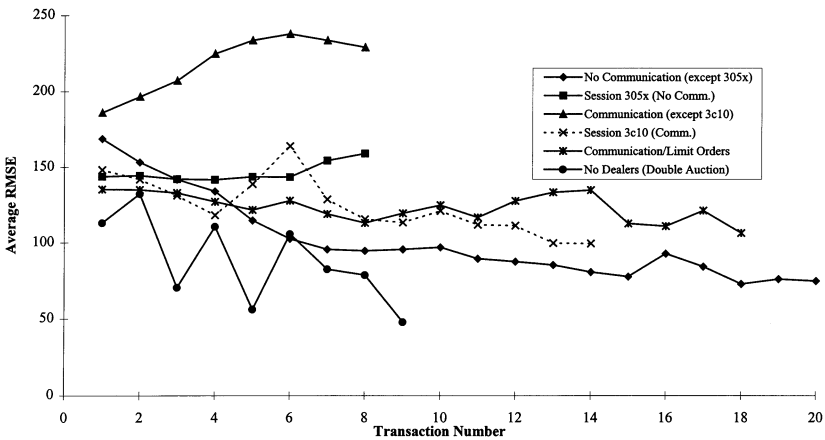

Figure 2
Average root mean squared pricing error (actual value from spread midpoint), by transaction.

Before turning to regression analysis to study these dynamic properties in the data, consider the summary of spreads, pricing error, and trading activity with a period shown in Figures 1–4. Figure 1 presents the average spread and Figure 2 presents the average RMSE across transactions after pooling over all periods in each treatment. The tacitly collusive session 305x and the competitive dealer session 3c10 in the communication treatment are not pooled with the other four sessions in their respective treatments, because the results shown earlier suggest that behavior in these two sessions is qualitatively different than other sessions in their treatment. The lines differ in

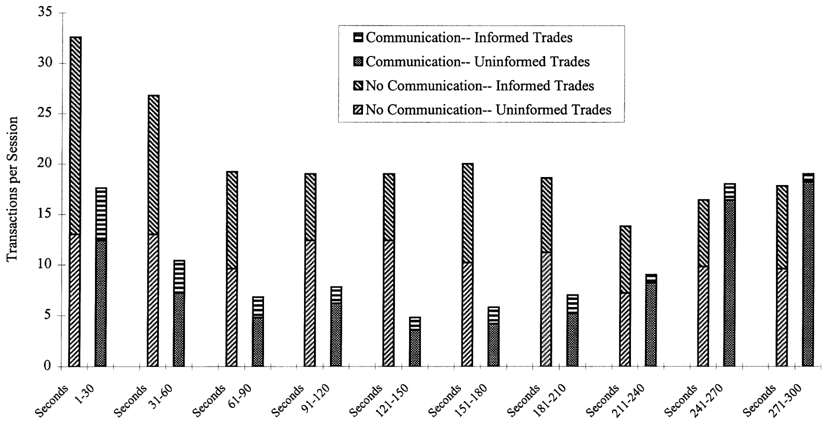

Figure 3
Trading volume and trade type, by 30-second interval: communication and no communication treatments.

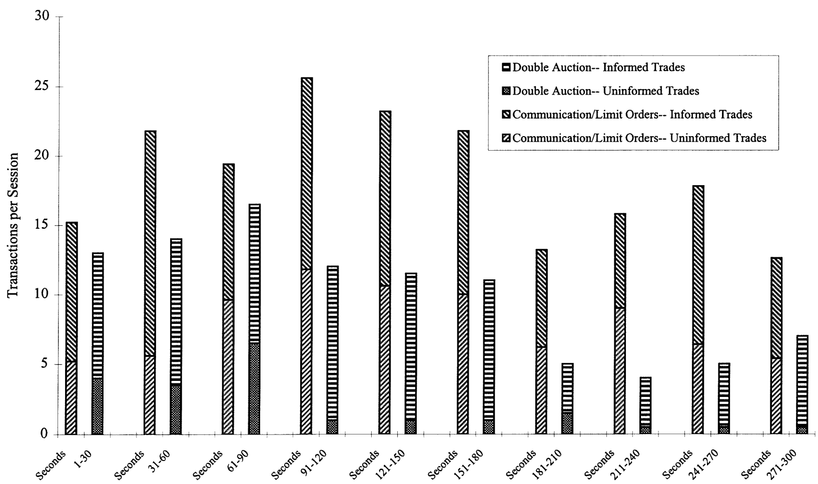

Figure 4
Trading volume and trade type, by 30-second interval: communication/limit order and double-auction treatments.

length because the average is shown only when at least one-half of the periods of that treatment have the indicated number of transactions. Figure 1 illustrates that spreads tend to fall across transactions, except for the four pooled communication sessions and (other than the initial transactions) the tacitly collusive session 305x. Likewise, Figure 2 illustrates that pricing errors usually decline except in the communication and tacitly collusive sessions.

Figures 3 and 4 present the trading volume in 30-second increments for each treatment, as well as the distribution of trades executed by informed traders and uninformed traders. In the communication and no communication treatments shown in Figure 3, the classification of trades into these categories is unambiguous, as the counterparty to a trader is necessarily a dealer. In the limit order and double-auction treatments shown in Figure 4, however, two traders can trade directly without a dealer intermediary. In this case I define a transaction as an "informed trade" if at least one transaction party is an informed trader.[18]

In the no communication treatment (Figure 3), trading volume tends to occur early in the period, and informed traders are initially more active than uninformed traders. Informed traders are also more active in the limit orders treatment (Figure 4), although here volume is concentrated more in the middle of the period. The modest early trading activity of uninformed traders and the continued trade by the uninformed even toward the end of the period in these treatments implies that the liquidity demand remains high even late in the trading period. This is consistent with the observation from Figure 1 that spreads decline across transactions in these treatments. Finally, notice that in the communication treatment informed traders rarely transact, and that the uninformed often concentrate their trades at the end of the period (Figure 3). This occurs because the wide spreads in this treatment rarely make trading profitable.[19]

The regression models in Tables 5–7 more formally examine the evolution of informational efficiency, spreads, and price quotes over time. Table 5 presents a model of the absolute price error based on the seconds left in the trading period, which begins the period at 300 and ends the period at 0:

$$\text{ABSERROR}_{\text{it}} = \beta_0 + \beta_1\text{SECLEFT}_{\text{it}} + \beta_2(\text{SECLEFT}_{\text{it}})^2 + \beta_3\text{DUM500}_{\text{it}} + \nu_{\text{it}}. \quad (1)$$

The quadratic term SECLEFT^2 is included because theory does not suggest that the relationship between pricing error and time is necessarily linear; higher order polynomial and log specifications generally performed worse,

[18] Recall that in the double-auction treatment there are no dealers. In this treatment, trades between two informed traders are rare, representing less than 6% of the transactions. Trades between an informed and an uninformed trader are most common, representing more than 74% of the transactions, and trades between two uninformed traders make up the remaining 20% of the transactions. In the limit orders treatment, direct trades between an informed and an uninformed trader represent only 24% of the transactions, and two-thirds of the transactions involve a dealer.

[19] One can also observe that most trades are informed in the double-auction treatment (Figure 4). This is in part an artifact of the way that an informed trade is defined. In this treatment, only when a trade involves two uninformed traders is it classified as uninformed, and as written in the previous note 18, this occurs in 20% of the transactions.

leading me to adopt this quadratic specification.[20,21] The model indicates that pricing error decreases over the entire period if β_1 and β_2 are positive, or within a particular range of time if $\beta_1 + 2\beta_2\text{SECLEFT} > 0$ for some SECLEFT between 0 and 300. The coefficient on DUM500 controls for the possibility that price errors differ with the asset value state.

The results in Table 5 for the no communication and limit order treatments roughly parallel the findings of Lamoureux and Schnitzlein (1997b) despite substantial differences in laboratory environments.[22] In this table (as well as in Tables 6 and 7) I present separately the estimate for the tacitly collusive session 305x and the competitive dealers session 3c10 in the communication treatment, rather than pooling them with the other four sessions in their respective treatments. The estimation results are similar when pooling all five sessions within each treatment, although the treatment differences are less pronounced. Panel A of this table (as well as Tables 6 and 7) presents estimates based on all periods, and panel B presents estimates based only on the final five periods.

The data reject the null hypothesis of $\beta_1 = \beta_2 = 0$ in all cases shown in panel A, and for all but the communicating dealers treatment in panel B. This indicates that the pricing error varies over time. The direction that the error changes differs across treatments, however. Pricing error declines over time in the four competitive no communication sessions and in the limit order treatment, and declines for about the first two-thirds of the period in the double-auction treatment. By contrast, the pricing error does not change or increases over time in the four communication sessions. The estimated β_3 exhibits no interpretable pattern. Finally, the intercept term β_0 differs across sessions in a manner similar to the RMSE calculation shown in column 2 of Table 2. This parameter can be interpreted roughly as an estimate of

[20] This model of the pricing error cannot distinguish between the speed of reaction and total reaction to information that dealers and traders observe; that is, a fast but incomplete reaction and a slower but more complete reaction could lead to similar estimates. Furthermore, except for the case in which dealers believe the high and low states are equally likely—such as the start of the period—the zero profit bid and ask are not placed symmetrically about the dealers' expected value of the asset [Glosten and Milgrom (1985)]. Therefore the expected squared deviation of the spread midpoint from the true value is the squared deviation of the spread midpoint from the expected value plus the remaining variance. This first term is relatively small in theory, as the squared deviation of the spread midpoint from the expected value never exceeds 100. For example, suppose that dealers believe that the likelihood of the high state is 0.7 and that the probability of encountering an informed trader is 0.4 (the proportion of informed traders in the population). Then the dealers' expected asset value is 410, and the zero expected profit bid and ask are 350 and 453.45 (midpoint = 401.7). The remaining variance of value (relative to expected value) is much larger than this spread midpoint squared deviation from expected value. I am grateful to Lawrence Glosten for bringing these issues to my attention.

[21] In the reported maximum likelihood estimates I correct for significant first-order serial correlation in the error term ν_{it} and allow for random session effects for all models presented in Tables 5–7. I also estimated the models shown in Tables 5–7 separately for each session, with qualitatively similar results.

[22] The TIME variable in Lamoureux and Schnitzlein (1997b) is clock time (counting up from zero), exactly opposite of my SECLEFT variable (which counts down to zero).

Table 5
Estimation of Pricing Error Model

Session	β_0	β_1	β_2	β_3	ρ	Wald test of $\beta_1 = \beta_2 = 0$	Observ.	R^2
Panel A: All periods								
Three dealers, no communication permitted								
4 sessions	28.3	0.067	0.0011**	−0.3	0.75**	413.4**	871	0.28
(except 305x)	(21.2)	(0.089)	(0.0003)	(3.8)	(0.02)			
Session 305x	134.1**	0.169	−0.0009†	33.2**	0.61**	12.5**	88	0.31
	(8.5)	(0.126)	(0.0004)	(8.0)	(0.09)			
Three dealers, nonmarket communication permitted								
4 sessions	275.0**	−0.713*	0.0015	−154.5**	0.84**	22.2**	357	0.30
(except 3c10)	(41.3)	(0.286)	(0.0010)	(12.0)	(0.03)			
Session 3c10	97.5**	0.091	0.0002	1.5	0.78**	28.0**	144	0.22
	(20.4)	(0.222)	(0.0007)	(13.7)	(0.05)			
Three dealers, nonmarket communication and limit orders permitted								
5 sessions	23.0	0.037	0.0010*	61.6**	0.85**	136.7**	886	0.23
	(23.4)	(0.125)	(0.0004)	(4.9)	(0.02)			
No dealers (double auction)								
2 sessions	61.9**	−0.894**	0.0041**	−12.2	0.52**	50.9**	118	0.33
	(23.0)	(0.284)	(0.0009)	(11.3)	(0.08)			
Panel B: Final five periods only								
Three dealers, no communication permitted								
4 sessions	32.1	−0.264*	0.0023**	3.9	0.69**	323.4**	385	0.41
(except 305x)	(22.5)	(0.114)	(0.0004)	(5.4)	(0.04)			
Session 305x	151.3**	0.037	−0.0007	41.1**	0.58**	45.4**	35	0.67
	(8.8)	(0.128)	(0.0005)	(10.9)	(0.14)			
Three dealers, nonmarket communication permitted								
4 sessions	250.5**	−0.056	−0.0005	−173.5**	0.89**	4.6†	157	0.31
(except 3c10)	(45.3)	(0.474)	(0.0016)	(22.2)	(0.04)			
Session 3c10	155.6**	−0.136	0.0006	−25.3	0.79**	1.6	75	0.03
	(30.0)	(0.299)	(0.0009)	(18.1)	(0.07)			
Three dealers, nonmarket communication and limit orders permitted								
5 sessions	2.7	0.344†	0.0002	55.6**	0.81**	70.4**	348	0.27
	(15.2)	(0.195)	(0.0006)	(7.5)	(0.03)			
No dealers (double auction)								
2 sessions	86.2*	−1.253**	0.0057**	−22.6	0.46**	41.8**	55	0.53
	(38.6)	(0.447)	(0.0013)	(18.5)	(0.12)			

Panel regression results of $\text{ABSERROR}_{it} = \beta_0 + \beta_1 \text{SECLEFT}_{it} + \beta_2(\text{SECLEFT}_{it})^2 + \beta_3 \text{DUM500}_{it} + \nu_{it}$, where i represents the session and t represents the transaction. ABSERROR denotes the absolute price error |Spread Midpoint − Asset Value|, SECLEFT denotes the seconds left in the trading period (which ranges between 300 and 0), and DUM500 is an indicator for the high asset value state. The error term ν_{it} follows the random effects model (with the session as the random effect) with an autocorrelated error structure: $\nu_{it} = u_i + e_{it}$; $e_{it} = \rho e_{it-1} + \eta_{it}$

Heteroskedasticity-consistent standard errors are shown in parentheses.

** Indicates significance at the 1% level (two-tailed).

* Indicates significance at the 5% level (two-tailed).

† Indicates significance at the 10% level (two-tailed).

the average pricing error at the end of the period, because the time trend SECLEFT runs down from 300 at the beginning of the period to 0 at the end of the period.

Consider next the model of the spread shown in Table 6:

Table 6
Estimation of Spread Trend Model

Session	β_0	β_1	β_2	β_3	β_4	ρ	Wald test of $\beta_1 = \beta_2 = 0$	Wald test of $\beta_3 = \beta_4 = 0$	Observ.	R^2
Panel A: All periods										
Three dealers, no communication permitted										
4 sessions	44.8	0.25†	0.0013**	−0.79	0.28	0.63**	210.9**	2.1	871	0.22
(except 305x)	(48.6)	(0.14)	(0.0004)	(4.20)	(0.41)	(0.02)				
Session 305x	433.5**	−0.41	0.0022†	0.57	0.48	0.83**	6.1*	2.6	88	0.18
	(34.6)	(0.36)	(0.0012)	(11.48)	(0.93)	(0.06)				
Three dealers, nonmarket communication permitted										
4 sessions	685.6**	−3.64**	0.0059*	144.91**	−11.67**	0.81**	130.2**	59.6**	357	0.35
(except 3c10)	(167.6)	(0.75)	(0.0026)	(20.45)	(2.02)	(0.02)				
Session 3c10	232.8**	0.04	0.0011	−4.65	−0.09	0.76**	4.1	0.9	144	0.13
	(37.7)	(0.52)	(0.0016)	(14.99)	(1.28)	(0.05)				
Three dealers, nonmarket communication and limit orders permitted										
5 sessions	49.5†	−0.28†	0.0034**	−1.53	−0.06	0.72**	262.2**	2.8	886	0.26
	(26.0)	(0.17)	(0.0005)	(3.94)	(0.34)	(0.02)				
No dealers (double auction)										
2 sessions	95.8**	−1.24**	0.0062**	−4.40	−0.03	0.38**	44.8**	1.7	118	0.31
	(34.3)	(0.48)	(0.0014)	(10.66)	(0.84)	(0.08)				
Panel B: Final five periods only										
Three dealers, no communication permitted										
4 sessions	52.9	−0.02	0.0028**	14.08*	1.20†	0.61**	168.7**	4.5	385	0.29
(except 305x)	(54.1)	(0.23)	(0.0007)	(6.67)	(0.62)	(0.03)				
Session 305x	362.4**	1.48**	0.0069**	34.81**	−3.79**	0.43**	38.0**	9.9	35	0.86
	(17.5)	(0.30)	(0.0010)	(12.19)	(1.20)	(0.15)				

Table 6 *continued*

Three dealers, nonmarket communication permitted										
4 sessions	552.7**	−2.25†	0.0011	228.97**	−19.50**	0.80**	48.9**	55.4**	157	0.33
(except 3c10)	(211.3)	(1.22)	(0.0042)	(36.04)	(4.11)	(0.03)				
Session 3c10	355.6**	−0.64	0.0027	−4.64	−0.55	0.67**	2.2	1.7	75	0.05
	(44.9)	(0.67)	(0.0021)	(19.56)	(1.71)	(0.09)				
Three dealers, nonmarket communication and limit orders permitted										
5 sessions	59.7*	−0.38	0.0041**	6.61	−1.19†	0.70**	101.9**	10.6**	348	0.31
	(25.7)	(0.31)	(0.0009)	(7.54)	(0.61)	(0.04)				
No dealers (double auction)										
2 sessions	134.2*	−1.95**	0.0081**	8.98	−0.97	0.42**	29.6**	1.2	55	0.40
	(59.2)	(0.73)	(0.0021)	(14.14)	(1.08)	(0.12)				

Panel regression results of $\text{SPREAD}_{\text{it}} = \beta_0 + \beta_1 \text{SECLEFT}_{\text{it}} + \beta_2 (\text{SECLEFT}_{\text{it}})^2 + \beta_3 \text{LIQ}_{\text{it}} + \beta_4 (\text{LIQ}_{\text{it}})^2 + \nu_{\text{it}}$, where i represents the session and t represents the transaction. SPREAD = (inside ask − inside bid) is the spread in francs on each transaction, SECLEFT denotes the seconds left in the trading period (which ranges between 300 and 0), and LIQ denotes the absolute liquidity demand and supply in aggregate remaining in the period for the uniformed traders to satisfy (it ranges between 12 and 0). The error term ν_{it} follows the random effects model (with the session as the random effect) with an autocorrelated error structure: $\nu_{\text{it}} = u_{\text{i}} + e_{\text{it}}$; $e_{\text{it}} = \rho e_{\text{it}-1} + \eta_{\text{it}}$

Heteroskedasticity-consistent standard errors are shown in parentheses.

** Indicates significance at the 1% level (two-tailed).

* Indicates significance at the 5% level (two-tailed).

† Indicates significance at the 10% level (two-tailed).

$$\mathrm{SPREAD}_{\mathrm{it}} = \beta_0 + \beta_1 \mathrm{SECLEFT}_{\mathrm{it}} + \beta_2 (\mathrm{SECLEFT}_{\mathrm{it}})^2 + \beta_3 \mathrm{LIQ}_{\mathrm{it}} + \beta_4 (\mathrm{LIQ}_{\mathrm{it}})^2 + \nu_{\mathrm{it}}. \quad (2)$$

The intercept coefficient β_0 represents an estimate of the spread at the close, because at the close both SECLEFT and LIQ are zero. As in the previous model, I include quadratic terms because theory does not suggest only a linear relationship between the spread and these explanatory variables.[23] If spreads decrease through the period, this would imply positive β_1 and β_2 estimates, or at least $\beta_1 + 2\beta_2\mathrm{SECLEFT} > 0$ for some SECLEFT between 0 and 300. I include the absolute aggregate liquidity demand and supply (LIQ) in this model because theory suggests a relationship between the remaining liquidity demand and the spread. Theoretical explanations exist for both a negative and a positive relationship, however. On the one hand, increased demand for liquidity trades makes the adverse selection problem less severe, reducing spreads. On the other hand, increased liquidity demand could decrease the demand to trade elasticity, providing an opportunity for greater dealer profits through wider spreads. It is reasonable to expect that the second effect is most important late in the trading period, as the deadline for liquidity traders' order execution approaches. The second effect may also be more important in collusive session if colluding dealers are able to commit to wider spreads and take advantage of increased liquidity demand.

The results shown in Table 6 indicate that the data reject the null hypothesis $\beta_1 = \beta_2 = 0$ in all cases except session 3c10. Spreads generally exhibited a downward trend in the no communication and limit order treatments, and a downward trend in the double-auction treatment until the final minute or two of the period. In the treatment with dealer communication opportunities, spreads exhibited an *increasing* trend except for the competitive dealer session 3c10.

The coefficients on the liquidity demand variables are usually jointly insignificant; the hypothesis $\beta_3 = \beta_4 = 0$ is rejected only for the communicating dealers treatment and in the limit order treatment for the final five periods only (shown in panel B). In the communicating dealers treatment the estimates imply that spreads fall as the absolute liquidity demand falls below six units. The β_3 and β_4 estimates for the final five periods of the limit order treatment (panel B) imply an increasing spread as absolute liquidity demand falls over most of the range of this variable. Although these results are contradictory, they are consistent with the conjecture presented above that increased liquidity demand is associated with wider spreads mainly in collusive sessions. In any case, the estimates shown in Table 6 indicate that overall, the changing elasticity of liquidity demand to trade does not appear to exert a strong influence on the spread. This may be because dealers do not

[23] As before, higher-order terms generally proved to be insignificant and a log specification fit poorly.

observe the liquidity demand directly; dealers only know the probability distribution underlying the liquidity demand, and they cannot be certain which trades liquidity traders execute. This suggests an errors in variables problem that biases these coefficients toward zero.

Finally, consider the simple model of bid and ask revisions in response to buy and sell orders shown in Table 7, following Lamoureux and Schnitzlein (1997b):

$$\Delta MP_{\text{it}} = \beta_0 + \beta_1 Q_{it-1} + \beta_2 NetQ_{it-1} + \nu_{\text{it}}. \qquad (3)$$

ΔMP_{it} is the change in the spread midpoint between transaction t and transaction $t-1$ (i.e., $\text{midpoint}_t - \text{midpoint}_{t-1}$). In models such as Glosten and Milgrom (1985), dealers update their expectations of the asset value (and therefore revise quotes) based on the order flow. This implies $\beta_1 > 0$ because a buy order (coded as $Q_{t-1} = 1$) should increase the spread midpoint and a sell order (coded as $Q_{t-1} = -1$) should decrease the spread midpoint. $NetQ_{\text{it}-1}$ is the net order imbalance throughout the period preceding the quote revision, so the β_2 coefficient permits information provided by earlier orders in the period to have a cumulative effect, beyond the impact reflected immediately in the subsequent quote revision.

The estimated β_1's in Table 7 are positive in all cases except in the no dealer (double-auction) treatment, where β_1 is not significantly different from zero. The cumulative order imbalance coefficient β_2 is at times significantly greater than zero, particularly when β_1 is not significantly different from zero. The key result here is that quotes are sensitive to the order flow in all treatments, including those in which dealers collude.[24]

4. Conclusion

This study finds that in a relatively simple laboratory environment, markets with as few as three dealers were sufficiently competitive to generate high informational efficiency even when the subjects who were informed of the asset value could not post limit orders. This is an important conclusion because, a priori, the privileged position of dealers and restrictions imposed on the trader message space in this dealer market could limit price competition and informational efficiency. Market performance in this treatment was comparable to performance in continuous double-auction sessions without dealer intermediaries and in a treatment with trader limit order privileges.

[24] The response to the order flow may vary with the amount of time left in the period; for example, early transactions might generate more information, which in this model could be reflected in higher estimated coefficients β_1 and β_2. I estimated a set of additional (unreported) models after excluding the transactions in the second half of each period that support this conjecture. In about two-thirds of the estimates the β_1 and β_2 coefficients increase when they are significantly different from zero. Due to the small sample size, however, the change in the β_1 and β_2 estimates is not statistically significant compared to estimates from the full sample of transactions.

Table 7
Estimation of Price Adjustment Model

Session	β_0	β_1	β_2	ρ	Observ.	R^2
Panel A: All periods						
Three dealers, no communication permitted						
4 sessions	0.15	16.3**	0.37	−0.16**	814	0.13
(except 305x)	(4.07)	(1.6)	(0.30)	(0.03)		
Session 305x	0.34	1.6	2.28*	−0.02	77	0.11
	(2.37)	(2.1)	(0.99)	(0.11)		
Three dealers, nonmarket communication permitted						
4 sessions	6.96*	13.7**	0.40	−0.15**	306	0.06
(except 3c10)	(3.48)	(3.9)	(1.18)	(0.06)		
Session 3c10	1.83	8.7**	−0.63	−0.10	129	0.06
	(3.26)	(3.4)	(0.91)	(0.09)		
Three dealers, nonmarket communication and limit orders permitted						
5 Sessions	−1.65	9.2**	−0.02	−0.09**	833	0.04
	(1.73)	(1.6)	(0.27)	(0.03)		
No dealers (double auction)						
2 sessions	5.27	−3.0	4.61	−0.28*	62	0.04
	(10.38)	(9.3)	(2.91)	(0.12)		
Panel B: Final five periods only						
Three dealers, no communication permitted						
4 sessions	−4.86	16.2**	1.06*	−0.10†	363	0.15
(except 305x)	(4.09)	(2.3)	(0.53)	(0.05)		
Session 305x	−2.28	2.7	3.33*	0.46**	30	0.21
	(4.36)	(2.5)	(1.60)	(0.17)		
Three dealers, nonmarket communication permitted						
4 sessions	11.79	14.5*	1.72	0.04	134	0.06
(except 3c10)	(7.48)	(5.7)	(1.75)	(0.09)		
Session 3c10	6.35†	10.8**	−1.95†	−0.17	70	0.10
	(3.84)	(3.8)	(1.12)	(0.12)		
Three dealers, nonmarket communication and limit orders permitted						
5 sessions	1.09	6.2*	−0.16	−0.15**	329	0.01
	(2.94)	(2.9)	(0.53)	(0.05)		
No dealers (double auction)						
2 sessions	11.75	−2.0	12.35*	−0.23	28	0.09
	(30.12)	(15.5)	(6.19)	(0.18)		

Panel regression results of $\Delta MP_{\text{it}} = \beta_0 + \beta_1 Q_{it-1} + \beta_2 NetQ_{it-1} + \nu_{\text{it}}$, where i represents the session and t represents the transaction. ΔMP_{it} is the change in the spread midpoint between transaction t and transaction $t-1$ (i.e., $\text{midpoint}_t - \text{midpoint}_{t-1}$), $Q_{\text{it}-1}$ is the order preceding this quote revision (i.e., $Q_{\text{it}-1} = 1$ for a buy and $Q_{\text{it}-1} = -1$ for a sell), and $NetQ_{\text{it}-1}$ is the net order imbalance throughout the period preceding the quote revision. The error term ν_{it} follows the random effects model (with the session as the random effect) with an autocorrelated error structure: $\nu_{\text{it}} = u_{\text{i}} + e_{\text{it}}$; $e_{\text{it}} = \rho e_{\text{it}-1} + \eta_{\text{it}}$

Heteroskedasticity-consistent standard errors are shown in parentheses.

** Indicates significance at the 1% level (two-tailed).

* Indicates significance at the 5% level (two-tailed).

† Indicates significance at the 10% level (two-tailed).

These competitive dealer sessions without dealer communication opportunities also provide some evidence that is generally consistent with the qualitative implications of models of asset pricing when intermediaries face adverse selection.

The experiment also demonstrates that dealers had little difficulty identifying and implementing collusive agreements in this setting when they could communicate face-to-face between periods. Moreover, one group of dealers who had no such communication opportunities was successful in colluding tacitly. The successful collusion observed here might result from the screen-based trading system, which allows dealers to immediately detect "cheaters" who cut the (either tacitly or explicitly) agreed upon spread. Dealers on the Nasdaq also observe other dealers' quotes in nearly continuous time because of a screen-based trading system, but several observers [e.g., Furbush et al. (1995) and Grossman et al. (1995)] have emphasized that dealers can trade anonymously on Instinet and other systems. Furbush et al. (1995, p. 25) argue that "[t]he ability to negotiate transactions at prices other than at the quoted prices and through mechanisms that cannot easily by monitored by other market makers would make it difficult to detect deviations from any alleged price coordination." Future experiments can test this hypothesis by adding anonymous trading opportunities to the screen-based system employed in the present environment.[25]

One should be cautious when drawing policy conclusions from laboratory studies. Some features of the environment can have an impact on the results, and further laboratory study and empirical work based on field data is required to identify this sensitivity. For example, as noted earlier, future experiments should investigate the consequences of permitting communication between only subgroups of dealers to better reflect communication constraints in the field. Another obvious next step is to study more complex laboratory dealer market environments with more asset values. Nevertheless, accumulating laboratory evidence based on this study, as well as Lamoureux and Schnitzlein (1997b) and Bloomfield and O'Hara (1998) in substantially different environments, suggests that quote competition among only three market makers is sufficient to generate informationally efficient asset prices. The present study highlights the importance of antitrust enforcement to limit nonmarket communication between dealers, and shows that this enforcement substantially improves dealer competition. It also demonstrates that the introduction of trader limit orders results in informational efficiency and transaction costs (spreads) comparable to the setting with effective antitrust enforcement. In the sessions with trader limit orders, the results are broadly consistent with the limit order submission strategies proposed by Harris (1998).

Unfortunately, in this experiment limit orders have the undesirable side effect of negative dealer trading profits.[26] These dealer losses are clearly

[25] In Dutta and Madhavan (1997), however, the implicit dealer collusion does not depend on other dealers knowing the identity of the defector. In this context it is also worth noting that Lamoureux and Schnitzlein (1997a) find that dealers compete more aggressively when traders can circumvent the centralized dealer market by trading (relatively anonymously) in a decentralized search market.

[26] The present environment probably overstates the negative impact of limit orders on dealer profits for several reasons. For example, in the experiment dealers' exogenous information regarding the asset value is always inferior to the information available to a subset of traders; and in the experiment dealers cannot maintain a limit order book that could provide them with additional information regarding asset demand conditions that is not publicly available.

not consistent with equilibrium behavior, and these losses were often lower in later trading periods. Many in the securities industry have expressed the concern that the new order display rules could make market making less profitable, and that this could have a severe negative impact on immediacy and liquidity if the supply of market-making services falls significantly. This issue cannot be addressed in this experiment because the exogenous liquidity demand I employ substantially enhances liquidity—which is why the double-auction (no dealer) markets perform well. Nevertheless, in this laboratory environment, compared to effective antitrust enforcement, requiring the public display of trader limit orders appears to be a rather blunt policy instrument with potentially adverse side effects. Future work can allow dealers to enter and exit the market endogenously. This can assess more directly how new market regulation—such as requiring the public display of limit orders—impacts liquidity and other measures of performance. This future work can also investigate special advantages to market makers arising from their ability to monitor limit orders, as in Seppi (1997).

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