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# THE EFFECTS OF MARKET ORGANIZATION ON CONSPIRACIES IN RESTRAINT OF TRADE

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Mindful of the market structure–conduct–performance paradigm fundamental to industrial organization research, this paper uses laboratory experimental techniques to study the impact of conspiratorial opportunities on market performance. We compare 'posted-offer' markets where sellers (but not buyers) are allowed conspiratorial opportunities with observations from three control groups: (1) posted-offer markets without conspiratorial opportunities, (2) 'double-auction' markets with conspiratorial opportunities and (3) posted-offer markets with true single-seller monopolists. The basic conclusions generated by our experimental design are: (1) seller conspiracies in posted-offer markets tend to raise prices (but not profits) relative to similarly organized markets without conspiracies, (2) posted-offer conspiracies tend to generate higher prices (but not profits) than double-auction conspiracies, and (3) posted-offer monopolies tend to generate higher profits (but not prices) then posted-offer conspiracies.

## 1. Introduction

One of the fundamental paradigms of industrial organization research is the distinction between market structure, conduct and performance [see, for example, Scherer (1980)]. While this trichotomy might be used merely as a system of analytical organization, it can also take on the role of a theory of causality in which market structure determines conduct which determines performance. This latter structure–conduct determinism is evident in parts of the U.S. antitrust laws in which particular market structures or types of participant conduct are illegal *per se* regardless of any examination of the resulting market performance. A striking example of a *per se* prohibition is that which proscribes virtually any attempt to fix prices and/or quantities. As

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the Supreme Court ruled in the Socony–Vacuum Case,[1] 'this Court has consistently and without deviation adhered to the principle that price fixing agreements are unlawful *per se* under the Sherman Act...'.

In a recent paper, Isaac and Plott (1981) report on the results of laboratory market experiments designed to test the linkages between the structural opportunities for market conspiracy, the conduct of participants, and ultimate market performance. They report that some of the links implicit in *per se* prohibition of price fixing were indeed strong. The opportunity for conspiracy was typically followed by the discussion of that possibility among the market participants. These discussions were, in turn, typically followed by agreements to implement conspiracies which were, in turn, followed by at least some market conduct indicative of attempts to enact the conspiratorial designs.

The weakest link found by Isaac and Plott was between the initial attempts to implement the conspiracies and the ultimate market performance. The conspirators (who could not discuss side payments nor look to any 'government' to enforce the agreements) were often unable to translate their designs into cartel-like outcomes (price, quantity or profitability). The authors state that, while the markets appear to have behaved differently than in the control (non-conspiracy) experiments, there was no clear evidence of a successful conspiracy equilibrium away from the competitive price and quantity. Contributing factors were (1) the conspirators did not know (except through actual trading) the nature of the market parameters, and (2) the high frequency of apparent cheating on the conspiratorial agreements.

Isaac and Plott conclude their paper with a conjecture that the market exchange mechanism could be an important determinant of market conduct and performance when conspiracies exist. Specifically, they suggest that their use of oral double-auction trading rules might have magnified the tendency for individuals to cheat on the conspiracies and that their results might have been different if the markets had been organized using posted-offer trading rules.[2] Posted-offer trading, by eliminating the possibility of price reductions within a given trading period, might serve to increase the ability of the conspirators to obtain consistently favorable market prices. On the other hand, posted-offer trading eliminates the possibility of successful first-degree price discrimination, which could render conspiracies less effective in extracting exchange surplus. This occurs because in posted-offer markets sellers have no mechanism for insuring that buyers with the highest marginal

[1] *United States v. Socony–Vacuum Oil Co., Inc.*, 310 U.S. 150 (1940).

[2] In posted-offer markets (see section 2), each seller posts an offer price and quantity, then buyers, in a random sequence, make purchases given the 'take it or leave it' prices entered by sellers. In double-auction markets, the trading procedures parallel those used on the floors of major stock and commodity exchanges; both buyers and sellers have the right to enter and accept price quotes (see footnote 6).

values (highest demand steps) will be forced to buy at the highest posted prices. Since each buyer rationally chooses the lowest offer price, it is the shopping sequence of buyers (rather than the ranking of marginal valuations) that determines the matching of purchase prices and marginal valuations. In this paper we report the results of a series of experiments designed to address the conjecture that market organization can make a difference in the performance of markets with opportunities for conspiracy in restraint of trade.

The posted-offer market is not simply a contrived alternative to the double auction. Because sellers post offer prices and quantities and then watch as buyers 'shop' among the postings, it characterizes the price formation mechanics found in certain retail markets. The critical distinction, however, is not merely one of retail versus wholesale markets. The essence of the posted-offer market is that it arises when it is relatively costly (perhaps prohibitively so) for sellers to negotiate a separate price for each trade. The posting of a multiple-unit 'take-it-or-leave-it' offer price replaces unit by unit bargaining. Certainly, the relatively large transactions costs of individual haggling are evident in many (but not all) retail markets as well as in some wholesale markets.[3] There are other types of transactions costs which could generate a posted-offer type market. One example, explored by Hong and Plott (1981), is government regulation. In some circumstances, a firm which is otherwise free to set price may be required to post that price with a government (or quasi-government) agency. These posted prices might be regulated in that they can be changed only at statutory intervals or with prescribed notice.[4] Thus, a posted-offer market captures the flavor not only of many retail markets but also of certain types of intermediate or professional markets for goods and services.

This research reports the results of six experiments in which conspiratorial opportunities are allowed among the sellers in a posted-offer market. Observations from these experiments are compared with experimental observations from three 'control' conditions: (1) posted-offer markets using identical market parameters but lacking conspiratorial opportunities, (2) true

[3]For example, in some U.S. oilfields, large purchasers of crude oil will post bids to buy crude oil from the producers.

[4]On June 18, 1982, the U.S. Supreme Court ruled illegal as a form of price fixing a coordinated price-posting system of Arizona doctors. According to the Tucson *Citizen*, the case involved two medical 'foundations' in Arizona representing over 70% of the physicians in the Phoenix and Tucson areas. The foundations announced a fee schedule after consulting with the member physicians. There was no indication of profit sharing arrangements in this plan. Since the fee schedules were technically maximum prices, individual physicians apparently still had some freedom to deviate from the posted scheduled, although private insurers, according to the *Citizen*, 'had to agree to pay member physicians up to the maximum fees for treating insured patients' if they desired foundation approval for their plans. The lower courts and the Supreme Court dissenters argued for a 'rule of reason' approach to the question, while the Supreme Court majority embraced the *per se* prohibition against price fixing.

single-seller monopolies using the same posted-offer market parameters and (3) double-auction conspiracies using these same parameters (as a check of the robustness of the Isaac–Plott conclusions using our design).

Since our primary conjecture is that using the posted-offer institution will increase the frequency of cartel-like outcomes compared to the findings of Isaac and Plott, a failure to observe them would raise further serious questions about the validity of the antitrust structures based on structure–conduct–performance criteria. Suppose, however, that non-competitive outcomes are clearly observed. This would tend to support the traditional arguments that, since certain market structures or patterns of conduct foreshadow undesirable market performance, their occurrence can be a cause for action even without checking their 'reasonableness'. Yet, given the results of Isaac and Plott, this linkage would be much more subtle than previously thought.

The paper is organized as follows: section 2 briefly describes the computer operated posted-offer market, section 3 describes the experimental design and market parameters, section 4 reports the experimental results, section 5 summarizes our conclusions and suggests areas for further research.

## 2. PLATO posted-offer trading procedures

The posted-offer experiments reported below use the PLATO computerized trading mechanism described in detail by Ketcham, Smith and Williams (1984). This program allows buyers and sellers sitting at individual PLATO computer terminals to exchange an undefined homogeneous commodity for a maximum of 25 market 'days' or trading periods. The display screen for each buyer (seller) shows his/her private record sheet listing marginal valuations (costs) for a maximum of five units potentially purchased (sold) in each period. Sales are 'to order' in the sense that there are no penalties, or carry-over inventories, associated with untraded units. Consequently the assigned marginal valuations and costs induce the well-defined flow supply and demand conditions described in section 3 [see Smith (1982b) for a detailed discussion of induced valuation theory].

Each trading period begins with buyers being placed in a 'waiting loop' and sellers being requested to select an offer price by typing a price into the computer keyset. This offer is displayed privately on the seller's screen. The seller is then asked to select a corresponding quantity to be made available at that offer price. The maximum number of units a seller can offer corresponds to the number of the last unit whose cost is not greater than the offer price. The minimum number of units a seller can offer corresponds to the number of the first unit whose cost is not greater than the offer price. This procedure permits individual induced marginal costs to be declining, constant or increasing. Since it is time and effort costly for a seller to calculate the profit that any given offer may provide, PLATO always informs

the seller of the potential profit (loss) if all offered units are sold. When a seller is satisfied with the selected price and quantity, he/she taps a touch sensitive 'offer box' displayed on the screen. This action places, irrevocably, that seller's offer into the market. Before touching the 'offer box' the seller may change the price and/or quantity as many times as desired.

The viewing screen of each buyer displays one touch sensitive 'price box' for each seller in the market. After all sellers have entered their offers, each seller's offer price is posted in one of these boxes. PLATO then places the sellers in a 'waiting loop', randomly orders the buyers in a 'shopping' sequence, and then informs the first buyer that he/she may now begin purchasing the good. To purchase a unit from a particular seller, the buyer touches the box displaying that seller's offer price and then depresses a 'confirm' key on the keyset. Repeating this sequence causes a second unit to be purchased, and so on. Upon confirming the acceptance of a seller's offer, the seller is informed of this fact by PLATO and the contract information is automatically logged in both the buyer's and seller's record sheet. A buyer is allowed to purchase up to his/her buying capacity from any seller or sellers. A buyer cannot, however, purchase a unit whose price is greater than the unit's marginal valuation, and cannot buy from a seller who has sold all of the units offered. When a seller's last available unit is sold the price appearing in the buyer's box for that seller is replaced with the message 'out of stock' on the buyer's screen. After the first buyer has finished making purchases, the next buyer in random order may begin purchasing, and so on. The period ends when the last buyer completes this buying procedure.

It is important to emphasize that buyers and sellers have only limited information. All unit values (costs) assigned to individual buyers (sellers) are strictly private, known only to the subject (and the experimenter). Each buyer sees all of the seller's offer prices but not the quantities available at these prices. In the experiments reported below sellers saw the prices posted by other sellers, but the PLATO computer program allows this information to be suppressed.

The opportunities for conspiracy were organized as follows. In all of the multiple seller experiments, the sellers were told that, because of the multisite nature of the experiment, it was complicated to use the computer program to move from one trading period to another, and that this process would be facilitated if the sellers would leave their terminals for a few moments and wait in chairs adjacent to the terminals.[5] While seated in these chairs,

[5]This was not necessarily a ruse. We had a very specific reason for wanting to control the transition from one period to another. In the PLATO posted-offer program, the participants choose to move to the next trading period by pressing the NEXT key. Having pressed NEXT, subjects see a message that the market is 'waiting for $N$ traders to get ready' before moving to the next period where $N$ decrements as more press NEXT. The next period begins when all have pressed NEXT. If we had not controlled the transition, two undesirable events could have occurred. First, in the experiments with no conspiracy followed by conspiracy, buyers might very well have perceived that the period-to-period transition took only a few seconds in periods 1–10,

participants were given a printed summary of the previously posted offer prices of each seller and the quantity actually sold by each seller. (Providing this information was an attempt to keep participant information parallel with the Isaac-Plott experiments in which all trades were recorded on a blackboard.)

During non-conspiracy experiments, participants waited silently for about two minutes and then returned to their terminals. During conspiracy experiments, the participants were informed of the opportunity for discussion in language virtually identical to that of Isaac and Plott. Specifically, the participants were told that, while waiting in the chairs, they were free to discuss all aspects of the market except that (1) they could not discuss side-payments or physical threats, and (2) they could not discuss quantitative information about their payoff tables. The conspirators had a maximum of four minutes for discussion. Buyers (located in Indiana) were given no information about sellers' collusive opportunities.

## 3. Experimental design and market parameters

Market participants were volunteers recruited from the undergraduate student populations at Indiana University and the University of Arizona. All subjects were 'experienced' in the sense that they had participated in a previous posted-offer experiment (using entirely different market parameters). The experiments were run 'multisite' with buyers (in Indiana) and sellers (in Arizona) interacting through the PLATO computer system based at the University of Illinois.[6] Upon arriving at the PLATO lab subjects were paid $3 for keeping their appointment and then assigned to an individual computer terminal. At the experiment's conclusion subjects were paid, in cash, their accumulated earnings from the experimental market.

Table 1 classifies trading periods in each of the 14 experiments that we report on as being characterized by one of the following experimental conditions:

(1) Posted-offer, no conspiracy — all subjects (4 buyers, 4 sellers) were isolated at their terminals both during and between trading periods. No verbal communication was allowed; market information was transmitted solely via the posted-offer mechanism.

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but up to four minutes thereafter. Second, in the conspiracy periods, buyers might have had an additional reason to assume the existence of a conspiracy if the repeated pattern was that the message 'waiting for 4' appeared on the screens in just a few seconds and then remained for up to four minutes. Therefore, we used a random number device to move some, but not all, of the seller terminals into the 'ready' mode across the waiting interval. Our concern was to standardize information flowing to the buyers. Coursey, Isaac, Smith (1984) offer evidence that buyers might strategically alter behavior based upon their perceptions of sellers' market power.

[6] The only exception to this rule was in the case of the monopoly experiments. Two of the experiments had the single seller in Indiana and the four buyers in Arizona.

Table 1
Experiment classification.

| | Experiment | Number of buyers, sellers | Final period | C.E. price |
|---|---|---|---|---|
| Posted-offer no seller conspiracy, periods 1–10; conspiracy, periods 11–20 | po66 | 4,4 | 19 | 2.20 |
| | po67 | 4,4 | 20 | 7.05 |
| Posted-offer seller conspiracy, all periods | po58 | 4,4 | 23 | 6.80 |
| | po59 | 4,4 | 15 | 6.80 |
| | po77 | 4,4 | 16 | 3.85 |
| | po83 | 4,4 | 15 | 5.75 |
| Double-auction seller conspiracy all periods | da64 | 4,4 | 10 | 6.80 |
| | da65 | 4,4 | 10 | 3.20 |
| | da69 | 4,4 | 10 | 5.10 |
| | da71 | 4,4 | 11 | 4.90 |
| Posted-offer monopoly, all periods | po92 | 4,1 | 25 | 5.75 |
| | po94 | 4,1 | 25 | 4.15 |
| | po102 | 4,1 | 20 | 5.75 |
| | po107 | 4,1 | 25 | 5.75 |

(2) Posted-offer, seller conspiracy — as discussed at the end of section 2, four sellers (in Arizona) were allowed to gather at a table away from their terminals for a four-minute verbal communication session between each trading period. Four buyers (in Indiana) were isolated and not informed that sellers were permitted to communicate. Sellers were not permitted to discuss side-payments or to make explicit mention of their individual unit costs during the communication sessions.

(3) Double-auction, seller conspiracy — conspiracy rules were the same as in posted-offer conspiracies, but using the PLATO double-auction mechanism.[7]

(4) Posted-offer, monopoly — a single seller was given a marginal cost array corresponding to the lowest 10 units in the supply array used in the non-monopolistic markets. Four buyers were isolated but were aware that there was a single seller.

From table 1 we note that four experiments were run using each of the double-auction and posted-offer seller conspiracy conditions, four experiments were run under the posted-offer monopoly condition, and two posted-offer experiments were run in which ten 'no conspiracy' trading

[7]The PLATO double-auction mechanism is described in detail by Williams (1980), and Smith and Williams (1983). In the interest of brevity, the specifics of computerized double-auction trading are not described in this paper.

periods were followed by a sequence of trading periods where sellers were allowed to conspire between periods.

Individual cost and valuation assignments and the resulting induced aggregate market supply and demand arrays are shown on the left of fig. 1. The competitive equilibrium (C.E.) quantity ($Q_0$) is seven units per period with C.E. prices defined over the ten cent interval centered on $P_0$ and bounded above (below) by the seventh step on the demand (supply) array. The market supply and demand arrays differed by an additive constant across experimental replications. The monopolist's profit maximizing price is sixty cents above $P_0$ ($P_m = P_0 + 0.60$) with three units exchanged ($Q_m = 3$). Buyers (sellers) earn the difference between unit valuation and price (price and unit cost) for each unit traded plus a five cent 'commission' to cover subjective transaction costs. Thus, buyers have a small inducement to trade a third unit at $P = P_m$. To equalize individual buyer earnings over the course of a posted-offer experiment, the four valuation sets (shown in fig. 1) were randomly reassigned or rotated among the buyers each trading period. Note that this design feature, (1) has no effect on the induced market demand across periods, (2) practically eliminates the possibility that, at $(P_m, Q_m)$, two buyers would earn zero total profit (net of commissions), and (3) would presumably have little or no effect on contract prices since the only strategic action available to buyers is under-revelation of demand.

At a C.E. allocation $(P_0, Q_0)$ total exchange surplus (exclusive of commissions) is \$6.00 per period split equally between buyers and sellers. At the sellers' joint profit maximizing allocation $(P_m, Q_m)$ producer surplus is \$4.35 and consumer surplus is \$0.75. Defining an index of allocative efficiency ($E$) as actual buyer plus seller earnings (exclusive of commissions) expressed as a percentage of the maximum possible group earnings (at a C.E.), we note that $E = 85$ at $(P_m, Q_m)$ if the three lowest marginal cost units trade.

Another criterion for evaluating market performance and, more specifically, the ability of monopolists and cartels to extract supracompetitive profit from the market is given by an 'index of monopoly effectiveness' defined as follows:

$$M = (\pi - \pi_c)/(\pi_m - \pi_c), \quad \text{where}$$

$\pi$ = sellers' realized total profits,
$\pi_c$ = theoretical total seller profits at C.E.,
$\pi_m$ = theoretical total seller profits at $P_m$.

Note that $M > 0$ holds if the seller(s) effectively raises profit above the C.E. level, with $M = 1$ corresponding to the theoretical single-price monopoly profits. If successful first-degree price discrimination is achieved, $M$ would be greater than 1. The use of this index of monopoly effectiveness to analyze the

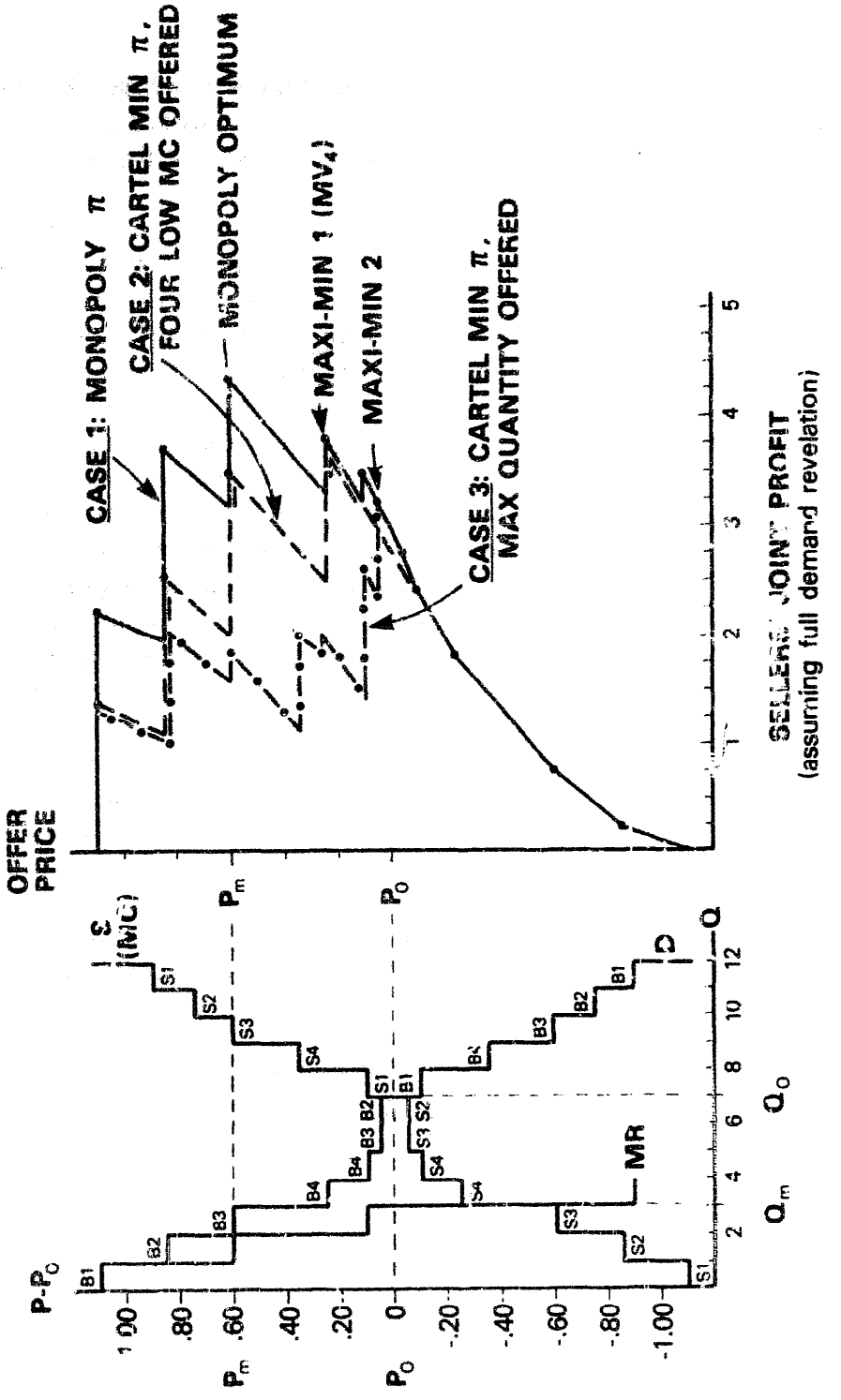


Fig. 1. Market parameters and profit polygons.

effects of seller conspiracies can paint a somewhat different picture than aggregate price and quantity data. A monopolist or cartel that sets price above $P_m$ will have a great impact on the market yet earn a smaller profit than at the C.E. This occurred in period 1 in po55, where sellers posted prices so high that no buyer could profitably purchase. A double-auction conspiracy with end-of-period price movements toward the C.E. may generate mean prices near the C.E. but large $M$ statistics if that price series resulted in successful price discrimination.

Before turning to the next section and the presentation of our experimental results, it is important to make note of some market design features that may be relevant to predicting outcomes in the posted-offer seller-conspiracy and monopoly experiments. First, since $Q_m=3$, a four-seller cartel fixing prices at $P_m$ would have at least one member being completely rationed out. It seems reasonable to conjecture that the highest offer price allowing all four sellers to trade one unit could have some drawing power especially since specific discussions of earnings redistribution schemes were prohibited. Second, for a given offer price above $P_0$, it is quite possible that a cartel's (group) profit will be less than a monopolist's (assuming full demand revelation by buyers in both cases). This is true by virtue of the fact that our monopolists must trade 'up' the marginal cost array (lowest cost units first) while our cartels (not being fully 'rationalized') might not trade up the market supply array (although each seller must trade up his/her individual marginal cost array). A simple example of this is if each seller offers one unit for sale at $P_m$ and buyers choose (unknowingly) not to purchase the lowest-cost unit supplied. Thus at a fixed price and assuming full demand revelation, cartel profits can be considered a random variable rather than being strictly determined as in the monopoly case. The random nature of cartel profits is illustrated in the right-hand portion of fig. 1 which displays three possible group profit polygons for sellers. These profit polygons are constructed assuming that all sellers quote a single offer price, buyers fully reveal demand at this price, and one of the following conditions holds:

*Case 1.* Each seller offers for sale all units for which price exceeds marginal cost and the *lowest-cost* units supplied to the market always trade first (as in the case of pure monopoly).

*Case 2.* Each seller offers for sale only his/her first (lowest-cost) unit and the *highest-cost* of these units supplied to the market always trade first.

*Case 3.* Each seller offers for sale all units for which price exceeds marginal cost and the *highest-cost* units supplied to the market always trade first (the highest-cost unit available to buyers at a given point in time is constrained by the fact that each seller must trade his/her units in sequence from low-cost to high-cost).

Note that cases 1 and 3 place upper and lower bounds, respectively, on cartel profits for a given offer price assuming full demand revelation.

Two important points can be made based on the profit information displayed in fig. 1. First, the discrete (integer quantity) supply and demand arrays generate discontinuous seller profit polygons; the 'saw-toothed' nature of these polygons could cause a simple sequential price-search process (by a group-profit maximizing cartel or monopolist) to converge to any of the top four steps on the demand array, depending on the choice of the starting price and the size of the price increment/decrement. Second, the profit polygon labelled case 2 has a maximum value at a price corresponding to the *fourth* step on the demand curve ($P_m - 0.35$). For a cartel strategy based on each seller offering a single unit for sale at a fixed price, ($P_m - 0.35$) can be thought of as a maxi-min theoretic equilibrium; it maximizes the minimum possible cartel profit for a given offer price. If sellers offer for sale all units for which the offer price exceeds marginal cost, then the maximum value of the (case 3) profit polygon ($P_0 + 0.05$, the upper bound of the C.E. range) becomes the maxi-min price.

Taking all of these considerations into account, one might conjecture that a monopolist, and quite certainly a cartel, would have a difficult time finding and then maintaining prices at $P_m$. In particular, the fourth demand step appears to have potential drawing power for cartels quoting a single offer price.

## 4. Experimental results[8]

### *4.1. Double-auction, seller-conspiracy experiments*

Figs. 2–5 display sequential contract prices and descriptive statistics for da64, da65, da69 and da71. These four double-auction control experiments were conducted to see if the basic seller-conspiracy results of Isaac–Plott could be replicated using our market parameters, experimental procedures and the PLATO computerized double-auction mechanism with multisite trading.

The data appear to be generally consistent with the Isaac–Plott results. In three of the four markets (da64, da65, da71) most prices were above the C.E. range but well below $P_m$. The opportunity for seller conspiracies and attempts to implement conspiracies appear to have caused these markets to deviate from the well-documented, robust tendency for C.E. convergence in

[8]As in Isaac–Plott, the 'early' links between the opportunity for conspiracy, discussions of conspiracy, plans for conspiracy and attempts to implement the conspiracy were robust. Therefore, we will focus our discussion on the 'final' link, that to market performance.

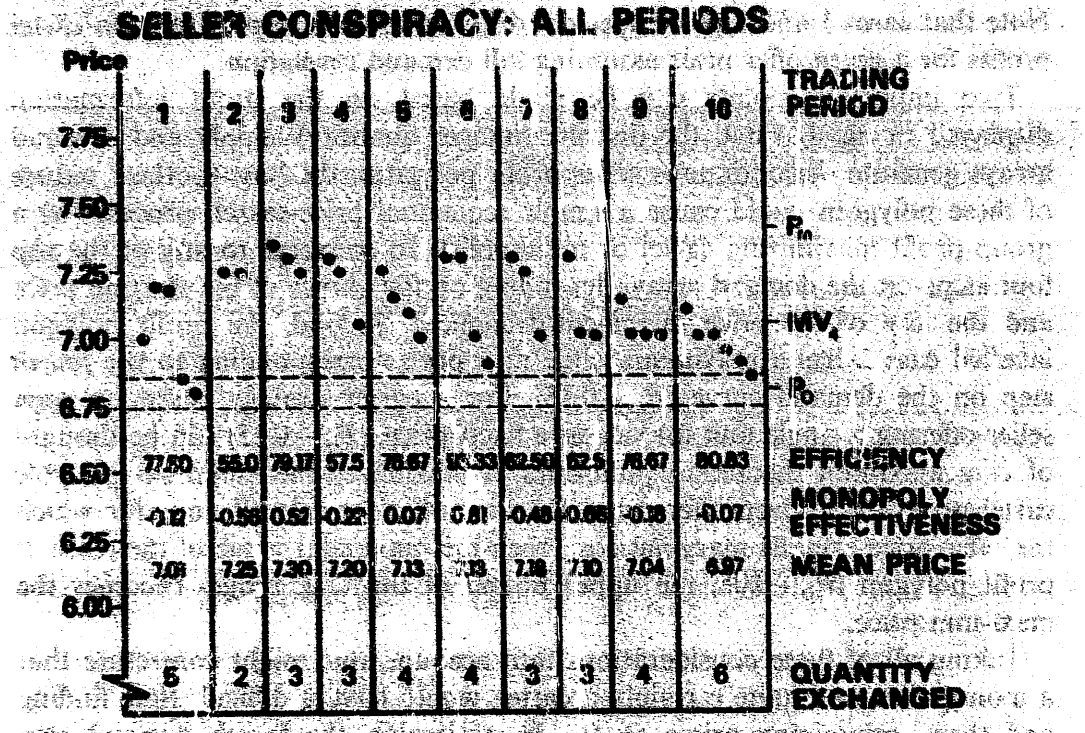


Fig. 2. Experiment da64.

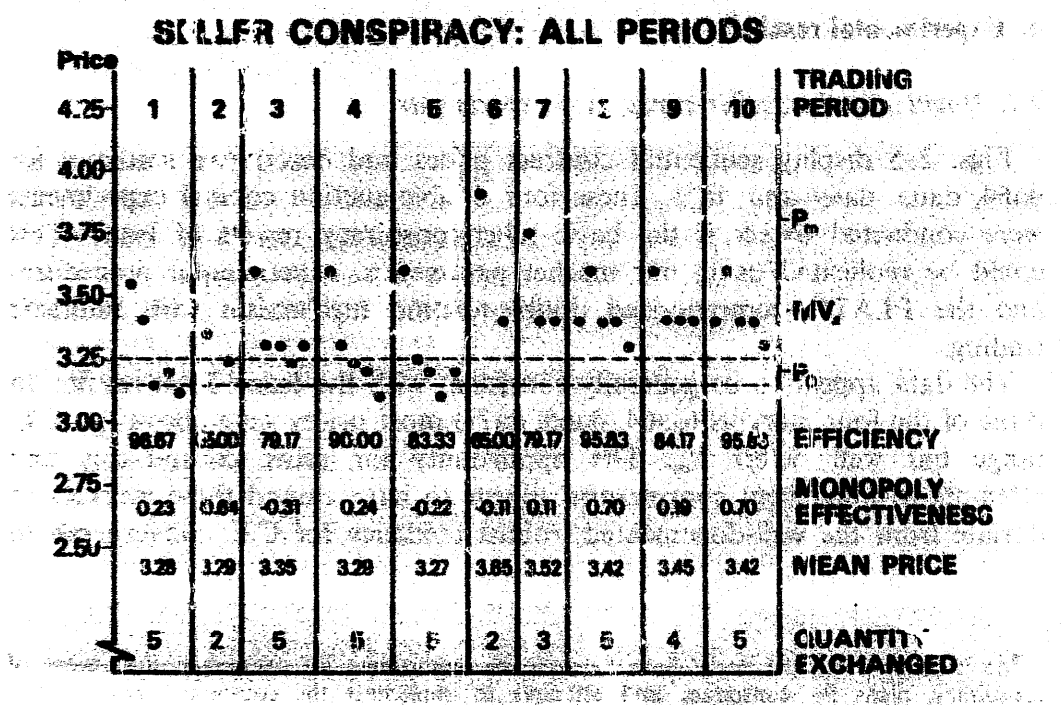


Fig. 3. Experiment da65.

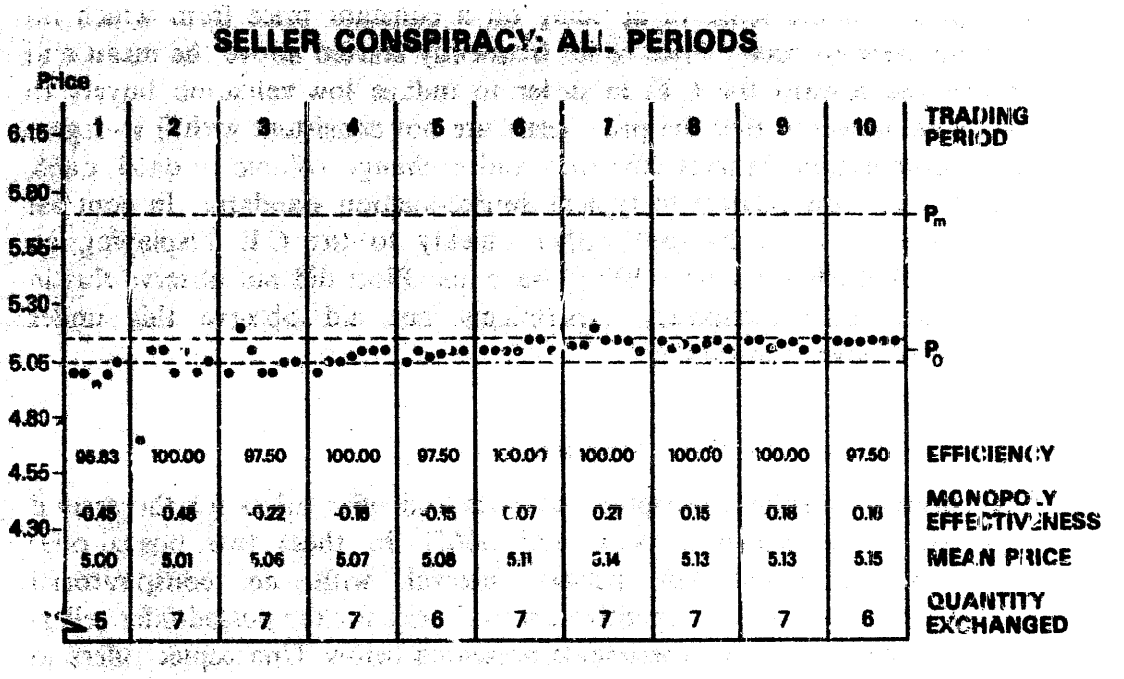


Fig. 4. Experiment da69.

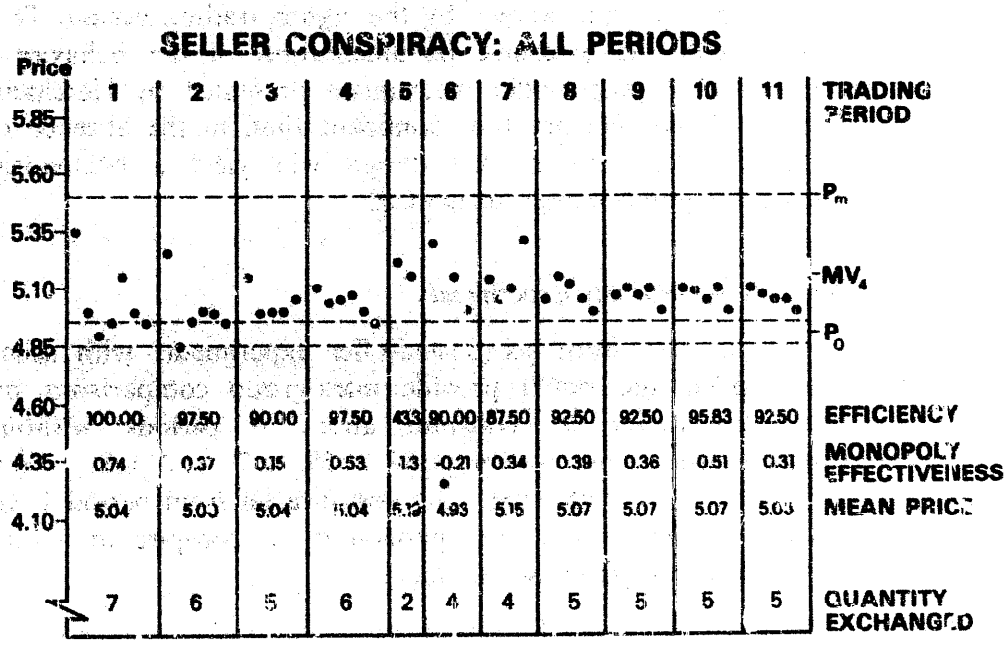


Fig. 5. Experiment da71.

double-auction markets.[9] Note, however, that none of the trading periods had a zero variance price series. The conspiracies were not successful in 'fixing prices' in the sense of agreeing on a constant price from which no deviations were to occur. Price series frequently started above the mean and then decayed toward the C.E. in order to induce low-valuation buyers to trade. Note, however, that the price series are not consistent with first-degree price discrimination. Market efficiency and exchange volume in da64, da65, and da71 were low relative to typical double-auction standards. In contrast to this result, da69 converged rather quickly to the C.E. displaying an average market efficiency near 100%. Isaac and Plott did not observe this in any of their seller-conspiracy experiments but did observe this under conditions of buyer conspiracy.

### 4.2. Posted-offer, no-conspiracy experiments

Figs. 6 and 7 display descriptive statistics and offer prices for the second set of control experiments (po66 and po67). In these two posted-offer experiments an initial ten period interval with no conspiratorial opportunities served as a competitive control series for the posted-offer seller-conspiracy and monopoly experiments presented below. Unaccepted offers to sell are plotted as open circles and accepted offers (contract prices) are plotted as solid dots. The mean contract price in both experiments converged to the range of C.E. prices (from above) by the eighth trading period. This result is consistent with the extensive documentation of the behavioral properties of the PLATO posted-offer mechanism presented by Ketcham, Smith and Williams (1984). We are thus confident that, in the absence of conspiratorial opportunities, our market design will yield a behavioral equilibrium which corresponds closely to the C.E.

### 4.3. Posted-offer, seller-conspiracy experiments

We next report the data from six posted-offer experiments with seller conspiracies. Two (po66 and po67) provide intra-group comparisons by introducing the opportunity to conspire after 10 periods without conspiratorial opportunities. Four others (po55, po59, po77 and po83) allow for intergroup comparisons by allowing seller conspiracies from period 1. In all six experiments, the links from the opportunity to conspire to actual attempts to implement a conspiracy were robust.

[9] See Smith (1982b), propositions 4 and 5, for additional discussion and documentation of double-auction convergence properties. One ('inexperienced subject') double-auction experiment without conspiratorial opportunities was conducted using the market design shown in fig. 1. The results were consistent with previous double-auction experiments; the mean price was within 10 cents of $P_0$ after one trading period (averaging $P_0 + 0.05$ over all periods), volume was 6 or 7 units in all periods, and efficiency averaged 91 percent over all periods (somewhat low by double-auction standards).

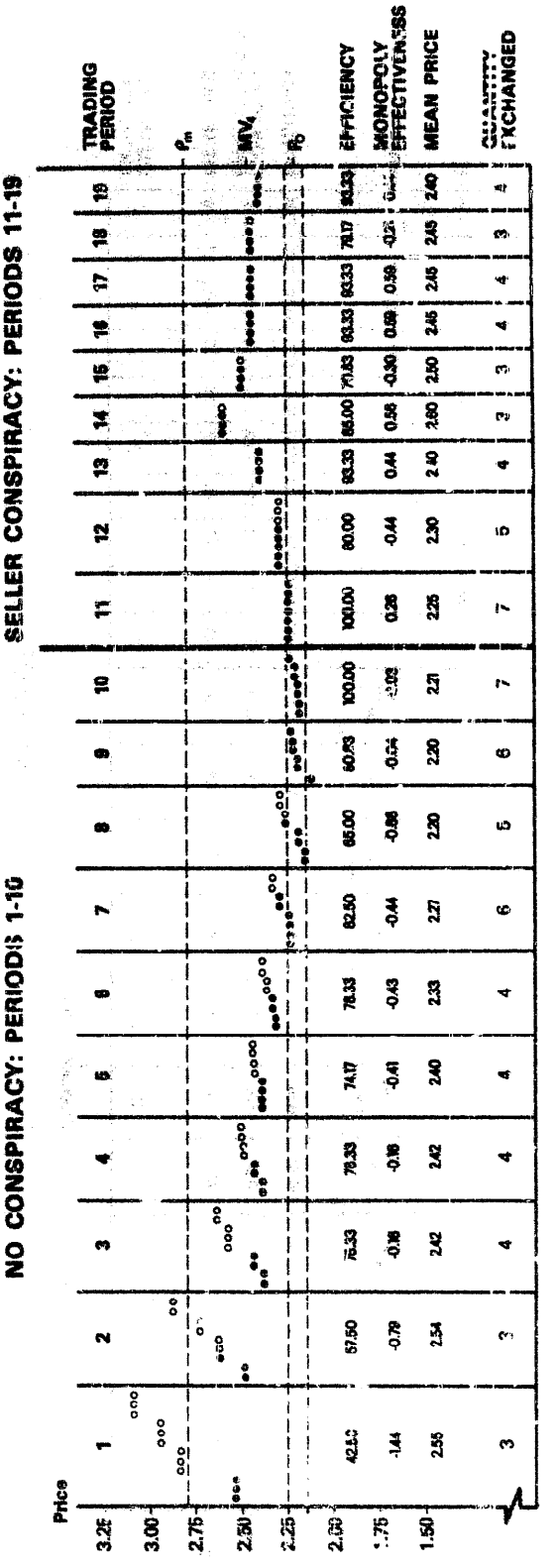


Fig. 6. Experiment p066.

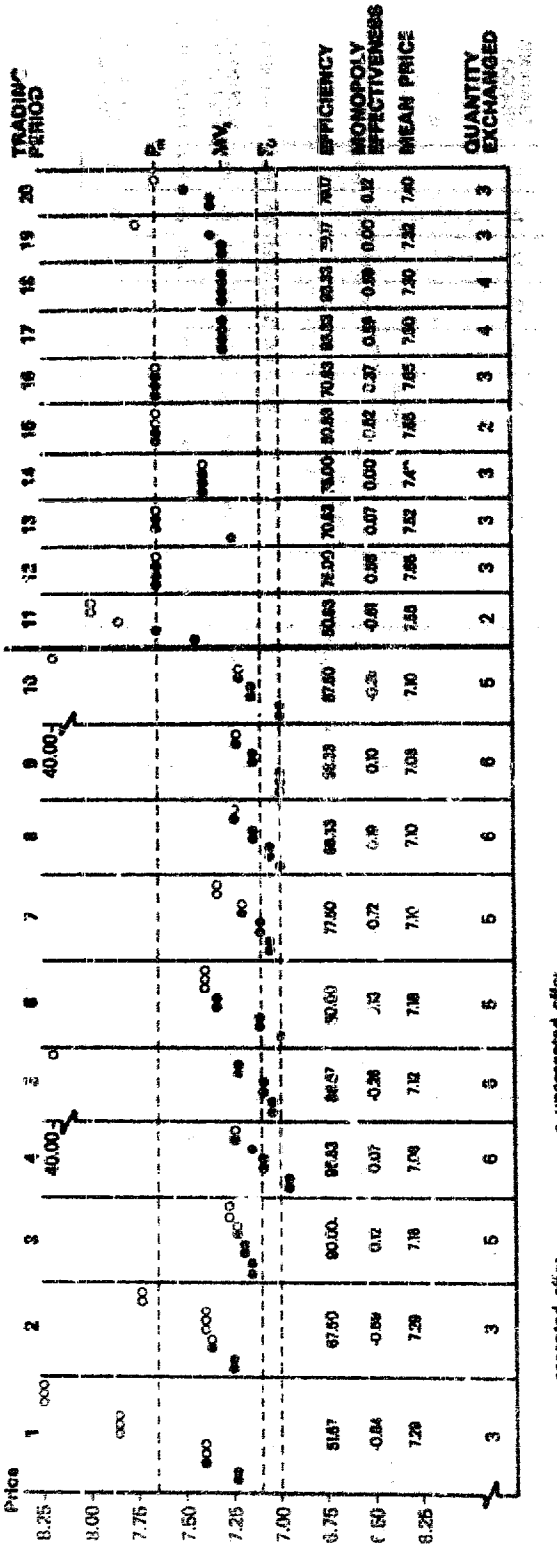


Fig. 7. Experiment po67.

In the experiments where seller conspiracies were allowed only after period 10 (figs. 6 and 7), notice that market performance became less competitive after the opportunities for conspiracy were allowed. However, neither group of sellers chose to consistently fix contract prices at $P_m$. Sellers in po66 coordinate the offer price posted, and contracts appear to have stabilized at a price very near the fourth step on the demand curve where, with full demand revelation by buyers, each seller trades one unit. Note that full revelation occurred in periods 16 and 17 but that under-revelation in period 18 was followed by a five cent price decline (and full revelation) in period 19. Experiment po67 displayed less price stability than po66 during periods 1-10 as well as periods 11–20. In periods 17 and 18, four contracts occurred at the fourth step on the demand array but this was followed by two periods of dispersed offers and an upward movement of the mean contract price.

Figs. 8 through 11 display sequential offer prices and descriptive statistics for experiments po55, po59, po77 and po83 where sellers were allowed to communicate prior to each trading period. Experiment po55 is striking in that sellers conspire to fix the market price at the C.E. price. In contrast with this, sellers in experiment po77 formed a fairly effective cartel, posting offers at $P_m$ in six of the final seven trading periods. Showing more sophistication than other seller groups, the cartel rotated the assignment of which individual would post a price above $P_m$ and thus not trade a unit. Experiments po59 and po83 both appear to have stabilized at a price near the fourth demand step. Prices in po59 were five cents below the fourth step in periods 10–15 with buyers fully revealing demand in five of these six periods. In po83, offer prices were slightly above the fourth demand step in periods 12–15 thus prohibiting a fourth unit from trading and lowering cartel profits. However, the gradual lowering of offer prices in the last few periods is a pattern consistent with eventually stabilizing at the highest price where buyers are willing to trade four units.

For the six posted-offer seller-conspiracy sessions we thus observe one market converging to the monopoly optimum price, one converging to the C.E. price, and four generating final contract prices in a range near the fourth demand step.

### 4.4. *Posted-offer, monopoly experiments*

Finally, we turn to the four experiments (po92, po94, po102 and po107) designed to observe the behavior of the same posted-offer market operating under a true monopoly. Figs. 12 through 15 display descriptive statistics and sequential offers for these experiments.

The seller in po102 was the least successful monopolist. He chose to offer units at the C.E. price in the last twelve periods of the experiment. This occurred in spite of the fact that in periods 1, 5, 6 and 8 charging a price above the C.E. resulted in larger profits. Based on informal comments at the

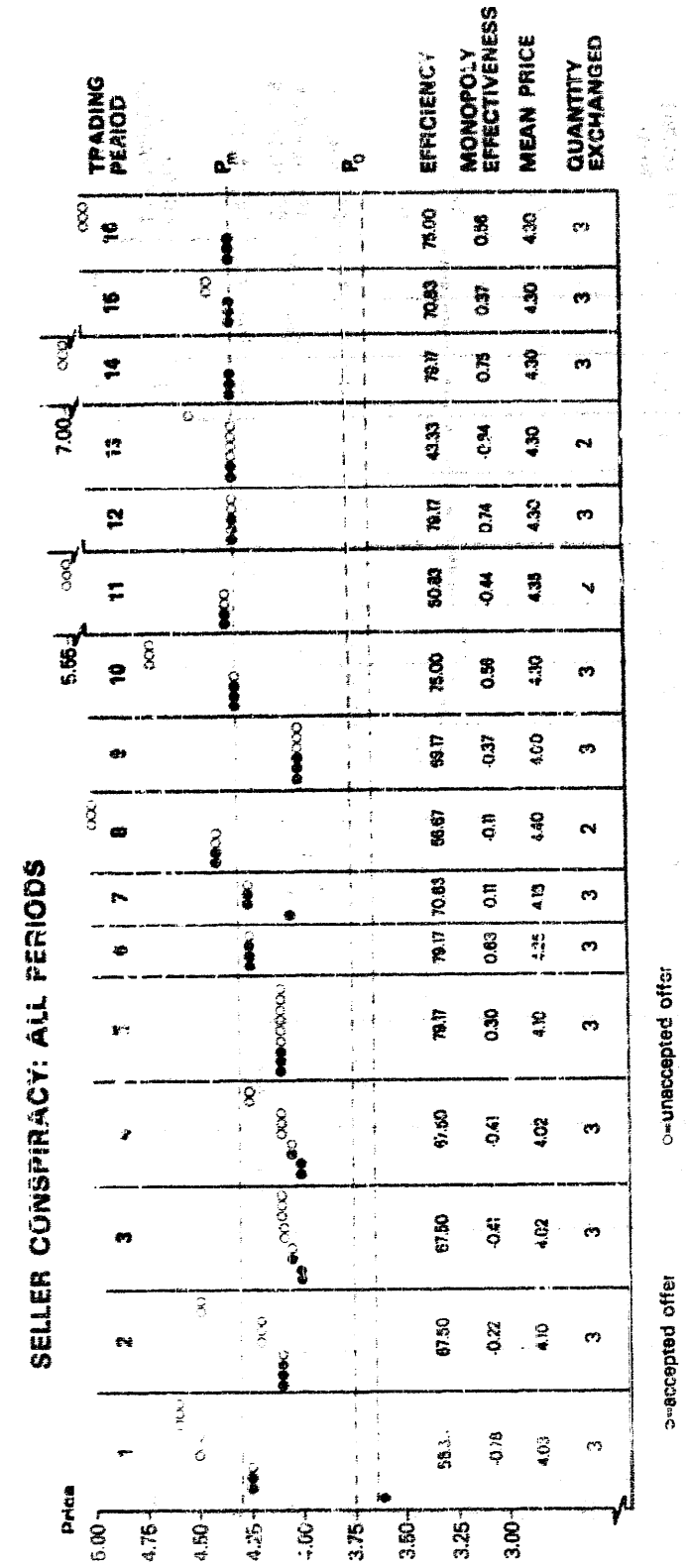


Fig. 10. Experiment po77.

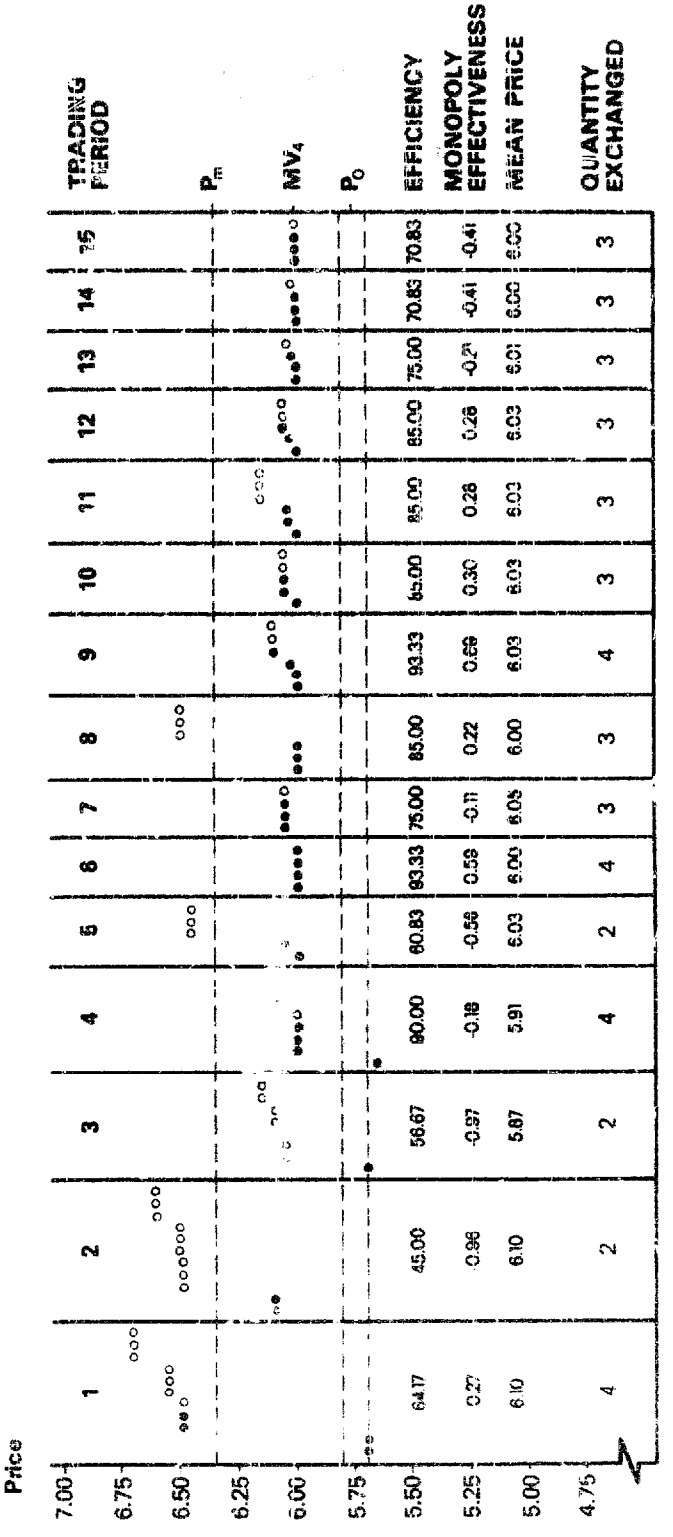


Fig. 11. Experiment po83.

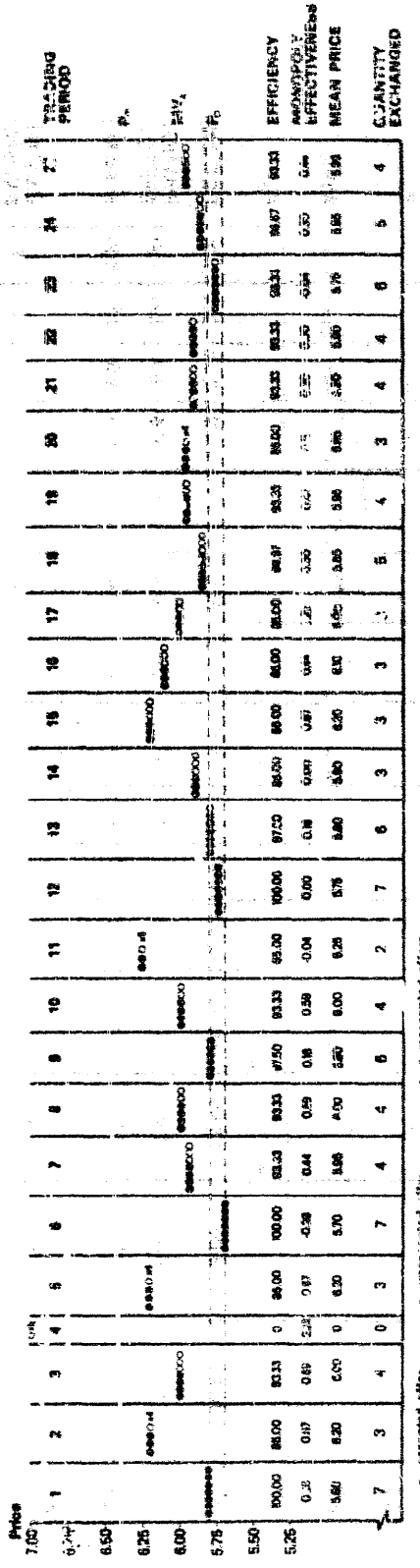


Fig. 12. Experiment po92.

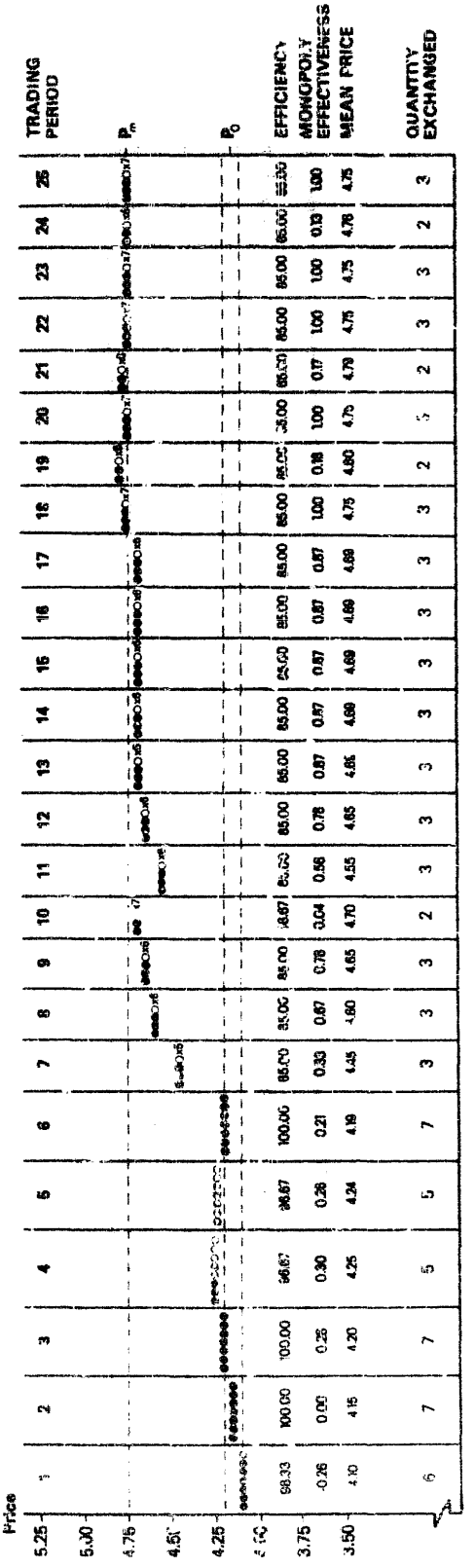


Fig. 13. Experiment po94.

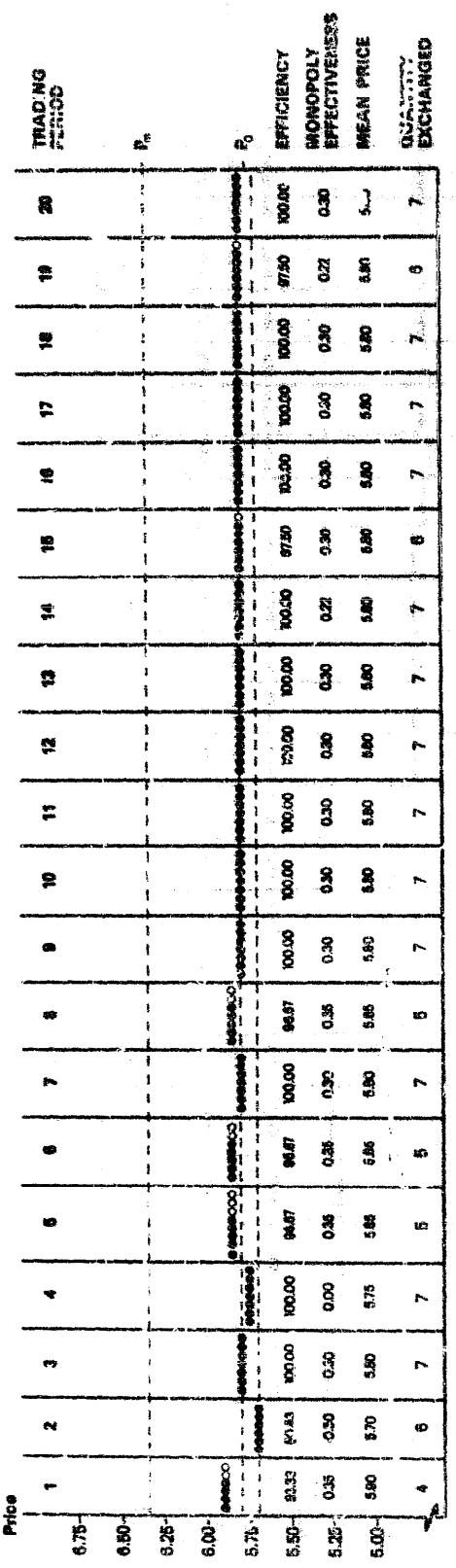


Fig. 14. Experiment po102.

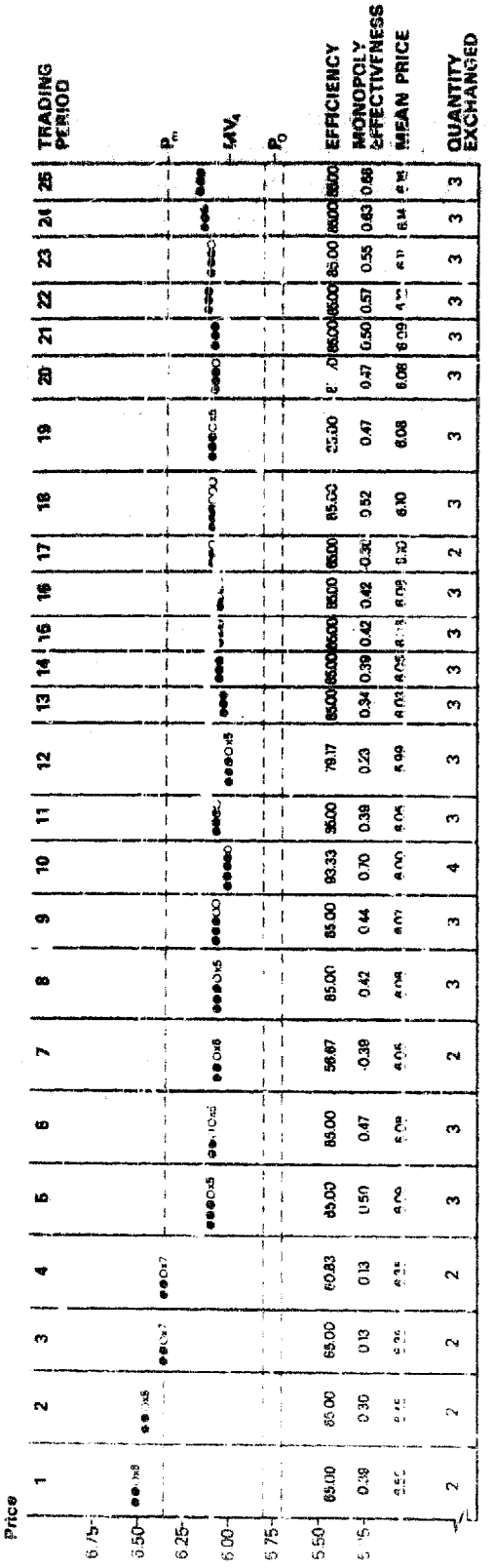


Fig. 15. Experiment pc107.

experiment's conclusion, it appears that the monopolist was quite satisfied with his per period earnings and, given the effort-cost and preceived risk embodied in price searching, he simply chose not to deviate from the 5.85 offer price beyond period 9. In sharp contrast with this, the monopolist in po94 posted an offer price within ten cents of $P_m$ in periods 12 through 25. The seller's price searching was aided by full demand revelation after price increases in periods 7, 8 and 9. The withholding of one demand unit in period 10 resulted in a price decline in period 11, but there was full revelation after price increases in periods 12, 13 and 18 allowing the seller to successfully zero in on $P_m$.

The other two monopoly experiments display less extreme results. The contract prices in po92 wander from the C.E. price to a level slightly below $P_m$, eventually stabilizing in the range from the C.E. to the fourth demand step during the last nine periods. It is unclear what (if any) price-searching strategy the monopolist was using. Under-revelation of demand did occur in periods 11, 14 and 20 but the seller did not appear to be making full use of the information provided by other periods. In po107 the monopolist's offer price drifts in a range from the fourth demand step to midway between this price and $P_m$ during periods 5 through 25. Under-revelation of demand in periods 3 and 4, when the seller posted $P_m$ as the offer price, resulted in a 26 cent price decline in period 5 and most likely had a restraining influence on the monopolist's offer prices during the remainder of the experiment.

### 4.5. *Analysis of pooled data*

Fig. 16 presents a visual comparison of the time series of four market performance criteria (normalized mean price, volume, efficiency index and monopoly effectiveness index) for each of the four experimental groupings. The chartings are sample means derived from observations pooled across all intragroup experimental replications. Based on this empirical evidence (and figs. 2–15) we offer the following three general summary statements regarding *intragroup* market behavior:

(1) Opportunities for seller conspiracies tend to inhibit the empirically documented C.E. convergence properties of both the double-auction and posted-offer mechanisms. Of the ten seller-conspiracy experiments, two (da69, po55) do not support this statement.
(2) Double-auction markets with seller conspiracies do not converge to the monopoly profit-maximizing equilibrium (given the market parameters and procedure used in our experiments). All four double auctions support this statement.
(3) Posted-offer markets with either seller conspiracies or monopolies tend not to converge to the monopoly equilibrium (using our market

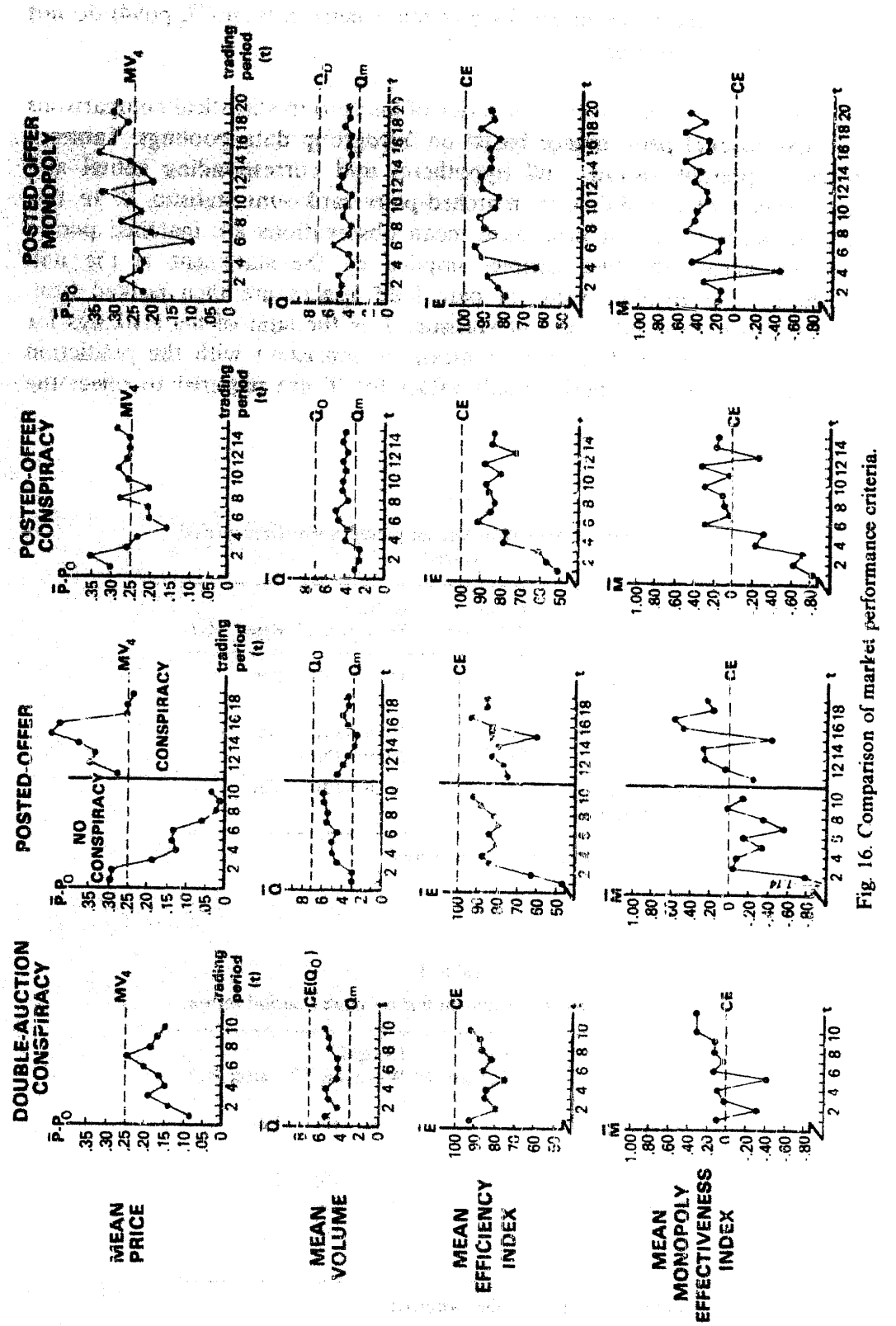


Fig. 16. Comparison of market performance criteria.

parameters and procedures). Two of ten experiments (po77, po94) do not support this statement.

We now present the results of a series of *inter*group statistical comparisons of relative market performance based on *intra*group data poolings. Tables 2 through 5 present various null hypotheses and corresponding actual and critical values of the Wilcoxon matched-pairs rank-sum statistic, $T$. In this non-parametric statistical procedure mean observations are matched period by period across the two samples implied by the statement of the null hypothesis. The absolute value of paired differences are then ranked from lowest (1) to highest ($n$). The test statistic, $T$, is the sum of the rankings for which the sign of the difference in means is consistent with the prediction based on the null hypothesis. Small values for $T$ are required to reject the null hypothesis.

Table 2

Price comparison: Wilcoxon test on paired normalized period means.

| $H_0$ | Actual Wilcoxon $T$[a] | Critical Wilcoxon $T$[b] | Reject $H_0$? |
|---|---|---|---|
| p.o. conspiracy ≦ d.a. conspiracy | 8 | 11 ($n=10$) | Yes |
| p.o. conspiracy ≧ p.o. monopoly | 29<br>55,57 | 11 ($n=10$)<br>30 ($n=15$) | No |
| p.o conspiracy ≦ p.o. no-conspiracy | 0 | 11 ($n=10$) | Yes |

[a]Range of $T$ is given if ties occurred.
[b]One tailed test, $\alpha=0.05$.

Table 3

Quantity comparison: Wilcoxon test on paired period means.

| $H_0$ | Actual Wilcoxon $T$[a] | Critical Wilcoxon $T$[b] | Reject $H_0$? |
|---|---|---|---|
| p.o. conspiracy ≧ d.a. conspiracy | 4,6 | 11 ($n=10$) | Yes |
| p.o. conspiracy ≦ p.o. monopoly | 38,40<br>85,88 | 11 ($n=10$)<br>30 ($n=15$) | No |
| p.o. conspiracy ≧ p.o. no-conspiracy | 3 | 11 ($n=10$) | Yes |

[a]Range of $T$ is given if ties occurred.
[b]One tailed test, $\alpha=0.05$.

Table 4

Monopoly effectiveness index comparison: Wilcoxon test on paired period means.

| $H_0$ | Actual Wilcoxon $T$ | Critical Wilcoxon $T^a$ | Reject $H_0$? |
|---|---|---|---|
| p.o. conspiracy ≦ d.a. conspiracy | 44 | 11 ($n=10$) | No |
| p.o. conspiracy ≧ p.o. monopoly | 9<br>10 | 11 ($n=10$)<br>30 ($n=15$) | Yes |
| p.o. conspiracy ≦ p.o. no-conspiracy | 15 | 11 ($n=10$) | No |

[a]One tailed test, $\alpha=0.05$.

Table 5

Efficiency index comparison: Wilcoxon test on paired period means.

| $H_0$ | Actual Wilcoxon $T^a$ | Critical Wilcoxon $T^b$ | Reject $H_0$? |
|---|---|---|---|
| p.o. conspiracy ≧ d.a. conspiracy | 12,13 | 11 ($n=10$) | No |
| p.o conspiracy ≦ p.o. monopoly | 45<br>104 | 11 ($n=10$)<br>30 ($n=15$) | No |
| p.o. conspiracy ≧ p.o. no-conspiracy | 21 | 11 ($n=10$) | No |

[a]Range of $T$ is given if ties occurred.
[b]One tailed test, $\alpha=0.05$.

The information presented in table 2 indicates that period mean prices in the posted-offer conspiracy experiments stochastically dominate those generated in both the posted-offer competitive and the double-auction conspiracy control groups. We may not conclude that period mean prices generated by posted-offer monopolies dominate those generated by posted-offer conspiracies. The same set of qualitative conclusions is conveyed by table 3 period mean exchange volume in both the posted-offer competitive and double-auction conspiracy control groups exceeds volume generated in posted-offer conspiracies. No significant difference is found between mean volume in posted-offer conspiracy and posted-offer monopoly.

Turning to the two measures of market performance based on actual vs. theoretical profit ($M$ and $E$) we obtain somewhat different results. From table 4 we see that the only significant difference indicated for the three treatment-control group comparisons is that $M$ is significantly larger in the

true monopoly experiments relative to the posted-offer conspiracy experiments. Given our inability to reject the equality of mean price and volume in this comparison, we might have predicted this result based on our discussion (section 3) of the nature of the profit function faced by a cartel versus the one faced by the true monopolist. Recall that the monopolist's profit function represents the upper bound of the cartel's stochastic profit function (assuming full demand revelation and a single price quote coming from all cartel members). Our inability to reject the null hypothesis in the other two comparisons stresses the fact that an elevated mean price does not necessarily translate into an elevated profit level for sellers. The results presented in table 5 indicate that we may not reject any of the stated *a priori* null hypotheses regarding efficiency. It should be noted, however, that for the fifteen-period comparison of posted-offer conspiracy vs. posted-offer monopoly ($T=104$), a reversal of the inequality in the statement of the null hypothesis would lead us to conclude that efficiency in monopolistic markets is significantly greater than in markets with seller conspiracies ($T=16$). This suggests that the increase in monopoly profit over cartel profits revealed in table 4 was not accompanied by a significant decrease in buyer profits.

## 5. Parting comments

The results presented here demonstrate that opportunities for seller conspiracy in a posted-offer market can foster market prices consistently above the competitive equilibrium. This phenomenon is above and beyond the previously observed tendency of such markets to converge to the competitive equilibrium from above. There is not, however, a clear tendency for prices to rise to levels at or near the theoretical monopoly predictions given the market parameters used in this study. Even though prices did not tend to coincide with the theoretical monopoly prediction, posted-offer conspiracies generated prices not significantly different from those of the true monopolies (see table 2). We cannot interpret this as meaning that the posted-offer conspirators were as successful as the true monopolists given the conspirator's significantly lower performance on our index of monopoly effectiveness (see table 4).

When viewed in light of the previous experimental results of Isaac and Plott, our data suggest at least two points about the relationships between market structure and market performance: (1) market structures allowing conspiracy can affect market performance, and (2) the nature of the links between market structures allowing seller conspiracy and the ultimate market outcomes are complex in that the nature of the trading institution can make a difference. Essentially identical conspiratorial opportunities seem to be more effective in raising prices (but not profits) in posted-offer markets than in markets organized as double auctions. We conjecture that one primary

cause of the apparently different effects on pricing decisions of the opportunities for conspiracy in posted-offer markets (as opposed to double auctions) is the temporary rigidity of prices in the former. This inflexibility seems to substantially reduce the temptation to cheat on the conspiracy agreements.

Several interesting questions remain for future study. Within the realm of the posted-offer trading institution, there are several features of structure and/or conduct that could influence the success of opportunities for conspiracy: (1) non-passive buyer responses, (2) the type of information available to market participants, (3) the market supply and demand parameters, and (4) the possibility of side-payments within a cartel. The difference in pricing behavior among our four monopoly experiments demonstrates the power of strategic buyer withholding of demand. One question for further study is to examine the conditions under which such withholding is more or less likely to occur. The nature and amount of information about market parameters is a variable which has already drawn the attention of U.S. antitrust authorities.[10] In our design, each conspirator knew all exchange prices and quantities traded, but not the cost conditions of other sellers nor the true demand parameters of the buyers. The effect of changing this information mix is an open question.

Another area for further research involves looking at other market trading institutions. For example, part of the electrical conspiracy of the 1950s involved a different kind of trading mechanism, the sealed-bid auction. In addition, neither of the two exchange mechanisms studied so far has looked at sellers who produce for inventory instead of 'to order'. We speculate that the trading institution will continue to be shown to be an important component of the relationship between market structure and economic performance.

The use of laboratory experimental techniques in industrial organization and general microeconomic research is a relatively new and rapidly evolving methodology with inherent advantages and limitations [see, for example, Plott (1981, 1982), Smith (1982a, b) and Isaac (1983)]. From a positive perspective, variations in the trading institution and opportunities for communication among market participants were conducted using an experimentor-controlled underlying set of market parameters. Critical measures of market performance were directly observable relative to theoretical predictions and hypothesis testing based on experimental replications of a particular market environment was relatively straightforward. However, it is important to stress that the direct relevance of our laboratory data to policymaking or predicting outcomes in 'naturally occurring' markets (external validity) should not be assumed. Smith (1981)

[10]For example see *Tag Manufacturer's Institute v. Federal Trade Commission* 174 F 2d 452 (1st Cir. 1979).

has cautioned that bilateral comparisons of experimental treatment groups should not be used as a basis for universal extrapolation. Such results should not be construed as suggesting a generalized condemnation or recommendation of any component of market structure or policy without reference to the entire set of potential alternatives.

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