

CONSPIRACIES AND SECRET DISCOUNTS IN LABORATORY MARKETS*

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This paper uses laboratory methods to evaluate whether price-fixing conspiracies break down in the presence of opportunities to offer secret discounts. The primary treatment difference is whether or not buyer-specific discounts from the posted list price are permitted. In standard posted-offer markets, conspiring sellers almost uniformly find and maintain near-monopoly prices. But when the possibility of offering secret discounts is introduced, sellers find sustaining collusive agreements much more difficult, and transactions prices tend to fall toward competitive levels. Secret discounts yield competitive outcomes less consistently, however, when sellers are provided *ex post* information about sales quantities.

Economists often appear inconsistent in their attitudes toward collusion. On the one hand, there is a general agreement with Adam Smith that explicit communications among sellers in the same market almost inevitably result in an attempt to fix prices.¹ On the other hand, the problems of implementing, monitoring and maintaining an illegal agreement make many economists sceptical that the conspiracies will have much of an impact on prices.²

Some limited empirical evidence on the effectiveness of collusion is available. Porter (1983), for example, studied a legal rail freight cartel that existed before the passage of the Sherman Act, and concluded that although price wars were common, prices were raised from competitive to approximately Cournot levels under favourable conditions. However, the long-standing illegality of explicit price fixing imposes serious data limitations. Not only are conspiracies difficult to identify, but even when such agreements can be documented, actions that are unobservable to competitors (such as chiseling on a price agreement) are also generally unobservable to independent outsiders. Without such evidence, it is difficult to evaluate the nature and success of price-fixing agreements, and the causes of breakdowns in pricing discipline.

The laboratory provides a good platform for the study of conspiracies, since it is possible to introduce controlled opportunities for explicit collusion, holding constant other key elements of the market structure that may affect the success of a conspiracy. Much of the relevant laboratory research has focused on the effects of trading institutions on the robustness of price-fixing

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¹ Smith, (1776, p. 144) however, does not seem to view meetings that produce conspiracies as inevitable, and he argues that government policy should do nothing to facilitate such assemblies.

² Some economists even doubt the effectiveness of conspiracies organised out of the shadow of illegality. For example, Milton Friedman (1974) predicted that the Organisation of Petroleum Exporting Countries' price-fixing efforts would have only transitory effects.

agreements. Isaac and Plott (1981), for example, considered conspiracies in double auctions, i.e. markets where all bids, asks, and contracts are publicly displayed on a ticker tape. This institution, which is modelled after centralised stock exchanges, is well known to be quite competitive. Isaac and Plott found that sellers invariably tried to fix prices, but that agreements were always violated, and generally competitive outcomes prevailed. Isaac *et al.* (1984) allowed conspiracies in posted-offer markets, where sellers set prices independently, and then buyers make purchases from the sellers at the posted prices on a take-it-or-leave-it basis. In contrast to sellers in the double-auctions, posted-offer sellers consistently implemented successful conspiracies.

The observation of successful conspiracies in laboratory posted-offer markets is important, because this institution is a good approximation of pricing in many retail and service markets. Nevertheless, many of the posted price markets of interest to industrial organisation economists differ from the standard laboratory posted-offer implementation, in that sellers can offer private discounts from the 'list' prices. Discounting opportunities of this sort are typical, for example, in producer goods markets, and in markets for big-ticket consumer items like appliances and cars. The effectiveness of conspiracies in such markets is particularly important for antitrust policy, since many of the celebrated price-fixing cases involve producer goods.³

Still other institutional factors may counter any pro-competitive effects of secret discounting. In particular, the *ex post* reporting of aggregate seller sales information, such as that often published by trade associations, may facilitate collusion even when substantial discounting opportunities are available.⁴ Clearly, the publication of sales information is unlikely to reveal discounting directly: Price concessions can often be hidden in reduced delivery and service fees, and sellers who defect from an illegal agreement will, of course, not report their defections. Nevertheless, large swings in sales volume may signal aggressive discounting, and may therefore temper such behaviour (Stigler, 1964).

The effects of these kinds of institutional details on the success of conspiracies are of considerable antitrust policy relevance. While there is little disagreement that price fixing should be punished where it is discovered, the policy issue arises in terms of optimal resource allocation. Given scarce investigative and litigation resources, antitrust agencies must concentrate on cases that are most likely to improve market performance.

This paper reports the results of 21 laboratory markets that were conducted to examine the effects of explicit conspiracies on performance in markets with

³ For example, possibly the most well known price-fixing cases in the United States involved the electrical equipment industry. The conspiracies extended from the 1920s until the 1950s, and grew to encompass virtually every aspect of electrical equipment sales. For an interesting account see Carlton and Perloff (1990) p. 216–9.

⁴ The link between trade associations and conspiracy has been a matter of continuing concern to antitrust authorities. For example, in their review of horizontal price-fixing cases brought by the U.S. Department of Justice between 1963 and 1972, Hay and Kelley (1974) found that trade associations were present in almost all cases with 15 or more conspirators.

posted prices. In addition to a series of three baseline posted-offer markets conducted to confirm the competitive nature of our market structure without collusion, we report 18 markets where sellers were given the opportunity to conspire before posting prices. These 18 markets are evenly divided into three treatments. The first treatment used standard posted-offer trading rules following the seller price discussions, i.e. all trades must be made at the posted prices. In the second treatment, sellers were given an opportunity to grant secret discounts from the list price. Secret discounts were also allowed in the third treatment, but sellers were provided with *ex post* sales quantity information before price discussions for the following period. Below, we describe in detail the experiment design and procedures in Section I. Market results are reported in Section 2, while conclusions and suggestions for further investigation are offered in a third section.

1. Experiment Design and Procedures

The experiment is designed to evaluate factors affecting the success of collusion in a market context. Our interest in providing decision-makers with an array of conditions that confront potential conspirators in naturally-occurring markets (such as cost heterogeneities, incomplete information and time-horizon uncertainty), forces us to gloss over issues to which game-theoretic models of collusion are notoriously sensitive. As a consequence, the experiment is not a direct test of any specific game-theoretic model of collusion. Rather, we evaluate the predictions of less formally structured policy models. Nevertheless, we believe that the experiment should be of interest to theorists as well as to policy-makers, because experiment results provide some direct insight into the reliability of nonbinding agreements when elements of the agreements are unverifiable.

Consider then, the market design. Economists generally agree that successful price-fixing conspiracies are more likely with small numbers of firms and a homogeneous product.⁵ Since homogeneity and small numbers are dictated by our laboratory market software, we sought a market structure that would otherwise be fairly competitive. Aggregate demand and supply arrays for the market are shown in Fig. 1. In the figure, buyers' reservation values and sellers' marginal costs are measured as deviations from P_c , the highest competitive price in the range of overlapping demand and supply. The design consists of three buyers ($B1$, $B2$, $B3$) and three sellers ($S1$, $S2$, $S3$). Each unit is labelled by the subject's ID. Buyer $B1$, for example, has three units with values above, equal to, and below the competitive price, respectively. For each purchase, buyers earn the difference between a unit's value, and the purchase price. Thus, buyer values are maximum reservation prices for each unit. Similarly, sellers' unit costs are minimum reservation prices. For each sale, a seller earns

⁵ Hay and Kelley (1974) report that most price-fixing cases investigated by the Department of Justice involved 10 or fewer firms. They also report that conspiracies were more likely with relatively homogenous products.

Our market deviates from the typical laboratory procedure in that buyers are charged five cents each time they approach a seller. Shopping costs are a necessary feature of our discounting treatments, since trading time is limited in the laboratory. The five cent shopping fee keeps buyers' switches from causing extensive delays. Shopping costs also add an element of realism, since the process of approaching different sellers and eliciting price quotes is rarely costless. When we impose these costs in the lab, we get a more economically meaningful measure of shopping intensity than would be the case if shopping were costless. Notice that the five cent shopping cost does not alter the competitive price; all three buyers are willing to purchase two units from a seller who posts a (normalized) price of zero since the surplus from the buyer's first unit far exceeds the shopping cost.

The dashed marginal revenue line (MR) in Fig. 1 intersects the industry supply function at a quantity of three, and the joint-profit-maximising price (P_m) is \$0.55, which is 5 cents below the value step for B_3 to allow for the shopping cost. A joint-profit-maximising outcome requires each seller to produce a single (low-cost) unit, and this symmetry may facilitate stable collusion.⁷ A small price cut from the monopoly level, however, will enable each seller to exhaust its capacity of three units. This large incentive to defect from any joint-profit-maximising price serves to balance the design factors that may facilitate collusion: small numbers, product homogeneity, demand and supply stability, and the symmetric market shares at a joint monopoly outcome.

The effects of collusion-induced price increases can be evaluated in terms of market efficiency, which is measured by expressing the sum of buyers' and sellers' realised earnings as a percentage of the maximum possible aggregate surplus. The competitive outcome for the design in Fig. 1 achieves the maximum aggregate surplus of \$5.10, and the aggregate surplus in a joint-profit-maximising outcome is \$4.20, for an efficiency of 82.4%.⁸ Besides reducing efficiency, a joint-profit-maximising outcome increases the sellers' aggregate earnings relative to the buyers: Seller surplus increases from \$1.95 in the competitive outcome, to \$3.00 in the joint-profit-maximising solution, while buyer surplus falls from \$3.15 to \$1.20. The most common measure of the extent to which sellers manage to exploit monopoly power is an index of monopoly effectiveness, M , developed by Smith (1980). M is the increase in aggregate sellers' earnings over their earnings in a competitive equilibrium, expressed as a percentage of the difference between the theoretical joint monopoly earnings and the seller earnings in a competitive equilibrium. The M value will be 100 for a perfect cartel and zero in an efficient competitive

⁷ Isaac *et al.* (1984) found that agreements were more stable if each seller was able to sell at least one unit at the selected price. The analogue to natural environments is that simple price-agreements, when feasible, tend to generate more stable conspiracies than the more complicated arrangements needed when a collusive price results in serious sales imbalances.

⁸ In calculating the theoretical competitive and joint monopoly efficiencies, we assume that the three buyers each shop from a single seller, which reduces aggregate earnings by three times the shopping cost, i.e. by 15 cents. Shopping costs are similarly deducted from earnings when actual efficiencies are calculated for each market session.

outcome. Negative M values are also possible, and may be caused by either sub-competitive prices or inefficient production allocations.

Markets in the first treatment are conducted under standard posted-offer trading rules. Sellers independently post prices on a take-it-or-leave-it basis at the beginning of each market period. Then buyers are selected in a random order to shop, and the market period ends when all buyers have had a chance to make purchases. In the second, 'list-discount' treatment, sellers post prices independently as before, but buyers, when shopping, may solicit discounts. Upon approaching a seller, the buyer may request a discount from the posted price (by tapping 'r' on their keyboard). The seller then responds by typing in a price offer that is less than or equal to the initial posting. This new offer is seen only by the buyer who made the request. After seeing the seller's response, the buyer may purchase the unit (by tapping 'p'), or reject the offer (by tapping 's'). In the event of a rejection, the buyer must pay the shopping cost to approach a different seller if the buyer wishes to continue shopping.⁹

Critically, any response to a discount request by a seller requires the same number of keystrokes, so other buyers and sellers cannot directly observe any discounting. Moreover, all traders are visually isolated by foam board partitions that prevent them from seeing transactions prices, and prevent sellers from seeing sales quantities. Thus, the discounts are made secretly and selectively (to some buyers but not necessarily to others). This secrecy is diminished in the third treatment, which is conducted exactly as the second, except that sales quantity information is announced to the sellers at the end of each period, prior to their price discussions for the following period.

The major procedural problem with introducing a secret seller conspiracy was getting buyers out of the laboratory at the end of each period without arousing their suspicions. This was handled by taking the buyers to another room where they were randomly reassigned their roles as $B1$, $B2$ or $B3$ for each period. This random assignment was done by drawing colored marbles from a cup. When buyers returned to the laboratory several minutes later, they were seated at the computer that corresponded to their role for the next period.¹⁰ The rotation had the additional advantage of equalising buyer earnings, since $B3$ earns nothing at the joint-profit-maximising price.

After buyers left the room, sellers were read a message that allowed

⁹ After switching away, buyers may return to a seller that he or she has approached previously. But each time the buyer switches sellers, the shopping cost must again be paid, and the seller is not committed to any previous offer made to the buyer. Also, to avoid paying unnecessary shopping costs, a 'NO UNITS' message appears below a seller's identifier on the buyers' screens when a seller has no remaining units in a period. This information is not provided to sellers. Finally, in the sessions, buyers almost uniformly requested a discount every time they approached a seller. The bulk of the exceptions occurred in the early periods of some sessions, and may reasonably be attributed to some lack of experience by some buyers with the discounting mechanism (despite having participated in a prior session).

¹⁰ The rotation process took several minutes each period. In addition to drawing marbles, a monitor also had to verify earnings calculations, since the random role assignment procedure required that the buyers maintain their earnings records on paper.

conspiracy, but barred physical threats, any post-session side payments, and the discussion of nonpublic information such as unit costs, sales quantities (if these were not announced), or discounts (if permitted).¹¹ The restriction on physical threats and subsequent division is standard for this type of experiment.¹² The restriction on discussion of private information was intended to protect subjects from the stress of having to misrepresent their actions in the event they chose to defect from an agreement.¹³ During the discussion period, sellers slid their chairs back about three feet so that they could see each other, but the sides of the foam board dividers continued to obscure the individual sellers' computer monitors. The discussions varied considerably from one cohort of sellers to another, but they clearly understood the instruction as permitting collusive price fixing. One seller, for example, jokingly remarked that 'We could go to jail for this.' Discussions lasted until a monitor signaled the return of the buyers, or about three minutes. The sellers then returned to their visually isolated computer screens to enter price and maximum quantity decisions for the period. Requiring that decisions be made in private adds an important element of realism, i.e. the temptation that arises once sellers leave the 'smoky hotel room.'

All markets consisted of 15 trading periods. The final period was not announced, and if asked, we did not tell participants the terminal period. Record sheets for buyers had plenty of room for extra information, and we recruited for a considerably longer time period than it took to conduct most of the sessions. Procedures were standard in other respects. Ten of the market sessions were conducted at the University of Virginia and the remaining eleven at Virginia Commonwealth University. Each session involved six subjects who were recruited from undergraduate classes. All subjects were experienced in the sense that they had participated in a previous posted-price session, with

¹¹ The message read: 'While waiting for the buyers to return, we give you the opportunity to talk among yourselves about the market. You are free to discuss all aspects of the market, with three exceptions: (a) you may not discuss side payments or make physical threats, (b) you may not discuss quantitative information about your costs, or the maximum number of units you have available, and (c) you may not discuss any actions you took in the previous periods that were not directly observable by the other sellers. (Pricing decisions, you will recall, are public information. On the other hand, quantity postings, actual sales quantity and discounts, if any, are private). You may discuss any other aspect of the market. In particular, you may discuss what you think is an appropriate action for the next period, but you may not discuss what you did in the previous period other than to indicate that you were either pleased or not pleased with the outcome. After receiving the signal that the buyers are returning, please maintain strict silence. Also, if the sellers return to terminals to make price/quantity decisions, they may no longer communicate, even if the buyers have not returned.'

¹² This restriction was used in all of the laboratory studies cited in the introduction. Physical punishments and post-session earnings divisions are neither observable nor enforceable, and would thus generate a substantial loss in experimental control. With the restriction, the conspiracy procedure implements a situation where cartel members avoid explicit payments in order to reduce the chances of detection.

¹³ To ensure that terms of the discussions were not violated, a monitor remained with the sellers as they talked. Any allusion to the exchange of proscribed information was abruptly stopped by the monitor, who then reread instructions pertaining to acceptable types of communications. Sellers tended to obey the communications rules scrupulously, and if there was any question about the permissibility of a particular topic, sellers usually asked the monitor prior to discussing it. There were no persistent or systematic attempts to violate the communications rules.

and without discounting.¹⁴ Subjects were paid \$6 plus salient earnings, and their earnings averaged a little less than \$20 for a two-hour session. Finally, except for the manual recording of buyers' earnings, both the instructions and experimental environment were fully automated.¹⁵

2. Results

Outcomes are reviewed by treatment, starting with the six posted-offer conspiracies, *CPO1–CPO6*. Results of this first treatment are evaluated in light of a series of three baseline sessions *NPO1–NPO3*, which were conducted under conditions identical to the other posted-offer sessions, except that conspiratorial opportunities were absent. Results of the two list-discount treatments are considered subsequently, starting with the six sessions with collusion but no sales quantity reports, *CD1–CD6*, and concluding with the six list-discount conspiracies with sales quantity reports, *CDQ1–CDQ6*.¹⁶

Posted Offer Conspiracies

The mean transactions price paths for the each of the posted-offer sessions are shown in Fig. 2. For reference, dotted lines in the figure indicate the monopoly price P_m and the highest competitive price P_c . With the exception of session *CPO6*, the posted-offer conspirators consistently maintained near-monopoly prices toward the end of each session, despite the strong individual incentives to chisel on prices high above marginal costs. Moreover, each of the six price sequences is quite distinct from the mean transactions price paths for baseline sessions *NPO1–NPO3*, which appear toward the bottom of the figure.

As suggested by the convergence of the control sessions to the competitive price prediction, the market structure does not by itself promote outcomes particularly favourable to sellers. Summary performance measures for the final five periods of sessions *NPO1–NPO3* are listed just below the competitive predictions at the top of Table 1.¹⁷ In two of these markets (sessions *NPO1* and

¹⁴ Also, no one participated as a conspiring seller more than once, and with a few exceptions, per subject participation was restricted to one of the markets reported in this paper. Exceptions are as follows: In one conspiracy market (*CDQ5*) a design-experienced buyer was used as a buyer. In a second conspiracy market (*CDQ6*) both a design-experienced buyer and a design-experienced seller were used as buyers. None of these participants engaged in behavior that was in any sense atypical. Design-experienced buyers were also used in two of the baseline no-conspiracy sessions. In session *NPO2*, four of six participants (and one seller) were design-experienced buyers. In session *NPO3*, five of six participants (and all three sellers) were design-experienced buyers.

¹⁵ The instructions for the posted-offer version are in Davis and Holt (1993, Appendix A4.2). The software that presents these instructions and controls market trading was written by Davis. The special instructions read to buyers are available along with the data at the FTP site mentioned in the acknowledgments.

¹⁶ In each collusion treatment cell sessions 1–3 were conducted at the University of Virginia, and sessions 4–6 were conducted at Virginia Commonwealth University. Baseline sessions *NPO2* and *NPO3* were also conducted at Virginia Commonwealth University.

¹⁷ As a crude means of controlling for the effects of price searching and convergence, we restrict our attention to data from the last five periods in Tables 1 and 3. Inclusion of data from all periods in these tables generates qualitatively identical, but quantitatively smaller treatment differences.

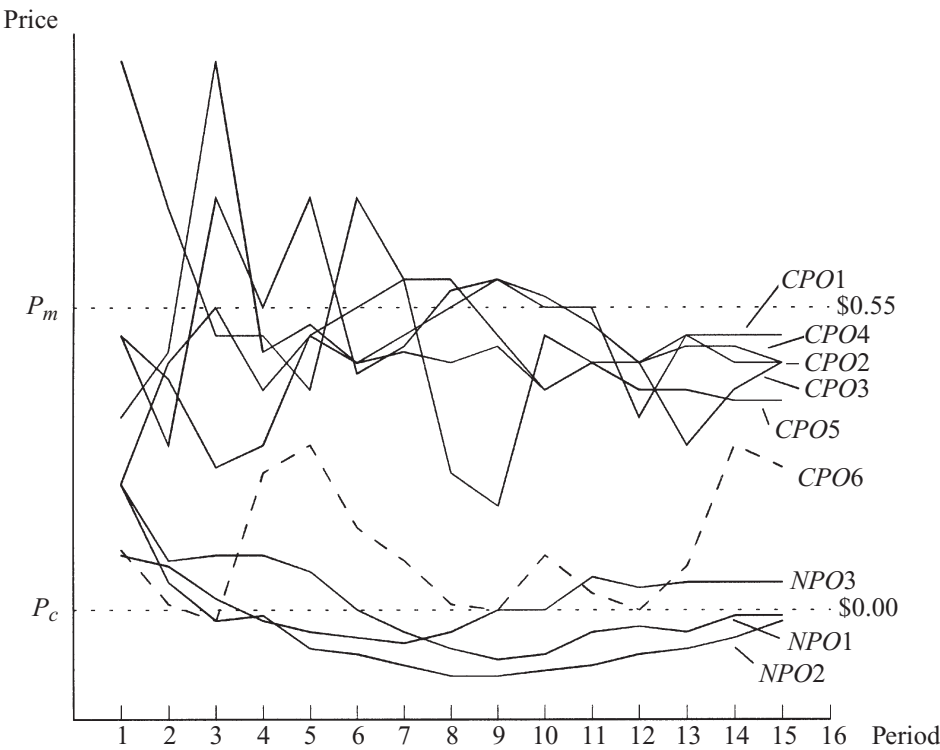


Fig. 2. Mean Contract Prices: No-Conspiracy Posted Offer (NPO) Sessions and Conspiracy Posted Offer (CPO) Sessions.

Table 1
Mean Performance Measures, Last Five Periods of Posted-Offer Sessions

(1) Session	(2) Price Deviation $P - P_c$	(3) Monopoly Effectiveness M	(4) Efficiency E	(5) Shopping Expenses
competitive	0.0	0.0	100.0	15.0
<i>NPO1</i>	-2.6	-24.2	98.0	15.0
<i>NPO2</i>	-6.3	-36.2	100.0	15.0
<i>NPO3</i>	<u>4.7</u>	<u>-35.4</u>	<u>88.6</u>	<u>15.0</u>
	-1.4	-31.9	95.5	15.0
<i>CPO1</i>	48.0	16.2	69.2	15.0
<i>CPO2</i>	46.0	71.2	81.8	15.0
<i>CPO3</i>	40.2	44.4	79.6	15.0
<i>CPO4</i>	47.7	44.8	75.3	15.0
<i>CPO5</i>	41.0	-21.0	65.7	15.0
<i>CPO6</i>	<u>13.3</u>	<u>-20.0</u>	<u>85.3</u>	<u>16.0</u>
	39.4	22.6	76.2	15.2
monopoly	55.0	100.0	82.4	15.0

NPO2), average transactions prices were below the highest competitive price, and at least 98% of the possible gains from trade were extracted. The large negative monopoly effectiveness measures in these markets (-24.2 and -36.2) were largely a consequence of the below-competitive prices posted by the sellers. The third market (session *NPO3*) is distinct in that several of the sellers experimented with relatively high trial list prices in the latter periods of the session. Although this price signalling had some effect on transaction prices, the effect was small, and mean transactions prices remained within five cents of P_c . More prominent consequences of the high list prices were lower mean efficiencies (88.6%) and low M values (-35.4), brought about by one of the three sellers being left out of the market in all but one of the final five periods. Session *NPO3* may be regarded as an example of the consequences of attempted collusion in the absence of explicit communication.

Viewed in light of the baseline sessions, the success of the posted-offer conspirators is impressive. The implementation and maintenance of a conspiracy is not a trivial matter, particularly since sellers could not discuss the numbers of units sold in previous periods. Thus, sellers had to solve the problem of finding the optimal (or at least an acceptable) price in the absence of accurate information regarding aggregate demand. Moreover, the lack of quantity information created cartel maintenance problems, since each seller could unilaterally exhaust demand at any common price more than 30 cents above P_c . Without some more or less observable capacity allocation scheme, one or two sellers could be excluded from the market each period. This potential imbalance is compounded by a slight tendency for buyers to choose $S1$ when prices are identical.¹⁸ This lever bias is like a slight locational advantage, such as a leading telephone listing.

Sellers addressed these allocation problems in different ways, and for this reason, it is instructive to consider a more detailed history of contracts for some selected markets. Consider the price sequence for session *CPO1* in Fig. 3. Data for the 15 trading periods are separated by vertical bars. Within periods, price postings for sellers $S1$, $S2$ and $S3$ are represented, in respective order, by crosses (+). Contracts for single units are denoted by dots (•), diamonds (◆) and boxes (■) for $S1$, $S2$ and $S3$, respectively; multiple units sold at the same price show as an overlap of markers to the right of the price postings. Thus, for example, period 1 of session *CPO1* is illustrated between the left-most pair of vertical bars in Fig. 3. In this period, sellers $S1$ and $S2$ posted relatively high prices. Seller $S3$ subsequently sold all three of his available units, $S2$ sold a single unit, and $S1$ sold nothing.

The sellers in session *CPO1* adopted a price-rotation scheme. Starting in period 6, seller $S1$ offered the lowest price, with sellers $S2$ and $S3$ posting prices one or two cents higher, as can be seen from the staggered position of the three '+' marks in the period 6 slot of Fig. 3. The low-price role was then

¹⁸ If buyers showed no 'lever bias' for low numbers and approached the sellers in random order, the sellers might find a simple agreement to post the monopoly price acceptable. Even though a given seller could be left out of the market in any single period, over the course of the session, earnings should be roughly equal, on average.

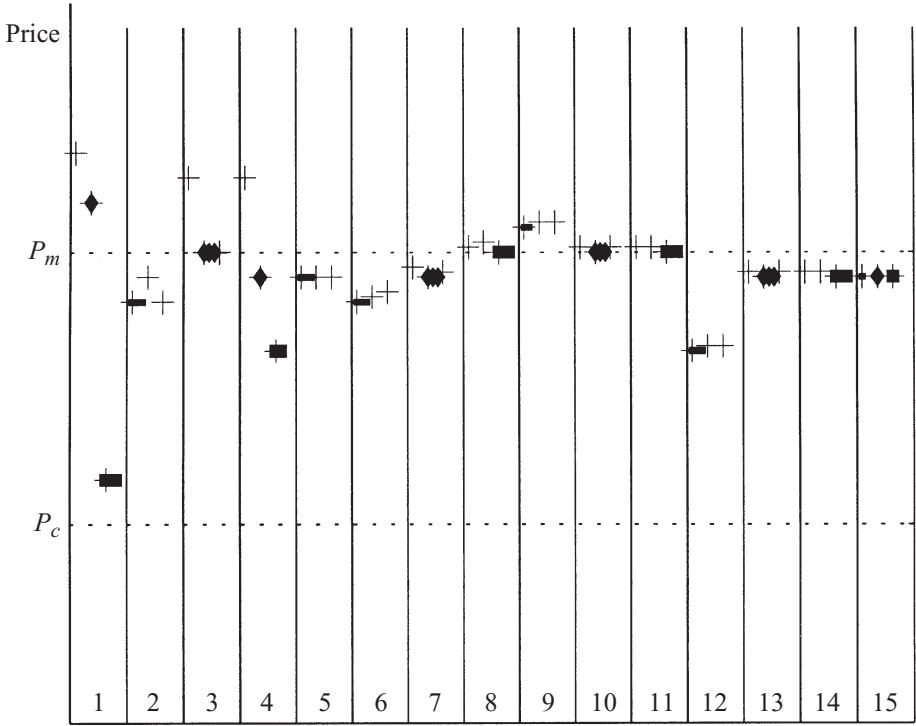


Fig. 3. Price Sequence for Session CPO1. (Key: + price postings, ■ S1 contracts, ■ S2 contracts, ◆ S3 contracts)

passed to seller S2 in period 7, and to seller S3 in period 8. Sellers completed two more rotations in periods 9 to 14. The rotation is analogous to the celebrated ‘phases of the moon’ bid rotation of the electrical equipment conspiracy. Rotation of the low price produces a stable, monitorable conspiracy, which allowed considerable freedom for price experimentation. Sellers tried gradually higher prices in periods 7, 8 and 9, and settled on the monopoly price in period 10. Except for a single attempt to examine the elasticity of demand in period 12, prices remained near the monopoly level for the remainder of the session.¹⁹

Despite the ease of monitoring a price rotation scheme, it is extremely inefficient, since a single seller serves the entire market, as indicated by the row of contract dots on the cross for the lowest price in each period 6–14. With production concentrated in this way, two of the three lowest-cost units on the aggregate supply schedule remain untraded, and one high-cost extra-marginal unit is exchanged instead. For this reason, both aggregate efficiency

¹⁹ The sellers terminated the quantity rotation agreement in period 15, perhaps fearing that the session would soon end. Although participants were not close to the end of the two-hour time block for which they had been recruited, they thought that they were making very high profits with little effort, and in their discussion they reflected some concern about how long the monitor would let them take money from him. They then agreed on a more efficient, but much less monitorable scheme of posting a common price, with quantity restricted to a single unit.

and sellers' earnings remain quite low relative to those in an efficient joint monopoly, despite mean transactions prices very close to P_m . This point is made clear by comparing the monopoly predictions in the bottom row of Table 1 with the summary measures for the last five trading periods of *CPO1*. The 48.0 cent average deviation from the competitive price in *CPO1* is quite close to the 55 cent monopoly deviation. Nevertheless, only 69.2% of the maximum possible gains from trade were extracted in the last five periods of this session, compared with 82.4% that would be extracted by an efficient monopolist. The low monopoly-effectiveness measure in column 3 indicates that much of the efficiency loss was borne by the sellers, who obtained only 16.2% of the difference between efficient joint monopoly earnings and seller earnings in the competitive outcome.

In fact, the sellers in session *CPO1* did about as well as is possible under a price rotation scheme. Given that a single seller supplies the entire market each period, the average maximum efficiency is 65.7% and the average maximum M value is 19.1. If the price rotation is combined with even modestly inefficient prices (perhaps due to price-searching), M falls precipitously. For example, a price-rotation scheme was also implemented in session *CPO5*. The M value of -21 for this session indicates that average seller earnings were below the competitive prediction, despite a mean price 41 cents above P_c .

In sessions *CPO2*, *CPO3* and *CPO4*, sellers eventually adopted a quantity restriction scheme, which increased efficiency, as seen in column 4 of Table 1. Although not evident from the summary information in presented in Table 1, the quantity-restriction schemes also suffered from an increased incidence of defections. Defections became a critical problem in a sixth posted-offer conspiracy market. As suggested by the low and very volatile mean contract price path for session *CPO6* in Fig. 2, the repeated efforts of sellers to engineer price arrangements were continually frustrated by an unwillingness of the sellers to trust each other across periods, reinforced by observable (price) defections. Results of this session emphasise that successful conspiracy is not a foregone conclusion, even in the standard posted-offer environment. Nevertheless, even in this least successful of conspiracy sessions, mean price deviations are higher, M values higher and efficiencies lower than in any of the baseline sessions.

Overall, performance differences between the posted-offer baseline and conspiracy treatments are sizable. As illustrated in the first column of Table 2, mean prices are 40.8 cents higher, mean M index values are 54.6 points higher and mean efficiency is 19.4 points lower in the posted-offer conspiracy sessions than in the posted-offer baselines. As indicated by the double-asterisks by each of the column 1 entries, each of these differences are statistically significant at a 97.5% confidence level, using the nonparametric Mann-Whitney test.²⁰ Combined, these results form our first conclusion, which is similar to a principal finding reported by Isaac *et al.* (1984).

²⁰ The reported significance level is for a one-tailed test. One-tailed tests are appropriate here and throughout the following analysis, since each of the hypotheses that are evaluated have a clear directional prediction. For a description of the Mann-Whitney test, see Conover (1980), p. 216.

Table 2
Mean Differences in Performance Measures Across Treatments[†]

	(1) <i>CPO</i>	(2) <i>CD</i>	(3) <i>CDQ</i>
	<i>P – P_c</i>		
<i>NPO</i>	40.8**	15.3*	26.2**
<i>CPO</i>		–24.5*	–14.5*
<i>CD</i>			9.9
	<i>Monopoly Effectiveness</i>		
<i>NPO</i>	54.6**	9.7	46.0*
<i>CPO</i>		–44.9**	–8.6
<i>CD</i>			36.3'
	<i>Efficiency</i>		
<i>NPO</i>	–19.4**	–12.6**	–13.3**
<i>CPO</i>		6.8'	6.1
<i>CD</i>			0.6

[†] Cell entries report the mean difference between column and row treatments for the respective performance measures. Differences are based on data reported in Tables 1 and 3. All statistical comparisons evaluate the null hypotheses that the column treatment is no larger than the row treatment (when the difference is positive) or that the row treatment is no smaller than the column treatment (when the difference is negative).
** reject H₀ at a 97.5% confidence level or higher (1-tailed test).
* reject H₀ at a 95% confidence level (1-tailed test).
' reject H₀ at a 90% confidence level (1-tailed test).

CONCLUSION 1: *In a standard posted-offer market, opportunities for explicit communications between sellers increase prices, raise seller earnings, and lower efficiencies.*

Conspiracies and Private Discounts

In stark contrast to the stable conspiracies observed in the posted-offer sessions, the additional uncertainty created by allowing secret discounts generated dramatically lower prices, as is evident from the mean transactions price series in Fig. 4. Despite a wide diversity of mean prices in early periods, the cartels failed in five of the six cases, and prices collapsed toward the competitive level, as indicated by the cluster of lines close to the dotted *P_c* line on the right side of Fig. 4.

Some insight into the increased complexity of the cartel problem can be gained by inspecting market histories in more detail. Consider, for example, the sequence of contracts for session *CD5*, illustrated in Fig. 5. This figure is formatted as Fig. 3, but unlike the earlier contract history, discounts cause the transactions price markers to stagger below posted price crosses. In the fourth period, the sellers agreed to post a common list price five cents below *P_m*, but to restrict sales to a single unit each. The sellers could grant any discount they desired. Pleased with the results, sellers continued with the arrangement, and experimented with progressively higher common list prices. However, in period 6 seller *S3* began cheating on the quantity restriction. Initially, the

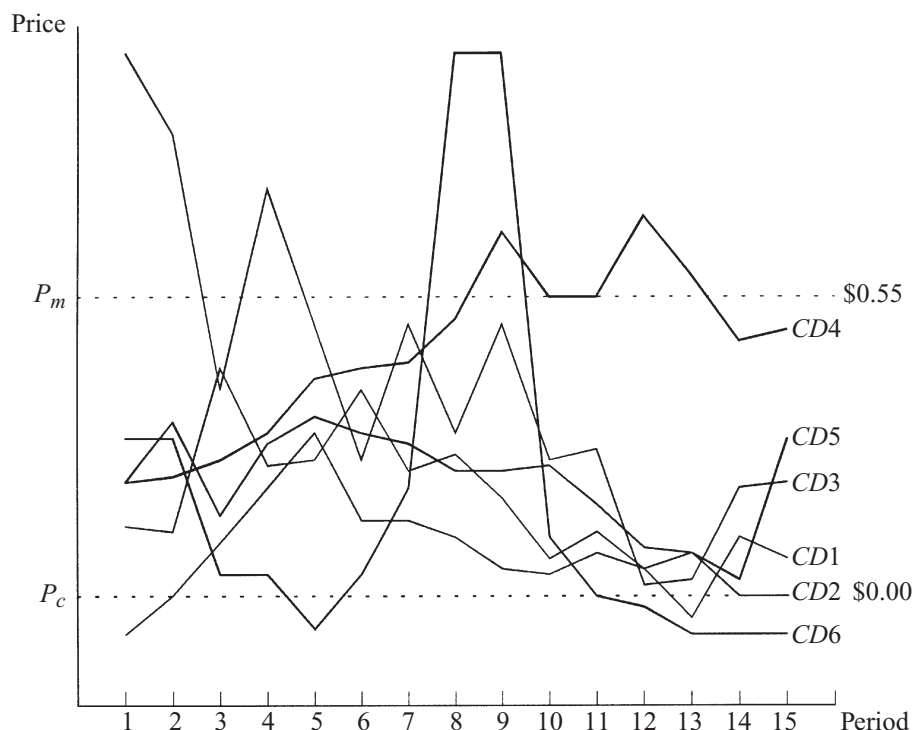


Fig. 4 Mean Contract Prices: Conspiracy List Discount (CD) Sessions.

defection went unnoticed, as sellers *S1* and *S2* each sold a single unit. One of the sellers was left out of the market each period in periods 7, 8 and 9, but seller *S3* was able to mask her continued defections by suggesting that the common list price was too high, and that the other sellers were perhaps not offering sufficiently generous discounts. But after selling no units in periods 8 and 9, seller *S2* concluded that the others were cheating on the agreement in period 10, when no buyer even approached him for a discount. (In fact, *S1* was also offering more than one unit by this time.) Very angry, seller *S2* refused to even talk with the others after periods 10, 11 and 12, and both list and transactions prices decay to near competitive levels. Rather chiding discussions began after period 13, and for the final period the sellers again all promised to stick to an agreement to each offer a single unit at a common list price five cents below P_m . Although the renewed conspiratorial effort did raise transactions prices in period 15, seller *S3* immediately defected from the agreement, and sold two units. Similarly competitive patterns were observed in sessions *CD1*, *CD2*, *CD3* and *CD6*, where the average transactions price for the final five periods ranged between 14.6 cents and -4.6 cents, as indicated in column (2) of Table 3.

But just as successful conspiracy is not a foregone conclusion in the posted-offer markets, the opportunity to grant discounts is not a guarantee that conspiracies will fail. In the remaining list-discount market (session *CD4*),

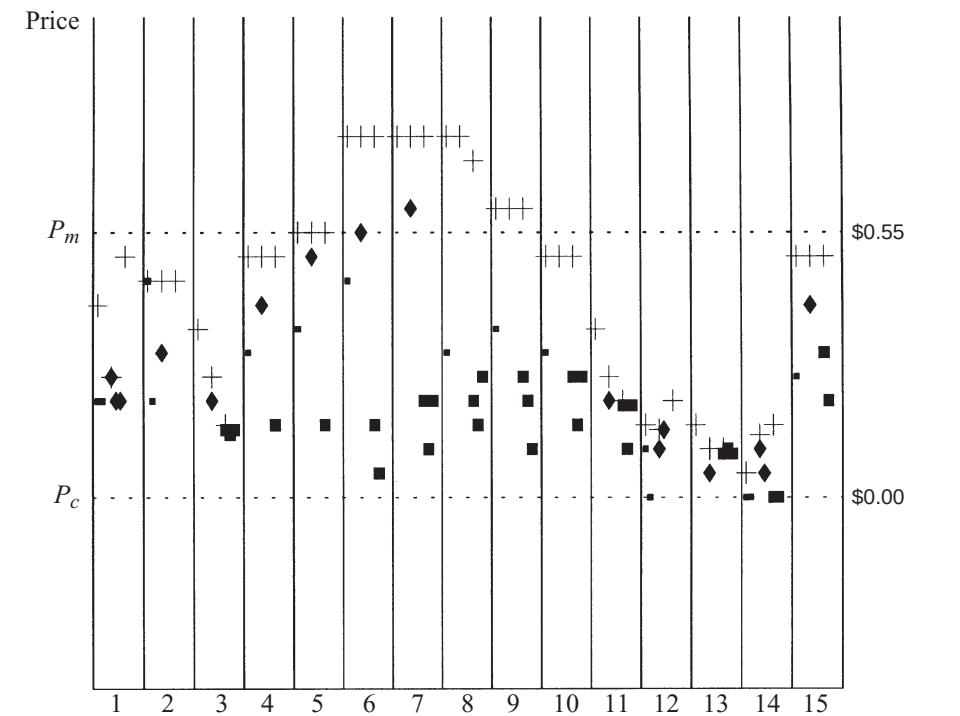


Fig. 5. Price Sequence for Session CD5. (Key: + price postings, ■ S1 contracts, ■ S2 contracts, ◆ S3 contracts)

Table 3
Mean Performance Measures, Last Five Periods of List/Discount Sessions

(1) Session	(2) Price Deviation $P - P_c$	(3) Monopoly Effectiveness M	(4) Efficiency E	(5) Shopping Expenses
competitive	0.0	0.0	100.0	15.0
CD1	6.2	-47.6	82.9	26.0
CD2	4.2	-38.2	85.7	32.0
CD3	14.6	-57.9	73.7	18.0
CD4	56.1	43.8	69.6	18.0
CD5	13.0	-3.8	88.0	19.0
CD6	-4.6	-30.1	97.5	24.0
	14.9	-22.3	82.9	22.8
CDQ1	37.6	70.5	84.5	20.0
CDQ2	41.2	33.5	77.1	17.0
CDQ3	7.0	-39.2	80.6	20.0
CDQ4	9.6	4.6	92.0	27.0
CDQ5	41.5	32.4	74.1	17.0
CDQ6	12.1	-17.7	85.3	23.0
	24.8	14.0	82.3	20.7
monopoly	55.0	100.0	82.4	15.0

none of the sellers ever defected from their agreements in any dimension, whether verifiable or not. As suggested by the extremely high mean transactions price path for this session, sellers maintained near-monopoly transactions prices throughout last half of the market. The mean price deviation of 56.1 cents for periods 11–15 of session *CD4*, shown in column (2) of Table 3 was the largest price deviation observed in any session. At 43.8, the monopoly index for periods 11–15 of session *CD4* is more than 47 points higher than the *M* values any of the other list-discount conspiracies.

Nevertheless, session *CD4* is clearly an outlier, and as a rule, discounting opportunities tend to undermine the success of conspiracies. The powerful effects of discounting are apparent from the mean performance comparisons listed under column (2) of Table 2. Even including session *CD4*, competitive price deviations and monopoly effectiveness are both sizably and significantly lower in the conspiracy with discount (*CD*) treatment than in the posted-offer conspiracy (*CPO*) treatment. In fact, mean transactions prices sessions in the *CD* treatment are considerably closer to prices observed in the *NPO* baseline treatment than were prices in the *CPO* treatment to prices in the *NPO* treatment. Moreover, *M* values across the *CD* and *NPO* treatments do not differ significantly. These observations constitute a second result.

CONCLUSION 2: Private selective discounting opportunities are an important impediment to the success of explicit conspiracies. Both prices and seller earnings are significantly lower in explicit conspiracies when discounts are allowed than when discounts are prohibited. Moreover, seller earnings approach levels observed in competitive no-conspiracy sessions.

Despite lower prices and essentially competitive seller earnings, discounting opportunities do relatively little to improve overall efficiency. As indicated by the efficiency comparisons shown in column (2) in the lower panel of Table 2, efficiency is only modestly higher in the *CD* discount conspiracy treatment than in the *CPO* posted-offer conspiracy treatment, and the difference is of only marginal statistical significance. Moreover, mean efficiency in the discount conspiracies remains sizably and significantly below the *NPO* baseline efficiencies.

Part of the efficiency loss in these sessions is accounted for by higher shopping expenses, as summarised in column (5) of Tables 1 and 3. Shopping costs, however, are not the whole story; efficiency losses are also generated by foregone sales of infra-marginal units, and by the tendency for high-cost units sold at discount to crowd out low-cost units of more cooperative rivals. In fact, the direct relationship between efficiency (column 4) and mean shopping expenses (column 5) in Table 3 suggests that shopping tends to be socially beneficial in the *CD* sessions: The discounts elicited through more aggressive shopping appear to induce the sale of inframarginal units. In any event, the failure of discounting opportunities to significantly improve market efficiency is an important qualification of Conclusion 2.

CONCLUSION 2(a): *Despite lowering prices and seller earnings, discounting opportunities do little to improve market efficiency when sellers engage in explicit conspiracies. Efficiencies remain significantly lower in list-discount conspiracies than in baseline (no-conspiracy) posted-offer markets.*

Secret Discounts and Public Sales Information

Despite Conclusion 2(a), an immediate implication of the above results is that concerns regarding the profitability of price-fixing conspiracies in retail-type markets are attenuated to the extent that private discounts are possible. This implication is tempered by results of six sessions in a final treatment, where the secrecy of discounts is partially eliminated. Conditions for these sessions were identical to those in the list-discount/conspiracy treatment, except that the number of units sold in the previous period were announced to the sellers at the beginning of each seller discussion.²¹ These public announcements were intended to parallel the sales information that is often collected and disseminated by trade associations.²² The mean transactions price paths for these *CDQ* sessions in Fig. 6 show that *ex post* quantity reports can facilitate collusion; near monopoly prices were observed in three of the six markets.

Given public quantity announcements, an efficient and fully monitorable conspiracy could be effected in the absence of transactions price data, if all sellers simply agreed to each offer a single unit at a common list price at or above the monopoly level. Since each seller is certain to sell a single unit in this scheme, nobody has an incentive to discount below P_m .

Rather surprisingly, sellers instituted a quantity-restriction scheme in only one instance (session *CDQ5*). In the other relatively successful conspiracies (*CDQ1* and *CDQ2*) sellers followed a series of much less monitorable price-discrimination schemes, and used the sales information as a rough measure of equity. Such collusion-with-discounting schemes can easily go awry, even with *ex post* sales information. This is evidenced by the sequence of contracts for session *CDQ3* in Fig. 7. In this session the sellers managed to effect a common list price above P_m after period 7. Throughout the session, however, they agreed to offer discounts of unspecified magnitude. Seller *S1*, however, began making very generous discounts to ensure the sale of her first unit starting in period 6. By period 8, two of the three buyers had purchased units from *S1* at

²¹ This information was announced only to the sellers. Due to a record maintenance error, there was some heterogeneity in the way the sessions in this final treatment were conducted. In the first three sessions (*CDQ1*, *CDQ2* and *CDQ3*) each seller's sales quantity was announced at the end of each period. In the final three sessions (*CDQ4*, *CDQ5* and *CDQ6*) only aggregate sales information was reported. Although this procedural variation is regrettable, we suspect that it made little behavioural difference in this particular design. In any even moderately successful conspiracy, three units sell per period, and any defection is obvious from aggregate sales information. Indeed, as a rule, sellers seemed fully aware of one another's sales in each of the latter sessions. For example, of the six sessions in this treatment, quantity information was used most effectively in session *CDQ5*, a session conducted under the aggregate sales information condition.

²² Although the type and quantity of information in U.S. trade association magazines varies widely across industries, the publication of competitors' production figures is a primary common function of the publications.

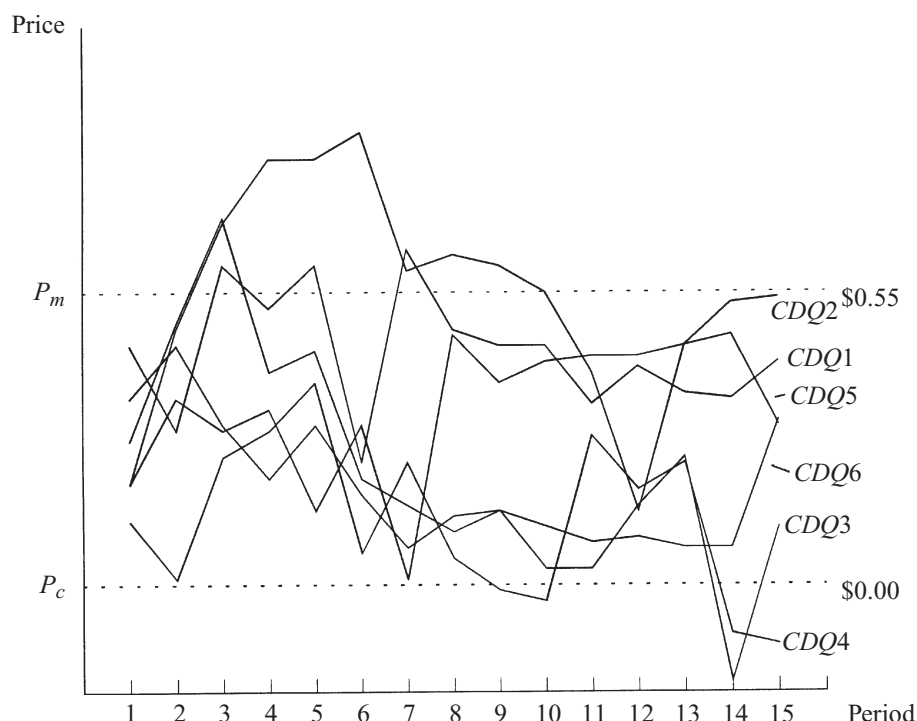


Fig. 6. Mean Contract Prices: Conspiracy List Discount with Quantity Information (CDQ) Sessions.

deeply discounted prices, and these buyers subsequently made their initial approaches to and purchases from S1. As a consequence, one of the other sellers was frequently not even approached by buyers in a period. After seller S3 was left out of the market in periods 10 and 11, seller S1 started encouraging her compatriot to follow her strategy of giving a large discount to ensure the sale of a first unit. Also, to attract an initial sale, seller S3 was allowed by the others to post a lower list price in period 12. This strategy failed, and seller S3 was again left out of the market in period 13. Seller S3, however, did manage to attract buyers when the others agreed to allow her to post a sub-cartel list price a second time, in period 14. Seller S3 responded by offering deep discounts. The consequent collapse of transactions prices was exacerbated by seller S1, who, out of apparent concern for losing her reputation as a low-price seller, not only shaded on the cartel list price but also sold her low-cost unit at only five cents above cost.²³ Similarly competitive sequences were observed in sessions CDQ4 and CDQ6.

²³ It is important to observe that although the deep discounts offered by these sellers resulted in much lower earnings than these sellers might have realised in a different context, their actions were not irrational, given the information available to them. Consider for example seller S3's behaviour in period 14. The seller had sold no units for the four preceding periods, and during that four period span he was approached only infrequently (a total of 3 times) and his progressively lower offers were all rejected. Barring any information pertaining to the discounts offered by the others, it was quite reasonable to guess that he should increase discounts.

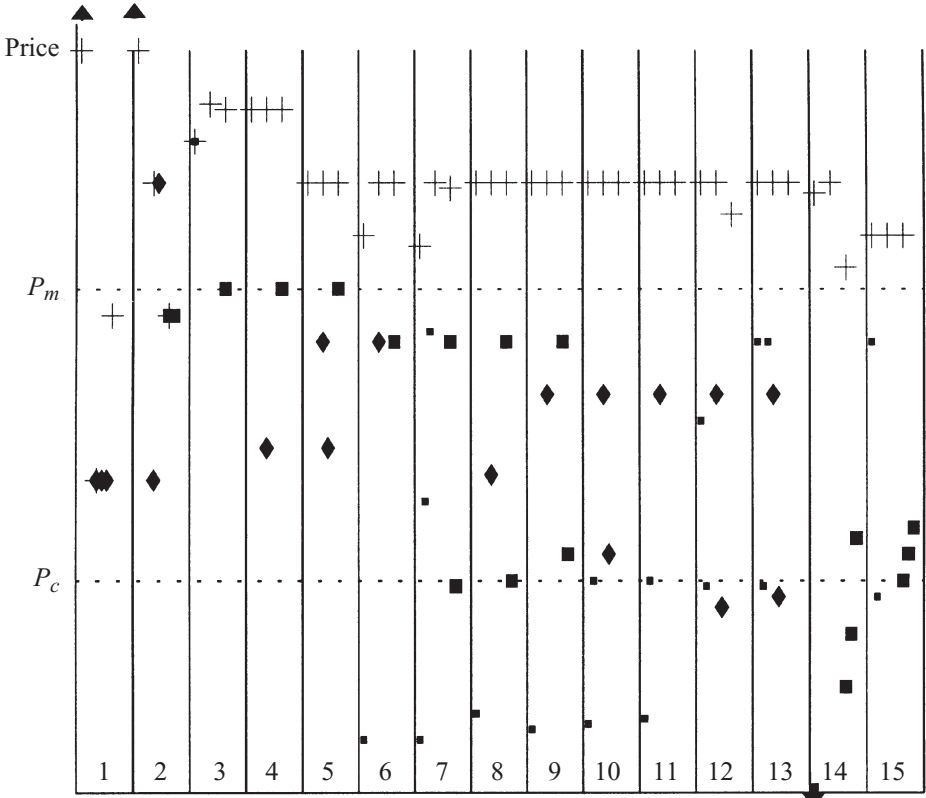


Fig. 7. Price Sequence for Session CDQ3. (Key: + price postings, ■ S1 contracts, ■ S2 contracts, ◆ S3 contracts)

Comparison of outcomes in the list-discount/quantity information (*CDQ*) treatment with the other treatments confirms the intermediate nature of the treatment. As shown in column (3) of Table 2, the *CDQ* treatment resembles the discount conspiracy (*CD*) treatment in that neither mean prices nor efficiencies differ significantly. Further, prices in the *CDQ* treatment are significantly lower than those observed in the posted-offer conspiracy (*CPO*) treatment. Nevertheless, conspiracies in the *CDQ* treatment are more successful than the standard list-discount conspiracies when evaluated in light of the baseline no-conspiracy (*NPO*) sessions. Prices and monopoly effectiveness differences between the *CDQ* and *NPO* sessions are larger, and statistically significant at a higher level of confidence than the comparable differences between the *CD* and *NPO* sessions. These observations motivate our third primary conclusion.

CONCLUSION 3: *Given discounting opportunities, ex post sales information tends to facilitate explicit conspiracies. Unlike conspiracies when discounting is possible, but where sales quantity information is not provided, prices and seller earnings are significantly higher than in baseline posted-offer (no-conspiracy) markets. Nevertheless, successful conspiracies are less frequent, and average transactions prices are*

significantly lower in list-discount/quantity information markets than in conspiracies without quantity information, when discounts are not possible.

4. Conclusions

When prices are posted on a take-it-or-leave-it basis, financially motivated subjects who collude are frequently able to discover and agree on a joint-profit-maximising price, despite cost asymmetries and incomplete information about demand and supply conditions. The possibility of secret discounts, however, creates monitoring and enforcement problems that can cause transactions prices to collapse, often to near-competitive levels. The importance of secrecy is indicated by the 50% incidence of successful price-fixing arrangements when discounting is permitted but sales quantity information is later revealed in a trade association report. The reports used in the experiments were *ex post* and did not identify specific buyers and terms. Such reports would probably pass a rule-of-reason test for legality, in the absence of other factors like prior price-fixing convictions. The implication is that antitrust hostility to agreements to limit or coordinate discounts is well founded, and that trade association activities should receive careful scrutiny in markets where collusion is suspected.

The laboratory results strike ringing parallels with anecdotal evidence from antitrust cases. For example, a decline in market share will often cause a subject to defect from the cartel agreement, and the prevalence of secret price concessions generally causes subjects to reduce list prices. Even the widely discussed electrical equipment conspiracy was subject to frequent chiseling and price wars.²⁴ In fact, defection on these explicit arrangements was so pervasive that turbo-generator manufacturers were probably more successful at raising prices after the conspiracy ended, using various implicit tactics to coordinate price movements and limit discounts (Lean *et al.* 1985).

In general, the subjects proposed and defected from agreements in a fairly resourceful manner. Given this resourcefulness and the severe limits that the software imposes on buyer/seller communications, the general inability of sellers to implement an effective conspiracy given the possibility of secret discounts is highly suggestive. This result is qualified, however by the ability of some cartels to use public sales quantity reports to maintain price discipline, even in the presence of incentives to discount. The primary lesson from this research, then, is twofold: First, explicit conspiracies can generate sizable price effects in markets that parallel many important features commonly found in natural markets. Second, the success of these conspiracies may be sensitive to relatively subtle institutional alterations, such as the opportunity to offer discounts, or the dissemination of sales information. The anti-competitive effects of other institutional arrangements, such as the revelation of transactions prices via meet-or-release clauses or reductions in

²⁴ 'The Incredible Electrical Conspiracy,' *Fortune*, April 1961.

cost heterogeneity via standardised freight pricing, remain topics for future research.²⁵

In closing we wish to stress the usefulness of experimentation in the study of illegal activities like price-fixing. Econometric evaluation of market pricing behaviour with naturally occurring data is often a challenge, but special problems of observability arise when the behaviour under investigation is illegal. Sometimes it is possible to infer when an agreement was in effect and who defected (e.g., Porter, 1983), and comparison of experimental and econometric results could be beneficial. For example, Porter and Zona (1993) suggest that collusion in sealed-bid auctions can be detected by a low correlation between costs and losing bids. In the laboratory, it is not necessary to infer who is involved in bid rigging on a particular contract, and the effect of collusion on bid/cost correlations could be independently verified.

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²⁵ Grether and Plott (1984) consider the effects of meet-or-release and other contractual provisions in the context of experiments with no opportunities for seller collusion. This paper was motivated by an FTC case, *In the Matter of Ethyl Corp.*, in which price fixing was suspected but not litigated.