Supra-Competitive Prices and Market Power
in Posted-Offer Experiments

Douglas D. Davis
Charles A. Holt
Anne P. Villamil

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Douglas D. Davis
Virginia Commonwealth University

and

Charles A. Holt
University of Virginia

and

Anne P. Villamil
Department of Economics
University of Illinois, Champaign, IL

ABSTRACT

This paper reports the results of a series of posted-offer market experiments designed to examine the effects of static market power on prices, when other factors such as excess supply, the number of sellers, and dynamic increase in posted-offer triopolies with complete demand information. The effects of market power are not as obvious in duopolies, since it is possible to observe considerable tacit collusion with only two sellers, even in the absence of market power.

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Douglas D. Davis, Charles A. Holt, and Anne P. Villamil[*]

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I. Introduction

Despite the marked tendency for prices in laboratory experiments to converge to competitive predictions, laboratory sellers are sometimes able to maintain prices above competitive levels.[1] A variety of factors have been associated with supra-competitive prices in laboratory markets. Plott and Smith (1978) showed that the rules of the market institution are important: prices are higher when sellers choose prices simultaneously in a posted-offer auction than when buyers and sellers make bids and offers sequentially in an oral double auction. Smith (1965) attributed price deviations during an adjustment phase of an oral double auction to a relatively low excess supply at supra-competitive prices, and Davis and Williams (1990) offer a similar conjecture about price patterns in posted-offer auctions. Dolbear et al. (1968), Isaac and Reynolds (1989), Wellford (1990), and others have reported that a decrease in the number of sellers tends to increase prices in several distinct trading institutions. Finally, Dolbear et al. find that prices are more likely to exceed noncooperative levels when sellers are given complete cost and demand information, as compared with the case in which they must learn about market demand and supply conditions through experience.

From an antitrust perspective, the standard approach to assessing the likelihood of supra-competitive prices is to calculate measures of

[1] See the survey in Holt (1989).

concentration, and then to consider factors that 'facilitate' collusion. Rather than viewing these factors as unrelated, a careful antitrust analysis should evaluate them collectively, in the context of the incentive structure of the underlying market game. In particular, the concept of market power is a useful way to organize the effects of qualitatively different characteristics. Market power is generated when alteration of a particular structural or institutional condition provides an incentive for one or more firms to raise price above a common, competitive price level.[2]

Since both the existence and recognition of market power may change with alterations in each of its underlying determinants, the purpose of this paper is to provide a careful analysis of the relationship between pricing behavior and market power, ceteris paribus. To do this, we designed a parallel series of experiments, with without market power, where the trading institution, excess supply, information conditions, and the number of sellers were held constant.[3]

Our primary focus is on market power in a static sense. This power exists when one or more sellers can increase <u>current</u> profit with a unilateral price increase above a common competitive level. It is not obvious, however,

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[2] A test for market power in this sense is based on the analysis of the profitability of a unilateral price increase when initial prices are perfectly competitive. This approach is basically motivated by the Department of Justice <u>Horizontal Merger Guidelines</u> test. A firm (or group of firms) are said to have market power if a small but significant price increase would be profitable.

[3] Structural changes that can affect market power have been investigated by others in laboratory markets. However, no research has isolated the effects of market power on supra-competitive pricing, independent of the number of sellers. This research differs from studies with explicit antitrust motivations, e.g., Isaac and Reynolds (1989) and Wellford (1990). In such studies, a merger-induced change in the number of sellers may have anticompetitive effects because it creates market power, so it is natural to change both numbers and market power.

that static notions of market power are particularly relevant, because both naturally occurring markets and laboratory markets evolve over time. In repeated market periods, sellers must compare the long-term gains from cooperating with the gains from a unilateral price reduction that may instigate a price war. A relatively unprofitable equilibrium outcome in the stage game can serve as a mutual punishment that is "triggered" by deviations from higher, collusive prices. In other words, noncooperative equilibria in the stage game can serve as punishments that support high levels of collusion in dynamic, "trigger-price" equilibria.[4]

Although noncooperative stage-game profits are typically higher in the presence of static market power, the absence of this power may actually increase market power in a dynamic sense. This is because the harsh penalty of competitive pricing may enforce collusive outcomes more effectively than is the case when static market power produces more profitable noncooperative outcomes that serve as mutual punishments. If this observation is valid, the antitrust implications are striking; markets that appear to be highly competitive, e.g. markets with large excess capacity, many sellers, etc., may in fact generate the least competitive price outcomes. With sufficiently low discount rates, dynamic market power is pervasive, and this motivated our decision to run parallel series of experiments, with and without static market power, but always with enough dynamic power to support perfect collusion in a trigger-price equilibrium.

Sections II and III below contain discussions of the experimental design and procedures respectively. The price data is analyzed in section IV. The

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[4] These equilibria are analyzed in Friedman (1971) and Green and Porter (1984), for example.

results indicate that static market power has a clear, even dramatic, effect in triopoly markets, but the effect in duopoly markets is less clear. The final section contains a summary discussion.

II. Separating Market Power Predictions

Figure 1 presents the four market structures used in the experiments. The market demand and supply functions in the four parts of this figure are determined by buyers' values and sellers' costs [see Plott (1989) for a discussion of standard procedures]. All values and costs are measured in penny deviations from the perfectly competitive price, which is normalized to zero. The experiments to be reported are posted-offer auctions, in which sellers choose prices and maximum quantity amounts simultaneously, and buyers may then purchase at the posted prices. In the analysis that follows, assume that buyers are ·passive price-takers who purchase all earnings-enhancing units at the best available prices, and who divide their purchases equally between sellers with equally low prices.

Consider first the 2-seller design with no market power, labeled 2SNP in the upper left-hand part of figure 1. Sellers' units are indicated on the market supply curve by seller designations, S1 and S2, for sellers 1 and 2 respectively. Each seller has one unit with a cost of -35 and one unit with a cost of 0. Two units are demanded inelastically at all prices less than or equal to 60. The range of competitive prices is from -35 to 0. When demand is divided equally at the maximum competitive price of 0, each seller sells 1 unit and earns profit of 35. Obviously, neither seller has static market power at a common price of 0, since a unilateral price increase by either would result in no sales and zero profits, and a price decrease would reduce

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earnings.

Market power can be introduced by increasing demand so that the high-price seller in the range of supra-competitive prices will not lose all sales. However, increasing demand in the 2SNP design reduces excess supply. As noted above, low excess supply itself may have an upward influence on prices. Therefore, we decided to increase demand and seller capacity together, in order to maintain a constant level of excess supply. In our two-seller/power design, labeled 2SP in figure 1, we doubled each seller's capacity and increased demand to 6 units to maintain a constant excess capacity of 2 units. When demand is divided equally at the (maximum) competitive price of zero, each seller earns 70 on the sale of the two low-cost units. Static market power exists in this design since a unilateral price increase now results in the sale of the two low-cost units, but at a much higher earnings level.

The noncooperative equilibrium in the 2SP design involves randomization over the range of prices that constitute an "Edgeworth cycle".[5] It is straightforward to determine this randomization range when prices are real-valued. The upper end of the Edgeworth cycle is a price of 60, where demand exceeds either seller's capacity by 2 units, so either seller can earn a sure profit of $2[60 - (-35)] = 190$. However, the best response to one seller's price of 60 is for the other to offer and sell 4 units at a price just below 60. Best responses to successively lower prices involve small price cuts, until price falls to 30, since selling 4 units at a price below 30 is not as profitable as selling 2 units at a price of 60. (Each seller always sells the two low-cost units, so profit comparisons are equivalent to revenue

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[5] The equivalence of the Edgeworth cycle and the support of the mixed distribution is valid in the present context, but not in general. See Holt and Solis-Soberon (1990) for a counterexample.

comparisons in this range, and 2 times 60 equals 4 times 30.)

Obviously, there is no pure-strategy equilibrium in this design, and noncooperative sellers would attempt to price in an unpredictable manner in order to avoid being slightly underpriced in the cycle. In equilibrium, sellers must be indifferent over all prices in the support of the mixed distribution, i.e. each price in this support must yield the security expected profit of 190 that results from a price of 60. Formally, let G(p) denote the probability that the price p posted by a seller i is the highest price posted in the market for a period. Since sellers i and j have identical profit functions, we will consider a symmetric mixed equilibrium in which G(p) is the distribution function for the common price distribution. If a price p is the highest price, the seller will sell 2 units, and will earn H(p) = 2[p + 35] = 2p + 70. If p turns out to be the lowest price, the seller sells two low-cost units and two high-cost units and earns L(p) = 2[p + 35] + 2[p + 0] = 4p + 70. Recall that G(p) is the probability that p is the higher price, so the expected profit is: G(p)H(p) + [1-G(p)]L(p). Since a seller must be indifferent between all prices over which randomization occurs, the function G(p) must equate the expected profit at each price in [30,60] to the security profit at p = 60. The resulting equation yields:

(1)   G(p) = [4p - 120]/2p .

As can be verified, G(30) = 0, G(60) = 1, and G(40) = .5 . Therefore, the median of the common mixed distribution is 40, and the expected profit is 190.

The analysis of the three-seller designs in figure 1 is analogous. In the no-power design, 3SNP, each seller earns 35 on the sale of a single low-

cost unit at the competitive price of 0. Each seller's capacity of 2 units equals the excess supply at supra-competitive prices, so there is no market power; a unilateral increase from a common competitive price will result in no sales or profits. The 3-seller power design, 3SP, is obtained by doubling each seller's capacity from 2 to 4 units and increasing demand from 4 to 10 units in order to maintain the 2 units of excess supply at prices in range [0,60]. The symmetric mixed distribution is calculated as before. First note that each seller can be sure of selling 2 units at a price of 60, and the security profit is 155 (profits of 95 on the single low-cost unit and 60 on the high-cost unit). If one of the triopolists posts a price p that turns out to be the highest price, this seller would sell two units and earn $H(p) = 2p + 35$. If the price is not the highest of the three prices, the seller sells four units and earns $L(p) = 4p + 35$. Again let $G(p)$ denote the probability of having the highest price, so the expected profit is $G(p)H(p) + [1 - G(p)]L(p)$, which when equated to the security level of 155, yields equation (1). Since $G(30) = 0$ and $G(60) = 1$, sellers in the 3SP design randomize in the range [30,60], as in the duopoly/power design. However, the median price falls from 40 to 34.[6]

The analysis of each of the 4 designs has been static, and the expected profits from noncooperative behavior that have been derived are summarized in the (N) row of Table 1. Now it is useful to consider the other extreme, perfect collusion, which may be supported in each of the above designs by the use of noncooperative stage-game equilibria as punishments for defection. In

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[6] Calculation of the median price in the triopoly is a bit more involved than in the duopoly since $G(p)$, the probability that p is the highest price, is now the probability that both of the other sellers price below p. If $F(p)$ denotes the common price distribution, $G(p) = F(p)^2$, and therefore, $F(p) = .5$ when $G(p) = .25$, or when p is approximately 34.

the 2SNP design, for example, if each seller picks the collusive price of 60 and limits quantity to 1, then they each earn 95. Now consider the incentive for a seller to defect from this collusive arrangement. The defector could sell 2 units at a price just below 60, for a profit of about 120 + 35 = 155. The profits for collusion and defection in the other treatments are calculated similarly, and are shown in the (C) and (D) rows of Table 1 respectively. For each treatment, D > C > N.

In a trigger-price equilibrium, defection is followed by noncooperative play forever. If x represents the constant probability of continuation to the next period, the potential defector would earn D in the period of a defection and would earn N in all subsequent periods, for an expected profit of D + Nx/(1-x). This expected profit for defection (and punishment) is less than the expected profit from collusion, C/(1-x), if x > [D - C]/[D - N]. This latter ratio, which will be called the Friedman coefficient, is calculated in the bottom row of Table 1.[7] By making the probability of continuation exceed .5, it is possible to provide dynamic market power in all designs. Moreover, the Friedman coefficient is the same for both paired power/no-power designs.

III. Procedures

We conducted 4 experiments for each of the four designs, for a total of 16 posted-offer experiments. Half of these sessions (2 sessions in each of the 4 treatment cells) were conducted at the University of Illinois (UI), and

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[7] See Friedman (1971).

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used the PLATO implementation of the posted-offer institution.[8] The remaining eight experiments were conducted at Virginia Commonwealth University (VCU), and used a networked-PC implementation of a posted-offer, written by Davis. All participants in the VCU experiments were experienced in the sense that they had previously participated in computerized posted-offer experiments, but with different cohorts of subjects and experimental designs.[9] Participants in the UI experiments were inexperienced. All seller participants were undergraduate business students, who were paid a standard $3.00 appearance fee, plus earnings from trades in the experiments. Buyers were simulated.[10] Sellers earned money by selling units at prices above their costs. Earnings varied widely (between $7 and $55 per subject), but from all external indications, subject motivation was excellent.

Identification codes for the 16 experiments listed in table 2 are interpreted as follows: The number of sellers is indicated by 2S or 3S, the presence or absence of static market power is indicated by P or NP, and experience is indicated by an X. Finally, the last digit of each code is a number 1-4, which distinguishes replications within treatment cells. Thus, 2SNP1 refers to the first experiment in the 2-seller/no-power treatment.

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[8] Sellers in a posted-offer auction choose prices and maximum sale quantities simultaneously. After prices are displayed publicly, buyers are selected in a random sequence and are given the opportunity to purchase at the posted prices, up to the maximum sales quantities. For example, see Ketcham, Smith and Williams (1984) for a description of the PLATO posted-offer program.

[9] No VCU participant was in more than one of the experiments reported in this paper.

[10] Buyers are simulated because we are primarily interested in the strategic behavior of sellers, when buyers have no countervailing power, e.g., when there are many small buyers. In fact, buyers in posted-offer experiments typically reveal demand fully [Ketcham, Smith and Williams (1984)]. However, complete public information about costs and values may encourage strategic withholding, especially with few buyers [Davis and Williams (1989)].

With the following notable exceptions, laboratory procedures were standard for computerized posted-offer environments [see Ketcham, Smith, and Williams (1984)]. First, although each participant proceeded though interactive instructions presented at their own terminal, the instructions were also read aloud by the experimenter.[11] Second, explicit information regarding demand limit prices, as well as the purchasing decision of the simulated buyers, was presented to participants, using an appropriate version of the following message:

> The buyers' side of the market in today's experiment is simulated. There is a single buyer. This buyer will purchase __ units at prices equal to or below $____, and will purchase no units at any price above $____. If seller prices do not exceed $____, the buyer will make purchases first from the seller posting the lowest price, then from the seller with the second lowest price, and so· forth. If two sellers post identical prices, then the buyer will use the throw of a die to choose which seller to approach first. Once a seller has been selected, the buyer will purchase all units that can be afforded from that seller. If the buyer finishes making purchases from one seller and still has more units available, then the buyer will switch to the seller with the next lowest price, and so forth.

This information was provided to more closely approximate the informational assumptions underlying the static and dynamic game theories used to generate the equilibrium power predictions.[12] [13] Third, rather than stop each

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[11] Our experience in other contexts suggests that reading instructions aloud facilitates learning, and it may also increase common knowledge regarding procedures.

[12] Participants were not explicitly told that all other sellers had identical costs, although they may have assumed this.

[13] The rationing procedure implemented in the simulated buyer announcement, does not divide demand when prices are equal. Consequently, the competitive equilibrium price of zero is not a noncooperative equilibrium outcome, since each seller has an incentive to reduce price slightly to ensure

experiment at a preset time for the entire session, or after a given number of trading periods, we employed the following stopping rule: The experiment proceeded uninterrupted for 15 periods. After period 15 and each subsequent period, a 6-sided die was rolled, and the experiment was continued if the outcome was 1-4 and stopped otherwise. Running uninterrupted for 15 periods gave us a standard data set in all experiments. The 2/3 probability of continuation was high enough to avoid end-period effects, i.e., it exceeds the Friedman coefficients shown in table 1. Although subjects were recruited for 2 hours, experiments always ended at least 40 minutes early, and thus it was extremely unlikely that the experiment would require more time than had been scheduled on a single day. The stopping rule was publicly announced in advance.

IV. Results

Before discussing specific results, we will discuss the way that the data are presented. First, we report posted prices rather than contract prices, because the static, mixed-strategy predictions derived in section II pertain to posted prices. Second, we report the price medians rather than means, since the median was calculated explicitly, and it places less weight on a few very low and very high contract price postings in the inexperienced

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the sale of the seller's low-cost unit. A mixed strategy equilibrium exists for both the 2 and 3 person games, and each is very close to the competitive equilibrium: The median for the 2SNP design is .5 cents, while the median price for the 3SNP design is 2 cents. The mixing distributions are calculated exactly as in the 2SP and 3SP designs, with security earnings equal to 35 (since either seller can ensure a profit of approximately 35 by pricing slightly below 0. In either design, each seller earns competitive profits (35) on average, and the calculations in Table 1 are unaffected. Details are available on request.

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trials.[14] Finally, we pool the data from the experiments done at the two locations (with different experience levels and computer PO implementations), since there is no statistically significant location effect.[15]

Table 2 presents the medians of the posted prices for each of the 16 experiments for periods 11-15. When the four experiments in each of the four treatment cells are pooled, the median prices for each period are plotted in figure 2(a) for the 2SP and 2SNP treatments, and in figure 2(b) for the 3SP and 3SNP treatments. In each figure, the competitive price is drawn as a dotted extension from the relevant no-power design, illustrated on the left side of the chart, while the lower and upper bounds of the static mixing distribution (deviations of 30 and 60) are drawn as dotted extensions from the relevant market power design, shown in the right side of the chart. Median prices for the market-power design experiments are summarized in each figure by a heavy bolded line, while median prices for the no-power design experiments are summarized by the lighter line that connects a series of dots.

From examination of figure 2(b), it is clear that power dramatically affects performance in the 3-person experiments. The lower bound of the mixing distribution cleanly separates median contract prices, with prices approaching the limit price in the 3SP experiments and approaching a level of 10-15 cents over the competitive prediction in the 3SNP experiments. As illustrated in figure 2(a), the median posted price path for the 2SP

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[14] Post-experiment comments by participants reveal that some of these aberrant price postings were keystroke errors.

[15] Using the median of prices across periods 11-15 for each experiment as data points, a test of the null hypothesis of no experience effect generates a t-test statistic of 0.62. A test of the same hypothesis using the unit normalized version of the nonparametric Mann-Whitney test generates a test statistic of 0.74. Neither statistic allows rejection of the null hypothesis at conventional significance levels.

experiments also lies above the median posted price path for the 2SNP experiments. However, unlike the triopoly results, differences across power treatments are small in the duopolies, and the lower bound of the mixing distribution does not separate the power treatments nearly as well.

These data plots suggest several principal issues, which can be stated as hypotheses about coefficients of a regression model. The dependent variable will be the deviation of the median price from the competitive price, denoted: $P_{med} - P_e$. Some cohorts of subjects are more competitive than others, and therefore, prices in successive periods of a given experiment are unlikely to be statistically independent. Consequently, we decided to use each of the 16 experiments to generate an independent observation, by calculating the median of posted prices for periods 11-15 for each experiment. The two independent variables are zero-one dummy variables: $D_p = 1$ when static market power is present, and $D_2 = 1$ for the duopoly experiments.

The symmetric specification in equation (2) is convenient in that it allows identification of static and dynamic market power effects in both the two-person and three-person designs.

$$(2) \quad P_{med} - P_e \quad = \quad [B_2 + B_{2p}D_p](D_2) \quad + \quad [B_3 + B_{3p}D_p](1-D_2)$$

The coefficients, B_2 and B_3, are measures of tacit collusion in the 2-seller and 3-seller treatments respectively, while the coefficients B_{2p} and B_{3p} capture the incremental effects of introducing static market power. To see this, suppose that there are two sellers ($D_2 = 1$), so the right side of (2) becomes: $B_2 + B_{2p}D_p$. A positive and significant value of B_2 would indicate that duopolists are able to collude tacitly to maintain prices above

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competitive levels, whether or not market power exists, and B_{2p} measures the additional effect of introducing (static) market power in a duopoly. A similar interpretation applies to B_3 and B_{3p}.

The primary null hypotheses to be considered are:

	duopoly	triopoly
no tacit collusion:	$B_2 = 0$	$B_3 = 0$
no market power effect:	$B_{2p} = 0$	$B_{3p} = 0$
perfect collusion:	$B_2 = 60$, $B_{2p} = 0$	$B_3 = 60$, $B_{3p} = 0$.

The perfect-collusion hypothesis is based on the exercise of dynamic market power, since sellers are theoretically able to maintain collusive prices with trigger-price strategies in all four treatments.

The OLS estimates of the parameters of equation (2) are:

$$(3) \quad P_{med} - P_e = \underset{(3.69^{**})}{[23.6} + \underset{(0.875)}{7.87D_p]}(D_2) + \underset{(2.30^{*})}{[14.75} + \underset{(3.80^{**})}{34.75D_p]}[1-D_2]$$

with $\bar{R}^2 = .46$, $F_{(3, 12)} = 5.34^{*}$, and t-test statistics (for the null of no difference from zero) printed below coefficient estimates. A single asterisk by a test statistic indicates rejection of the null at a 95% confidence level, while a double asterisk indicates rejection of the null at a 99% level (direction not predicted). These results provide statistical support for our principle conclusions.

Market Power: First, the positive signs on the B_{3p} and B_{2p} coefficients suggest that static market power generally increases prices. The large and

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statistically significant coefficient for the B_{3p} term indicates that static market power effects are only important in the 3-person markets, but the coefficient for the B_{2p} is smaller and is not significant. The small marginal effect of introducing market power in duopolies is probably due to the fact that, with only two sellers, it is sometimes possible to maintain prices above static noncooperative levels solely on the basis of mutual trust or implicit threats (see the discussion of tacit collusion below).

Conclusion 1: Static market power affects median prices, particularly in the 3-person experiments.

Randomization: One obvious question is whether the supra-competitive pricing in the power designs is due to randomization. In figure 2(a), median prices are below the predicted level of 40 in the 2SP experiments. In figure 2(b), median prices exceed the predicted level of 34 in the 3SP experiments, and $B_{3p} + B_3 = 49.5$, which is far above the median price of the mixed distribution for the stage game. Reexamination of median prices by experiment in table 2 provides further evidence of a lack of predicted randomization. For periods 11-15 of the 2SP and 3SP experiments, median prices are near the level consistent with mixing in only one of the eight power design experiments (experiment 2SPX4). Aside from 2SPX4, prices in two of the remaining 2SP design experiments are well below the 40 cent prediction (2SP1 and 2SPX3) and (ignoring the posting error in 2SP2) are generally above it in 2SP2. In the 3SP design experiments, median prices exceed the 34 cent prediction consistent with randomization in every period of every experiment by at least 7 cents.[16]

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[16] Posted prices in Kruse, Rassenti, Reynolds, and Smith (1987) tended toward the mean of the randomization distribution, although the hypothesis of randomization could be rejected in most markets. Their design differed from the

Thus, our second conclusion.

Conclusion 2: Although participants are able to maintain prices above competitive levels in the market-power designs, they generally do not randomize according to the static Nash predictions in these designs.

Tacit Collusion: The significance of the B_2 and B_3 coefficients indicates the presence of tacit collusion, in the sense that prices are significantly different from zero, independent of the effects of static market power. Figures 2(a) and 2(b) provide additional evidence. In both no-power treatments, median prices clearly exceed zero, even in the later periods: Median deviations are on the order of 25 cents for periods 11-15 of the 2SNP experiments, and 10 cents for periods 11-15 of the 3SNP experiments. Also, as shown in figure 2(b), aggregate median prices for the last 5 periods of the 3-seller/power experiments range between 45 and 50 cents, clearly above the 34 cent deviation consistent with randomization. Median prices in the 2SP treatment, however, are on the order of 25-32 cents for the last 5 periods, substantially below the 40 cent median in the static mixed equilibrium.

Examination of median posted prices by experiment provide some insight into differences in the nature of the deviations. With the exception of the 3-seller/power cell, where median prices are almost uniformly near the 60 cent limit price, there are particular instances where prices far exceed those observed in the other experiments in the treatment cell (e.g. 2SNPX3, 3SNPX3, and 2SP2, except for period 14). These occasional instances suggest the tacit conspiratorial behavior that might generally be expected in thin markets.

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design used in this paper, and sellers in their experiments were not given complete demand information. We believe that the critical difference is the difference in demand information, and we have been told by Kruse et al. that higher prices are more prevalent in their subsequent (as yet unreported) experiments with complete information.

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Also, the presence of high prices in 2SP2 suggests that there is nothing particular about the 2SP design that hinders tacit conspiracy. Perhaps more curious are the persistent deviations above both the competitive price and the median price of the noncooperative equilibrium observed in the 3SP experiments. However, with the exception of these 3SP experiments, there is no evidence of perfect collusion resulting from the full exercise of dynamic market power, which existed in all four designs.

Conclusion 3: Although participants infrequently adopt a profit maximizing conspiracy, there is some evidence of tacit collusion in each of the treatment cells, and median prices exceed the noncooperative prediction in 3 of the 4 cells.

Market Power and the Number of Sellers: An obvious question is why market power has a more pronounced effect in the triopoly markets. The clean separation of price performance across the power treatments in the triopoly experiments illustrated in figure 2(b) also applies to the individual experiments, as may be seen by again referring to table 2. In each of the 3SNP experiments, all median prices are at or below 30 (in fact 16 of the 20 prices are 15 cents or less) while median prices exceed 30 in each of the experiments in the 3SP design.

In contrast, not only is the aggregate difference in prices across power treatments smaller in the 2-person experiments, but performance is much more heterogenous. In the 2SNP experiments there are both nearly competitive price postings (2SNPX4) and very high posted prices (2SNPX3). Similarly, in the 2SP experiments, there are both low median prices (2SP1) and high median prices (2SP2 and 2SPX4). It appears that, independent of static market power conditions in duopolies, sellers are occasionally able to tacitly conspire, and also occasionally price very competitively. Cooperative opportunities may

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increase with decreases in the number of sellers for standard reasons, e.g.,
with only two sellers it is easy to punish cheating directly. In addition,
increased rivalry may be a consequence of providing demand information. If
the sellers assume cost symmetry, they may start to view relative earnings as
more important than total earnings.[17] We summarize this discussion as a
conjecture:

Conjecture: The capacity to both directly monitor and punish competitors in
full information duopolies provides increased opportunities for tacit
conspiracy, and increased rivalry; and therefore generates much more variable
pricing performance.

V. Conclusion

Seller market power exists in a static sense when one or more sellers
can profit from a unilateral increase in price above the competitive level
(and consequently the competitive price is not a noncooperative equilibrium in
the stage game). In laboratory experiments, a small number of sellers with
market power are sometimes able to exercise it, especially in posted-offer
markets. In the duopoly and triopoly posted-offer experiments reported here,
market power is induced by having a relatively small (two-unit) excess supply
at supra-competitive prices, to ensure that price increases do not result in
large quantity reductions. An important question is whether the observed
supra-competitive pricing is a result of market power as a theoretical
construct, or whether it is an artifact of either small numbers or of small
excess supply above the competitive price. This question is addressed by
running control experiments in which market power is eliminated by
simultaneously reducing demand and giving each seller a lower capacity that

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[17] See, Holt (1989) for an expanded discussion of these effects.

just equals excess supply at supra-competitive prices. Although there is no static market power in the control, the probabilistic termination rule admits the possibility of "collusive" outcomes, supported by trigger price strategies, in both the power and no-power designs.

In the no-power treatments, observed posted prices significantly exceeded static noncooperative levels, which indicates some degree of tacit collusion in both the duopoly and triopoly markets. However, this supra-competitive pricing cannot be explained by the full exercise of dynamic market power, since prices were never close to the perfectly collusive level. The introduction of static market power is associated with higher posted prices, especially with three sellers. The observed prices in triopoly markets with power are even higher than the levels predicted by the static noncooperative equilibrium exercise of market power, which again indicates some degree of tacit collusion in these repeated market games.

The results in the duopoly markets are more variable; some duopolies generate high prices and others exhibit more rivalistic behavior.[18] One would expect more disperse results with fewer participants, since individual differences have a larger effect in small markets. In addition, incentive effects may induce variability in 2-seller pricing decisions. With a single rival, it is possible to direct a punishment at that rival without sending a message to a third person, which may facilitate tacit collusion. On the other hand, duopolists may be more likely to become envious and vindictive, which may cause some duopolies to end up being very competitive. These opposing tendencies could produce varied results across duopoly experiments.

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[18] Similarly, Dolbear et al. (1968) reported more variability in markets with 2 sellers than with 4 sellers.

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Table 1. Calculation of Profits and the Friedman Coefficient, by Treatment

seller/power treatment:	2SNP	2SP	3SNP	3SP
noncooperative profits (N):	35	190	35	155
collusive profits (C):	95	250	115	235
unilateral defection profits (D):	155	310	155	275
Friedman coefficient [D - C]/[D - N] :	.5	.5	.33	.33

Table 2. Median Posted Prices for Periods 11-15

2 Sellers, No Power

Period	2SNP1	2SNP2	2SNPX3	2SNPX4
11	19	22	26	-1
12	28	22	40	3
13	25	26	53	5
14	23	23	49	23
15	20	15	49	8

2 Sellers, Power

Period	2SP1	2SP2	2SPX3	2SPX4
11	17	52	35	40
12	13	42	34	50
13	13	29	38	40
14	18	-11*	29	30
15	21	60	21	21

3 Sellers, No Power

Period	3SNP1	3SNP2	3SNPX3	3SNPX4
11	2	20	15	15
12	2	15	15	15
13	2	13	30	14
14	2	11	30	15
15	2	9	30	10

3 Sellers, Power

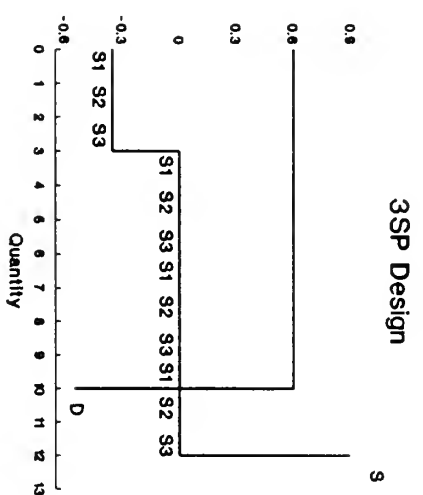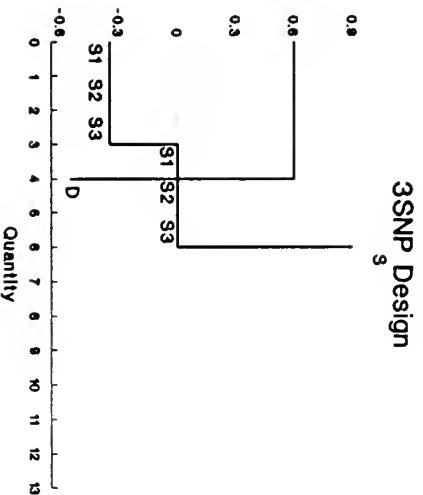| Period	3SP1	3SP2	3SPX3	3SPX4
11	57	47	51	35
12	63	45	53	60
13	59	45	52	48
14	59	44	53	41
15	55	43	54	60

* One participant commented that he mistakenly entered a price that was $1.00 below his intended price in this period. The median price for this period would have been 60 had the participant entered his intended price.

Figure 1

Supply and Demand Arrays

2SNP Design

2 Seller Designs

3SNP Design

3 Seller Designs

2SP Design

3SP Design
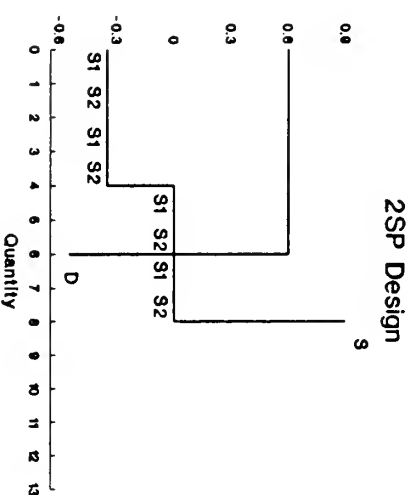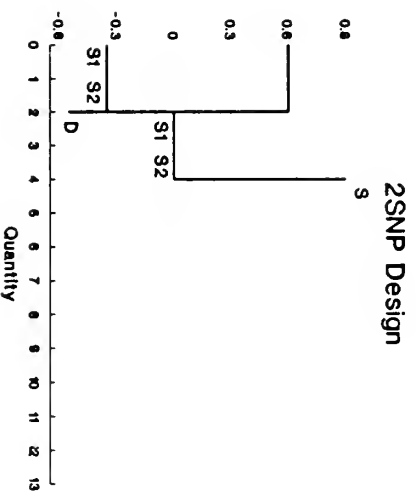
Figure 2(a)

Median Prices, 2 Seller Experiments

2SNP Design

2SP Design
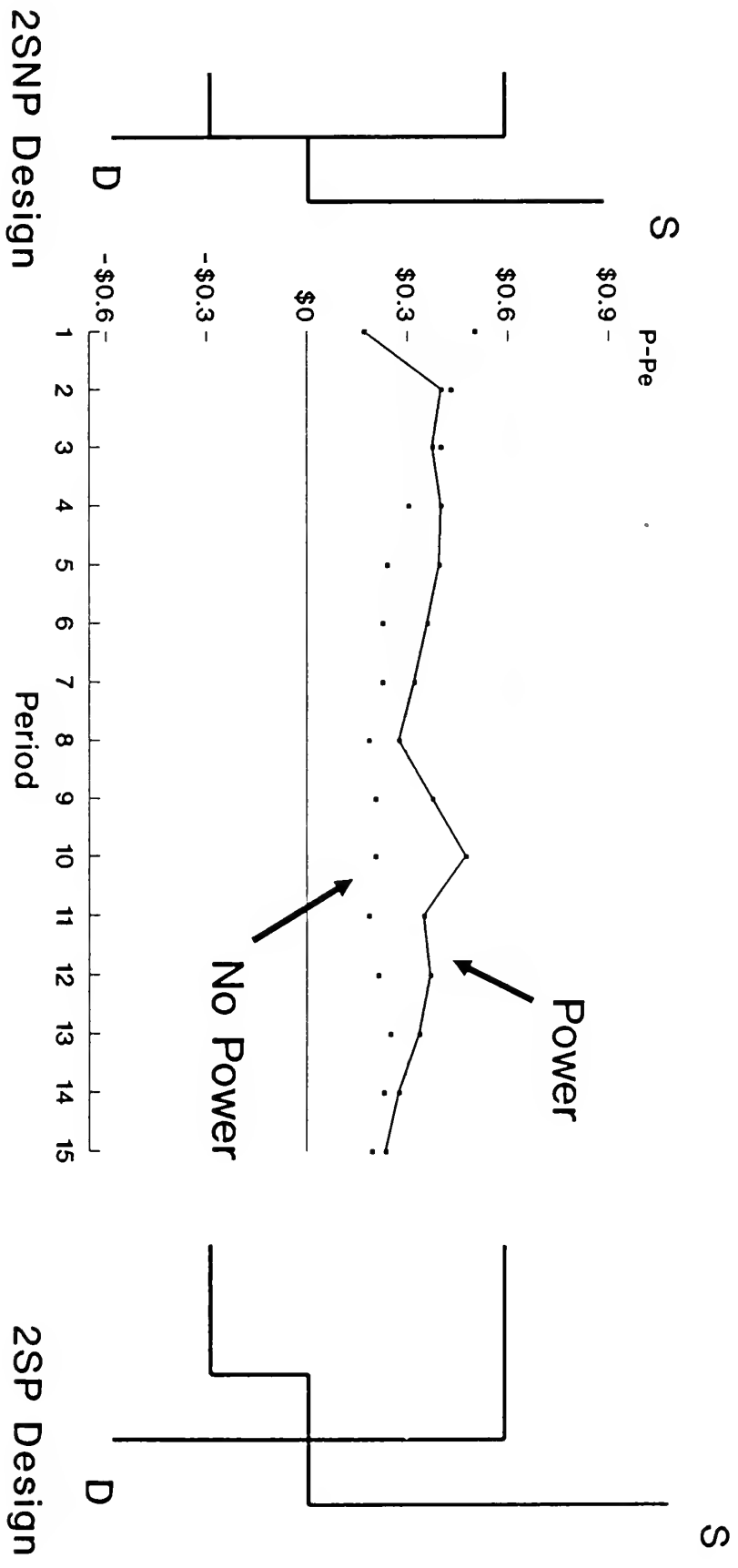
Figure 2(b)

Median Prices, 3 Seller Experiments

3SNP Design

3SP Design

P-Pe

Period

No Power

Power