

Cooperation without Coordination: Signaling, Types and Tacit Collusion in Laboratory Oligopolies

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Abstract: We study the effects of price signaling activity and underlying propensities to cooperate on tacit collusion in posted offer markets. The primary experiment consists of an extensively repeated baseline sequence and a ‘forecast’ sequence that adds to the baseline a forecasting game that allows identification of signaling intentions. Forecast sequence results indicate that signaling intentions considerably exceed those that are counted under a standard signal measure based on previous period prices. Nevertheless, we find essentially no correlation between either measure of signal volumes and collusive efficiency. A second experiment demonstrates that underlying seller propensities to cooperate more clearly affect collusiveness.

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1. Introduction

Although not illegal, “tacit” collusion, or coordinated pricing, is a primary concern of antitrust authorities.¹ In the United States, horizontal merger enforcement activity focuses extensively on preventing the formation of market structures that facilitate tacit collusion.² When assessing the likelihood of coordinated behavior, antitrust authorities attempt to identify factors that affect the capacities of competitors to establish a common price and to develop an arrangement-maintaining enforcement mechanism.³

Evaluations of the relative importance of such factors can help inform antitrust policy. Laboratory studies provide a useful complement to more standard empirical and theoretical work on tacit collusion, because the laboratory allows control over costs, demand conditions, information flows and seller communications unavailable in natural contexts. This control allows a direct assessment of the extent of supra-competitive prices and of the factors that facilitate such outcomes. In some laboratory market contexts, such as duopolies and market designs where some or all sellers have unilateral market power, supra-competitive prices arise frequently.⁴ More interesting for the study of tacit collusion are supra-competitive outcomes in market designs where tacit collusion is less expected. Experiments reporting supra-competitive pricing outcomes in ‘no power’ designs with more than two sellers include Cason and Davis (1995), Cason and Williams (1990), Davis and Korenok (2009), and Durham et al. (2004).

Curiously, the pricing patterns observed in these latter experiments exhibit none of the characteristics that antitrust scholars argue typify tacit collusion. Rather than establishing a common price and coming to some agreement as to how that price is to be

¹ This stands in contrast to cartelization or ‘explicit collusion’, which tends to be treated as illegal. In the United States, for example, cartelization is illegal *per se*, and may be prosecuted as a felony subject to treble damages and prison time.

² Assessing the likelihood of coordinated pricing post-merger is one of the two primary thrusts of horizontal merger analysis. The other enforcement objective pertains to the likelihood that a proposed consolidation will increase unilateral market power. See, for example, the *US Department of Justice and Federal Trade Commission Horizontal Merger Guidelines*, 1992 (revised, 1997).

³ Typically cited ‘suspect’ factors include product homogeneity, firm symmetry, the frequency of communications, multi-market contacts and the stability of demand.

⁴ In a review of experiments that focus on collusion, Haan et al. (2009) concludes that instances of collusion observed in laboratory markets arise most typically in industries with just two sellers. Examples of tacitly collusive outcomes in laboratory market designs with unilateral market power include Davis and Holt (1994) and Davis and Wilson (2000).

maintained, prices more typically vacillate substantially, both within and across markets, with prices ranging from near collusive to competitive levels.⁵

Two explanations for such ‘uncoordinated’ supra-competitive pricing have some appeal. The first pertains to within-market fluctuations. While sellers may not settle on fixed prices, they may, with the signals and responses sent through their pricing activity, develop a ‘language of coordination’ that allows the implementation and maintenance of higher prices. The bulk of the pertinent laboratory market studies focus on such signaling behavior. A second possible explanation for supra-competitive pricing pertains to inherent propensities of sellers to cooperate. In experiments studying the voluntary provision of public goods, underlying cooperative propensities have been identified as an important determinant of behavior (e.g., Fischbacher, Gächter and Fehr, 2001, Burlando and Guala, 2005, Kurzban and Houser, 2005 and Gunthorsdottir, Houser, and McCabe, 2007). Seller ‘type’ however, has received almost no consideration as a factor that might explain tacit collusion.⁶

Papers that study signaling activity in pricing games include Holt and Davis (1990), Cason (1995) and Cason and Davis (1995). In each of these studies sellers were given the opportunity to submit non-binding pricing recommendations prior to making binding price offers each period. Results uniformly indicate that such ‘cheap talk’ signals raise prices, albeit only temporarily.⁷ The limited duration of each of these studies (20 periods or less) represents one possible explanation for the failure of signals to affect overall cooperation levels. More recently Durham et al. (2004) study signaling activity in an experiment where actions are more extensively repeated (80 periods), but where sellers may send signals only via pricing decisions. Durham et al. observe supra-

⁵ As a theoretical matter, variable prices and quantities may be part of a trigger strategy equilibrium in an environment with demand uncertainty and imperfect information regarding rivals’ actions (e.g., Green and Porter, 1984). However, we are aware of no model of tacit collusion that has variable prices as part of an equilibrium strategy in a full information, constant cost and constant demand environment, where sellers possess no unilateral market power in the stage game.

⁶ Interestingly, the exceptions include some of the very first oligopoly experiments. Fouraker and Segal (1963) classify participants as ‘cooperative’, ‘profit maximizing’ and ‘rivalistic’. Hoggatt (1967, 1969) uses a more continuous classification scheme and examines the effects of ‘cooperative’ types against robot opponents.

⁷ Cason and Davis (1995) do find considerable evidence of persistent tacit collusion in the multi-market context they study, in which sellers were given pre-posting opportunities to submit price signals. However, as in the other papers, signaling opportunities appeared to play, at best, a minor role in raising prices.

competitive prices in many of their markets.⁸ Further, they report some evidence that price signals elicit at least immediate responses. However, while these investigators do not discuss the cumulative effect of signaling activity on cooperation levels, casual inspection of their results suggests that signaling volume is a weak predictor of cooperation levels.⁹

The way Durham et al. (2004) measure signaling activity represents one possible reason for the weak link between signaling activity and cooperation levels. Their experiment was not specifically designed to study signaling behavior, and to proceed the authors had to construct a relatively imprecise measure of signaling activity based on the relation between current period price posting decisions and the previous period's prices. As we discuss below, such a definition measures intended signals accurately only when seller expectations regarding rivals' price decisions are naively adaptive.

This paper reports an experiment designed to assess the effects of price signaling activity and player cooperativeness (type) on tacit collusion in posted offer markets. As a study of signaling behavior and its effects, our approach improves on Durham et al. (2004) by including more extensive repetition and by using a design that allows a more precise identification of signaling activity. We find that nearly all participants send signals, and that those signals tend to elicit immediate responses. However, similar to the previous literature, we also find little evidence that signaling activity is a predictor of overall collusive efficiency. On the other hand, seller propensities to cooperate do prominently affect overall collusiveness, and these propensities are relatively stable across markets.

The remainder of this paper is organized as follows. Section 2 overviews the experiment design. Section 3 explains experiment procedures. Section 4 reports experimental results. The paper concludes with a discussion in a short fifth section.

2. Experiment Design

We use here a variant of a 'swastika' design initially studied by Smith and Williams (1990) which we examine in an extensively repeated variant of the standard posted-offer

⁸ The market structure used by these authors undoubtedly drives some of their results. The experiment design includes fixed costs that assure sellers a loss in the competitive equilibrium.

⁹ See in particular the plot of signals and mean transactions prices for each market in figure 5 (Durham et al. 2005, p. 160)

institution. Posted offer rules both parallel many features of retail trade, and may be analyzed as a game of Bertrand-Edgeworth competition. Below we explain our implementation of the posted offer trading institution in subsection 2.1. The subsequent subsection 2.2 develops the market design, and subsection 2.3 explains our experimental treatments and conjectures.

2.1. The ‘Near Continuous’ Posted Offer Institution

Under posted-offer rules, the market consists of a series of trading periods. In each period sellers, endowed with unit costs, simultaneously make price decisions. Once all seller decisions are complete, prices are displayed publicly, and an automated buyer program makes all purchases profitable to the buyer at the posted prices. Figure 1 illustrates a screen display for a seller S1 in a computerized implementation of the posted offer market. As seen in the upper left corner of the figure, seven seconds remain in trading period 4. Moving down the left side of the figure, observe that in this period seller S1 has four units, each of which cost \$2.00. To enter a decision, a seller just types an entry in the ‘price’ box and presses ‘enter’.

In a ‘near continuous’ variation of this institution developed recently by Davis and Korenok (2009), the maximum length of decision periods is truncated sharply to only several seconds. The large number of periods allowed by this ‘near continuous’ framework very considerably increases sellers’ opportunities to both send and respond to price signals.

Numerical and graphical summaries of the previous period’s prices and earnings, as shown in Figure 1 help participants to process market results quickly in the near-continuous framework. The ‘Standing Prices’ displayed at center top of Figure 1 indicate that in just completed period 3, S1 posted a price of \$4.20 while S2 and S3 posted prices of \$4.60 and \$4.00, respectively. The bolded bars shown at the bottom center of the figure make it clear that S1 posted the second highest price in period 3. Further, comparison of the bolded bars to the light gray bars, which illustrate prices for the previous period 2, shows that sellers S1, S2, S3 all reduced their prices in period 3 relative to period 2. In period 3, S1 sold three of the four units he offered and earned \$6.60 (S1 also earned \$1.00 from the forecasting game, explained below), as shown on

the ‘Period Earnings’ bar chart. The earnings chart also indicates that seller S1’s earnings in period 3 fell relative to period 2.

2.2. Market Design

Figure 2 illustrates supply and demand arrays for the variant of the ‘swastika’ design used here. Three sellers are each endowed with 4 units with a constant cost of \$2 per unit. Aggregate supply is thus 12 units at prices in excess of \$2. The (simulated) buyer purchases seven units at any price of \$6 or less. Our implementation of the ‘swastika’ design differs from previous implementations in that we impose a minimum price of \$3 per unit. Given the excess supply of four units, in the competitive equilibrium all sellers post a price of \$3 and earn strictly positive expected earnings of \$2.33 per trading period.

The competitive equilibrium is the unique Nash equilibrium for the market evaluated as a stage game. To see that this outcome is an equilibrium, observe that earnings will fall to zero for any seller who unilaterally raises price above a common price of \$3. For uniqueness, observe that at common price above \$3 a seller could increase earnings by posting a price 1¢ below the common price. For any vector of heterogeneous prices above \$3, the highest pricing seller will sell nothing.

This design is useful for studying price signaling, for three reasons. First, previous research indicates that this design frequently stimulates tacit collusion (Cason and Williams, 1990, Davis and Korenok, 2009). Second, the simple demand and cost conditions help participants understand the underlying market structure, thus reducing the number of initial periods participants need to appreciate the incentives that the design induces. Third, this design, in conjunction with a forecasting treatment described below, helps isolate signaling activity.

2.3. Treatments and Conjectures

Our primary experiment consists of two treatments: a ‘baseline’ treatment, and a ‘forecasting’ treatment.

2.3.1. Baseline Treatment

In the baseline treatment, participants make price decisions in an extensively repeated market, using the supply and demand arrays shown in Figure 2. Each period lasts a maximum of 12 seconds. Decisions in the baseline treatment allow us to verify that tacit collusion persists in this variant of the swastika design. In particular, we are concerned

that the guarantee of positive earnings induced by our inclusion of a minimum admissible price does not undermine the tacitly collusive behavior observed in other markets. This is our first conjecture.

Conjecture 1(a): Tacit collusion in our variant of the ‘swastika’ design is resilient to the inclusion of a minimum admissible price that guarantees a positive profit.

Results of the baseline treatment are further useful in that they allow an analysis of the effects of other treatments, as discussed below.

2.3.2. Forecasting Treatment

To study signaling behavior, Durham et al. (2004) define a price signal as “any price submitted by any firm that is greater than or equal to the lowest posted price that failed to attract buyers in the previous period” (p. 155). This definition suffers the potential deficiency that it accurately identifies signals only when sellers have naively adaptive expectations, i.e., expectations based only on the immediately preceding period outcomes. If sellers form expectations differently, the Durham et al. definition may both include some price postings that were not intended to be signals, and may exclude other postings that were. For example, if prices are trending upward, their measure may errantly include as signals those prices posted by sellers who expect rivals to continue raising their prices. Reasoning identically, rivals may not regard such postings as signals. Similarly, if prices are trending downward, their measure would miss any signals sent by a seller who expects rivals to post lower prices, and attempts to interrupt or slow the downward trend by submitting price postings that either maintain current price levels, or decrease the price level by less than rivals.

In an effort to isolate price signaling in our laboratory markets independent of expectational assumptions, we introduce a ‘forecasting’ treatment where we directly elicit sellers’ expectations. In this treatment, sellers predict the maximum price their rivals will post in the subsequent period. If a seller’s forecast is within 5¢ of the subsequently observed maximum price posted by rivals, the seller earns a ‘high’ forecast prize of \$1.00. If the seller’s forecast is within 25¢ of the rivals’ maximum, the seller earns a ‘low’ forecast prize of 50¢. Otherwise the forecast prize is zero.

A review of the screen display in Figure 1 illustrates the presentation of the forecasting game to sellers. After posting a price, the cursor moves to the ‘forecast’ box.

The seller then enters a price forecast and presses ‘enter’. Forecast earnings are illustrated graphically as supplements to the period earnings bar. For example, in Figure 1, S1’s forecast for period 3 was within 5¢ of the rivals’ maximum \$4.30 so seller S1’s earnings for period 3 are supplemented by \$1.00, as indicated by the supplemental shaded box in the seller’s earnings chart.¹⁰

Although we did not expect the addition of the forecasting feature to influence market outcomes, we recognize that such factors may have unanticipated effects.¹¹ In particular, Croson (2000) observes that asking players to submit beliefs about the likely contributions levels of others (analogous to posting forecasts of others’ prices here) tends to reduce group contributions in a voluntary contributions game.¹² As a second conjecture we explore whether or not our forecasting procedure tends to reduce ‘collusive efficiency,’ or the extent of tacit collusion in a market.

Conjecture 1(b). *Relative to the initial sequence, eliciting forecasts does not affect collusive efficiency.*

The forecasting game, in conjunction with a design where the highest pricing seller sells zero units, allows a clean identification of signaling intentions: any price above a seller’s forecast maximum price will result in sales of zero, and would be irrational unless the

¹⁰ Our introduction of a forecasting game emulates the expectations elicitation techniques used in some early asset market experiments (e.g., Williams, 1987 and Smith, et al. 1988). Concerns about biasing pricing behavior with the forecasting game are somewhat diminished relative to this earlier literature because sellers here are unable to use their market decisions to influence their chances of winning the forecasting game.

¹¹ One additional minor procedural difference between the baseline and forecasting markets is that we obligated sellers to enter a new price and forecast each period, even if they wanted to repeat the same posting. (Absent this requirement, we might lose information regarding forecasts in those periods where sellers decided they didn’t want to post a new price). This difference is minor because participants tended to enter new price (and forecast) entries each period even when they repeated their previous period decisions. Periods ended either with the expiration of the maximum time limit, or when all participants posted prices. In both sequences, due either to impatience or an effort to increase hourly earnings, periods virtually always ended prior to the time deadline as a result of all sellers posting a price. Identification of sellers who changed price frequently in each treatment provides further evidence that the reposting requirement in the forecasting treatment did not stimulate added price volatility. In the baseline sequences, the average seller posted new prices in 76.3% of periods where prices were not initially competitive. In the forecasting sequences the average seller changed prices an almost identical 78.5% of periods.

¹² In a related experiment Wilcox and Feltovich (2002) were unable to replicate the Croson result. However, as Wilcox and Feltovich observe, their ‘elicitation’ treatment differed from Croson in that they required participants only to guess the number of players who would submit positive contributions each period, rather than to guess the expected contributions levels.

seller intends to send a signal, i.e. to invite rivals to raise their prices.¹³ This forecasting treatment permits us to evaluate the Durham et. al. signaling definition as a measure of signaling intentions. In particular, we posit

***Conjecture 2:** The Durham et. al. signaling definition provides a weak measure of intended signaling activity.*

We are also interested in the relationship between various measures of signaling activity and supra-competitive outcomes. This is a third conjecture.

***Conjecture 3:** Price signaling activity is a predictor of collusive efficiency.*

Two separate dimensions of conjecture 3 merit discussion. First is the question of whether or not rivals recognize signals and respond to them with immediately higher prices. The second dimension regards the link between overall signaling activity and collusive efficiency levels. Even if rivals tend to respond immediately to signals, signals will not necessarily have a lasting impact on overall collusiveness. Sellers, for example, might respond to signals but then quickly undercut each other, resulting in a price history that differs little from a competitive outcome. We will assess both dimensions of conjecture 3.

Note also that although price postings above a seller's forecast of maximum rival prices accurately measure signaling intentions, such postings may not necessarily be perceived as signals by the other sellers. Indeed, absent some message sent in conjunction with a price, virtually no price postings can unambiguously be regarded as signals.¹⁴ Here, we further use data from the forecast sequences to identify 'communicated' signals. We regard a forecast-based signal as a successfully

¹³ Conceivably, a risk-taking seller might post a price that exceeds his forecast (the expected maximum) placing a high weight on the potential for the rival maximum to fall above his expectations. For such an action to be optimal, however, the seller must be extremely risk-preferring. As shown in table A1 of an online appendix, the costs of such 'hedging' activity are at least half of the expected earnings from posting the profit maximizing price under a variety of distributional assumptions regarding a seller's beliefs about the expected actions of rivals.

¹⁴ Postings above \$6 (which occurred only rarely) are the only possible exception. Instances of price 'tagging' have occasionally been observed in the antitrust literature. For example in the *ATPCO* case, airlines allegedly annotated price postings with references that communicated pricing intentions to other airlines. For a discussion, see Borenstein (2004). Also, as a referee observes, price postings in excess of forecasts of rivals' maximum prices are arguably even less detectable as signals than are price postings in excess of previous period prices, since only the latter are delivered with respect to a publicly observable reference. As discussed at the outset of this subsection, however, postings in excess of previous period prices cannot be regarded either as intended or communicated signals unless seller expectations are naively adaptive.

‘communicated’ invitation to raise price if it exceeds the prices of both rivals (and is thus out of the money). In evaluating conjecture 3 we also assess the relationship between these communicated signals and collusive efficiency.

2.3.3. Collusive Efficiency and Propensities to Cooperate

Market outcomes may be importantly affected by the inherent propensities of participants to cooperate or compete. We assess this propensity by examining whether sellers aggressively undercut standing prices. In a market populated by sellers who do not aggressively undercut each other, even one signal might be sufficient to achieve a high degree of collusion. On the other hand, if sellers aggressively undermine each others’ attempts to cooperate, even frequent signals may fail to sustain a high degree of collusiveness.

Here we begin by classifying participants endogenously on the basis of their decisions in a set of preliminary markets, as has been done frequently in the VCM literature (e.g., Kurzban and Houser, 2005). A variety of other methods, including strategy elicitation (Fischbacher et al. 2001), elicitation of cooperative preferences in a related game (Offerman et al., 1996), and the use of questionnaires (Burlando and Guala 2004) have also been used in the VCM literature. However, given our uncertainty as to the characteristics that drive cooperative behavior in a market game, a classification scheme based on previous decisions in a similar environment seems a most reasonable starting point. In our design, high collusive efficiency levels in later markets that are populated by sellers who were, on average, ‘cooperative’ in the preliminary markets, would indicate of a link between stable propensities to cooperate and resulting collusive efficiency.

As stated above our design separates determination of cooperative propensity in a set of initial markets from the test of its predictive reach in subsequent markets. In each subsequent experimental market we duplicated the experimental design of the initial markets except that participants in a particular subsequent market were homogeneous in cooperative propensity. If cooperative propensities *determine* performance and are relatively stable across sessions, we should observe higher collusive efficiencies in markets composed of cooperative types than in the markets composed of competitive (non-cooperative) types. This is a fourth conjecture.

***Conjecture 4:** Individual propensities to cooperate, or seller ‘types’, can predict collusive efficiency levels. Markets comprised of sellers who have been identified as more cooperative will have higher collusive efficiency levels than markets comprised of sellers who were previously identified as less cooperative.*

3. Experiment Procedures

To evaluate the above conjectures we conducted the following experiment. In a series of preliminary sessions, nine (and in one instance twelve) player cohorts are invited into the laboratory to participate in three (four) triopolies. At the outset of each session a monitor randomly seats participants at visually isolated computers. The monitor then reads aloud instructions, as the participants follow along on printed copies of their own. Each *session* consists of two 120 period *sequences*, a baseline sequence and a forecasting sequence. Prior to the first sequence, instructions explain price-posting procedures, as well as the minimum admissible price of \$3.00. Participants are also given as common knowledge full information regarding underlying aggregate supply and demand conditions. To ensure that participants understand these underlying conditions the monitor elicits responses to a series of possible price postings. After giving participants an opportunity to ask questions, the first sequence begins, and consists of 120 periods each with a maximum length of 12 seconds. After period 120, the baseline sequence is terminated without prior announcement, and participants record their earnings.

Following the first sequence, a monitor remixes participants into entirely new groups, and reads instructions for a second sequence.¹⁵ Conditions for the second sequence match those in the first, except that the maximum period length is increased to 18 seconds and the forecasting game is added. After giving participants time to ask questions, the second sequence began. After 120 periods, this sequence ended, again, without prior announcement. Participants were paid privately the sum of their earnings for the two sequences, converted at U.S. currency at a rate of \$100 lab = \$1 U.S. plus a \$6 appearance fee, and were dismissed one at a time.

Participants were volunteers recruited primarily from upper level undergraduate business and economics classes at Virginia Commonwealth University. All participants

¹⁵ In the nine person cohorts, for example, markets in the initial sequences are ordered (1,2,3), (4,5,6) and (7,8,9). In the second sequence markets shift to (1,4,7), (2,5,8) and (3,6,9). Thus, for the second sequence, each participant is in a market with participants he or she has not been paired with previously.

were ‘experienced’ in the sense that they had previously participated in a near continuous posted offer session, but for a different study, with different supply and demand conditions. No one participated in more than one session.

Following the initial sessions, we ranked participants by their cooperativeness ‘type’ and invited groups of nine homogenously cooperative and nine homogenously competitive participants to participate in subsequent pair of sessions. Other than the homogeneity of participant types, procedures for the ‘experienced’ pair of sessions matched exactly the procedures in the preliminary sessions. The fact that rivals were of like types was not revealed to participants.

Earnings for the initial sessions, which lasted between 80 and 100 minutes, ranged from \$21 to \$46 and averaged about \$31. In the two experienced sessions, earnings ranged from \$21 to \$57 and averaged \$37.

4. Results

We present data in terms of a collusive efficiency index, which in period t of market j is

defined as $\psi_{jt} = \frac{\pi_{jt} - \pi_{NE}}{\pi_{JPM} - \pi_{NE}}$ where π_{jt} denotes realized aggregate period profits, π_{NE}

denotes static Nash (competitive) profits and π_{JPM} denotes earnings at the joint maximizing level. Collusive efficiency values range between zero at the competitive level and one in the joint profit maximizing outcome. In our design ψ_{jt} closely parallels mean transaction price paths.¹⁶

Figure 3 provides an overview of results for the baseline and forecasting sequences, respectively. Inspection of the panels in Figure 3 provides information pertinent to conjectures 1(a) and 1(b). The clearly positive $\bar{\psi}_t$ values for the baseline sequences indicate that markets do not collapse on competitive outcomes. Nevertheless, we recognize that classifying market outcomes as either ‘collusive’ or ‘competitive’ is to some extent arbitrary. No well-defined standard has emerged from the behavioral

¹⁶ In our market design collusive efficiency is essentially a linear transformation of mean transaction prices (e.g., $(\bar{p}_{jt} - 3)/(6 - 3)$, where 6 and 3 are joint maximizing and competitive prices, respectively). The only deviation occurs when more than one seller posts a price above \$6 in a period. We evaluate results in terms of collusive efficiency rather than transaction prices in order focus on sellers’ relative success as tacit conspirators. This ‘collusive efficiency’ index has been used to calculate the efficiency of tacit collusion and monopoly power exercise in a variety of contexts, under titles such as a ‘cooperativeness index’ (Potters and Suetens, 2008) and a ‘monopoly efficiency index’ (e.g., Isaac, Ramey and Williams, 1984).

oligopoly literature that identifies the level of deviations from static Nash predictions necessary for an outcome to be ‘collusive.’ Sellers in laboratory markets rarely settle on zero variance outcomes, and the current environment is no exception. In both our more ‘competitive’ and more ‘cooperative’ market sequences, high outcome variability was a persistent feature.¹⁷

To distinguish this inevitable noise from sellers’ success in raising prices, we adopt the convention of defining a segment of periods as ‘collusive’ if $\bar{\psi}_j > .10$, meaning that sellers on average extract more than 10% of the supra-competitive profits available from joint maximization. As indicated by the entries in column (4) of Table 1, using this measure we can reject with high significance the null hypothesis that baseline markets are competitive, for periods 1-60, periods 61-120, and overall, using the Wilcoxon test.¹⁸ At the same time, sellers in the baseline sequence were rarely particularly efficient conspirators. As seen in column (2) of Table 1, average collusive efficiency levels reached only 0.32 in the last 60 periods of the baseline sequences. Finally, as suggested by the breadth of the inter-quartile ranges for mean collusive efficiency rates, shown in Figure 3, outcomes varied widely across sequences. For the baseline sequences, $\bar{\psi}_j \leq 0.1$ in two of the 16 baseline sequences, and $\bar{\psi}_j > 0.5$ in another three sequences. Combined, these results represent a first finding.

Finding 1(a): Mean collusive efficiency levels above those consistent with a competitive outcome are frequently observed in our version of the swastika design where sellers earn strictly positive earnings in the competitive equilibrium.

Mean and inter-quartile ranges for collusive efficiency in the forecasting sequences, shown in the lower panel of Figure 3, parallel results for the baseline markets. Sellers again tended to achieve supra-competitive outcomes. As summarized in column (5) of Table 1, using the Wilcoxon test the null hypothesis that collusive efficiency is 10% or less can be rejected with high significance for the segments consisting of periods 1-60, periods 61-120, and overall. However, as with the baseline sequences, the width of

¹⁷ For example, only one of the 32 combined baseline and forecast sequences stabilized on a single-price outcome (this was a competitive baseline sequence). Mean contract price plots for individual baseline and forecast markets appear as Figures A-1 and A-2 in an online appendix.

¹⁸ In the discussion of results we term results as ‘highly significant’ if $p < .01$, ‘significant’ if $p < .05$ and ‘weakly significant’ if $p < .10$.

the inter-quartile ranges in the forecast sequences again suggests that outcomes were quite variable across markets. For example, in the forecasting treatment $\bar{\psi}_j \leq 0.10$ in two instances and $\bar{\psi}_j > 0.50$ in six instances.

Comparing the upper and lower panels of Figure 3 suggests that sellers exhibited somewhat higher collusive efficiencies in the forecasting sequences, particularly in the initial 60 periods. However, as indicated in column (6) of Table 1, the data are too dispersed to conclude that these differences are significant. Further, comparing mean entries in columns (2) and (3), observe that the overall lower collusive efficiencies of the baseline sequences are driven largely by outcomes in the first 60 periods. Since the baseline sequences uniformly preceded the forecasting sequences, the comparatively lower initial collusive efficiencies in the baseline sequences may quite possibly be attributable to learning effects. In any case, the forecasting treatment clearly does nothing to dampen prices in this design, as results by Croson (2000) would suggest. This is a second result.

***Finding 1b:** The addition of a forecasting treatment does not significantly affect collusive efficiency.*

These initial results allow us to focus more specifically on the larger issues: the effects of signaling and propensities to cooperate on collusive efficiency. We consider first signaling behavior.

4.2. Signaling and Collusive Efficiency

In this subsection we first compare our results with previously generated results by examining signals defined in terms of previous period prices and the effects of these signals on collusive efficiency. Then we consider why the accuracy of this measure might be questionable, and evaluate an alternative signal measure based on seller forecasts.

4.2.1. Signaling under the Durham et al. Definition

Applying the signal definition developed by Durham et al. (2004) to the present context, we define a signal s_d as a price posting that either exceeds either the maximum price

posted in the previous period, or the \$6.00 limit price.¹⁹ A first pertinent question regarding such signals is whether or not they elicited responses from sellers. Columns 1 and 2 of Table 2 present results of a regression of lagged transaction prices and signals, sent under definition s_d for the baseline and forecasting sequences, respectively.

Generally, we estimate in Table 2

$$p_{jt} = c + \beta_p p_{jt-1} + \beta_s I_{s^*,jt-1} + \varepsilon_{jt}, \quad (1)$$

where $I_{s^*,j,t-1}$ is an indicator variable of the signal of type s^* sent in market j during period t . In columns (1) and (2) signals s^* are s_d signals. To control for possible interdependencies within markets, we cluster by markets and estimate robust standard errors.

As the large and highly significant coefficients on I_{s_d} suggest, actions classified as signals sent under s_d tend to elicit next period responses from sellers, both in the baseline sequences (summarized in column 1) and in the forecasting sequences (shown in column 2). These results closely parallel results observed previously by Durham et al. (2004).

A broader question regards the relationship of signaling activity under the Durham et al. definition to collusion *levels* across markets. To assess relative signaling activity as a predictor of overall collusive efficiency across markets, we regress the number of s_d signals on mean collusive efficiency values for each market $\bar{\psi}_j$ for the initial and forecasting treatments combined. As a check for possible treatment effects we introduce both intercept ($fcst$) and slope ($s_d \times fcst$) terms for the forecasting sequences, as summarized in equation (2).

$$\widehat{\psi}_j = 0.25^* + 0.31 fcst + 0.001 s_d - 0.009 (s_d \times fcst) + \varepsilon_j \quad \bar{R}^2 = -0.02 \quad (2)$$

(0.145) (0.23) (0.005) (0.008) $n = 32$

As is clear from equation (2), signal volumes collected under s_d organize essentially none of the variation in collusive efficiencies observed across sessions. Other than the intercept, none of the individual parameters are significant, and for the regression as a whole, $\bar{R}^2 = -0.02$.

4.2.2. Are Seller Expectations Naively Adaptive?

¹⁹ In our design, the maximum price posted in a period is the lowest price that failed to yield a transaction (unless the maximum price was shared by two sellers). Price postings above \$6 can uniformly be interpreted as signals since sellers know that the buyer will purchase no units at such prices.

For price postings counted as signals under the Durham et al. definition to represent plausibly either intended or communicated signals, seller expectations about rivals' actions must be naively adaptive. The forecasting treatment allows insight into the extent to which this assumption is correct: a seller has naively adaptive expectations if the seller's forecast of his or her rivals' maximum price equals the maximum price that rivals posted in the previous period. However, a comparison of forecasts and previous period postings in the forecast sequences suggest that this is not the case.²⁰ Overall, sellers submitted forecasts consistent with naively adaptive expectations only 23% of the time. Further, many of these consistent choices occurred in markets where prices had collapsed on the \$3 competitive outcome. Confining attention to those periods where the maximum rivals' price exceeded \$3, sellers made choices consistent with naively adaptive expectations only 11% of the time. That seller expectations are generally not naively adaptive provides some basis for skepticism regarding the extent to which postings counted as signals under s_d reflect either signaling intentions, or instances where messages regarding cooperative intentions were communicated.

4.2.3. Forecast-Based Signals

The forecasting sequences allow a refined identification of signaling behavior. In these sequences we can isolate signaling intentions as price postings that exceed own forecasts of the maximum price that rivals will post in the upcoming period. Formally, we define a 'forecast signal' (s_f) as a price that is either in excess of a seller's forecast of rivals' maximum posted price for a period or above \$6.00.

The histogram in Figure 4 plots the incidence of postings counted as signals under s_f and s_d in the 16 forecasting sequences. As seen in the histogram, the set of postings counted as signals under s_d differ markedly from the set of signals identified as intended under s_f . While sellers on average sent less than 10 signals per sequence under s_d , they averaged nearly 21 per sequence under s_f . Further, signaling activity under s_f is far more dispersed. Under s_f 13 of 48 participants sent 30 or more signals, while under s_d no one sent more than 29 signals.

²⁰ A histogram plotting the frequency of forecasts consistent with naively adaptive expectations appears as Figure A4 of an online appendix.

The higher incidence of signals under s_f suggests that sellers' interests in cooperation may considerably exceed the levels indicated by s_d . Notably, in some circumstances s_f systematically captures intentions missed by s_d . In particular, while both s_f and s_d can capture efforts to encourage other sellers to raise price from a low level, only s_f can capture signals intended to retard the erosion of market prices from a high level. Further, unlike signals sent under s_f , many price postings interpreted as signals under s_d were 'unintended' in the sense that a seller expected rivals' prices to be higher than the previous period's maximum price. As seen in row (2) of Table 3, 28.3% of the signals sent under s_d (128 of 453) were 'unintended' in the sense that a seller posted a price above the market maximum for the previous period, but below their forecast of their rivals' maximum price.

Although the s_f signal definition reveals a high frequency of signaling intentions, we also observe that in many instance sellers may have failed to communicate their intentions. If a signaler underestimates the expected maximum price of his rivals, then his signal will fall below the actual maximum in the upcoming period, thus masking his signal. As summarized in row (3) of Table 3, 27.6% of signals sent under s_f (275 of 998) were 'masked' in the sense that the signal price is below the maximum price charged by rivals in the period of the signal. Forecast signals are not uniquely susceptible to being missed in this way. As seen in the right column of row (3) nearly 15% of signals sent under s_d (62 of 453) were similarly 'masked'. Given the above comparison of s_d and s_f signals, it is not surprising that 67.4% of signals sent under s_f (674 of 998) were missed by measure s_d , and that the average simple correlation between s_d and s_f is only 0.41, as shown in row (5). These observations form a second finding.

Finding 2: Signaling activity measured under s_f reveals a stronger interest in cooperation than is suggested by the use of the s_d measure. Further, s_d is far from a subset of s_f .

Further exploitation of forecast sequence data allows a refinement of forecast signals that can be interpreted as both intended and communicated. Define s_{fp} as a signal under s_f for which the posted price either (a) strictly exceeds the maximum price posted by rivals in period t or (b) exceeds \$6. Thus, price postings counted as signals under the s_{fp} measure are both intended (since they exceed a seller's forecast of the maximum

price), and can reasonably be viewed as communicated (since it is clear to all participants that the posting is out of the money).

A total of 631 signals were sent under s_{fp} , considerably less than the aggregate signal volume recorded under s_f (998), but considerably more than the aggregate volume recorded under s_d (453). As shown in column (3) of Table 2, the single period price response to s_{fd} signals (21¢) is less than half the single period response for s_d signals (45¢), a result that is unsurprising in light of the much higher volume of s_{fp} signals. Moreover the alternative definitions measure distinct activities: the average within-market correlation between s_d and s_{fp} signals is 0.32, and the correlation between s_d and s_{fp} signal volumes across markets is 0.46.

Nevertheless, s_{fp} signal volumes are no better predictor of collusive efficiency levels across markets than is s_d as equation (3) indicates.²¹

$$\hat{\psi}_j = 0.35^* + 0.001 s_{fp} + \varepsilon_j \quad \bar{R}^2 = -0.07$$

(0.19) (0.004) $n = 16$ (3)

The small and insignificant coefficient on s_{fp} in (3) suggests that even volumes of ‘communicated’ signals do nothing to explain collusive efficiency across markets. This is a third finding.²²

Finding 3: *Price signals tend to elicit increases in transaction price responses in the immediately following periods. However, even restricting attention to ‘communicated’ signals, signal volumes and overall collusive efficiency levels are uncorrelated across markets.*

Importantly, we are not concluding with finding 3 that signaling activity does not affect prices. To the contrary, holding sellers constant within market, we expect that increased volumes of signals may often yield increased levels of cooperation. Rather, we

²¹ Notice that regression equation (3) involves only the forecasting sequences. For that reason, it does not include intercept and slope dummies that distinguish baseline and forecast sequences, as in (2).

²² As an alternative measure of ‘communicated’ signals we counted as signals those price postings that exceed *all* sellers’ forecast of the maximum price for the period. This ‘ s_{f-all} ’ measure is communicated in the sense that it exceeds every sellers’ expectations. Nevertheless we view it as inferior to the s_{fp} measure discussed in the text in that s_{f-all} signals are not communicated as common knowledge – a seller may intend to send a signal, but unless the posting meets some publicly observable criterion (such as being out of the money, as with s_{fp}) the sender does not know that the message was received. In any case, signal volumes sent under alternative s_{f-all} measure do no more to explain collusive efficiency than signals measured under

s_{fp} . Regressing collusive efficiency on s_{f-all} yields. $\hat{\psi}_j = 0.36^{**} + 0.001 s_{fp-all} + \varepsilon_j \quad \bar{R}^2 = -0.07$
(0.14) (0.005) $n = 16$

suggest that the differing reactions of seller cohorts to signals across markets may explain the absence of a pronounced relationship between signal volumes and collusive efficiency overall. Even if a price unambiguously exceeds a responder’s expectations for the period’s maximum price, he (the responder) must decide whether the ‘signal’ is merely an effort by her (the signaler) to lure him into raising prices that she would undercut. Further, even if he believes in the sincerity of her intention to cooperate, he may prefer to raise current period profits by undercutting a price leader rather than attempt to participate in some sort of longer term coordinated outcome.²³

The propensity of sellers to “cooperate” or compete non-aggressively may play as large or even a larger role in determining the establishment of a successful collusive arrangement than signals themselves. We address this issue in the next subsection.

4.3 Seller Types and Collusive Efficiency

As was the case in our discussion of signaling activity, cooperation may potentially be measured in a number of ways. Among other possibilities, ‘cooperative’ sellers may be those who tend not to undercut the maximum price in the immediately preceding period, those who tend to shade under the maximum price in the preceding period by only a small margin, or those who tend not to undercut the second highest price in the preceding period. In Table A2 of an online appendix we consider each of these possibilities and find that a cooperativeness measure based on a seller’s tendency to undercut the previous period maximum only slightly at most, best organizes observed collusive efficiency levels in the preliminary markets.²⁴

More precisely, we define a ‘price leader minus five’, or p_{l-5} index as the percentage of periods that a seller posted a price above \$3, and was either above \$6, or no more than 5¢ below the previous period maximum price. Figure 5 plots mean collusive

²³ Inspection of s_{fp} volumes and collusive efficiency for some specific markets supports the conjecture that signal responses differ importantly across markets and may thus affect collusive efficiency. In the forecast sequence with the highest observed collusive efficiency ($\bar{\psi}_j=0.77$) only 15 s_{fp} signals were sent, suggesting that generally non-aggressive competition rather than signaling activity explains the high observed collusive efficiency. At the same time, in the sequence with the third highest s_{fp} signal volume (56 signals) collusive efficiency was only $\bar{\psi}_j=0.07$.

²⁴ The increase in explanatory power associated with using the p_{l-5} measure over the alternatives mentioned in the text is marked. Regressions of collusive efficiency on these alternative measures of ‘type’, similar to the regression reported in equation (4) yield no individual parameter estimates that approach significance, and in each case $\bar{R}^2 \leq 0.18$.

efficiencies against the average p_{l-5} values per market, \bar{p}_{l-5j} . The scatter-plot illustrates the impressive organizing power of the p_{l-5} measure. Regressing the observed collusive efficiency levels on \bar{p}_{l-5j} for each market j yields the estimate summarized as equation (4)

$$\hat{\bar{\psi}}_j = -0.14 + 0.11 fcst + 2.09^{***} \bar{p}_{l-5j} - 0.26 (p_{l-5j} \times fcst) + \varepsilon_j \quad \bar{R}^2 = 0.63 \quad (4)$$

(0.11) (0.13) (0.50) (0.59) n = 32

The large and highly significant coefficient on \bar{p}_{l-5j} in equation (4) reflects the high correlation between mean p_{l-5} values for each market and collusive efficiencies. Observe further that the forecast treatment does not significantly affect either the intercept ($fcst$) or the slope ($fcst \times slope$) of this relationship. As indicated by \bar{R}^2 in (4) the regression explains 63% of the variation in mean collusive efficiencies, a vast improvement over the negative \bar{R}^2 's in equations (2) and (3).

Although we do not at this point argue that with (4) we have established a causal relationship between our ‘price leader minus five’ cooperativeness measure and collusive efficiency, two features of this cooperativeness measure do merit comment. First, we emphasize that the average \bar{p}_{l-5j} measure as a predictor of collusive performance is in no sense tautological. On the contrary, high \bar{p}_{l-5j} values could be associated with very low $\bar{\psi}_j$ outcomes, and vice versa. For example, if all sellers persistently posted prices just above the \$3 minimum \bar{p}_{l-5j} could take on a value of 1 with $\bar{\psi}_j$ was near zero. Alternatively very low \bar{p}_{l-5j} values easily can be associated with a near unitary $\bar{\psi}_j$.²⁵ Indeed, looking again at Figure 5 notice that despite the generally high correlation between collusive efficiency and \bar{p}_{l-5j} , we observed some significant outliers, including a forecast sequence with a collusive efficiency of $\bar{\psi}_j = .75$ and a $\bar{p}_{l-5j} = .21$ (highlighted in the figure with a circle), and a baseline sequence with a collusive efficiency $\bar{\psi}_j = .39$ and a $\bar{p}_{l-5j} = .29$ (highlighted in the figure with a box).

²⁵ For example, $\bar{\psi}_j = .091$ and $\bar{p}_{l-5j} = .10$ if all sellers dropped in 6 cent increments from a uniform price \$6 to \$5.46 over a series of repeated 10 period cycles.

Second, we observe that *individual* $p_{l-5,i}$ measures are quite highly correlated across sequences ($\rho_{p_{l-5,i}^{base}, p_{l-5,i}^{fst}} = .55, p < .001$), suggesting that the p_{l-5} index may be a fairly stable measure of seller type. Indeed, given that sellers are remixed into completely different groups each sequence, this correlation may understate the stability of seller propensities to act cooperatively. A seller may be quite cooperative by nature, for example, but may give up on cooperative efforts in a sequence where her rivals are extremely competitive.

To assess whether seller “type” is a predictor of collusively efficiency we conducted a second experiment, using participants from the first experiment. For this second experiment we classified individuals on the basis of their average cooperativeness measures in the two initial sequences ($\bar{p}_{l-5,i}$), and then invited those identified as ‘cooperators’ and ‘competitors’ back to form new experimental cohorts.

The $\bar{p}_{l-5,i}$ measure permitted good type separation in the initial markets. Overall, $\bar{p}_{l-5,i}$ values in the first session ranged from 0.03 to 0.55 and averaged 0.22. For the nine participants selected to form our ‘competitive’ cohort in the subsequent session, the index averaged 0.13, with a maximum of 0.21. For the nine individuals selected to form our ‘cooperative’ cohort in the second, the index averaged 0.40 with a minimum of 0.27 and a maximum of .55. Except for the homogeneity of types within groups (which was not known to participants) procedures for these second subsequent markets duplicated exactly those used in the preliminary markets.

Figure 6, which plots $\bar{p}_{l-5,i}$ values in the second session against comparable values in the preliminary session, summarizes results of these out of sample (subsequent) markets. Type appears to be quite stable across sessions. Initially competitive and cooperative types form separate clusters in the second session roughly along the 45° line. The correlation between initial session and subsequent session $\bar{p}_{l-5,i}$ values is a highly significant $\rho = 0.69$ ($p < 0.001$).

Further, differences in propensities to cooperate powerfully impact collusive efficiency levels. The mean collusive efficiency level in the six markets with ‘cooperative’ types (76.4) is almost four times the mean cooperation rate in the six

markets with competitive types and is more than double the mean rate in the 32 preliminary markets (34.2) in which cooperative and competitive types were not controlled for. These results combine to form a fourth finding.

***Finding 4:** Sellers' propensities to cooperate strongly influence collusive efficiency. Collusive efficiencies are higher in markets where sellers were previously identified as cooperative than in markets where sellers were previously identified as competitive. Further, out-of-sample, markets composed of homogeneously cooperative types achieved higher average collusive efficiency than markets in which seller types had not been controlled for.*

5. Discussion

This paper investigates two dimensions of behavior that may explain tacit collusion in the absence of any obvious coordinating mechanism: price signaling activity and a propensity for participants to behave cooperatively or competitively. With regard to signaling, we are unable to find any obvious simple link between signaling activity and collusiveness that is predictive across markets even with extensive repetition and an enhanced capacity to identify intended and 'communicated' signals. On the other hand, we find that individual propensity to cooperate, seller 'type', is relatively stable across markets, and that type variations prominently affect collusive efficiency.

Our findings are important for two reasons. First, seller type represents an identifiable determinant of tacit collusion within the laboratory. Failure to control for type will at least significantly increase the variability of market outcomes, and will bias results if subjects are drawn from populations that are not random with respect to type.

Second and more generally, although sellers in natural contexts may certainly develop a 'language of coordination' via their pricing activity, our results suggest that at least in some situations an underlying propensity for sellers to cooperate may sustain collusion absent the development of such a language. For that reason, attention to characteristics that may reveal a propensity to cooperate, in the sense that sellers do not aggressively undercut rivals, may be merited. In this respect our results are consistent with the call by Baker (2002) for increased attention to 'maverick' firms in horizontal merger analysis. In our markets, for example, although cooperative types were sometimes able to maintain quite high levels of collusive efficiency in homogenous-group markets, cooperators benefited considerably from the elimination of low pricing

‘mavericks.’ For the nine ‘cooperative’ sellers who we invited back to participate in a second session, average collusive efficiencies for the homogenous markets in which they participated were 37% higher, and average earnings 87% higher than those in the initial mixed-group markets.

In closing we mention two areas for future investigation regarding the relationship between seller type and market performance. First, an investigation of the factors underlying cooperativeness is merited. Cooperative sellers may be driven by some sense of altruism, as is frequently the focus of discussion in the VCM literature. However, successful cooperation in a market context is personally profitable, and so other factors such as strategic sophistication, low discount rates, and preferences for risk are also potentially influential explainers. Identifying the mix of factors consistent with cooperativeness might help identify characteristics of sellers prone to tacit collusion in natural contexts. Second, it would be useful to examine the importance of seller types in environments less conducive to tacit collusion. Our market, with a small number of symmetric sellers who interact extensively, contains a number of ‘facilitating’ factors. Exploration of the importance of seller type in environments that are less conducive to tacit collusion would be of some policy interest as it would allow some assessment of the importance of seller type relative to the more standard structural indicators of tacit collusion.

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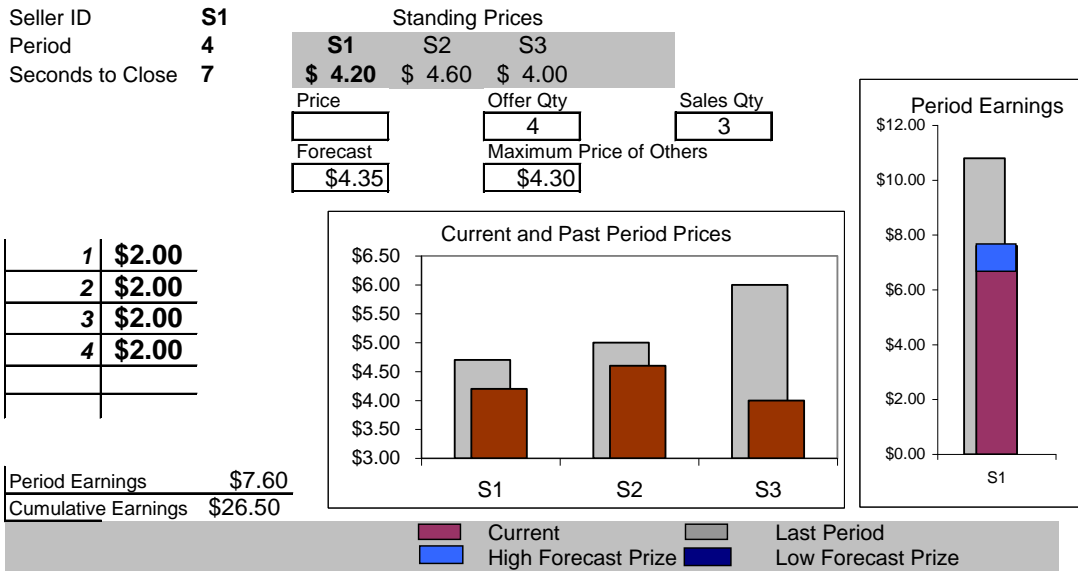


Figure 1. Screen Display for a ‘Near Continuous’ Posted Offer Institution

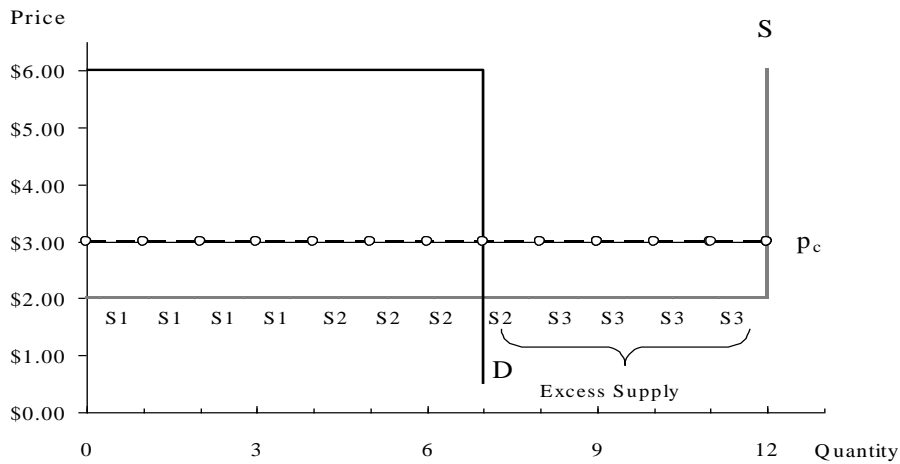


Figure 2: Supply and Demand Arrays for a Three-Seller Swastika Design

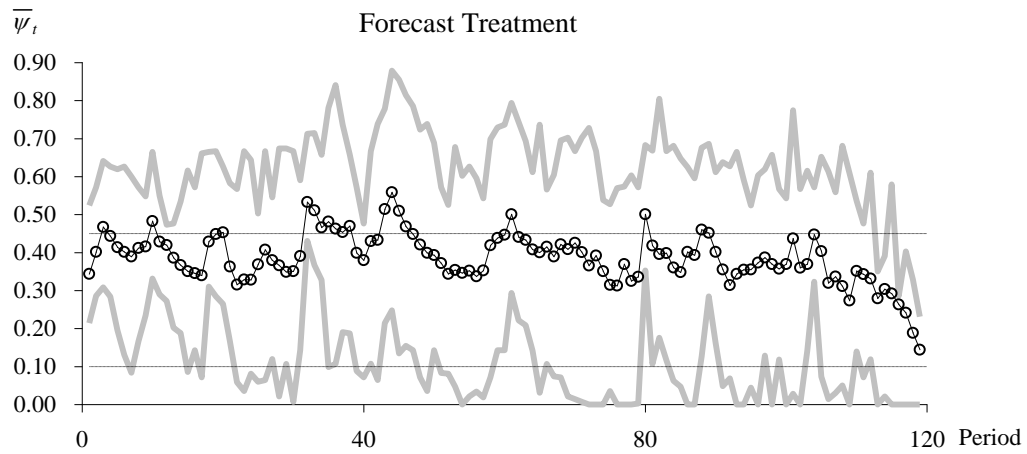
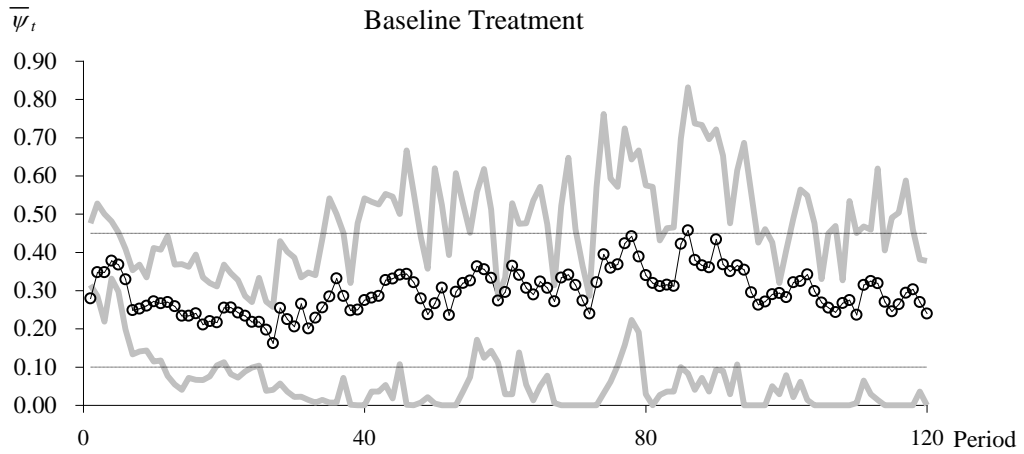


Figure 3. Collusive Efficiencies: Means and Inter-quartile Ranges.

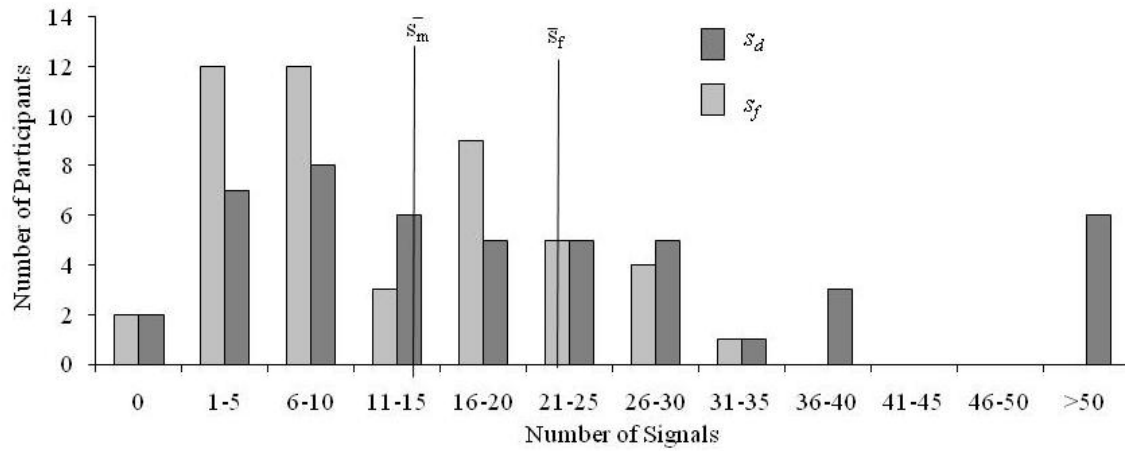


Figure 4. A Comparison of s_f and s_d .

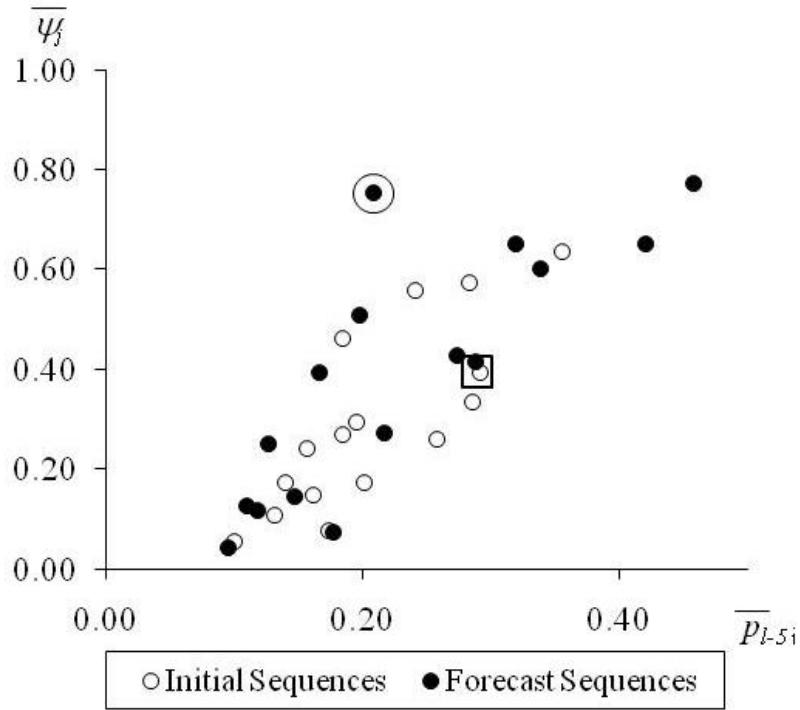


Figure 5. Mean Collusive Efficiencies ($\bar{\psi}_j$) and Mean Cooperativeness (\bar{p}_{l-5j}) of participants in market j .

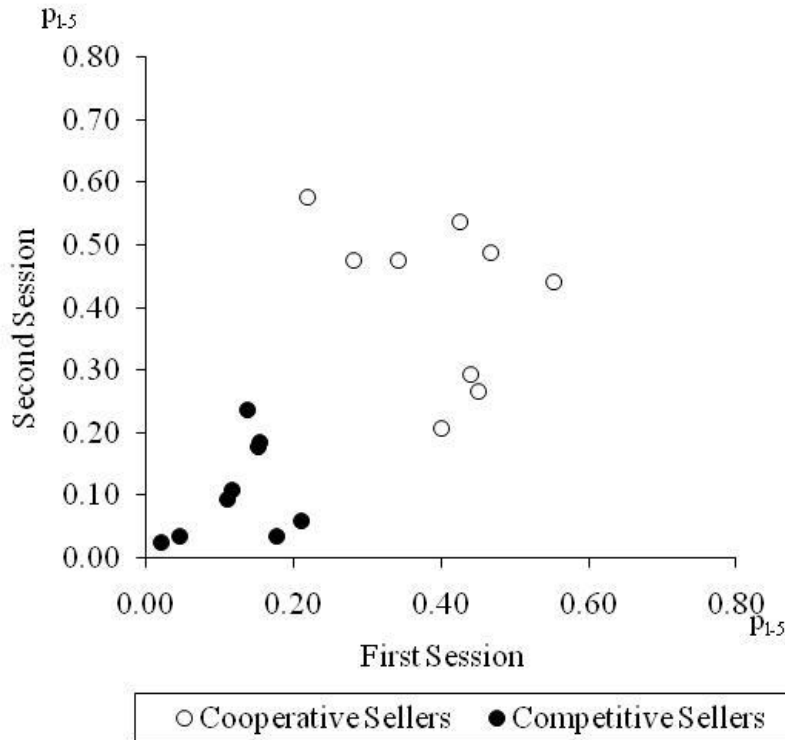


Figure 6. $\bar{p}_{l-5,i}$ Cooperativeness Measures across Sessions.

Table 1. $\bar{\psi}$ (s.d.)

Periods	Treatment		H ₀			
	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	Forecast	$\bar{\psi}_B \leq .10$	$\bar{\psi}_F \leq .10$	$\bar{\psi}_B = \bar{\psi}_F$	
1-60	0.26 (0.15)	0.41 (0.24)	0.00	0.00		0.14
61-120	0.32 (0.25)	0.37 (0.37)	0.00	0.00		0.60
All	0.29 (0.18)	0.39 (0.39)	0.00	0.00		0.34

Notes: Entries in columns (4) and (5) are Wilcoxon p -values (one tailed tests). Entries in column (6) are Mann Whitney p -values (two-tailed tests).

Table 2. Signals and Transactions PricesDependent Variable: \bar{p}_{jt}

	Sequence		
	Baseline	Forecast	
	(1)	(2)	(3)
<i>Constant</i>	0.37*** (0.09)	0.36*** (0.11)	0.47*** (0.12)
\bar{p}_{jt-1}	0.89*** (0.03)	0.89*** (0.03)	0.87*** (0.03)
<i>I_{sd}</i>	0.32*** (0.03)	0.45*** (0.06)	
<i>I_{sfp}</i>			0.21*** (0.04)
Number of clusters	16	16	16
Number of observations	1872	1872	1872
\bar{R}^2	0.80	0.79	0.77
F _(2, 15)	1152.82***	510.48***	475.5***

Notes: Robust standard errors in parentheses are adjusted for clustering at the market level. Significant at * 10 percent level; ** 5 percent level, *** 1 percent level.

Table 3. Comparing s_f and s_d

Signal Measure	s_f	s_d
(1) Number (average per seller)	998 (21.0)	453 (9.4)
(2) Unintended Signals (% of Signals)		128 (28.3%)
(3) Masked Signals (% of Signals)	275 (27.6%)	66 (14.6%)
(4) s_f signals missed by s_m (% of Signals)	674 (67.4%)	
(5) Correlation	$\bar{\rho}_{s_m, s_f} = 0.41$	

Key: Masked Signals' under s_f are prices less than \$6 that exceed forecasts, but are below the period's maximum price. Masked signals under s_d are prices less than \$6 that exceed the previous period's maximum price, but are below the maximum posted by others in the current period. 'Unintended' signals under s_d are postings below the period forecast, but above the previous period's maximum price. Signals missed by s_d are signals sent under s_f that were not recorded as signals under s_d .