Re-matching, Information and Sequencing Effects in Posted Offer Markets*

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Abstract: This paper evaluates the effects of some standard procedural variations on outcomes in posted offer oligopoly experiments. Variations studied include the presence or absence of market information, the use of re-matched or fixed seller pairs and alterations in the order of sequencing. Experimental results indicate that such variations can have first order effects on outcomes. For this reason, we recommend that results in oligopoly experiments be carefully interpreted in light of the procedures selected.

Keywords: Market Experiments, Oligopoly, Re-Matching, Information, Market Concentration.

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1. Introduction

Laboratory market experiments, particularly oligopoly experiments, are conducted under remarkably diverse conditions. Often-varied dimensions include the presence or absence of information regarding underlying supply and demand conditions, the use of single or multiple treatments in a laboratory session, and, increasingly, either re-matching participants into different groups after each trading period or using fixed groups throughout a market session.

Some procedural variations, such as the decision whether or not to conduct multiple treatments in a single session, involve a balancing of the increased statistical power of matched-pairs tests, with a concern that later sequences in a session may exhibit what Williams and Smith (1984) term 'hysteresis' effects, or carryover effects from the conditions pertinent in the immediately preceding sequence. Other variations, however, such as the decision to provide or not provide information regarding underlying market supply and demand conditions, reflect longstanding methodological differences of opinion. Many investigators provide decision-makers with complete market information, because the game-theoretic foundations upon which the relevant Nash equilibrium predictions are based typically assume that agents possess such information. Other investigators argue against providing such information because agents typically do not have such information in natural contexts.[1]

Technological developments drive still other procedural variations. In particular, an increasing number of oligopoly experiments re-match participants into new markets after each trading period (e.g., Cason and Datta, 2006, Cox and Walker, 1998, Datta and Dechenaux, 2006, Davis, Reilly and Wilson, 2003, Dufwenberg and Gneezy, 2000, Fonseca, Müller and Normann 2006, Holt, 1985, Huck, Müller and Normann, 2001, Martin, Normann and Snyder 2001, Normann, 2006, Orzen 2006, and Snyder, Ruffle and Normann, 2006). In large part, the decreased costs of implementing such procedures drive their increased use. Oligopoly experiments where participants are re-matched into different groups after each period date at least to Holt (1985). However manually re-

[1] Most notable among these is Smith (2003) who argues that matching the information conditions of a theory with market conditions reflects a failure of investigators to appreciate the difference between the 'ecological' rationality that drives market outcomes, and the 'constructivist' methods social scientists use to analyze or predict these outcomes.

matching participants in a 'pen and paper' experiment is extremely cumbersome. The increased availability of computer software that allows re-matching, such as Z-Tree, as well as reduced software development costs, make this procedure far more practical.

Importantly, the use of a re-matching procedure also raises significant methodological questions. To the extent that re-matching fully informed participants into new markets after each trading period effectively induces static conditions, re-matching may allow investigators to more nearly approach the assumptions of the underlying game theory (when that theory is static). However, in many contexts, the very process of approaching the conditions appropriate to the theory being tested moves the experiment away from conditions pertinent to any naturally occurring context.[2]

Prior to engaging in a debate regarding best combinations of procedures for various types of oligopoly experiments, it is appropriate to develop some appreciation for what is at stake. In particular, we ask how prominently do those conditions that most clearly distinguish market environments from the assumptions that underlie the game-theoretic models used to analyze those markets – e.g., full versus limited information and rematched ('static') versus fixed groups - affect experimental outcomes? This paper reports an experiment conducted to address this question.

Our experiment design consists of a pair of discrete unit 'market power' designs. In such designs, capacity restrictions allow symmetric sellers to profitably and unilaterally deviate from the competitive equilibrium determined by the intersection of market supply and demand curves. Analyzed as a stage game, the static equilibrium for such a market is in mixed strategies. Further, a reallocation of units that reduces the number of symmetric sellers increases the central moments of the equilibrium mixing distribution.

Very practical considerations motivate our use of these designs. We frequently use such designs in our research, and in a pair of recent papers we reach differing conclusions regarding the effects of changes in the number of sellers on prices. Davis

[2] Of course, in some instances, such as e-bay trading, a re-matching protocol may more nearly parallel a pertinent field context. Similarly, re-matching is non-problematic when used as a treatment against which the effect of repetition in fixed groups is compared. We are concerned here with papers where re-matching is employed as a way of implementing the static conditions of the theory, without either control or any reference to the parallel field context. Snyder, Ruffle and Normann (2006) is an important (but not unique) recent example.

and Wilson (2007) report an experiment conducted in a full-information market power design where a pair of mergers that reduced the number of symmetric sellers from four to two resulted in significant and very substantial price increases. On the other hand, Davis (2007) finds at best guarded support for predicted 'numbers' effects in laboratory markets conducted in private information power designs of much longer duration, where structural changes were separate treatments rather than introduced as mergers. The differing results in these studies suggest that procedural 'details' may importantly affect experimental outcomes, and raise the question of how best to proceed.[3]

Results of a recent meta-analysis of oligopoly experiments by Engel (2006) generate an additional pertinence for our experiment. Engel introduces his study as a means of informing policy makers about what the experimental literature says regarding tacit collusion in oligopolies. From his analysis Engel draws some conclusions that appear to be at odds with what might be expected either from the theoretical analysis of oligopoly interactions, or from a casual review of the pertinent experimental literature. Notable among these are conclusions that (a) capacity restrictions tend to reduce rather than raise prices relative to both static Nash and Walrasian predictions, and (b) the use of a re-matching protocol tends to raise rather than reduce prices. Although our experiment was conducted without knowledge of Engel's study, ex post we think it worthwhile to consider Engel's conclusions in light of our results, in the hope of gaining some insight into what might drive such unexpected conclusions.

By way of pre-summary, we find that some primary results are reasonably robust to all procedural variations. Most notably, in all markets prices exceed the Walrasian (competitive) prediction, and at least in triopolies we uniformly observe tacit collusion in the sense that mean prices significantly exceed the mean of the static Nash equilibrium mixing distribution. On the other hand, we also find that procedural variations can exert first order effects on outcomes. In particular the comparative static effects associated with having two rather than three competitors in a market are most uniformly generated in full information/ re-matched environments. These protocol effects lead us to

[3] The effects of alterations in the procedures examined here on outcomes in other environments remains an open question. Of particular interest would be the impact of procedural variations on outcomes in quantity-setting Cournot games, where reducing the number of sellers appears to more reliably increase prices (reduce quantities) as predicted (e.g., Huck, Normann and Oechssler, 2004, Mason and Phillips, 1997a).

recommend that experimentalists select procedures with care, and interpret results in light of the procedures chosen. Most specifically, our experiment highlights the potential danger of misinterpreting the implications of results from full information ‘static’ environments for (repeated, limited information) market contexts. Finally, we observe that our results with respect to capacity constraints and re-matching differ sharply with the conclusions of Engel (2006). We speculate as to some possible reasons for the observed differences.

The remainder of this paper is organized as follows. Section 2 below presents the experimental design and behavioral conjectures. Section 3 presents the matrix of treatments and explains procedures. Section 4 reports results. The paper concludes with a short 5th section.

2. Experiment Design

We divide our presentation of the experimental design into four subsections. A first subsection briefly reviews our ‘near continuous’ implementation of the posted-offer trading mechanism, as well as our re-matching procedure. A second subsection presents supply and demand arrays for our ‘market power’ designs. A third subsection explains our implementation of information and sequencing treatments. Finally, a fourth subsection summarizes behavioral conjectures.

2.1 The Near-Continuous Posted-Offer Institution. In most respects, our implementation of the posted-offer institution follows standard procedures. Trading periods follow a two-step sequence. First, sellers endowed with unit costs, simultaneously make price decisions. Production is ‘to order’ in the sense that sellers incur costs only for the units that they subsequently sell. Once all prices are posted, they are displayed publicly and an automated buyer routine makes fully demand revealing purchase decisions. The period ends by presenting sellers with sales quantities, as well as per period and cumulative earnings.

Our markets are distinctive in that they are extensively repeated. Each treatment ‘sequence’ in our experiment consists of 160 trading periods. We were able to include this large number of trading periods by truncating the maximum length of decision periods to twelve seconds.[4] To facilitate price-posting and the interpretation of results, we streamlined price posting procedures, and supplemented a tabular display of prices with bar graphs.[5] In a previous related experiment Davis and Korenok (2007) establish that participants are quite capable of interpreting and responding to pricing results in trading periods of such short duration. We do not analyze separately the effects of extensive repetition on pricing performance.[6] However, as will be evident below, extensive repetition does allow some insight into the persistence of other treatment conditions.

In many sequences we also use a re-matching protocol. In these sessions participants made pricing decisions each period without knowing the identity of the participants with whom they were paired until the end of the trading period.[7]

2.2 Supply and Demand Arrays. Figure 1 illustrates the pair of market power designs investigated here. In each design, units cost $2 each and sellers may collectively offer a total of 12 units. Aggregate demand is nine units, each of which will be purchased by a fully revealing simulated buyer at a price of $6 or less.

The designs differ in the number of symmetric sellers. In the two seller '*2p*' design, each seller may offer six units. In the three seller or '*3p*' design each seller may offer four units. Under posted offer trading rules, the competitive prediction determined by the intersection of market supply and demand schedules is not a Nash Equilibrium in either the *2p* or the *3p* design.

To see this, consider first the *2p* game. At a price of $2 sellers earn nothing. Each seller may improve earnings by raising the price to $6, where each seller is certain to sell a minimum of three units and realize security earnings of $12. Six dollars is not an equilibrium for the game, since either seller could unilaterally increase earnings over

[4] In a number of recent experimental studies, investigators have reduced the maximum length of decision periods in order to increase the decision-profile. Some pertinent oligopoly experiments include Deck and Wilson (2002, 2006), Davis (2007) and Davis and Korenok (2007).

[5] For a more complete description of the near continuous posted offer trading institution, see Davis and Korenok, (2007).

[6] Davis (2007) and Davis and Korenok (2007) explore more explicitly the effects of extensive repetition.

[7] By 'identity' we mean the participant identifiers used within the session by the program. Participants never learned who was associated with participant identifiers other than themselves. Our implementation of a re-matching protocol differs slightly from that used, for example, in Z-Tree in that once the trading period is complete, participants learn the identity of the participant(s) with whom they were paired in the period. Providing this information has the advantage of emphasizing to participants that they are matched with different sellers each period.

those available at a common price of \$6 by posting the minimum admissible increment below \$6, and selling six units. Sellers face similar incentives to deviate from any common price posting down to a lower limit p_{min2} = \$4.00, where earnings from selling 6 units as the low pricing seller just equal the \$12 security earnings from posting the limit price (12 = 6×[p_{min2}-2]). The symmetric equilibrium for the game has the two sellers randomizing over the range from \$4 to \$6 according to the distribution

$$F_2(p) = \frac{2p-8}{p-2}. \qquad (1)$$

Solving implicitly, the mean of the mixing distribution is $\overline{p}_2 = \$4.78$, as shown in Figure 1.

Reasoning similarly, in the *3p* design sellers randomize over the range *p*={\$3, \$6}. In the symmetric mixing equilibrium, each seller randomizes according the distribution

$$F_3(p) = \sqrt{\frac{4p-12}{3p-6}}. \qquad (2)$$

Again solving implicitly, the mean of the symmetric mixing distribution for three sellers is $\overline{p}_3 = \$3.52$.

2.3 Information Conditions and Sequencing Effects. In addition to the use of fixed or re-matched groups, we study two other procedural variations. The first regards changes in information conditions. The effect of information on laboratory market performance has been studied previously (e.g., Dolbear et al., 1968, Fouraker and Siegel, 1963, Mason and Phillips, 1997b, Kruse, Rassenti, Reynolds and Smith, 1994, Huck, Normann and Oechssler, 2000, Davis, 2002). However, the type of information provided varies markedly across experiments, ranging from no feedback regarding market outcomes (e.g., Fouraker and Siegel), to the provision of sales quantity and profit outcomes for each firm (Mason and Phillips, 1997b, Huck, Normann and Oechssler, 2000). Here we examine the effects of providing information regarding underlying supply and demand conditions, a treatment Engel (2006) defines as the difference between partial and full ex ante information. The full/partial ex ante information treatment is most pertinent for our purposes because it distinguishes standard assumptions about ‘markets’ from those pertinent to the non cooperative games on which markets are often analyzed. Engel finds information effects to be sensitive to the number of sellers. In duopolies, outcomes do not distinguishably differ across partial and full ex ante information conditions. However, in markets with three or more sellers full information tends to reduce prices. Engel's conclusion in this regard differs particularly from Kruse et al. (1994), where the provision of full rather than partial ex ante information markets with four sellers exerts a modest but significant price increasing effect, at least in markets with inexperienced participants.

The comparatively long duration of our markets allows insight into the persistence of information effects, as well as into the way information conditions interact with other procedural variables. Specifically, in our 'no information' treatments, sellers know only their own costs, prices and the number of units they sell each period. In a 'full information' condition, underlying aggregate supply and demand conditions are explained to participants in the instructions. Instructions also explained the (fully revealing) purchasing rule used by the buyer, and that all sellers are symmetric as well. Finally, to facilitate understanding of this information, a monitor presented several possible pricing outcomes to participants at the end of the instructions, and reviewed with participants the consequences of these choices. Notice that the starkly simple supply and demand structures in Figure 1 facilitate an understanding of market fundamentals.

A final procedural variation regards alterations in the order-of-sequencing. As is well known, hysteresis effects can importantly affect results in multi-sequence market experiments. While sequencing effects are not a primary focus of our experiment, market experiments are routinely conducted in a multi-sequence format, and we think it important to evaluate both the potential magnitude of such effects, and how such effects may interact with the information and rematching treatments that are our primary focus of this investigation. Considered here are changes that either increase expected per seller earnings, in the form of 'mergers' that reduce the number of sellers in the market from three to two, or changes that reduce expected earnings, in the form of 'spinoffs' that increase the number of sellers per market from two to three.

2.4 *Conjectures*. In total, our investigation consists of three procedural variations (a) no information/full information, (b) fixed seller groups /re-matched sellers and (c) sequencing order treatments. To organize an analysis of outcomes, we evaluate performance in all treatments in terms of the performance of static Nash predictions, both absolutely, and in a comparative static sense. This is our first conjecture.

Conjecture 1: *Nash mixing equilibrium predictions organize outcomes. Specifically, observed mean posted prices in our markets do not differ significantly from the mean of the static mixing distributions and (b) mean posted prices in duopolies exceed mean posted prices in triopolies.*

We have diffuse priors regarding conjecture 1. With respect to the absolute organizing power of static Nash predictions, several studies in related contexts suggest that sellers not only recognize unilateral market power (and price above the Walrasian level), but that sellers price in excess of the mean of the mixing distribution (e.g., Davis and Holt, 1994 and Wilson, 1998). On the other hand, from his meta-analysis, Engel (2006) concludes that capacity restraints tend to reduce prices. Given the generally competitive performance of posted offer markets with three or more sellers, this would seem to imply that in our markets with capacity constraints we should observe prices at least below static Nash predictions, and perhaps even approaching Walrasian levels. With respect to the comparative static effect of having three rather than two sellers in a market structure, Davis (2007) and Davis and Wilson (2007) report conflicting results, as observed in the introduction.

Three additional conjectures address the procedural effects that are the primary focus of this paper. Consider first the effects of using a re-matching protocol. To the extent that a re-matching protocol induces static conditions, we would expect it to improve any drawing power static Nash mixing predictions might exhibit. If mean prices should exceed the mean of the Nash mixing distribution in the fixed group sessions, we would expect prices to fall under re-matching.

Conjecture 2. *A re-matching protocol reduces prices.*

Again, Engel (2006) concludes that the use of a re-matching protocol tends to raise prices.

A third conjecture addresses the effects of shifting between partial and full ex ante information. Kruse et al. (1994) suggest that in a symmetric context, the provision of full information may allow sellers to more clearly interpret the actions of their rivals, and may thus facilitate tacit collusion. However, other investigators have concluded that the provision of full ex ante information can exert either no effect or a price-reducing effect (e.g., Engel, 2006). This is a third conjecture.

Conjecture 3: *Full ex ante information regarding underlying market supply and demand conditions tends to increase posted prices.*

Finally, we explore sequencing effects. In addition to changes in the predicted static equilibrium, sequencing effects may impact the price-adjustment process. This is a fourth conjecture,

Conjecture 4*: Hysteresis effects affect the price adjustment process. Following either a merger or a spinoff, prices adjust slowly from prices generated in an initial sequence toward those predicted in the new underlying environment.*

We observe that in distinction to other studies where 'mergers' have been introduced, the 'mergers' (as well as the 'spinoffs') we induce here are purely nominal. As explained more fully below, we hold the number of participants in a market session uniformly constant at twelve. In our 'merger' condition we reassign participant identifiers and randomly reshuffle participants from four initial triopolies into six new duopolies. In our 'spinoff' condition we do the reverse. For this reason, the absence of sequencing effects in our experiment says little about their potential effects in other environments (although the observation of such effects is informative). Finally, prior to proceeding we note that the changes in treatment conditions are not intended to replicate mergers or spinoffs in natural contexts. Rather, we proceed with the narrow intention of investigating sequencing effects in the laboratory, with particular attention focused on how such effects interact with information and re-matching conditions.

3. Experiment Procedures and the Matrix of Treatments

3.1. Matrix of Treatments. The experiment consists of a series of twelve market *sessions*. In each session twelve subjects participated in a pair of two salient *sequences*. We blocked information and re-matching conditions across sessions, as shown in the treatment cells of Table 1. The first two parts of the treatment labels in Table 1 identify the salient sequences, by information condition (*i* information or *n* no information) and by constancy of groups (*r* re-matched or *f* fixed groups). The latter two numbers indicate the order of triopoly and duopoly sequences. For example, the treatment cell *ir32* shown in the upper left corner of Table 1 identifies the full *i*nformation treatment with *r*e-matched participants, where an initial triopoly is followed by a duopoly. Below, in the results section, we use a '_' to distinguish the two sequences of each treatment. For example, *ir3_* and *ir_2* denote, respectively, the initial (triopoly) and terminal (duopoly) sequences of treatment *ir32*. As can be seen in Table 1, we held information and re-matching conditions constant across sequences in each session to avoid confounding experience and associated sequencing effects with information and re-matching treatments. Notice also in Table 1 that we conducted two re-matching sessions and one fixed group session in each combination of order-in-sequence and information variables.

3.2 Experimental Procedures. At the outset of each session, participants were randomly seated at visually isolated computer terminals and the monitor read instructions aloud as participants followed along on printed copies of their own. A ten-minute practice sequence followed, which consisted of a series of twenty period triopolies. Each period lasted a maximum of 30 seconds, with the period terminating early if all twelve participants entered pricing decisions prior to the expiration of the 30 second limit.[8]

The practice sequence was used solely to make participants comfortable with price-setting procedures. Supply and demand conditions for the practice session differed substantially from those used in the salient sequences, and participants were not paid for their decisions in the practice sequence.[9] Following the practice sequence, a first salient sequence with either duopoly or triopoly markets commenced. The sequence consisted of 160 periods, each of which lasted a maximum of 12 seconds. After the conclusion of the first salient sequence, participants were randomly reassigned new identities, regrouped into the triopoly or duopoly condition not conducted in the first sequence, and a second 160 period sequence began. At the end of the second salient sequence participants were paid privately, and dismissed one at a time. While participants were not informed of the total number of sequences in the session, they were told that each sequence would consist of 160 periods.

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[8] If a seller failed to post a price within the 30 second limit, the software repeated the seller's previous-period posting decision. Even at the outset this happened only rarely and after the first period or two of the training sequence, periods ended well in advance of the 30 second period maximum. Similarly, in the salient sequences trading periods typically lasted less that the 12 second maximum.

[9] In the practice sequence, all participants were induced with the same, increasing unit cost schedule. This information was provided to participants as common knowledge.

Participants were 144 business and economics students Virginia Commonwealth University enrolled in courses in the spring semesters of 2006 and 2007. Most students were upper level undergraduates, but a small number of MBA and economics MA students also participated. Participants were 'inexperienced' in the sense that no one had previously participated in a market experiment. No one participated in more than one session. Lab dollars were converted to U.S. currency at a $200 Lab =$1 U.S. Earnings for the 100-120 minute session ranged from $20.00 to $34.50 and averaged about $25.50 (inclusive of a $6.00 appearance fee).

4. Results.

Given the number of treatment variables and potential interaction effects, it is most efficient to analyze results with a regression analysis that controls appropriately for interaction effects between treatment conditions. Prior to proceeding, however, we offer two general observations about market dynamics, both of which pertain to the use of the re-matching protocol. First, use of a re-matching protocol induces considerable homogeneity in results. For example, for both the triopolies and duopolies, the standard deviation of mean price postings per sequence for the re-matching sequences is no more than half that of the comparable fixed group markets.[10] Second, and as a consequence of this increased homogeneity, markets using a re-matching protocol are at least potentially subject to super-group effects. We would err in treating individual pricing decisions as strictly independent observations.

In light of these comments, we here use the individual mean posted price choices as the unit of observation, but control for possible within-sequence interdependencies by clustering observations by markets (in the fixed group sequences) or by session (in re-matching sequences), and then using a robust estimation technique. We also control for possible individual dependencies across sequences (e.g., the same person participating in the first and second sequences in a session) by running separate regressions for the first-in-sequence and second-in-sequence markets, and then evaluating hypotheses with a series of within-sequence comparisons. Finally, to allow insight into the effects of

[10] The standard deviation of mean posted prices for the re-matched triopoly sequences (.25) is slightly less than half the comparable measure for the fixed group triopoly markets (.51). For the duopolies, the difference is even greater, with the standard deviation of posted prices for the re-matched duopoly sequences (.16) being roughly one quarter the standard deviation for the fixed group duopolies (.62).

extensive repetition we divide the 160-period sequences into three *segments* consisting of 53 periods for the first and second segments, and 54 periods for the last segment.

Specifically, we estimate the following model,

$$\bar{p}_{it} = \beta_o + \beta_{ni} D_{ni} + \beta_{fixed} D_{fixed} + \beta_{ni\,fixed} D_{ni} D_{fixed} + D_2[\beta_2 + \beta_{ni2} D_{in} + \beta_{fixed2} D_{fixed} + \beta_{ni\,fixed2} D_{ni} D_{fixed}] + u_{it} \quad (3)$$

where $\bar{p}_{it}$ is the average price posted by seller *i* in period block *t*={1, 2, 3} of a sequence.[11] The D variables are a series of dummy variables: D_2 = 1 when the sequence consists of a duopoly, 0 otherwise, D_{ni} =1, when no information regarding underlying supply and demand conditions is provided, 0 otherwise, and D_{fixed} =1, when participants remain in fixed groups throughout the sequence, 0 otherwise.

We specify the error term, u_{it}, to allow for possible within-sequence interdependencies among sellers in the same markets (in fixed group treatments) or sessions (in treatments with re-matching). In particular, we allow contemporaneous correlation, $corr(u_{it}, u_{jt}) \neq 0$, if sellers i and j are in the same fixed markets or in the same remixed session. We assume, however, that there is no dependence across fixed markets or sessions, as they are isolated from each other either physically or in time.

As should be evident from the structure of equation (3), we estimate for each sequence both direct effects as well as the complete set of possible interactions between information, re-matching and number of sellers variables, using as a baseline condition the full information/re-matching triopolies. Thus, for the first-in-sequence data, the intercept term β_o estimates the mean price for the *ir3_* treatment, while for the second-in-sequence data β_o estimates the mean price for the *ir_3* treatment. Other coefficients assess the marginal effects of information, fixed markets and two sellers, as well as interactions between these variables. Linear combinations of the coefficients recover estimates of the average price for each of the other treatment cells, as summarized in Table 2. Importantly, by including all higher order interaction effects, the linear combinations of the parameters in (3) recover exactly the observed mean price for each treatment cell (reported below in Table 4).

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[11] In calculating these mean prices we truncated all price postings above the limit price of $6.00 at $6.01. Failing to make such an adjustment distorts pricing decisions, as sellers in some instances posted prices several orders of magnitude outside the feasible contract price range. Truncating prices at $6.01 is a consistent way to treat equivalently as 'signals' all price postings for which purchases are not possible.

Table 3 reports regression results for the first sequence (columns 1a to 3a) and for the second sequence (columns 1b to 3b). Inspection of the estimates suggests that different experimental conditions can interact with each other importantly. In particular, comparing across columns (2a) and (3a) observe that the second order interaction between information and the number of sellers (β_{ni2}) is significant in the second and third periods of the first sequence. Note also in column (3a) that the third order interaction of all three treatments ($\beta_{nifixed2}$) is significant in the third segment of the first sequence. Given these interaction effects, the unambiguous tests of our conjectures come from assembling coefficients across treatment conditions as summarized in Table 2.[12] Using Wald tests on these linear coefficient combinations, we can then test for the significance of treatments and differences between treatments. We follow this approach to formally establish findings in the remainder of this section.

We proceed by evaluating, in order, conjectures 1 to 4. Consider first conjecture 1, that static Nash mixing distributions organize outcomes. We evaluate respectively the absolute drawing power of static Nash predictions, and the capacity of static Nash predictions to explain the effect of having two sellers in a market rather than three. Table 4 reports results of Wald tests of the null hypothesis that the mean observed prices in each treatment (listed in columns for the first, second and third period blocks, respectively), do not differ from the predicted mean of the pertinent symmetric equilibrium mixing distribution. As seen in the upper two row blocks in Table 4, mean observed prices in the triopolies uniformly exceed the mean of the Nash mixing prediction (\$3.52), and the differences are significant in all but one instance, even after adjusting for multiple comparisons.[13] For the duopoly treatments, listed in the bottom half of Table 4, mean prices again tend to exceed the \$4.78 mean of the predicted Nash

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[12] As Braumoeller (2004) very cleanly observes, when assessing both primary effects and interaction effects, conclusions may be drawn from a primary effect estimate only when the primary variable has no significant interactions with other variables in the estimation.

[13] When testing for the statistical significance of a series of linear combinations of regression coefficients, the likelihood that one of the tests exceeds zero by chance increases monotonically with the number of tests conducted. To correct for this problem, we use a sequential Bonferroni adjustment (see, e.g., Rice, 1989). Specifically, given a table of k orthogonal tests and a desired significance level α, rank order p values for k independent simultaneous comparisons from lowest to highest. The smallest i of the k comparisons are significant at level α, if unadjusted p values for these comparisons satisfy $p_i \leq (1-[1-\alpha]^{1/(1+k-i)})$. Note finally that the sixteen comparisons listed in each column of Table 4 are based on two separate regressions, one for the first-in-sequence observations, and another for the second-in-sequence observations.

mixing distribution (in 20 of 24 cells). Differences, however, are smaller than in the triopolies, and they significantly exceed the predicted mean in only 10 of 24 instances. This is a first part of finding 1.

Finding 1(a)*: In triopolies, prices consistently and significantly exceed Nash predictions. In duopolies deviations also tend to be positive, though they are smaller and differ less uniformly from Nash predictions.*

Thus, we reject the first part of conjecture 1. We find interesting the tendency for prices to deviate from Nash predictions to a greater extent in triopolies than in duopolies here. This outcome contradicts results both in experimental Cournot markets (see. e.g. Huck et al, 2004) and in Bertrand markets where sellers have no market power (e.g., Dufwenberg and Gneezy, 2000, Davis, 2007). We suspect that relatively stronger incentives to collude tacitly in the *3p* design than in the *2p* design play a large role in driving this result. For example, using the well-known 'Friedman' index as a measure of sellers' incentives to collude tacitly, note that the minimum discount factor (e.g., probability of continuation) necessary to support tacit collusion in the *3p* design, θ_{3p} = .20, is considerably smaller than the comparable minimum discount factor for the *2p* design, θ_{2p} = .50.[14] By way of contrast, in an extensively-repeated variant of a 'strong Bertrand' markets reported Davis (2007), comparatively smaller deviations from Nash predictions occurred in triopolies than in duopolies. In these markets, the minimum discount factors necessary to support tacit collusion in the three seller (*3np*) and two seller (*2np*) markets were, respectively, θ_{3np} = .73 and θ_{2np} =.57.[15]

Despite the weaker tendency for prices to exceed static Nash predictions in the duopolies, we also observe that in all treatments, prices unambiguously exceed the \$2.00 competitive prediction. Thus, capacity restrictions clearly result in supra-competitive prices. This result does not definitively contradict Engle's conclusion that capacity constraints exert a price-damping effect, since our experiment includes no treatment where sellers face no capacity constraints. Nevertheless, the very high mean prices in the

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[14] In each design *i* , $\theta_i = \dfrac{\pi^D - \pi^{JPM}}{\pi^D - \pi^N}$, where π^N = static Nash earnings, π^J = earnings in the joint maximizing outcome and π^D = earnings from a unilateral deviation from join maximizing outcome for that design. Details of these calculations are available in an unpublished data appendix.

[15] On average, however, mean posted prices in the no power markets reported by Davis (2007) tended to converge on the Walrasian prediction.

capacity constrained markets examined here certainly raise the question of whether less competitive outcome might be reasonably anticipated were the capacity constraints eliminated.[16]

We turn attention now to the price performance of markets with two and three sellers. Table 5 reports the difference in mean prices for two-seller markets over the three-seller markets that occurred in the same order-in-sequence/ information/ group structure treatment conditions. [17] A glance at these results reveals that duopoly prices tend to exceed prices in otherwise comparable triopolies (21 of the 24 entries are positive). In the two full-information sessions with re-matching (*ir2_-ir3_* and *ir_2-ir_3*), differences are uniformly significant. Duopolies also yield significantly higher prices than triopolies in second-in-sequence re-matching comparisons (*nr_2-nr_3*), suggesting that the experience of a first sequence may effectively convey information, at least in markets with re-matching.

The higher variability of outcomes within the fixed group treatments makes results for the fixed group markets less uniformly significant. In the full information fixed group comparisons (*if2_- if3_* and *if_2 – if_3*), duopoly prices are higher, but never significantly so. In the no information fixed group markets, significant differences arise but are difficult to interpret, as duopoly prices are higher in the second sequence (*nf_2-nf_3*) and lower in the first sequence (*nf2_-nf3_*). These rather diffuse results represent a second part of finding 1.

Finding 1(b): *Controlling for order of sequence effects, duopolies generate higher prices than triopolies if sellers are re-matched across periods, unless sellers are both inexperienced with the market and do not have ex ante information. In fixed group markets, the high variability of outcomes undermines significant 'numbers' effects.*

The effects of a re-matching procedure, suggested by comparing across the upper and lower panels of Table 5, combined with the rather heterogeneous results within panels, particularly in the fixed group treatments, make interesting an evaluation of the

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[16] As mentioned in the previous note, a series of extensively-repeated 'strong Bertrand' markets recently reported by Davis (2007) were quite competitive. In these markets prices uniformly collapsed on the Walrasian outcome when the number of sellers exceeded two, and frequently collapsed on the Walrasian outcome even when the number of sellers equaled two. Sellers were not capacity constrained in this 'strong Bertrand' design, in the sense that any seller could unilaterally service the entire market.

[17] Thus, for example, the upper left entry in Table 5 reports the difference between the coefficient sums for the *ir2_* and *ir3_* treatment cells listed in Table 2 (in this case the coefficient 'β_{two}'), for a regression using first-in-sequence observations. Comparisons in Tables 6, 7 and 8 are structured similarly.

procedural variables that are the primary purpose of our experiment. In the remainder of this section, we address re-matching, information and order-of sequencing effects, in turn.

Consider first conjecture 2, which concerns the effects of re-matching. Table 6 reports the differences between mean prices in the fixed group / re-matched group treatments, holding constant other treatment conditions, in a way that parallels Table 5. Looking over Table 6, observe that as a general matter the positive sign on the fixed group / re-matched group differences indicates that re-matching tends to reduce prices. The most consistent effect of re-matching occurs in the final segment of the triopolies, where re-matching significantly reduces prices in three of four parameter combinations (after controlling for multiple comparisons). In the fourth triopoly comparison (*if_3-ir_3)*, the mean difference in the final segment is also large, but the heterogeneity of results in the fixed group markets makes the difference insignificant. In the duopolies, summarized in the lower portion of the table, re-matching again tends to reduce prices (with the exception of the *nf2_-nr2_* comparison). However, differences are significant only once (for the *nf_2- nr_2* comparison). These observations form a second finding.

Finding 2*: Re-matching tends to reduce prices in triopolies of long duration. Re-matching less consistently affects prices in duopolies.*

This second finding differs from Engel (2006), who finds that re-matching tends to raise prices. We note, however, that re-matching tends to reduce prices most consistently in the latter segments of extensively repeated triopolies, a range of periods outside the range of any re-matching experiments included in Engel's database. We suspect that the effects of re-matching are delayed in the triopolies because (fixed-group) sellers learn to coordinate only after extensive repetition. We also find it notable that re-matching does not here consistently affect duopolies, a result which is probably explained in large part by the very large outcome-variability in the fixed-group duopolies.

Consider next the effects of full as opposed to partial ex ante information. The mean price differences across information conditions reported in Table 7 allow evaluation of information effects. The spotty incidence of positive signs, and the limited number of significant entries in Table 7 merit comment. Full ex ante information raises mean posted prices in less than half of the comparisons (10 of the 24 instances). Only four of the comparisons across information conditions differ significantly from zero, and only one of these significant differences is positive.

A reasonable conclusion from Table 7 is that our information treatment does not importantly affect outcomes. We do observe, however, that effects of full ex ante information are not entirely random. Most prominently, notice that all significant information effects occur in the first sequences of sessions, an outcome that suggests that, to the extent full ex ante information affects outcomes, it speeds the learning process about market conditions. Curiously, this 'learning effect' is not the same across duopolies and triopolies, as full ex ante information raises mean prices in the early periods of first-in-session fixed group duopolies, but then reduces prices in the latter periods of first-in-session triopolies. In any case, given that significant information effects are typically negative, and are uniformly confined to first session sequences, we find it unlikely that information facilitates tacit collusion. We summarize these observations as a third finding

Finding 3*: The provision of full ex ante information may facilitate learning about underlying market conditions in the initial session sequences .There is little evidence to suggest that full ex ante information facilitates tacit collusion.*

Results in Table 7 are largely consistent with the literature. The higher prices in the full-informaiotn first-sequence fixed group duopolies parallel results (in sessions of relatively short duration) by Mason and Phillips (1997b) and the conclusions in Engel (2006). The price reducing effect of full ex ante information in markets with more than two sellers, while somewhat at odds with the modest price increases reported by Kruse et al. (1994), is also consistent with Engel (2006). However, our results are consistent with Kruse et al. in that these effects dissipate with experience (in our case with a second sequence, in their case when inviting participants back for a session). Given the temporary nature of these effects, we suspect that differences across our experiment and Kruse et al. (1994) are driven by the different designs in the two papers, rather than by differing propensities for (inexperienced) sellers to tacitly collude.

Consider finally order-of-sequence effects, the subject of conjecture 4. To identify 'hysteresis' effects we compare in Table 8 the difference in prices in the final segment of a first sequence with prices in the three segments of the second sequence.

Evidence consistent with a hysteresis effect would be a relatively small difference in the sequence immediately following a structural change, followed by larger adjustments as the sequence progresses. Table 8 highlights significant price changes across columns (a) and (b) with asterisks, and significant price changes across columns (a) and (c) with crosses. (No differences across columns (b) and (c) are significant at a p-value<.10 or less).[18] Inspecting Table 8 suggests some asymmetry in the pattern of price adjustments across mergers and spinoffs. In the fixed group markets, no significant adjustment pattern characterizes the markets with spinoffs. However, in response to a merger, the price adjustment path consists of a relatively large adjustment that tapers over time (at least comparing the first and third segments). In the sessions with re-matching, the asymmetry in responses is even more pronounced: initially small price reductions in 'spinoff' sessions give way to significant price increases by the sessions' conclusions, while in the 'merger' sessions, the direction of adjustment is reversed, with relatively large initial differences decaying somewhat in the final session segments. This is a fourth finding.

Finding 4*. Sellers respond asymmetrically to mergers and spinoffs. Following a merger, immediate price increases tend to taper off over time. Following a spinoff, sellers either do not follow a discernable inter-temporal adjustment pattern (in the case of fixed group markets) or increase prices over time from a relatively small initial reduction.*

The asymmetric response of sellers to mergers and spinoff suggests that not only may conducting multiple sequences in a session cause adjustment effects, but also that the way that the sequences are introduced may affect the adjustment process. Here, for example, sellers appear more willing to respond quickly to a structural change that increases their expected earnings than to one that reduces their earnings.[19]

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[18] To evaluate statistically the significance of price deviations across sequence segments, we exploit the constancy of mean prices in the final segment of the first sequence across segments (a), (b), and (c). For example, for the first row, the difference between columns (a) and (b) simplifies to *ir_2[1]-ir_2[2]*. We then use the same methodology applied in Tables 4-7 by substituting dependent variable $\overline{p}_{i1} - \overline{p}_{i2}$ .for $\overline{p}_{it}$ in equation (3)

[19] The mean differences of 57¢ for the *if_2[a] – if3_[c]* comparison and -17¢ for the average of the *nf_2[c] –nf3_[c]* and *nf_3[c]-nf2_[c]* comparisons in the bottom panel of Table 8 replicate the differing effects of changing the number of competitors in this kind of design observed by Davis and Wilson (2007) and Davis (2007). (Recall that entries for the spinoffs in Table 8 are the price difference of a triopoly over a

Prior to concluding, we comment on the differences between some of the results observed here with specific conclusions regarding the effects of re-matching and capacity constraints that Engel (2006) draws from his meta-analysis of oligopoly experiments. One possible explanation for the differing conclusions across papers is that our results, examined in a specific parameterized context, are not robust. Another possibility is that the extensive repetition in our markets falls outside the previously observed range of observations. While we do not dismiss either of these explanations, we note that certain elements of Engel's methodology may account for the observed differences as well. First, Engel's conclusions regarding the effects of re-matching and capacity constraints are driven by comparisons of experiments where re-matching or capacity constraints were uniformly present with other experiments where the same procedure was uniformly absent. Results of such 'across experiment' comparisons could be spurious if investigators tended to use one or the other of these procedures in designs that elicited either generally high or generally low prices. For example, suppose that investigators used re-matching procedures primarily in designs that were susceptible to high prices (e.g., with the thought that re-matching might control for the effects of fixed groups in explaining supra-competitive outcomes.) In this case a design-choice bias would generate greater departures from competitive prices under re-matching than would be observed in many fixed group designs. Notably, confining attention to experiments where a re-matching procedure <u>was</u> applied as a treatment condition, Engel finds no statistically difference in outcomes. Identical comments apply to Engel's conclusion regarding the price reducing effects of capacity constraints, with the minor qualification that in this second case Engel's sample includes no experiments where capacity constraints were a treatment.[20]

A second, more technical issue regards Engel's interpretation of the multi-dimensional analyses of variance. Engel allows for both multiple primary effects and

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comparable duopoly. Thus, the average effect of a duopoly over a triopoly structure in the final segment of the *nf* treatments is [9¢ +- 43¢]/2 = -17¢.)

[20] Notably, a number of the studies included in Engel references do examine the effects of capacity constraints that create market power. Pertinent examples include Kruse et al. (1994), Davis and Holt, (1994) and Davis and Wilson (2000). It is the case, however, that in these designs no seller could unilaterally service the entire market, even when no seller possessed market power. As mentioned above, Davis (2007) reports an experiment conducted under circumstances that parallel the present study where capacity constraints for each seller were eliminated.

interaction effects. However, he treats interaction terms largely as 'controls' and draws conclusions mostly from the primary effects. As Braumoeller (2004) observes, such an interpretation, while typical in the literature using ANOVA analyses, can be highly misleading. Given significant interaction effects, the direction, magnitude and statistical significance of estimated main (primary) effects, taken in isolation, are unreliable. The best that can be done is to study primary effects for every environment (as reported, for example, in Tables 4 to 8 above). As we illustrated, significant main effects do not always translate into significant differences between individual cells of an experiment, while non-significant main effects together with significant interaction effects may result in significant differences between individual cells.

5. Conclusions

Experimental results indicate that protocol choices 'matter', and largely in the expected ways. That is, experiments conducted under conditions of full information and with re-matching yield outcomes that are closest to static Nash equilibrium predictions. These results are useful in that they illustrate both the benefits of and the restrictions imposed by protocol choices. To address questions regarding the organizing power of static Nash equilibrium predictions, a full information/re-matching environment generates the most useful information.[21] Here, for example, our full information/remixing environment provides data most pertinent to the absolute and relative drawing power of static Nash mixing predictions.

On the other hand, many theories, particularly in industrial organization, are more or less formal static theories intended to generate predictions for (indefinitely repeated limited information) markets.   If the primary research questions of interest regard the capacity of static predictions to organize market outcomes, then protocol combinations closer to the relevant naturally occurring circumstance are more informative. Here, for example, in limited information, fixed group markets, we see that increased variability within treatments and a propensity toward tacit collusion undermine comparative static predictions.

[21] We observe, however that testing a theory strictly on its 'domain' is a difficult, if not impossible undertaking. Unless the experimenter could verify that participants truly understood the instructions, all information regarding underlying market conditions, and that participants fully believed that a re-matching protocol made useless any efforts to pursue a repeated game strategy, the possibility of 'slippage' between the theory and its test exist.

Perhaps the greatest potential problem we see in the selection of procedures lies in drawing inappropriate inferences from a given protocol. In the present experiment, an investigator might find a full information/re-matching protocol to be fairly natural choice, given that the experiment evaluates the drawing power of static Nash mixing predictions. Despite rejecting the hypothesis that sellers mix according to the static Nash equilibrium prediction, the investigator might well also observe that markets do respond to changes in the number of sellers in the predicted way. Although the data from the full information/re-matching environment do support such a conclusion, concluding more generally that these experimental results indicate that static predictions organize market outcomes would be inappropriate (as results of the our information/fixed group markets suggest). As re-matching protocols become progressively more conventional, we think it well to emphasize that this type of error should be avoided. Experimentalists, we think, would do well to pick protocols carefully in terms of the questions they wish to address, and to interpret conclusions in light of the selected protocols.

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Table 1. Matrix of Treatments

	Rematched Groups (Number of Sessions)	Fixed Groups (Number of Sessions)
Information	*ir32 (2)*	*if32 (1)*
	ir23 (2)	*if23 (1)*
No Information	*nr32 (2)*	*nf32 (1)*
	nr23 (2)	*nf23 (1)*

Table 2. Parameter Combinations that Yield Treatment Cell Means.*

Triopolies	
ir3	β_o
nr3	$\beta_o + \beta_{ni}$
if3	$\beta_o + \beta_{fixed}$
nf3	$\beta_o + \beta_{ni} + \beta_{fixed} + \beta_{nifixed}$
Duopolies	
ir2	$\beta_o + \beta_{two}$
nr2	$\beta_o + \beta_{two} + \beta_{ni} + \beta_{ni2}$
if2	$\beta_o + \beta_{two} + \beta_{fixed} + \beta_{fixed2}$
nf2	$\beta_o + \beta_{two} + \beta_{ni} + \beta_{fixed} + \beta_{nifixed} + \beta_{ni2} + \beta_{fixed2} + \beta_{nifixed2}$

*Separate regressions are conducted for first-in-sequence and second-in-sequence observations

Table 3. Regression Analysis of Average Price Postings

	First Sequence Segment			Second Sequence Segment		
	(1a)	(2a)	(3a)	(1b)	(2b)	(3b)
Coefficient	1-53	54-106	107-160	1-53	54-106	107-160
β_o	4.42***	4.41***	4.23***	4.22***	4.17***	4.14***
	(0.07)	(0.03)	(0.02)	(0.10)	(0.16)	(0.07)
β_{ni}	-0.25	0.47**	0.55***	0.19*	-0.1	0.05
	(0.29)	(0.17)	(0.05)	(0.11)	(0.16)	(0.09)
β_{fixed}	0.12	0.34**	0.49***	0.59**	0.43	0.64*
	(0.18)	(0.16)	(0.16)	(0.24)	(0.33)	(0.37)
$\beta_{nifixed}$	-0.55	-0.5	0.18	-0.23	0.26	-0.02
	(0.70)	(0.41)	(0.19)	(0.39)	(0.46)	(0.45)
β_{two}	0.37**	0.38***	0.62***	0.93***	0.83***	0.86***
	(0.14)	(0.11)	(0.07)	(0.12)	(0.17)	(0.14)
β_{ni2}	0.06	-0.38*	-0.52***	-0.24	0.15	-0.01
	(0.31)	(0.21)	(0.10)	(0.17)	(0.18)	(0.15)
β_{fixed2}	0.13	-0.08	-0.32	-0.46	-0.23	-0.55
	(0.29)	(0.29)	(0.28)	(0.31)	(0.39)	(0.46)
$\beta_{nifixed2}$	0.01	-0.13	-0.85*	0.73	0.11	0.43
	(0.76)	(0.55)	(0.47)	(0.46)	(0.54)	(0.54)
N	144	144	144	144	144	144
R^2	0.26	0.16	0.36	0.58	0.58	0.49
F(7,27)	2.89**	16.11***	61.52***	43.79***	430.74***	52.30***

Key: *** *p-value*<0.01, ** *p-value*<0.05, * *p-value*<0.10 (two tailed tests).

Table 4. Average Prices Per Treatment Cell.

Segment	**Pds. 1-53**	**Pds. 54-106**	**Pds. 107-160**
	Triopolies with Re-Matching ($\bar{p} = \$3.52$)		
*ir3_*	4.42***	4.41***	4.23***
ir_3	4.22***	4.17***	4.14***
*nr3_*	4.18*	4.88***	4.78***
nr_3	4.41***	4.08***	4.18***
	Triopolies with Fixed Groups ($\bar{p} = \$3.52$)		
*if3_*	4.54***	4.75***	4.72***
if_3	4.82***	4.61***	4.78**
*nf3_*	3.75	4.72**	5.45***
nf_3	4.78***	4.77***	4.81***
	Duopolies with Re-Matching ($\bar{p} = \$4.78$)		
*ir2_*	4.79	4.79	4.84
ir_2	5.15***	5.01***	5.00
*nr2_*	4.61	4.88*	4.88
nr_2	5.10**	5.05***	5.04***
	Duopolies with Fixed Groups ($\bar{p} = \$4.78$)		
*if2_*	5.04	5.05	5.02
if_2	5.29**	5.21	5.09
*nf2_*	4.32*	4.51	4.38
nf_2	5.73***	5.63***	5.54***

Key: Asterisks highlight significant differences of observed mean from mean of predicted N.E. mixing distribution. *** *p value*<0.01, ** *p-value*<0.05, * *p-value*<0.10 (two tailed tests). The sequential Bonferroni method is used to adjust for the 8 multiple comparisons in each column (e.g., 8 comparisons for regressions using first-in-sequence observations and 8 comparisons for regressions using second-in-sequence observations).

Table 5. Duopoly vs. Triopoly Market Structures.

Segment	**Pds. 1-53**	**Pds. 54-106**	**Pds. 107-160**
Sessions with Re-Matching			
*ir2_-ir3_*	0.37**	0.38*	0.62***
ir_2-ir_3	0.93***	0.83***	0.86***
*nr2_-nr3_*	0.43	0.00	0.10
nr_2-nr_3	0.68***	0.98***	0.85***
Sessions with Fixed Groups			
*if2_-if3_*	0.50	0.30	0.30
if_2-if_3	0.47	0.61	0.31
*nf2_-nf3_*	0.57	-0.21	-1.07**
nf_2-nf_3	0.96***	0.86**	0.73**

Key: Asterisks highlight significant differences from zero. *** *p-value* <.01, ** *p-value*<.05, * *p-value*<.10 (two tailed tests). The sequential Bonferroni method is used to adjust for the 4 multiple comparisons in each column (e.g., 4 comparisons for regressions using first-in-sequence observations and 4 comparisons for regressions using second-in-sequence observations).

Table 6. Mean Differences Across Fixed Group/ Re-Matching Conditions, and Tests for Re-Matching Effects

Segment	**Pds. 1-53**	**Pds. 54-106**	**Pds. 107-160**
Tripolies			
*if3_-ir3_*	0.12	0.34	0.49**
if_3-ir_3	0.59*	0.43	0.64
*nf3_-nr3_*	-0.43	-0.17	0.67***
nf_3-nr_3	0.36	0.69	0.63**
Duopolies			
*if2_-ir2_*	0.24	0.26	0.18
if_2-ir_2	0.14	0.21	0.09
*nf2_-nr2_*	-0.29	-0.38	-0.50
nf_2-nr_2	0.64***	0.58**	0.50***

Key: Asterisks highlight significant differences from zero. *** *p-value* <0.01, ** *p-value*<0.05, * *p-value* < 0.10 (two tailed tests). The sequential Bonferroni method is used to adjust for the 4 multiple comparisons in each column (e.g., 4 comparisons for regressions using first-in-sequence observations and 4 comparisons for regressions using second-in-sequence observations).

Table 7. Mean Differences Across Information Conditions, and Tests for Information Effects

Segment	**Pds. 1-53**	**Pds. 54-106**	**Pds. 107-160**
	Triopolies		
*ir3_-nr3_*	0.25	-0.47**	-0.55***
ir_3-nr_3	-0.19	0.10	-0.05
*if3_-nf3_*	0.79	0.03	-0.73***
if_3-nf_3	0.04	-0.16	-0.03
	Duopolies		
*ir2_-nr2_*	0.19	-0.09	-0.03
ir_2-nr_2	0.06	-0.05	-0.04
*if2_-nf2_*	0.72*	0.54	0.64
if_2-nf_2	-0.44	-0.42	-0.45

Key: Asterisks highlight significant differences from zero. *** *p-value*<0.01, ** *p-value*<0.05, * *p-value*<0.10 (two tailed tests). The sequential Bonferroni method is used to adjust for the 4 multiple comparisons in each column (e.g., 4 comparisons for regressions using first-in-sequence observations and 4 comparisons for regressions using second-in-sequence observations).

Table 8. Differences Between Mean Prices for the Second Sequence and Periods 107-160 of the First Sequence.

Segment *i*= {a, b, c}		2nd Sequence Periods (a) 1-53	(b) 54-106	(c) 107-160
	Sessions with Re-Matching			
Mergers	*ir_2[i]-ir3_[c]*	0.93**††	0.78**	0.77††
	nr_2[i]-nr3_[c]	0.32	0.27	0.26
Spinoffs	*ir_[3]- ir2_[c]*	-0.62†††	-0.67	-0.71†††
	nr_3[i]-nr2_[c]	-0.47***†††	-0.80***	-0.69†††
	Sessions with Fixed Groups			
Mergers	*if_2[i]-if3_[c]*	0.57†	0.49	0.37†
	nf_2[i]-nf3_[c]	0.28†	0.18	0.09†
Spinoffs	*if_3[i]-if2_[c]*	-0.20	-0.42	-0.24
	nf_3[i]-nf2_[c]	0.39	0.39	0.43

Key: Asterisks indicate significant differences for comparisons across (a) and (b) in a row. Crosses indicate significant differences for comparisons across (a) and (c).*** ††† *p-value*<0.01,** †† *p-value*<0.05, *† *p-value*<0.10 (two tailed tests) No differences across (b) and (c) are significant at conventional significance levels.. . The sequential Bonferroni method is used to adjust for the 8 multiple comparisons in each column.

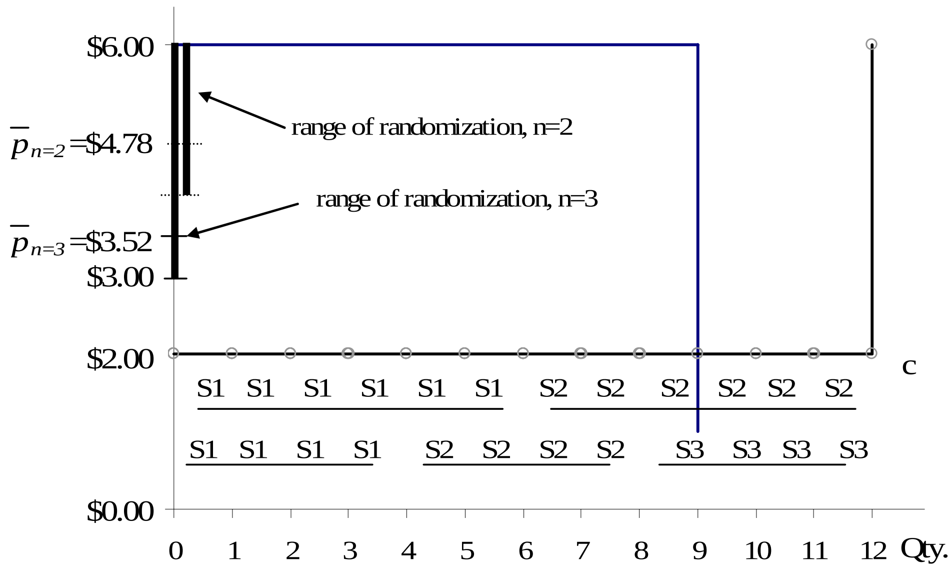

Figure 1. Two and Three Seller 'Market Power' Designs.