Experimental Methods and Antitrust Policy[*]

by

Douglas D. Davis
Department of Economics, University of Mississippi
University, Mississippi 38677
and
Department of Economics, Virginia Commonwealth University
Richmond VA 23284-4000
E-mail: *dddavis@bus.olemiss.edu*
Phone: (662) 915-5841 Fax: (662) 915-5821

and

Bart J. Wilson
Economic Science Laboratory, University of Arizona,
Tucson, AZ 85721-0108
E-mail: *bwilson@econlab.arizona.edu*
Phone: (520) 621-7104 Fax: (520) 621-5642

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Abstract

Antitrust policy has evolved substantially in the last two decades. Although the incorporation of new empirical methods has played a critical role in this evolutionary process, with only a few exceptions experimental tools have not been employed. This omission is unfortunate, because appropriately constructed experiments can uniquely provide important insights. This paper summarizes some of our recent research as a means of illustrating the potential value of antitrust policy experiments.

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1. Introduction

Over the last twenty years, antitrust enforcement policy has evolved substantially. A primary impetus for this evolution has been the continuing advancement of oligopoly theory. Changes in the Horizontal Mergers Guidelines issued jointly by the Department of Justice (DOJ) and the Federal Trade Commission (FTC) illustrate. By the early 1980's academic economists had grown vocally skeptical of the antitrust agencies' strictly structuralist approach for assessing the competitive consequences of horizontal mergers. A major revision of the Guidelines in 1982 abandoned this strictly structuralist stance and articulated, for the first time, a strategy for balancing the potential benefits of horizontal consolidations with concerns about market power increases. The enforcement focus of the 1982 Guidelines centered on deterring *cooperative* behavior among firms. If a merger increased industry concentration by a critical amount, the consolidation would be challenged if the government deemed the transaction sufficiently likely to result in improved and effective coordinated activity.

The Guidelines were revised again in 1992 to add an enforcement focus on *unilateral* activity. A prominent result of several important models of oligopoly interactions written in the latter part of the 1980's is that reducing the number of sellers changes the underlying strategic situation in a manner that generates higher prices via unilateral activity (e.g., Davidson and Deneckere, 1985; Farrell and Shapiro, 1990). Most horizontal mergers are now evaluated in light of unilateral effects (Freob, 1994). The shift in emphasis to include unilateral activity has led to the consideration of horizontal mergers in terms of specific models of oligopoly performance (see e.g., Werden and Froeb, 1994).

Innovations in computing technology are a second motivation for the evolution of antitrust enforcement policy. The widespread adoption of computer-based management information systems for inventories, sales, orders, prices, and contracts generates volumes of potential firm-level data in a specificity and detail that was previously unimaginable. Antitrust investigations have taken advantage of this newly available data by including more rigorous and extensive empirical analysis as complementary support to the theoretical arguments in such areas as merger analysis and price-fixing. Increased computing power has also affected theoretical modeling in antitrust analysis. Cheap and quick computer processing permits the use of more sophisticated models and their associated simulations. Moreover, these simulation models require input estimates of the own price and cross price elasticities from firm-level data.

Although experimental methods have been used extensively to evaluate industrial organization issues (see, e.g., Plott, 1989; Davis and Holt, 1993; or Holt, 1995 for reviews), relatively little experimental work has focused explicitly on antitrust policy issues (Grether and Plott, 1984 and Wellford, 1990 are notable exceptions).[1] We find this omission both curious and regrettable. It is curious because questions central to antitrust analysis provided much of the impetus for using experimental methods in economics. Seminal contributors including Fouraker and Seigel (1963), Dolbear et al. (1968) and Friedman (1967) report oligopoly experiments designed to examine the effects of changing the number of sellers and information conditions on price-cost markups. Further, a study of "facilitating practices" like those used by antiknock lead compound producers was one of the pioneering policy applications of experiments (Grether and Plott, 1984).

The relative paucity of experimental methods in the evolution of antitrust policy over the last decade is regrettable because experimental methods can provide potentially important information not available from other empirical sources. Granted, the streamlined environments and the preferred subject pools (undergraduate volunteers) used by experimentalists provide a poor substitute for natural markets. However, the appropriate use of experimental methods lies not in the capacity to simulate markets, but rather in the links between theory and policy. Policies are based on more or less explicit economic theories. Experiments provide the opportunity to evaluate the capacity of the theory to organize behavior in a streamlined "best shot" context.

To the extent that policies are based on explicit models, market experiments conducted on the domain of the theory can evaluate the behavioral relevance of the institutions that the models stipulate implicitly. Evidence of this type is simply unavailable in natural contexts. To give a concrete example, horizontal merger policy for differentiated-product markets is increasingly based on a particular model of oligopolistic interactions (see e.g., Werden and Froeb, 1994). Experiments can help evaluate the drawing power and stability of the equilibrium predictions generated by such a model in the ideal case where demand and cost structures are pre-specified and where sellers interact exactly as the model stipulates. The failure of

[1] Here we distinguish between experiments with general antitrust relevance, such that examine topics such as collusion, predatory pricing and market power, with general antitrust relevance, and experiments designed to evaluate particular antitrust policies, such as the usefulness of a simulation tool used by antitrust authorities to

equilibrium predictions to organize behavior in this "best shot" context would cast some doubt on the usefulness of this model as a basis for policy. As a second example, consider the value of examining bidding data for suspicious patterns as a means of detecting collusion. Critics of such a policy would argue that given appropriate underlying conditions, virtually any "suspicious" bid pattern can be generated as a noncooperative equilibrium for the relevant bidding "game." Examining bid patterns in laboratory environments specifically designed to elicit "suspicious" outcomes in noncooperative environments can provide some evidence as to whether noncooperative equilibrium predictions can have a behavioral drawing power sufficiently strong to render useless any effort to detect collusion from bid data.

Experiments can also be useful in the policy development process by highlighting the potential importance of considerations omitted from standard oligopoly models. For example, the treatment of cost savings in merger enforcement analysis is based on oligopoly models with constant marginal costs. However, cost savings plausibly may affect the cost schedules of the merging parties in a variety of ways, and the competitive consequences of these cost savings can turn on where on the industry supply schedule they impact. Experiments, used in conjunction with theoretical analysis can illustrate the potential dangers of overlooking the interrelations between cost savings and their impact on the incentives to exercise market power.

This paper reviews our recent research applying experimental methods to examine antitrust policy questions. As will be seen, experimental data generally provide neither the poison arrow that condemns a theory nor the impenetrable armor that unequivocally endorses it.[2] Such critical evidence of any type, however, rarely appears. Rather, experimental evidence is generally insightful, for three reasons. First, the experimental data provide some evidence about the behavioral importance of the assumptions underlying a model. Second, the process of designing a "best shot" environment prompts reflection and debate on the reasonableness of those assumptions. Third, the experimental data frequently suggest something unanticipated that stimulates further thinking.

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identify potential problems with horizontal mergers, or the capacity of antitrust authorities to detect collusive behavior from the actions of firms. The paucity of contributions is in the latter category.

[2] In retrospect, the overwhelmingly convincing nature of results in the Grether and Plott (1984) experiment may have actually hampered the use of experiments in antitrust policy. In Ethyl et al. counsel for the defense argued that a collection of "facilitating practices" could not result in elevated prices. Grether and Plott demonstrate quite convincingly that in a simple world, elevated prices are possible. Rarely do the policy questions involve such extreme assertions. More generally, experimental evidence remains useful in a policy context, even though the evidence is less conclusive.

The paper is organized as follows. Section 2, below summarizes an experiment designed to evaluate detection schemes for bid-rigging. Section 3 describes an experiment that examines the links between market power and the way that merger-specific cost savings affect the merging parties' cost curves. Section 4 reviews our recent research evaluating the behavioral relevance of the Antitrust Logit Model "ALM" model. Section 5 concludes.

2. Price Fixing in Sealed Bid Auctions

Despite improvements in the quality of data generated by firms, the collection of data relevant to the workings of effective conspiracies remains extremely problematic. The existence of a conspiracy is most frequently detected via a tip-off or complaint, and there are good reasons to suspect that conspiracies detected in this fashion tend to be the least profitable. A conspirator is likely to "rat" on co-conspirators only if disenchanted with the scheme, suggesting that the conspiracy may collapse anyway. Similarly suspect are conspirators who successfully raise prices, but are sufficiently clumsy to raise the protests of their buyers.

Posner (1969), however, contends that such explicit evidence is generally unnecessary. He argues that the distinction between "explicit" and "tacit" collusion is artificial because the behavioral patterns of conspirators differs identifiably from that of competitors. Commentators such as Kuhlman (1969) and Gallo (1977) further advocate the use of continuous computerized monitors to examine bid patterns in suspect environments. However, as observed by Marshall and Muerer (1998), a potentially condemning defect of any attempt to detect collusion via bid patterns is that, depending on the underlying market conditions, virtually any "suspect" bidding pattern can be generated as a stage-game Nash (e.g. competitive) equilibrium.[3]

Identical prices, for example, are one pattern that is typically viewed as suspect. However, identical pricing may be part of a static noncooperative equilibrium. The supply and demand arrays in Figure 1 present a stylized characterization of a situation where identical prices are predicted as a static Nash Equilibrium. A situation like this might arise, for example, in procurement auctions. The market consists of a single buyer *B*, and four sellers, *S1* to *S4*. Buyer *B* is fully revealing in the sense that the buyer will purchase all units that can be bought without incurring a loss. Furthermore, in the event of identical prices buyer *B* will rotate purchases

[3] In the discussion that follows we assume that conspirators cannot strike binding agreements. As mentioned at the end of this section, if sellers can strike binding agreements, cooperative behavior can be disguised perfectly.

among the tied sellers. McAfee and McMillan (1992) report that government procurement agencies often use the rotating rule when collusion is not suspected. The numbers 1 to 4, printed below cost steps on the supply curve, indicate the identities of the units allocated to each seller *S1* to *S4*, respectively. We denote this environment as the *set cost* design.

Viewed as a stage game, establishing that all equilibria for this market involve the price P_c is straightforward. Any unilateral price reduction below P_c will reduce earnings on a unit certain to sell at price P_c, and any unilateral price increase will reduce earnings to zero, provided that at least one of the other sellers offers at least one zero profit unit at price P_c. To the extent that static Nash predictions organize behavior in this context, we should expect to observe "suspect" identical prices in a noncooperative environment. We note that the Nash equilibrium in this context is weak in the sense that some sellers must be indifferent between their elected strategy and cutting their offer-quantity to a single unit. However, weak pure-strategy equilibria are an unavoidable feature of any plausible procurement auction context.[4]

Bid rotations, a second scheme that is typically cited as a suspect pattern, may also be consistent with a noncooperative Nash equilibrium (Zona, 1986). Consider, for example, a market with a limited number of sellers, each of whom bid under conditions of a temporally binding capacity constraint, as might be the case in the bidding for construction or other service contracts. After winning a contract, the seller is "busy" for next period, and the marginal cost of taking on any new contracts increases substantially. Costs decrease in subsequent periods, as contractual obligations become increasingly satisfied. Eventually, costs return to their initial level. A simple representation of this environment is illustrated in the sequence of markets shown in Figure 2. The figure is formatted as Figure 1, with the numbers 1 to 4 denoting the allocations of costs and capacity for each seller *S1* to *S4*. A single fully revealing buyer purchases up to 4 units. We denote this environment as the *endogenous cost* design.

At the outset all sellers have identical costs, as seen in panel A. After a first round of bidding, one seller (*S1*) wins a contract. As shown in panel B, *S1*'s costs rise substantially. In panel C, assume that *S2* wins the bidding in a second round. Then, *S2*'s costs increase to the level of *S1*'s in period 2 while unit costs for *S1* decay as this seller frees up some capacity.

[4] The set of Nash equilibria includes all combinations of bids at P_c where at least two sellers offer zero-profit units. To generate a strong Nash equilibrium, the demand curve must intersect the cost curve vertically, so that supply is exhausted at the competitive price. (See for example the baseline sessions for the cost savings/market power

Finally, suppose *S3* wins the auction in period 3. Then as shown in panel D, each seller occupies a different step on the period 4 market supply schedule. For an indefinitely repeated version of this game, a family of noncooperative Nash equilibria exists in which each seller services the entire market each period.[5] Depending on the discount factor, a variety of prices up to the joint profit maximizing price can be part of an equilibrium strategy.[6] Of course, identical costs and/or bid rotations might also be anticipated in variants of the above design where the sellers engage in some form of explicit cooperation.

Experimental investigation of the simple designs presented above can render useful insights in two ways. First, provided that sellers collude when given opportunities to communicate explicitly, we can examine whether conspirators in fact select "suspect" bidding schemes. Second, inducing comparable markets without opportunities for explicit collusion provides some insight into whether the drawing power of static noncooperative Nash predictions are sufficiently strong to generate the same "suspect" patterns absent collusion.

Davis and Wilson (2000b) report an experiment designed to shed some light on these issues. The experiment consisted of a series of sixteen 40-period "posted offer" markets. Trading periods consisted of the standard two-step sequence: First, sellers, induced with unit costs, simultaneously submit bids. Second, once all bids are submitted, the offer prices (but not offer quantities) are announced publicly and a fully revealing buyer makes purchases. In these markets neither costs, nor demand, nor the number of units actually sold by a seller in a period

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experiment described in the next section). Such a design is doubtfully relevant in a procurement auction, where a single buyer solicits bids for a pre-determined quantity of a good or service from multiple sellers.

[5] Bid rotations will occur if the probability of tied bids are zero, or if in the event of a tie, the buyer selects one of the tied sellers and makes as many purchases as possible. If the buyer rotates purchases among tied sellers, alternative equilibria can arise where sellers share capacity each period.

[6] A variety of bids are consistent with a subgame perfect Nash equilibrium in this game. In particular, a transaction price of $P_c - e$ is part of an equilibrium strategy where all sellers post a price equal to cost unless they are the low cost seller, in which case they post $P_c - e$. That no unilateral deviation from this strategy is possible is readily verified. Higher transaction prices could also be part of an equilibrium strategy, provided that the minimum price posted by sellers without the minimum cost slightly exceeds the price posting of the low cost seller. The multiplicity of prices consistent with an equilibrium does not detract from our interest in this game as a means of identifying equilibrium quantity rotations. Equilibrium refinements, however, can narrow the set of equilibrium prices. In particular, notice that the above strategy with a transactions price at $P_c - e$. fails trembling-hand perfection. Suppose that the low cost seller "trembled" by posting a price above $P_c - e$ with some positive probability. Then the seller who wins the auction "out-of-sequence" does so at the cost of pushing him or herself back in the cost rotation. For example, in panel D of Figure 2, if *S4* posts a price above *S1*, *S1* would forego earnings as the low cost seller in the subsequent period. Since absent a tremble price postings by *S1*, *S2* and *S3* do not result in transactions, these sellers will raise prices even as the probability that *S4* "trembles" goes to zero. Strategies satisfying trembling hand perfection involve higher transactions prices and high discount rates. See Davis and Wilson (2000b) for details.

are provided as public information. Further, the number of periods in the session is not announced in advance.

The experiment was conducted with an incomplete block 2 × 2 design. The sessions were split evenly between the *set cost* and *endogenous cost* designs. Within each of these designs, we again evenly divide the sessions into two groups: sessions in which the sellers were afforded the opportunity to talk explicitly about the market (denoted as *communication* sessions) and those sessions in which they were not (denoted as *no communication* sessions). The blocks are incomplete since the *communication* condition was not varied within sessions. Prior research indicates that changing the communication condition exerts considerable carryover effects. (See e.g., Daughety and Forysthe, 1987).

In the *communication* sessions sellers were given the opportunity to talk for 3 or 4 minutes prior to every fourth period, beginning with period 1. Discussions were unstructured and unregulated, except for the following restrictions: (1) nothing quantitative could be said about costs, (2) nothing could be said about previously-taken actions that are not publicly observable (such as defections from un-monitorable quantity restriction schemes), (3) side-payments and physical threats are prohibited, (4) any agreements among sellers are unenforceable, and (5) participants are not obligated to comply with the terms of any arrangement.[7]

The contract sequences from some representative session segments, shown as the four panels in Figures 3 and 4, provide some insight into results. Within each panel trading periods are summarized as vertical stripes. Information about sellers *S1* to *S4* is listed horizontally for each period and in respective order: crosses (+) denote costs of units sold, hollow dots (○) offer prices, and solid dots (•) contracts. Figure 3 illustrates contract sequences for the last 10 periods (periods 31-40) of a pair of representative markets conducted in the *set cost* design. Forming effective conspiracies under the above conditions is not a trivial task, as observed by Davis and Holt (1998). Nevertheless, we observe in the *set cost/communication* session in the upper panel, that the sellers develop a stable conspiracy at the monopoly price after several periods of practice. Each period, each seller both offers and sells a single low-cost unit at the

[7] Participants were given the instructions for collusive opportunities used by Davis and Holt (1998). The restrictions mentioned in the text are imposed to create an environment where information conditions are incomplete, as would be the case in natural contexts, and where violating collusive arrangements do not require explicit misrepresentation or violations of social norms.

monopoly price. Prices are also fairly stable in the final ten periods of a *set cost*/*no communication* session, shown in the lower panel of the figure. Unlike its *communication* counterpart, identical prices are infrequent. Even after 30 periods of practice, a combination of futile attempts at price signaling (periods 33, 36 and 40), and gaming about the competitive price (note in particular the contract losses for in seller both in period 37 and in 39) generate heterogeneous prices. Thus, in this *set cost* environment, a unique static (weak) Nash equilibrium prediction is not sufficient to generate "suspect" identical pricing, absent some coordinated activity.

The session segments shown in Figure 4 (formatted in the same way as Figure 3) provide a flavor of results in the *endogenous cost* design. Consider first the session with the *communication* treatment, shown in the upper panel of the figure. As with the *set cost* design sessions, organizing an effective and stable collusive arrangement was a nontrivial task. However, by the end of the sessions sellers had organized an efficient scheme, this time involving sales rotations. Notice that, *S3* sells four low-cost units at the monopoly price P_m, in period 31 followed by *S4*, then *S2* and *S1*. Sales rotation is also a feature of the *no communication* session, shown in the bottom panel, with sales rotating at or near the competitive price P_c from *S4* to *S1*, then to *S3* and *S2*. Comparing across panels, however, observe further, that markets with and without communication opportunities differ by more than price–cost mark-ups. In the session with collusive opportunities, sellers other than the low-cost seller uniformly submit high prices to "stay out of the way" of any price searching activity of the low cost seller. In contrast, without collusive opportunities the non-winning bids are much more scattered to reflect the cost differences of the sellers each period.

Figure 5, which summarizes the incidence of identical bids and sales quantity rotations for periods 31-40 of all sessions, confirms the observations of the representative contract sequences in Figures 3 and 4. Two features of the figure bear emphasis. First, although several instances of both identical prices and bid rotations occurred, notice that identical prices (the front row of bars) occurred exclusively in the *set cost* markets, and sales rotations (the back row of bars) occurred exclusively in the *endogenous cost* markets. Thus, the underlying cost structure prominently affects observed bidding patterns. Second, notice that quantity rotations occur with some frequency in *endogenous cost* markets, both with and without the explicit ability to conspire.

Consider for a moment the way that these results might contribute to the debate regarding the appropriate treatment of collusion investigations. With respect to the *set cost* markets, it would, of course, be preliminary to suggest that a judge regard every instance of identical pricing brought before him or her as conclusive evidence of a conspiracy. However, the results do prompt reflection on what identical pricing implies. If unique noncooperative static (weak) Nash equilibria are insufficient to generate identical prices in a very simple world, when would such prices be observed? Would more experienced participants eventually tire of "gaming" and settle on the same price?[8] On the other hand, perhaps the identical prices are a consequence of other institutional arrangements, such as multi-market competition, information sharing, or joint cost synergies. If this is the case, the innocuousness of such practices bears investigation.[9]

In the *endogenous cost* markets, we have a simple example of an instance where "suspect" market rotations do not necessarily indicate cooperative behavior. Nevertheless, cooperation is potentially identifiable from the correlation between losing bids and costs. As suggested by the relatively losing bid series shown in the upper panel of Figure 4, when sellers conspire, the correlation between costs and losing bids breaks down. This illustrates the idea underlying the econometric approach to identifying collusion taken by Porter and Zona (1994, 1997) and others. We are continuing this line of inquiry by investigating the accuracy of the econometric techniques now being used to identify conspiratorial behavior. (For example, we observe that idiosyncrasies in bid patterns by the cooperating sellers can increase substantially bid-cost correlations.) The laboratory is an excellent context to evaluate these tools, since only in the laboratory does the investigator fully know both the underlying cost structure, and when sellers are cooperating.

Overall, results suggest that cooperative behavior can often be identified, but only if a very considerable amount is known about underlying market conditions.[10] Attention here has been restricted to "weak form" conspiracies, where agreements are not binding, and sellers thus face a continual opportunity to defect. In the theoretical literature, weak form conspiracies are differentiated from "strong form" conspiracies, where sellers strike binding arrangements.

[8] Reasoning by extension, does this suggest that airline executives are typically bored?

[9] For example, Anton and Yao (1992) build a model where identical prices are predicted when sellers enjoy "dual source efficiency", or reduced costs when two sellers rather than one service a contract. Cason and Davis (1995) report an experiment designed to evaluate whether publicity of pricing information and multi-market competition could generate higher prices. Indeed, supra competitive prices were observed.

[10] Hendricks and Porter (1989) make a similar argument.

Strong form conspiracies can perfectly disguise cooperative behavior (e.g., LaCasse, 1995). As a legal matter, the machinery necessary to identify strong from conspiracies is usually presumed sufficient to render the arrangement observable, but it seems to us that a context somewhere between strong and weak form conspiracies is relevant. Specifically, what type of arrangements might weak form conspirators make when they are worried about detection? Again, manipulating communication conditions in the laboratory may yield some insight into these patterns.

3. Enforcement Standards for Merger-Specific Efficiencies

This second application, as well as the one discussed in the following section pertain to enforcement policy for horizontal mergers. The experiment described in this section focuses on the treatment of efficiency claims in merger analysis. The increasing recognition by antitrust authorities that proposed consolidations among horizontal competitors may have socially desirable consequences has motivated closer attention to how potential efficiencies should be treated when evaluating proposed consolidations. The 1992 Guidelines were revised in 1997 solely for the purpose of articulating standards for the treatment of efficiencies. Under the 1997 Guidelines, the burden of showing potential anti-competitive consequences of a merger remains on the federal government. The burden of demonstrating the verifiability and merger specificity of potential efficiencies, however, was implicitly placed onto the merging parties.

The cases of interest involve both some likelihood of increased market power and some reasonable claims to reductions of marginal cost. Since the mandate of the federal antitrust enforcement agencies is to maximize consumer surplus rather than total surplus, this question becomes a matter of assessing the likelihood that the efficiencies will be sufficient to offset any price increases brought about by changes in market power. In an effort to generate pertinent quantitative evidence, antitrust authorities recently have turned to considering rates at which firms historically passed-through cost savings to consumers by reducing prices. Firms that have passed along to consumers a sizable portion of past firm-specific cost-savings presumably have a stronger basis for arguing that any efficiencies arising from a merger offset any potential anti-competitive effects.[11]

[11] Historical pass-through rate estimates, for example, were a matter of considerable debate in the Staples/Office Depot merger litigation, *Federal Trade Commission v. Staples, Inc.*, 970 F. Supp. 1066, 1090 (D.D.C. 1997).

This policy stance is consistent with predictions concerning the interrelationship between market power and efficiencies demonstrated by Farrell and Shapiro (1990) for homogenous product Cournot competitors, and by Werden (1996) for differentiated-product Bertrand competitors. A potentially important limitation of these models for policy purposes is the common underlying assumption that all firms are constant marginal cost competitors. In many naturally occurring contexts, firms have increasing marginal cost schedules. Post-merger consumer welfare can be prominently affected by the way that cost savings affect increasing marginal cost schedules.

This point is illustrated by the supply and demand arrays for three triopolies shown in Figure 6, which is formatted in the same manner as the panels in Figures 1 and 2. The *baseline* design, shown in the left panel, illustrates a market where seller *S1* initially has power, since this seller is certain to sell a single unit at any price up to the limit price r. The limit price r cannot be part of a pure-strategy equilibrium for this game, since at a common price of r, *S1* would increase sales to three units by posting $r - e$. Similarly, *S1* would find shading on any common price profitable, down to a lower limit $P_{\min}$, where profits from selling three units as the low price seller equal profits from selling a single unit at r. The equilibrium for this market involves mixing over the range $P_{\min}$ to r. The equilibrium Nash mixing distributions can be derived following the method outlined by Holt and Solis-Soberon (1992). Notice also in the *baseline* design that power can be eliminated by reallocating one of seller *S1*'s high cost units to seller *S2*, as emphasized by the bolded and underlined 2. With the reallocation, any unilateral deviation above P_c reduces sales (and earnings) to zero for the deviator. Price P_c is the unique Nash equilibrium price for this market viewed as a stage game.

Suppose again that seller *S1* initially has market power, and then realizes a reduction in costs that affects marginal capacity, as illustrated by the *cost savings-no power* market in the center panel of the figure. The Nash equilibrium for this market viewed as a stage game is a unique pure-strategy equilibrium at P_c. As before, *S1* is assured of selling a single unit at the limit price. However, if the cost savings (the vertically-striped box) exceed the gain from raising the price to r (shown as the dotted box), market power disappears. This cost reduction generates a large pass-through rate for consumers as prices fall from a supra-competitive level to the competitive price.

Consider the effects of cost savings on only infra-marginal capacity, as shown as market *cost savings-power* in the right hand panel of Figure 6. In this case, despite a cost savings of the same magnitude that was induced in the middle panel (the vertically-striped box) pre-existing power conditions are unaffected. The cost savings do not affect market power since only the unit that is certain to sell is affected. This reduction of costs generates no benefits for consumers. Via parallel reasoning, starting in a *baseline* market without initial market power, a reallocation of units among sellers as might occur in a merger, can create market power that a simultaneous cost savings can either offset (as in the *cost savings-no power* panel), or leave unaffected (as in the *cost savings-power* panel).

In a recent paper (Davis and Wilson, 2000a), we examine the interactions between market power and cost savings. The experiment consisted of a series of 16 triopolies using combinations of the designs shown in Figure 6. Each market consisted of a series of 60 posted offer trading periods. As is standard in posted offer markets, at the outset of each period, sellers simultaneously make price and offer quantity decisions, which are recorded by a monitor. The monitor then announces prices and purchase decisions from a fully revealing buyer. Sellers were given complete information about the market demand schedule, but only private cost information.

The first 30 periods of each market was conducted in a baseline regime. In half of the markets, sellers had market power initially, while in the remaining eight markets no market power was present initially. After period 30 an unannounced regime change was introduced. Unit costs were reduced, and in the case of the sessions where sellers did not initially have power units were reallocated. For each initial baseline condition, the cost savings with market power condition was induced in four markets, and the cost savings without market power condition was induced in the remaining four markets. Thus, as with the bid-rigging experiment, an incomplete block 2 × 2 design was used: Although initial and terminal market power conditions are evenly rotated across treatments, the post intervention condition always included a cost savings. This limitation is justified, since our interest is in examining the effects only of cost savings (and not cost increases).

The mean price paths for some representative sessions, shown in the upper and lower panels of Figure 7 illustrate the general experimental results. In each panel, the hollow and solid dots illustrate mean prices in separate sessions. The two sessions shown in the upper panel

suggest that the cost savings hardly affect prices absent a change in underlying market power conditions. In the *no power baseline/cost savings-no power* session, illustrated with the solid dots, prices collapse on the competitive prediction, both before and after the cost savings. In contrast, in the *power baseline/cost savings-power* session, illustrated by the hollow dots, prices are both much higher and persistently volatile both before and after the synergy (as predicted by the static mixed strategy Nash equilibrium). As seen in the bottom panel, price performance changes quite markedly when market power conditions change with the introduction of cost savings. In the *no power baseline/cost savings-power* session, illustrated by the black dots, a cost savings that eliminates market power causes high initial prices to collapse to the competitive prediction. In the *power baseline/cost savings-no power* session, illustrated by the hollow dots, reallocating units in a way that creates market power, in spite of a cost savings, tends to raise prices substantially.

Figure 8 summarizes the relationship between "pass-through rates" and market power for experiment as a whole. The bars in the figure represent the proportion of the cost savings passed on in the form of lower prices in each of the sixteen sessions. This ratio has neither an upper bound nor a lower bound. However, positive pass-through rates indicate that prices fell, and hence savings were passed along, while negative pass-through rates indicate that prices increased after the regime change. Discount for the time being the deep negative spike for session n-ns2 shown as the second bar from the left (We will discuss this session momentarily). The remaining fifteen sessions indicate that introducing or eliminating market power clearly causes the price effects that occur with the regime change. Notice from the middle two bar clusters that cost savings that remove existing market power uniformly generate positive pass-through rates, while cost savings along with a reallocation of capacity that creates market power generates negative pass-through rates. From the leftmost and rightmost bar clusters, we find that cost savings that do not affect market power conditions are not passed along, regardless of whether market power was present initially (as in the rightmost column cluster) or not present (as in the leftmost column cluster).

Let us reflect for a moment on the way this experiment can contribute to the policy development process. In any natural context, institutional details are undoubtedly more complicated. Sellers may neither face the sharp capacity constraints of the type induced here, nor interact as strict price competitors. The relevance of these issues, both theoretical and

behavioral, certainly merits investigation. Nonetheless, this experiment conveys an important insight into the interaction between cost savings and market power. A policy of assessing efficiency claims in light of historic pass-through rates may err badly, because pass-through rates can be importantly affected by changes in underlying market power conditions. The incentives that drive this theoretical result can be very salient behaviorally. The point is not that merger-specific efficiencies are irrelevant to the price effects of mergers. Rather, it is that not all efficiencies are the same. To lower prices, cost savings must affect marginal costs. Even sizable efficiencies resulting from reductions in administrative overhead costs, for example, are unlikely to affect prices regardless of a pre-existing pass-through rate. On the other hand, a sizable reduction in the cost of producing marginal capacity may overwhelm any power conditions created by a merger.

A final insight comes from the deep spike in session *n-ns*2, the second of the initial and terminal no power sessions (the second bar from the left in Figure 8). As the spike suggests, prices increased very substantially after the intervention, despite the fact that the cost savings created no static market power. Figure 9 illustrates the sequence of price postings for this session. As illustrated in the figure, large seller *S1*, whose prices are illustrated as the solid dots, tried persistently to manipulate prices throughout the session. In periods 1-30, seller *S1* found this strategy to be exceedingly costly, since anytime he posted a price above P_c, he sold nothing, as occurred, for example, in periods 9, 10 and 12 through 17. The policy was ultimately ineffective and seller *S1* resorted to competitive pricing. In periods 31-60 seller *S1* again tried to stimulate a price increase through price signaling. After the cost savings, however, the signaling costs were considerably lower, since seller *S1* was certain to sell at least one unit at any price. Notice that in these periods the seller quite effectively raised the market price. Of course, the generality of this single session bears further inquiry, but this session suggests that all markets without static power may not be the same. In some circumstances, size may matter independent of static market power conditions. Even without power, a very large seller may be able to stimulate high prices through repeated signaling.

4. Differentiated Product Competition and the Antitrust Litigation Model.

An important consequence of the shift in emphasis in merger enforcement policy toward unilateral effects has been the consideration of horizontal mergers in terms of specific models of

oligopoly performance. Most prominent is the analysis of horizontal mergers in markets where products are differentiated, and are therefore imperfect substitutes. DOJ policy-analysts argue that in such contexts the standard concentration measure (the Herfindahl-Hirschman Index) provides little guidance as to when competitive problems might arise (see e.g., Werden and Froeb, 1996). In such cases petitioners may reasonably argue either that products of the merging parties are part of a very broad market and that concentration is thus very low, or that markets are very narrow and that the proposed consolidation presents no problem, since products of the merging entities are unrelated. As a substitute for standard concentration measures DOJ staff have developed an Antitrust Logit Model (ALM) merger simulation as a screening device to help guide determining when competitive problems arise in differentiated-products markets.

The ALM assumes a logit demand system and that sellers interact as Bertrand competitors (see Werden and Froeb, 1994 for a description).[12] The logit demand assumption is extremely useful in this context to generate predictions, since investigators only need data on prices and market shares, and measures of the demand elasticity and the rate of substitutability between products. The ALM predicts the effects of a horizontal consolidation in a two-stage process. First, analysts collect the price and market share data and estimate relevant demand parameters. Then, the data and the demand parameters are inserted into the ALM to generate the implied unit costs and the predicted price effects of the proposed consolidation.

The convenience of the ALM is appealing, and the approach may be very useful. As Werden and Froeb (1996, p. 65) observe, "even if considered unrealistically simplistic, merger simulations provide a little light in a very dark place." Nevertheless, a number of questions critical to the usefulness of this approach remain unanswered. First, it is not a forgone conclusion that ALM accurately predicts behavior in more general naturally-occurring circumstances, given the very specific assumptions about the demand system and the nature of strategic interactions. Second, even on the domain of the theory, i.e., even if sellers are Bertrand competitors and the underlying demand system is logit, the incentives that drive predicted unilateral effects are subtle and may have little explanatory power. Third, the model is used to distinguish between relatively subtle differences in markets. Some information about the potential accuracy of pre-merger demand parameter estimates would therefore be useful.

[12] Froeb and Werden have also developed simulation alternatives, including a Cournot simulation for homogeneous product competition, and an auction simulation. See Froeb (1994).

In a recent paper, we report an initial experiment designed to address some of these questions (Davis and Wilson, 2000c). This experiment design differs from the designs discussed above in that predictions involve results from oligopoly models with continuous units. Prior to reviewing the experiment, we outline some essential components of the underlying model.

4.1. Bertrand Competition with Differentiated Products

Suppose that each of i = 1, …, n – 1 sellers produce a single good with a constant marginal cost c_i, and with no fixed costs. When sellers are Bertrand competitors, each seller maximizes the function

$$p_i = (p_i - c_i)q_i(\mathbf{p}), \tag{1}$$

where p_i is the price of seller i, $\mathbf{p}$ is a vector of prices $\{p_1, p_2, \ldots\ p_n\}$, and q_i is the demand function for seller i. Taking first order conditions for seller i and solving generates the standard result

$$p_i - c_i = p_i / h_i, \tag{2}$$

where h_i is the own price elasticity.

Suppose that two firms j and k merge to form firm m. Assume that merger does not generate any efficiencies, and that the new consolidated firm selects prices on both products to maximize profits. That is,

$$p_m = (p_j - c_j)q_j(\mathbf{p}) + (p_k - c_k)q_k(\mathbf{p}) \tag{3}$$

Rearranging the first order conditions to solve for the price-cost difference yields

$$p_j - c_j = p_j / h_j - (p_k - c_k)\frac{h_{kj}}{h_j}\frac{q_k}{q_j}, \tag{4}$$

where h_{kj} is a cross price elasticity. Notice from the rightmost term in (4) that the difference between merged firms, and two firms acting separately is that the merged firm accounts for the effects of the profitability of related products when selecting an optimizing price. Specific predictions require specification of a demand system in order to generate quantities from prices, as well as own and cross price elasticities values in (2) and (4). We consider two demand systems, logit and linear.

The logit demand system, attributable to McFadden (1974), is motivated by a random utility model. Suppose that a large number of consumers each make a discrete choice among a set of n alternatives. For consumer j, the utility of a choice i = 1, …, n, may be written as

$u_{ij} = a_i - b\,p_i + v_{ij}$, where a_i is a quality parameter, b is a common slope parameter reflecting sensitivity of consumers to a price change, and v_{ij} is a consumer-specific preference for the product. If v_{ij} follows an extreme value distribution $f(v_{ij}) = e^{-e^{v_{ij}}}$, then the probability that consumers collectively select a particular seller takes on a simple logistic form $P_i = \dfrac{e^{a_i - bp_i}}{\sum_{k=1}^{n} e^{a_k - bp_k}}$,

where P_i is the probability that any consumer makes a purchase from firm i.

Let good n denote the choice of an outside good reflecting consumer preferences for all other goods and $\bar{p}$ denote the share-weighted average price for the inside goods, i = 1, …, n–1. Taking partial derivatives and weighting appropriately the own and cross-price elasticities of each firm's demand function yields

$$h_i = b\,p_i(1 - P_i) = [\,b\,\bar{p}(1 - s_i) + h\,s_i]p_i/(\bar{p}) \tag{5}$$

and

$$h_{ij} = b\,p_j P_j = (\,b\,\bar{p} - h)s_j\,p_j\,/(\bar{p}), \tag{6}$$

where h, the aggregate elasticity for all inside goods, is

$$h \equiv -\frac{\partial P_I(\,l\,\mathbf{p})}{\partial\,l}\frac{l}{P_I(\mathbf{p})} = b\,\bar{p}P_n\,, \tag{7}$$

and where, l is a scalar evaluated at l = 1, and $P_I = (1 - P_n)$.

Inserting (5) and (6) for own and cross price elasticities into (3) and (4) yields noncooperative equilibrium predictions both pre-merger and post-merger. Anderson, de Palma and Thisse (1982) (pp. 264-66) show that a unique equilibrium exists in these cases. However, since price appears on both sides of each equation, the solution must be calculated numerically. Notice also that prices, shares, b, and h, are the "primitives" of the model. This follows since h_j and h_{kj} are a function of h, b, shares and prices. Quantities, q_k and q_j, can be converted to shares in (4) by multiplying and dividing by aggregate output.

Werden and Froeb (1994) establish a variety of implications of this model. Among them are (a) all sellers raise prices post-merger, but the merging parties raise prices by a larger margin than the non-merging parties, (b) both merging firms lose market share to the non-merging firms, and (c) the price-increasing effects of merging firms are asymmetric. Post-merger, the smaller firm increases price more than the larger firm.

The role of costs in this analysis drives implication (c) above, and merits some emphasis. In the ALM costs are imputed, and adjust to make both sides of equilibrium condition in (5) balance. An implication of treating costs endogenously in this way is that when sellers are asymmetric, costs adjust to induce the asymmetry.[13] Thus, larger sellers always have lower costs relative to smaller sellers. When asymmetric sellers merge, the high-cost small seller raises prices by a larger margin than the large seller, and loses more market share.

Although a logit demand system offers analytic advantages, there is no good reason for believing that a logit demand system characterizes demand in many natural demand contexts.[14] Indeed Crooke, Froeb, Tschantz and Werden (1999) use simulation techniques to show that changing the demand specification can affect the predicted effects of a merger.[15] However, alterations in the demand system may also affect the stability of outcomes. For example, holding the observable primitives constant (price, shares, own price elasticities and cross price elasticities) the reaction functions of Bertrand games may vary importantly when the underlying demand system is, say linear, rather than logit. To investigate the behavioral importance of this latter issue, consider the simple linear demand system used recently by Huck, Normann and Oechssler (2000):

$$q_i = V_i - a\ p_i + q \sum_{j \neq i} p_j \ . \tag{8}$$

For this system, the own price elasticity is

$$h_i = a\ p_i / q_i \tag{9}$$

and the cross price elasticity is

$$h_{ij} = q\ p_j / q_i \ . \tag{10}$$

Inserting (9) and (10) into left hand portion of (3) and into (4) generates pre-and post-merger first order conditions for a linear system. Notice also that by manipulating V_i, a and q it

[13] This is an important auxiliary assumption of the model. Froeb, Tardiff and Werden (1998) show that if this assumption is true, the welfare effects of mergers among asymmetric firms are typically positive. However, it is not necessarily true that this condition holds in any particular natural circumstance.

[14] Indeed, logit demand systems have the disturbing feature of independence of irrelevant alternatives; i.e., the elimination of one product implies that demand for all remaining products increases in proportion to pre-existing market shares. For example, consider a market for transportation services in a metropolitan area, and suppose that the market consisted of public bus, public metro and private autos. Under a logit demand system, elimination of the public bus service would increase public metro and private auto use in proportion to their pre-existing shares of the market.

is not difficult to generate pre-merger prices, shares, and own and cross price elasticities that are identical to those in comparable logit quadropolies. (Post-merger predictions, however, will differ slightly.)

4.2 An Experimental Examination of the ALM.

A variety of behavioral questions arise when considering the behavioral relevance of the ALM. However, perhaps the most basic questions are the following. First, can the predictions of the ALM discriminate between problematic and non-problematic mergers in a simple streamlined "best shot" environment conducted strictly on the domain of the theory? Second, how sensitive is the ALM to an incorrect specification of demand? This latter question is crucially important, since uniformly imposing a logit demand system on all mergers probably implies that demand is, in many instances, misspecified.

The experiment consisted of a series of sixteen markets, conducted in a standard 2 × 2 design. Half the sessions were conducted in a "large effects" design, where post-merger prices are predicted to increase by an amount that antitrust authorities would easily consider troublesome, and a "small effects" design, where the predicted price increase would typically not be viewed as troublesome. As a reference, we use the 5% standard identified as a "small but significant" price increase identified in the Guidelines as critical for defining the relevant product and geographic markets. To cast some light on the effects of altering the underlying demand system, a logit demand system dictates purchasing decisions in half the markets. In the remaining markets a linear system, calibrated to generate pre-merger predictions identical to those in the comparable logit model, describes the buying decisions.

Markets were conducted under posted offer rules with simulated demand. Each period, after making price decisions, a monitor records and publicly announces prices. The monitor then determines individual sales, and privately communicates this information to each seller. The sellers, in turn, use this information to calculate earnings prior to making decisions for the following period. Periods repeat exactly as described, with one exception. After period 30 a surprise "merger" is announced, and one seller (the "acquired seller") is paid a $7 buyout fee, and leaves the experiment. A second seller (the "acquiring seller") is then invited to make (and

[15] More generally, Froeb and Tschnatz (2000) develop a theoretical methodology for examining the amount of demand information necessary to predict the effects of mergers. They conclude that first and second derivatives provide demand information that is necessary, and in most cases sufficient.

earn profits from) two pricing decisions for the remainder of the session. Unknown to participants, the acquiring seller was the subject with the highest earnings in periods 1-30, and the acquired seller was the subject with the lowest earnings. The market continued for an additional 30 periods. Neither the merger, nor the final period was announced in advance.

Our intention was to construct a "best shot" environment for the ALM. Thus, the environment included considerable symmetry. In all markets, sellers had identical costs and faced symmetric demand. In all markets the noncooperative equilibrium price is 55 cents, and each seller takes a 25% market share. Further, parameters are selected so that demand system variations do affect predictions importantly. For example, in the *logit large effects* design predicted post-merger prices increase by 8% overall and by 15.6% for the merging firms. In the *linear large effects* design, prices increase slightly more overall (9.3%), but slightly less for the merging firms (14.7%). Similarly in the *logit small effects* design, predicted prices increase 2.2% overall, and 4.5% for the merging parties. This compares with an overall increase of 1.8% and a 3.2% increase for the merging parties in the *linear small effects* design.

The upper and lower panels of Figure 10 illustrate mean prices for some representative sessions conducted in the *large effects* and *small effects* designs, respectively. In each panel, the joint profit maximizing prices, P_{JPM}, for the linear and logit designs are shown as dashed lines, respectively, while the competitive (marginal cost) line, c, is shown as a bolded solid horizontal line. The thin horizontal lines in the middle of each panel illustrate the symmetric pre-merger Nash prediction, P_a, and the post-merger share weighted average price (SWAP) prediction, P_m in the linear and logit designs. Finally, the line connected by solid dots illustrates the SWAP in a *linear* market, while the line connected by the hollow dots represents the SWAP for a *logit* market.

Inspection of the mean price paths in Figure 10 suggests several relevant general characteristics of market performance. Observe first that the price paths in each of the markets tends to follow a rather slow drift. Although the direction of the drift is not always monotonic, mean prices are generally closer to the noncooperative prediction than to rival JPM or competitive predictions. Second, markets nevertheless do not converge with any precision to the noncooperative prediction. For example, in the *small effects* design, only the *logit* market falls within a 5% band about the noncooperative prediction pre-merger, and only the *linear* market falls within a 5% band post-merger. However, the *small effects* markets comes closer to the

noncooperative predictions than the l*arge effects* markets. Both *large effects* markets miss both pre-merger and post-merger price predictions by a large margin. Third, the markets do not follow comparative static predictions with any precision. In instances such as the *linear large effects* market, prices increase more than predicted post-merger, while in other instances, such as the logit small effects market, prices fall. Nevertheless, the underlying demand system does appear to affect performance, with prices tending to increase more in the linear systems than in the logit ones.

The comparison of mean prices to underlying noncooperative predictions for all sessions, shown in Figure 11, confirms the generality of these observations. In each panel of Figure 11, a horizontal dash illustrates the SWAP for the final 5 periods of a sequence in a session (pre-merger or post merger), while a dotted line illustrates the Nash prediction. Vertical lines highlight a $\pm$5%range about the noncooperative prediction. Notice in the upper panel of the figure that pre-merger mean prices fall within the vertical $\pm$5% band in only five of the sixteen markets. Post-merger performance, shown the bottom panel, conforms only marginally better to equilibrium predictions, with mean prices falling within the $\pm$5% band only six times.

In natural contexts, of course, the underlying equilibrium is unknown. Rather, the analyst generates post-merger predictions by assuming that markets are in equilibrium pre-merger, and then uses current prices, quantities and estimated own and cross effects to make post-merger predictions. The standard procedure is to combine current observed and estimated parameters with a vector of implied costs that are manipulated to satisfy an equilibrium condition. Post-merger, own and cross elasticities and the implied cost vector are inserted into the post-merger equilibrium condition, and a price vector is manipulated until the equilibrium condition is satisfied.

Importantly, when markets are out of equilibrium the implied cost structure can deviate very substantially from actual underlying costs, as Figure 12 illustrates. The frequency polygons in the upper panel of the figure illustrate the distribution of implied costs for the *logit* (light) and *linear* (dark) *large effects* markets. Implied costs deviate wildly from the actual 20 cent unit costs, ranging from –9.71 cents to 71.28 cents in the *logit* markets, and –17.12 cents to 32.33 cents in the *linear* markets. Errors are somewhat smaller in *small effects* markets, yet they

remain substantial, ranging from 27.77 to 59.89 in the *logit* markets and from 10.02 to 41.77 in the *linear* markets. The actual costs were 36.2 cents per unit for all sellers.[16]

In light of the large errors in the implied cost structures, a poor correspondence between the predicted and observed effects of mergers is unsurprising. As the upper panel of Figure 13 illustrates, predicted effects (shown as the white bars) track subsequently observed price increases (grey bars) rather poorly. For example, the 12% predicted increase in session lin-h1 vastly understates an observed 107% price increase. Again, the 1% price increase predicted in session log-s4 baldy misses the observed 14% post-merger price drop. Nevertheless, the correlation between predicted and observed effects is reasonably high (r = .682). Moreover, the predictions based on pre-merger outcomes screen out the problematic mergers quite well. For example, the seven predictions of 5% or larger price increases capture five of the seven instances where prices increase 5% or more post-merger. In addition, these five captured markets are those with the largest price increases. A finer 2.5% screen yields more accurate results. The 9 instances where prices are predicted to increase by 2.5% or more (shown as the dashed line) capture all 7 of the cases where prices subsequently increased by at least 5%.

Furthermore, as shown in the lower panel of Figure 13, this screening ability appears to survive some misspecification of demand. In Figure 13 predictions for the *logit* markets are based on an assumed linear demand function, while predictions for the *linear* markets presume a logit demand system. Notice that the misspecification further degrades the predictive power of pre-merger predictions. For example, the correlation between predicted and observed price increases is only r = .279. Performance falls particularly in the *logit large effects* treatments. Nevertheless, the screening function of noncooperative predictions remains largely unaffected. Price increases of at least 5% are predicted in eight instances, and these instances capture five of the seven cases where prices increase by 5% (again including the five largest observed price increases). The eleven markets that pass a 2.5% screen capture all seven of the instances where prices increase by 5% or more.

Given the very poor organizing power of underlying noncooperative predictions, the potential screening capacity of the ALM is surprising. Why does it work? We suspect that part of

[16] Large and perhaps implausible of estimated costs from actual costs is not solely an artifact of our experiment. For example, in an illustrative example of the ALM, Froeb, Tardiff and Werden (1998) predict the effects of a series of mergers among Japanese long distance telephone companies. In their implied pre-merger equilibrium, costs for the small providers were 75% higher than for the dominant firm.

the explanation lies in the very strong correlation between predicted price increases and the extent to which pre-merger prices are below the non-cooperative equilibrium. (The correlation between pre-merger deviations from the Nash prediction and post-merger predicted prices $r = -.75$.) Low prices pre-merger reduce own price elasticities, thus increasing post-merger predictions. Conversely, pre-merger supra-competitive prices increase own price elasticities. Thus, the post-merger predictions capture a combination of potential for market power exercise and incentives to equilibrate post-merger.

This is far from useless information. Prices are unlikely to increase substantially post-merger when very small effects are predicted. Our results suggest that dropping these cases from further consideration is probably safe. Perhaps this screening function is precisely what the advocates of the model have in mind. However, the extreme variability of the post-merger outcomes suggests that this information is clearly one-sided. Post-merger, prices may either increase as predicted, increase more than predicted or they may even fall. Basing a case on large predicted effects [as the DOJ arguably did in *U.S. v. Interstate Bakeries* (Froeb, 1994)] would seem inappropriate.

Even these conclusions are preliminary. Prior to making any claims about the policy use of merger simulations, an examination of predictions in more robust environments is warranted. In particular, the effect of simple asymmetries on behavior merits investigation. Also, the assumption that sellers interact strictly as Bertrand competitors is a strong one. This warrants examining environments with Cournot interactions. These experiments are underway as we write this paper.

5. Summary and Conclusions

This paper argues that experimental methods are valuable to the development of antitrust policy. The three examples introduced above are far from exhaustive, and indeed considerable research on each of the topics remains to be done. We hope that these examples usefully illustrate both the kind of problem that can benefit from the application of experimental analysis, and the way that experimental tools can be useful in this context. In closing, we draw three general observations from the above examples. Our first observation pertains to the type of problems that can benefit from experimental analysis. The behavioral relevance of theories and techniques turns on the reasonableness of relevant auxiliary assumptions. Manifestations of such assumptions range from the presumption that markets are in equilibrium to the presumptions that

players make static pure-strategy Nash plays and that marginal costs are constant. These maintained assumptions are often not testable with naturally occurring data, but can be examined in the laboratory. Historically, armchair assessments of "realism" or "reasonableness" define the process of acceptance or rejection of a theory for policy application. A professional admiration for cleverness in technique and theoretical elegance undoubtedly also colors the acceptance process. The standards adopted by such a process are undoubtedly susceptible to changes in political preferences, desired outcomes, and even the changes in the vogue in economic modeling. Experimental tools can help create a firmer foundation for assessing the reasonableness of policy models.

Second, we offer a comment about appropriate experimental techniques. If the benefits of experimentation are in evaluating auxiliary assumptions, it follows, via not particularly circuitous logic, that the best experiments (and often the preferred analyses of experimental data) are those that invoke as few additional auxiliary assumptions as possible. Thus, rather than trying to create "lifelike" or "real" environments, we stress the desirableness of constructing environments that require as few assumptions as possible. Further, in analyzing experimental data, we stress the desirability of transparent methods. We appreciate that this runs against the current in academic economics, but we believe that it offers the most useful results for policy purposes.

Finally, we close with an observation about where experimental methods "fit" into the antitrust process. As we observed at the outset of this paper, situations rarely arise where an experiment provides evidence critical to the specifics of a particular legal case (Grether and Plott, 1984 is a notable exception). Rather, experiments are more useful for evaluating the policy standards themselves. For example, is using a merging simulation tool as a screening device reasonable? Do historic pass-through rates provide useful evidence about the future effects of merger synergies? Thus, although experimental results may find their way into antitrust litigation, for example, by private counsel, in questioning a particular standard, we would expect to see relatively few instances where either the government or outside counsel commissions experimental work to provide evidence that pertains to a particular matter. It is perhaps the infrequency with which experimental evidence can be usefully applied to particular cases that explains the relative paucity of antitrust policy experiments to date.

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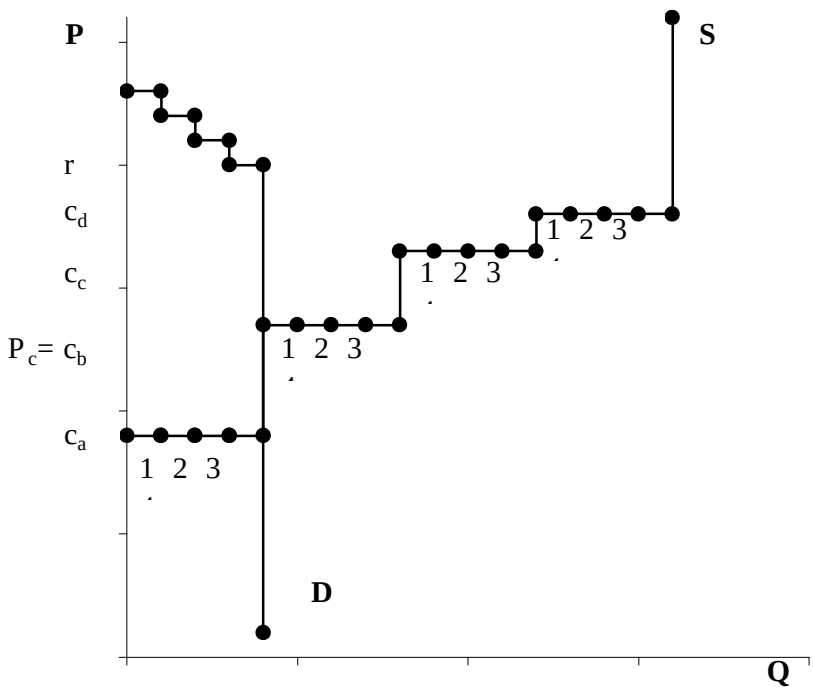

Figure 1. The *Set Cost* Design

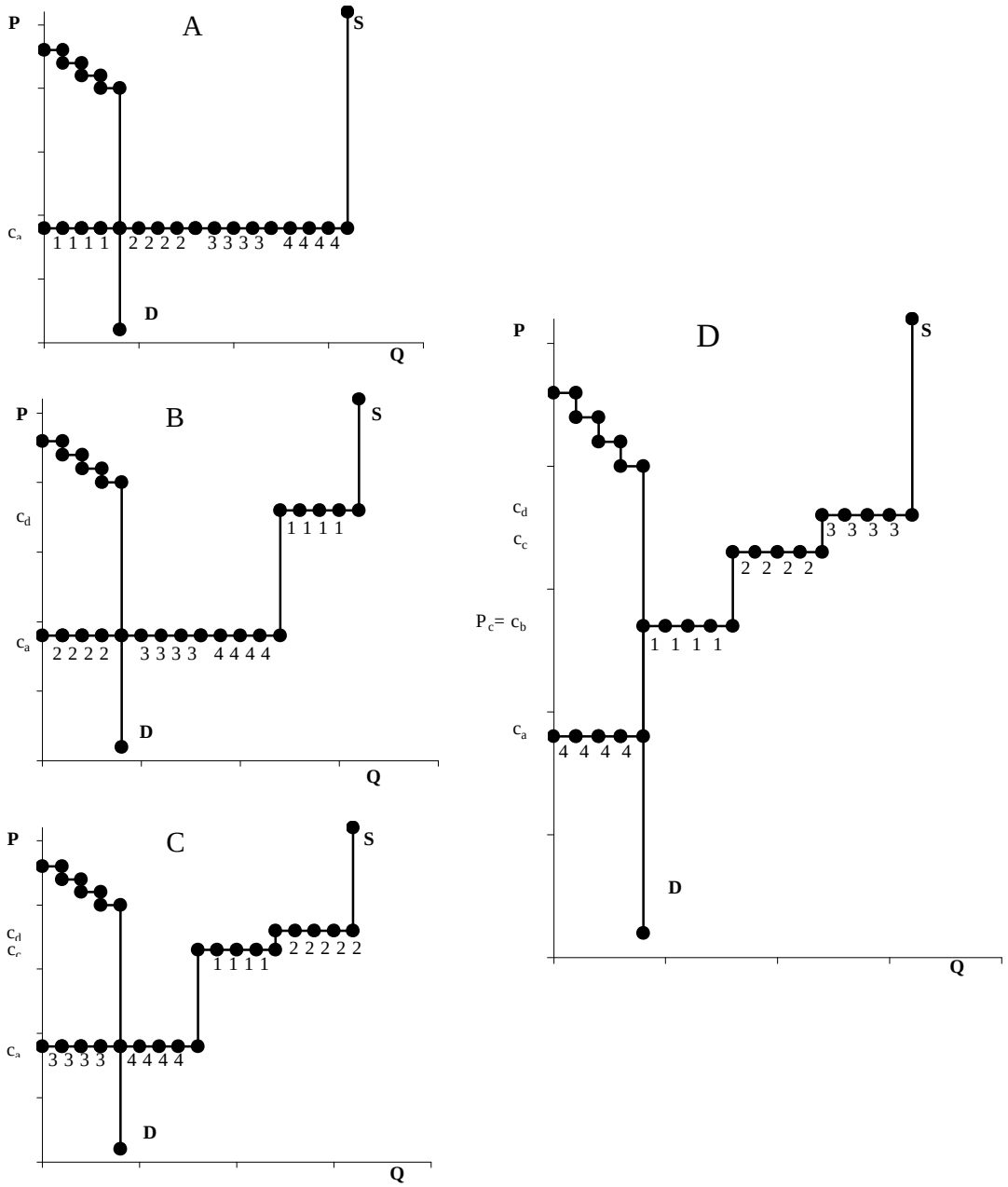

Figure 2. The *Endogeneous Cost* Design.

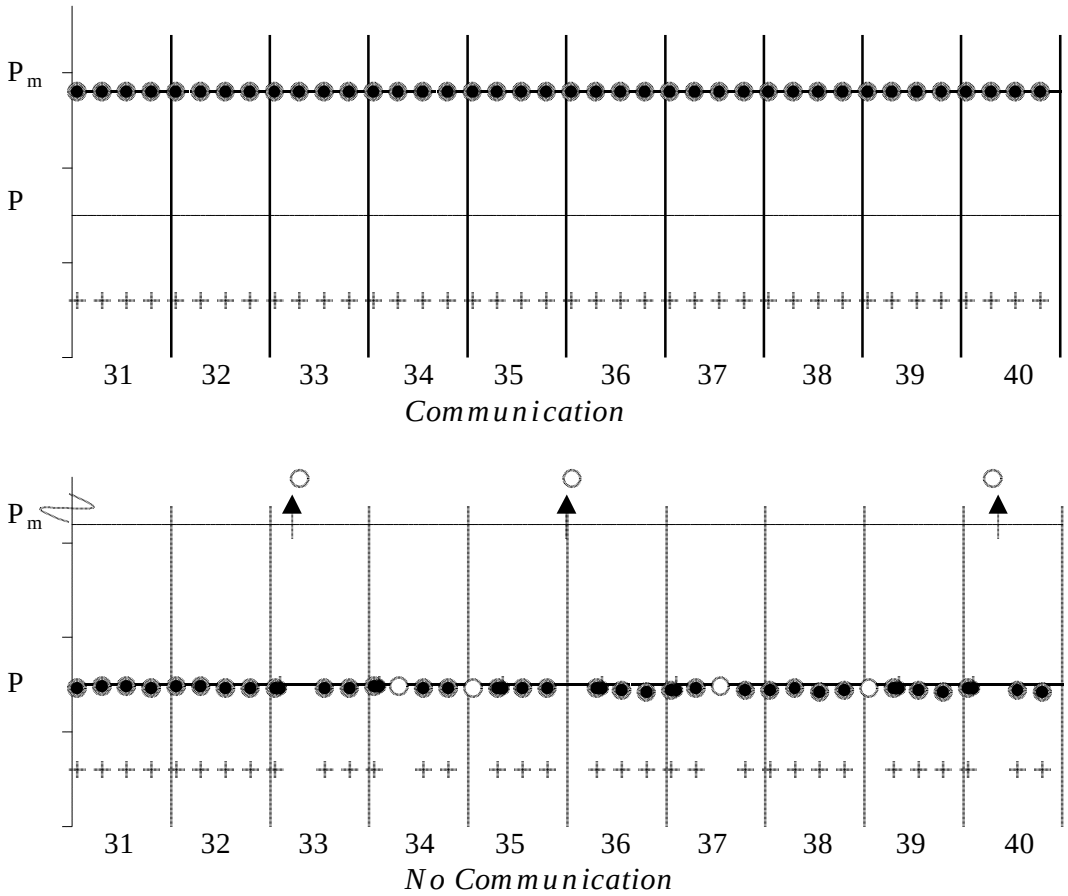

Figure 3. Contract Sequences for periods 31-40 of Sessions in the Set Cost Design. Key: Hollow circles represent offers, solid circles represent contracts and crosses represent unit costs.

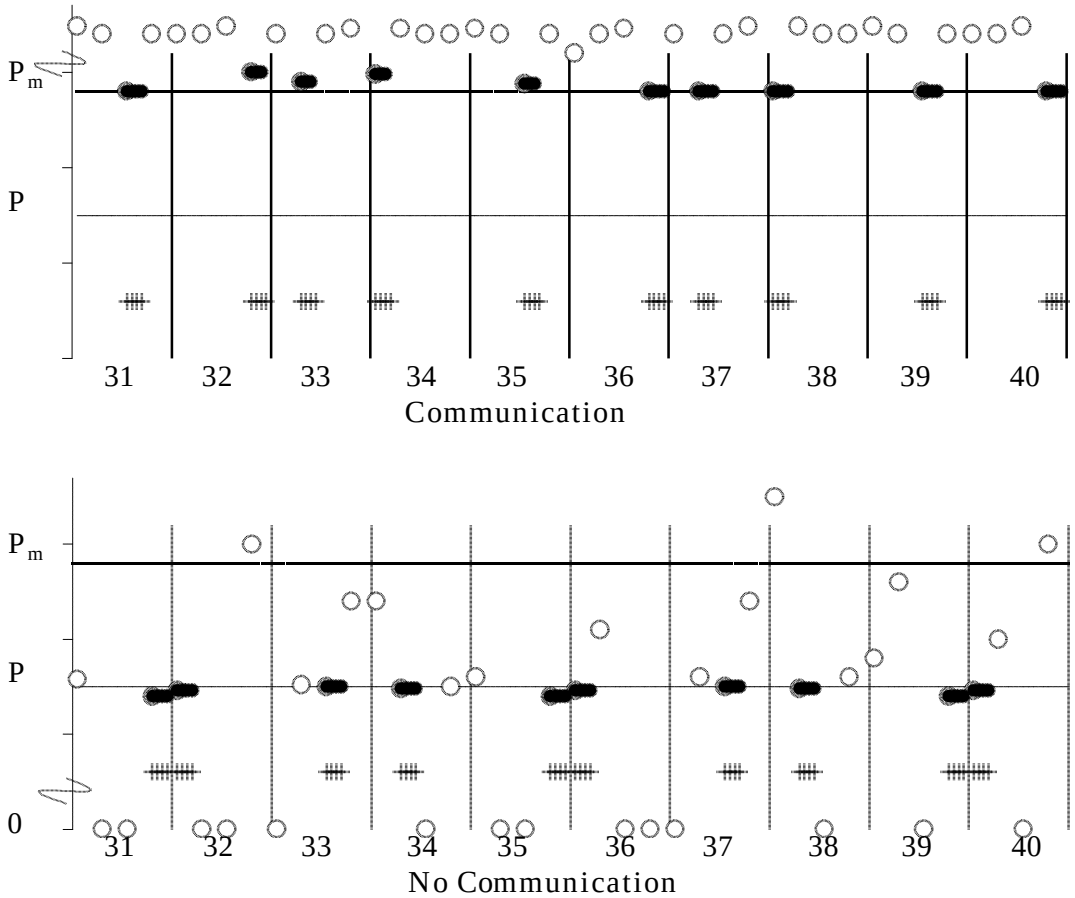

Figure 4. Contract Sequences for periods 31-40 of Sessions in the *Endogenous Cost* Design. Key: Hollow circles represent offers, solid circles represent contracts, crosses represent unit costs

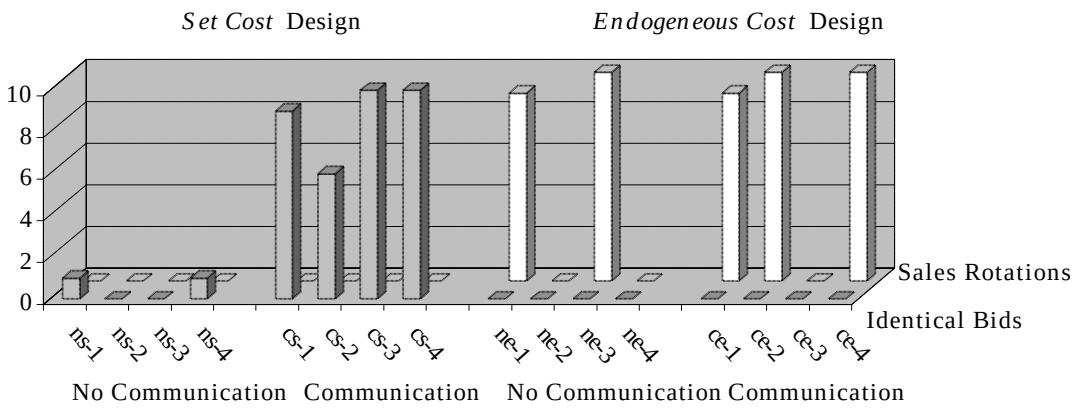

Figure 5. Incidences of "Suspect" Sales Rotations and Identical Bidding in Periods 31-40. Key: Gray bars illustrate the number of instances (out of ten possible) where sellers submitted identical bids in a period. White bars illustrate the number of instances where sellers rotated sales across periods.

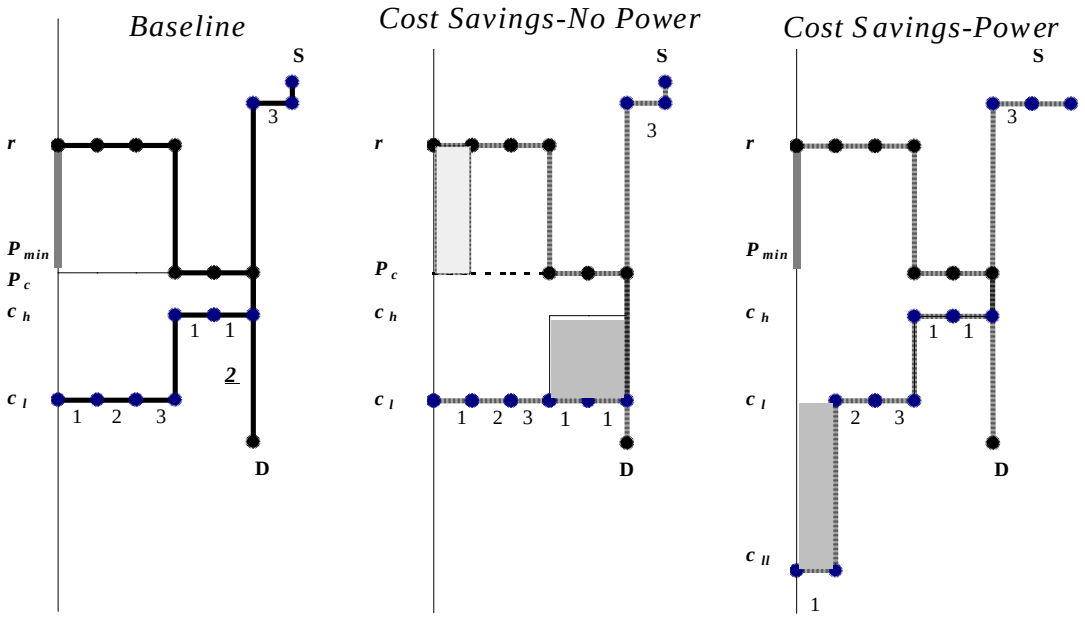

Figure 6. Supply and Demand Arrays for a Cost Savings and Market Power Experiment.

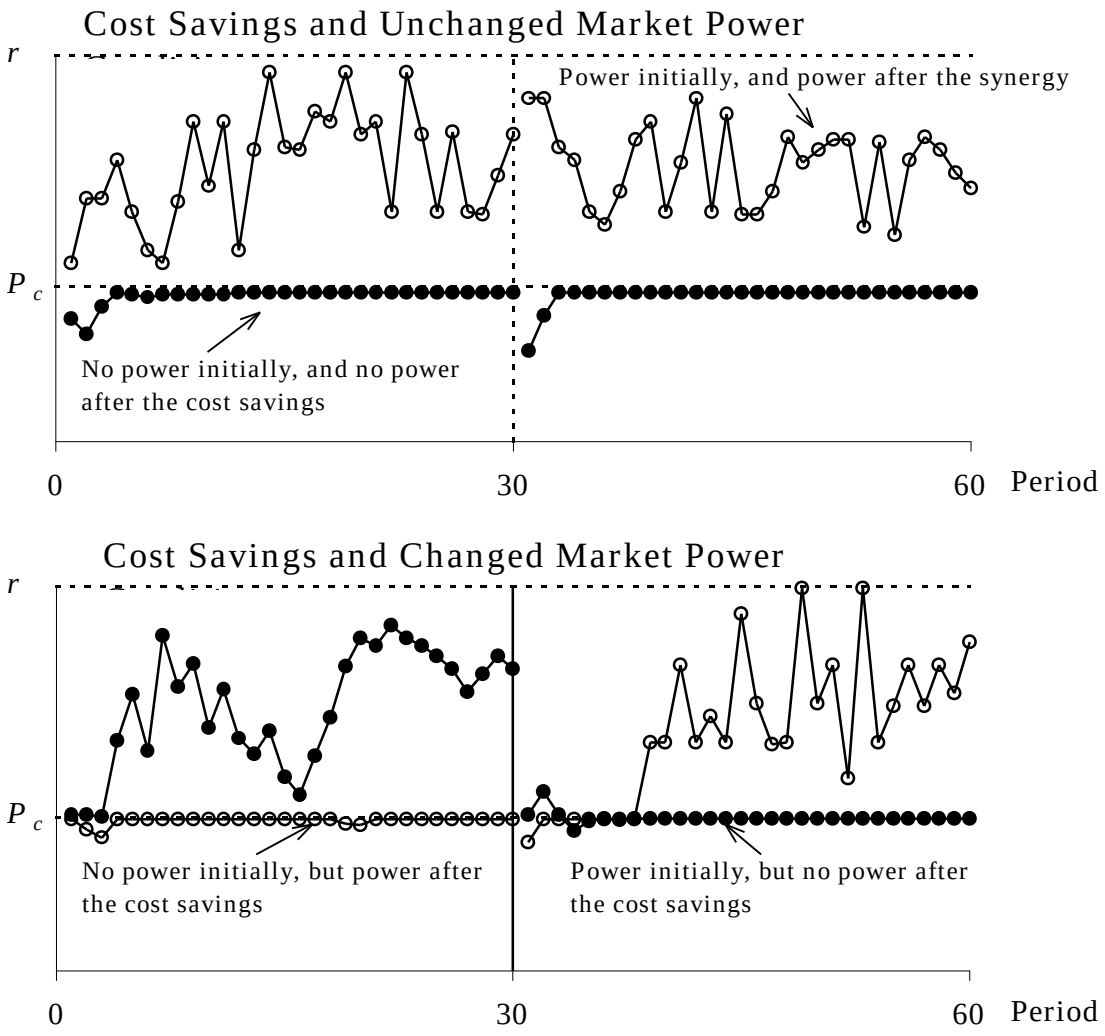

Figure 7. Mean Contract Price Path for Some Representative Sessions in a Cost Savings-Market Power Experiment. Key: In each panel, the solid and bolded dots illustrate mean contract prices for a single session.

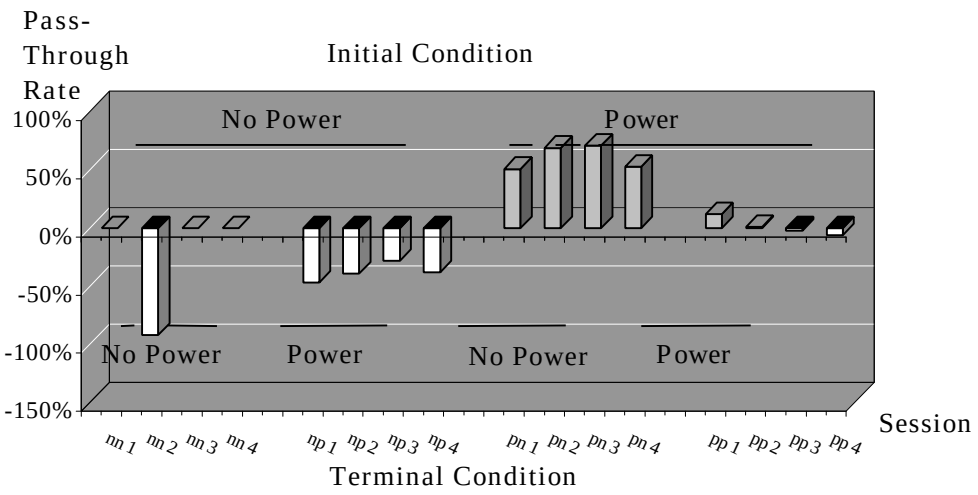

Figure 8. Pass-Through Rates.

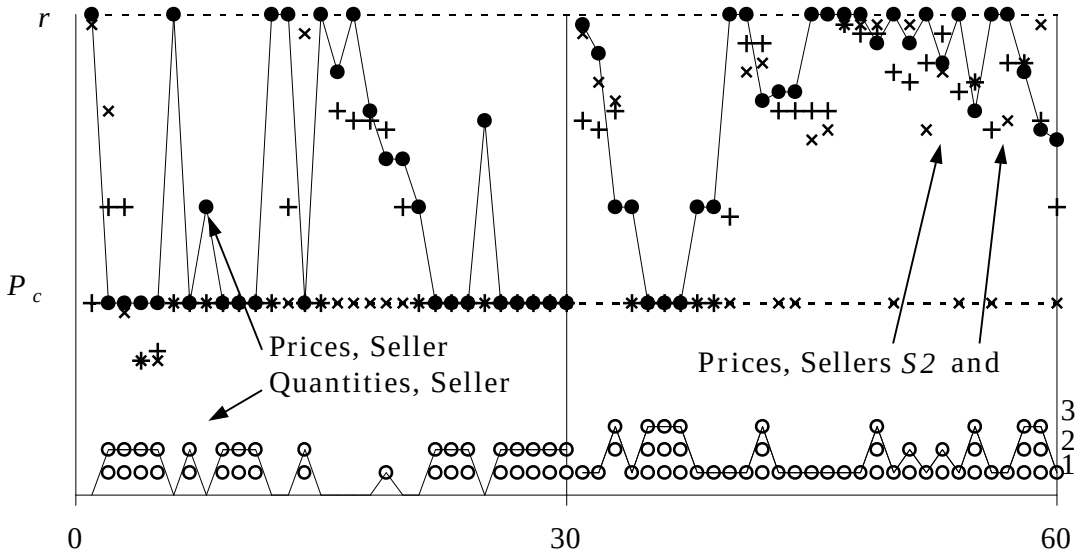

Figure 9. Sequence of Contract Postings for Session nn2. Key: '+', '*' and '●' symbols indicate price postings for sellers *S1*, *S2* and *S3*, resepectively. The vertically aligned '™' symbols at the bottom of the figure show the number of units sold per period by seller *S1*.

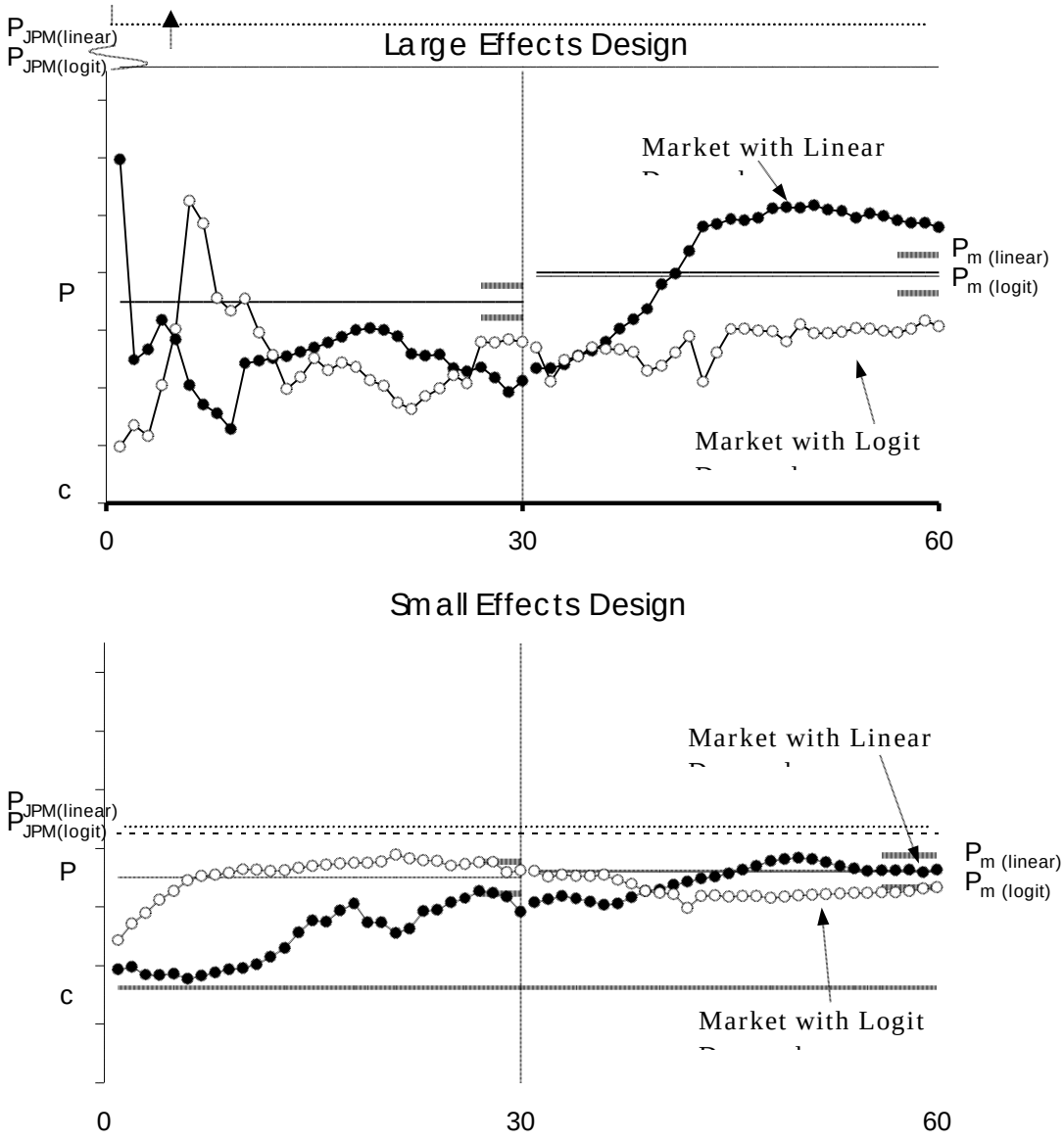

Figure 10. Mean Contract Price Path for Some Representative Differentiated Product Oligopoly Sessions. Key: In each panel, the solid and bolded dots illustrate mean contract prices for a single session.

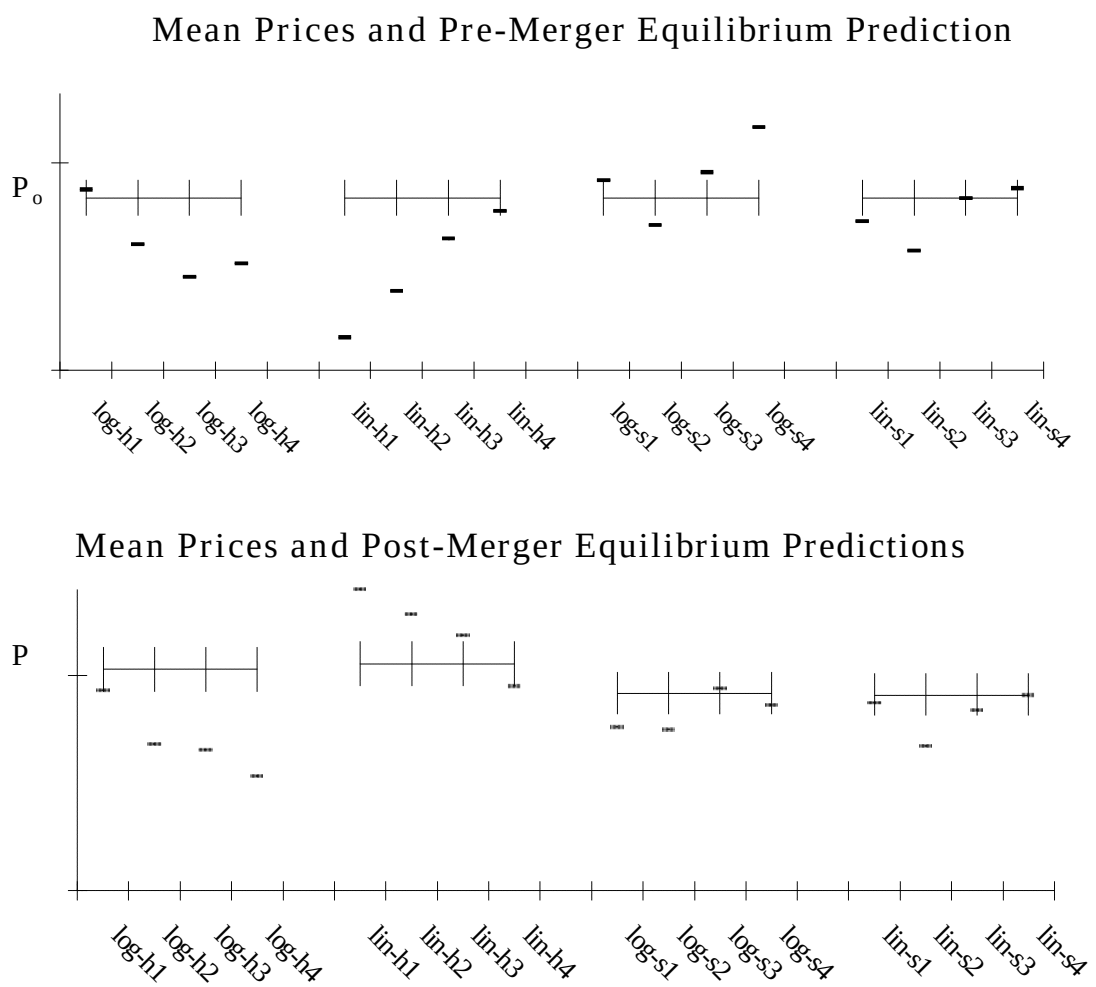

Figure 11. Predicted and Observed Share Weighted Average Prices. Key: In each panel, the dashes illustrate the SWAP for the final 5 periods of a sequence, pre merger (top panel) or post merger (bottom panel). The vertical lines mark the bounds of 5% deviations from the underlying equilibrium prediction (the dotted line).

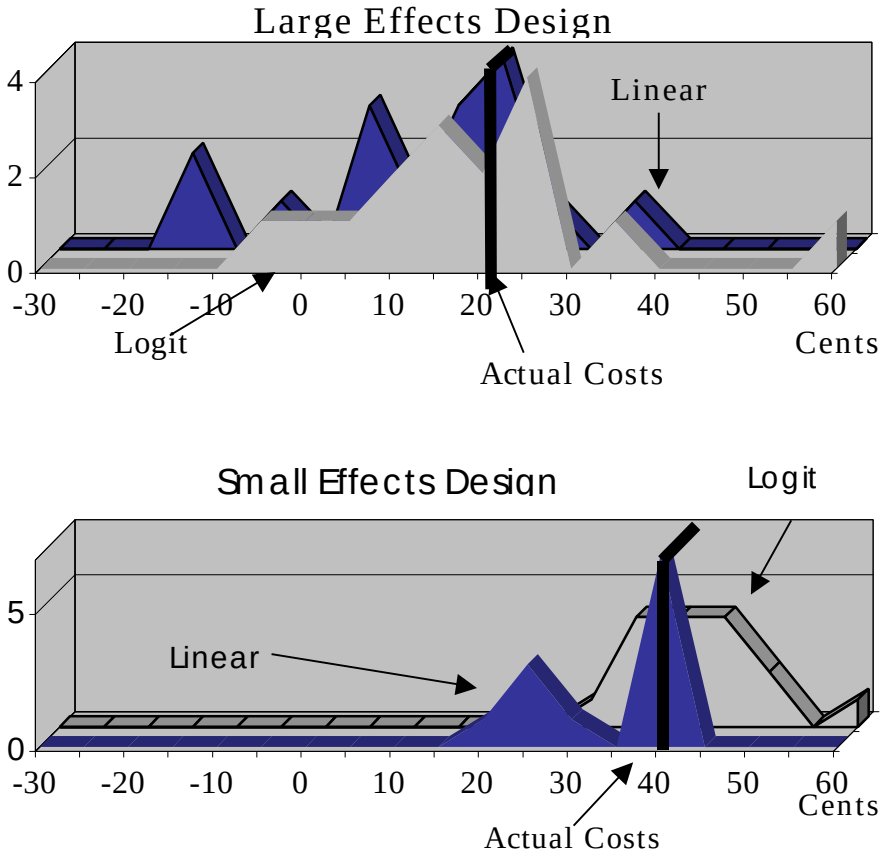

Figure 12. Implied Cost Distributions. Key: The Frequency Polygons in Each panel illustrate the distribution of implied costs for the Logit (Light) an Linear (Dark) sessions in a Design. Actual Costs are Marked with a Bolded Line.

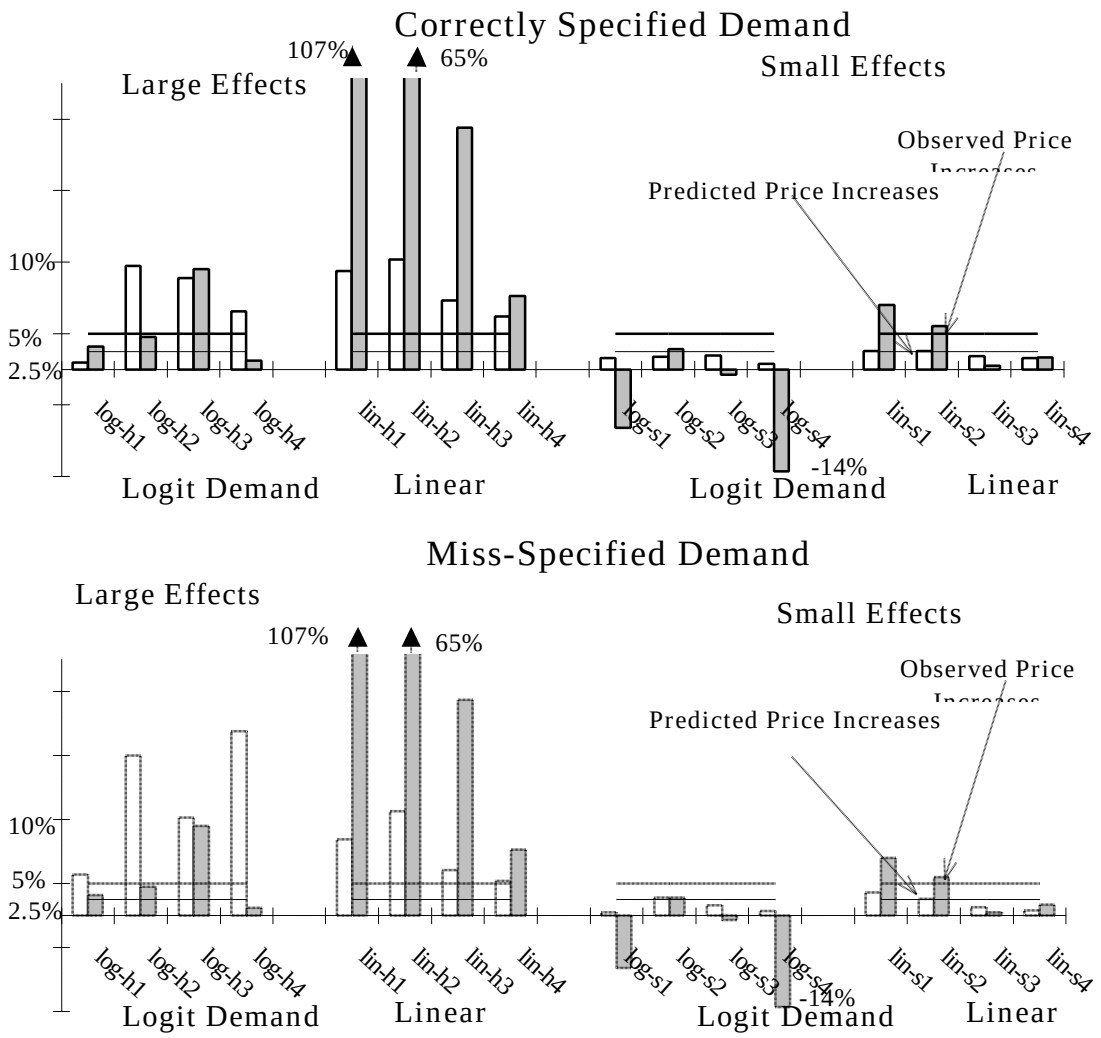

Figure 13. Post-Merger Price Effects, Predicted (White Bars) and Observed (Grey Bars)