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# Differentiated product competition and the Antitrust Logit Model: an experimental analysis

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**Abstract**

This paper reports a laboratory experiment designed to begin a behavioral examination of the Antitrust Logit Model (ALM), a merger simulation device that U.S. antitrust authorities use to help determine when anticompetitive problems may arise from horizontal mergers in differentiated-product markets. We find that the ALM *screens out* non-problematic mergers rather well, even though the ALM predicts performance in specific markets imprecisely. Further examination of the data suggests that in this context, adjustments to pre-merger deviations from the underlying Nash equilibrium, rather than the exercise of market power drive post-merger performance.


*JEL classification:* C9; L1; L4

*Keywords:* Differentiated products; Antitrust analysis; Experimental economics

## 1. Introduction

Antitrust merger enforcement policy has changed substantially in the last decade. In 1992, the Department of Justice (DOJ) and the Federal Trade Commission (FTC) revised their Horizontal Merger Guidelines (Guidelines) and added an enforcement focus on unilat-

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eral activity to existing considerations about the potential for cooperative behavior.[1] Most horizontal merger investigations now focus on potential unilateral effects and are evaluated in terms of specific models of oligopoly performance (Froeb, 1994). Horizontal mergers in differentiated product markets are an important example. DOJ analysts argue that in such contexts the standard concentration measure (the Herfindahl–Hirschman Index) provides little guidance as to when anticompetitive problems might arise (see, e.g., Werden and Froeb, 1996). In such cases, petitioners may reasonably argue either that the merging parties compete in 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 to help identify anticompetitive problems in differentiated-product markets.

The ALM assumes a logit demand system, and that sellers interact as Bertrand competitors.[2] The logit demand assumption is extremely useful in this context since investigators need only prices, market shares, a measure of the rate of substitutability between products, and a measure of demand elasticity to generate predictions. The convenience of the ALM is appealing, and the approach may be useful for antitrust work. 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 are unanswered. First, the model’s underlying assumptions may be frequently violated in natural markets. In particular, demand may be mis-specified as a logit system. Second, the incentives that drive unilateral effects are subtle and may fail to affect behavior as predicted. Third, demand parameter estimates from naturally occurring data may not be sufficiently precise to allow accurate predictions of post-merger behavior. The relevance of the ALM’s predictions in these more general circumstances bears scrutiny.[3]

This paper reports an experiment designed to shed light on these issues. The usefulness of experimental methods in this context bears emphasis. The laboratory provides a unique medium for evaluating the predictive power of the ALM under “best shot” circumstances, where the investigator constructs an environment that conforms strictly to the underlying assumptions of the model and where seller choices are not clouded by the rich variety of considerations extraneous to the equilibrium analysis that may affect decisions in natural circumstances. Although observing predicted behavior in the laboratory would say little about the relevance of the model in richer natural circumstances, a failure to observe predicted outcomes in the laboratory should raise serious questions about the potential value of the model as a predictor of behavior in the more complex natural world. Further, provided that the ALM works acceptably well strictly on its domain, the laboratory investigator can

[1] A primary result of several prominent theoretical analyses of horizontal mergers is that reducing the number of sellers alters the underlying strategic situation in a way that results in higher equilibrium prices via unilateral activity (e.g., Deneckere and Davidson, 1985; Farrell and Shapiro, 1990).

[2] Froeb offers simulation alternatives that have been developed for homogeneous Cournot competition and for one-sided auctions.

[3] Crooke et al. (1999) take a first step in exploring the effects of deviations from the ALM’s restrictive assumptions. Using Monte Carlo methods they report that the magnitude of comparative static effects on consolidations can be affected by the choice of the underlying demand system.

also examine the effects of specific deviations from maintained underlying assumptions on the predictive value of the model.

The related experimental research includes Davis and Holt (1994), Davis and Wilson (2000), and Wilson (1998), who find that sellers both recognize and exploit market power in a simple homogeneous-product price-setting environment with discrete units. In each of these experiments, market power is carefully distinguished from the number of sellers, and market power clearly affects prices more than market structure. On the other hand, Wellford (1990) finds that sellers encounter difficulties in exploiting the comparatively subtle strategic advantages conferred by mergers in more conventional oligopoly markets. In a series of homogeneous Cournot markets, she finds that prices never increase significantly post-merger, as predicted by a non-cooperative model.

The limited experimental research on differentiated product oligopolies focuses on the effects of information on performance. Dolbear et al. (1968) report that common knowledge about cost and demand structures tends to generate marginally lower prices in a differentiated product price-setting game. They also find that prices move inversely with the number of sellers. More recently, Huck et al. (2000) report that information about the actions of individual rivals tends to increase competitiveness in a series of 40-period differentiated product quadropolies. However, the effects of the added information were significant only in Cournot markets and not in comparable Bertrand markets. In their Bertrand markets, aggregate average results followed Nash predictions closely in either information condition.

In this paper we find, as with previous work, that Nash predictions organize outcomes better than the obvious rival joint-profit maximizing (JPM) and competitive predictions. However, as is also the case with much of the previous research, the correspondence between predicted and observed outcomes is uniformly weak, suggesting that the ALM holds little promise as a means of *predicting* the price effects of a merger. Surprisingly, the ALM performs reasonably well in this environment as a *screening device*, in the sense that most of the large post-merger price effects occurred in markets where a large price increase was predicted. However, closer examination of experimental results suggests that deviations from the underlying equilibrium rather than market power exercise drive the ALM's predictive power.

The remainder of the paper is organized as follows. Section 2 sketches out pre-merger and post-merger predictions for a non-cooperative Bertrand oligopoly with differentiated products. We develop predictions for logit and linear demand specifications. Section 3 describes experimental design, parameters, and procedures, while Section 4 presents the results. Section 5 concludes.

## 2. Non-cooperative predictions in Bertrand competition

Consider a market with $n$ price-setting sellers, each of whom produces without fixed costs and with a constant marginal cost, $c_i$. Defining $q_i(\boldsymbol{p})$ as seller $i$'s demand function given the vector $\boldsymbol{p}$ of own and other price choices, each seller $i$ optimizes

$$\pi_i = (p_i - c_i)q_i(\boldsymbol{p}).$$

Taking first order conditions and rearranging terms generates the standard result

$$p_i - c_i = \frac{p_i}{\eta_i}, \tag{1}$$

where own price elasticity $\eta_i$ is defined as a positive number.

Suppose, now that two firms $j$ and $k$ merge to form firm $m$. The objective function for the consolidated parties becomes

$$\pi_m = (p_j - c_j)q_j(\boldsymbol{p}) + (p_k - c_k)q_k(\boldsymbol{p}).$$

Rearranging the first order conditions to solve for the price–cost difference yields for firm $j$,

$$p_j - c_j = \frac{p_j}{\eta_j} + (p_k - c_k)\frac{\eta_{kj}}{\eta_j}\frac{q_k}{q_j}, \tag{2}$$

and similarly for firm $k$. The rightmost term in (2) indicates that the difference between the pricing decisions of a merged and unmerged firm is that the merged parties account for the effects of their own prices on the profitability of their related products.

We evaluate pre-merger and post-merger predictions in light of standard reference outcomes. The competitive outcome, where price equals marginal cost, represents one natural alternative since this outcome represents a limit to non-strategic, non-cooperative behavior. The joint-profit maximizing condition, a limit to gains from cooperation, is another. Acting in concert, firms optimize

$$\pi_{\mathrm{JPM}} = \sum_j (p_j - c_j)q_j(\boldsymbol{p}).$$

Rearranging first order conditions yields the optimal condition for any firm $j$,

$$p_j - c_j = \frac{p_j}{\eta_j} + \sum_{k \neq j}(p_k - c_k)\frac{\eta_{kj}}{\eta_j}\frac{q_k}{q_j}. \tag{3}$$

Looking at the right side of (3) we note that in the JPM condition, each firm accounts for interaction effects on all firms when making an optimal pricing decision. More specific predictions require specification of a demand system. Below we consider two demand systems, logit and linear.

### 2.1. Logit demand

The logit demand system, developed by McFadden (1974), is based on a random utility model of consumer choice. Here, we develop a very simple version, where consumers substitute equally among all "inside" products.[4] Consider a market consisting of $n-1$ inside goods and an "outside" good $n$ that represents the all other goods. For consumer $l$,

[4] Werden and Froeb (1994) develop more complicated variants that include "nests" of closely related inside products.

the utility of a choice $i = 1, \ldots, n$, may be written as $u_{il} = \alpha_i - \beta p_i + v_{il}$, where $\alpha_i$ is a quality parameter, $\beta$ is a common slope parameter reflecting sensitivity of consumers to a price change, and $v_{il}$ is a consumer-specific preference for the product.

If $v_{il}$ follows an extreme value distribution $f(v_{il}) = \mathrm{e}^{-\mathrm{e}^{v_{il}}}$, the probability $P_i$ that consumers collectively select a particular seller $i$ becomes

$$P_i = \frac{\mathrm{e}^{\alpha_i - \beta p_i}}{\sum_{m=1}^{n} \mathrm{e}^{\alpha_m - \beta p_m}}.$$

The logit demand system produces the following own and cross-price elasticities,

$$\eta_j = \beta p_j (1 - P_j) = \frac{[\beta \bar{p}(1 - s_j) + \eta s_j] p_j}{(\bar{p})} \tag{4}$$

and

$$\eta_{kj} = \beta p_j P_j = \frac{(\beta \bar{p} - \eta) s_j p_j}{(\bar{p})}, \tag{5}$$

where $s_i$ is firm $i$'s market share conditional on the choice of an inside good (e.g., $s_i = P_i/[1 - P_n]$) and $\bar{p}$ is the share-weighted market price. The implied (positive) aggregate elasticity for all inside goods is

$$\eta \equiv -\frac{\partial P_{\mathrm{I}}(\lambda \boldsymbol{p})}{\partial \lambda} \frac{\lambda}{P_{\mathrm{I}}(\boldsymbol{p})} = \beta \bar{p} P_n, \tag{6}$$

where $\lambda$ is a scalar evaluated at $\lambda = 1$, and $P_{\mathrm{I}} = (1 - P_n)$.

Only relative differences in $\alpha_i$'s matter, so estimates are generated by arbitrarily selecting an $\alpha_n$ for the outside good. Relative $\alpha_i$'s are derived from logs of the ratio of $P_i$ to $P_n$:

$$\ln\left(\frac{P_i}{P_n}\right) = \alpha_i - \alpha_n - \beta p_i. \tag{7}$$

The pre-merger prediction is found by inserting the expression for $\eta_j$ in Eq. (4) into Eq. (1) to produce

$$p_i - c_i = \frac{\bar{p}}{\beta \bar{p}(1 - s_i) + \eta s_i}. \tag{8}$$

Similarly, inserting the expressions for $\eta_j$ and $\eta_{jk}$ in Eqs. (4) and (5) into Eq. (2) generates the post-merger prediction. Solving this yields

$$p_j - c_j = p_k - c_k = \frac{\bar{p}}{\beta \bar{p}(1 - s_m) + \eta s_m}, \tag{9}$$

where $s_m = s_j + s_k$. Anderson et al. (1992) establish than a unique equilibrium exists for the above system. However, since price appears on both sides of Eqs. (8) and (9), the solution must be computed numerically.

In general, the ALM simulation process proceeds as follows. First, assuming that a market is in equilibrium, $\boldsymbol{s}$ (vector of market shares), $\boldsymbol{p}$, $\beta$, and $\eta$ are inserted into both sides of Eq. (8). Adjusting an implied cost vector $\boldsymbol{c}$ to balance both sides of Eq. (8) generates the pre-merger equilibrium condition. The post-merger prediction is found by inserting $\boldsymbol{s}$, $\beta$, and $\eta$ and the implied costs into Eq. (9) and adjusting the post-merger price vector until both sides of Eq. (9) balance for each firm.

It is worth noting that both the pre-merger equilibrium condition in Eq. (8) and the post-merger equilibrium in Eq. (9) are developed entirely in terms of $\boldsymbol{p}$, $\boldsymbol{s}$, and single measures of $\beta$ and $\eta$. Antitrust authorities typically either have these data or can reasonably estimate them in the very tight timeframes of the merger review process.[5]

Finally, we find the JPM condition for all inside firms by inserting Eqs. (4) and (5) into Eq. (3) and simplifying for each firm $i$:

$$p_i - c_i = \frac{1}{(1 - \sum_k P_k)\beta} = \frac{\bar{p}}{\eta}. \tag{10}$$

### 2.2. Linear demand

Demand, of course, need not be logit. Indeed, logit demand systems incorporate the potentially troubling independence of irrelevant alternatives assumption, which may be violated frequently in practice.[6] Deneckere and Davidson illustrate with a linear demand system some general results for mergers with differentiated products and price competition that parallel those generated above for logit demand.[7] A full development of their results is not warranted here. However, to obtain a flavor of their results, consider the standard demand system used by Huck et al.[8]

$$q_j = V_j - \alpha p_j + \theta \sum_{i \neq j} p_i, \tag{11}$$

where $\alpha > \theta > 0$. In this case, the (positive) own price elasticity is

$$\eta_j = \frac{\alpha p_j}{q_j}, \tag{12}$$

and the cross-price elasticity is

$$\eta_{kj} = \frac{\theta p_j}{q_k}. \tag{13}$$

[5] Under the Hart–Scott–Rodino Merger Improvements Act of (1974), federal antitrust authorities have at most 30 days to challenge a merger. Separating combined assets post-merger is very difficult.

[6] Consider, for example, an automobile market consisting of sports cars, mini-vans, and full-size sedans. Under logit demand, the elimination of, say, mini-vans would increase the share of each other type of automobile in proportion to its initial sales share.

[7] Deneckere and Davidson (1985) generate general results for a family of symmetric demand systems where products are gross substitutes and where demand is downward sloping.

[8] Deneckere and Davidson use a somewhat simpler linear system by Shubik (1980) to illustrate their results. The linear system used here includes independent own and cross-price effects, which adds the flexibility needed to calibrate pre-merger conditions across linear and logit demand systems. More generally, the pre-merger and post-merger equilibrium conditions developed below are solved as a system of linear equations.

Inserting Eq. (12) into Eq. (1) generates the pre-merger equilibrium condition

$$p_j - c_j = \frac{q_j}{\alpha}. \tag{14}$$

When firms *j* and *k* consolidate, substituting Eqs. (12) and (13) into Eq. (2) yields

$$p_j - c_j = \frac{q_j}{\alpha} + (p_k - c_k)\frac{\theta}{\alpha},$$

which implies that the post-merger mark-up of prices over costs is inversely related to own price sensitivity ($\alpha$) and directly related to the consumers' preferences for substituting between products ($\theta$). The relative effects of a merger on prices are more easily seen in the special case where the two merging firms have identical costs:

$$p_j - c_j = p_k - c_k = \frac{q_m}{\alpha - \theta}, \tag{15}$$

where $q_m = q_j + q_k$. A comparison of Eqs. (14) and (15) reveals that the price–cost mark-up for the consolidated parties exceeds that for independent firms, provided that $q_m/q_j > \alpha/(\alpha - \theta)$, a condition that Deneckere and Davidson show is certain to hold for a downward sloping symmetric linear demand system.

To find the JPM predictions, Eqs. (12) and (13) are inserted into Eq. (3) to yield

$$p_j - c_j = \frac{q_j}{\alpha} + \sum_{k \neq j}(p_k - c_k)\frac{\theta}{\alpha}. \tag{16}$$

In the special case where costs are symmetric, Eq. (16) simplifies to

$$p_{\mathrm{JPM}} - c_{\mathrm{JPM}} = \frac{q_{\mathrm{JPM}}}{\alpha - (N-1)\theta}. \tag{17}$$

Eq. (17) exceeds Eq. (15) as long as $q_m/q_{\mathrm{JPM}} < (\alpha - (N-1))/(\alpha - \theta)$, which again is a condition that Deneckere and Davidson show is certain to hold with down-sloping, symmetric linear demand.

Unlike the logit case, the linear differentiated product oligopoly may be solved explicitly. However, even for the simple demand system used here, the explicit solutions yield complicated, rather unenlightening expressions. More important for the present purpose of developing an experimental design, we note that given equilibrium parameters for the logit model, an identical equilibrium condition can be generated in the linear model. In the logit model, one imputes $\boldsymbol{c}$ as an equilibrium condition for given parameters $\boldsymbol{p}$, $\boldsymbol{s}$, $\beta$, and $\eta$. Presumably, the number of (linear demand) sellers $N$ and total market sales $Q = \sum_{i=1}^{N} q_i$ are also known. These variables determine unique own and cross-price elasticities, which by Eqs. (12) and (13) determine $\alpha$ and $\theta$ in the linear demand system. Inserting $\alpha$, $\theta$, and $p_j$ into the equilibrium condition for the linear system represented in Eq. (11) yields a unique $V_j$.

Unlike pre-merger conditions, post-merger equilibrium conditions vary across demand systems (see Crooke et al., 1999). In this experiment, we focus on the effects of the demand system on the behavioral stability of markets, so our parameter choices minimize predicted differences across demand systems.

## 3. Experimental design and procedures

A primary goal of this experiment is to give the ALM a "best shot" of generating predicted behavior. Toward this end, we include as much symmetry in the design as possible. Sellers have symmetric costs, and products differ so that all sellers have symmetric prices, shares, and profits in the pre-merger equilibrium. The market structure consists of four sellers pre-merger, which decays to three sellers post-merger. This choice of market structure reflects a balance of design needs. On the one hand, the magnitude of predicted price effects moves inversely with the number of sellers, suggesting that we minimize the number of sellers. On the other hand, we need to leave enough sellers post-merger to maintain a reasonable expectation of non-cooperative behavior, because the ALM predicts price adjustments on the basis of changes in non-cooperative circumstances.[9]

### *3.1. Design*

The experiment consists of a series of 24 differentiated product quadropolies. Each market consists of 60 trading periods. For the first 30 periods, each of four symmetric sellers makes a series of simultaneous price choices. After period 30, a merger occurs, and one seller is given control over the production decisions of another seller. The market then continues with three sellers for an additional 30 periods. Comparing post-merger behavior with pre-merger predictions allows an assessment of the model's predictive power.

The experiment includes two design conditions. First, to generate some insight into the ALM's capacity to differentiate between problematic and non-problematic mergers, we induce a "*Large Effects*" design, where predicted prices increase post-merger 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 problematic. As a reference, we use the 5% price increase identified as critical to defining the scope of markets in the Guidelines.[10] Second, to allow some insight into the consequences of changing the underlying demand system (albeit fairly subtlely), we examine both linear and logit demand systems in each design.

The four columns in Table 1 summarize the experimental parameters for each of the design conditions. As is clear from the bolded entries listed in the row labeled "Nash: pre-merger" predictions, all markets are calibrated to generate identical pre-merger equilibrium prices, $p_i = 55$, and market shares, $s_i = 25\%$, for all sellers. In the *Large Effects* design, logit parameters ($\eta = .22449$ and $\beta = .036734$) and linear parameters ($V_i = 100$, $\alpha = 7/3$ and $\theta = 2/3$) are chosen so that post-merger price increases easily exceed 5%. The overall share-weighted average price (SWAP) increases 8.0% and 9.3% in the *Logit* and *Linear* specifications, respectively. In the *Small Effects* design, logit parameters ($\eta = 1.575$ and $\beta = .061375$)

[9] A considerable body of experimental evidence suggests that tacit collusion often affects decisions in duopoly markets. See, for example, Chapter 4 in Davis and Holt. Tacit collusion is rarely a problem in markets with three or more sellers.

[10] The 5% criterion used here is only a rule of thumb. The Guidelines do not explicitly identify a post-merger price increase that would be regarded as troublesome. Nevertheless, the 5% criterion used in the Guidelines as a general standard for market definition (Section 1.1.1) reveals that the antitrust authorities are concerned about relatively small price adjustments.

Table 1
Parameters and predictions

| Design | Logit demand | | | | | | Linear demand | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | *Large Effects* | | | *Small Effects* | | | *Large Effects* | | | *Small Effects* | | |
| Parameters | $\eta = .22449$ | | | $\eta = 1.575$ | | | $V = 100$ | | | $V = 209.8$ | | |
| | $\beta = .036734$ | | | $\beta = .061375$ | | | $\alpha = 7/3$ | | | $\alpha = 13/3$ | | |
| | $s_i = 25\%$ | | | $s_i = 25\%$ | | | $\theta = 2/3$ | | | $\theta = 2/3$ | | |
| | $p_i = 55$ | | | $p_i = 55$ | | | $c_i = 20$ | | | $c_i = 36.2$ | | |
| Equilibrium conditions | $c_i = 20$ | | | $c_i = 36.2$ | | | | | | | | |
| | $\eta_i = 1.57$ | | | $\eta_i = 2.92$ | | | $\eta_i = 1.57$ | | | $\eta_i = 2.92$ | | |
| | $\eta_{ij} = .45$ | | | $\eta_{ij} = .45$ | | | $\eta_{ij} = .45$ | | | $\eta_{ij} = .45$ | | |
| | $(Q_{\mathrm{I}} = 367.5)$ | | | $(Q_{\mathrm{I}} = 611)$ | | | | | | | | |
| Predictions | $p_i$ | $s_i$ | Profit | $p_i$ | $s_i$ | Profit | $p_i$ | $s_i$ | Profit | $p_i$ | $s_i$ | Profit |
| Competitive | 20.0 | – | 0 | 36.2 | – | 0 | 20.0 | – | 0 | 36.2 | – | 0 |
| Nash: pre-merger | **55.0** | **25.0** | 2858 | **55.0** | **25.0** | 1532 | **55.0** | **25.0** | 2858 | **55.0** | **25.0** | 1532 |
| Nash: post-merger | | | | | | | | | | | | |
| Merged firms | 63.6 | 21.6 | 3016 | 57.6 | 23.1 | 1550 | 63.1 | 22.5 | 3093 | 56.8 | 23.8 | 1550 |
| Other firms | 56.2 | 28.4 | 3288 | 55.1 | 26.9 | 1595 | 56.7 | 27.5 | 3315 | 55.3 | 26.2 | 1580 |
| $\%\Delta\mathrm{SWAP}_{\mathrm{all}}$ | **8.0** | | | **2.3** | | | **9.3** | | | **1.8** | | |
| Joint-profit maximizing | 95.8 | 25.0 | 4467 | 63.5 | 25 | 1686 | 160 | 25.0 | 6533 | 63.1 | 25.0 | 1683 |

and linear parameters ($V_i = 209.8$, $\alpha = 13/3$ and $\theta = 2/3$) are chosen so that the post-merger price increases less than 5%. The SWAP increases 2.3% and 1.8% in the *Logit* and *Linear* specifications, respectively.

Despite the similarity of pre-merger and post-merger predictions across the linear and logit demand systems, shifting demand systems may have behavioral consequences. Most particularly, cooperative predictions differ substantially across demand systems, particularly in the *Large Effects* treatments. As seen in the JPM predictions listed at the bottom of Table 1, the share-weighted joint-profit maximizing price, $P_{\mathrm{JPM}}$, increases from 95.8 in the *Logit Large Effects* treatment to 160 in the *Linear Large Effects* treatment. Thus, the latitude for gains from cooperative behavior is much higher in the linear model than in the logit model. In the *Small Effects* treatments, the change in the JPM price across demand systems is much smaller, increasing only from 62.5 in the *Logit Small Effects* treatment to 63.1 in the *Linear Small Effects* treatment.[11]

## 3.2. Procedures

Each session consists of 60 posted-offer trading periods. At the outset of each session, participants sit in visually isolated booths with personal computers, and a monitor reads

[11] Changing the demand system also affects the magnitude of sellers' incentives to respond to the action's rivals. Sellers are somewhat less sensitive to price choices of rivals in a logit demand system.

instructions aloud as participants follow along on a copy of their own. In addition to explaining trading rules and record-keeping procedures, the instructions indicate that (a) sellers all have the same unit costs, (b) these costs remain unchanged throughout the session, and (c) sellers are differentiated in the eyes of the fully revealing buyer, but that buyer preferences vis-à-vis each seller are symmetric.[12] The very complete cost and demand information provided in the instructions is intended to facilitate non-cooperative outcomes, as suggested by Huck et al. (2000). Finally, the instructions indicated that lab dollars were converted into cash at the rate of 10,000 lab dollars for US$ 1.[13]

A period proceeds as follows. First, sellers simultaneously choose prices that a monitor records. The monitor then publicly announces the price decisions as he enters the information into a spreadsheet that determines individual sales quantities. The monitor then privately discloses sales quantities to participants. Sellers record all price decisions and individual sales quantities in their spreadsheets. Spreadsheet formulas calculate individual and cumulative earnings prior to eliciting price decisions for each subsequent period. In addition to individual earnings information, sellers may access the pricing history in both tabular and graphical form.[14]

Each period repeats the above process exactly, with one exception. After period 30 a merger occurs, and one seller (the “acquiring seller”) assumes operation of a second seller’s computer (the “acquired seller”) for the remainder of the session. For 16 sessions (eight *Small Effects* sessions and eight *Large Effects*), the acquiring seller had the highest earnings for periods 1–30, and the acquired seller had lowest earnings for those periods. This selection procedure was not explained to participants. As a design parameter, the acquisition rule is not well established in the experimental literature.[15] We selected this “highest earner acquirer” rule in an effort to maximize the chances of observing market power post-merger. However, other acquisition rules may better characterize the natural parallel circumstances. As a check on the importance of the acquisition rule on post-merger performance, we conducted an additional eight *Large Effects* sessions (four *Logit* and four

[12] Revealing relative demand symmetry provides about as much demand information as we felt could reasonably be communicated. Participants can doubtfully digest an explicit characterization of the demand function and demand parameters.

[13] Other things constant, paying participants directly in terms of US dollars is preferable. In this case, however, penny increments provided too coarse a grid for predictions based on continuous demand functions. In some of the “random acquirer” sessions an exchange rate of 10,000 lab dollars to US$ 1.50 was used.

[14] Thus, our information condition is between the BASIC and EXTRA conditions used by Huck et al. (2000) in that we provide full information about demand and costs, but only partial information about others’ actions (e.g., others’ prices but not their profits). We expect that sellers know each others’ prices in most relevant natural contexts, and withholding price information seemed needlessly artificial here, particularly since Huck et al. find that even complete information about others actions does not significantly affect outcomes in a Bertrand environment.

[15] Davis and Holt (1994) selected the merging parties randomly, while Wellford selected the acquired seller randomly and then chose the acquiring seller as the winner of a trivia game unrelated to the experiment. Wellford’s decision was motivated by Hoffman and Spitzer (1982), who report that subjects better exploit their position when it is won rather than conferred. Given the absence of a predicted treatment effect in Wellford, we believed that in this rather subtle strategic context, understanding the strategic environment might be more important than feeling entitled to exploit it and that high earnings pre-merger was a reasonable indicator of understanding the strategic environment. Davis and Wilson (2000) select the acquirer on the basis of winning a game of strategy played prior to opening the market, a procedure we did not use here for reasons of time limitations.

*Linear*) with a “random acquirer” rule. In the random acquirer markets, after period 30 one seller is randomly selected to be acquired, then after excluding the high earner pre-merger, another seller was randomly selected to be the acquirer.[16]

In all sessions, the acquired seller receives a US$ 7 “buyout fee” in addition to their appearance fee and their salient earnings for the session. The instructions did not disclose in advance either the merger or the total number of periods in a session.

The participants were 96 undergraduate students at Virginia Commonwealth University and the University of Arizona. No one participated in more than a single session. In addition to their salient earnings, the participants were paid a US$ 6 appearance fee. Earnings for the 90–130 min sessions averaged about US$ 22.50 and ranged from US$ 14.75 to US$ 32.75.

To summarize, the experiment consists of 24 sessions: four sessions in each cell of the $2 \times 2$ design, {*Logit*, *Linear*} $\times$ {*Small Effects*, *Large Effects*}, with the high earner acquisition rule. In addition, the experiment includes eight additional *Large Effects* sessions (four *Logit* and four *Linear*) using the random seller acquisition rule.

## 4. Experimental results

The share-weighted average price for four representative markets in Fig. 1 allows us to overview some of the experimental results. In each panel of the figure, the hollow and solid dots denote *Logit* and *Linear* sessions, respectively. Notice that all four time series drift smoothly across periods. Further, markets are more nearly drawn to Nash predictions than to either $P_{\mathrm{JPM}}$ or $c$. Nevertheless, the markets do not uniformly converge to Nash predictions. The SWAP falls with a $\pm 5\%$ band about the pre-merger Nash prediction $P_{\mathrm{o}}$ and post-merger Nash predictions $P_{\mathrm{m}}$ (highlighted as bolded lines) only in the *Small Effects* markets.

Comparing across panels suggests a treatment effect associated with changing the size of predicted price effects. Considerably larger price swings are shown in the upper *Large Effects* panel. On the other hand, the representative markets suggest no obvious effect of changing the underlying demand form. Although the *Large Effects* price series vary widely, the overall magnitude of price swings within-each panel appears similar. In what follows we more formally establish these observations suggested by Fig. 1 and then drawn some conclusions regarding the predictive power of the ALM.

### *4.1. Outcomes relative to Nash predictions*

The absence of a uniform tendency for markets to converge to a particular outcome toward the ends of sessions complicates our summary of market performance. On the one hand, absent general convergence, a summary based on a specific subset of “final period” market data would provide potentially misleading information regarding treatment effects. On the other hand, weighting equally decisions made in the final periods of a market with those made in the initial early periods where sellers are just learning about demand and the

[16] We thank an anonymous referee for suggesting this treatment. The high pre-merger earner was excluded from the pool of potential acquirers in order to avoid duplicating observations in the “high earner acquires” markets.

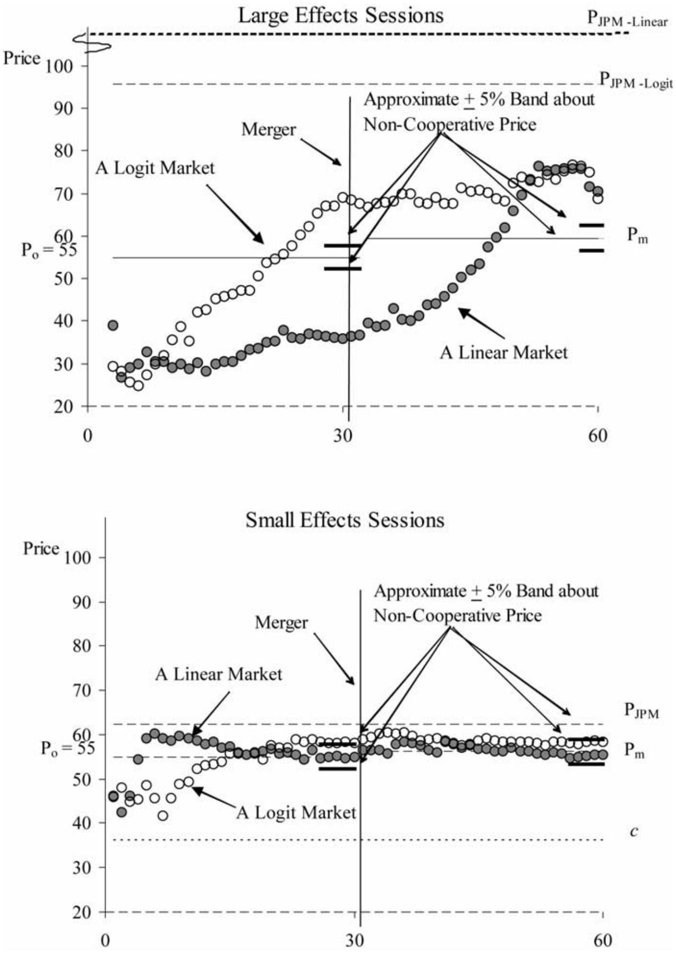


Fig. 1. SWAP paths for some representative markets.

market process may also reflect inaccurately seller responses to treatment effects. We address this problem by evaluating performance using three different market segments as units of analysis: the entire pre-merger and the entire post-merger segments (periods 1–30 and periods 31–60, respectively), the final half of pre-merger and post-merger segments (periods 16–30 and periods 46–60), and for the final five pre-merger and post-merger segments (periods 26–30 and periods 56–60). Tables A1.1–A1.3 in the Appendix report the mean pre-merger and post-merger SWAP for each market for each of these three segments. For purposes of brevity, we present in the text only summary comparisons of these measures.

Table 2 provides summary evidence regarding the relative convergence of markets to Nash predictions. Columns (1) and (2) of Table 2 report, respectively, the number of instances where the mean SWAP is closer to unit costs, $c$, or to the joint-profit maximizing price, $P_{\mathrm{JPM}}$, than to the Nash prediction for the pre-merger period, $P_{\mathrm{o}}$. The Nash predictions

Table 2
Proximity of mean SWAP to $c$ and to $P_{JPM}$

| Treatment (markets) | Pre-merger | | Post-merger | |
|---|---|---|---|---|
| | (1) Instances where (SWAP − $c$) < ($P_o$ − SWAP) | (2) Instances where ($P_{JPM}$ − SWAP) < (SWAP − $P_o$) | (3) Instances where (SWAP − $c$) < ($P_m$ − SWAP) | (4) Instances where ($P_{JPM}$ − SWAP) < (SWAP − $P_m$) |
| All 30 periods | Periods 1–30 | | Periods 31–60 | |
| Logit | | | | |
| *Large Effects* (8) | 1 | 0 | 0 | 0 |
| *Small Effects* (4) | 0 | 1 | 0 | 1 |
| Linear | | | | |
| *Large Effects* (8) | 2 | 0 | 0 | 0 |
| *Small Effects* (4) | 2 | 0 | 0 | 0 |
| Total (24) | 5 | 1 | 0 | 1 |
| Last-15 periods | Periods 16–30 | | Periods 46–60 | |
| Logit | | | | |
| *Large Effects* (8) | 0 | 0 | 0 | 0 |
| *Small Effects* (4) | 0 | 1 | 0 | 0 |
| Linear | | | | |
| *Large Effects* (8) | 1 | 0 | 0 | 0 |
| *Small Effects* (4) | 0 | 0 | 0 | 0 |
| Total (24) | 1 | 1 | 0 | 0 |
| Last-5 periods | Periods 26–30 | | Periods 56–60 | |
| Logit | | | | |
| *Large Effects* (8) | 0 | 0 | 0 | 0 |
| *Small Effects* (4) | 0 | 1 | 0 | 0 |
| Linear | | | | |
| *Large Effects* (8) | 2 | 0 | 0 | 0 |
| *Small Effects* (4) | 0 | 0 | 0 | 0 |
| Total (24) | 2 | 1 | 0 | 0 |

overwhelmingly out-perform the standard rival reference predictions. Pre-merger only the 30-period segment had more than 2 of the 24 markets closer to $c$ than to $P_o$. (The five 30-period segments where markets were closer to $c$ than to $P_o$ reflect some tendency for prices to start below $P_o$.) Similarly, notice in column (2) that the mean SWAP is closer to $P_{JPM}$ than to $P_o$ only once, independent of the segment choice. Post-merger, the relative drawing power of the post-merger Nash prediction, $P_m$, is even stronger. As summarized in columns (3) and (4), the mean SWAP is closer to $c$ or $P_{JPM}$ only once and only in the 30-period segment.

Despite the relative drawing power of Nash predictions, convergence remains rather incomplete, as summarized in Table 3. Columns (1) and (3) report that the mean SWAP's are within 5% of the underlying Nash prediction in no more than one-third of the markets (8 of 24), regardless of the segment-length used as a basis of analysis. Turning to columns (2) and (4), observe that no more than 14 of the 24 markets fall with 10% of the Nash

Table 3
Proximity of mean SWAP to the Nash prediction

| Treatment (markets) | Pre-merger | | Post-merger | |
|---|---|---|---|---|
| | (1) Within $P_o \pm 5\%$ | (2) Within $P_o \pm 10\%$ | (3) Within $P_m \pm 5\%$ | (4) Within $P_m \pm 10\%$ |
| All 30 periods | Periods 1–30 | | Periods 31–60 | |
| Logit | | | | |
| *Large Effects* (8) | 2 | 2 | 1 | 3 |
| *Small Effects* (4) | 2 | **3** | 1 | **4** |
| Linear | | | | |
| *Large Effects* (8) | 2 | 3 | 3 | 4 |
| *Small Effects* (4) | <u>2</u> | **<u>2</u>** | <u>3</u> | **<u>3</u>** |
| Total (24) | 8 | 10 | 8 | 14 |
| Last-15 periods | Periods 16–30 | | Periods 46–60 | |
| Logit | | | | |
| *Large Effects* (8) | 1 | 3 | 0 | 3 |
| *Small Effects* (4) | 2 | **3** | 2 | **4** |
| Linear | | | | |
| *Large Effects* (8) | 1 | 2 | 3 | 3 |
| *Small Effects* (4) | <u>2</u> | **<u>3</u>** | <u>3</u> | **<u>4</u>** |
| Total (24) | 6 | 11 | 8 | 14 |
| Last-5 periods | Periods 26–30 | | Periods 56–60 | |
| Logit | | | | |
| *Large Effects* (8) | 2 | 3 | 1 | 2 |
| *Small Effects* (4) | 1 | **3** | 2 | **4** |
| Linear | | | | |
| *Large Effects* (8) | 1 | 1 | 2 | 3 |
| *Small Effects* (4) | <u>2</u> | **<u>3</u>** | <u>3</u> | **<u>3</u>** |
| Total (24) | 5 | 10 | 8 | 12 |

prediction, again independent of the segment-length used as a unit of analysis. Further, the segment blocks in Table 3 suggests no obvious tendency for markets to converge to Nash predictions toward the end of sessions. For example, as seen at the bottom of column (2), using the last-5 period segment as the unit of analysis, only 10 of the 24 markets are within 10% of $P_o$, and the same number are within 10% of $P_o$ of using a 30-period segment as the unit of analysis. Similarly, as seen in column 4, using the last-5 period segment as the unit of analysis post-merger, 12 of the 24 markets are within 10% of $P_m$, compared to the slightly larger number of 14 who are within 10% of $P_m$ using a 30-period segment as the unit of analysis. In summary, *static non-cooperative predictions organize behavior better than rival competitive or joint-profit maximizing outcomes*; *however, markets generally do not converge fully to Nash predictions*.

This first result was expected, and indeed, the anticipation of this result was one of the motivating factors for this research. The experimental market literature is replete with examples of the relative drawing power of non-cooperative predictions in “Bertrand” type (posted-offer) environments (see e.g., Chapter 4, Davis and Holt, 1993). Nevertheless, the “convergence” observed in laboratory markets typically does not entail anything approaching a precise achievement of static Nash predictions. We suspected that the persistent variability in such markets would confound the behavioral relevance of relatively small changes in Nash predictions motivated by merger-induced changes to market power.

Our second result evaluates the effects of changing the underlying demand form and own- and cross-effect parameters. First, holding predicted price increases fixed, changing the underlying demand system does not appear to affect convergence. Consider, for example, post-merger performance using the 30-period segment as the unit of analysis, summarized in the upper right block of Table 3. Three of the eight *Logit Large Effects* markets are within 10% of $P_{\mathrm{m}}$, compared to four of the eight *Linear Large Effects* markets. Similarly, all four of the *Logit Small Effects* markets are within 10% of $P_{\mathrm{o}}$, as are three of the four *Linear Small Effects* markets. Second, changing underlying parameters does affect convergence: 7 of the 8 *Small Effects* markets are within 10% of the post-merger Nash price $P_{\mathrm{m}}$ as compared to only 7 of the 16 *Large Effects* markets.

The absence of a demand function effect and the significance of a parameter effect is robust to the choice of period segment used as a basis of analysis and to both pre-merger and post-merger behavior. The three columns of Table 4 list *p*-values for Fisher exact probability tests of the null hypothesis that the number of mean SWAP’s that are within 10% of the Nash prediction does not differ across groups. As seen in columns (1) and (2) one cannot reject this null hypothesis for demand system changes either for *Large Effects* or *Small Effects* markets at anything approaching conventional significance levels, regardless of the period-segment used as the unit of analysis and regardless of whether we consider pre-merger or post-merger behavior. On the other hand, as seen in column (3), *Small Effects* markets clearly converge more completely than *Large Effects* markets. For every comparison except the 30-period segment pre-merger, *Small Effects* markets are closer to the relevant Nash prediction than the comparable *Large Effects* markets at a minimum 95% confidence level. To summarize, *changing underlying parameters affects behavior*. *Convergence to underlying non-cooperative predictions is more complete in markets with smaller*

Table 4
Fisher exact probability tests for differences in the tendencies of markets to converge within 10% of the underlying Nash prediction

| Segment | Cells compared (sessions per cell) | | |
|---|---|---|---|
| | (1) *Logit Small Effects* (4) vs. *Linear Small Effects* (4) | (2) *Logit Large Effects* (8) vs. *Linear Large Effects* (8) | (3) *Large Effects* (16) vs. *Small Effects* (8) |
| Pre-merger | | | |
| All 30 periods | .43 | .36 | .125 |
| Last-15 periods | .571 | .36 | **.049** |
| Last-5 periods | .571 | .25 | **.026** |
| Post-merger | | | |
| All 30 periods | .50 | .66 | **.047** |
| Last-15 periods | 1.00 | .66 | **.004** |
| Last-5 periods | .50 | .65 | **.013** |

Table 5
Percentage price increases post-merger and pre-merger deviations from $P_o$, under different acquirer rules

| Segment | Acquirer rule: Highest earner [Median, mean (S.D.)] | Acquirer rule: Random [Median, mean (S.D.)] | Difference | Mann–Whitney $U$ (8, 8 d.f.)[a] |
|---|---|---|---|---|
| Percent price increase post-merger | | | | |
| All 30 periods | 10, 16 (32) | 11, 19 (27) | −1 | 28 |
| Last-15 periods | 21, 32 (38) | 13, 14 (25) | 8 | 24 |
| Last-5 periods | 11, 29 (38) | 3, 5 (17) | 8 | 18 |
| Percent deviation from pre-merger $P_o$ | | | | |
| All 30 periods | −11, −11 (18) | −20, −18 (15) | 9 | 22 |
| Last-15 periods | −17, −14 (14) | −11, −9 (20) | −6 | 29 |
| Last-5 periods | −15, −15 (32) | −10, −6 (21) | −5 | 24 |

[a] A 90% level of confidence requires $U < 15$.

*predicted comparative-static effects. However, holding the predicted comparative static effects constant, changing the demand form does not to affect convergence in these markets.*

This second result suggests that the ALM might perform well as a screening device despite the generally poor drawing power of non-cooperative predictions. Even though *Small Effects* markets do not latch on to non-cooperative predictions, larger price swings, and more sizable deviations occur in *Large Effects* markets, where larger effects would be predicted by the ALM.

Prior to considering the predictive power of the ALM, we comment briefly on the effect of changing the acquiring-seller rule on post-merger performance. The high variability of market outcomes and the relative subtlety of predicted price increases confound our drawing conclusions regarding the effects of acquiring-seller rule alterations on the exercise of market power, and more generally on the propensity of sellers to exercise market power at all. Using the percentage increases in prices post-merger to measure price changes, compare—the eight sessions with the “random acquirer” rule with the eight sessions with the “high earner acquirer” rule (all *Large Effects* sessions). As the upper portion of Table 5 illustrates, differences across the merger rules never approach significance, even though for the last-15 and last-5 period segments, median price increases are 8 percentage points higher in the markets using the “high earner” acquirer rule than in the markets using the “random acquirer” rule. The very large standard deviations reflect the very high variability in outcomes within treatment cells.

Further, the variability of pre-merger prices relative to the pre-merger equilibrium, $P_o$, confounds our using observed post-merger price increases as a measure of market power. Markets substantially below $P_o$ may increase post-merger purely out of a weak tendency to equilibrate. The median price deviations from $P_o$, shown in the bottom half of Table 5, provide evidence suggesting that this effect may drive some of the differences in median price increases observed across the “random acquirer” and “highest earner acquirer” sessions. Notice that for the last-15 and last-5 period segments, median pre-merger prices are 5 to 6 percentage points lower in the “highest earner acquirer” markets than in the “random acquirer” markets, which would lead us to anticipate the larger median increases observed post-merger in the “highest earner acquirer” markets. Other measures of market power similarly do not differ significantly across the acquiring seller rules. In fact, overall, these measures suggest that sellers exercised market power rather infrequently. For example, in this symmetrical game, the model predicts that the merged firm should post the same price in each market, and this price should be the highest price posted. Using the final-15 period segment as the unit of analysis, the consolidated firms posted the same price for each firm more than 7 times in only 3 of the 24 markets. The merged seller was a price leader more than seven times in only eight markets.[17] *In sum, nothing strongly suggests that changing the rule for identifying the acquiring seller alters outcomes, and indeed, there is little evidence to suggest that sellers in any treatment exercised market power frequently*.

### 4.2. The predictive power of the ALM

We now turn our attention to how well predictions based upon pre-merger outcomes distinguish problematic mergers from non-problematic ones. If the form of the underlying demand function is known a priori, the true demand parameters ($V_i$, $\alpha$, and $\theta$ for a linear system, and $\beta$ in a logit system) are perfectly recoverable from observed price and quantity data.[18] These parameters, when combined with the observed price and quantity choices, imply a cost vector $\boldsymbol{c}$ and a post-merger price vector $\boldsymbol{p}$. As in the previous subsection, we consider all 30, last-15, and last-5 period segments as units of analysis. For each segment, we use each seller's average price choice and average quantity outcome for that segment as the relevant measure of price and market share data.

In natural contexts, the analyst doubtfully knows with certainty the underlying functional form of the demand system. To obtain some feel for the potential effects of demand misspecification, we also estimate linear parameters $V_i$, $\alpha$, and $\theta$ with data from the logit demand markets and logit parameter $\beta$ with data from the linear demand markets. Regressions with price and share (quantity) data from the first 30 periods of each market yield estimates of $V_i$, $\alpha$, and $\theta$ or $\beta$.[19] Given estimates of the relevant demand parameters and an implied equilibrium cost vector $\mathbf{c}$, we can generate post-merger prices and market share predictions, as in the case of known demand.

Brevity considerations prevent a complete display of the pre-merger cost estimates and post-merger price predictions (see Tables A2.1–A2.3 in Appendix A). We confine our comments to two observations. First, using observed price and quantity data to generate implied costs results in a cost structure that deviates dramatically from the actual costs, as Fig. 2 illustrates for the last-15 period pre-merger segment.[20] Each of the four horizontally

[17] Summary measures of market power exercise are reported in an unpublished data appendix available at the website listed in the Acknowledgements.

[18] Actually, even with a priori knowledge of the underlying demand form, $\eta$ cannot be directly derived from observations in a logit demand system. We skirt this issue here by using the actual $\eta$.

[19] Procedurally, we estimate each seller's demand parameters as a system of seemingly unrelated equations where relevant demand parameters are constrained to equality across systems. For a linear system, regress observed quantities on the demand function (11). For a logit system, estimate substitutability $\beta$ as the log ratio of any two shares. $\ln(s_i/s_j) = (\alpha_i - \alpha j) - \beta(p_i - p_j)$. We arbitrarily select the price series for S4 as the $p_j$ series and then simultaneously estimate $\beta$ with equations using S1, S2, and S3 as the numerator. As for the case when demand is known, $\eta$ is not recoverable from observed data but must be estimated separately.

[20] Implied costs distributions for all 30 and last-5 period segments are similar and are available in the Supplementary Data Appendix.

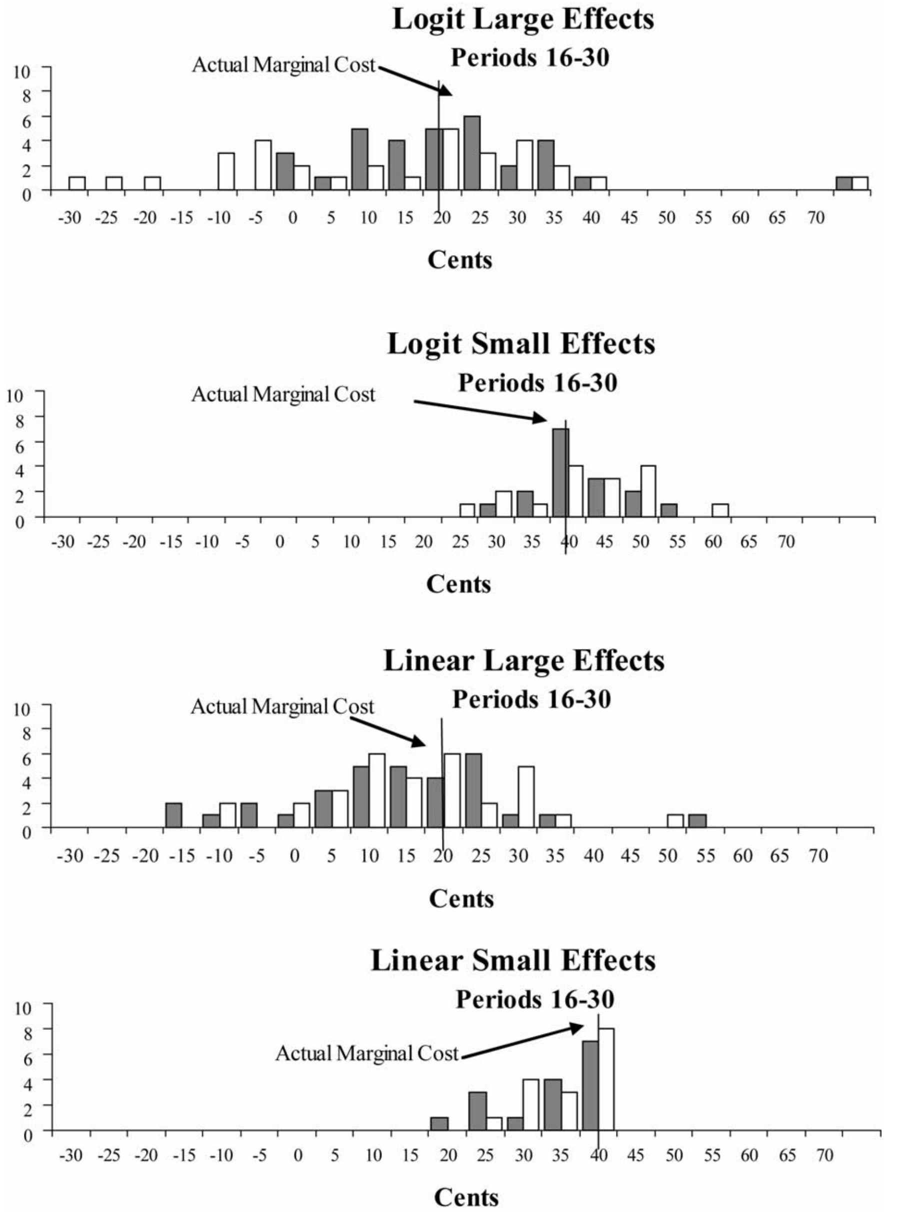


Fig. 2. Implied cost distributions, based on price decisions for the last-15 periods pre-merger (periods 16–30). Key: grey bars reflect distribution of implied costs for correctly specified demand. White bars indicate reflect implied costs with mis-specified demand.

aligned panels in the figure displays the distribution of implied equilibrium costs for all sellers in each treatment cell. The black bars illustrate the distribution of implied costs with correctly specified demand, while the white bars display the implied cost distribution with mis-specified demand. As each of the panels illustrate, implied costs vary wildly from actual costs. Consider first the black bars when the demand system is correctly specified. For example, in the *Logit Large Effects* markets, shown at the top of Fig. 2, implied costs range from −3.90 cents to 91.37 cents, dramatically different from the actual unit costs of 20 cents. Even in the *Linear Small Effects* treatment, where the deviation of implied costs from actual costs is the smallest, implied costs range from 15.82 cents to 39.37 cents, considerably different than the actual unit costs of 36.2 cents.[21] With the possible exception of some of the *Logit Large Effects* sessions, the white bars illustrating implied costs for mis-specified demand are not substantially more scattered than the black bars, suggesting that disequilibrium pricing, rather than incorrectly estimated demand causes the bulk of the implied cost dispersion.[22] To summarize, *our data indicate that the assumption of equilibrium behavior pre-merger is an important one and noticeably unobserved in this experiment*. *Disequilibrium pricing behavior can result in implied cost vectors that deviate very substantially from actual costs*. *On the other hand, demand misspecification in this environment does not importantly affect implied costs*.

This result is important for two reasons. First, implied cost heterogeneities imply similarly heterogeneous post-merger price predictions. Markets with generally below equilibrium prices pre-merger will generate smaller own and cross-price elasticities, which in turn result in larger predicted price increases post-merger. Subsequently observed price increases may thus reflect, at least in part, some tendency for markets to equilibrate post-merger. Second, demand mis-specification does not appear to be a primary motivation for errant predictions in this context. For this reason, in what follows we confine our attention to predictions with correctly specified demand.[23]

Our second observation pertains to the relationship between predicted and observed price increases. Despite the imprecision of implied costs, we surprisingly find that the ALM serves some role as a screening device. The scatter plot in Fig. 3 illustrates this using last-15

[21] Large, perhaps implausibly large variations in implied unit cost structures are hardly an artifact of our laboratory data. For example, Froeb et al. (1998) use the market for long distance telephone services in Japan as an illustrative example of the ALM. In their example, implied marginal costs for fringe carriers are 73% higher than for the dominant carrier.

[22] The difference in the implied cost distributions in the *Logit Large Effects* markets for correctly specified and mis-specified demand is largely an artifact of the way linear demand parameters were estimated for these markets. In the *Linear* sessions (typically initial), periods where some sellers posted disparately high prices imply negative sales for the period. We truncated negative sales quantities to zero. To generate logit estimates from the linear market data, we deleted those periods with sales quantities of zero for some sellers, since log(0) is undefined. The converse, however, was not true. *Logit* markets always generate positive sales, even with very disperse prices. Thus, no periods were deleted from the linear estimates of the logit markets. As detailed in the Supplementary Data Appendix, deleting a very few initial periods from some the *Logit* markets greatly improves *Linear* estimates.

[23] Tables A2.1–A2.3 reported predicted increases with both correctly specified and mis-specified demand. As can be verified from inspection of these tables, the degradation in the relationship between observed and predicted price increases due to demand mis-specification is fairly small.

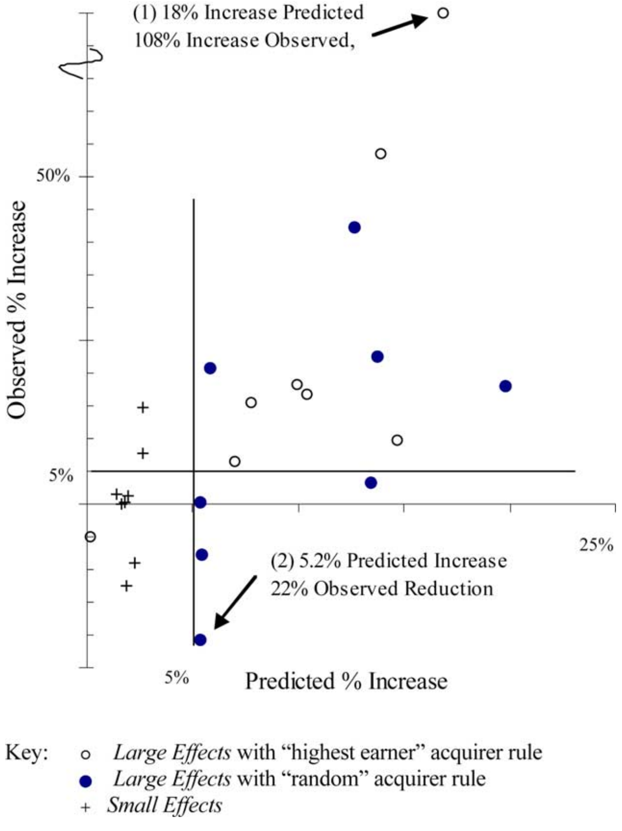


Fig. 3. Predicted price increases (periods 16–30) vs. observed price increases (periods 46–60).

periods segments.[24] In the figure, the bolded horizontal and vertical solid lines illustrate 5% predicted and 5% observed increases, respectively. Notice first that predictions sometimes err very badly. For example, the observed 108% increase shown as the hollow dot labeled (1) in the figure very substantially exceeds the predicted 18% increase. Similarly, the observed 22% post-merger price drop, shown as a solid dot labeled (2) was poorly predicted as a 5.2% price increase. Nevertheless, 17 of the 24 markets are correctly classified by the ALM. Eleven dots fall in the upper right quadrant of the figure, indicating instances where a 5% or greater increase is correctly predicted. Another six markets fall in the lower left quadrant, indicating instances where 5% or lower price increases were both predicted and observed.

The middle panel of Table 6 summarizes the classification of outcomes by a predicted 5% increase, using last-15 periods segments as the unit of analysis. Using the Fisher exact probability test, the null hypothesis that the classification process is random may be rejected

[24] Comparable figures for all-30 and last-5 period segments are similar and can be constructed from data in Tables A2.1–A2.3

Table 6
Classification of outcomes

| Observed | All-30-period segments | | Last-15 period segments | | Last-5 period segments | |
|---|---|---|---|---|---|---|
| | Predicted | | Predicted | | Predicted | |
| | <5% | >5% | <5% | >5% | <5% | >5% |
| <5% | 2 | **11** | 2 | **11** | 2 | **10** |
| >5% | **7** | 4 | **6** | 5 | **7** | 5 |
| Fisher *p* | | .02 | | .049 | | .04 |

at a 95% confidence level. The left and right panels of Table 6 report a comparable classification of predicted and observed price increase for last-5 and all-30 period segments. As is clear from the table, the selection of a subset of periods as the unit of analysis does not affect the result that the ALM has some capacity to identify potentially problematic mergers.[25] Further and perhaps as noteworthy is the small number of "false negatives" (instances where post-merger price increases are not predicted). Regardless of the segment used as the unit of analysis, of the 24 markets, the ALM fails to anticipate just 2 instances where prices increased by more than 5% post-merger. Thus, even though the ALM does not accurately predict post-merger performance, the ALM here serves some role as a screening device.

Given the extremely poor organizing power of Nash predictions and the rather questionable evidence of market power exercise, this last result is surprising (at least to us). Why does it happen? A large part of the explanation lies in the relationship between the predicted increases and pre-merger price levels. In particular, large price increases tend to occur in markets that are substantially below equilibrium pre-merger. This tendency is illustrated by the scatter plot in Fig. 4 which graphs the relationship between the percentage deviation from $P_o$ pre-merger and the percentage observed price increase post-merger, using last-15 period segments as the unit of analysis. The strong inverse relationship between pre-merger price deviations and post-merger price increases shown in Fig. 4 is striking. In fact, comparing Figs. 3 and 4 suggests that the relationship between pre-merger equilibrium deviations and post-merger price increases is stronger than that between ALM predictions and post-merger performance.[26]

To evaluate more formally the relationship between predicted and observed post-merger behavior, we regress observed post-merger percentage price increases on two competing explainers of post-merger performance: (a) the percentage price increase predicted by the

[25] As shown in Appendix Tables A2.1 to A2.3, incorrect demand specification does not reduce the ALM's predictive capacity.

[26] Although elasticity adjustments driven by deviations from the underlying Nash predictions can allow ALM predictions to have some explanatory power even without market power exercise, it is interesting to observe where the ALM adjustments fail. Notice in particular in Fig. 4 that the two "false negatives" that appear to the left of the 5% observed increase and above the 5% predicted increase are crosses, indicating that these were *Small Effects* sessions. Pre-merger prices were substantially below $P_o$ in both of these sessions. However, ensuing adjustments predicted by the ALM did not allow sufficiently for post-merger equilibration. Notice in Fig. 4 that crosses no longer stand out as exceptional.

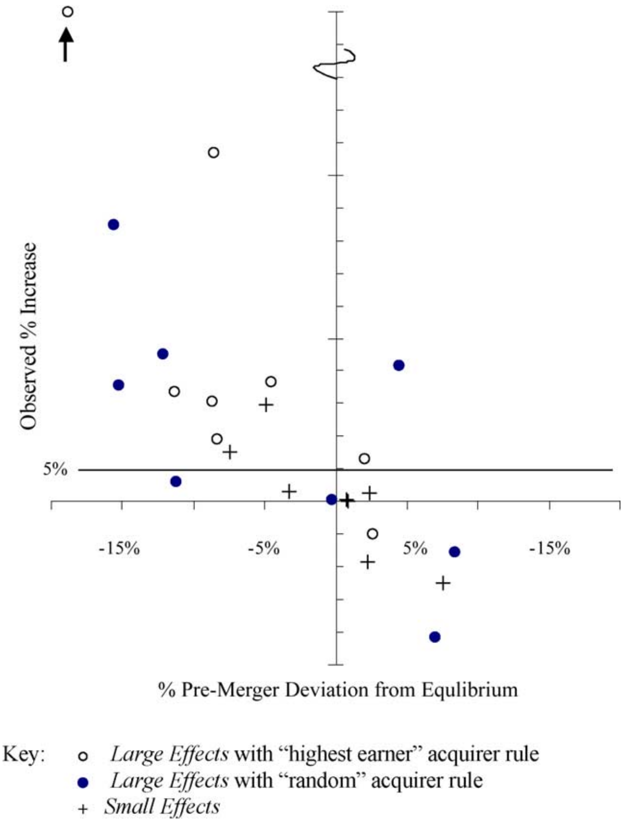


Fig. 4. Mean SWAP deviation from $P_o$ (periods 16–30) vs. observed price increases (periods 46–60).

ALM and (b) the deviation of pre-merger prices from $P_o$.[27] Specifically, we estimate

$$O_i = \beta_o + \beta_r D_r + \beta_1 X_i + \beta_2 X_i^3 + \varepsilon_i, \tag{19}$$

where the dependent variable $O_i$ is the observed percentage increase in prices post-merger and $X_i$ is a measure from pre-merger outcomes, either the percentage increase predicted by the ALM, %PD$_i$, or the percentage deviation from the pre-merger Nash equilibrium price, %ND$_i$. A cubic term $X_i^3$ is included to allow for the possibility of non-linear effects between predicted and observed behavior that may work in the negative domain as well as in the positive domain. Finally, $D_r$ is an indicator variable that takes on a value of 1 if the session used the "random acquirer" rule.

Columns (1)–(3) of Table 7 report the estimates relating ALM predictions to observed price increases. Notice that independent of the segment used as the unit of analysis, the

[27] Ideally, both pre-merger price deviations and ALM predictions would be included in a single regression. However, the two variables are so co-linear that distinguishing between their effects with a single regression is impossible.

Table 7
OLS to explain observed percentage price increases $O_i = \beta_o + \beta_r D_r + \beta_1 X_i + \beta_2 X_i^3 + \varepsilon_i$

| Estimate (standard error) | ALM prediction, $X_i$ = %PD$_i$ | | | Percentage deviation of the SWAP from $P_o$, $X_i$ = %ND$_i$ | | |
|---|---|---|---|---|---|---|
| | (1) 30 periods | (2) Last-15 periods | (3) Last-5 periods | (4) 30 periods | (5) Last-15 periods | (6) Last-5 periods |
| $\beta_o$ | 4.09 (8.26) | −7.17 (8.33) | −8.40 (7.99) | 1.26 (3.85) | 5.38 (3.88) | 6.20 (4.11) |
| $\beta_r$ | 4.37 (9.45) | −18.04$^*$ (9.21) | −24.45$^{**}$ (8.91) | −0.08 (6.85) | −11.48$^*$ (6.42) | −11.22 (6.71) |
| $\beta_1$ | −0.41 (1.70) | −3.64 (1.67) | 3.89$^*$ (1.39) | −0.618$^*$ (0.37) | −0.126 (.337) | −0.035 (.36) |
| $\beta_2$ | 0.965$^{**}$ (0.48) | −0.065 (0.49) | −0.007 (0.32) | −0.067$^*$ (0.29) | −0.156$^{***}$ (0.40) | −0.167$^{**}$ (0.04) |
| $\bar{R}^2$ | 0.41 | 0.43 | 0.49 | 0.67 | 0.70 | 0.68 |
| $\beta_1 = 0$ and $\beta_2 = 0$ ($F_{(2,21)}$) | 7.98$^{***}$ | 10.05$^{***}$ | 11.84$^{***}$ | 22.71$^{***}$ | 27.58$^{***}$ | 24.53$^{***}$ |

$^*p < .10$, $^{**}p < .05$, $^{***}p < .01$, two tailed tests.

regression does a reasonable job explaining observed price increases ($\bar{R}^2$ is at least 0.41). Further, as indicated at the bottom of the table, $\beta_1$ and $\beta_2$ are jointly different from zero in each equation.

Columns (4)–(6) of Table 7 report the estimates of post-merger price increases as a function of deviations from the underlying pre-merger Nash equilibrium. Notice that price deviations do a better job explaining post merger price movements than ALM predictions ($\bar{R}^2$ ranges between .67 and .70). Further, the choice of the acquiring seller rule less clearly affects performance in Eqs. (4)–(6), than in (1)–(3), $\beta_r$ differs significantly from zero only at 90% confidence level and only for the last-15 period segment, reflecting the result that pre-merger prices were higher in several of the sessions conducted with the random rule. *In summary then, although ALM predictions appear to explain a considerable portion of post-merger price increases, the deviation of pre-merger prices from the underlying equilibrium appears to explain even more of post-merger price increases.*

## 5. Discussion and conclusion

The ALM appears to screen out problematic mergers quite well here, in the sense that large price increases tend to arise in markets where sizable increases are predicted. However, the evidence reported here suggests that the ALM predicts post-merger price increases primarily by the extent to which markets deviate from the pre-merger equilibrium. Thus, in our markets, it appears that the pre-merger deviations from the underlying pre-merger equilibrium is a far more powerful driver of behavior than the exercise of market power per se.

In naturally occurring markets, sellers who are experienced with their market and well aware of their strategic circumstances may exercise market power more obviously than we observe here, but naturally occurring markets also undoubtedly deviate from their underlying equilibrium due to a plethora of uncontrolled factors that affect them regularly. Allowing for the possibility that markets are out of equilibrium means that ALM predictions necessarily represent a combination of a potential for increased market power and the extent to which markets deviate from the equilibrium. To us, this represents an extremely questionable basis for public policy. Large predicted effects may be merely a consequence of depressed pre-merger prices. Small predicted effects may mean that pre-merger prices exceed the underlying equilibrium sufficiently to dwarf the subsequent consequences of market power. Unless the ALM can be modified to distinguish potential market power exercise from the effects of pre-merger deviations, we are skeptical of its value.

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## Appendix A. Supplementary data

Supplementary data associated with this article can be found, in the online version, at 10.1016/j.jebo.2004.11.001.

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