TRACKING CUSTOMER SEARCH TO PRICE DISCRIMINATE

CARY A. DECK and BART J. WILSON*

The electronic technologies of the Internet make it possible for sellers to track potential customers and discriminate between the informed and uninformed. In this article, we report an experiment that investigates the market impact of firms tracking customers and offering discriminatory prices based on search history. We find that consumers, on average, face the same prices when sellers have the ability to track customers and price discriminate as when sellers post a single price for all buyers. However, informed buyers receive lower prices when sellers can detect buyer search, whereas uninformed buyers receive lower prices when firms cannot track customers. (*JEL* D43, L13, C92)

Suppose that every time you walked around the mall, somebody put a bar code on your shoulder and, as you walked into the shops in the mall, someone came up and scanned your shoulder and got the number ... went to a database, saying, "Ah, yes, that's Lesley who visited the shop next door 15 minutes ago." That's the level of surveillance that's going on on the Internet.

—Internet entrepreneur Jason Catlett in a *60 Minutes* interview with Lesley Stahl ("Not As Private As You Think," aired November 28, 1999)

I. INTRODUCTION

Transaction mechanisms in an electronic marketplace can be much different from those used in traditional markets. Predominantly, "brick and mortar" markets are described as sellers posting single offer prices that buyers either accept or reject through their purchase decisions. Without necessarily holding everything else constant, two buyers, each making concurrent purchases in the same store, pay the same posted price for an identical item.[1] In particular, a buyer that has visited multiple rival stores receives the same price as a buyer that impulsively purchases without comparing any prices.

In contrast, a combination of Internet technologies makes it possible to identify the search history of potential customers. Online retailers generally make use of a standard programming device that produces electronic files to tag individual customers with a unique identification. Commonly referred to as *cookies*, these small computer files are stored on the hard drive of the customer's computer. Developers created cookies so that a Web site could maintain state even though the HTTP protocol is stateless. This allows a Web site to recognize an individual and accommodate multiple-item purchases from an Internet retailer.[2] An unintended consequence of this technology is that other Web sites, namely rival sellers, could use a second Internet device called a *Web bug* to retrieve the identity of the Internet domain that placed a cookie, even though the

*We thank two anonymous referees for their comments that have improved this paper. The paper has also benefitted from conversations with Gundoz Caginalp, Raja Kali, Dan Kovenock, Vernon Smith and Ferenc Szidarovsky and from seminars at Florida State University, Purdue University, the University of Mississippi, the University of Massachusetts, Virginia Commonwealth University and the Meetings of the Southern Economic Association. The data and a sample copy of the instructions are available upon request. We thank Jia Jing Liu, Prashanth Murthy, and Ben Toff for excellent research assistance in running these experiments. The authors gratefully acknowledge financial support from the International Foundation for Research in Experimental Economics.

Deck: Department of Economics, Walton College of Business, University of Arkansas, Fayetteville, AR 72701. Phone 1-479-575-6226, Fax 1-479-575-3241, E-mail cdeck@walton.uark.edu

Wilson: Interdisciplinary Center for Economic Science, George Mason University, 4400 University Drive, MSN 1B2, Fairfax, VA 22030-4444. Phone 1-703-993-4845, Fax 1-703-993-4851, E-mail bwilson3@gmu.edu

1. Exceptions include various traditional forms of price discrimination, such as coupons or quantity discounts.

2. Weber (2000) reports that Web sites were initially designed to process each information requests one by one. Thus, without cookies, an individual customer would have to purchase each item separately. Netscape designed the original browser specifications so that a Web site could access only its own cookies.

Economic Inquiry
(ISSN 0095-2583)
Vol. 44, No. 2, April 2006, 280–295

doi:10.1093/ei/cbj014
Advance Access publication November 16, 2005

specific information content of the cookie is typically encrypted. Two different types of Web bugs can ascertain the search history of a customer. A Web bug is a graphic—typically, one pixel by one pixel in size—on a Web page, and it is designed to monitor who is reading the Web page. Web bugs are virtually invisible because they are uncolored and so small. One type, an executable Web bug, is a file that monitors a machine's traffic and hard drive and periodically sends the information back to the Web site that planted the bug on the machine. The second type is not physically located on the machine and uses a technology called *scripts* (e.g., JavaScript, ActiveX, and Perl) to scan a hard drive searching for files.[3,4]

Because the process of identifying a customer and posting an offer price is electronic and immediate, sellers on the Internet can use this technology to post dynamic, customer-specific prices.[5] However, the major contributions to the rich literature on consumer search consider only models in which firms post a single price regardless of how informed the customer may be. For example, Braverman (1980), Varian (1980), Stiglitz (1979), and Salop and Stiglitz (1977) assume that a consumer is either fully informed or completely unaware of other stores' prices and that each firm posts a single price to any customer. Other work considers sequential consumer search, but again each firm posts a single price regardless of how informed the customer may be—see, for example, Stahl (1989), Stiglitz (1987), Rob (1985), Burdett and Judd (1983), Carlson and McAfee (1983), and Wilde and Schwartz (1979).[6] This article explores the fundamental changes to traditional sequential customer search in posted offer markets when firms have the ability to track customers and price discriminate accordingly.

Using controlled experimental markets, we investigate how the ability to track customers and offer differentially informed customers different prices affects the prices that buyers receive in aggregate. We can also readily identify the level to which customers are informed, and we can ascertain the distribution of prices that these well-defined customer segments receive. Within the confines of the laboratory, we can control such variable factors as whether customers can be tracked, the degree to which customers compare prices, seller competition, customer preferences, and seller costs. As a control group, we also study markets in which buyers cannot be tracked, thereby forcing sellers to post a single price regardless of how well informed a customer may be.

We conducted a laboratory experiment for two reasons. First, even in the rather straightforward analytical framework that we employ, there is an inherent problem in attaining a theoretical solution without making auxiliary assumptions. Thus, our experiment provides insight into a new market phenomenon, price discrimination of differentially informed customers. Second, a field study is problematic because of the difficulty in finding naturally occurring data. The nature of Internet transactions makes it difficult to collect data from consumers on the distribution of prices for heterogeneous segments of price searchers. Furthermore, Internet firms would naturally be reluctant to release information if indeed they are discriminating based on tracking data, because customers may become peeved ex post on learning that

3. See, for example, Olsen (2001) and Rodgers (2001). In an article on the technology of spyware, adware, and pop-up ads, Sullivan (2003) reports, "'When are people going to get it, that every time they look at something they are being watched?' said one industry watcher, who asked not to be named. [Keith Smith CEO of 180Solutions] and others in the adware side of the industry insist there is no privacy invasion when consumers are watched anonymously. To the contrary, consumers enjoy having the chance to do last-minute comparison shopping before they finish the checkout procedure at an e-commerce site, he said. 'I would challenge anyone to make the case that giving consumers more options at the point of sale is bad,' he said."

4. Another way for Web sites to track customers is to exploit flaws in new versions of browsers that to permit a Web site to gain access to the cookies placed by a second Web site. Even when patches are developed and posted to fix these glitches, the diffusion of these patches is unlikely to be complete, leaving some people trackable.

5. Two instances of cookie-specific pricing (but without Web bugs), have recently transpired, both involving Amazon.com. Amazon customers learned in an online discussion forum that the price of several DVDs differed among buyers, depending on whether or not they had cached cookies—see Streitfeld (2000) and Wolverton (2000b). Streitfeld (2000) and Wolverton (2000a) also report that customers discovered that Amazon was offering "random" prices on an MP3 player (up to $51 less than the regular $233.95 price). Amazon defended the random pricing as market research.

6. See Davis and Holt (1996) and Cason and Friedman (2003) for examples of experimental studies on costly buyer search and price dispersion. The sellers in their designs could not price discriminate to informed and uninformed buyers.

they paid more than what other customers paid for the same item.[7]

As a preview of the results, we find that consumers, on average, face the same prices when sellers have the ability to track customers and price discriminate as when sellers post a single price for all buyers. However, different types of buyers are affected differently by tracking. Informed buyers receive lower prices when sellers can detect buyer search, whereas uninformed buyers receive lower prices when firms cannot track customers.

The outline of the chapter is as follows. Section II describes our experimental design for comparing markets with and without customer tracking. In section III we discuss the results of the market experiment, and in section IV we offer our concluding remarks.

II. EXPERIMENTAL DESIGN AND PROCEDURES

To explore how tracking buyers affects the performance of markets, we conducted a series of experimental markets. Each computerized experimental session consisted of undergraduates at the University of Arizona. Many of our participants had experience in various unrelated economic experiments; however, for some this was their first experiment.

In each session, four participants competed as sellers of a fictitious good in a posted offer market. Buyers in the market were fully revealing automated robots; that is, if a seller posted a price below the buyer's value for the good, the agents completed a transaction in experimental dollars. Every three seconds, which we refer to as a period, a new computerized buyer would enter the market with a randomly drawn value v for one unit of the good drawn from a uniform distribution with support $[\underline{v}, \bar{v}.]$ The number of periods was fixed at 1,200 but was unknown to the participants. Each buyer also had a randomly drawn order over which to query prices from the sellers, and each buyer was randomly assigned the number of sellers that the buyer would query. We refer to a buyer who searches i sellers as a type i buyer. It is public information that fraction $\omega_i > 0$ of the buyers are of type i with

$$\sum_{i=1}^{n=4} \omega_i = 1.$$

As the number of sellers that the buyer visits increases, the buyer can be considered more informed.[8] In the event that multiple sellers offered the same lowest price to a buyer, the buyer randomly determined from which of the tied low-price sellers to consider a purchase.

Each seller quotes a single price p and receives a profit of $p - c$ if a buyer agrees to make a purchase, where c is the common-knowledge constant marginal cost. In the event that two sellers quote the same minimum price, the buyer randomly selects a seller to consider. The buyer receives utility of $v - p_L$ if $p_L < v$, as the buyer is assumed to be completely rational and truthfully revealing. In the event that the buyer makes no purchase, the buyer's utility is 0.

We conducted 16 experimental sessions, 4 under each treatment pair in a 2×2 design. For the first treatment variable, participant sellers participated in one of two buyer tracking institutions. Sellers in the No Tracking treatment did not know the number of sellers visited by the buyer. Each seller could set only a single price, which was described to the participants as "Set price at □," where the participants fill in the box. In contrast, a seller in the *Tracking* treatment could determine the number of different sellers visited by the buyer. More specifically, each participant chose a price schedule described as "Price if visited 1st = □, Price if visited 2nd = □, Price if visited 3rd = □, and Price if visited 4th = □," where participants filled in the boxes.

Participants maintained their single price or price schedule for 15 to 25 periods, with the exact number of periods randomly drawn

7. See Wolverton (2000b). In reference to the variable DVD prices, one customer at DVD Talk Forum posted the message, "Okay quick solution for Amazon's little dishonest pricing scam," and another customer wrote, "Sounds like illegal price descrimination [*sic*]." A hard copy of the forum discussion is available upon request. In addition, Internet retailers are likely to be wary of drawing regulatory attention.

8. Following Braverman (1980), Varian (1980), Stiglitz (1979), and Salop and Stiglitz (1977), we treat consumer search as being exogenously determined. In these works buyers are either completely informed of prices or are uninformed, dependent on personal characteristics, essentially the buyers of Type 1 or Type n. We generalize this by allowing buyers to be of Types 2, …, n – 1. This is consistent with certain shoppers having preferences for or knowledge of some subset of the retail outlets.

from a discrete uniform (15, 25) distribution each time a new pricing policy was implemented by the participant.[9] When the required duration of a pricing policy expired, participants could either maintain the current pricing policy indefinitely or adopt a new one, the form of which depended on the treatment (No Tracking or Tracking). While the current price or price schedule was forced to remain unchanged for 15 to 25 periods, the subjects (1) knew the number of periods until they could change their price or price schedule again and (2) could enter a new pricing policy to implement at the first possible opportunity. We chose to implement this procedure because we believe that a variable timing of decision making better represents the naturally occurring (Internet) economy than does the game of simultaneous moves.

The second treatment variable is the market environment. In all sessions, buyer values were drawn from a uniform (25, 125) distribution. However, the distribution of buyer types differed across treatments. In the Low Search treatment, $\omega_1 = .61$ and $\omega_2 = \omega_3 = \omega_4 = .13$, but in the High Search treatment, the likelihood that a buyer will comparison shop increases with $\omega_1 = \omega_2 = \omega_3 = \omega_4 = .25$. In all sessions, $c = 25$ for each of the four sellers. These market conditions were common knowledge among the subjects.

After each period participants received feedback about the price they quoted to the buyer if they were visited and their profit if they made a sale. In the No Tracking treatment, sellers were given the prices posted by their rivals last period, and in the Tracking treatment subjects were given the price that their competitors would have quoted to anyone who visited that competitor first.[10] At the conclusion of the experiment the participants were paid their earnings over the entire 1,200 periods, which were converted into U.S. dollars at a rate of 1 dollar for 300 experimental dollars. Additionally, participants received a fee of $5 for showing up on time. Table 1 provides the average payoff by treatment for the 75-minute sessions, excluding the $5 show-up fee.

TABLE 1
Average Payoffs (US$) by Algorithm

Game theoretic–Low Search	15.25
Game theoretic–High Search	6.25
No Tracking–Low Search	15.25
No Tracking–High Search	10.75
Tracking–Low Search	15.75
Tracking–High Search	10.00

Note: These payoffs do not include the $5 payment for showing up on time.

Reference Predictions

One potential reference prediction for the No Tracking treatment is the competitive outcome, $p = c$, but as Varian (1980) shows, no pure strategy equilibrium at any price exists in the stage game. For $\omega_1 > 0$, p_m strictly dominates the competitive price. Given the repeated nature of this game and the unknown time horizon, a second potential reference prediction is $p = p_m$.

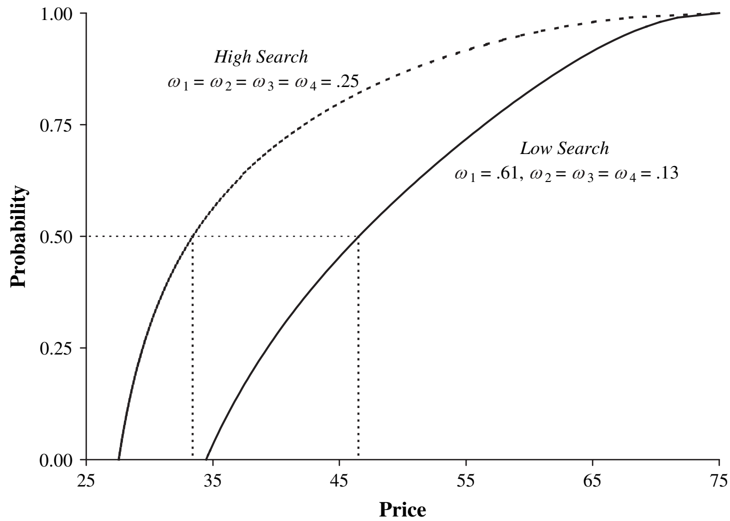
The Varian model makes a prediction for the one-shot simultaneous move game. To determine the symmetric mixed-strategy equilibrium in this environment, we first calculate the seller's "security profit," or the payoff that the seller can unilaterally achieve. In this environment a monopolist receives profit π_m, which can be calculated by maximizing with respect to p the monopolist's profit function, $\pi = [(\bar{v} - p)/(\bar{v} - \underline{v})](p - c)$. As described, $p - c$ is the seller's profit from a sale. The first term in the profit calculation is simply the probability that a buyer has a value $v > p$. The profit-maximizing price is $P_m = (\bar{v} + c)/2$, which generates $\pi_m = (\bar{v} - c)^2/[4(\bar{v} - \underline{v})]$. In an n-seller environment, a seller can act as a monopolist for a proportion ω_1/n of the buyers, and therefore the seller's expected "security" profit is $(\omega_1 \pi_m)/n$.

Next we determine the probability that a buyer will consider a purchase from a particular seller when the other sellers price according to the cumulative density function $F(p)$. First, we

9. The model described in section II assumes simultaneous decision making in a one-shot environment, two assumptions that are unrealistic in the naturally occurring electronic economy. As we are conducting a heuristic experiment in the vein discussed by Smith (1982), we choose to implement the pricing game in an asynchronous repeated-play environment.

10. The same technology that allows a firm to determine the price that competitors quote to a customer when visited first could also be used to determine the competitor's entire price schedule; however, we believed that providing information on 12 other prices every three seconds (plus the four prices in one's own schedule) would be an overwhelming amount of information for the participants to handle. Moreover, this keeps the amount of price feedback similar across the Tracking and No Tracking treatments.

FIGURE 1
Symmetric Equilibrium Cumulative Distribution Functions



note that there are ${}_nC_i$ total groups of size i, and ${}_{n-1}C_{i-1}$ possible groups of size i to which a seller can belong. Thus, for a type i buyer, a particular seller will be one of a group sampled by a buyer with probability ${}_{n-1}C_{i-1}/{}_nC_i = i/n$. Second, that same seller will have a price less than $i-1$ rivals with probability $[1 - F(p)]^{i-1}$. Last, recall that with probability ω_i the buyers are of type i. Hence, the overall probability that a buyer will consider a purchase from a particular seller is

$$\sum_{i=1}^{n}[i\omega_i[1 - F(p)]^{i-1}]/n.$$

In a mixed-strategy equilibrium, the firm must be indifferent over all possible pure strategies in the support; hence, equating the security profit and the expected profit at a price p yields the equation that determines the equilibrium cumulative distribution functions:

$$(1) \qquad [(\bar{v} - p)/(\bar{v} - \underline{v})](p - c) \times \left(\sum_{i=1}^{n}[i\omega_i[1 - F(p)]^{i-1}]/n\right) = (\omega_1 \pi_m)/n.$$

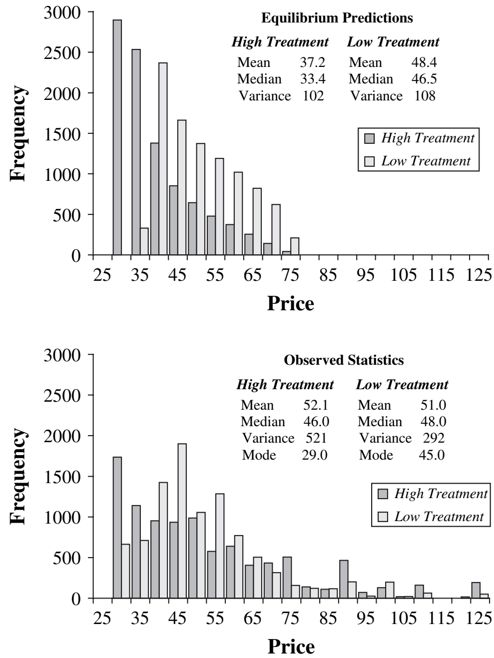
With nonzero weights on all $n \geq 3$ buyer types, no closed form solution for $F(p)$ exists. However, one can determine several key features of the resulting pattern of pricing behavior. First, if $\omega_1 = 1$, then each seller should charge a pure strategy equilibrium price of p_m because there is no competition among the sellers when the buyers do not search. However, if $\omega_1 = 0$, then the unique equilibrium is for each firm to charge a price of c, the Bertrand solution.[11] The upper bound of $F(p)$ is p_m whereas the lower bound of the support, p^*, is implicitly defined by setting $F(p) = 0$ in equation 1. Mass points do not exist in a symmetric mixed-strategy Nash equilibrium, as the probability of a tie would lead sellers to a lower price in an attempt to capture more expected profit than would sharing the profit at the common price.

Figure 1 displays the symmetric Nash equilibrium mixing distributions when sellers post a single price. Notice the clear separation in the predicted distribution of prices, though both share the same monopoly price p_m of 75. The median of the Low Search treatment is 46.5, approximately (and purposely not exactly) in the middle of the interval $(c, p_m) = (25, 75)$, but the median in the High Search treatment is 33.4, which is strictly below the lower bound of the support for the Low Search treatment.

Varian's framework, however, becomes intractable for a one-shot simultaneous move

11. Baye and Morgan (2000) discuss the conditions under which the only Nash equilibrium of a price setting game is the Bertrand solution.

FIGURE 2
Histogram of Low Search and High Search Posted Prices with No Tracking (Last 600 Periods)



version of the game in the Tracking treatment (and hence is the primary motivation for this experiment).

III. EXPERIMENTAL RESULTS

For each of the 19,200 simulated buyers (16 sessions × 1,200 periods/session × 1 buyer/period) in our experiment, our data include the price schedule employed by each seller, the random draw of the buyer's value, the buyer's type, and the order in which the buyer visited the sellers. The data permit us to compare posted price schedules, buyers' best-quoted prices, seller profitability, and the distribution of total surplus for the four separate treatments: (No Tracking, Tracking) × (Low Search, High Search).

We report our results as a series of five findings. As a control for potential learning effects over the course of a session, our analysis focuses exclusively on the last half of each session, a total of 9,600 simulated buyers. We begin by comparing observed behavior to the game-theoretic prediction for the static model in the No Tracking environment. Even though our environment involves asynchronous moves, the static model Nash equilibrium serves as a basis of comparison for analyzing our results.[12]

Finding 1

When sellers set a single price for all buyer types because all buyers are untrackable, the resulting distribution of prices is a mean-preserving spread of the static symmetric Nash equilibrium mixing distribution when sellers are competing in the Low Search treatment. In contrast, the central tendencies of the price distribution for the No Tracking–High Search treatment are considerably greater than the static symmetric game-theoretic predictions.

Evidence

Figure 2 provides the qualitative support and Table 2, the quantitative support. The median and mean of all prices aggregated across all subjects and sessions in the No

12. Deck and Wilson (2003), Davis and Wilson (2000), and Kruse et al. (1994) have found that the central moment of the static Nash equilibrium mixed strategy captures behavior in a repeated pricing game quite well.

TABLE 2
Estimates of the Linear Mixed-Effects Model for the *No Tracking* Treatment
$Y_{ij} = \alpha + e_i + \beta_1 LowSearch_i + \varepsilon_{ij}$, where $e_i \sim N(0, \sigma_1^2), \varepsilon_{ij} \sim N(0, \sigma_{2,i}^2)$

Y	Estimate	Std. Error	Degrees of Freedom*	H_a	*t* Statistic	*p* Value
MedianP						
α	45.4	1.47	24	$\alpha \neq 33.4$*	8.19	0.0000
LowSearch	4.00	2.13	6	$\beta_1 > 0$	1.88	0.0548
				$\alpha + \beta_1 \neq 46.5$*	1.86	0.0757
MeanP						
α	52.6	3.48	24	$\alpha \neq 37.2$*	4.42	0.0002
LowSearch	−2.56	4.80	6	$\beta_1 > 0$	−0.53	0.6933
				$\alpha + \beta_1 \neq 48.4$*	0.50	0.6208

Note: The linear mixed-effects model for repeated measures treats each session as one degree of freedom with respect to the Low versus High treatments. Hence, the degrees of freedom for the estimates of this fixed effect is 6 = 8 sessions − 2 parameters. Each linear mixed-effects model is fit by maximum likelihood with 32 observations and 8 sessions. For brevity, the session random effects are not included in the table.

*Theoretical prediction.

Tracking–Low Search treatment are close to the static game-theoretic predictions. The Low Search static symmetric game-theoretic predictions for the mean and median are 48.4 and 46.5, respectively, whereas the observed mean and median are 51.0 and 48.0, respectively. However, the observed mean and median for the No Tracking–High Search treatment, 52.1, and 46.0, respectively, are considerably greater than the static predictions, 37.2 and 33.4, respectively. We employ a linear mixed-effects model for analyzing data with repeated measures as the basis for quantitative support.[13] The results from estimating this model for the median price (*MedianP*) and mean price (*MeanP*) of the No Tracking sessions are reported in Table 2. The treatment effect (Low Search versus High Search) is modeled as a zero-one fixed effect, whereas the sessions are modeled as random effects, e_i. Each observation is a seller's median or mean posted price for the last 600 buyers. We index sessions by $i = 1, \ldots, 8$ and sellers by $j = 1, 2, 3, 4$. Specifically, we estimate the model

$$Y_{ij} = \alpha + e_i + \beta_1 LowSearch_i + \varepsilon_{ij}, \tag{2}$$

where $e_i \sim N(0, \sigma_1^2)$, $\varepsilon_{ij} \sim N(0, \sigma_{2,i}^2)$and $Y =$ *MedianP* or *MeanP*. The linear mixed-effects model for repeated measures treats each session as one degree of freedom with respect to the treatment.[14]

The mean and median prices for the No Tracking–High Search treatment are significantly different from the static predictions (p values = 0.0002 and 0.0000, respectively). The mean price for the No Tracking–Low Search treatment ($\hat{\alpha} + \beta_1 = 50.0$) is not statistically different from theoretical prediction 48.4 (p value = 0.6208); however, at a 90% confidence level, the median No Tracking–Low Search price ($\hat{\alpha} + \hat{\beta}_1 = 49.4$) differs from the prediction of 46.5 (p value = 0.0757). As predicted by intuition and the static theory,[15] the No Tracking–Low Search treatment median price is greater than the No Tracking–High Search treatment median price (p value = 0.0548), but the means are not statistically different (p value = 0.6933). ■

Finding 1 suggests that the model of sales organizes behavior in such markets fairly well for the No Tracking–Low Search treatment, but sellers in the No Tracking–High Search treatment clearly price less competitively than what the static model predicts. Figure 2 illustrates that the Search variable shifts the

13. See Longford (1993) for a description of this technique that is commonly employed in experimental sciences. It is well known that dummy variables such as *LowSearch* are not identifiable in a model with a full set of session fixed effects, because α and *LowSearch* are a perfect linear combination of the eight session fixed effects.

14. Hence, with two parameters, the degrees of freedom for the estimate of the *Search* treatment fixed effect are 6 = 8 sessions – 2 parameters. We also accommodate sessionwise heteroskedastic errors when estimating the model via maximum likelihood.

15. Morgan and Sefton (2000) show that in Varian's model of sales (1980), an increase in the number of uninformed consumers raises informed customers' expected price.

distribution of posted prices. From the No Tracking–High Search to the No Tracking–Low Search treatment, the distribution of prices shifts distinctly to the right, with the mode shifting from 29 to 45, even though it is a mean-preserving shift. The means are the same because the mode for the No Tracking–High Search treatment of 29 is offset by increased frequency of prices greater than or equal to the monopoly price, 75: 19% of the No Tracking–High Search treatment prices are in the 75 or greater range, whereas just 10% of the No Tracking–Low Search treatment prices are in this same range. One hypothesis is that the extremely competitive environment of the No Tracking–High Search treatment leads to more signaling than does the less competitive environment of the No Tracking–Low Search. By setting higher prices, subjects can signal a willingness to raise prices. Such signaling is less costly in the more competitive environment due to the lower expected profit margin. If subjects are signaling higher prices for the purpose of mitigating the competitive pressure of comparison shopping, then the signaling is apparently quite effective. Table 1 reports that the sellers in the No Tracking–High Search treatment earned \$10.75 on average as compared to the \$6.25 predicted by the static theory. The price signals in the High Search treatment appear to be an intelligent response to a highly competitive environment. It is this signaling that prevents a statistical separation of the results in Table 2 and Figure 2. Without the small tails above the monopoly price of 75, the means and medians of these two treatments would be quite different, as is graphically evident in the histograms in Figure 2. The observed distributions appear to shift as the static predictions shift, but the quantitative effect in aggregate and across-buyer types is minor (see findings 2a and 2b). Morgan et al. (forthcoming) similarly find that increasing the proportion of informed consumers decreases the prices paid by informed and uninformed customers. Given our asynchronous laboratory environment, we would expect, a priori, less of a pronounced conformity with Varian's predictions. What is interesting is that the differing levels of consumer search conform differently with his model.

Our next four findings examine the impact of buyer tracking with the No Tracking environments serving as a baseline. Finding 2 pertains to the prices quoted to buyers. Because the sellers in the Tracking treatments choose a price schedule containing up to four different prices depending on the order in which they are visited, we use the metric of the best-quoted price to compare prices across the Tracking and No Tracking treatments. Each buyer visits $n_s = 1, 2, 3$, or 4 sellers dependent on type, receiving a quoted price potentially dependent on the order visited. The best-quoted price is the minimum price of the n_s prices that the buyer observes.[16] For ease of exposition, we break this finding into four parts.

Finding 2a

Buyers, when aggregated across types, do not receive statistically different mean best-quoted prices from any of the primary or interaction treatment effects.

Evidence

Table 3 reports the results of a mixed-effects model to determine the primary and interaction effects of the treatments on the best-quoted prices for the 9,600 best-quoted prices (16 sessions $\times$ 600 buyers/session). The treatment and interaction effects (No Tracking versus Tracking, Low Search versus High Search) are modeled as fixed effects, whereas the sessions are modeled as random effects, e_i. The dependent variable for each observation is the best-quoted price for the last 600 buyers in a session. Sessions are indexed by $i = 1, \ldots, 16$ and buyers by $j = 1, \ldots, 600$. Specifically, we estimate the model

$$(3) \quad BestQuotedPrice_{ij} = \alpha + e_i + \beta_1 LowSearch_i + \beta_2 Tracking_i + \beta_3 Tracking_i \times LowSearch_i + \varepsilon_{ij},$$

where $e_i \sim N(0, \sigma_1^2), \varepsilon_{ij} \sim N(0, \sigma_{2,i}^2)$.

Because all of the primary and interaction effects are statistically insignificant at conventional levels of significance, we conclude that, overall, buyers receive the same best-quoted price regardless of the level of search or the sellers' ability to track buyers. ■

Initially, finding 2a may seem counterintuitive; buyers do not receive lower best-quoted

16. The analysis of transaction prices closely resembles the results reported for best-quoted prices and is hence not reported. A transaction did not occur if the best-quoted price exceeded the buyer's reservation value.

TABLE 3
Estimates of the Linear Mixed-Effects Model
$Y_{ij} = \alpha + e_i + \beta_1 LowSearch_i + \beta_2 Tracking_i + \beta_3 Tracking_i \times LowSearch_i + \varepsilon_{ij}$,
where $e_i \sim N(0, \sigma_1^2), \varepsilon_{ij} \sim N(0, \sigma_{2,i}^2)$.

Y	Estimate	Std. Error	Degrees of Freedom*	H_a	*t* Statistic	*p* Value
BestQuotedPrice[a]						
α	43.6	2.99	9,584	$\alpha \neq 37.2$	2.13	0.0333
LowSearch	4.30	4.22	12	$\beta_1 > 0$	1.01	0.1642
Tracking	−7.06	4.22	12	$\beta_2 \neq 0$	−1.67	0.1202
Tracking × *LowSearch*	8.36	5.97	12	$\beta_3 \neq 0$	1.40	0.1868
				$\alpha + \beta_1 \neq 48.4$	0.18	0.8545
SellerProfit[b]						
α	1763	207.4	48	$\alpha \neq 937.5$*	3.98	0.0002
LowSearch	547.1	285.9	12	$\beta_1 > 0$	1.91	0.0399
Tracking	−663.8	289.5	12	$\beta_2 \neq 0$	−2.29	0.0407
Tracking × *LowSearch*	737.1	404.2	12	$\beta_3 \neq 0$	1.82	0.0932
				$\alpha + \beta_1 \neq 2287.5$*	0.12	0.9083
				$\beta_2 + \beta_3 \neq 0$	0.26	0.7994
TotalSurplus[b]						
α	6901	565.2	48	$\alpha \neq 0$	12.2	0.0000
LowSearch	269.2	774.3	12	$\beta_1 < 0$	0.34	0.6330
Tracking	513.4	974.2	12	$\beta_2 \neq 0$	0.53	0.6078
Tracking × *LowSearch*	−760.9	1140.6	12	$\beta_3 \neq 0$	−0.66	0.5173
ConsumerSurplus%[b]						
α	73.8	1.82	48	$\alpha \neq 0$	40.6	0.0000
LowSearch	−10.9	2.52	12	$\beta_1 \neq 0$	−4.32	0.0010
Tracking	10.4	2.26	12	$\beta_2 \neq 0$	4.60	0.0006
Tracking × *LowSearch*	−11.4	3.85	12	$\beta_3 \neq 0$	−2.95	0.0119
				$\beta_2 + \beta_3 \neq 0$	−0.32	0.7565

Note: The linear mixed-effects model for repeated measures treats each session as one degree of freedom with respect to the treatments in the 2 × 2 design: Low, Tracking, and Low × Tracking variables. Hence, the degrees of freedom for the estimates of these fixed effects are 12 = 16 sessions – 4 parameters. The linear mixed-effects model is fit by maximum likelihood with 16 groups. For brevity, the session random effects are not included in the table.

*Theoretical prediction.

[a]9,600 observations.

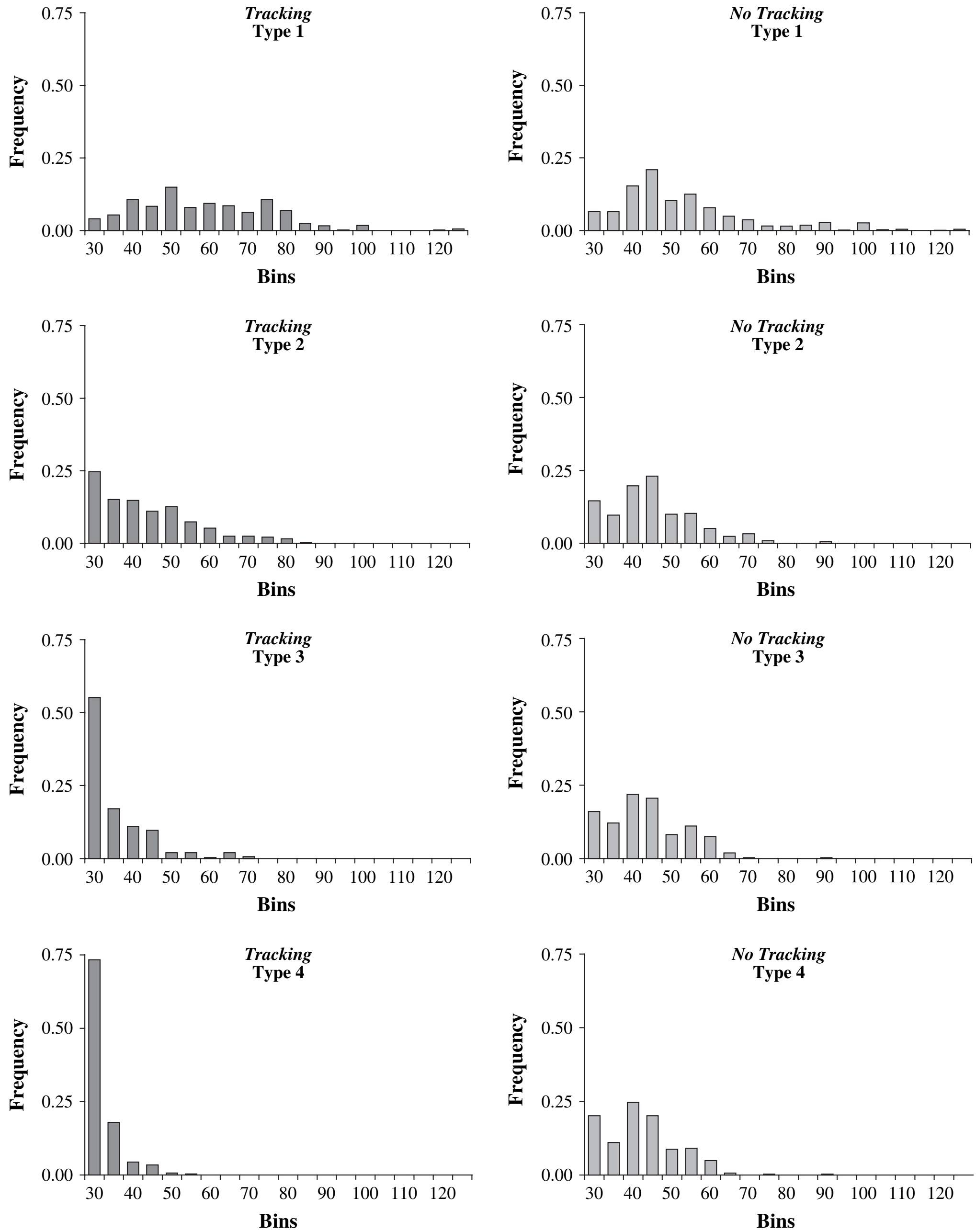
[b]64 observations.

prices in more competitive environments. However, finding 2a is based on prices aggregated across buyer types. As the remainder of finding 2 demonstrates, a shopper's search pattern significantly affects the best-quoted price. The insignificance of the market characteristics at the aggregate level is due to offsetting price movements across buyer types.

Having established that the treatments do not affect the best-quoted prices that the buyers in aggregate receive, we focus now on how the degree of buyer search affects best-quoted prices under the various treatments. For quantitative support, we add the following variables to the model in Table 3: the buyer's type (type m) and the interaction of the buyer types with both treatment variables. We first consider the primary effect of buyer search without tracking. This is largely a calibration result, because buyers purchase from the minimum price of the i sellers visited; hence, a greater search will lead to a lower best-quoted price in expectation.[17]

17. It is not necessarily the case that increased buyer search will lead to lower prices in the tracking environment, as sellers could use pricing distributions for comparison shoppers that are greater than, in a first-order stochastic dominance sense, the distribution employed for buyers making an initial inquiry. Recall that the agents are allowed to make decisions in an unrestricted action space. Finding 2b is thus a calibration, as it identifies a baseline by which to evaluate the treatment effect of tracking on consumers who search.

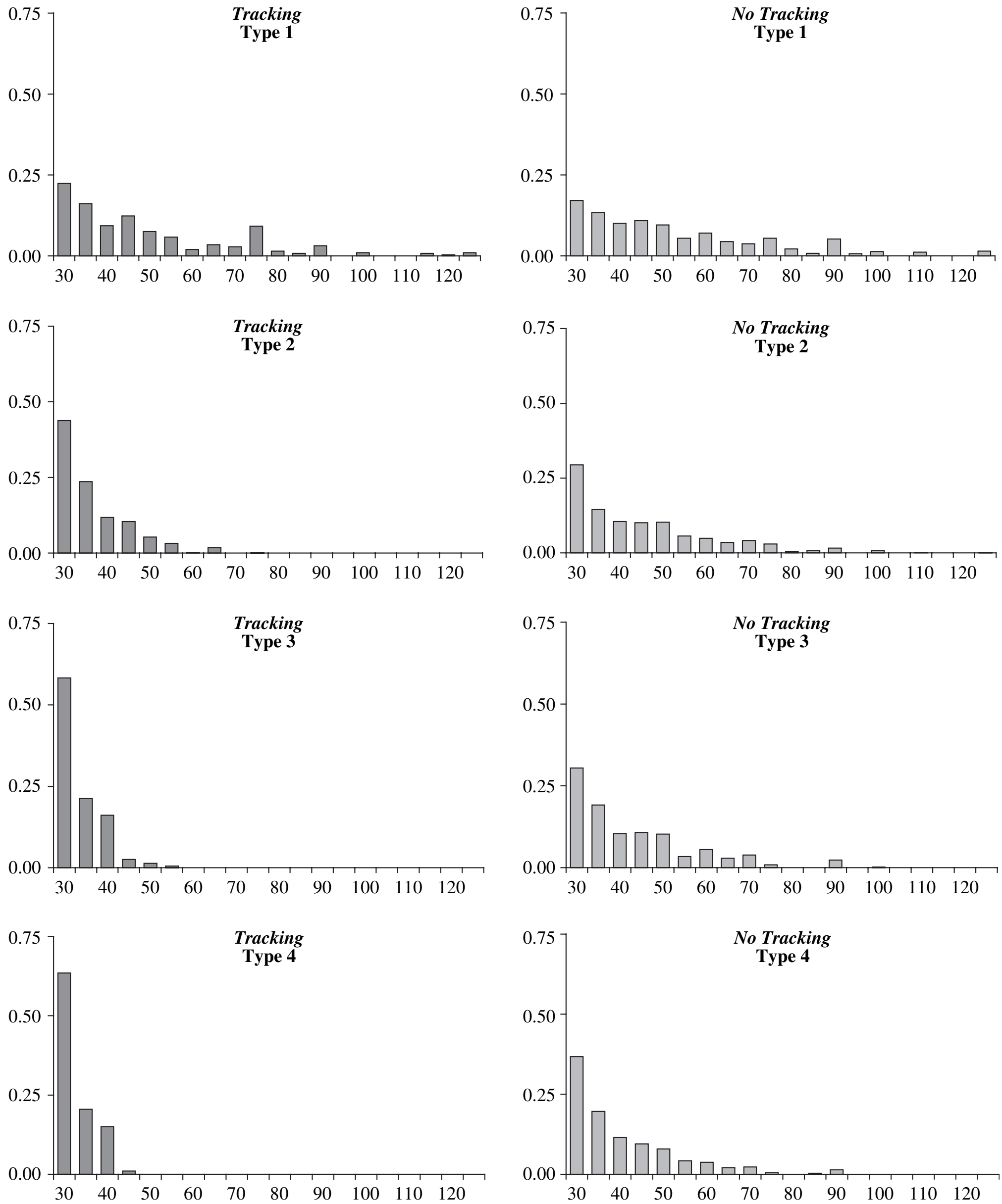
FIGURE 3
Density of Low Search Best-Quoted Prices (Last 600 Periods)



When buyers cannot be tracked, buyers that visit fewer sellers receive higher best-quoted prices than do more informed customers. Figures 3 and 4 report the observed densities of best-quoted prices by treatment and buyer type and depict the story that the linear mixed-effects model reports statistically in Table 4.

The type 4 buyers serve as the baseline type of buyer, and No Tracking–High Search serves as the baseline treatment in Table 4. A priori, in the No Tracking treatments we

FIGURE 4
Density of High Search Best-Quoted Prices (Last 600 Periods)



anticipate that the expected values of the order statistics for the price distribution decrease as the number of seller visits increase. The right panels of Figures 3 and 4 illustrate that the densities shift to the left as the number of sellers visited by the buyer increases. This shift is particularly noticeable in the No Tracking–High Search treatment. Statistically, we find that the point estimates of the type variables increase as the buyers search less ($\hat{\delta}_3 = 1.64$, $\hat{\delta}_2 = 3.72$, and $\hat{\delta}_1 = 12.1$) and that all of these estimates are highly statistically significant (p values $= 0.0257$, 0.0000, and 0.0000, respectively). Furthermore, with a 95% confidence interval (10.4, 13.8), the type 1 term is significantly greater than the type 2 and type 3 terms.

TABLE 4
Estimates of the Linear Mixed-Effects Model for the Best-Quoted Prices

$$\begin{aligned} BestQuotedPrice_{ij} = {} & \alpha + e_i + \beta_1 LowSearch_i + \beta_2 Tracking_i + \beta_3 Tracking_i \times LowSearch_i \\ & + \sum_{m=1}^{3} \delta_m (Type\ m)_j + \sum_{m=1}^{3} \phi_m (Type\ m)_j \times LowSearch_i \\ & + \sum_{m=1}^{3} \phi_m (Type\ m)_j \times Tracking_i + \sum_{m=1}^{3} \gamma_m (Type\ m)_j \\ & \times Tracking_i \times LowSearch_i + \varepsilon_{ij}, \end{aligned}$$

where $e_i \sim N(0, \sigma_1^2), \varepsilon_{ij} \sim N(0, \sigma_{2,i}^2)$

BestQuotedPrices	Estimate	Std. Error	Degrees of Freedom*	H_a	t Statistic	p Value
α	**39.1**	**3.08**	9572	$\alpha \neq 0$	**12.7**	**0.0000**
Low	2.05	4.37	12	$\beta_1 > 0$	0.47	0.3236
Tracking	**−8.61**	**4.33**	12	$\beta_2 \neq 0$	**−1.99**	**0.0700**
Tracking × LowSearch	−2.49	6.16	12	$\beta_3 \neq 0$	−0.40	0.6937
Type 1	**12.1**	**0.83**	9572	$\delta_1 > 0$	**14.5**	**0.0000**
Type 2	**3.72**	**0.83**	9572	$\delta_2 > 0$	**4.47**	**0.0000**
Type 3	**1.64**	**0.84**	9572	$\delta_3 > 0$	**1.95**	**0.0257**
LowSearch × Type 1	−1.84	1.16	9572	$\phi_1 \neq 0$	−1.58	0.1134
LowSearch × Type 2	−1.35	1.32	9572	$\phi_2 \neq 0$	−1.02	0.3071
LowSearch × Type 3	−0.53	1.34	9572	$\phi_3 \neq 0$	−0.40	0.6876
Tracking × Type 1	**5.71**	**1.02**	9572	$\varphi_1 \neq 0$	**5.59**	**0.0000**
Tracking × Type 2	1.25	1.02	9572	$\phi_2 \neq 0$	1.26	0.2166
Tracking × Type 3	−0.41	1.03	9572	$\phi_3 \neq 0$	−0.40	0.6910
Tracking × LowSearch × Type 1	**11.6**	**1.51**	9572	$\gamma_1 \neq 0$	**7.70**	**0.0000**
Tracking × LowSearch × Type 2	**8.26**	**1.74**	9572	$\gamma_2 \neq 0$	**4.76**	**0.0000**
Tracking × LowSearch × Type 3	**3.23**	**1.77**	9572	$\gamma_3 \neq 0$	**1.83**	**0.0671**
	H_0:$\beta_1 = \varphi_1 = \varphi_2 = \varphi_3 = 0$		LR = 3.00	0.5575		
	H_0:$\beta_3 = \gamma_1 = \gamma_2 = \gamma_3 = 0$		LR = 67.73	0.0000		

Note: 9,600 observations.
LR = likelihood ratio.

Finding 2b

With No Tracking, different buyer types do not receive statistically different best-quoted prices in the Low and High Search treatments, but with Tracking, there is a statistical difference.

Evidence

Using a likelihood ratio test of the estimates in Table 4, we test the joint hypothesis that $\beta_1 = \varphi_1 = \varphi_2 = \varphi_3 = 0$ against the alternative that at least one buyer type with No Tracking receives different prices in the Low Search treatment than in the High Search treatment. Table 4 reports that we cannot reject the null hypothesis that buyers of all types receive the same best-quoted prices (p value = 0.5575). In contrast, we can reject the corresponding null hypothesis in Tracking treatment, namely, that $\beta_3 = \gamma_1 = \gamma_2 = \gamma_3 = 0$ (p value = 0.0000). ■

We now turn our attention to comparing the No Tracking and Tracking treatments holding the search variable constant.

Finding 2c

Type 4 buyers in the Tracking–High Search treatment receive statistically lower best-quoted prices than do type 4 buyers who are not tracked in the High Search environment. Type 1 buyers in the Tracking–High Search treatment, however, do not receive this same price break.

Evidence

We refer again to the results reported in Table 4. Because the Tracking × Type *m* variables control for the specified buyer types, the Tracking term captures the effect that type 4 buyers in Tracking–High Search markets receive a lower best-quoted price ($\beta_2 = -8.61$, p value $= 0.0700$). However, not all buyer types received this price break. The Tracking × Type 1 term is highly significant (p value $=$ 0.0000), raising the best-quoted price for Tracking–High Search type 1 buyers by 5.71 experimental dollars. ■

Finding 2d

In addition to the effects discussed, buyers that visit fewer sellers in the Low Search–Tracking treatment receive additionally higher quoted prices.

Evidence

As reported in Table 4, all three Low × Type *m* interaction terms are statistically significant, raising the best-quoted-price by $\hat{\gamma}_3 = 3.23$, $\hat{\gamma}_2 = 8.26$, and $\hat{\gamma}_1 = 11.6$ (p values $= 0.0600$, 0.0000, and 0.0000, respectively). Figure 3 illustrates the dramatic leftward shift in the density of best-quoted prices as the buyer searches more in Tracking–Low Search treatment. The estimated best-quoted price for a type 1 buyer in the Tracking–Low Search treatment is 57.6 ($= \hat{\alpha} = \hat{\beta}_1 + \hat{\beta}_2 + \hat{\beta}_3 + \hat{\delta}_1 + \hat{\varphi}_1 + \hat{\varphi}_1 + \hat{\gamma}_1$), whereas a type 4 buyer receives a price of 30.1 ($= \hat{\alpha} + \hat{\beta}_1 + \hat{\beta}_2 + \hat{\beta}_3$). Visiting three other sellers reduces a buyer's price by 47.7%. ■

In sum, finding 2 shows that buyers in aggregate do not receive higher best-quoted prices when sellers have the ability to track customers and charge sequentially different prices. One buyer segment, buyers who do not search but are in an environment where they can be tracked, receives higher prices than their counterparts who are in an environment without seller tracking. However, as Figures 3 and 4 markedly illustrate, this effect is offset because buyers who search more receive correspondingly lower prices in an environment in which they are tracked.

Our next three findings consider how the treatments influence seller profit, total surplus, and the distribution of the surplus, respectively.

Finding 3

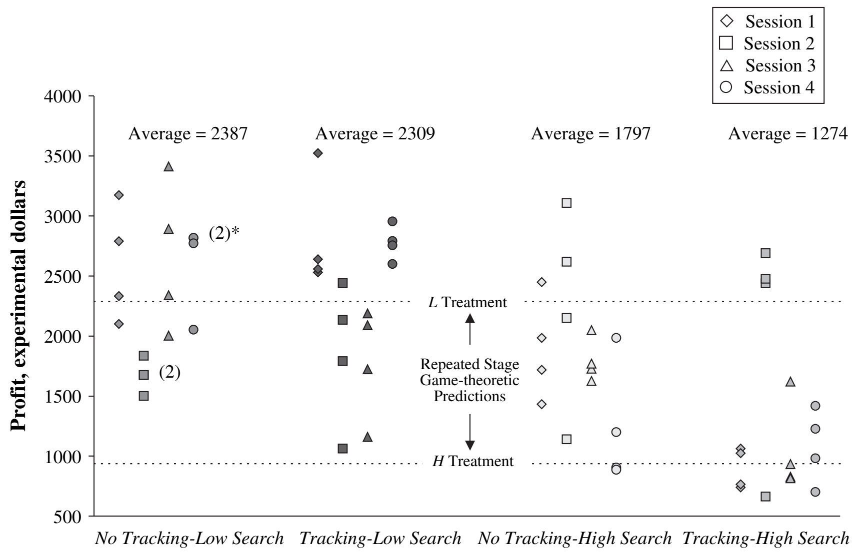
The ability to track buyers does not affect seller profit in the Low Search treatment, but the Tracking treatment does reduce seller profit in the High Search treatment.

Evidence

Figure 5 and Table 3 provide the relevant support for this finding. Figure 5 displays the individual seller profit in experimental dollars for the last 600 buyers in the seller's market. The first two clusters of markers show that the ability to track customers and price sequentially has no discernable influence on the profitability of the sellers under Low Search. In comparing across the four treatment cells, notice that profits in the Low Search treatments are greater than profits in the High Search treatments as predicted by the static model. It is also apparent that the Tracking–High Search profits are markedly lower (29% on average) than the No Tracking–High Search treatment. The dependent variable *SellerProfit* in Table 3 is the individual profit for each seller in experimental dollars. The estimates of the mixed-effects model indicate that Low Search significantly increases seller profit by 547 experimental dollars (p value $= 0.0399$). With the No Tracking–High Search treatment serving as the baseline, the inclusion of tracking reduces seller profits by 664 experimental dollars (p value $= 0.0407$). The interaction effect, Tracking × Low Search, is positive and marginally significant and thus offsets the Tracking effect in the Low Search sessions ($\hat{\beta}_2 + \hat{\beta}_3 = 73.3$, p value $= 0.7994$). From this we can conclude that given the Low Search treatment, Tracking has no affect on seller profits. ■

Two reasons explain why seller profits fall with buyer tracking in the High Search environment but not in Low Search, even though we report in finding 2a that the primary and interaction effects are mean preserving. First, even though tracking raises type 1 buyer prices and lowers type 4 buyer prices (findings 2b and 2d), there are more type 4 buyers (25% versus 13%) and fewer type 1 buyers (25% versus

FIGURE 5
Individual Profits by Session and Treatment



*Number of graphically indistinguishable data points.

61%) with High Search than with Low Search. Second, prices are relatively higher for the type 1 buyers who are tracked in the Low Search environment (finding 2c), so the profit margin from type 1 buyers is higher.

Finding 4

Total surplus generated does not statistically differ across treatments.

Evidence

To be parsimonious, we conducted the same analysis with total surplus as the dependent variable as we did for seller profit. Total surplus is calculated as the sum of seller profit and consumer surplus generated by each seller in the experiment. As Table 3 reports, all three primary and interaction effects are insignificant; that is, the level of total surplus is unaffected by the treatment variables. ■

The intuition for this result can also be found in the price distributions. For the Search treatment variable, Figure 2 illustrates that the mode of the High Search distribution is shifted to the left relative to the Low Search distribution, which would increase the total number of trades and total surplus in the High Search treatment. However, the greater weight of prices in the right tail of the High Search treatment tends to decrease total surplus relative to the Low Search treatment. Finding 4 indicates that these effects offset each other. Similarly for the Tracking treatment variable, the increase in total surplus due to lower prices for type 4 buyers is offset by the higher prices incurred by type 1 buyers.

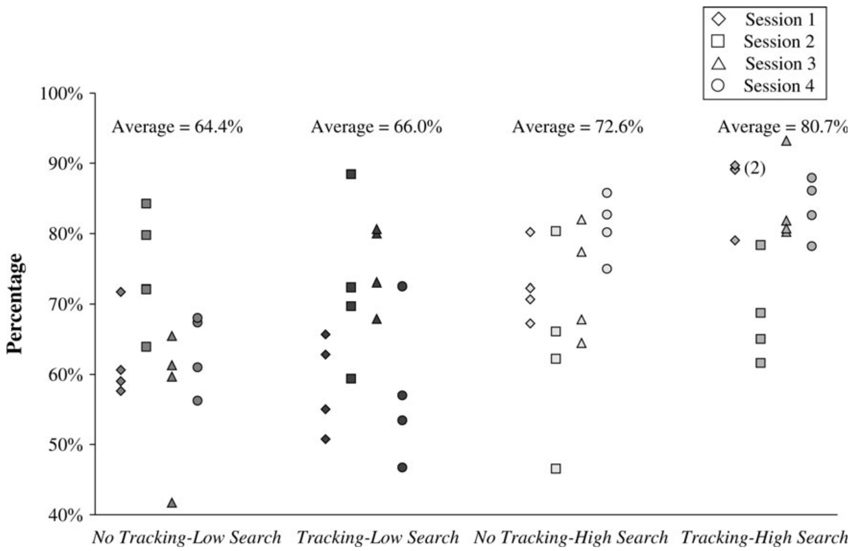
Finding 5

The ability to track customers does not alter consumer surplus as a percentage of the total surplus when search is low. However, the Tracking–High Search treatment increases consumer surplus as a percentage of total surplus relative to the No Tracking–High Search treatment.

Evidence

Figure 6 and Table 3 provide the relevant support for this finding. By seller, Figure 6 displays the consumer surplus as a percentage of total surplus generated for the last 600 buyers of a session. The first two clusters of markers show that the ability to track customers has no discernable influence on consumer surplus as

FIGURE 6
Consumer Surplus as a Percentage of Total Surplus Attributed to Each Seller



a percentage of total surplus when search is low. Comparing all four sets of markers, both Low Search consumer surplus percentages are lower than in the High Search treatments. It is also apparent that the Tracking–High Search consumer surplus percentages are noticeably higher (8.1 percentage points on average) than the No Tracking–High Search treatment. The estimates of the mixed-effects model indicate that Low Search significantly lowers the consumers' share of the surplus by 10.9 percentage points in the absence of tracking (p value $= 0.0010$). With the No Tracking–High Search treatment serving as the baseline, tracking increases the consumer surplus as a percentage of total surplus by 10.4 percentage points, holding the level of search constant (p value $= 0.0006$). The interaction effect, Tracking $\times$ Low Search offsets the tracking effect in the Tracking–Low Search sessions ($\hat{\beta}_2 + \hat{\beta}_3 = -1.0$, p value $= 0.7565$), indicating that given the Low Search treatment, tracking has no affect on consumer surplus as a percentage of total surplus. ■

IV. CONCLUSION

From an analytical standpoint, the features of an electronic marketplace can be fundamentally different from those used in more traditional markets. As the major contributions on buyer search theory have assumed, a "brick and mortar" firm cannot readily distinguish the informed customers from the uninformed. Such a firm posts a single price, independent of the degree to which customers are informed about competitors' prices. In contrast, the interactive technologies of electronic markets allow a firm to identify the extent to which customers may be informed about competitors' prices, and with this information, the firm can then tailor a price for each individual customer.

Using controlled experimental markets, we investigate how the ability to track customers and price discriminate accordingly affects the prices that buyers receive and the associated profits of the sellers. For a baseline, we study markets in which buyers cannot be tracked. We find that the sellers' ability to track customers and implement search history–based pricing does not harm customers as a group. However, tracking-based pricing affects distinct customer segments differently. In particular, buyers who could be tracked and who search several sellers receive lower prices than did buyers who could not be tracked but who also search several sellers. In contrast, uninformed buyers who could be tracked receive higher prices than did uninformed buyers who could not be tracked. Thus, we find that buyers on average do not receive higher prices in a tracking-based pricing environment, because the buyers who searched more receive lower prices and buyers who search less receive higher prices. Furthermore, aggregate consumer surplus rises and seller profits fall when 75% of the buyers search at least two sellers as opposed to when only 39% of the buyers search in the tracking environment.

The exploratory nature of this article raises both theoretical and empirical questions for future work. First, how would customer search patterns react to tracking by sellers? Customers may not become price searchers en masse, because the more that buyers as a group search, the lower the prices for the uninformed customers; that is, there is an opportunity to free-ride off the searching of other buyers. Second, the evolution of customer search patterns may affect the degree to which sellers adopt tracking technology. Also, incorporating the ability afforded by the Internet to closely monitor individual rivals, firms may be able to further tailor customer prices based on the specific firms that a potential customer has visited. Hence, this study really provides only an initial foray into the effects of customer tracking in an electronic marketplace.

REFERENCES

Baye, M., and J. Morgan. "Revisiting Bertrand's Competition: Paradox Lost or Paradox Found?" Mimeo, Indiana University, 1996.

Braverman, A. "Consumer Search and Alternative Market Equilibria." *Review of Economic Studies*, 47, 1980, 487–502.

Burdett, K., and K. Judd. "Equilibrium Price Dispersion." *Econometrica*, 51, 1983, 955–70.

Carlson, J., and R. P. McAfee. "Discrete Equilibrium Price Dispersion." *Journal of Political Economy*, 91, 1983, 480–93.

Cason, T., and D. Friedman. "Buyer Search and Price Dispersion: A Laboratory Study." *Journal of Economic Theory*, 112(3), 2003, 232–60.

Davis, D., and C. Holt. "Consumer Search Costs and Market Performance." *Economic Inquiry*, 34, 1996, 133–51.

Davis, D., and B. J. Wilson. "Firm-Specific Cost Savings and Market Power." *Economic Theory*, 16(3), 2000, 545–65.

Deck, C. A., and B. J. Wilson. "Automated Pricing Rules in Electronic Posted Offer Markets." *Economic Inquiry*, 41, 2003, 208–23.

Kruse, J., S. Rassenti, S. Reynolds, and V. Smith. "Bertrand-Edgeworth Competition in Experimental Markets." *Econometrica*, 62, 1994, 343–71.

Longford, N. T. *Random Coefficient Models*. New York: Oxford University Press, 1993.

Morgan, J., H. Orzen, and M. Sefton. "An Experimental Study of Price Dispersion." *Games and Economic Behavior*, forthcoming.

Morgan, J., and M. Sefton. "A Model of Sales: Comment." Mimeo, 2000.

Olsen, S. "New Tools Hatch for Sniffing Out Web Bugs" *CNET News.com*, 5 March 2001, http://news.cnet.com/news/0-1005-200-5008849.html.

Rob, R. "Equilibrium Price Distributions." *Review of Economic Studies*, 52, 1985, 457–504.

Rodgers, A. "Checking for Bugs." *Smart Computing*, 12(7), 2001, 92–95, http://www.smartcomputing.com/editorial/article.asp?article=articles/2001/s1207/35s07/35s07.asp.

Salop, S., and J. Stiglitz. "Bargains and Ripoffs: A Model of Monopolistically Competitive Price Dispersion." *Review of Economic Studies*, 44, 1977, 493–510.

Smith, V. "Microeconomics as an Experimental Science." *American Economic Review*, 72(5), 1982, 923–55.

Stahl, D. "Oligopolistic Pricing with Sequential Consumer Search." *American Economic Review*, 79, 1989, 700–712.

Stiglitz, J. "Equilibrium in Product Markets with Imperfect Information." *American Economic Review*, 69, 1979, 339–45.

———. "Competition and the Number of Firms in a Market: Are Duopolies More Competitive Than Atomistic Markets?" *Journal of Political Economy*, 95, 1987, 1041–61.

Streitfeld, D. "On the Web, Price Tags Blues." *Washington Post*, 27 September 2000, Sec. A, p. 1.

Sullivan, B. "Pop-Ups Prove Profitable, Persistent," *MSNBC.com*, 20 November 2003, http://www.msnbc.com/news/995180.asp.

Varian, H. "A Model of Sales." *American Economic Review*, 70(4), 1980, 651–59.

Weber, T. "The Man Who Baked the First Web Cookies Chews Over Their Fate," *Wall Street Journal*, 28 February 2000, Sec. B, p. 1.

Wilde, L., and A. Schwartz. "Equilibrium Comparison Shopping." *Review of Economic Studies*, 46, 1979, 543–53.

Wolverton, T. "MP3 Player 'Sale' Exposes Amazon's Flexible Prices." *CNET News.com*, 17 May 2000a, http://news.cnet.com/news/0-1007-200-1889854.html.

———. "Now Showing: Random DVD Prices on Amazon," *CNET News.com*, 5 September 2000b, http://news.cnet.com/news/0-1007-200-2703210.html.