When Providing Competitive Information to Your Own Customers Encourages Trust, Consideration, and Sales

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Abstract

A US automaker ("USAM") launched excellent vehicles but many consumers would not consider the vehicles. USAM sought to build trust and encourage consideration and sales by offering unbiased competitive information. USAM tested four communications strategies: experiential (competitive test drives), product information (competitive brochures), word-of-mouth (community forums), and online advisors. The year-1 test was a six-month, random-assignment 2 x 2 x 2 x 2 field experiment. The year-2 test simulated a national rollout comparing an advertising-plus-website opt-in strategy to a randomly-assigned control. We use raw analyses, conditional-logit models, and continuous-time Markov models to evaluate the experiments and quasi-experiments. All analyses suggest that competitive information treatments are mediated through trust and that experiential strategies are the most effective. Despite prior beliefs, trust signaling did not overcome negative information and opt-in was not effective. USAM's tests identified cost-effective marketing strategies which were implemented. We interpret the data to suggest generalizable insights.

Keywords: *Communications, Competitive Information, Consideration, Continuous-time Markov Processes, Electronic Marketing, Field Experiments, Information Search, Mediation, Quasi-experiments, Trust, Trust Signaling*

INTRODUCTION AND MOTIVATION

In 2003-2005 a US automaker ("USAM") launched vehicles that it believed were more reliable and satisfied consumer needs better than key competitors. Despite better vehicles USAM failed to gain market share because over half of the consumers in the US did not trust USAM sufficiently to invest the time and energy to evaluate USAM's vehicles. Roughly half of all US consumers (and almost 2/3^{rds} in California) would not even consider USAM. By not considering USAM (and not evaluating USAM's vehicles), consumers never learned that a USAM brand was among the very top brands in a recent J. D. Power's vehicle dependability ranking, was a top US brand in a recent *Consumer Reports*, and was a best-selling brand abroad.

USAM's situation provides a unique opportunity to test the theory of providing competitive information to enhance trust, consideration, and sales. USAM was producing improved products and would likely benefit from direct comparisons with competitive products. Because many consumers did not trust USAM, there was considerable upside if a competitive-information strategy was effective. If a competitive-information strategy was implemented well yet failed in USAM's 2003-2005 situation, then we might infer that competitive-information strategies are less likely to succeed when consumers already trust a firm or when the firm's products have not improved sufficiently.

Enhancing trust by providing competitive information might be applicable in many markets. If consumers reject a brand before considering it, they are unlikely to learn whether the brand meets their needs better than the brands they now consider. Because consumers often consider but a small fraction of the brands in a category (less than 10% in most markets, Hauser and Wernerfelt 1990), marketing actions, which encourage consumers to consider brands that they would not otherwise consider, can be key to profitable marketing. When products have recently

improved relative to competitive products, competitive comparisons become a potentially important tactic.

USAM's trust-based competitive-information strategy was based on the evaluation-cost model of consideration (Hauser and Wernerfelt 1990). For a rational consumer to consider a brand, the utility of choosing from the expanded consideration set (current set plus the newly added brand) must exceed the cost of considering that brand. To entice consumers to consider a brand that is not now considered, the manufacturer (USAM) must either convince consumers that the benefits are sufficient or must reduce the cost of consideration. Competitive information will be effective if it provides information to consumers that increases perceived benefits and reduces evaluation costs. However, enhanced consideration (and sales) might be mediated through trust. If so, and if the firm increases trust sufficiently, consumers might perceive that the benefit-vs.-cost tradeoff is sufficient to invest the time and effort to evaluate (consider) a brand. Conditioned on consideration, competitive information might also increase sales among considerers.

Although many USAM managers believed that competitive information was a viable strategy that would enhance consideration (and sales) by enhancing trust, a national rollout would require investing possibly hundreds of millions of dollars. USAM decided to first test the types of information that would be most effective. Prior to these tests, USAM's communication efforts focused on information about USAM's vehicles (not competitors' vehicles). The tests were necessary to evaluate the effectiveness and cost-efficiencies of alternative competitive-information strategies and to gather data to evaluate a national rollout.

In year 1 USAM used randomized experiments. Consumers were assigned to combinations of four competitive-information treatments (2 x 2 x 2 x 2 full-factorial experimental design), each representing a generic communications strategy. The treatments represented four ba-

sic strategies to make it easy (1) to experience USAM and competitive vehicles, (2) to get information about competitive vehicles (only USAM in year 1, competitors in year 2), (3) to get trusted word-of-mouth feedback about USAM and competitive vehicles, and (4) to get trusted and unbiased advice about which vehicles match consumers' needs. The year-1 experiments ran for six months to capture the dynamics inherent in automotive markets.

In year 2, USAM sought to test whether an advertising-plus-website opt-in strategy of competitive information would be cost-efficient in a national rollout. With an advertising-plus-website opt-in strategy, test-group consumers were allowed to opt-in to the treatments and control-group consumers were given no treatments. Assignments between groups were random. The year-2 treatments drew from the same generic communications strategies, but were updated based on USAM's year-1 experience. For example, year-2 brochures were electronic rather than direct-mail. As in year 1, data were collected monthly throughout the six-month test period.

In this paper we use USAM's data to explore whether and when competitive information encourages consumers to consider and purchase a brand that they would otherwise not have considered. We compare alternative types of information and explore whether effects are mediated through trust. We begin by summarizing the raw results of the year-1 randomized experiments. We then use simple (conditional-logit) models to explore direct and indirect effects including analyses that account for (1) heterogeneity in response due to prior ownership and demographics, (2) interactions with prior ownership, and (3) complementarities of information. The simple models account for trust, consideration, and purchase dynamics through lagged variables (stock models). To fully account for dynamics we then examine continuous-time Markov processes.

The three types of analyses (raw, logit, and Markov) yield complementary insights. They suggest that <u>experiential</u> competitive information (competitive test drives) is the most effective

treatment. Unbiased competitive brochures might also enhance trust, consideration, and purchase, although the evidence is not unambiguous. The lack of effects due to other treatments (word-of-mouth and unbiased advisors) may be due to USAM's relative competitive situation at the time. The competitive information in these treatments (word of mouth, advisors) are affected by negative information on USAM's past performance. In contrast, competitive test drives and brochures provide more positive information based primarily on USAM's current vehicles. All analyses agree that observed effects are likely mediated through trust.

USAM's experiments provide a proof-of-concept test which illustrates a setting where unbiased competitive information enhances trust, consideration, and purchase. USAM used the results of the experiments to devise and implement successful competitive-information communications strategies that continue today. Interpretation of the USAM's experiments, with suitable caveats, enable us to speculate on more-generalized insights.

ENCOURAGING CONSUMERS TO CONSIDER PRODUCTS

To earn consideration for not-yet-considered brands, a firm must either the increase expected benefits of evaluating the new brand or must reduce the costs of evaluation. But there is an inherent circular dilemma, in order to assess expected benefits consumers must evaluate the brand—exactly what they are not now doing. In essence, consumers must make an exploration vs. exploitation tradeoff to reduce uncertainty about brand attributes (Erdem and Keane 1996; Roberts and Urban 1988). In many cases consumers use trust as a mediator: "I trust Dell, therefore I will consider and evaluate Dell computers." This is especially true in automotive markets where, over many years of experience, word-of-mouth, and media accounts, consumers have developed trust, or lack thereof, in manufacturers. Each of USAM's four experimental treatments seeks to increase consideration (and sales) by providing information (perceived benefits), reduc-

ing evaluation costs, and building trust. We classify the actions by the generic competitive-information strategy they were designed to test.

Direct Experience with the Product (Competitive Test Drives)

Frequently-purchased brands often distribute trial samples to provide consumers with direct experience with their brand. The equivalent of direct experience in the automotive market is a test drive. USAM set up a test-drive experience at a California test track in which consumers could drive vehicles from Acura, BMW, Buick, Cadillac, Chevrolet, Chrysler, Dodge, Ford, Honda, Lexus, Lincoln, Mercedes, Pontiac, Saab, Saturn, Toyota, Volkswagen, and Volvo and do so without any sales pressure (Figure 1a). Competitive test drives clearly reduce evaluation costs. Competitive test drives improve perceived benefits when a brand is better than consumers perceive it to be or when the test drive reduces uncertainty sufficiently. Competitive test drives also signal that the firm believes in its brand and, thus, should be trusted (Urban 2004). See Erdem and Swait 1998 and Milgrom and Roberts 1986 for related signaling theories. Costs of competitive test drives were substantial—about $100 per consumer assigned to the treatment.

Insert Figure 1 about here.

Information about the Product (Customized Brochures, Competitive Brochures)

Brand communications (advertising, brochures, salesforce, etc.) provide information about a brand. Although brand-to-consumer communications may be less salient than experience, such communications can be effective. In automotive markets brochures are particularly relevant. In year 1, USAM developed direct-mail brochures customized to consumers' expressed interests. The glossy brochures provided targeted information on USAM (increased benefits) and lowered the cost of getting that information (Figure 1b). USAM hoped the personalization would increase trust. In year 2, the brochures were less customized and web-based; both USAM bro-

chures and competitive brochures were made available. USAM hoped that the information provided and the act of providing competitive information (in year 2) would increase trust.

Word-of-Mouth (Online Competitive Community Forums)

Consumers are more likely to trust their peers than to trust a manufacturer. When the information content is the same, word-of-mouth communication is more likely to affect trust than brand-to-consumer communications. Indeed, word-of-mouth communication is becoming more important with the advent of social media. Providing access to word-of-mouth might signal consumer trust, but there is also a risk. Brands have less control over word-of-mouth communication and risk negative comments. Negative word-of-mouth may undermine trust, especially because negative information is often disproportionally salient (see an experimental examples in Hauser, Urban, and Weinberg 1993).

At the time of the experiments USAM had recently introduced improved vehicles, but consumers' negative experiences with prior, lesser-quality vehicles were also salient. About 20% of the comments about USAM were observed to be negative. On the benefit side, USAM hoped that enabling word of mouth would signal trust and that the signal would overcome the negative comments. On the cost side, USAM's word-of-mouth implementation lowered evaluation costs.

USAM tested unbiased word-of-mouth using an online CommuniSpace^{TM} forum in which consumers could participate in over 30 dialogues about both USAM and competitive vehicles (Figure 1c). The community forum was repeated in year 2. In year 2 USAM launched improved vehicles and hoped for more-positive word-of-mouth.

Competitive Online Advisors

Information about products is widely available on the Internet. In the automotive market, websites such as Autotrader.com, Cars.com, ConsumerReports.org, Edmunds.com, Kelly Blue

Book (kbb.com), and TheAutoChannel.com compete to provide specifications, reviews, prices, and availabilities. Online information is probably less salient than direct experience or word-of-mouth and, like word-of-mouth, can be a two-edged sword if the online advisors do not recommend USAM. Nonetheless, online advisors match vehicles to consumer needs, lower evaluation costs, and could enhance trust.

USAM developed an online advisor that was co-branded with Kelley Blue Book and similar to that developed by Urban and Hauser (2004). See Figure 1d. In year 1, the online advisor recommended competitors' roughly 83% of the time. In year 2 the advisor was improved and re-branded, but was mostly similar to year 1. USAM hoped the positive signal provided by the advisor would enhance trust and that the signal would more than offset the tendency of the advisor to recommend competitors.

In summary, USAM chose four treatments that it believed had the potential to increase trust, consideration, and sales. The four treatments represented four generic strategies. USAM invested heavily in each treatment. It was by no means obvious to USAM at the time which, if any, of the treatments would work. By analyzing USAM's experiment we gain insight into which types of strategies, if implemented in favorable conditions, are effective. We also learn whether the trust signal can offset negative information about USAM. (We later address cost efficiency.)

DYNAMICS IN THE AUTOMOTIVE INDUSTRY

Consideration and Purchase Dynamics

Automotive purchasing decisions happen over months. A consumer might come to a competitive test drive in May, consider and seriously evaluate vehicles in June, and purchase in August. To capture these dynamics USAM measured consumers' behavioral states at the end of each of six monthly periods. Consumers had either (1) not yet considered USAM, (2) considered

USAM but not yet purchased USAM, or (3) considered and purchased a USAM vehicle. To avoid demand artifacts, consideration was measured with a drop-down menu in which consumers report which of 348 make-model combinations (USAM and non-USAM) they are now considering (or have purchased recently). USAM had many brands throughout the list so we expect few, if any, order effects. Even if there were absolute order effects, USAM's treatments are designed to affect changes in consideration. We do not expect order effects to be correlated with the treatments.

The purchase dependent variable was measured in the surveys. Because consumers might evaluate and reject a vehicle, they can report no consideration in period t even if they considered a USAM vehicle in period $t-1$. Once consumers purchase a USAM vehicle we assume they cannot un-consider and un-purchase USAM during the six-month observation period.

We begin with raw main-effect and conditional-logit analyses that analyze the flows on a period-by-period basis. We then examine a continuous-time Markov model that allows multiple, continuous flows during a period and which estimates parameters for all flows simultaneously.

Trust and Search Dynamics

Trust builds or declines over time. A consumer might experience a treatment in period t and, as a result, increase his or her trust in a brand. But the consumer may not trust the brand enough to consider it. Another treatment in period $t+1$ might increase trust further and be enough to encourage consideration. To capture this phenomenon, we model trust as a "stock" variable that increases or decreases over time as a result of treatments. (Trust might also decay.) Specifically, trust in period t is a convex combination of trust in period $\mathrm{t}-1$ and effects due to the treatments in period t. Trust was measured in each period using a five-item scale with items such as "I believe that this company is willing to assist and support me." or "Overall, this com-

pany has the ability to meet customer needs." The items exhibited high construct reliability: Cronbach's $\alpha = 0.95$.

USAM believed the effects of competitive information would be mediated through trust. For example, competitive test drives might cause consideration by first causing consumers to trust USAM. Later, the more trusting consumers would consider USAM. To test trust mediation we consider models that contain trust and contain treatments – both one- and two-stage models. To avoid confounding the effects, we allow treatments to drive trust but lagged trust to drive consideration and/or purchase. (Technically, we examine models with "instrumented" lagged trust to address potential joint influences in unobserved errors.)

Hauser, Urban and Weinberg (1993) examine consumers' information search for automobiles and find that the value of an information source sometimes depends upon whether consumers had previously experienced another information source. (Their data suggest some two-level interactions, but no three-level interactions.) We examine models that allow interactions.

Heterogeneity of Response Based on History or Demographics

Although the treatments were randomized, consumers with different purchase histories and different demographics (age and sex) might react differently (e.g., Anderson and Simester 2003, 2004; Simester, Hauser, Wernerfelt and Rust 2000). For example, consumers who now own USAM may base their trust, consideration, or purchase on their prior ownership experience. USAM ownership can be positive if the consumer owns a relatively recent vehicle, but negative if the customer owns an older vehicle. We also include dummy variables for "own other American" and own "Japanese." These dummy variables are relative to "own European."

Heterogeneity might also be unobservable. One way to correct for unobserved heterogeneity would be to compute differences in trust, consideration and purchase. But such differ-

ences assume no decay and perfect reliability of repeated measures. Furthermore, consideration and purchase are quantal (0 vs. 1) measures which pose additional complications. Fortunately, by including lagged variables we model decay explicitly, allow imperfect measurement reliability, and mostly account for unobserved heterogeneity. (With repeated noisy measures, the best estimate of the true score at t is not the score at $t-1$, but rather the square-root of the reliability times the lagged score. See Nunnally and Bernstein [1994, 222].) In the analysis in the next section, we find that, even with potential decay and imperfect measurement reliability, the coefficient of (instrumented) lagged trust (in the trust regression) is close to one and, hence, close to a differences model. (Because the conditional-logit model is non-linear, the coefficient of lagged consideration must be interpreted relative to the logit transform.) The combination of random assignment, the inclusion of demographic and purchase-history variables, and lagged variables should minimize unobserved respondent-specific effects.

YEAR 1: RANDOMIZED EXPERIMENTS

Consumer Panel Observed over Six Months

The year-1 panel ran monthly from October 2003 to April 2004. (This was five years prior to the bankruptcies of two US automakers.) Members of Harris Interactive's panel were screened to be in the market for a new vehicle in the next year, on average within the next 6.6 months, and invited to participate and complete six monthly questionnaires. In total, Harris Interactive enrolled 615 Los Angeles consumers of whom 317 completed all six questionnaires for an average completion/retention rate of 51.5%. USAM did not retain recruitment rate statistics for year 1, but USAM estimates an initial recruitment rate of about 40%. "Consideration" was measured with the drop-down menu and "purchase" was a stated purchase measure matched to USAM purchase records.

Random Assignment to Treatments in Year 1

Consumers were assigned randomly to experimental treatments in a 2 x 2 x 2 x 2 full-factorial field experiment so that various respondents received 0, 1, 2, 3, or 4 treatments. Assignments were random, but not guaranteed to be 50-50, for example approximately 40% of the panel members were randomly assigned to competitive test drives. The other treatments were close to 50-50. The competitive online advisor was available in all periods, the competitive community ran for all but the last period, the customized brochures were mailed in periods 2 and 3, and the competitive test-drive took place in period 4. The exact numbers of consumers assigned to each treatment in year 1 is summarized in Table 1.

Insert Table 1 about here.

Examining Issues Of Self-Selection To Treatments in Year 1

Although consumers were assigned randomly to treatments, a few consumers in each treatment did not experience the treatment(s) to which they were assigned. For example, some consumers experienced technical difficulties with the competitive online advisor and a few could not come to the competitive test drive despite incentives to do so. Take-up rates were 91.1% for competitive test drives, 99.4% for brochures, 97.4% for the community forum, and 82.1% for the online advisor. Qualitative data suggest that take-up was random and not due to self-selection. Nonetheless, we address self-selection in two ways.

First, all primary analyses use treatment-assignment dummies. In these analyses the impact of a treatment is measured on all respondents for which the treatment was available whether or not they experienced the treatment. We compared analyses based on self-reported treatments to analyses based on treatment assignments. The pattern of coefficients and their significance was similar for both analyses suggesting that self-selection had little or no effect in year 1. None-

theless, we rely on the more-conservative treatment-assignment analyses.

Second, we examine consumers who (1) were not assigned to the treatment, (2) were assigned to the treatment but did not report participation, and (3) were assigned and reported participation. Self-selection would imply that consumers in group (3) behave differently from consumers in (2). But if that were true, self-selection into (3) from (2 & 3) would leave a non-random set of consumer behaviors in (2). Self-selection would imply differences in the dependent measure for group (2) compared to the random set of consumers in group (1). We find no differences. For example, measured consideration does not vary among the groups (1) and (2) for competitive test drives (t = .05, p = .96), customized brochures (t = .60, p = .56), competitive forums (t = .90, p = .37), or competitive advisors (t = 1.14, p = .26). While we can never rule out self-selection completely, it does not appear to be a problem in the year-1 randomized experiments. (We return to self-selection issues in the year-2 opt-in quasi-experiments where self-selection is a more-central issue.)

MAIN EFFECTS OF TREATMENTS IN YEAR 1 (RAW ANALYSES)

We begin with treatment main effects from the fully crossed 2 x 2 x 2 x 2 experiment. We explore interactions, dynamics, and other variables in the next section.

The raw analyses of main effects are summarized in Table 2. The first column is the percent increase in the consumers who are considering or have purchased a USAM vehicle at the end of the experiment. For example, among consumers assigned to competitive test drives, 20.5% more considered USAM relative to consumers who were not assigned to competitive test drives. This difference is significant (t = 3.6, p < .001). More detailed analyses (not shown) suggest that the most dramatic change in consideration occurs in period 4 when consumers experienced the competitive test drives, but the effect endured through subsequent periods. The en-

during effect is consistent with a "stock" model of trust. For example, relative trust among test-drive-assigned consumers increases to 13% in period 4 relative to controls, but stays 9% above controls in both periods 5 and 6. The treatment-vs.-control lift is not significant for customized brochures, the competitive forum, and the competitive advisor. Brochures do increase trust, but not as much (7%), while the community forum has a negative effect (– 9%) and the competitive advisor has a negligible effect (1%). The increase in cumulative purchase follows the same pattern with an 11.1% lift for competitive test drives ($t = 2.4$, $p = .02$) and insignificant effects for the other treatments.

Insert Table 2 about here.

We might posit the effect of competitive information to be larger among USAM owners because current vehicles are improved relative to prior USAM vehicles. Alternatively, the effect might be larger among non-USAM owners because they have less experience with older USAM vehicles. Prior to examining the data, the relative effect of USAM ownership is ambiguous. Empirically, the data suggest that there is no interaction effect due to prior USAM ownership. Experiential competitive information (test drives) is the only treatment with a significant impact among USAM non-owners and the magnitude of that impact is virtually identical to the magnitude among all consumers (lower left of Table 2). We explore interactions further in the next section. We also find no differential impact due to age or sex (not shown).

In summary, the main-effect analyses suggest that the experiential treatment, competitive test drives, had the strongest effect. But simple analyses might disguise effects. There are hints that effects persist, say into the fifth and sixth period for competitive test drives. The raw analyses do not account for dynamics, interactions among treatments, the conditioning of purchase on consideration, the potential for trust mediation, or heterogeneity due to demographics.

CONDITIONAL-LOGIT ANALYSES IN YEAR 1

We now examine whether or not competitive information enhances consideration and purchase when we account for dynamics, persistence, more-complete prior-ownership effects, interactions among treatments, and unobserved external shocks. The basic models are conditional-logit analyses of consideration and purchase (see Figure 2). Specifically, we ask whether the treatments increase consideration and, among those consumers who consider USAM, whether the treatments also affect purchase.

Insert Figure 2 and Table 3 about here.

In the conditional-logit analyses we attempt to control for many effects. We include lagged consideration as an explanatory variable to focus on changes in consideration. We include dummy variables for observation periods to account for unobserved USAM marketing actions and to account for unobserved environmental shocks. (Period 1 is a pre-measure and the period-2 dummy variable is set to zero for identification.) The period dummy variables also account for any measurement artifact that might boost consideration (e.g., "Hawthorne" effect). To attempt to account for heterogeneity in past purchases we include prior ownership of USAM, other American, and Japanese (relative to European) vehicles. Age and sex were included but suppressed to simplify Table 3. (They were not significant.)

Direct Effects of Treatments

We begin with main effects of the treatments as shown in the first and second columns of parameters in Table 3. (Standard errors available upon request.) Both the consideration and purchase analyses explain substantial information with U^2 of 25.3% and 56.2%, respectively. (U^2, sometimes called a pseudo-R^2, measures the percent of uncertainty explained.) Consideration is increased if consumers own USAM or other American vehicles and decreased if they own Japa-

nese vehicles. Consideration is also higher in Periods 3 to 6 relative to Period 2. The only significant direct treatment effect is due to competitive test drives. Purchase, conditioned on consideration, also increases with competitive test drives (marginally significant), but there are no direct effects of prior ownership or period of measurement on purchase.

Trust as a Mediator

Trust was central to USAM's strategy. They hoped that competitive information would build trust and that trust would lead to consideration and purchase. There is ample precedent in the literature for trust as a mediator of purchase or purchase intentions (e.g., Bart, et al. 2005; Büttner and Göritz 2008; Erdem and Swait 2004; Morgan and Hunt 1994; Porter and Donthu 2008; Urban, Amyx, and Lorenzon 2009; Yoon 2002). For example, in a series of experiments, Trifts and Häubl (2003) demonstrate that competitive price information affects preference, but the effect on preference is mediated through trust.

We use the methods of Baron and Kenny (1986) to test whether USAM's competitive information treatments were mediated through trust. See Tybout and Hauser (1981) for a field-experiment marketing example of mediation analyses. Specifically, if the treatments affect trust and also have a direct effect, we estimate a third model. We add an indicator of trust as an explanatory variable in the conditional-logit model. If the treatments are mediated through trust, then (1) the indicator of trust should be significant in the new model and (2) the direct effect of treatments should now be insignificant. Partial mediation includes (1), but requires only that the direct effect decrease in magnitude.

We must be careful when we add trust to the model. First, to avoid joint causality in the errors (such as a common measurement component), we lag trust. Lagged trust makes sense empirically. If competitive information in period $t-1$ increases trust at the end of period $t-1$, we

expect that the increased trust will enhance consideration (and purchase) in period t. Second, unobserved shocks in trust might also affect consideration and purchase. Thus we "instrument" trust by estimating trust as a function of the treatments and other explanatory variables. We then use estimated lagged trust as an explanatory variable in the analysis of consideration and purchase. (Traditional mediation analyses use lagged trust directly. In our data, these tests also indicate mediation. The analyses are available upon request.)

To estimate trust, we recognize that trust is a "stock" variable. Trust might decay over time, but some fraction endures. Thus, the trust equation includes lagged trust as an explanatory variable. In each period the treatments increase or decrease the stock of trust. (This specification also accounts for measurement errors in trust.) Prior ownership, age, and sex account for heterogeneity in base trust and period dummies account for unobserved shocks. The last column of Table 3 describes the trust equation where we have again suppressed age and sex to simplify exposition.

Competitive test drives clearly increase trust and there is evidence that customized brochures increase trust. The impact of customized brochures is consistent with other published studies of customization (e.g., Ansari and Mela 2003; Hauser, et al. 2010). The effect of customized brochures was less apparent in the raw analyses because it decayed to become insignificant in the last period (raw analyses). The impact was stronger in earlier periods. The trust regression picks up this growth and decay. The raw analyses, conditional-logit analyses, and trust regression identify no impact for the community forum and the competitive advisor. Contrary to USAM's prior beliefs (and popular beliefs in the trust literature), the signal of trust was not sufficient to overcome the negative comments in the community forum and the large percentage of competitive recommendations by the competitive advisor.

We now add lagged estimated trust to the conditional-logit analyses. Such two-stage estimates are limited-information maximum-likelihood estimates. The two-stage estimates are consistent but require bootstrap methods to estimate the standard errors for the coefficients (Berndt, et al. 1974; Efron and Tibshirani 1994; Wooldridge 2002, p. 354, 414). The third through sixth columns of parameters in Table 3 report the means from 1,000 bootstrap replicates. Significance is based on the bootstrap estimates of standard errors.

For both consideration and conditional purchase, the treatments are mediated through trust—estimated trust is significant and including trust in the model increases fit. The increase is significant for consideration and marginally significant for purchase (χ_1^2 = 86.7, $p < .001$, χ_1^2 = 3.1, p =0.08, respectively.). Once we partial out trust, there remain no significant direct effects due to the treatments.

Interaction Effects for Prior Ownership and for Multiple Treatments

Prior ownership might influence the impact of the treatments and there might be interactions due to multiple treatments. To test whether prior ownership affects the impact of competitive information we cross prior ownership of USAM with the treatment-assignment dummies. For trust, consideration, and purchase the interactions are not significant (F = 1.91, p = .11; χ_4^2 = 4.3, p = .37, χ_4^2 = 7.0, p = .13, respectively).

We now test interactions among the treatments. Treatment interaction-effects do not add significantly to a trust regression using a specification that allows all interactions (F = .85, p = .59). We continue to use estimated lagged trust (without interactions) and estimate a conditional-logit model allowing interactions. The fully-saturated consideration model is marginally significant relative to a main-effects model, but provides no additional insight (χ_{11}^2 = 17.1, p = .09). A few coefficients are significant, but all include competitive test drives with slight variations in

parameter magnitudes depending upon the other combinations of treatments. To avoid overfitting with a fully-saturated model, we examined a more-parsimonious model in which we add a variable for two or more treatments. This parsimonious model is consistent with the findings of Hauser, Urban and Weinberg (1993). The "two or more treatments" variable is not significant and does not add significantly to the models whether the variable is added after USAM ownership interactions or before (χ_1^2 = .6, p = .42, χ_1^2 = .6, p = .46 for consideration and χ_1^2 = .1, p = .75, χ_1^2 = .03, p = .85 for purchase). Despite the marginal significance in a fully-saturated model, neither the fully-saturated nor the parsimonious analysis highlights any managerially-insightful interactions. For example, the fourth (consideration) and sixth (conditional purchase) columns of Table 3 display models with interactions due to prior ownership and due to two or more treatments.

The net result of the conditional-logit analyses complements the raw main-effect analyses and provides further insight on trust mediation and the lack of interaction. Specifically:

- experiential competitive information (test drives) increases consideration and conditional purchase, but is mediated through trust.
- customized brochures increase trust and, though trust, increase consideration and conditional purchase. However, the effect decays by the last period.
- word of mouth (competitive forum) and competitive advisors did not increase trust, consideration, and purchase. For USAM's situation, the positive trust signal does not appear sufficient to overcome the negative information about USAM in these treatments.

In subsequent sections we discuss the managerial and proof-of-concept implications of these results, but first we examine continuous-time Markov analyses and report on the year-2 quasi-experiment.

CONTINUOUS-TIME MARKOV ANALYSIS IN YEAR 1

The conditional-logit analyses improve and clarify the main-effect analyses, but they do not capture fully the dynamics of the automotive market. Conditional-logit analyses capture some dynamics, such as "stock" models of persistence, conditional flows, and "flows" among "not consider (state 1)," "consider but not yet purchase (state 2)," and "consider and purchase (state 3)." However, they do not allow flows to happen in continuous time nor to do they allow reverse flows from "consider" to "not consider." We address these issues with continuous-time Markov analyses (Cox and Miller 1965; Hauser and Wisniewski 1982, hereafter "Markov" analyses). An added advantage of Markov models is a single likelihood function for all parameters of all defined flows.

Insert Figure 3 about here.

The Markov model is similar to the conditional-logit model in Figure 2, except, as shown in Figure 3, we model a more-complete set of flows and allow the flows to occur in continuous time. Consumers "flow" among states with instantaneous flow rates dependent upon the treatments and other variables. Mathematically for $j \neq i$, $a_{ijn}\Delta t$ is the probability that the consumer flows from state i to state j in the time period between t and $t + \Delta t$ for very small Δt during the n^{th} observation period. We specify the flow rate as a log-linear function of the treatment-assignments, prior ownership, age, sex, period dummies, and interactions as relevant—the same types of specifications as in the conditional-logit analyses. Although we model instantaneous flow rates, we only observe the state that describes each consumer at the end of each period. Fortunately, using the a_{ijn}'s, we can calculate the probability, p_{ijn}, that the consumer was in state i at the beginning of the n^{th} period and in state j at the end of the period. Specifically:

$$P_n = e^{A_n(T_n - T_{n-1})} \equiv \sum_{r=0}^{\infty} \frac{A_n^r (T_n - T_{n-1})^r}{r!} \equiv V_n [\exp \Lambda_n] V_n^{-1} \quad (1)$$

where P_n is the matrix of the p_{ijn}'s, A_n is the matrix of the a_{ijn}'s, T_n is the time at the end of the n^{th}period, V_n is the matrix of eigenvectors of $A_n(T_n - T_{n-1})$, and $[\exp \Lambda_n]$ is the matrix with the exponentiation of the eigenvalues on the diagonal.

Prior applications in marketing used regression approximations to Equation 1 (Hauser and Wisniewski 1982). With today's computers we use maximum-likelihood methods with all flows estimated simultaneously. See Kulkarni (1995) for a review of computational methods to deal with matrix exponentiation. While we would like to repeat the Markov model for all of the specifications tested by conditional-logit analyses, the convergence of the Markov estimates and the computation times appear to be most appropriate for more-parsimonious models. Thus, we use the Markov analyses as a confirmation of the conditional-logit analyses by carefully selecting the explanatory variables based on the logit analyses. (We do not need lagged consideration in the Markov analyses because the Markov model is based on transitions from "not consider" rather than based on estimating consideration as a function of lagged consideration and other variables.) For simplicity of exposition we report key analyses in Table 4. Other analyses and R-code are available from the authors.

Insert Table 4 about here.

The Markov reinforce the conditional-logit analyses. Competitive test drives have a significant effect on consideration, but that effect is likely mediated through trust. Lagged estimated trust has a significant effect on key flows. The Markov analyses, which model dynamics more completely and estimate all flows simultaneously, suggest that the conditional-logit interpretations are reasonable. We also modeled potential misclassification of "consider" vs. "not consid-

er" as in Jackson, et al. (2003). The misclassification analyses improved fit, but provided no additional managerial insights. Estimated misclassification was moderate.

YEAR 2 – TEST OF ADVERTISING-PLUS-WEBSITE OPT-IN

In year 2 USAM sought to test the feasibility of a national launch of competitive-information strategies. To control costs, USAM wanted to test a communications strategy in which advertising brings consumers to a website from which consumers could opt-in to treatments. USAM recognized that an opt-in strategy would encourage self-selection, but they hoped that consumers would self-select on interest in gathering information rather than affinity to USAM. If consumers self-selected on information, then the advertising-plus-website opt-in strategy could be rolled out nationally. If consumers self-selected on USAM affinity, then an advertising-plus-website opt-in strategy may not provide incremental consideration and purchase; USAM would have to investigate other means to rollout the competitive information treatments.

USAM assigned consumers randomly to one of two groups. Consumers in the control group received no treatments. Consumers in the test group received an advertisement inviting them visit a "My Auto Advocate" website (Figure 4a). We call these consumers the "opt-in" test group. Those consumers in the test group who did not visit the "My Auto Advocate" website in response to advertising, were invited to an "Internet study" that included a visit to the "My Auto Advocate" website. We call these consumers the "forced-exposure" test group. Once at the "My Auto Advocate" website, both the opt-in test group and the forced-exposure test group could opt-in to any of five treatments. USAM interpreted the "opt-in" test group as a surrogate for a national opt-in strategy. USAM interpreted the opt-in-plus-forced-exposure groups as a surrogate for a national rollout in which USAM used more-substantial incentives to encourage consumers to visit the "my Auto Advocate" website.

Insert Figure 4 about here.

The year-2 panel ran monthly from January to June, 2005. Members of Harris Interactive's panel were screened to be in the market for a new vehicle, on average within the next 2.2 years, and invited to participate and complete six monthly questionnaires. In year 2 the relaxation beyond 12-month intenders (as in year-1) simulated the target of an advertising-plus-website national rollout. Once consumers visited the "My Auto Advocate" website they were given incentives to opt-in to the treatments. (The incentives anticipated national-rollout incentives.) For example, consumers received 20 reward certificates (worth $1 each) for participating in the competitive test drives. Incentives for the other treatments were the order of 5 reward certificates.

In total, Harris Interactive invited 6,092 Los Angeles consumers of which 1,322 completed all six questionnaires for an average response/completion/retention rate of 21.7%. This rate was not significantly different across the three groups (control vs. opt-in vs. forced-exposure, $p = .25$). "Consideration" and "purchase" in year 2 were measured as in year 1.

Treatments in Year 2

Three of the treatments in year 2 were similar to year 1. The direct-experience competitive-test-drive treatment and the word-of-mouth competitive-community-forum treatment were virtually the same with only minor updates (Figure 4c). The competitive online advisor was improved with a better interface and a "garage" at which consumers could store vehicle descriptions (Figure 4d). The online advisor still favored other manufacturers' vehicles in year 2, although a bit less so than in year 1. The major change was the direct-information treatments. In year 2 USAM offered electronic brochures for USAM vehicles (called eBooklets). They were online or downloadable rather than mailed and were less customized. USAM added competitive direct information by allowing consumers to download competitive brochures. Although many

competitive brochures were available on manufacturers' websites, USAM's single-source web-page made it more convenient for consumers to compare vehicles (Figure 4b). Table 5 summarizes the numbers of consumers who opted-in to treatments in year 2. All but competitive test drives were reasonably popular and available in all periods.

Insert Table 5 about here.

Test vs. Control and Self-Selection Issues

We first examine consideration and purchase in the test vs. control groups. There were no significant differences in either consideration, purchase, or purchase intentions (t = .45, p = .65 and t = .45, p = .66, t =.18 and p = .86 respectively). Although the opt-in test group achieves slightly higher consideration and purchase intentions, these differences were not significant (t = .07, p = .95 and t = .05, p = .96, respectively). The test-vs.-control results suggest that the advertising-plus-website communications strategy was not cost-effective because, overall, it provided little or no lift in consideration and purchase.

Using arguments similar to the self-selection tests in year 1, we compare consumers in the control group (who received no treatments) to those in the test group who visited "My Auto Advocate" but did not select any treatments. Among these no-treatment consumers, the control group had significantly higher consideration and purchase intentions than the non-treated members of the test group (t = 2.2, p = .03, t = 2.1, p = .04, respectively). These comparisons suggest that consumers who self-selected one or more treatments were consumers who were otherwise more likely to consider or purchase USAM (or at least vehicles in general). The self-selection on consideration remains significant when we restrict the test-group consumers to the forced-exposure cell (t = 2.0, p = .04). Purchase intentions are higher in the control group, but the comparison is not significant (t = .78, p = .44). Based on these results, we conclude that the advertis-

ing-plus-website strategy is more likely to draw consumers who are more likely to consider or purchase USAM vehicles. This effect is mitigated, but not eliminated, for the forced-exposure test group. In year 2, self-selection is likely confounded with the treatments in terms of causing lifts in consideration and purchase. Self-selection cannot be ruled out as a potential explanation for these observed lifts. However, by combining managerial judgment, the year-1 results, and the year-2 analysis we might still gain insight subject to self-selection caveats.

Analysis Of The Effects Of Competitive Information In Year 2

Following the philosophy of Little (1970), we rely on the insights obtained from the year-1 random-assignment experiments and examine the year-2 data recognizing that self-selection is a partial explanation for any treatment impact. Philosophically, year 1 is a quasi-control for year 2. It is not a pure control because the sample definition varied (not just 12-month intenders), the treatments varied (eBrochures and eBooklets versus customized brochures, improved online advisor), and USAM's products and image were slightly better in year 2. To reduce the effect of self-selection, we compare the forced-exposure test to the control group.

We first examine the raw analyses (Table 6). Unlike in the year-1 randomized experiments, where the experiential treatment (competitive test drives) dominated direct effects, we see significant effects for all treatments on either consideration or purchase. The direct effects are similar when the sample is limited to non-USAM owners. This is consistent with an hypothesis that self-selection is a major force. It casts doubt on the advertising-plus-website opt-in communications strategy as a means to attract and convert heretofore skeptical consumers. The low take-up of competitive test drives in year 2 (only 11% of the consumers who were forced to visit "My Auto Advocate" selected the competitive test drive vs. 91% in year 1) reinforces this doubt.

Insert Tables 6 and 7 about here.

We examine the trust in each period (details available from the authors). Those consumers who selected treatments are significantly more likely to trust USAM. This is true for all treatments with all *p*'s equal to .01 or less. The lifts of roughly two-to-seven percent do not vary dramatically across periods. Furthermore, when we examine the trust regression (under self-selection, last column of Table 7), the incremental change in trust does not seem to be strongly driven by the treatments. The trust regression reinforces the suspicion that USAM's advertising-plus-website strategy attracts consumers who would otherwise have trusted, considered, or purchased USAM vehicles.

But not all is lost. As in year 1, when we account for dynamics, persistence, heterogeneity, prior-ownership effects, and unobserved external shocks with conditional-logit analyses, we observe direct effects due to experiential (test drives) and information-about-the-product treatments (eBrochures). Thus, despite self-selection issues, there are hints that experiential and information-about-the-product competitive information is relatively more effective than word-of-mouth and online advisors. Competitive test drives have an overall significant impact on consideration and purchase and, for purchase, the effect is driven by non-USAM owners. Perhaps because self-selection contaminated the trust regression, we do not find evidence in year 2 of trust mediation. Markov analyses, not shown, are consistent with the conditional-logit analyses.

We, and USAM, believe that the lack of trust mediation in year 2 is due to self-selection toward consumers more likely to either be interested in USAM or, at least, the class of vehicles sold by USAM. Opt-in is by far the biggest difference between the year-1 experiments and the year-2 simulated-rollout quasi-experiments. But, because self-selection is confounded with the treatments in year 2, any inference we make must rely on both the year-1-vs.-year-2 comparison and on managerial judgment.

Summary of Year-2 Quasi-Experiments

Based on the year-1 experiments, competitive test drives and customized brochures increased trust among randomly chosen consumers. The increased trust drove consideration and purchase. Despite self-selection issues, the relative year-2 quasi-experiments provide an indication that competitive test drives and competitive brochures also increase consideration and purchase.

The new insight from year 2 is that competitive information may not be effective if the information is only made available with an advertising-plus-website opt-in strategy. The opt-in strategy does not appear to reach consumers who are skeptical about USAM and who would be affected most by the competitive-information treatments. The bottom line for USAM is that competitive-information treatments can increase trust, consideration, and purchase, but only if USAM invests to provide sufficient incentives to consumers who would not otherwise seek information on the USAM's products. The opt-in incentives in the simulated national rollout were not sufficient.

INTERPRETATIONS

Many authors have championed trust-based strategies (e.g., Hacker, Willard and Couturier 2002; Urban 2004). The popular belief is that if firms build trust with consumers, the trust will cause consideration and (repeat) purchase. Taken at face value, this philosophy says that any communications strategy will build trust if the firm signals that it is willing to lay bare its strengths and weaknesses relative to competition. USAM's situation in 2003-2005 was a excellent test bed for this theory. Because of past experiences with USAM's vehicles many consumers would not even consider USAM's vehicles in 2003-2005. And because USAM's 2003-2005 vehicles had improved relative to prior vehicles, USAM had an opportunity to build trust. USAM had good news to tell consumers. Under popular theories, all four generic treatments should have

built trust and, subsequently, consideration and purchase. But they did not. Furthermore, an opt-in strategy itself should build trust. If the opt-in did not work for USAM, it is unlikely to work in situations that are not as favorable for providing competitive information.

Although pure trust signaling did not appear to overcome negative information, specific competitive information strategies were effective. USAM's tests suggest that the experiential strategy (competitive test drives) was the most effective communications strategy, especially in year 1. Consumers experienced the improved vehicles and saw that the vehicles had improved relative to competition. Subject to the stated caveats, there is also a suggestion in year 2 that competitive product information (eBrochures) was effective in building trust, at least relative to the other treatments. Qualitative data and USAM's managerial judgment reinforced this belief. We posit that competitive product information was effective because, like competitive test drives, eBrochures provided consumers with an unbiased comparison in which USAM did well.

Neither word-of-mouth (community forums) nor competitive advisors increased trust. Community forums relied on other consumers' opinions—opinions contaminated with past experience. Online advisors, especially USAM's advisor, relied in part on past consumer experience and may have lagged USAM's improved perceptions. (These advisors use Bayesian methods based on prior preferences.) It appears that competitive information helps only if it is actually good news. The trust signal did not overcome the negative information. While this may seem obvious *ex post*, it was far from obvious *a prioi*. USAM believed that openness alone would engender trust. Year-1-analyses (with partial confirmation from year 2) imply that openness alone is not sufficient. We therefore summarize our interpretations of the USAM experiments and quasi-experiments with a proposed generalization.

> Hypothesis. *Unbiased competitive information can build trust and trust can enhance consideration and purchase, but openness alone is not sufficient. The firm will build trust if the firm's products satisfy customer needs better and if the communication strategy enables the firm to communicate that fact to consumers. Furthermore, the consumers that are most affected by competitive information are not necessarily those consumers who will opt-in. Strong incentives are necessary to encourage consumers to use competitive information.*

This generalization is consistent with the USAM experiments and quasi-experiments, but subject to tests in different categories and with different implementations of the four generic communications strategies and a different implementation of an opt-in strategy.

COST EFFECTIVENESS AND MANAGERIAL IMPLICATIONS

USAM ultimately implemented competitive-information strategies, but not immediately following the 2003-2005 experiments. The following calculations illustrate the motivation behind USAM's decision at the time. We maintain the USAM disguise by using publicly available data as an approximation to USAM's cost-benefit analyses.

For illustration we assume a 15% market share. Based on this share, an 11.1% sales lift (year-1 data), and an approximate cost of $120 per participating consumer (with incentives), the cost of an incremental vehicle sale is approximately $7,200. [$7,207 = $120/(0.15 * 0.111).] Similar calculations for competitive eBrochures suggest about $140 per incremental sale (using managerial judgment based on the results of year 1 and year 2). Typical margins for the automotive industry are about 9% and the average price of a new car is about $28,400 (Thomas and Cremer 2010, http://www.ehow.com/facts _5977729_average-cost-new-car.html). These public numbers suggest an incremental profit of approximately $2,500 per vehicle sold, much less than

the $7,200 cost. Competitive test drives, as implemented in USAM's randomized experiments, are not profitable. However, similar calculations for competitive eBrochures suggest a positive payback, even if managerial judgment of the estimated sales lift is off by a factor of 10 or more. (Neither the competitive forum nor the online competitive advisor suggested any lift, hence they are judged not to be profitable.)

Large-scale competitive information strategies were put on hold during the distractions of the automotive and financial crises of 2005-2009. A large traveling competitive test-drive format was neither feasible nor cost effective. However, the concept of competitive test drives gained traction in situations where such test drives could be implemented cost efficiently and targeted at skeptical consumers. When the financial situation improved, USAM tested competitive test drives for SUVs with a dealer in Arizona. These competitive test drives proved to be cost-effective—about $100-200 per incremental sale. Costs were substantially lower because the test drives were from the dealer's lot (no need to rent a test track), because fewer vehicles were necessary (only SUVs), and because the dealer could borrow or rent vehicles from competitive dealers. On the benefit side, gross margins were higher than average for SUVs. USAM continued to experiment with competitive test drives in key local markets, especially when high-value skeptical consumers could be targeted cost-effectively. In late 2010 the head of US marketing for USAM launched a yearlong series of weekend competitive test drives at dealerships. Each event invites a few thousand potential buyers who compare USAM vehicles with competitive vehicles. USAM's name for this initiative was a modification of the name it used for competitive test drives in the year-1 experiments (disguised in this paper).

USAM is testing strategies to overcome adverse self-selection by targeting competitive information toward skeptical consumers. For example, USAM is considering using bill-paying

records to target consumers with recent repairs of competitive vehicles. New methods include interactive screening to identify consumers who answer questions that indicate they are disbelievers. Targeted consumers would get substantial incentives (hundreds of dollars) to participate.

Based on the 2003-2005 data and managerial judgment, USAM believes that the concept of competitive eBrochures is cost effective. USAM now includes competitive comparisons on its website using standardized Polk data on prices, specifications, and equipment for preselected competitive and consumer-specified vehicles. In 2009, USAM used a national advertising campaign that encouraged consumers to compare USAM's vehicles to competitors on good fuel economy and styling. Many USAM dealers offer unsolicited extended weekend test drives and encourage competitive comparisons. USAM marketing managers now believe that competitive information builds trust, consideration, and sales and is profitable, but only if implemented cost effectively to skeptical consumers for vehicle categories in which USAM has good vehicles relative to competitors. This more-nuanced trust-based strategy is believed to be more profitable than a general strategy of trust signaling.

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TABLE 1
CONSUMERS RANDOMLY ASSIGNED TO TREATMENTS IN YEAR 1

Number of respondents who assigned to the indicated treatment in that period

Treatment		Period 2	Period 3	Period 4	Period 5	Period 6	Treatment Cell
Competitive Test Drives	Yes	0	0	124	0	0	124
	No	317	317	193	317	317	193
Customized Brochures	Yes	164	164	0	0	0	164
	No	153	153	317	317	317	153
Competitive Forum	Yes	151	151	151	151	0	151
	No	166	166	166	166	317	166
Competitive Advisor	Yes	156	156	156	156	156	156
	No	161	161	161	161	161	161

TABLE 2
RAW ANALYSES

Treatment	Consideration (% lift in last period)	Purchase (% cumulative lift)
Competitive Test Drives	**20.5% ***	**11.1% ***
Customized Brochures	-2.9%	4.8%
Competitive Forum	-2.4%	3.3%
Competitive Advisor	0.5%	-4.4%
Treatment Among Non-USAM-Owners		
Competitive Test Drives	**20.0% ***	7.3%
Customized Brochures	2.2%	5.0%
Competitive Forum	1.1%	6.1%
Competitive Advisor	2.0%	-0.9%

TABLE 3
CONDITIONAL-LOGIT ANALYSES AND TRUST REGRESSION – YEAR 1 RANDOM ASSIGNMENTS

	Conditional-Logit Analyses (five periods, 317 respondents)						**Trust Regression** (lagged trust is used in this regression)
	Direct Effects Not Mediated		**Mediated by Trust (bootstrap estimates)**				
Dependent Measure	**Consider**	**Purchase**	**Consider**		**Purchase**		
Constant	**-1.492***	**-2.567***	**-3.945***	**-4.452 ***	**-3.896***	**-3.893***	.714
Lagged Consider	**2.537***		**2.394***	**2.405 ***			
Lagged Trust Hat			**.531***	**.525 ***	***.245***†	.249	**.857 ***
Competitive Test Drives	**.579***	***.938***†	.392	.346	.879	.498	**.371 ***
Customized Brochures	.079	.477	-.059	-.202	.405	.575	**.127 ***
Competitive Forum	-.023	-.103	.136	.133	-.059	.025	-.056
Competitive Advisor	.144	.122	.133	.124	.182	.522	.016
Prior Own USAM	**.399***	.137	***.327***†	**.734 ***	.079	.219	.000
Prior Own Other American	**.304***	-.005	***.253***†	***.238***†	.037	.009	.011
Prior Own Japanese	**-.577***	-.188	**-.464***	**-.478 ***	-.126	-.105	-.023
Period 3	.313	.200	**.461***	**.461 ***	.269	.275	**-.243 ***
Period 4	***.419***†	.264	***.433***†	***.423*** †	.270	.281	**-.282 ***
Period 5	**.523***	-.238	***.390***†	***.383*** †	-.276	-.253	**-.119 ***
Period 6	**.722***	.185	**.654***	**.644 ***	.194	.253	**-.238 ***
Prior Own USAM with							
Competitive Test Drives				-.211		1.612	
Customized Brochures				.109		-.116	
Competitive Forum				-.495		-.966	
Competitive Advisor				-.444		-.280	
Two or more treatments				.208		-.169	
Log likelihood	- 820.6	-218.2	-777.2	-774.8	-216.6	-213.1	adjusted-R^2
U^2 (aka pseudo-R^2)	25.3%	56.2%	29.3%	29.5%	56.5%	57.3%	.748

* Significant at the 0.05 level. † Significant at the 0.10 level. Sex and age coefficients not shown (not significant. Trust regression Interactions not significant.

TABLE 4
CONTINUOUS TIME MARKOV PROCESS ANALYSIS – YEAR 1 RANDOM ASSIGNMENTS

	Continuous Time Markov Estimation Not Mediated			**Continuous Time Markov Estimation Mediated by Trust**		
	Consider		**Purchase**	**Consider**		**Purchase**
Dependent Measure	**Not Consider to Consider (1→2)**	**Consider to Not Consider (2→1)**	**Consider to Purchase (2→3)**	**Not Consider to Consider (1→2)**	**Consider to Not Consider (2→1)**	**Consider to Purchase (2→3)**
Constant	.139	.231	**.120** *	**.146** *	**.249** *	**.102** *
Lagged Trust Hat				**.221** *	***-.230*** †	**.313** *
Competitive Test Drives	***1.060*** †	-.408	-.003	1.001	-.262	-.124
Customized Brochures	.130	.140	.252	.107	.217	.169
Competitive Forum	-.227	-.236	.124	-.273	-.300	.241
Competitive Advisor	-.003	-.342	-.144	-.088	-.359	-.089
Prior Own USAM	-.407	-.773		-.403	-.624	
Prior Own Other American	**.599** *	.070		**.525** *	.039	
Prior Own Japanese	-.305	.292		-.249	.197	
Period 3	.012	-.172	.445	.032	-.289	.565
Period 4	**-.971** *	.544	-.394	**-1.004** *	.423	-.366
Period 5	**-.698** *	.122	.224	**-.760** *	.083	.194
Log likelihood		-616.46			-608.12	

* Significant at the 0.05 level. † Significant at the 0.10 level. All flows are estimated simultaneously.

TABLE 5
CONSUMERS WHO SELECTED TREATMENTS IN YEAR 2
(Test of Advertising-then-Website National Rollout)

Number of respondents who selected the indicated treatment in that period

Treatment		Period 2	Period 3	Period 4	Period 5	Period 6	Treatment "Cell"
Competitive Test Drives	Opt-in	70	0	0	0	0	70
	Forced	140	0	0	0	0	140
	Not Treated	1,182	1,322	1,322	1,322	1,322	1,182
Competitive eBrochures	Opt-in	88	178	199	202	211	289
	Forced	149	361	411	425	432	621
	Not Treated	1173	961	911	897	890	701
USAM eBooklets	Opt-in	49	158	184	194	205	252
	Forced	114	355	411	417	438	549
	Not Treated	1208	967	911	905	884	773
Competitive Forum	Opt-in	71	139	168	194	208	249
	Forced	114	294	352	409	420	538
	Not Treated	1208	1028	970	913	902	784
Competitive Advisor	Opt-in	97	180	206	226	246	290
	Forced	199	378	441	493	535	645
	Not Treated	1123	944	881	829	787	677

* Not treated = those in control group plus those in test group who did not select the treatment

TABLE 6
RAW ANALYSES

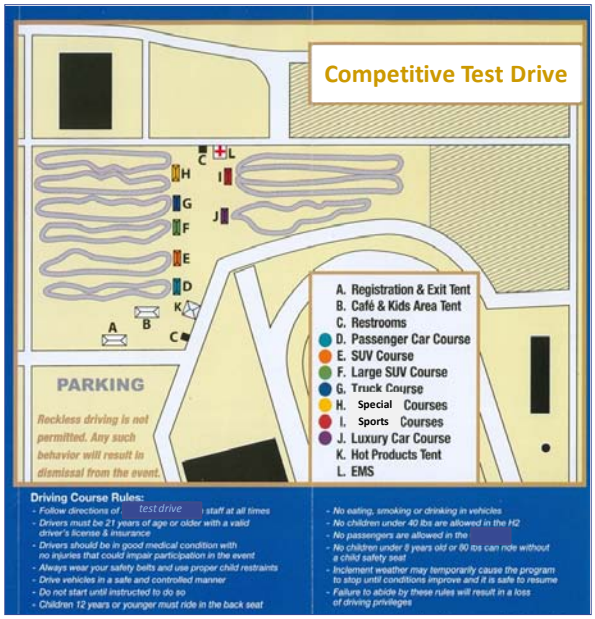
Treatment	Consideration (% lift in last period)	Purchase (% cumulative lift)
Competitive Test Drives	6.6%	**5.3% ***
Competitive eBrochures	**8.5% ***	**3.3% ***
USAM eBooklets	**8.6% ***	**4.9% ***
Competitive Forum	**7.7% ***	1.5%
Competitive Advisor	**6.3% ***	2.6%
Treatment Among Non-USAM-Owners		
Competitive eBrochures	5.2%	**6.6% ***
USAM eBooklets	**7.7% ***	**2.8% ***
Competitive Forum	**8.2% ***	**3.5% ***
Competitive Advisor	**8.5% ***	**3.1% ***
Competitive eBrochures	**6.6% ***	**2.4% ***

TABLE 7
CONDITIONAL-LOGIT ANALYSES AND TRUST REGRESSION – YEAR 2 ADVERTISING-PLUS-WEBSITE OPT-IN

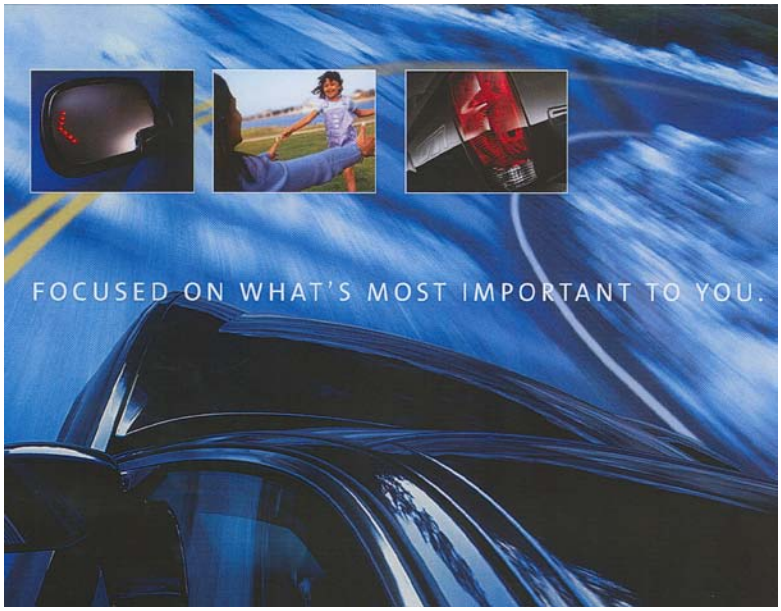
	Conditional-Logit Analyses (five periods, 1,322 respondents)						Trust Regression
	Direct Effects not Mediated		Mediated by Trust (bootstrap estimates)				(lagged trust is used in this regression)
Dependent Measure	**Consider**	**Purchase**	**Consider**		**Purchase**		
Constant	**-2.042 ***	**-3.034 ***	**-4.933 ***	**-5.532 ***	**-5.003 ***	**-7.311 ***	**.697 ***
Lagged Consider	**2.668 ***		**2.460 ***	**2.463 ***			
Lagged Trust Hat			**.605 ***	**.605 ***	**.369 ***	***.365*** †	**.833 ***
Competitive Test Drives	**.783 ***	-.025	**.804 ***	**1.082 ***	-.032	**1.391 ***	.081
Competitive eBrochures	**.235** *	***.473*** †	.118	***.238*** †	.392	.552	***.052*** †
USAM eBooklets	.019	-.214	-.022	.004	-.227	.112	.026
Competitive Forum	.085	-.177	.110	.170	-.138	.228	-.013
Competitive Advisor	-.044	.209	-.009	.034	.222	.086	-.017
Prior Own USAM	**1.349 ***	**.879 ***	**1.033 ***	**1.444 ***	***.714*** †	**1.157 ***	**.138 ***
Prior Own Other American	***.122*** †	.018	.023	.025	-.017	-.015	***.032*** †
Prior Own Japanese	**-.419 ***	-.133	**-.293 ***	**-.290 ***	-.075	-.093	**-.046 ***
Period 3	-.094	-.386	-.095	-.096	-.400	-.387	**-.067 ***
Period 4	-.001	**-.665 ***	.005	.004	**-.675 ***	**-.665 ***	***-.049*** †
Period 5	.027	**-.864 ***	.021	.017	**-.875 ***	**-.869 ***	***-.052*** †
Period 6	**.201** *	**-.740 ***	***.198*** †	**.198 ***	**-.754 ***	**-.742 ***	**-.070 ***
Prior Own USAM with							
Competitive Test Drives				-.676		**-3.001 ***	
Competitive Brochures				-.230		-.159	
USAM eBooklets				-.018		-.385	
Competitive Forum				-.086		-.490	
Competitive Advisor				-.027		.167	
Two or more treatments				0.104		.113	
Log likelihood	-2836.9	-469.8	-2701.3	-2698.4	-465.4	-459.0	adj-R^2
U^2 (aka pseudo-R^2)	38.1%	70.3%	41.0%	41.1%	70.5%	70.9%	0.708

* Significant at the 0.05 level. † Significant at the 0.10 level. Sex and age coefficients not shown (not significant).

FIGURE 1
YEAR-1 (RANDOM ASSIGNMNET) COMPETITIVE-INFORMATION TREATMENTS



(a) Competitive Test Drive



(b) Customized Brochures

Site contained over 60 dialogues averaging over 60 comments.

(d) Competitive Community Forum

(c) Competitive Online Advisor

FIGURE 2
CONSIDERATION AND PURCHASE DYNAMICS: CONDITIONAL-LOGIT ANALYSES

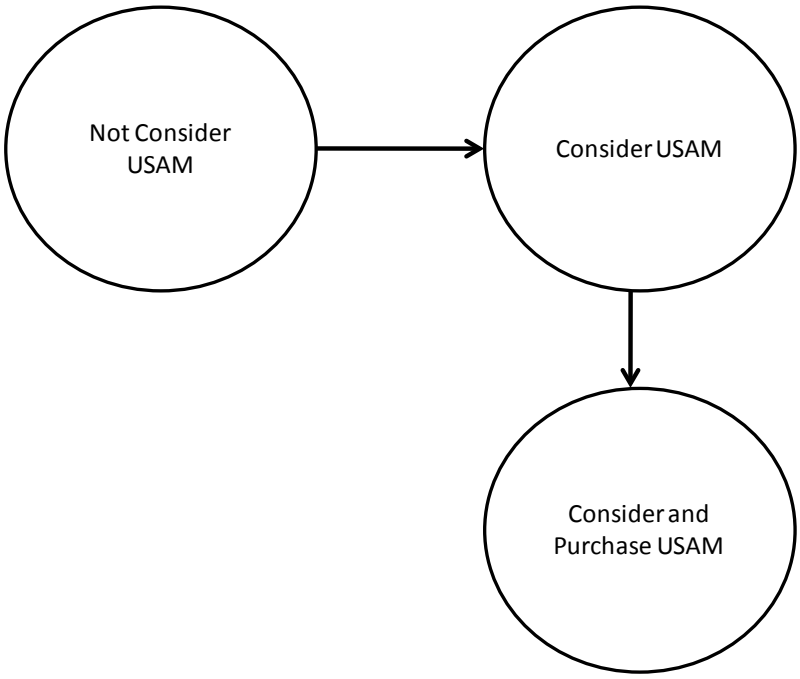


FIGURE 3
CONTINUOUS-TIME MARKOV FLOW DYNAMICS IN EACH PERIOD

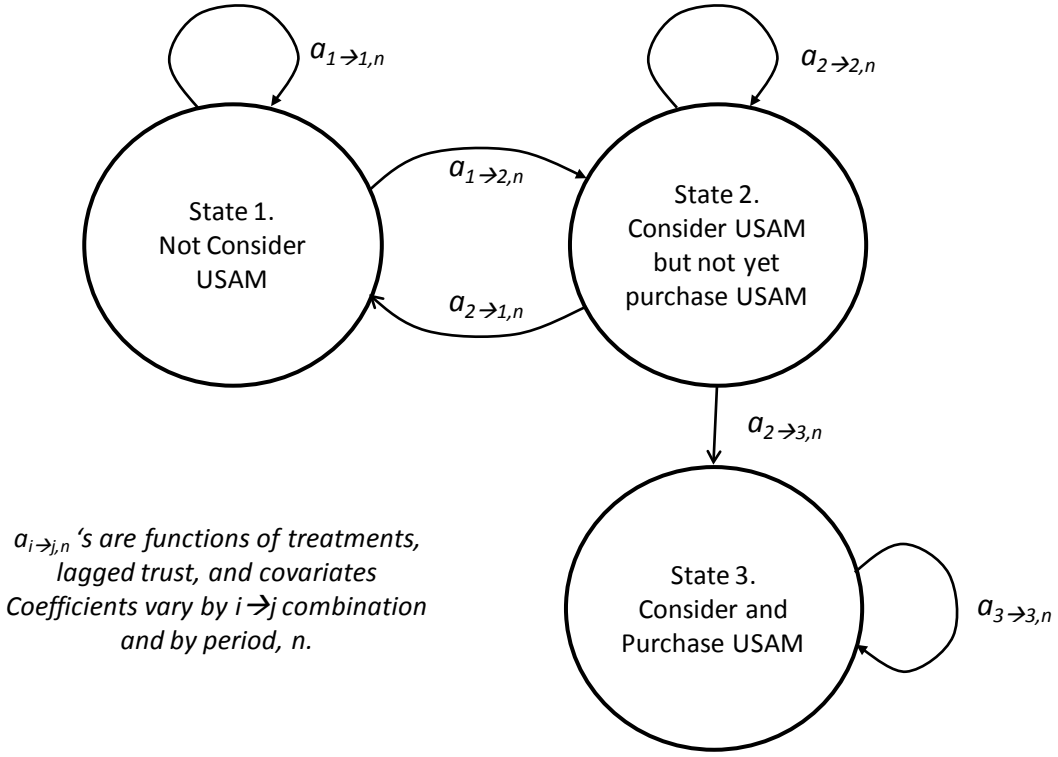
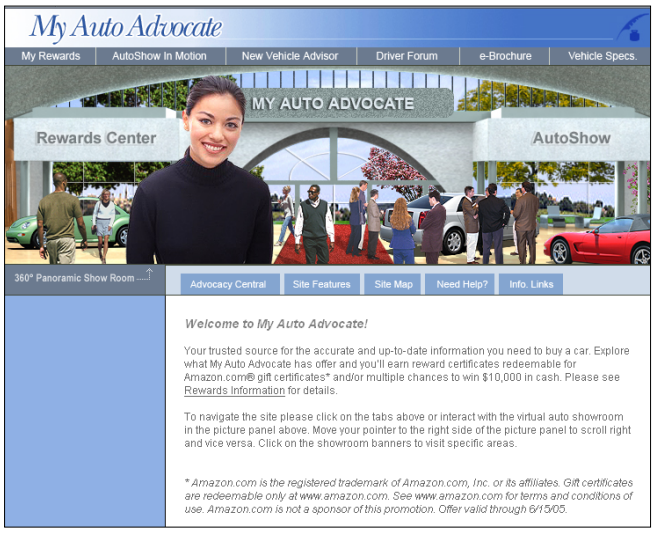
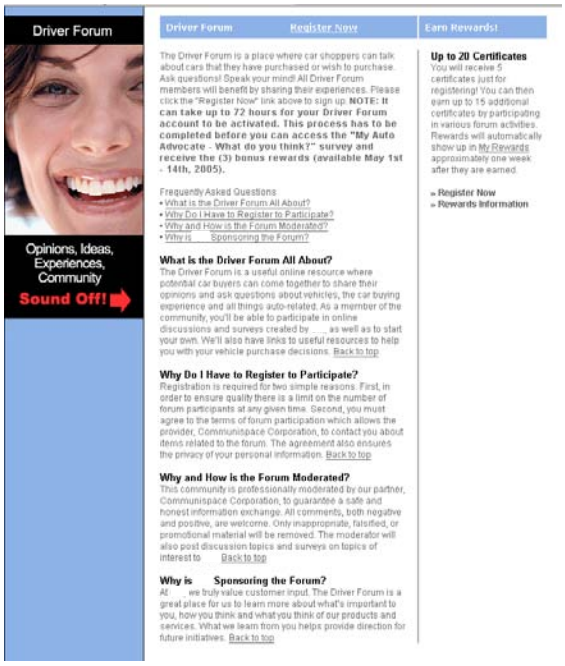


FIGURE 4
YEAR-2 COMPETITIVE-INFORMATION QUASI-EXPERIMENTAL SIMULATED ROLL-OUT

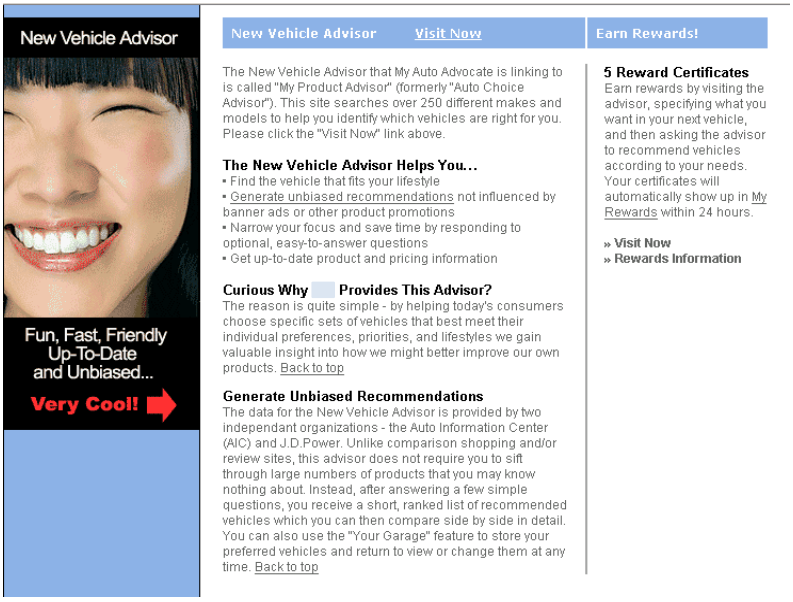


(a) My Auto Advocate Homepage

(b) Competitive E-Brochures



(c) Competitive Community Forum



(d) Competitive New-Vehicle Advisor