Providing Competitive Information to Encourage Trust, Consideration, and Sales: Two Field Experiments

by

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September 2011

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This research was supported by the MIT Sloan School of Management, the Center for Digital Business at MIT (ebusiness.mit.edu), and an anonymous US automobile manufacturer. We gratefully acknowledge the contributions of our industrial collaborators, research assistants, and faculty colleagues: Hunt Allcott, Eric Bradlow, Michael Braun, Luis Felipe Camargo, Daria Dzyabura, Patricia Hawkins, Xitong Li, Natalie Mizik, Nick Pudar, Daniel Roesch, Dmitriy A Rogozhnikov, Joyce Salisbury, Duncan Simester, Catherine Tucker, and Juanjuan Zhang.
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Abstract

An American automaker launched excellent new vehicles but consumers would not consider its vehicles. The automaker sought to build trust and encourage consideration and sales by offering (unbiased) information on competitors’ vehicles. It developed four communications strategies: experiential (competitive test drives), product information (competitive brochures), word-of-mouth (community forums), and online advisors. In year 1, a $2 \times 2 \times 2 \times 2$ field experiment ran for six-months and used random-assignment. In year-2, a simulated-national-rollout field experiment compared an advertising-plus-website opt-in strategy to a no-treatment control. We use main-effect analyses, conditional-logit models, and continuous-time Markov models to analyze the experiments. Analyses consistently suggest that experiential strategies are the most effective, but are mediated through trust (as defined by the automaker). However, contrary to managerial beliefs, opt-in trust signaling did not overcome negative information (community forums and competitive advisors), in part because it did not attract “rejecters.” We evaluate cost-effectiveness and describe the revised successful implementation of competitive-information strategies.

Keywords: Communications, Competitive Information, Consideration, Continuous-time Markov Processes, Electronic Marketing, Field Experiments, Information Search, Mediation, Quasi-experiments, Trust, Trust Signaling
“Other [American] brands were ailing too. Internal marketing surveys showed that many [American] brands didn’t even make the ‘consideration list’ of young shoppers.”

Paul Ingrassia (2010, p. 163)

_Crash Course: The American Automobile Industry’s Road from Glory to Disaster_

**INTRODUCTION AND MOTIVATION**

In 2003-2005 an American automaker (“AAM”) launched vehicles that it believed were more reliable and satisfied consumer needs better than key competitors. Despite better vehicles the automaker failed to gain market share because over half of the consumers in the US (and almost 2/3rd in California) would not even consider its vehicles. By not considering (and not evaluating) its vehicles, consumers never learned that an AAM 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.

The automaker’s managers decided to test an innovative marketing strategy – they would provide (unbiased) information on competitors’ vehicles to their own customers. For the remainder of the paper we call this information “competitive information.” Their goals were multi-faceted. First, they believed that the sheer act of providing competitive information would build trust among customers and cause them to consider the automaker’s vehicles. Second, they believed that a single information source for all automotive manufacturers would make it easier for consumers to evaluate AAM’s new vehicles – and the new vehicles would win many of those competitive evaluations. However, there was a downside. For many years the automaker had not produced superior vehicles; open discussions among consumers, poisoned by experience prior to 2003-2005, might undermine trust.

The automaker tested its competitive-information strategy with two field experiments. In
year 1 they used random-assignment to evaluate the effectiveness of (1) competitive experience, (2) directed brochures, (3) competitive advisors, and (4) competitive word-of-mouth. Respondents were assigned to treatments in a $2 \times 2 \times 2 \times 2$ field experiment that lasted six months. In year 2, they tested whether the strategy could be rolled out nationwide allowing consumers to opt-in to treatments. Managers believed that an opt-in strategy itself would build trust. In a test-vs.-control field experiment they assigned respondents randomly to either an advertising-plus-website opt-in competitive-information treatment or to a no-information control.

In this paper we describe and analyze the two linked field experiments. The year 1 experiments suggest that two of the treatments were effective and two were not. The effective treatments built trust, which, in turn, encouraged consideration and sales (trust mediation). Two treatments did not work because they were hampered by consumer experience with prior year’s vehicles. In year 2, despite prior theory and managerial beliefs, opt-in competitive information did not increase trust, consideration, or sales. Unbiased (and favorable) competitive information is most effective for those who would not otherwise consider the automaker’s vehicles but such “rejecters” were less likely to opt-in, particularly to the information source that was most effective in year 1.

Careful analysis of the two field experiments identified characteristics of competitive-information strategies that are both effective and cost-effective. Revised competitive-information strategies are rifle-like rather than shotgun-like to target “rejecters.” Information is unbiased, but for information sources and vehicle categories in which AAM is superior.

Enhancing trust by providing competitive information is 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 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. The field experiments suggest that when products have recently improved relative to competitive products, the right unbiased competitive comparisons are an important tactic. However, communication strategies should avoid spill-over effects of consumer perceptions based on past quality levels. Opt-in alone is not effective; “rejecters” must be targeted.

By design our field experiments focus on one manufacturer in one industry. Fortunately, the lessons learned in these field experiments have been crucial in the automaker’s return to profitability. To the extent feasible we seek to understand the tested context in the hope that the lessons from the automotive context are important and generalize. As such, field experiments generate hypotheses upon which formal theory and laboratory experiments can elaborate.

At minimum the field experiments suggest refinements to extant theories about the impact of the transparency of information on trust, consideration, and purchase. The right competitive information built trust, but competitive-information *per se* was not sufficient to build trust. This is counter to many articles and the popular press (e.g., Hacker, Willard and Couturier 2002; Urban 2004, 2005). Further, while opt-in may build trust, opt-in was not a panacea and may not always be cost-effective.

In the remainder of the paper we describe and analyze the field experiments. Our analyses are based on three convergent methods of increasing complexity, but with different strengths and weaknesses. The results are generally consistent leading to convergent insight. The first set of analyses is based purely on the experiments. These main-effect analyses are clean with respect to self-selection and unobserved heterogeneity, but do not capture the dynamics over the six
months of the experiments. The second set of analyses rely on conditional logit models that reflect the dynamics of the automotive industry. We test for self-selection and we control for as much unobserved heterogeneity as feasible. These analyses enable us to examine trust mediation through well-established methods. Our third set of analyses use continuous-time Markov process methods. Such models are more complex and somewhat limited in the number of variables that can be in the model, but, because they examine “flows” from unaware to consideration to sales, are less sensitive to heterogeneity in consumers’ prior propensities. We begin by describing the competitive-information treatments.

**COMPETITIVE-INFORMATION EXPERIMENTAL TREATMENTS**

Each of the four experimental treatments seeks to increase consideration (and sales) by providing information, reducing evaluation costs, and building trust. These strategies are consistent with the evaluation-cost model of consideration (Hauser and Wernerfelt 1990). We classify the actions by the generic competitive-information strategy they were designed to test. Treatments were assigned randomly and fully crossed. Each treatment required substantial investments ranging from approximately $150,000 (brochures) to $1 million (test drives).

**Direct Experience with Competitive Products (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. The treatment was 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 reduce evaluation costs and improve perceived benefits when a brand is better than consumers perceive it to be or when the
test drive reduces uncertainty. Competitive test drives also signal that the firm believes in its brand and, thus, should be trusted (Urban 2004).

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**Print and Online Information (Customized Brochures and Competitive Brochures)**

Brand communications (advertising, brochures, sales force, 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, brochures, customized to consumers’ expressed interests, were mailed to consumers, but did not contain competitive information. The glossy brochures provided targeted information about AAM’s vehicles (increased benefits) and lowered the cost of getting that information (Figure 1b). Managers sought to design the brochures so that the personalization would increase trust. In year 2, brochures were less customized, web-based, and included brochures from all competitors.

**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 automotive experimental example in Hauser, Urban, and Weinberg 1993).

At the time of the experiments AAM had recently introduced improved vehicles, but con-
Consumers’ negative experiences with prior, lesser-quality vehicles were also salient. About 20% of the comments about AAM’s vehicles were observed to be negative. Less so for other automakers. Managers believed that enabling word of mouth would signal trust and that the signal would overcome negative comments.

The treatment implemented unbiased word-of-mouth using an online CommuniSpace™ forum in which consumers could participate in over 30 dialogues about both AAM and competitive vehicles (Figure 1c). The community forum was repeated in year 2. By year 2 AAM had 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 AAM’s vehicles. Nonetheless, online advisors match vehicles to consumer needs, lower evaluation costs, and could enhance trust.

The treatment used 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 rebranded, but was mostly similar to year 1. Managers hypothesized that 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.

Investments in the four treatments were substantial. At the time of the experiments man-
agers believed that all four treatments had the potential to be effective, but because a national rollout would require an investment of hundreds of millions of dollars, they wanted to be sure. The four treatments represented four generic strategies because managers believed that the long-term lessons learned would be important to the direction of marketing in the automotive industry.

**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 American 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%. We were unable to obtain exact recruitment rate statistics for year 1, but Harris Interactive estimates an initial recruitment rate of about 40%.

Consumers were assigned randomly to experimental treatments in a $2 \times 2 \times 2 \times 2$ full-factorial field experiment so that various respondents received 0, 1, 2, 3, or 4 treatments. Assignments were random. Although the goal was a 50-50 assignment, the logistics were such that only approximately 40% of the panel members were randomly assigned to competitive test drives. The other treatments were close to 50-50. By design 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.

Managers believed prior quality, service, and consistency let to a lack of trust, which led to a lack of consideration. They believed that sales would increase if consumers were again will-
ing to consider AAM vehicles. “Consideration” was measured with the drop-down menu and “purchase” was a stated purchase measure matched to purchase records. The trust scale was based on prior literature but tailored specifically to the goals as defined by the automotive managers. The scale was defined by five items which exhibited high construct reliability: Cronbach’s $\alpha = 0.95$. The items were “Overall, this company is trustworthy.”, “I believe that this company is willing to assist and support me.”, “Overall, this company has the ability to meet customer needs.”, “This company makes excellent vehicles.”, and “The company is very competent in its dealings with its customers.” For the remainder of the paper we follow the managers’ definition and call the composite score “trust.” Even though the specific items contain elements of customer service and customer-need fulfillment, all items were highly correlated and appear to represent a common construct.

Randomization Tests in Year 1

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. This is consistent with managerial goals; the strategy is to offer a treatment. For completeness, we compared analyses based on treatment assignments to analyses based on self-reported treatments. The pattern of coefficients and their significance was similar for both analyses suggesting that treatment take-up (given it was offered) was sufficiently random that take-up selection had little or no effect in year 1. (Details available from the authors.)

Qualitative data are consistent with the hypothesis that take-up was random and not due to self-selection. For example, some consumers experienced technical difficulties with the competitive online advisor and a few could not come to the competitive test drive due to last-minute
issues. 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.

Although we only use treatment assignment as an independent measure, it is useful to examine further whether take-up was random. Specifically, 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. If there were adverse self-selection, then consumers in group (3) would 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). We find no differences in the dependent measures between groups (1) and (2), thus providing further evidence that take-up was not due to self-selection. For example, measured consideration does not vary between 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 ($t = 1.14, p = .26$).

**MAIN EFFECTS OF TREATMENTS IN YEAR 1**

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

The main effects are summarized in Table 2. The first column is the percent increase in the consumers who are considering or have purchased a AAM vehicle at the end of the experiment. For example, among consumers assigned to competitive test drives, consideration increased by 20.5% relative to consumers who were not assigned to competitive test drives. This difference is significant ($t = 3.6, p < .001$). The treatment-vs.-control lift is not significant for customized brochures, the competitive forum, and the competitive advisor. Brochures 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.

We might posit the effect of competitive information to be either larger or smaller among consumers who own AAM vehicles. It might be larger because current vehicles are improved relative to prior vehicles. It might be smaller because consumers who do not own AAM vehicles have less experience with older, less well-received vehicles. Empirically, the data suggest that there is no interaction effect due to prior AAM ownership implying either that the two effects cancel or that neither is strong. Test drives is the only treatment with a significant impact among AAM 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 in the next section. We find no differential impact due to age or sex. The age-sex comparisons are not shown to simplify the tables.

Main-effect analyses are the simplest and cleanest set of analyses. Heterogeneity and self-selection are mitigated because of randomization. However, we can improve insight by attempting to account for dynamics, interactions among treatments, the conditioning of purchase on consideration, the potential for trust mediation, and heterogeneity.

**DYNAMICS IN THE AUTOMOTIVE INDUSTRY**

**Consideration and Purchase Dynamics**

More detailed analyses (not shown in Table 1) 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. In automotive markets a consumer might come to a competitive test drive in one month, consider and seriously evaluate vehicles in the next month,
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and purchase in still a third month. We would like to model these dynamics.

In addition, managers believed that trust, as they defined it, was a construct that would prove key to untangling the effect of the treatments. They believed that trust would persist. For example, trust among test-drive-assigned consumers increases to 13% after the test drive (period 4) relative to controls, but stays 9% above controls in subsequent periods.

The data make it feasible to model these dynamics. Consumers’ behavior was measured at the end of each of six monthly periods. At the end of each observation period we observe whether consumers had either (1) not yet considered an AAM vehicle, (2) considered a vehicle but not yet purchased, or (3) considered and purchased. (To avoid demand artifacts, AAM was not identified and AAM vehicles occurred throughout the list of potential vehicles.) Because consumers might evaluate and reject a vehicle, they can report no consideration in period \( t \) even if they considered a vehicle in period \( t - 1 \). Once consumers purchase a vehicle we assume they cannot un-consider and un-purchase that vehicle during the six-month observation period.

**Trust and Search Dynamics and Interactions**

**Trust 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 \( t - 1 \) and effects due to the treatments in period \( t \).

**Trust mediation.** To examine trust mediation we consider models that contain trust and contain treatments – both one- and two-stage models. To be consistent with the measurement and
avoid confounding effects, we allow treatments to affect trust but lagged trust to drive consideration and/or purchase. For example, if a treatment occurs in April, then it affects trust as measured at the end of April. The trust at the end of April then affects consideration and sales in May. (We later examine a more-continuous model.)

**Interactions.** 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. In light of this prior research in automotive information-search, we examine models that allow interactions.

**Heterogeneity of Response**

Although the treatments were randomized, consumers with different purchase histories and different demographics (age and sex) might react differently. For example, consumers who now own AAM vehicles may base their trust, consideration, or purchase on their prior ownership experience. To capture purchase history, we 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 in the propensity to trust, consider, or purchase an AAM vehicle would be to compute period-to-period differences in trust, consideration and purchase. But such an analysis would implicitly assume (a) no decay and (b) perfect reliability of repeated measures. With repeated noisy measures, the best estimate of the true score at \( t \) is not the score at \( t - 1 \), but rather a function of reliability times the lagged score (Nunnally and Bernstein [1994, 222]). While we can never rule out unobserved heterogeneity completely, we can examine whether unobserved heterogeneity is likely to be a major effect or a second-order effect. Specifically, (a) a coefficient close to one in a
trust regression (0.833, yet to be shown), (b) explicit controls for observable heterogeneity, and (c) consistency with the main-effect analyses (yet to be shown) suggests that any effects due to unobserved heterogeneity are unlikely to reverse the primary insights.

**MODELING DYNAMICS 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 AAM vehicles, whether the treatments also affect purchase.

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 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 AAM, 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. The purchase analysis is conditioned on consideration – only those res-
pondents who consider AAM in a period are included when estimating the purchase logit. Thus, the effect on purchase is incremental above and beyond the effect on consideration. (Standard errors available upon request.)

Both the consideration and conditional-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, Hauser 1978.) Consideration is increased if consumers own AAM or other American vehicles and decreased if they own Japanese 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 AAM’s strategy. 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). 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 competitive information treatments were mediated through trust. 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 models. If the treatments are mediated through trust, then (1) the indicator of trust should be significant in the new models 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. We use lagged trust because it makes sense given the dynamics of measurement and causality. Lagged trust has the added benefit that joint causality in measurement errors is reduced because the trust measures occur in different periods than the consideration and purchase measures. Nonetheless, to account for unobserved shocks that affect trust in period $t - 1$ and consideration (purchase) in period, $t$, we use estimated lagged trust in an equation that predicts consideration (purchase). Traditional mediation analyses use lagged trust directly. In our data, these tests also indicate mediation and have similar managerial implications. The analyses are available upon request.

In summary, estimated trust in period $t - 1$ is a function of the treatments in period $t - 1$ and trust at the end of period $t - 2$. See the last column of Table 3. Change in consideration (purchase) is a function of estimated trust at the end of period $t - 1$ and the treatments in period $t$. (Change is implemented with the lagged structure.) See the third through sixth columns of parameters in Table 3. In other words, the treatments that affect lagged trust occur in a different period than the treatments that affect consideration (purchase) directly.

We first examine the trust regression. Competitive test drives clearly increase trust and there is evidence that customized brochures increase trust. The impact of customized brochures is consistent with published studies of customization (e.g., Ansari and Mela 2003; Hauser, et al. 2010). The effect of customized brochures was less apparent in the main-effects analyses because, although the effect was strong in earlier periods, it decayed to become insignificant in the last period. The trust regression picks up growth and decay. Consistent with the main-effect analyses, the conditional-logit analyses and the trust regression identify no impact for the communi-
ty forum and the competitive advisor. This result conflicts with prior managerial beliefs (and popular beliefs in the trust literature); our analyses suggest that the signal of trust was not sufficient to overcome negative information in the community forum and 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 parameter estimates and standard errors are based on 1,000 bootstrap replicates. (Table 3 reports significance; standard errors available upon request.)

Following Baron and Kenny (1986) the treatments are mediated through trust if: (a) including lagged trust in the model increases fit significantly (and the lagged trust variable is significant) and (b) the treatments are no longer significant when estimated lagged trust is in the model. The increase is significant for consideration and marginally significant for purchase ($\chi^2_1 = 86.7, p < .001$, $\chi^2_1 = 3.1, p = 0.08$, respectively.). Once we partial out lagged trust, there remain no significant direct effects due to the treatments. This suggests trust mediation.

**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 crossed prior ownership of an AAM vehicle with the treatment-assignment dummies. For trust, consideration, and purchase the interactions are not significant ($F = 1.91, p = .11; \chi^2_4 = 4.3, p = .37, \chi^2_4 = 7.0, p = .13$, respectively).

We also tested interactions among the treatments. Treatment interaction-effects do not add significantly to a trust regression using a specification that allows all interactions ($F = .85, p$
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 ($\chi^2_{11} = 17.1, p = .09$). 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 earlier automotive studies (e.g., 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 AAM ownership interactions or before ($\chi^2_1 = .6, p = .42$, $\chi^2_1 = .6, p = .46$ for consideration and $\chi^2_1 = .1, p = .75$, $\chi^2_1 = .03, p = .85$ for purchase). Neither the fully-saturated nor the parsimonious analysis highlights any managerially-insightful interactions. 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 main-effect analyses.

- competitive information in the form of test drives has a significant effect on consideration and (likely) purchase given consideration
- word of mouth (competitive forum) and competitive advisors did not increase trust, consideration, or purchase. The positive trust signal did not appear sufficient to overcome the negative information in these treatments.

The conditional-logit analyses provide further insight:

- the effect of test drives is mediated through lagged trust (as defined by AAM),
- customized brochures increase trust and, though lagged trust, may increase consideration
and conditional purchase. However, the effect decays by the last period.

- prior ownership does not interact with the treatments and there are no managerially relevant interactions among the 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 advertising-plus-website opt-in experiment.

**CONTINUOUS-TIME MARKOV ANALYSIS IN YEAR 1**

The conditional-logit analyses improve and clarify the main-effect analyses, but they do not capture the continuous dynamics of the automotive market. Conditional-logit analyses capture “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 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). There are two added advantages of Markov analyses: (a) a single likelihood function estimates all parameters for all defined flows simultaneously and (b) treatments affect differences in behavioral states directly. By the Markov property, observing a customer in “consider” in period $t$ is treated differently if the customer was in “not consider” versus in “consider” at time $t - 1$. By focusing on differences in behavioral states the Markov analyses are less sensitive to prior propensities to consider AAM vehicles (and hence heterogeneity in that propensity).

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Insert Figure 3 about here.

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The Markov analyses complement the conditional-logit analyses in Figure 2; the concepts are similar but we model a more-complete set of flows and allow the flows to occur in conti-
nuous 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 $\Delta t$ 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:

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

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 analyses 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 conditional-logit analyses. (We do not need lagged consideration in the Markov analyses because the analyses are 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.

The Markov analyses reinforce the conditional-logit and main-effect analyses. Competitive test drives have a significant effect on consideration, but that effect is likely mediated through lagged trust. Lagged 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 consider” as in Jackson, et al. (2003). The misclassification analyses improved fit, but provided no additional managerial insights. Estimated misclassification was moderate.

**YEAR 2 – FIELD TEST OF ADVERTISING-PLUS-SITE OPT-IN**

The year-2 experiments sought to test the feasibility of a national launch. Prior to the year-2 experiment, management believed that a cost-effective national launch would require a communications strategy in which advertising brings consumers to a website from which consumers could opt-in to treatments. Although consumers who already trust AAM would be more likely to opt-in, AAM’s managers hoped that enough not-yet-trusting consumers would opt-in to make the strategy cost-effective. We call these not-yet-trusting consumers “rejecters.” Given the magnitude of the investment necessary for a national rollout, a rigorous test of the opt-in strategy was necessary.

Consumers were assigned 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. At the “My Auto
Advocate” website, both the opt-in test group and the forced-exposure test group could select
any combination of five treatments. The opt-in test group was a surrogate for a national opt-in
strategy. Combining the opt-in and the forced-exposure test groups served as a surrogate for
more-substantial incentives to encourage consumers to visit the “my Auto Advocate” website
during the national rollout.

The year-2 panel ran monthly from January to June, 2005. Members of Harris Interac-
tive’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. Simulating a national
rollout, this 2.2-year average was designed to draw in more consumers (relative to the year-1
twelve-month intenders). Once consumers visited the “My Auto Advocate” website they were
given incentives to opt-in to the treatments. (The incentives were comparable to expected nation-
al-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 com-
pleted 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, purchase, and trust in year 2 were measured as in year 1.
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**Treatments in Year 2**

Three of the treatments in year 2 were similar to year 1. The competitive-test-drive treatment and the word-of-mouth 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 brochures. Year 2 used electronic brochures for AAM vehicles (called eBooklets). They were online or downloadable, not mailed, and were less customized. An additional treatment, eBrochures, allowed consumers to download competitive brochures. Although many competitive brochures were available on automakers’ websites, the single-source webpage 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.

**Was the Opt-in Strategy Effective?**

We first examine consideration and purchase in the test (opt-in and forced-exposure) vs. the control groups. There were no significant differences in either consideration or purchase intentions ($t = .45, p = .65$ and $t = .18, p = .86$, respectively). Similarly, the differences between opt-in and control were not significant ($t = .07, p = .95$ and $t = .05, p = .96$, respectively). These results suggest that the advertising-plus-website communications strategy was not cost-effective; it provided little or no lift in consideration and purchase relative to the control.

There are at least two explanations for the null effect in year 2. First, the null effect may be due to self-selection. “Rejecters” could have been less likely to opt-in to the treatments, even
with forced-exposure to the website. If this is the case, self-selection was a critical managerial finding with more-general implications—a successful national rollout would require that AAM identify a cost-effective means to target “rejecters” when they implement a competitive-information marketing strategy. The second (and complementary) explanation is that the test group merged effective treatments (test drives and possibly brochures) with potentially-negative treatments (word-of-mouth and online advisors). This was reinforced because the take-up of test drives was low compared to the other treatments. If this is the case, then one implication complements the self-selection insight: test drives require greater incentives to attract “rejecters.” The other more-general implication is that unbiased information that is not favorable should not be encouraged (word-of-mouth and online advisors). Recall that, prior to the experiments, managers believed that providing any unbiased information would signal trust and enhance consideration and conditional purchase. They also believed that opt-in would attract “rejecters.”

To test self-selection 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 opt-in to any treatments. Among these no-treatment consumers, the non-treated members of the test group had significantly lower consideration and purchase intentions than the control group ($t = 2.2, p = .03$, $t = 2.1, p = .04$, respectively). Because consumers who had the opportunity to opt-in and chose not to opt-in had lower consideration and purchase intentions, the opt-in consumers were those “non-rejecters” who were otherwise more likely to consider or purchase AAM vehicles (or at least vehicles in general). Comparing the control group to non-treated members of the forced-exposure test group gives similar results. The opt-in nature of the communications strategy ended up targeting consumers more likely to be “non-rejecters.” It tended not to reach “rejecters.”
The Effects of Competitive Information in Year 2, with Caveats

To examine the second implication (positive vs. negative information), we attempt to parse the effect of the treatments. This can only be done with caveats. The year-1 experiments are clean with respect to self-selection. The opt-in analyses make it clear that we can never fully disentangle self-selection from true effects in the year-2 data. With these caveats in mind, we cautiously repeat the main-effect, conditional-logit, and continuous-time-Markov-process analyses for year 2. The details are in an appendix.

Consistent with opt-in contamination, the main-effect analyses suggest that all competitive-information treatments increase either consideration or purchase. (This is true even when we limit the test group to the forced-exposure consumers.) When we account for dynamics, persistence, heterogeneity, prior-ownership effects, and unobserved external shocks with conditional-logit analyses, significant direct effects are limited to experiential test drives and competitive eBrochures. Even with opt-in self-selection, the competitive forum and the competitive advisor do not affect trust, consideration, and purchase. (Note that a opt-in effect, common across treatments, is merged into the constant; only relative opt-in effects are confounded with the treatment effects.) There are hints that eBrochures might be effective, but we cannot be sure without additional data or managerial judgment. Finally, 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.

Summary of the Year-2 Advertising-plus-Website Opt-in Communications Experiment

In year 1 competitive test drives and customized brochures increased trust among randomly chosen consumers. The increased trust drove consideration and conditional purchase. Despite opt-in contamination, the year-2 conditional-logit analyses are consistent with the year-1 experiments. Test drives are effective; competitive advisors and forums are not. Competitive
brochures, not tested in year 1, may also increase trust.

The new important insight from year 2 is that an advertising-plus-website opt-in strategy does not always increase consideration and purchase. As implemented, the opt-in strategy did not appear to reach consumers who are skeptical about AAM vehicles—exactly those consumers management most wants to reach. If a competitive-information strategy is to be successful in a national rollout, it must be targeted more-efficiently to those consumers most likely to change their behavior and it must emphasize those sources of unbiased information that are not contaminated with consumer experience from prior years.

**INTERPRETATIONS**

Many authors champion trust-based strategies (e.g., Hacker, Willard and Couturier 2002; Urban 2004, 2005). Popular belief is that if firms build trust with consumers, trust will cause consideration and (repeat) purchase. This philosophy suggests further that any communications strategy, that signals that a firm is willing to lay bare its strengths and weaknesses relative to competition, will build trust. The situation in the automotive industry in 2003-2005 provided an excellent test of these theories. Because of past experiences with AAM’s vehicles, many consumers would not even consider those vehicles in 2003-2005. Because 2003-2005 vehicles had improved relative to prior vehicles, AAM had an opportunity to build trust. Managers had good news to tell consumers. Under popular theories, all four generic competitive-information 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 AAM, 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. The experiments suggest that the experiential
strategy (competitive test drives) was the most effective communications strategy, especially in year 1. Tangible experience convinced consumers that AAM’s vehicles had improved relative to competition. Subject to the stated caveats, there is also a suggestion in year 2 that eBrochures were effective in building trust, at least relative to the other treatments. Qualitative data and AAM’s managerial judgment reinforced this belief. The communications strategies appear to have been successful because competitive test drives and eBrochures provided consumers with an unbiased comparison in which AAM 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 relied in part on past consumer experience and may have lagged any improvement in vehicles. (These advisors use Bayesian methods based on prior preferences.) It appears that providing unbiased competitive information sometimes builds trust, but the pure signal of providing competitive information is not sufficient to overcome believable negative information. While this may seem obvious ex post, it was far from obvious a priori. AAM’s managers believed that openness and transparency alone would engender trust. We summarize what we learned from two years of experimentation.

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, opt-in strategies are not sufficient. Strong incentives or targeted communications are necessary to encourage consumers to use competitive information.

Naturally, this summary is subject to tests in different categories, with different implementations.
of the generic communications strategies, and with other targeting or opt-in strategies.

**COST EFFECTIVENESS AND MANAGERIAL IMPLICATIONS**

AAM ultimately implemented competitive-information strategies, but not immediately following the 2003-2005 experiments. The following calculations illustrate the motivation behind managers’ decisions at the time. We disguise the proprietary data by using comparable publicly available data. It is a good approximation to AAM’s cost-benefit analyses.

For illustration we assume a 15% market share. Based on this share, competitive test drives provide an 11.1% sales lift (year-1 data) with an approximate cost of $120 per participating consumer (with incentives). These calculations suggest that the cost of an incremental vehicle sale is approximately $7,200. \[\text{\$7,207} = \frac{\$120}{(0.15 \times 0.111)}\]. 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 of a competitive test drive. As implemented in the year-1 randomized experiments, competitive test drives are not profitable. On the other hand, combining managerial judgment with a cautious use of the year-2 data suggest that competitive eBrochures provide a positive payback, even if managerial judgment of the estimated sales lift is off by a factor of 10 or more.

Large-scale competitive information strategies were put on hold during the distractions of the automotive and financial crises of 2005-2009. A multi-city competitive test-drive format was neither feasible nor cost-effective. Meanwhile the concept of competitive test drives gained traction in situations where they could be implemented cost efficiently and targeted at skeptical consumers. When the financial situation improved, AAM 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. AAM continued to experiment with competitive test drives in key local markets when high-value skeptical consumers (“rejecters”) could be targeted cost-effectively. In late 2010 the head of US marketing for AAM launched a yearlong series of weekend competitive test drives at dealerships. Each event invited a few thousand potential buyers to compare AAM vehicles with competitive vehicles. In 2011 AAM launched targeted multi-city competitive test drives using the insights from the field experiments.

Year 2 taught managers that adverse selection must be managed in an opt-in communications strategy. Managers now place a premium on targeting competitive information toward skeptical consumers. For example, managers are 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 rejecters. Targeted consumers would get substantial incentives (hundreds of dollars) to participate.

Based on the 2003-2005 data and managerial judgment, managers believe that providing information from competitive eBrochures is cost effective. AAM 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, AAM used a national advertising campaign that encouraged consumers to compare AAM’s vehicles to competitors on good fuel economy and styling. Many AAM dealers offer unsolicited extended weekend test drives and encourage competitive comparisons. AAM believes that competitive information
builds trust, consideration, and sales and is profitable, but only if implemented cost effectively to skeptical consumers for categories in which AAM 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.
REFERENCES


Competitive Information Field Experiments

Psychology, 7, 2, 131-157.


### TABLE 1
CONSUMERS RANDOMLY ASSIGNED TO TREATMENTS IN YEAR 1

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Period 2</th>
<th>Period 3</th>
<th>Period 4</th>
<th>Period 5</th>
<th>Period 6</th>
<th>Treatment Cell</th>
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<td>124</td>
<td>0</td>
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<td>No</td>
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<td>317</td>
<td>193</td>
<td>317</td>
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<td>193</td>
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<tr>
<td>Customized Brochures</td>
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<td>164</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>164</td>
</tr>
<tr>
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<td>153</td>
<td>317</td>
<td>317</td>
<td>317</td>
<td>153</td>
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<tr>
<td>Competitive Forum</td>
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<td>151</td>
<td>151</td>
<td>151</td>
<td>0</td>
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<td>166</td>
<td>166</td>
<td>166</td>
<td>317</td>
<td>166</td>
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<tr>
<td>Competitive Advisor</td>
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<td>156</td>
<td>156</td>
<td>156</td>
<td>156</td>
<td>156</td>
</tr>
<tr>
<td>No</td>
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<td>161</td>
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### TABLE 2
MAIN-EFFECT ANALYSES

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Consideration (% lift in last period)</th>
<th>Purchase (% cumulative lift)</th>
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<tbody>
<tr>
<td>Competitive Test Drives</td>
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<td>11.1% *</td>
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<td>Customized Brochures</td>
<td>-2.9%</td>
<td>4.8%</td>
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<tr>
<td>Competitive Forum</td>
<td>-2.4%</td>
<td>3.3%</td>
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<tr>
<td>Competitive Advisor</td>
<td>0.5%</td>
<td>-4.4%</td>
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Treatment Among Non-AAM-Owners

<table>
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<tr>
<th>Treatment</th>
<th>Consideration (% lift in last period)</th>
<th>Purchase (% cumulative lift)</th>
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</thead>
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<tr>
<td>Competitive Test Drives</td>
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<td>7.3%</td>
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<tr>
<td>Customized Brochures</td>
<td>2.2%</td>
<td>5.0%</td>
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<tr>
<td>Competitive Forum</td>
<td>1.1%</td>
<td>6.1%</td>
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<tr>
<td>Competitive Advisor</td>
<td>2.0%</td>
<td>-0.9%</td>
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### TABLE 3
CONDITIONAL-LOGIT ANALYSES AND TRUST REGRESSION – YEAR 1 RANDOM ASSIGNMENTS

Conditional-Logit Analyses (five periods, 317 respondents for consideration model, only those who consider for conditional-purchase model)

<table>
<thead>
<tr>
<th>Dependent Measure</th>
<th>Direct Effects Not Mediated</th>
<th>Mediated by Trust (bootstrap estimates)</th>
<th>Trust Regression</th>
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<tr>
<td></td>
<td>Consider</td>
<td>Purchase if Consider</td>
<td>Consider</td>
</tr>
<tr>
<td>Constant</td>
<td>-1.492*</td>
<td>-2.567*</td>
<td>-3.945*</td>
</tr>
<tr>
<td>Lagged Consider</td>
<td>2.537*</td>
<td>2.394*</td>
<td>2.405*</td>
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<tr>
<td>Lagged Trust Hat</td>
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<td></td>
<td>.531*</td>
</tr>
<tr>
<td>Competitive Test Drives</td>
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<td>.938†</td>
<td>.392</td>
</tr>
<tr>
<td>Customized Brochures</td>
<td>.079</td>
<td>.477</td>
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<td>Competitive Forum</td>
<td>-.023</td>
<td>-.103</td>
<td>.136</td>
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<td>Competitive Advisor</td>
<td>.144</td>
<td>.122</td>
<td>.133</td>
</tr>
<tr>
<td>Prior Own AAM</td>
<td>.399*</td>
<td>.137</td>
<td>.327†</td>
</tr>
<tr>
<td>Prior Own Other American</td>
<td>.304*</td>
<td>-.005</td>
<td>.253†</td>
</tr>
<tr>
<td>Prior Ownership Japanese</td>
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<td>-.188</td>
<td>-.464*</td>
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<tr>
<td>Period 3</td>
<td>.313</td>
<td>.200</td>
<td>.461*</td>
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<tr>
<td>Period 4</td>
<td>.419†</td>
<td>.264</td>
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<td>Period 5</td>
<td>.523*</td>
<td>-.238</td>
<td>.390†</td>
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<tr>
<td>Period 6</td>
<td>.722*</td>
<td>.185</td>
<td>.654*</td>
</tr>
</tbody>
</table>

Prior Ownership of AAM crossed with
- Competitive Test Drives
- Customized Brochures
- Competitive Forum
- Competitive Advisor

| Two or more treatments             | .208                        | -.169                                  | 1.612            | -.116                | -.966 |
| Log likelihood                     | -820.6                      | -218.2                                 | -777.2           | -774.8               | -216.6 | -213.1 | adjusted-R²² |
| U² (aka pseudo-R²)                 | 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

<table>
<thead>
<tr>
<th>Continuous Time Markov Estimation</th>
<th>Not Mediated</th>
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<th>Mediated by Trust</th>
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<tr>
<td>Consider to Consider (1→2)</td>
<td>Consider to Not Consider (2→1)</td>
<td>Consider to Purchase (2→3)</td>
<td>Consider to Not Consider (1→2)</td>
<td>Consider to Not Consider (2→1)</td>
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<td>Constant</td>
<td>.139</td>
<td>.231</td>
<td>.120 *</td>
<td>.146 *</td>
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<tr>
<td>Lagged Trust Hat</td>
<td>.221 *</td>
<td>-.230 †</td>
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<td><strong>Competitive Test Drives</strong></td>
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<td>Prior Ownership of AAM</td>
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<td>Prior Own Other American</td>
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<td>.070</td>
<td>**.525 ***</td>
<td>.039</td>
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<td>Prior Ownership of Japanese</td>
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<td>Period 3</td>
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<td>.445</td>
<td>.032</td>
</tr>
<tr>
<td>Period 4</td>
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<td>.544</td>
<td>-.394</td>
<td>**-1.004 ***</td>
</tr>
<tr>
<td>Period 5</td>
<td>**-.698 ***</td>
<td>.122</td>
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<td>**-.760 ***</td>
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<td>Log likelihood</td>
<td>-616.46</td>
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<td>-608.12</td>
<td></td>
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</tbody>
</table>

* Significant at the 0.05 level. † Significant at the 0.10 level. All flows are estimated simultaneously.
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FIGURE 1
YEAR-1 (RANDOM ASSIGNMENT) COMPETITIVE-INFORMATION TREATMENTS

(a) Competitive Test Drive
(b) Customized Brochures
(c) Competitive Online Advisor
(d) Competitive Community Forum
FIGURE 2
CONSIDERATION AND PURCHASE DYNAMICS: CONDITIONAL-LOGIT ANALYSES

FIGURE 3
CONTINUOUS-TIME MARKOV FLOW DYNAMICS IN EACH PERIOD

\[ a_{1 \rightarrow 1,n}, a_{1 \rightarrow 2,n}, a_{2 \rightarrow 2,n}, a_{2 \rightarrow 3,n} \] are functions of treatments, lagged trust, and covariates. Coefficients vary by \( i \rightarrow j \) combination and by period, \( n \).
FIGURE 4
YEAR-2 ADVERTISING-PLUS-WEBSITE OPT-IN SIMULATED ROLLOUT

(a) My Auto Advocate Homepage

(b) Competitive E-Brochures

(c) Competitive Community Forum

(d) Competitive New-Vehicle Advisor

Competitive Information Field Experiments

VIEW eBOOKLET

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### APPENDIX: YEAR 2 ANALYSES (SUBJECT TO CAVEATS ON SELF-SELECTION)

#### TABLE A1: CONSUMERS WHO SELECTED TREATMENTS IN YEAR 2
(Advertising-then-Website Simulated National Rollout)

*Number of respondents who selected the indicated treatment in that period*

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Period 2</th>
<th>Period 3</th>
<th>Period 4</th>
<th>Period 5</th>
<th>Period 6</th>
<th>Treatment “Cell”</th>
</tr>
</thead>
<tbody>
<tr>
<td>Competitive Test Drives</td>
<td>Opt-in</td>
<td>70</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>70</td>
</tr>
<tr>
<td></td>
<td>Forced</td>
<td>140</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>140</td>
</tr>
<tr>
<td></td>
<td>Not Treated</td>
<td>1,182</td>
<td>1,322</td>
<td>1,322</td>
<td>1,322</td>
<td>1,182</td>
</tr>
<tr>
<td>Competitive eBrochures</td>
<td>Opt-in</td>
<td>88</td>
<td>178</td>
<td>411</td>
<td>425</td>
<td>432</td>
</tr>
<tr>
<td></td>
<td>Forced</td>
<td>149</td>
<td>361</td>
<td>411</td>
<td>425</td>
<td>432</td>
</tr>
<tr>
<td></td>
<td>Not Treated</td>
<td>1,173</td>
<td>961</td>
<td>911</td>
<td>897</td>
<td>890</td>
</tr>
<tr>
<td>AAM eBooklets</td>
<td>Opt-in</td>
<td>49</td>
<td>184</td>
<td>194</td>
<td>205</td>
<td>252</td>
</tr>
<tr>
<td></td>
<td>Forced</td>
<td>114</td>
<td>355</td>
<td>411</td>
<td>417</td>
<td>438</td>
</tr>
<tr>
<td></td>
<td>Not Treated</td>
<td>1,208</td>
<td>967</td>
<td>911</td>
<td>905</td>
<td>884</td>
</tr>
<tr>
<td>Competitive Forum</td>
<td>Opt-in</td>
<td>71</td>
<td>139</td>
<td>168</td>
<td>194</td>
<td>208</td>
</tr>
<tr>
<td></td>
<td>Forced</td>
<td>114</td>
<td>294</td>
<td>352</td>
<td>409</td>
<td>420</td>
</tr>
<tr>
<td></td>
<td>Not Treated</td>
<td>1,208</td>
<td>1,028</td>
<td>970</td>
<td>913</td>
<td>902</td>
</tr>
<tr>
<td>Competitive Advisor</td>
<td>Opt-in</td>
<td>97</td>
<td>180</td>
<td>206</td>
<td>226</td>
<td>246</td>
</tr>
<tr>
<td></td>
<td>Forced</td>
<td>199</td>
<td>378</td>
<td>441</td>
<td>493</td>
<td>535</td>
</tr>
<tr>
<td></td>
<td>Not Treated</td>
<td>1,123</td>
<td>944</td>
<td>881</td>
<td>829</td>
<td>787</td>
</tr>
</tbody>
</table>

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

#### TABLE A2

**MAIN-EFFECT ANALYSES FOR ADVERTISING-PLUS-WEBSITE OPT-IN EXPERIMENT**

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Consideration (% lift in last period)</th>
<th>Purchase (% cumulative lift)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Competitive Test Drives</td>
<td>6.6%</td>
<td>5.3% *</td>
</tr>
<tr>
<td>Competitive eBrochures</td>
<td>8.5% *</td>
<td>3.3% *</td>
</tr>
<tr>
<td>AAM eBooklets</td>
<td>8.6% *</td>
<td>4.9% *</td>
</tr>
<tr>
<td>Competitive Forum</td>
<td>7.7% *</td>
<td>1.5%</td>
</tr>
<tr>
<td>Competitive Advisor</td>
<td>6.3% *</td>
<td>2.6%</td>
</tr>
</tbody>
</table>

**Treatment Among Non-AAM-Owners**

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Consideration (% lift in last period)</th>
<th>Purchase (% cumulative lift)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Competitive eBrochures</td>
<td>5.2%</td>
<td>6.6% *</td>
</tr>
<tr>
<td>AAM eBooklets</td>
<td>7.7% *</td>
<td>2.8% *</td>
</tr>
<tr>
<td>Competitive Forum</td>
<td>8.2% *</td>
<td>3.5% *</td>
</tr>
<tr>
<td>Competitive Advisor</td>
<td>8.5% *</td>
<td>3.1% *</td>
</tr>
<tr>
<td>Competitive eBrochures</td>
<td>6.6% *</td>
<td>2.4% *</td>
</tr>
</tbody>
</table>
## TABLE A3
### CONDITIONAL-LOGIT ANALYSES AND TRUST REGRESSION – YEAR 2 ADVERTISING-PLUS-WEBSITE OPT-IN

<table>
<thead>
<tr>
<th>Dependent Measure</th>
<th>Direct Effects not Mediated</th>
<th>Mediated by Trust (bootstrap estimates)</th>
<th>Trust Regression</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Consider</td>
<td>Purchase Given Consideration</td>
<td>Consider</td>
</tr>
<tr>
<td>Constant</td>
<td>-2.042 *</td>
<td>-3.034 *</td>
<td>-4.933 *</td>
</tr>
<tr>
<td>Lagged Consider</td>
<td>2.668 *</td>
<td>2.460 *</td>
<td>2.463 *</td>
</tr>
<tr>
<td>Lagged Trust Hat</td>
<td>.783 *</td>
<td>-.025</td>
<td>.804 *</td>
</tr>
<tr>
<td>Competitive Test Drives</td>
<td>.235 †</td>
<td>.473 †</td>
<td>.118</td>
</tr>
<tr>
<td>Competitive eBrochures</td>
<td>.019</td>
<td>-.214</td>
<td>-.022</td>
</tr>
<tr>
<td>AAM eBooklets</td>
<td>.085</td>
<td>-.177</td>
<td>.110</td>
</tr>
<tr>
<td>Competitive Forum</td>
<td>-.044</td>
<td>.209</td>
<td>-.009</td>
</tr>
<tr>
<td>Competitive Advisor</td>
<td>1.349 *</td>
<td>.879 *</td>
<td>1.033 *</td>
</tr>
<tr>
<td>Prior Own of AAM</td>
<td>.122 †</td>
<td>.018</td>
<td>.023</td>
</tr>
<tr>
<td>Prior Own of Japanese</td>
<td>-.419 *</td>
<td>-.133</td>
<td>-.293 *</td>
</tr>
<tr>
<td>Period 3</td>
<td>-.094</td>
<td>-.386</td>
<td>-.095</td>
</tr>
<tr>
<td>Period 4</td>
<td>-.001</td>
<td>-.665 *</td>
<td>.005</td>
</tr>
<tr>
<td>Period 5</td>
<td>.027</td>
<td>-.864 *</td>
<td>.021</td>
</tr>
<tr>
<td>Period 6</td>
<td>.201 *</td>
<td>-.740 *</td>
<td>.198 †</td>
</tr>
<tr>
<td>Prior Own of AAM crossed with</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Competitive Test Drives</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Competitive Brochures</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>AAM eBooklets</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Competitive Forum</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Competitive Advisor</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Two or more treatments</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Log likelihood</td>
<td>-2836.9</td>
<td>-469.8</td>
<td>-2701.3</td>
</tr>
<tr>
<td>$U^2$ (aka pseudo-$R^2$)</td>
<td>38.1%</td>
<td>70.3%</td>
<td>41.0%</td>
</tr>
</tbody>
</table>

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