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Competitive information, trust, brand consideration and sales: Two field experiments ☆

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ABSTRACT

Two field experiments examine whether providing information to consumers regarding competitive products builds trust. Established theory suggests that (1) competitive information leads to trust because it demonstrates the firm is altruistic, and (2) trust leads to brand consideration and sales. In year 1, an American automaker provided experiential, product-feature, word-of-mouth, and advisor information to consumers in a $2\times2\times2\times2$ random-assignment field experiment that lasted six months. Main-effect analyses, conditional-logit models, and continuous-time Markov models suggest that competitive information enhances brand consideration and possibly sales and that the effects are mediated through trust. However, in a modification to extant theory, effects are significant only for positively valenced information. The year-2 experiment tested whether a signal that the firm was willing to share competitive information would engender trust, brand consideration, and sales. Contrary to many theories, the signal did not achieve these predicted outcomes because, in the year-2 experiment, consumers who already trusted the automaker were more likely to opt in to competitive information. In addition to interpreting the field experiments in light of extant theory, we examine cost effectiveness and describe the automaker's successful implementation of revised competitive-information strategies.

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1. Introduction and motivation

In 2003–2005, an American automaker ("AAM") faced a situation common among firms that had once dominated their industries. New competitors had entered with products perceived to be higher in quality and better matched to consumer needs. The automaker's brands no longer connoted quality or innovation; brand strength had declined. By 2003, over half of the consumers in the U.S. (and almost two-thirds in California) would not even consider AAM brands; its brands were effectively competing for only a fraction of the market. Ingrassia (210, p. 163) cites the lack of brand consideration as a cause of the decline of American brands. Although the automaker had recently invested heavily in design and engineering, the automaker would never again regain strength nor market share unless its brands could earn consumers' brand consideration.

The situation of AAM was similar to that faced by many once-dominant brands. After the financial crises of 2008, consumers were hesitant to consider and purchase financial services from established firms such as New York Life (Green, 2010). Research in Motion (Blackberry), Motorola, and Nokia once dominated the smart phone market, but lost market share and brand image to Apple's iPhone. Many consumers reject these once-dominant firms as no longer relevant. Firms entering dominated markets also face the challenge of earning consumers' brand consideration. Examples include Barnes & Noble's Nook (after Amazon's Kindle) and the Kindle Fire (after Apple's iPad). In other situations, a firm might stumble, perhaps through no fault of its own, and face an uphill battle to be considered as a viable alternative again. Examples include Tylenol after the 1982 poisonings, the Audi Quattro after the 1986 publicity on sudden acceleration, Toyota after widespread media reports in 2010 that software problems led to sudden acceleration, and Genentech after production problems in critical drugs. In B2B markets, the Brazilian aircraft manufacturer Embraer launched a new line of well-designed and high-quality business jets, but found it hard to enter buyers' consideration sets when competing with such established firms as Bombardier, Dassault, Cessna, Hawker Beechcraft, and Gulfstream (Aboulafia, 2007).

Many authors recommend that firms earn brand consideration by first earning trust from consumers. If consumers trust a brand, then

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consumers are motivated to invest the time and effort to learn more about the features of the brands (see, e.g., Urban, 2005 and citations therein). Research suggests that to earn trust a firm should make itself vulnerable by acting altruistically and putting consumers' needs above its own (Kirmani & Rao, 2000; Moorman, Deshpandé, & Zaltman, 1993; Rousseau, Sitkin, Burt, & Camerer, 1998). For example, New York Life stated publicly that "the guarantees we make last a lifetime" and that "our promises have no expiration date" (Green, 2010). Embraer, primarily a manufacturer of commercial regional jets, took a risky strategy of announcing a new line of jets specifically designed for business travel (Aboulafia, 2007). Even Procter & Gamble's Global Marketing Officer began new initiatives because "market share is trust materialized" (Bloom, 2007).

The theory of making oneself vulnerable is persuasive, but for a major automaker, such a strategy puts billions of dollars at risk. Most evidence to date is based on cross-sectional surveys (structural equation models and meta-analyses) or laboratory experiments. For example, in a review of the literature, Geyskens, Steenkamp, and Kumar (1998) cite 20 studies based on surveys, seven based on laboratory experiments, and none based on field experiments. Even today, we find few field experiments and none in which a once-dominant firm made itself vulnerable to gain trust. Before committing to a trust-based strategy, AAM decided that the strategy had to be proven in rigorous field experiments. We were interested in partnering because we felt the need to field-test theories that were developed from surveys and laboratory experiments.

Specifically, AAM would signal that it was acting in the consumers' best interests by providing unbiased competitive information to its customers. If the theories were correct, consumers would come to trust AAM and consider its brands. The vulnerability signal was risky because AAM would not win all head-to-head comparisons with its competitors and because goodwill was lacking among consumers familiar with pre-2003 products. However, if the theories were correct, competitive information would signal trust and have a positive impact. AAM's post-2003 products would win on sufficiently many comparisons that brand consideration would overcome losses due to adverse short-term comparisons.

In this paper, we describe the field experiments and general lessons. The year-1 data suggest that competitive information leads to trust, brand consideration, and sales but that the effect of competitive information is mediated through trust. However, contrary to extant theory, these effects are significant only for positively valenced information. The year-2 data suggest that contrary to many theories, a signal alone, indicating that the firm is willing to share competitive information, does not engender trust, brand consideration, and sales. The signal was not effective because consumers who already trusted the automaker were more likely to opt in to competitive information. Managerial analyses suggest that competitive information is effective when targeted cost-effectively to consumers who are otherwise skeptical of the brand. We begin with the theory.

2. Underlying theory

There is considerable precedent in the trust and signaling literatures to support the theory that providing competitive information would enhance trust, leading to brand consideration and sales.

2.1. Competitive information, vulnerability, and trust

Morgan and Hunt (1994) provide evidence that commitment leads to trust and that the sharing of meaningful and timely information is an antecedent of trust. This classic article was of particular interest because the Morgan–Hunt data were based on independent automobile tire retailers. However, the theories cut across industries. In a study of market research suppliers, Moorman et al. (1993) suggest that sincerity, timeliness, and the willingness to reduce uncertainty (presumably by sharing information) all lead to trust. Indeed, vulnerability to gain trust is a common theme throughout the literature (Kirmani & Rao, 2000; Moorman et al., 1993; Rousseau et al., 1998). Other researchers define trust with the related concepts of credible information and acting benevolently toward the other party (Doney & Cannon, 1997; Ganesan, 1994; Ganesan & Hess, 1997; Geyskens et al., 1998; Gurviez & Korchia, 2003; Maister, Green, & Galford, 2000; Sirdeshmukn, Singh, & Sabol, 2002; Urban, 2004).

Providing competitive information to achieve vulnerability, credibility, and an image of altruism is a common recommendation (Trifts & Häubl, 2003; Urban, 2004; Urban, Amyx, & Lorenzon, 2009; Urban, Sultan, & Qualls, 2000). Bart, Shankar, Sultan, and Urban (2005) provide evidence in online settings that clear, unbiased navigation and presentation enhance trust. Shankar, Urban, and Sultan (2002) argue that quality and timeliness of information enhances trust. In laboratory experiments, Trifts and Häubl (2003) show that competitive price information enhances an online retailer's perceived trustworthiness. These authors go on to state, citing other authors, that "the sharing of relevant and potentially self-damaging information can be viewed as a type of communication openness, which is an important form of trust-building behavior and has been found to have a direct positive relationship with the level of trust" (p. 151).

However, gaining trust is only useful if it leads to sales. Fortunately, there is ample evidence supporting this conclusion. Bart et al. (2005) show that trust mediates relationships between website design characteristics and purchase intensions. Crosby, Evans, and Cowles (1990) show that enhanced trust leads to continued exchanges between parties and to sales. Doney and Cannon (1997) demonstrate that trust influences buyer's anticipated future interactions. Ganesan (1994) shows that trust and interdependence determine the long-term orientation of both retail buyers and their vendors. Geyskens et al. (1998), in a meta-analysis of 24 papers (27 studies), suggest that trust mediates roughly 49% of long-term outcomes. Trifts and Häubl (2003) show that the effect of competitive price information is mediated through trust. Hoffman, Novak, and Peralta (1999, p. 85) posit that "the most effective way for commercial web providers to develop profitable exchange relationships with online customers is to earn their trust." Sirdeshmukn et al. (2002) provide evidence that benevolent management policies and procedures enhance trust and that trust enhances long-term loyalty. Büttner and Göritz (2008) provide evidence that trust enhances intentions to buy.

Based on this literature, we posit that sharing competitive information with consumers will demonstrate that AAM is both altruistic and vulnerable. Altruism and vulnerability will cause consumers to trust the automaker, which, in turn, will lead to brand consideration and sales. We also posit that the effect of competitive information will be mediated through trust. The following hypotheses are tested in the year-1 field experiment.

H1. Competitive information provided to the test group will increase consumers' trust of the automaker relative to the control group.

H2. Competitive information provided to the test group will increase brand consideration relative to the control group.

H3. Conditional on brand consideration, competitive information provided to the test group will increase sales relative to the control group.

H4. The increase in brand consideration and sales in the test group is mediated through trust.

The literature does not distinguish between positively valenced and negatively valenced competitive information: both are assumed to communicate altruism and vulnerability and hence lead to trust, brand consideration, and sales. However, we know that negative information per se decreases brand consideration and sales relative to positive information (e.g., automotive experiments by Urban, Hauser, & Roberts, 1990, p. 407). Thus, we expect that H1 through H3 are more likely to be supported for positively valenced competitive information than for

negatively valenced competitive information. A priori, we do not know the relative strengths of the negative information and the altruism vulnerability effects; thus, it becomes an empirical question whether H1 through H3 are supported for negatively valenced information.

2.2. Trust as a signal

Automotive competitive information comes in many forms, varying from a simple list of competitive features to community forums or online advisors, all the way to providing consumers the ability to test drive competitive vehicles. Some data are inexpensive (simple lists) and some are quite costly (competitive test drives). We would therefore like to disentangle the effect of actually providing competitive information from the act of sending a vulnerability (trust) signal by offering to provide competitive information.

Kirmani and Rao (2000) provide a comprehensive review of the literature on signaling unobserved product quality. These researchers argue that revenue-risking signals work when consumers can infer that it is in the long-term revenue interests of high-quality firms to provide the signal but not in the long-term revenue interests of low-quality firms to do so. The theories require that (1) information is hard to obtain pre-purchase, (2) quality can be assessed post-purchase, and (3) that the "bond" of vulnerability (supplied in equilibrium only by high-quality firms) is credible. Condition 2 is clearly met in automotive markets. Condition 1 is met for expensive and difficult-to-obtain information such as competitive test drives, but it may not be met for simple lists of features. Condition 3 requires that the competitive information is expensive to provide so that consumers do not interpret the information as "cheap talk." This condition is met for the competitive information provided in AAM's experiments. Only an automaker with post-evaluation high-quality brands would want to risk providing competitive information that was expensive and otherwise hard to obtain.

Signaling theory is extensive, beginning with Spence's (1973) classic article. See also Milgrom and Roberts (1986). In marketing, Biswas and Biswas (2004, p. 43) provide experiments that "signals are stronger relievers of risk in the online setting, especially for products with high non-digital attributes." Erdem and Swait (1998, p. 137) argue that the brand itself can be a signal, especially if "the information about a brand's position that is communicated to the consumer by a firm [is] perceived as truthful and dependable." Erdem and Swait (2004) argue explicitly that brand credibility, operationalized as trustworthiness and expertise, is a signal that increases brand consideration.

Given the strong support for a signaling theory of trust and the fact that the willingness to accept vulnerability by providing competitive information could signal that an automaker is a high-quality, trusted brand, we state the following hypotheses that we sought to test in the year-2 field experiment:

H5. A signal that competitive information is readily available increases trust in the test group relative to the control group.

H6. A signal that competitive information is readily available increases brand consideration in the test group relative to the control group.

H7. Conditional on brand consideration, a signal that competitive information is readily available increases sales in the test group relative to the control group.

H8. If H5 is true and if either H6 or H7 is true, then the increase in brand consideration and/or sales due to the signal are mediated through trust (H8 is conditional on whether H5 through H7 are supported).

2.3. Types of competitive information

Urban et al. (2000) suggest that virtual advisors and complete-and-unbiased information on competitive products are important to building trust. Urban et al. (2009) suggest that firms provide honest open advice and information on competitive offerings. Häubl and Trifts (2000) provide evidence that virtual recommendation agents (advisors) and comparative product information assist consumers in brand consideration. Arora et al. (2008) suggest further that information sources be personalized by the firm to the consumer (as in customized brochures) or capable of being customized by the consumer (as in the availability of drive experiences for a wide range of vehicles). We (and AAM) wanted to test different types of competitive information. The generic types were chosen to span the range of information available in automotive markets (for managerial decisions made at AAM) and as representing generic types of information that would be available in other categories (for generality). To avoid cheap talk, the information sources (in year 1) were all expensive for AAM to provide and included the following:

- Direct product experience. In automotive markets, this means test drives or their equivalent, such as renting a car or truck. Unlike other information sources, no automaker (at the time) provided competitive test drives (auto shows were a poor substitute). Direct product experience is typical in other high involvement (but less expensive) categories. For example, Proctor & Gamble routinely sends out product samples and encourages consumers to do direct comparisons with currently used brands, and exercise equipment manufacturers place their products in fitness centers to allow consumers to try them.
- Print and online information. In automotive markets, this takes the form of competitive brochures or information abstracted from those brochures. In other markets, such information is available by searching product catalogs or from information aggregation websites, such as CNET in electronic goods.
- Word-of-mouth. In automotive markets (in 2003–2005), word-of-mouth information on competitors was available through online automotive communities. This type of information is a surrogate for information now available in Angie's List, Cyworld, Facebook, Google+, Yelp, and other social networks.
- Trusted advisors. Many websites provide unbiased online advisors (and some biased advisors). Such advisors are available in many product categories.

3. Year 1: randomized experiments for competitive information

The year-1 field experiment tested Hypotheses H1 through H4, whether competitive information leads to trust and trust leads to brand consideration and sales. To ensure that the year-1 field experiment tests competitive information and not just the signal that competitive information is available, consumers in year 1 are given incentives to experience the competitive information. The year-2 field experiment tested Hypotheses H5 through H8, whether the sheer act of offering competitive information signals vulnerability and altruism which in turn leads to trust, brand consideration, and sales. In year 2, competitive information was made available, but consumers experienced the information only if they opted in.

3.1. 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, both of which are now profitable). 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 were 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 approximately 40%.

Consumers were assigned randomly to four experimental treatments, each representing one of the four generic forms of competitive information. In year 1 (but not year 2), consumers assigned to a treatment were given sufficient incentives to experience that treatment. Treatments were assigned in a fully crossed orthogonal design giving us a $2\times2\times2\times2$ full-factorial field experiment such that various respondents received 0, 1, 2, 3, or 4 treatments. Although assignments were random with a goal of 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 months, the competitive community ran for all but the last month, the customized brochures were mailed in months 2 and 3, and the competitive test drive took place in month 4 (this differential assignment is analyzed with models that take account of the differential timing of assignments). The exact numbers of consumers assigned to each treatment in year 1 is summarized in Table 1.

3.2. Competitive information experimental treatments

All experimental treatments were produced and managed professionally and required substantial investments ranging from approximately $150,000 (brochures) to $1 million (competitive test drives). To avoid cheap talk, all treatments would be quite expensive on a national basis. Direct product experience was represented by 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 without any sales pressure (Fig. 1a).

Print and online information was represented by customized brochures (year 1) and competitive brochures (year 2). Customized brochures were glossy brochures tailored to the needs of individual consumers and mailed directly to the consumers (Fig. 1b). Competitive brochures were less customized, web-based, and included brochures from all competitors. While the year-1 print information was not competitive information, AAM believed it would engender trust and signal altruism. All other information is competitive.

Word-of-mouth information was represented by an unbiased online CommuniSpace™ forum in which consumers could participate in over 30 discussions about both AAM and competitive vehicles (Fig. 1c). Commensurate with concerns that such information might make the automaker vulnerable, approximately 20% of the comments about AAM brands were negative.

Trusted advisors were represented by an online advisor that was co-branded with Kelley Blue Book and similar to that developed by Urban and Hauser (2004) (see Fig. 1d). In year 1, the online advisor recommended competitors approximately 83% of the time.

3.3. Dependent measures: brand consideration and purchase

Brand consideration was measured with drop-down menus in which the consumer indicated the brands that he or she would consider. Brand consideration is a quantal measure (consider vs. not consider) where a consumer is coded as considering AAM if the consumer indicated that he or she would consider one of AAM's brands. To avoid demand artifacts, AAM was not identified as the sponsor of the study and AAM vehicles occurred throughout the list of potential vehicles. Because consumers might evaluate and reject a brand, they can report no brand consideration in month t, even if they considered a brand in month $t-1$. Purchase (sales) is also a quantal measure (purchase or not purchase) measured with a standard stated purchase measure. Once consumers purchase a vehicle, we assume they cannot un-consider and un-purchase that vehicle during the six-month observation period.

Table 1
Consumers randomly assigned to treatments in year 1.

Number of respondents who were assigned to the indicated treatment in that month.							
Treatment		Month 2	Month 3	Month 4	Month 5	Month 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

3.4. Trust

We hypothesize that trust is a dependent measure and a mediator for brand consideration and purchase (trust is a firm-specific property in our experiments). While the definition of trust varies widely in the literature, a common definition is "a single, global, unidimensional measure of trust" (Geyskens et al., 1998, p. 225). We include a global measure: "Overall, this company is trustworthy." Other authors include benevolence and credibility (e.g., Doney & Cannon, 1997; Ganesan, 1994; Ganesan & Hess, 1997; Geyskens et al., 1998). We implemented benevolence with "I believe that this company is willing to assist and support me," and we implemented credible with "Overall, this company has the ability to meet customer needs." Sirdeshmukn et al. (2002) suggest that trust includes operational competence, which we implemented with "The company is very competent in its dealings with its customers." Finally, Anderson and Narus (1990, p. 45) suggest that the trustworthy firm "will perform actions that will result in positive outcomes for the [consumer]" and Anderson and Weitz (1989) define trust as a [consumer's] belief that "its needs will be fulfilled in the future by actions undertaken by the other party." We implemented positive outcomes with "This company makes excellent vehicles." All five items were measured with 7-point Likert questions. Consistency with one's products, listening to customers and placing customers ahead of profit are becoming increasingly important to consumers, as indicated by the Edelman Trust Barometer (http://trust.edelman.com/what-the-2012-edelman-trust-barometer-means-for-purpose/).

Together, the five items reflected the goals of AAM on how they might engender trust among consumers. Before we use the five items to evaluate the impact of competitive information, we establish that the five items form a unidimensional construct and that the construct is related to overall trust. Using methods recommended in Churchill (1979), we purify the scale. The scale is unidimensional with high construct reliability and maximized with all five items: Cronbach's $\alpha=0.95$. As a further test, we compare a four-item scale without the global item; the reduced scale is highly correlated with the global item ($\rho=0.88$). We therefore conclude that the five items represent a single scale and that it is reasonable to call the scale "trust" for the purpose of evaluating the implications of competitive information. In the following analyses, trust is measured by the sum − score (divided by the number of items such that the trust scale ranges from 1 to 7).

3.5. Randomization tests in year 1

All models of consideration and purchase use treatment-assignment dummies. Following standard experimental procedures, the impact of a treatment is measured for all respondents for whom the treatment was available whether they experienced the treatment. This approach is conservative and avoids effects that might be due to differential take-up of the treatments. For completeness, we compared treatment-assignment analyses to self-reported-treatment analyses. The pattern of coefficients and their significance was similar for both analyses.

Qualitative data are consistent with the hypothesis that take-up was random in year 1. For example, a portion of consumers experienced

Fig. 1. Year-1 (Random Assignment) Competitive-Information Treatments.

technical difficulties with the competitive online advisor, and a few could not come to the competitive test drive due to last-minute scheduling 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. The high take-up rates and the similarity of coefficients suggest that treatment take-up (given it was offered) was sufficiently random that take-up selection had little or no effect in year 1. We thus leave analyses of self-selected take-up to year 2 when the trust signal is more cleanly implemented.

Although we use treatment assignments as independent measures, it is useful to examine further whether take-up was random. Specifically, we examine consumers who (1) were not assigned to a treatment, (2) were assigned to a treatment but did not report participation, and (3) were assigned and reported participation. If there were non-random take-up, then consumers in group (3) would be a non-random draw from (2 and 3). However, if that were true, non-random take-up from (2 and 3) to (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 sufficiently random. For example, measured brand 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 ($t=1.14$, $p=.26$). All t-tests in this paper are two-tailed.

4. Main effects of treatments in year 1

We begin with treatment main effects from the fully crossed $2\times2\times2\times2$ experiment. We explore interactions, dynamics, heterogeneity, and other issues in Section 5.

The main effects are summarized in Table 2. The first column is the percent increase in consumers who consider an AAM vehicle at the end of the experiment. For example, among consumers assigned to competitive test drives, brand consideration increased by 20.5% relative to consumers who were not assigned to competitive test drives. This difference is significant ($t=3.6$, $p<.01$). The treatment vs. control difference is not significant for customized brochures, the competitive forum, and the competitive advisor. The increase in cumulative purchase follows the same pattern with an 11.1% difference for competitive test drives ($t=2.4$, $p=.02$) and insignificant effects for the other treatments. The impact on trust is similar but not identical. Competitive test drives increase trust (11.6%), but the competitive advisor has a negative effect (−8.9%), and both are significant at $p<.01$. Neither customized brochures nor the competitive forum change trust significantly (we examine more-complete models and trust mediation in Section 5).

We might posit the effect of competitive information to be either larger or smaller among consumers who own AAM vehicles. This effect might be larger because current vehicles are improved relative to prior vehicles, but it might be smaller because consumers who do

Table 2
Main-effect analyses in year 1 randomly assignment field experiment.

	Brand consideration (difference in percent in last month)	Purchase (cumulative difference in percent)	Trust (% lift in last month)
Treatment			
Competitive test drives	20.5%[a]	11.1% [a]	11.6%[a]
Customized brochures	3.2%	4.8%	5.6%
Competitive forum	0.5%	3.3%	2.9%
Competitive advisor	−2.4%	−4.4%	−8.9%[a]
Treatment among non-AAM owners			
Competitive test drives	20.0%[a]	7.3%	12.3%[a]
Customized brochures	2.2%	5.0%	3.9%
Competitive forum	2.0%	6.1%	2.4%
Competitive advisor	1.1%	−0.9%	−5.7%

[a] Significant at the 0.05 level.

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. Competitive test drives is the only treatment with a significant impact among non-AAM owners, and the magnitude of that impact is virtually identical to the magnitude among all consumers (lower left of Table 2). We explore interactions more formally in Section 5. We find no differential impact due to age or sex. The age–sex comparisons are not shown to simplify the tables.

Analyses of the main effects are the simplest and cleanest set of analyses. Heterogeneity is mitigated because of randomization in treatment assignment. However, we can improve insight by accounting for dynamics, interactions among treatments, the conditioning of purchase on brand consideration, the potential for trust mediation, and heterogeneity.

5. Dynamics of trust, brand consideration, and purchase

5.1. Average trust, brand consideration, and purchase by month

Table 3 summarizes average trust, brand consideration, and purchase by treatment for months 2 to 6. The response to competitive information is more complex. For example, trust increases substantially in month 4 among consumers who experience test drives and declines in months 5 and 6, but it does not decline to pre-month 4 levels. A large change in brand consideration occurs in month 4 when consumers experience competitive test drives, but the effect endured through subsequent months. These observations might be due to an effect where competitive test drives increase trust which then decays slowly rather than immediately. In addition, in automotive markets, a consumer might come to a competitive test drive in one month, consider and seriously evaluate vehicles in the next month, and purchase in a third month. To untangle these effects, we need analyses beyond mere inspection of Table 3. We need analyses based on a theory of automotive purchasing dynamics.

Table 3 reports the main effects, but the experiment is a fully crossed $2\times2\times2\times2$ design with some consumers experiencing other forms of competitive information in earlier months, some in later months, and some not at all. More sophisticated analyses are necessary to account for the fully crossed design, potential interactions among treatments, and the time-varying nature of the treatments. Finally, the measures in Table 3 are averages and do not account for individual differences. To untangle the true effect of competitive information we now model the complexities of the dynamics of the automotive market, the complexity of the experimental design, and potential individual differences.

5.2. Trust dynamics

Trust builds or declines over time. A consumer might experience a treatment in month t and, as a result, increase his or her trust in a brand. However, the consumer may not trust the brand enough to consider it. Another treatment in month $t+1$ might increase trust further and be enough to encourage brand consideration. To capture this phenomenon, we model trust as a variable that can increase or decrease over time as a result of treatments. Trust might also decay. Specifically, we model trust in month t as a sum of γ times trust in month $t-1$ plus effects due to the treatments in month t, where γ and the coefficients of the treatment effects are to be estimated ($\gamma\leq1$). We provide detailed equations in Section 6.

5.3. Interactions among the treatments

Hauser, Urban, and Weinberg (1993) examine consumers' search for information concerning automobiles and find that the value of an information source sometimes depends upon whether consumers had previously experienced another information source. These researchers' data suggest two-level interactions but no three-level interactions. In light of this prior research, we examine models that allow interactions among the treatments.

5.4. Individual differences among consumers

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, brand consideration, or purchase on their prior ownership experience. To capture purchase history,

Table 3
Average brand trust, brand consideration (%), and purchase (%) by month in year 1 randomly assignment field experiment (cells are in bold if the treatment was available in that month).

Treatment		Month 2	Month 3	Month 4	Month 5	Month 6
Average trust among respondents assigned to the indicated treatment in that month (five-item scale)						
Competitive test drives	Yes	4.97	4.94	**5.17**	5.14	5.07
	No	4.66	4.66	4.55	4.65	4.54
Customized brochures	Yes	**5.01**	**4.94**	4.93	4.97	4.87
	No	4.53	4.58	4.65	4.69	4.61
Competitive forum	Yes	**4.81**	**4.87**	**4.78**	**4.82**	4.82
	No	4.75	4.67	4.81	4.85	4.68
Competitive advisor	Yes	**4.62**	**4.55**	**4.54**	**4.60**	**4.52**
	No	4.93	4.97	5.04	5.07	4.97
Brand consideration among respondents assigned to the indicated treatment in that month (%)						
Competitive test drives	Yes	49.2	55.6	**61.3**	59.7	57.3
	No	37.3	38.3	41.5	36.8	36.8
Customized brochures	Yes	**43.9**	**43.9**	52.4	47.6	46.3
	No	39.9	46.4	45.8	43.8	43.1
Competitive forum	Yes	**45.0**	**47.0**	**53.0**	**45.0**	45.0
	No	39.2	43.4	45.8	46.4	44.6
Competitive advisor	Yes	**37.2**	**46.2**	**47.4**	**43.6**	**43.6**
	No	46.6	44.1	50.9	47.8	46.0
Purchase among respondents assigned to the indicated treatment in that month (%)						
Competitive test drives	Yes	5.6	2.4	**12.1**	3.2	4.8
	No	2.6	6.2	3.6	2.1	2.6
Customized brochures	Yes	**4.9**	**5.5**	7.3	1.2	4.9
	No	2.6	3.9	6.5	3.9	2.0
Competitive forum	Yes	**4.0**	**5.3**	**7.9**	**2.6**	3.3
	No	3.6	4.2	6.0	2.4	3.6
Competitive advisor	Yes	**3.2**	**5.1**	**6.4**	**2.6**	**1.9**
	No	4.3	4.3	7.4	2.5	5.0

we include dummy variables for "own other American" and "own Japanese." There is no dummy variable for "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 month-to-month differences in trust, brand consideration, and purchase. However, both brand consideration and purchase are quantal (0 vs. 1) measures and month-to-month differences 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 & 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 ($\gamma = 0.833$, yet to be shown), (b) explicit controls for observable heterogeneity, (c) consistency with the main-effect analyses (yet to be shown), and (d) continuous-time Markov analyses based on differences (yet to be shown) all suggest that effects due to unobserved heterogeneity are negligible and unlikely to reverse primary insights.

6. Modeling dynamics in year 1

Ignoring for a moment dynamics, persistence, more complete prior-ownership effects, interactions among treatments, and unobserved external shocks, we see that trust is correlated with both brand consideration ($\rho = 0.22$) and purchase ($\rho = 0.17$), both significant at the 0.01 level. The correlation with lagged trust is higher for brand consideration ($\rho = 0.61$) but lower for purchase ($\rho = .05$). This result is consistent with a dynamic interpretation that trust at the beginning of a month is the driver of brand consideration during that month. However, these simple correlations do not capture all of the dynamics and, technically, misuse a correlation coefficient for two quantal outcomes (brand consideration and purchase).

To account for theoretical dynamics and to respect the quantal nature of the outcome variables, we use conditional-logit analyses of brand consideration and purchase (see Fig. 2). Specifically, we ask whether the treatments increase brand consideration and, among those consumers who consider AAM vehicles, whether the treatments also affect purchase.

In the conditional-logit analyses, we include lagged brand consideration as an explanatory variable to focus on changes in brand consideration. We include dummy variables for observation months to account for unobserved marketing actions and environmental shocks (month 1 is a pre-measure and the month-2 dummy variable is set to zero for identification). The month dummy variables also account for any measurement artifact that might boost brand consideration (e.g., "Hawthorne" effect). To account for observed heterogeneity in past purchases, we include prior ownership of AAM, other American, and Japanese (relative to European) vehicles. Age and sex effects were examined but suppressed to simplify Table 4, as they were not significant.

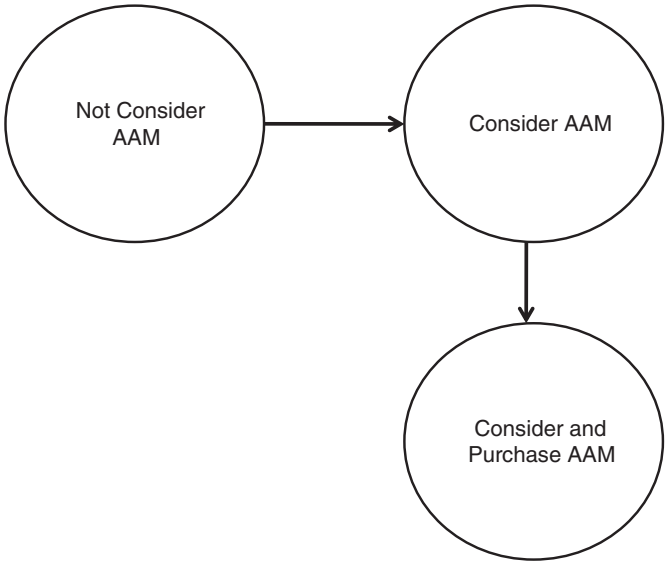

Fig. 2. Consideration and Purchase Dynamics: Conditional-Logit Analyses.

Let R_{it} be a measure of consumer i's trust in month t and let $x_{ijt} = 1$ if consumer i was assigned to treatment j in month t, and $x_{ijt} = 0$ otherwise. Let $y_{ik} = 1$ if consumer i has characteristic k, and let $y_{ik} = 0$ otherwise. Let $\delta_t = 1$ in month t and $\delta_t = 0$ otherwise. Trust dynamics are modeled with Eq. (1) where we estimate the γ^R, w_j^R, v_k^R, u_t^R, and b^R:

$$R_{it} = \gamma^R R_{i,t-1} + \sum_{j=1}^{4} w_j^R x_{ijt} + \sum_{k=1}^{K} v_k^R y_{ik} + \sum_{t=3}^{6} u_t^R \delta_t + b^R. \quad (1)$$

Let $C_{it} = 1$ if consumer i considers an AAM brand in month t and $C_{it} = 0$ otherwise. Let $P_{it} = 1$ if consumer i purchases an AAM vehicle in month t and $P_{it} = 0$ otherwise. The conditional-logit models are specified by Eq. (2):

$$\text{Prob}(C_{it} = 1) = \frac{e^{f_{it}}}{1 + e^{f_{it}}} \quad \text{where} \quad f_{it} = \gamma^C C_{i,t-1} + \sum_{j=1}^{4} w_j^C x_{ijt} + \sum_{k=1}^{K} v_k^C y_{ik} + \sum_{t=3}^{6} u_t^C \delta_t + b^C.$$
$$\text{Prob}(P_{it} = 1 | C_{it} = 1) = \frac{e^{g_{it}}}{1 + e^{g_{it}}} \quad \text{where} \quad g_{it} = \sum_{j=1}^{4} w_j^P x_{ijt} + \sum_{k=1}^{K} v_k^P y_{ik} + \sum_{t=3}^{6} u_t^P \delta_t + b^P. \quad (2)$$

6.1. Direct effects of treatments

We begin with main effects of the treatments, as shown in the first and second columns of parameters in Table 4. The brand consideration analysis includes all respondents. The purchase analysis is conditional on brand consideration: only those respondents who consider AAM in that month are included when estimating the purchase logit, and the effect on purchase is incremental above and beyond the effect on brand consideration (standard errors available upon request).

Brand-consideration analyses explain substantial information with a U^2 of 24.8% (U^2, sometimes called a pseudo-R^2, measures the percent of uncertainty explained, Hauser, 1978). Brand consideration is increased if consumers own AAM or other American vehicles and decreased if they own Japanese vehicles. Brand consideration is also higher in months 3 to 6 relative to month 2. The only significant direct treatment effect is due to competitive test drives. Purchase, conditional on brand consideration, also increases with competitive test drives (marginally significant), but there are no direct effects of prior ownership or month of measurement on purchase. The purchase model explains less uncertainty, suggesting that the treatments affected brand consideration more strongly than purchase.

6.2. Trust as a mediator

There is ample precedent in the literature for trust as a mediator of purchase or purchase intentions (e.g., Bart et al., 2005; Büttner & Göritz, 2008; Erdem & Swait, 2004; Morgan & Hunt, 1994; Porter & Donthu, 2008; Urban et al., 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 Kenney (1986) to test whether competitive information treatments were mediated through trust. Specifically, if the treatments affect trust and if the treatments affect brand consideration (or purchase), 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

Table 4
Conditional-logit analyses and trust regression with time-specific stimuli — year 1 random assignments.

Dependent measure	Conditional-logit analyses (five months, 317 respondents for brand consideration model, only those who consider for conditional-purchase model)						Trust regression (lagged trust is used in this regression)
	Direct effects not mediated		Mediated by trust (bootstrap estimates)				
	Consider brand	Purchase if consider	Consider brand		Purchase if consider		
Constant	−1.492[a]	−2.567[a]	−3.945[a]	−4.348[a]	−3.896[a]	−3.977[a]	.640[b]
Lagged brand consider	2.537[a]		2.394[a]	2.405[a]			
Lagged trust hat			.531[a]	.525[a]	.245[b]	.249	.860[a]
Competitive test drives	.579[a]	.938[b]	.392	.346	.879	.498	.380[a]
Customized brochures	.079	.477	−.059	−.202	.405	.575	.171[a]
Competitive forum	.144	.122	.133	.124	.182	.522	.045
Competitive advisor	−.023	−.103	.136	.133	−.059	.025	−.057
Prior ownership AAM	.399[a]	.137	.327[b]	.734[a]	.079	.219	.000
Prior own other American	.304[a]	−.005	.253[b]	.238[b]	.037	.009	.021
Prior ownership Japanese	−.577[a]	−.188	−.464[a]	−.478[a]	−.126	−.105	−.019
Month 3	.313	.200	.461[a]	.461[a]	.269	.275	−.220[a]
Month 4	.419[b]	.264	.433[b]	.423	.270	.281	−.295[a]
Month 5	.523[a]	−.238	.390[b]	.293[b]	−.276	−.253	−.127[a]
Month 6	.722[a]	.185	.654[a]	.644[a]	.194	.253	−.251[a]
Prior ownership of AAM crossed with							
Competitive test drives				−.211		1.61	
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)	24.8%	3.1%	28.8%	29.0%	3.8%	5.3%	.749

Sex and age coefficients are not shown (not significant). Trust regression interactions are not significant.
[a] Significant at the 0.05 level.
[b] Significant at the 0.10 level.

indicator of trust should be significant in the new models, and (2) the direct effect of treatments on consideration (or purchase) 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 to be consistent with 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 months than the brand consideration and purchase measures. Nonetheless, to account for unobserved shocks that affect trust in month $t-1$ and brand consideration (purchase) in month t, we use estimated lagged trust in an equation that predicts brand consideration (purchase) with reported treatments (see Online Appendix for other trust regressions). That is, we add $\hat{R}_{i,t-1}$ as an explanatory variable on the right-hand sides of Eq. (2) where $\hat{R}_{i,t-1}$ is estimated with Eq. (1). Traditional mediation analyses use lagged trust directly. In our data, these tests also indicate mediation and have similar implications.

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 & Mela, 2003; Hauser et al., 2010). The effect of customized brochures is less apparent in the main-effect analyses because, although the effect is strong in earlier months, it becomes insignificant in the last month. Review Table 3. Consistent with the main-effect analyses, the conditional-logit analyses and the trust regression identify no impact on brand consideration and purchase for 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, Hall, Hall, & Hausman, 1974; Efron & Tibshirani, 1994; Wooldridge, 2002, p. 354, 414). The parameter estimates and standard errors are based on 1000 bootstrap replications (Table 4 reports significance; standard errors are available upon request).

Following Baron and Kenney (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 brand consideration and marginally significant for purchase ($\chi^2_1 = 86.7$, $p < .001$ and $\chi^2_1 = 3.1$, $p = 0.08$, respectively). Once we partial out lagged trust, there remain no significant direct effects due to the treatments. This result suggests trust mediation.

6.3. Testing 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, brand consideration, and purchase, the interactions are not significant ($F = 1.91$, $p = .11$; $\chi^2_4 = 4.3$, $p = .37$, and $\chi^2_4 = 7.0$, $p = .13$, 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 ($F = .85$, $p = .59$). We continue to use estimated lagged trust (without interactions) and estimate a conditional-logit model that allows interactions. The fully saturated brand consideration model that allows all possible interactions is not significant relative to a main-effects model and provides no additional insight ($\chi^2_{11} = 12.0$, $p = .10+$). A few coefficients are significant, but all include competitive test drives with slight variations in parameter magnitudes that depend upon the other combinations of treatments. To avoid over-fitting 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 (see, e.g., Hauser et al., 1993). The "two or more treatments" variable is not significant for brand consideration or purchase. Neither the fully saturated nor the parsimonious analysis highlights any interpretable interactions suggesting that the fully saturated model was over parameterized. The fourth (brand consideration) and sixth (conditional purchase) data columns of Table 4 display models with

interactions due to prior ownership and due to two or more treatments. The addition of prior ownership and interactions among the treatments are not significant ($\chi^2_5 = 4.9, p = .42$ and $\chi^2_5 = 7.1, p = .21$ for brand consideration and purchase, respectively).

6.4. Interpretation of year-1 results relative to year-1 hypotheses

H4 is clearly supported. Enhanced trust at the end month $t-1$ is significantly correlated with brand consideration in month t. When there is an effect due to competitive information, it is mediated through trust. Enhanced trust at the end of month $t-1$ has a significant effect on consideration in month t and a (marginally) significant effect on purchase in month t. These field-experiment results are consistent with results from the structural-equation analysis of questionnaires and laboratory experiments found in the literature. More importantly, given the in vivo nature of the field experiment and the modeling of dynamics, causality and external validity are likely.

H1 through H3 are more complicated. Contrary to predictions and recommendations in the literature, all forms of competitive information do not enhance trust. Neither the competitive forum (word of mouth) nor the competitive advisor enhanced trust, brand consideration, or purchase. Only competitive test drives and possibly customized brochures enhanced trust, brand consideration, and purchase. Not only was this complexity not predicted by theory, but the results also surprised AAM. All four generic forms of competitive information were provided altruistically and made AAM vulnerable, but vulnerability and altruism were not sufficient to affect trust, and through trust, brand consideration and purchase.

The experiments alone do not explain why some forms of competitive information were better than others, but our belief and the judgment of managers at AAM suggest that the valence of the information made a difference. AAM does well relative to competition in competitive test drives; the customized brochures highlighted the benefits of AAM. However, there was substantial negative information in both the competitive forum and the competitive advisor. Empirically, it appears that the effect of negative information countered the effect of trust from competitive information. We believe that this possibility is the most likely interpretation, but we cannot rule out other interpretations, such as the strength of the signal. We can rule out cheap talk because both the most costly and least costly forms of competitive information were effective. This moderation of the vulnerability/altruism-to-trust link by the valence of information is worthy of further analyses.

Although it may seem intuitive a posteriori, an interpretation that negatively valenced information does not enhance trust refines theories of trust signaling. Many authors posit only that the firm should act altruistically and provide information that is truthful and dependable (e.g., Erdem & Swait, 2004; Urban et al., 2009; Urban et al., 2000). Prior to the field experiments, the managers at AAM believed that vulnerability and altruism would signal trust and that the enhanced trust would lead to brand consideration. The insight about the valence of the competitive information was managerially important.

6.5. Continuous-time analyses

As a final check, we estimate continuous-time Markov models that relax three restrictions. These Markov models allow flows to happen in continuous time (even though observations of the results of those flows are at discrete time intervals). In addition, because the dependent variables are differences in observed states (not consider, consider but not purchase, and consider and purchase), the Markov models are analogous to difference-in-differences models and thus account for unobserved heterogeneity in the propensity to consider AAM. Finally, all transitions are estimated simultaneously with a single likelihood function (details are in Appendix A). The results reinforce the implications of the conditional-logit analyses: competitive test drives have a significant effect on brand consideration, that effect is mediated through trust, and neither competitive forums nor competitive advisors have significant effects.

7. Year 2: field test of trust signaling

Year 1 established that positively valenced competitive information (competitive test drives and, possibly, customized brochures) enhances trust, which, in turn, precedes brand consideration. However, the most effective competitive information (competitive test drives) is extremely expensive to provide and may not be cost-effective. Conversely, competitive information might be cost-effective if a firm could signal trustworthiness by simply offering to make competitive information available. If the signal alone were sufficient, a national launch would be feasible. Trust signaling (H5 through H8) suggests that the offer of competitive information itself engenders trust. However, there is a danger that a trust signal would attract only those consumers who already trust and/or feel favorably toward the brand that is signaling trust. If this possibility is supported, H5 through H8 might be rejected.

To test trust signaling, year-2 consumers were assigned randomly to one of two groups. Consumers in the control group received no treatments whatsoever. Consumers in the test group received an advertisement inviting them visit a "My Auto Advocate" website (screenshots available in an Online Appendix). We call consumers who visited the website based on the advertisement alone the "website-visited" test group. 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 "website-forced-exposure" test group. At the "My Auto Advocate" website, both the website-visited test group and the website-forced-exposure test group could select (opt in to) any combination of five treatments. Taken together, these two sub-groups make up the test group that tests trust signaling. The website-forced-exposure group represents a stronger signal and more intensive national advertising.

The panel ran monthly from January to June 2005. In year 2, 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. 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, but unlike in year 1, they were not assigned to treatments. 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.

Harris Interactive invited 6092 Los Angeles consumers of which 1720 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. website-visited vs. website-forced-exposure, $p = .25$). Brand consideration, purchase, and trust were measured as in year 1.

7.1. Treatments in year 2

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 minor updates. The competitive online advisor was improved slightly with a better interface and a "garage" at which consumers could store vehicle descriptions. The online advisor still favored other manufacturers' vehicles in year 2, although somewhat less so than in year 1. The major change was the brochures. Year 2 used electronic brochures for AAM vehicles (called eBooklets). These brochures 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. Screenshots for "My Auto Advocate" and the treatments are available in an Online Appendix. Table 5 summarizes the numbers of consumers who opted in to treatments in year 2. All treatments except competitive test drives were reasonably popular and available in all months.

7.2. Testing whether the trust signal was effective

We first examine brand consideration and purchase in the test (website-visited and website-forced-exposure) vs. the control groups. There were no significant differences in trust, brand consideration, or purchase intentions ($t=.9$, $p=.37$, $t=.14$, $p=.88$, and $t=.18$, $p=.86$, respectively). Similarly, the differences between the website-visited sub-group and control group were not significant ($t=1.4$, $p=.15$, $t=-1.5$, $p=.14$, and $t=-.05$, $p=.96$, respectively). These results suggest that a trust signal provided little or no lift in trust, brand consideration, and purchase relative to the control. Contrary to extant theory, we reject H5, H6, and H7. Because these hypotheses are rejected, we cannot test the conditional hypothesis, H8.

There are at least two complementary explanations for the null effect in year 2. First, the null effect may be because offering competitive information does not increase trust. In this case, it might have been that only those consumers who already trusted AAM were likely to opt in to the treatments. A second (and complementary) explanation is that fewer consumers opted in to the effective treatments (test drives and possibly brochures) than potentially negative treatments (word-of-mouth and online advisors).

7.3. Examining potential explanations

If the signal did not enhance trust, brand consideration, and purchase in the test group relative to the control group, then we should see evidence that consumers who were more favorable to AAM opted in to the treatments (to be most effective, the signal needed to reach consumers who do not trust the automaker, not those who already trust the automaker). To test whether the trust signal reached targeted consumers we compare consumers in the control group (who received no treatments) to those in the test group who did not opt in to any treatments.

Among no-treatment consumers, the non-treated members of the test group had significantly lower trust, brand consideration, and purchase intentions than the control group ($t=6.1$, $p=.0$, $t=6.1$, $p=.0$, and $t=2.0$, $p=.05$, respectively). By implication, consumers who opted in to competitive-information treatments were consumers who were more trusting of AAM (or, at least, more favorable toward the automaker). Comparing the control group to non-treated members of the website-forced-exposure test group gives similar results. The trust signal ultimately targeted consumers more likely to be favorable toward AAM. The signal alone did not engender trust, brand consideration, and purchase.

We gain further insight by redoing for year 2 the main-effects analyses (as in Table 2) and conditional-logit analyses (as in Table 4). Details of the year-2 analyses are available in an Online Appendix. The opt-in main effects (year 2), relative to random-assignment main effects (year 1), are consistent with a hypothesis that for each treatment, consumers who opted in to that treatment were a priori more favorable to AAM. When we account for prior ownership, prior propensities, dynamics, and mediation with conditional-logit analyses, we find that the effects of the treatments are consistent with those observed in year 1. Thus, the null effect of the trust signal is probably observed because the signal did not encourage opt in among those to whom the signal was targeted.

8. Hypotheses revisited

Table 6 summarizes the implications of the two field experiments. H1 through H4 suggest that if firms build trust with consumers, trust will lead to brand consideration and purchase. H5 through H8 suggest further that trust signals alone should achieve these outcomes. 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 the vehicles from AAM had improved relative to prior years, the automaker had an opportunity to build trust by providing competitive information (year 1) or by signaling trustworthiness (year 2). Current theories suggest altruism and vulnerability will build trust but do not distinguish whether the information provided to the consumer should favor the brand. In fact, altruism and vulnerability are greater if the competitive information does not always favor the firm's brands.

The year-1 experiments were consistent with H4. Trust in one month was correlated with brand consideration in the next month, and the effect of competitive information was mediated through trust. However, the year-1 experiments also identified certain competitive information types as more effective than other competitive information types. Experiential information (competitive test drives) was the most effective communications strategy. Tangible experience convinced consumers that the automaker's products had improved relative to the competition.

Table 5
Consumers who selected treatments in year 2 signaling experiment (test of signaling trust through advertising then website opt in to treatments).

Number of respondents who selected the indicated treatment in that month.							
Treatment		Month 2	Month 3	Month 4	Month 5	Month 6	Treatment "cell"
Competitive test drives	Website-visited[a]	70	0	0	0	0	70
	Website-forced[b]	140	0	0	0	0	140
	Not treated[c]	1182	1322	1322	1322	1322	1182
Competitive eBrochures	Website-visited[a]	88	178	199	202	211	289
	Website-forced[b]	149	361	411	425	432	621
	Not treated[c]	1173	961	911	897	890	701
AAM eBooklets	Website-visited[a]	49	158	184	194	205	252
	Website-forced[b]	114	355	411	417	438	549
	Not treated[c]	1208	967	911	905	884	773
Competitive forum	Website-visited[a]	71	139	168	194	208	249
	Website-forced[b]	114	294	352	409	420	538
	Not treated[c]	1208	1028	970	913	902	784
Competitive advisor	Website-visited[a]	97	180	206	226	246	290
	Website-forced[b]	199	378	441	493	535	645
	Not treated[c]	1123	944	881	829	787	677

[a] Website-visited = consumers visited the "My Auto Advocate" website on their own and opted in to the treatment.
[b] Website-forced = consumers were forced to visit the "My Auto Advocate" website, but opted in to the treatment.
[c] Not treated = consumers in control group plus consumers in test group who did not opt in to the treatment.

Table 6
Summary of hypotheses, support, tests, and implications.

Hypotheses	Supported or not supported	Evidence	Implications
H1. Trust enhanced by competitive information (in test vs. control).	Supported for positively valenced information but not for information with substantial negative content (positive vs. negative remains a hypothesis).	Competitive test drives and customized brochures have significant effects in trust regressions. Other treatments have no significant effect.	There exist situations where competitive information leads to trust, especially positively valenced information. Negatively valenced information may not engender trust.
H2. Brand consideration enhanced by competitive information (in test vs. control).	Strongly supported for positively valenced information.	Trust-to-brand consideration is significant in conditional-logit and continuous-Markov analyses.	If a firm can engender trust it can enhance brand consideration.
H3. Sales enhanced by competitive information (in test vs. control).	Marginally supported for positively valenced information.	Trust-to-sales is marginally significant in conditional-logit and significant in continuous-Markov analyses.	If a firm can engender trust it might be able to obtain sales.
H4. Mediation by trust.	Strongly supported.	Baron–Kenney tests support trust mediation in both conditional-logit and continuous-Markov analyses.	Other means to gain trust might gain brand consideration and sales.
H5–H7. Offering to provide competitive information enhances trust, brand consideration, and sales.	Rejected. Opt-in communications attracted consumers who are already favorable.	Year-2 null effects. Also comparisons of control consumers to consumers in the test groups who did not opt in to treatments.	Signaling altruism (trust) by providing competitive information did not enhance trust, consideration, or sales. Consumers must experience the information.
H6. Mediation of signal by trust.	Not testable in the data.	H8 is conditioned on support for H5–H7.	Trust was not enhanced by offer of information.

Neither word-of-mouth (community forums) nor competitive advisors increased trust in the test group relative to the control group. From the year-1 experiments alone, we cannot determine whether these forms of competitive information are not effective or whether AAM's implementation of these forms of competitive information was not effective. 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 improvement in vehicles. We posit that positively valenced information enhanced and negatively valenced information diminished the effectiveness of competitive-information implementations.

Hypothesis. Unbiased competitive information can build trust, and trust enhances brand consideration and purchase. The firm builds trust if competitive information enables the firm to communicate to consumers that it is acting altruistically and that it has products that meet their needs. However, the availability of competitive information alone does not enhance trust; consumers must process the competitive information.

We believe that this hypothesis applies across a variety of situations and product categories as discussed in the introduction to this paper. Naturally, this hypothesis is subject to tests in different categories with different implementations of generic competitive-information treatments and with different signals of trust.

9. Cost effectiveness and managerial implications

We now move from theory back to practice. Because the experiments established that some forms of competitive information engender trust and that trust leads to brand consideration, AAM ultimately implemented competitive-information strategies. The automaker first used the more general insights to refine competitive test drives. The following calculations illustrate the motivation behind managers' decisions at the time. We disguise the proprietary data with comparable publicly available data.

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 $7200. [$7207 = $120/(0.15 * 0.111).] Typical margins for the automotive industry are approximately 9%, and the average price of a new car is about $28,400 (Thomas & Cremer, 2010, http://www.ehow.com/facts_5977729_average-cost-new-car.html, visited July 2012). These public numbers suggest an incremental profit of approximately $2500 per vehicle sold, much less than the $7200 cost per vehicle sold based on a competitive test drive. Post-sales parts and other considerations are unlikely to make up the $4700 difference. As implemented in the year-1 randomized experiments, competitive test drives are not profitable.

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 test drives 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 (automotive consumers often use agendas to screen on body-type; Hauser, 1986; Urban, Liberali, MacDonald, Bordley, & Hauser, 2012). These competitive test drives proved to be cost-effective — approximately $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 could be targeted cost-effectively. In late 2010, the head of U.S. 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.

Year 2 taught managers that they needed to do more than signal that competitive information was available. Communication strategies should be targeted at consumers who do not already trust the automaker. Managers now place a premium on targeting competitive information toward skeptical consumers. New methods include interactive screening to identify consumers who answer questions that indicate they do not trust AAM. Targeted consumers would receive substantial incentives to participate in competitive-information treatments.

Based on the 2003–2005 data and managerial judgment, managers believe that providing competitive information is effective when there is good news and when it is cost effective. As of this writing, AAM includes key 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 vehicles with those of competitors regarding fuel economy and styling. Many AAM dealers offer unsolicited extended weekend test drives and encourage competitive comparisons. AAM believes that competitive information builds trust, brand consideration, and sales and is profitable 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.

Online Appendices available from the authors

OA1. Screenshots of year-2 treatments.
OA2. Alternative specifications of trust regressions in year 1.
OA3. Main effects, trust regression, and conditional-logit analyses in year 2.

Year 1 and year 2 data as used in tables are available from the authors.

Appendix A. Continuous-time Markov analysis in year 1

The analyses in the text model month-to-month dynamics but 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 & Miller, 1965; Hauser & 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 month t is treated differently if the customer was in "not consider" versus in "consider" at month $t-1$. By focusing on differences in behavioral states, the Markov analyses are less sensitive to unobserved heterogeneity.

The Markov analyses complement the conditional-logit analyses in Fig. 2; the concepts are similar, but we model a more complete set of flows and allow the flows to occur in continuous time (Fig. A1). 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 nth month. We specify as relevant the flow rate as a log-linear function of the treatment assignments, prior ownership, age, sex, month 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 month. 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 nth month and in state j at the end of the month. Specifically,

$$P_n = e^{A_n(T_n - T_{n-1})} \equiv \sum_{r=0}^{\infty} \frac{A_n^r (T_n - T_{n-1})^r}{r!} \equiv V_n[\exp\Lambda_n]V_n^{-1}, \quad \text{(A1)}$$

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 nth month, 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 of Eq. (A1) (Hauser & 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 A1. Other analyses and R-code are available from the authors.

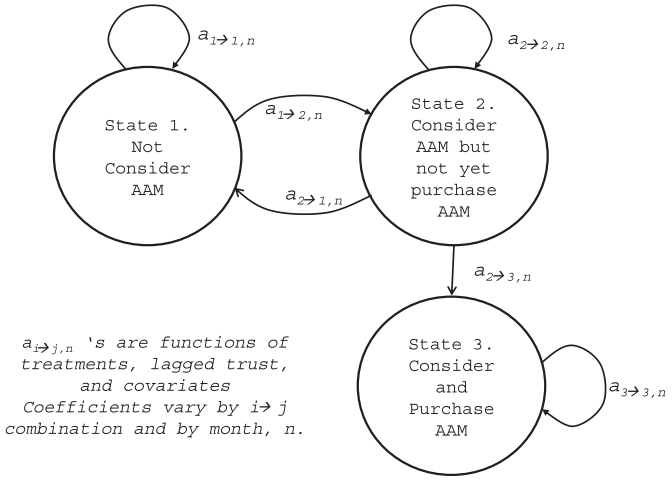

Fig. A1. Continuous-Time Markov Flow Dynamics In Each Period.

Table A1
Continuous time Markov process analysis — year 1 random assignments.

Dependent measure	Continuous time Markov estimation not mediated			Continuous time Markov estimation mediated by trust		
	Consider brand		Purchase	Consider brand		Purchase
	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[a]	.146[a]	.249[a]	.102[a]
Lagged trust hat				.221[a]	−.230[b]	.313[a]
Competitive test drives	1.060[b]	−.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 ownership of AAM	−.407	−.773		−.403	−.624	
Prior own other American	.599[a]	.070		.525 [a]	.039	
Prior ownership of Japanese	−.305	.292		−.249	.197	
Month 3	.012	−.172	.445	.032	−.289	.565
Month 4	−.971[a]	.544	−.394	−1.004[a]	.423	−.366
Month 5	−.698[a]	.122	.224	−.760[a]	.083	.194
Log likelihood	−616.46	−608.12				

All flows are estimated simultaneously.
[a] Significant at the 0.05 level.
[b] Significant at the 0.10 level.

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. We also modeled potential misclassification of "consider" vs. "not consider" as in Jackson, Sharples, Thompson, Duffy, and Couto (2003). The misclassification analyses improved fit but provided no additional managerial insights. Estimated misclassification was moderate.

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