

# Contextual Effects on the Revision of Evaluative Judgments: An Extension of the Omission-Detection Framework

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When consumers are presented with negative information about a brand that they have evaluated positively earlier, the extent to which they change their initial evaluation may depend on the formats in which information is presented (non-comparative vs. comparative) at the two stages. In four experiments, we manipulate the format in which information is presented at an initial and at a challenge stage and investigate their effects on the degree of revision in evaluative judgments. The results of the four experiments suggest that when consumers receive initial information in a noncomparative format, a comparative challenge causes a greater degree of revision in the evaluative judgments than does a noncomparative challenge. However, when the initial information is presented in a comparative format, this pattern reverses, and a greater degree of revision occurs under a noncomparative challenge than under a comparative challenge. We demonstrate that sensitivity to missing information in either of the two stages is the process by which these effects obtain. In a fifth experiment we examine a boundary condition for these effects.

Consumer preferences for products and brands evolve over time. In a variety of decision situations, people form judgments about objects and persons based on available information. Sometimes, a subsequent evaluation about the same object or person may be made upon receiving new information. It is not uncommon that the new information contests the beliefs formed out of the original information. For example, consumers may first receive positive information about attributes of a brand from an advertisement and may form an evaluation about the brand. Later, they may learn about a different set of attributes of the same brand in the product-review section of a magazine, and the story may portray the brand in a negative light. Alternatively, consumers may receive additional information about the same brand from a salesperson at a store, and in this instance, consumers may learn how the focal brand compares to a set of brands available in the store. These examples illustrate the dynamic nature of consumer judgments (Johar, Jedidi,

and Jacoby 1997). Dynamic judgments are widely prevalent in consumer settings but are underexplored from a theory-testing perspective.

In many of these situations, the initial set of information may not be available (except in memory) to the decision maker while receiving the new information. In such cases, these two-stage evaluations may be characterized by an anchoring-and-adjustment process of judgment updating (Hoch and Deighton 1989; Hogarth and Einhorn 1992); the original judgment may serve as an anchor and the revised judgment may be the outcome of an adjustment, which may be of different degrees. The problem becomes particularly interesting when the original and the new information conflict in terms of the valence of the claims.

The primary objective of our research is to examine the role of an important contextual variable—the format in which information is presented—on the degree of change in the original judgment. We manipulate the format in which the information is presented (noncomparative vs. comparative) at an initial and an updating stage, and examine the effects of these manipulations on the degree of change in the original evaluation. The role of this particular contextual factor—information about and judgment on a brand in isolation versus in the context of other brands—has been previously researched only for one-time judgments and choices (see, e.g., Hsee and Leclerc 1997; Nowlis and Simonson 1997; Tversky, Sattath, and Slovic 1988). Our

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current research extends this work in two ways. First, we study the role of context on judgment revision as opposed to one-time judgment or choices. Second, although the previous studies varied the response mode (judgment vs. choice, rating one brand vs. two brands), in our research the response mode (rating the focal brand) is constant across conditions. This allows us to distill the effects of changes in the information format alone.

We only focus on situations in which the first-stage information is positively valenced, and the second-stage information is negatively valenced, as these conditions may be expected to result in a considerable amount of change in the original evaluation. A consumer magazine, rather than an advertisement, is specified as the source of the two sets of information at the initial stage and the challenge stage. This control allows us to attribute differences between experimental conditions solely to the format of information presentation, without any confounds arising out of differences in source credibility, message believability, and so on (Grewal et al. 1997).

We derive predictions and a process explanation for the different experimental conditions by adapting and extending the omission-detection paradigm of research (Kardes and Sanbonmatsu 1993; Sanbonmatsu et al. 1991, 1997, 1998) and systematically test the outcome and process hypotheses suggested by this paradigm. Although the omission-detection model itself is silent with respect to judgments in dynamic contexts, a logical extension of this framework can offer predictions regarding the role of the informational context in judgment-updating situations.

In the next section, we describe the omission-detection hypothesis and previous research in this paradigm. We then present a conceptual extension of the omission-detection model to judgment-updating situations, in which an initial evaluation is challenged at a later stage. We derive predictions for the manipulation of information format at each of the two stages.

## THE OMISSION-DETECTION FRAMEWORK

Frequently, decision makers only have part of the relevant information available at the time of decision making. For example, in a memory-based or mixed decision situation, in which consumers need to retrieve from memory at least some information that they acquired earlier (Alba, Hutchinson, and Lynch 1991; Lynch and Srull 1982), consumers may experience uncertainty in evaluating the options. Often, even in stimulus-based decisions, in which all alternatives and information pertaining to these alternatives are present at the time of decisions, not all alternatives are described on the same attributes. In the absence of relevant information, consumers could exhibit a variety of behaviors. They could try to make inferences about the missing information from the information that is available (Alba and Hutchinson 1987; Kardes 1993, 1994). They could assign an "average" or a "typical" value for the missing informa-

tion and use this value in forming the initial impression. They could also form an overall judgment based on the available information and moderate it (i.e., adjust it away from the extremes) to reflect the unavailability of relevant information.

An important precondition to all these processes is that consumers need to sense that relevant information is missing in the decision environment or at least that the available information is insufficient. In other words, before deciding how to deal with the missing information, consumers first need to detect that information is missing. A recent program of research delineates several contextual factors that affect the likelihood that consumers will detect the absence of relevant information in the environment (e.g., Kardes and Sanbonmatsu 1993; Sanbonmatsu et al. 1991, 1997, 1998). These studies find that consumers assign a moderate evaluation to the target object when they recognize that information available is not sufficient. That is, irrespective of the values of the available information, sensitivity to missing information causes people to be cautious, or conservative, in their evaluations.

The omission-detection model has been used creatively in explaining the set-size effect reported in the information integration literature (Anderson 1971, 1981). Imagine that consumers are evaluating two brands, one described on a large set of information, and the second on a smaller set of information. The set-size hypothesis predicts that consumers will exhibit greater preference for the larger set when the features are all positive and the smaller set when the features are all negative. This effect occurs even when the value or evaluative implication of each piece of information is held constant (Anderson 1981; Kardes and Kalyanaram 1992). Importantly though, the effect was obtained mainly in studies that employed a within-subjects design, but rarely in those that employed a between-subjects design. Kardes and Sanbonmatsu (1993) suggested a conceptual explanation for the differential effects in within-subjects and between-subjects designs. According to their omission-detection hypothesis, on the one hand, the within-subjects designs afford an opportunity for subjects to realize that relevant information is missing for the object characterized by a smaller set of information, as the information about this object is presented in the context of the information about the larger set object. In a between-subjects design, on the other hand, people see only the smaller set, and they may not be aware that some additional and possibly more important attribute information is missing.

Thus, according to the omission-detection paradigm, the key driver of the set-size effect is the presence of information about an alternative in the judgment context of the object being evaluated, which cues the detection that information about the focal object is missing. The consequence of detecting that information is missing is a moderate (as opposed to an extreme) evaluation of the focal object.

The omission-detection paradigm has identified several

task and contextual factors that can influence judges' sensitivity to missing information (Kardes and Sanbonmatsu 1993). The factors include prior knowledge about the evaluation object (Sanbonmatsu, Kardes, and Herr 1992; Sanbonmatsu et al. 1991), evaluating the object by itself or amidst ratings for other objects (Sanbonmatsu et al. 1992), the order of information presentation (Kardes and Sanbonmatsu 1993), and the nature of the target product attributes (Sanbonmatsu et al. 1997, 1998). For example, even in a memory-based judgment, when presented with a smaller set of information, those who are experts in a product category recognize that certain important information is missing and consequently assign a moderate evaluation to the target brand (Sanbonmatsu et al. 1991).

Particularly relevant for the present research are the findings regarding the influence of judgmental contexts. Sanbonmatsu et al. (1997) showed that when the context of judgment includes an object that is similar to the target object but described on different dimensions, there is greater likelihood of sensitivity to the missing information and also moderation in evaluation in the large set as well as the small set. For example, automobile brands A and B may be described on five and three attributes, respectively, but the attributes may not overlap. In such a situation, both A and B would receive only moderate evaluations (compared to seeing A or B alone) because the available attributes of one object cue the recognition of the missing attributes in the other. Thus, the major theme underlying the omission-detection paradigm is that contextual cues induce judges to detect omission of information, and this sensitivity leads to a moderate evaluation.

A similar argument is presented in Hsee and Leclerc (1998) and Fox and Tversky (1995). Hsee and Leclerc (1998) found that for two different objects that were positively evaluated in isolation, presenting the objects together led to each object serving as a basis of comparison for the other. Consequently, both objects received lower evaluations when presented together (as the virtues of each object served to highlight the shortcomings of the other) relative to when they were each presented in isolation. Fox and Tversky (1995) found that subjects valued unambiguous gambles more highly than ambiguous gambles when the two types of gambles were presented together; when presented in isolation, the difference in valuation of the two types of gambles was statistically insignificant. More important, Fox and Tversky (1995) suggested that a greater degree of ambiguity aversion occurs in the comparative judgment condition because sensitivity to the ambiguous nature of one of the options occurs only when both the options are presented together. Thus, a recurrent finding in previous work in the omission-detection paradigm as well as in the behavioral decision theory stream (Fox and Tversky 1995; Hsee and Leclerc 1998) is that introducing a comparison object in the context of judging a focal object can cue the recognition that information is missing in the environment.

## INFORMATION FORMATS AND JUDGMENT UPDATING: AN EXTENSION OF THE OMISSION- DETECTION FRAMEWORK

Information about a focal object can be presented in a direct, noncomparative format or in a comparative format. In a noncomparative format, the focal object is described on several attributes in the absence of information about any other object, while in a comparative format, the focal object is described on several attributes in comparison with another object. In a judgment-revision situation, a decision maker may receive information about the focal object in each stage (an initial and an updating stage) in either of these two formats, resulting in four different conditions.

Of these four conditions, the only case considered in prior research is noncomparative information at the time of initial evaluation and noncomparative new information that is evaluatively inconsistent with prior information. For example, in Haugtvedt et al. (1994), subjects learned about several positive attributes of a target pen initially and a week later were exposed to a set of negative information about the same pen; format of information presentation was direct or noncomparative at both stages.

Extending the omission-detection paradigm to judgment-updating situations allows us to make predictions about the relative degree of adjustment of the original evaluation in each of the four conditions. Assume that the initial evaluations are controlled to be equal for both information formats, noncomparative and comparative, so that all four conditions have a similar anchor at the judgment-updating stage. At the second, updating stage, the degree of adjustment will then depend on three factors: the likelihood of omission detection, whether omission pertains to earlier information or new information, and the consequences of omission detection.

### What Influences the Likelihood of Omission Detection?

The first factor is the likelihood of missing information being detected. In our investigation at both stages subjects rate the target brand only. It may be recalled that prior research on omission detection has established that in between-subjects designs, sensitivity to missing information is low. Therefore, it is unlikely that the two formats will vary in terms of the degree of omission detection at the initial stage. Because omission detection occurs at the updating stage, the format of the *challenge* is the critical factor that determines the degree of omission detection.

Previous research suggests that at the time of judgment if the new information is in a comparative format, the likelihood of omission detection is greater (Fox and Tversky 1995; Hsee and Leclerc 1998). A straightforward extension of these findings to the judgment-revision context is that the tendency to seek whether something is missing will be greater when the challenge information is in a comparative

format. That is, a comparative format of challenge motivates consumers to search for plausible omissions. However, this motivation does not always transform to actual omission detection. A challenge that compares the target with a new brand that did not figure in the earlier comparisons on a new set of attributes offers a greater likelihood of omission detection. Because no information was provided about either the new comparison brand or the attributes earlier, consumers can readily perceive that the information provided is incomplete to update their target-brand judgments. Thus in these cases, consumers' check for omissions often leads to perceived insufficiency of information. In the following sections, we first consider these cases. Later, we discuss the conditions under which consumers consider information provided in a comparative challenge to be sufficient despite the tendency to seek omissions.

### Do the Omissions Identified Pertain to the Earlier Information or New Information?

The second factor in the extension of the omission-detection framework concerns the phase to which detection of the missing information pertains. Although omission detection occurs only at the second or updating stage, consumers may nonetheless detect either that the new information is insufficient or that the earlier information was insufficient. Although the likelihood of omission detection depends on the challenge only, whether omission pertains to the initial information or to the new information depends on both the initial information and the challenge. We now consider the four conditions in two pairs.

*Initial Information in a Noncomparative Format.* Consider the condition in which both the initial information and the subsequent information are received in a noncomparative format. In this condition, at the updating stage, there has been no comparison to cue the recognition of missing information (Fox and Tversky 1995; Hsee and Leclerc 1998). Consequently, there is little likelihood of missing information, pertaining to either stage, being detected. However, when the subsequent information is in a comparative format, a new brand is mentioned in the challenge. This may cue the respondent to realize that the information at time 1 was received in isolation and that there was no information about other brands provided, in particular about how the target brand performed relative to the new comparison brand on the attributes described at the initial stage. This should lead to the perception that the information at the first stage was insufficient. However, the two conditions will not vary in terms of sufficiency of information presented at the second stage.

*Initial Information in a Comparative Format.* Now, consider the conditions in which the initial information is received in a comparative format. At the initial stage, consumers evaluate the target brand, and the target brand alone, though in the context of a comparison brand. When the new negative information is in a noncomparative format, the focus will be on the target brand's attributes for two reasons.

**TABLE 1**  
POTENTIAL FOR OMISSION DETECTION OF THE TWO SETS OF INFORMATION IN THE FOUR EXPERIMENTAL CONDITIONS (EXPERIMENTS 1-4)

Time 1 format	Time 2 format	
	Noncomparative	Comparative
Noncomparative	Time 1 information: low Time 2 information: low	Time 1 information: high Time 2 information: low
Comparative	Time 1 information: low Time 2 information: low	Time 1 information: low Time 2 information: high

NOTE.—The entries in the four experimental cells pertain to the likelihood of omission detection (either of the initial or time 1 information, or subsequent or time 2 information) at the time of judgment updating. Detecting omission of time 1 information should lead to greater adjustment of time 1 evaluation (and hence a lower time 2 evaluation), while detecting omission of time 2 information should lead to lesser adjustment of time 1 evaluation (and hence a greater time 2 evaluation).

First, the rating once again pertains to the target brand only. Second, there is no comparison cue at the judgment-updating phase to evoke recognition of missing information (Fox and Tversky 1995; Kardes and Sanbonmatsu 1993). At the time of updating, even if consumers do recall some initial information, the rating task as well as the format of the new information causes consumers to recall information in a noncomparative manner and focus exclusively on the target brand's positive (earlier) and negative (new) attributes. However, if at the updating stage, the challenge is phrased in terms of a new comparison brand, which is presented as being superior to the focal brand, this new comparison brand may cue the recall that the earlier judgment was made in the context of a different brand. This should lead to the recognition that the current information phrased in terms of comparisons with a new brand is not complete in that they do not know how the new comparison brand fares on the attributes described at time 1. We should hence expect a greater level of omission detection of either time 1 or time 2 information when the challenge is in a comparative form relative to when the challenge is in a noncomparative form. Further, we should expect that if consumers are asked to recall the earlier information, they should be more likely to recall it in the context of the earlier comparison brand when the new information is comparative relative to when the new information is noncomparative. That the earlier message did not present information about the attributes contained in the new information is common between the two conditions. Hence, we should not expect any differences between the two conditions in terms of sufficiency of the earlier information. Table 1 summarizes the predictions about the types of omission detection likely in the four informational context conditions.



## Consequences of Omission Detection

The third factor in our extension of the omission-detection framework is the consequence of this omission detection for judgment updating. If, on the one hand, consumers realize that the *new, counterattitudinal* information is not sufficient, there may be only a *moderate change* in the original evaluation. Consistent with the omission-detection paradigm, we should find that the detection of missing information leads to a moderation in reaction to the valence of the new information. Thus, instead of changing the original evaluation drastically by making a large adjustment, consumers should make only a moderate downward adjustment to the original evaluation. If, on the other hand, there is no cue that the current (negative) information is insufficient, adjustment should be greater, leading to a greater downward adjustment to the original evaluation (because the new information, in terms of valence, is in the opposite direction of that of the original information).

However, if consumers realize that the *earlier* information was not sufficient, there may be a *large change* in evaluation. Here consumers realize that their original evaluation was based on incomplete information and, thus, the original evaluation was inflated or more extreme than it should have been. Hence, they adjust the original evaluation downward, and this results in greater evaluation change. If there is no cue that the earlier information was insufficient, then there will be less of a tendency to make a large adjustment to the original evaluation. Thus, the degree of change in the original evaluation depends not only on omission detection but also on whether the omission pertains to the initial information or the new information available at the time of judgment updating.

Thus, in summary, the extended omission-detection model leads to the following predictions about the two pairs of conditions:

- H1:** When the initial information is in a noncomparative format, a comparative challenge produces greater sensitivity to omissions in the *earlier* information and thus greater judgment revision in the direction of the new information than does a noncomparative challenge.
- H2:** When the initial information is in a comparative format, a comparative challenge produces greater sensitivity to omissions in the *new* information and thus lesser judgment revision in the direction of the new information than does a noncomparative challenge.

These predictions were tested in the first four experiments. Experiment 1 investigates the effects of the format of information at the two stages and their interaction on judgment revisions. We perform manipulation and confounding checks in this experiment. Experiment 2 examines the mediating role of sufficiency of the new, additional information in judgment updating for the comparative initial conditions. Experiment 3 studies the mediating role of

sufficiency of the earlier information in judgment updating for the direct initial conditions. Experiment 4 rules out an alternative explanation for our effect.

As suggested earlier, although a comparative format of challenge triggers a tendency to seek omissions, the actual degree of omission detection depends also on other contextual factors. For example, if a challenge involves comparing the target brand to a new brand on the same set of attributes that were used in the initial comparison, consumers do not have any reason to perceive insufficiency of the earlier or later information. Similarly, if the challenge compares the target brand to the same brand that was used in the initial comparison but on a different set of attributes, once again consumers' tendency to seek whether anything is missing will not yield any omissions being detected. A challenge of this nature causes consumers to believe that they acquired adequate information. Thus, even within a comparative format of challenge, a check for plausible omissions does not yield any results when the same brand is portrayed as the comparison brand in the initial information as well as in the challenge (on a different set of attributes). Experiment 5 investigates this issue.

## EXPERIMENT 1

### Judgment Revision as a Function of Information Formats in the Initial and Challenge Stages

In this experiment, we predict that when the initial information is in a noncomparative format (noncomparative initial), those who receive the new, negative information in a noncomparative format (noncomparative challenge) will exhibit a lesser degree of change in their prior evaluations than those who receive the new, negative information in a comparative format (comparative challenge). However, when the initial information is in a comparative format (comparative initial), those who receive the new, negative information in a direct format (noncomparative challenge) will exhibit a greater degree of change than those who receive the new information in a comparative format (comparative challenge). Further, we conduct manipulation and confounding checks.

### Method

*Subjects.* Subjects in this experiment as well as in the subsequent experiments were business undergraduates enrolled in a marketing course. Although a majority of subjects participated in the experiment to receive course credit, some subjects volunteered in exchange for about \$6 as compensation. Those who participated for money were more or less equally distributed across cells. One hundred and twelve subjects participated in the present experiment. Subjects were randomly assigned to one of four experimental conditions. Besides these, 52 subjects were assigned to two no-challenge control conditions. Data from five subjects who did not complete evaluations at both stages were excluded from the analysis.

*Stimuli and Pretests.* The target product was a pen. We adopted the target category, the name of the target brand (Omega 3), and the attributes of the target brand from previous research (Haugtvedt et al. 1994; Schuman, Petty, and Clemons 1990). We conducted a series of pretests to finalize the stimuli. The objective of the first pretest was to ensure that the names of the target brand (Omega), comparison brand in the comparative initial condition (Zebra), and comparison brand in the comparative challenge condition (Elegance) were of equal likability. Twenty-two subjects enrolled in a marketing class responded to the question, "How much do you like the following brand names?" and rated all three brand names on a nine-point like-dislike scale. The mean ratings of Omega, Zebra, and Elegance were 5.92, 5.88, and 6.04, respectively. Post hoc paired *t*-tests revealed that there was no difference between brands in any of the three pairs ( $t < 1$ ).

As in previous studies, subjects in our experiments received challenges (negative information at the second stage) that were on *less important* dimensions than the initial information. The reason for choosing a lesser important set of attributes to challenge prior evaluations is that our focus is on relative rather than absolute evaluation changes. A strong counterattitudinal message can create floor effects so that even subjects who possess a great deal of product-relevant information may succumb to the challenge (Haugtvedt et al. 1994). Given that experimentally induced attitudes lack stability and can easily succumb to challenges (Cook and Flay 1978), we decided against using a strong challenge on important dimensions. At the same time, an extremely weak challenge may result in absolutely no attitude change across experimental conditions. Hence, the challenge required some amount of calibration. Based on our judgment, we selected certain moderately important dimensions and, via a pretest ( $n = 63$ ) ensured that these dimensions were less important than the attributes provided in the initial information. We measured the importance of a set of pen attributes on a 1–11 scale anchored by unimportant and important. Based on the results (see the Appendix for the mean rating for each attribute), we chose the three attributes that received the three lowest ratings as the challenge attributes. A post hoc paired *t*-test revealed that there was a significant difference between the average ratings of the attributes used as initial information and those of the attributes that were used in the challenge ( $t(62) = 4.81, p < 0.001$ ).

A third pretest ensured that the initial attitudes did not differ between noncomparative and comparative conditions. Thirty-three subjects were assigned to one of three groups, two of which received the initial information in a noncomparative format and one received information in a comparative format. One of the noncomparative groups received information on six attributes while the other received information on only three attributes, which were a subset of the six. Subjects in the comparison group received information on the same three attributes that the latter group did, but in a comparative form (the details are provided in the next section). Subjects rated the brand on a single nine-point

scale anchored by like and dislike. There was no difference between the six-attribute noncomparative condition ( $\bar{X} = 5.91$ ) and the comparative condition ( $\bar{X} = 6.00$ ) in terms of evaluation. Although the differences were not statistically significant ( $F < 1$ ), the evaluation in the three-attribute condition ( $\bar{X} = 5.18$ ) was lower than that in the comparative condition ( $F(1, 30) = 1.67, p > 0.2; \omega^2 = 0.034$ ) or the six-attribute noncomparative condition ( $F(1, 27) = 1.59, p > 0.2; \omega^2 = 0.03$ ). The effects may be classified as small to medium sized (Keppel 1982). Hence, we decided to use the six-attribute noncomparative initial condition, which produced the same initial evaluation as the comparative condition. This leads to a possible confound between the format manipulations and the amount of information at the initial stage. From our theory-testing perspective, it is very important to control for the initial evaluations in both format conditions being of equal strength, so that the differences in the extent of revision can be attributed to the interaction of the formats rather than to the differential strength of the initial evaluations. We resolve this possible confound with an explicit comparison of three- versus six-attribute noncomparative formats in experiment 4, as described later.

A final pretest ( $n = 30$ ) tested whether the three conditions were equal in terms of negativity. A single nine-point scale (1 = not at all negative, 9 = extremely negative) measured the negativity of the challenge. The results revealed that the three-attribute noncomparative challenge ( $\bar{X} = 4.90$ ) and the six-attribute noncomparative challenge ( $\bar{X} = 5.10$ ) were closer to each other in terms of perceived negativity, and neither differed from the comparative challenge ( $\bar{X} = 5.20; F$ 's  $< 1; \omega^2$  three attributes vs. six attributes  $< 0.005$ ). Because the two noncomparative challenge conditions did not differ in terms of negativity, we decided to use the three-attribute challenge, so as to maintain the same number of attributes for both formats, and thus avoid a possible confound with the format of the challenge information. Thus, the last two pretests helped us to calibrate our stimuli in the noncomparative initial and challenge conditions.

*Design and Manipulations.* Two factors were manipulated and fully crossed, resulting in a two (format of the initial information)  $\times$  two (format of the additional, negative information that constituted a challenge to the initial information) between-subjects design. The format of the initial information that subjects received was either noncomparative or comparative. In the noncomparative initial information condition, subjects received favorable information about six attributes of the target pen, Omega 3 (e.g., "Because of its Benzene tip, Omega 3 offers smooth writing performance"). In the comparative initial information condition, the Omega 3 pen was compared to another pen, Zebra, on three attributes, and all three comparisons favored Omega 3 (e.g., "Because of its Benzene tip, Omega 3 offers a smoother writing performance compared to Zebra"). The three attributes in the comparative initial information condition were a subset of the six attributes that described the

target brand in the direct initial information condition. In both of these conditions, subjects were told that the information was supplied by a neutral source: a consumer magazine.

The format of the additional, negative information was manipulated in a similar manner. In both conditions, we told subjects that the consumer magazine featured a follow-up story three months after the introduction of the Omega 3. The new story highlighted Omega's weakness in terms of three attributes in either a noncomparative format or a comparative format. For example, one of the three attributes in the noncomparative challenge condition was, "The packages in which the Omega 3 pens were shipped were difficult to open if the instructions were not carefully followed." In the comparative challenge condition, the same story compared the Omega 3 with a new pen, Elegance, on the same three attributes as in the noncomparative challenge condition. All three comparisons portrayed Elegance to be superior. For example, "Compared to those of the Elegance, the packages in which the Omega 3 pens were shipped were more difficult to open if the instructions were not carefully followed." There were also two no-challenge control conditions to check that the two initial format conditions resulted in equal evaluation persistence, in the absence of a challenge.

*Dependent Variables.* Evaluative judgment of the Omega 3 pen was measured twice: after subjects received the initial information and after they received the additional, negative information. At both stages, product evaluation was measured on three nine-point scales anchored by very bad/very good, dislike/like, and unfavorable/favorable ( $\alpha = 0.89$ ). At time 1, subjects' confidence in evaluation was measured on a single nine-point scale anchored by not at all confident and extremely confident. Perceived negativity of the challenge information was measured on a nine-point scale anchored by not at all negative and extremely negative. Subjects' recall of the initial information served as an additional dependent variable.

*Procedure.* The experiment was administered in two sessions separated by about an hour. Subjects were told that, as participants in a marketing research project, they would be rating a pen that was to be introduced soon in the region. They were then given a report by a consumer magazine that described several pieces of favorable information about Omega 3, the target pen. At this stage, the format of the initial information was manipulated. Half the subjects received the information in a noncomparative format while the other half received it in a comparative format. Subjects were asked to rate the pen on a set of scales and to express confidence in their judgment; these ratings constituted the initial evaluative judgment of the target pen. For the next hour, subjects were asked to perform a number of other tasks that are not relevant for the present set of experiments. At the end of this period, subjects in the control conditions were asked to evaluate the target brand once again. Subjects in the experimental conditions were told that three months after the introduction of the pen the same

consumer magazine issued an updated report about the pen. This updated report described additional negative information about the target pen. Once again, the format of information was manipulated (direct or comparative). Finally, subjects rated the target pen on the same scales that constituted the initial judgments and also expressed confidence in their judgments. They also responded to a questionnaire that measured the dependent variables of recall of the earlier information and perceived negativity of the additional information.

*Analysis Plan.* As the primary objective of this research is to understand the extent to which judgments undergo change on being challenged, we performed a mixed ANOVA in which time between judgments served as a repeated factor at two levels and the experimental variables were treated as between-subjects factors.

## Results

*Confounding Checks.* We ensured that the manipulation of the challenge formats did not result in unequal negativity across conditions. A single factor, between-subjects ANOVA showed that there was no effect of the format of additional information on perceived negativity ( $F(1, 103) < 1$ ;  $\bar{X}_{\text{direct}} = 5.51$  and  $\bar{X}_{\text{comparative}} = 5.50$ ). To examine whether the noncomparative and comparative groups of initial information varied in terms of evaluation persistence, the two control groups' ratings of the target brand at time 1 and time 2 were submitted to a mixed ANOVA, with time as a within-subjects factor and initial information as a between-subjects factor. The analysis revealed that neither the effects of time and initial information nor their interaction was significant ( $F$ 's(1, 103)  $< 1$ ;  $\bar{X}_{\text{direct}}$  goes down from 6.82 to 6.64 and  $\bar{X}_{\text{comparative}}$  goes down from 6.88 to 6.55). Thus, there is no evidence that the two initial format conditions created unequal evaluation persistence. Finally, the two initial information conditions did not vary in terms of judgmental confidence measured at time 1 ( $F(1, 103) < 1$ ;  $\bar{X}_{\text{direct}} = 5.78$  and  $\bar{X}_{\text{comparative}} = 5.84$ ).

*Judgment Revision.* Table 2 presents the evaluation means of the experimental conditions at time 1 and time 2. Time 1 and time 2 evaluative judgments were submitted to a three-way mixed ANOVA in which the formats of initial information and challenge were considered as between-subjects factors and time as a within-subjects factor. The analysis revealed a significant time  $\times$  initial information  $\times$  challenge interaction ( $F(1, 103) = 7.60, p < 0.01$ ). The follow-up analyses (according to the initial information condition) revealed that in the noncomparative initial conditions, there was a significant time  $\times$  challenge interaction ( $F(1, 103) = 4.46, p < 0.04$ ). That is, the degree of change in the prior judgment depended on the type of challenge encountered. Specifically, the comparative form of attack resulted in greater change in prior judgments. When the challenge was in a noncomparative form, prior evaluation reduced from 6.81 to 5.32. However, when the attack was in a comparative form, there was a greater reduction in prior evaluation ( $\bar{X}_{\text{time 1}} = 6.96$  to  $\bar{X}_{\text{time 2}} = 4.65$ ). In the comparative initial conditions, the time  $\times$  challenge interaction



**TABLE 2**  
MEANS FOR EVALUATION AND CONFIDENCE IN DIFFERENT CONDITIONS OF EXPERIMENT 1

Time 1 (+ve) manipulations	Time 2 (-ve) manipulations	Time 1 evaluation	Time 1 confidence	Time 2 unadjusted evaluation
Noncomparative	Noncomparative	6.81	5.75	5.32
Noncomparative	Comparative-E	6.96	5.81	4.65
Noncomparative	No challenge	6.82	5.60	6.64
Comparative-Z	Noncomparative	6.73	5.81	4.35
Comparative-Z	Comparative-E	6.94	6.08	5.28
Comparative-Z	No challenge	6.88	6.02	6.55

NOTE.—Z = Zebra brand pen, and E = Elegance brand pen. +ve = positive information; -ve = negative information.

approached significance ( $F(1, 103) = 3.2, p < 0.1$ ). Nonetheless, the noncomparative challenge created greater judgment revision ( $\bar{X}_{\text{time } 1} = 6.73$  to  $\bar{X}_{\text{time } 2} = 4.35$ ) than did the comparative challenge ( $\bar{X}_{\text{time } 1} = 6.94$  to  $\bar{X}_{\text{time } 2} = 5.28$ ). Thus, the pattern of judgment revision obtained under noncomparative initial conditions was reversed for the comparative initial conditions.

## Discussion

The pretests and the confounding checks ensured that the evaluations at time 1 for both formats were equal in terms of extremity, confidence, and persistence. Thus, we were able to ensure that effects caused by the interaction of the formats operated only on the degree to which the original judgments were revised. The results of this experiment show that the revisions depend on the format of the initial information and the format of the additional, negative information that challenges the prior judgments. Specifically, as predicted by our extension of the omission-detection model, there is an interesting interaction between these two factors. When the initial information is in a noncomparative format, a comparative challenge creates greater judgment revisions relative to a noncomparative challenge. Conversely, when the initial information is in a comparative format, a noncomparative challenge causes a greater change in judgment relative to a comparative challenge.

This experiment does not throw direct light on the process predicted by this model. However, the result of an additional analysis on the recall protocols is suggestive. The recall protocols (recall at time 2 of information presented at time 1) were coded into accurate (if subjects mentioned the attributes presented at time 1) and inaccurate recalls (if subjects mentioned any aspect that was not part of the initial information) by two judges who were blind to the objectives of the experiment as well the experimental manipulations. There was no difference across conditions in terms of the proportion of the initial information recalled accurately. Of the two comparative initial conditions, subjects in the comparative challenge condition are expected to detect missing information by recalling that the earlier information was about a differ-

ent brand. That is, both these conditions prompted subjects to encode the initial information in a comparative form. However, only subjects in the comparative challenge condition are cued to recall the initial information in a comparative format. Subjects who receive the challenge in a noncomparative form will be cued to focus only on the target brand (its weaknesses presented in the challenge and its strengths presented earlier) because of the task (rating only the target brand) and the noncomparative format of the new information. Those who receive the challenge in a comparative manner, however, may focus on the differences between the initial and challenge information in terms of attributes as well as comparison brands. Prior research has shown that the comparison format creates mental impressions that are relative (Rose et al. 1993). Thus, subjects in the comparative challenge condition are more likely to recall the earlier information in comparative form than are those in the noncomparative challenge condition. The recall of the initial information was coded as being either of comparative or noncomparative nature by the same judges who coded them as accurate and inaccurate. There was perfect agreement between judges. The simple effect analysis for the number of accurate comparative-type recalls in the two conditions was in the predicted direction ( $\bar{X}_{\text{noncomparative challenge}} = 0.17, \bar{X}_{\text{comparative challenge}} = 0.40; F(1, 49) = 4.89, p < 0.05$ ). Although this may be considered indirect evidence of more omission occurring in the comparative challenge condition, in experiments 2 and 3 we conduct a direct mediational test of omission detection as the process by which differences in changes in evaluations occur across conditions of information format.

## EXPERIMENT 2

### The Mediational Role of Perceived Insufficiency of the Challenge Information in Judgment Revision

Based on our extension of the omission-detection model, we predict that the differences in the updated judgments in the two comparative initial conditions should be dependent



**TABLE 3**  
MEANS FOR EVALUATION AND SUFFICIENCY IN DIFFERENT CONDITIONS OF EXPERIMENT 2

Time 1 (+ve) manipulations	Time 2 (-ve) manipulations	Time 1 evaluation	Time 2 unadjusted evaluation	Sufficiency of time 2 information
Noncomparative	Noncomparative	6.74	4.73	5.54
Noncomparative	Comparative-E	6.39	3.81	5.21
Comparative-Z	Noncomparative	6.73	4.56	5.50
Comparative-Z	Comparative-E	6.46	5.25	4.07

NOTE.—Z = Zebra brand pen, and E = Elegance brand pen. +ve = positive information; -ve = negative information.

on the perceived insufficiency of the challenge information. In particular, perceived insufficiency should be higher for the comparative challenge condition relative to the noncomparative challenge condition because the comparative challenge involves a new brand along with a new set of attributes. We do not predict any differences between the two noncomparative initial conditions in terms of insufficiency of the new information. In these conditions, it is the perceived insufficiency of the initial information that is expected to drive the differences in the updated judgments; this prediction is tested in experiment 3.

## Method

The only change in the design from experiment 1 was that the two no-challenge control groups were excluded. This experiment followed almost exactly the same procedure. Apart from the main dependent variable of change in judgments, the only other variables measured (after evaluation at time 2) were perceived sufficiency of the additional, negative information and perceived negativity of the additional information, in that order. A single nine-point scale anchored by insufficient and sufficient (similar to but not exactly the same scale used by Sanbonmatsu et al. [1997]) measured sufficiency. Also, we did not collect information about recall of the earlier information or confidence, so that they would not interfere with the sufficiency measure. One hundred and five subjects were randomly assigned to one of four experimental conditions. Data from two subjects who did not complete all tasks were excluded.

## Results

*Judgment Revision.* As in the previous experiment, the initial and postchallenge evaluative judgments (evaluations at time 2) were submitted to a three-way mixed ANOVA with the format of initial information and the format of challenge as the between-subjects factors and time as a repeated factor. Again, as predicted, the analysis revealed a significant time  $\times$  initial information  $\times$  challenge interaction ( $F(1, 97) = 11.97, p < 0.01$ ). Further, the follow-up analyses according to the initial information conditions replicated the pattern obtained in experiment 1. When the initial information was in a noncomparative format, a com-

parative challenge resulted in a greater change in the postchallenge evaluation ( $\bar{X}_{\text{time } 1} = 6.39$  to  $\bar{X}_{\text{time } 2} = 3.81$ ) than did a noncomparative challenge ( $\bar{X}_{\text{time } 1} = 6.74$  to  $\bar{X}_{\text{time } 2} = 4.73$ ;  $F(1, 97) = 4.41, p < 0.04$ ). We observed a reversal of this pattern when the initial information was presented in a comparative format. The simple effect of the format of additional information on evaluation change (time-by-challenge interaction) was significant ( $F(1, 97) = 7.91, p < 0.01$ ). However, the change in evaluation was lower in the comparative challenge condition ( $\bar{X}_{\text{time } 1} = 6.46$  to  $\bar{X}_{\text{time } 2} = 5.25$ ) than in the noncomparative challenge condition ( $\bar{X}_{\text{time } 1} = 6.73$  to  $\bar{X}_{\text{time } 2} = 4.56$ ).

*Sufficiency of the Challenge Information.* The omission-detection model predicts that there would be differences between the two comparative initial information groups, but not between the two noncomparative initial information groups. We performed two planned contrasts to test this prediction. The contrast between the noncomparative challenge and comparative challenge groups within the comparative initial condition was significant ( $F(1, 97) = 11.75, p < 0.01$ ;  $\bar{X}_{\text{noncomparative challenge}} = 5.50$  and  $\bar{X}_{\text{comparative challenge}} = 4.07$ ). However, as predicted, there was no difference between the two groups in the direct initial conditions ( $F < 1$ ) in terms of perceived sufficiency of the additional information ( $\bar{X}_{\text{noncomparative challenge}} = 5.54$  and  $\bar{X}_{\text{comparative challenge}} = 5.21$ ). Table 3 presents the time 1 and time 2 evaluations as well as sufficiency scores.

*Mediational Analysis.* To investigate the mediating role of perceived sufficiency of additional information in the information format postchallenge evaluation relationship within the comparative initial condition, we conducted the tests suggested by Baron and Kenny (1986). The authors recommend three tests to establish the mediation process. First, the independent variable should have a significant effect on the dependent variable. Second, the independent variable should have a significant effect on the presumed mediator. Third, when both the independent variable and the mediator are included in the model, the mediator but not the independent variable should have a significant effect on the dependent variable. The tests strongly supported the mediational role of sufficiency of the challenge information for the comparative initial conditions. First, as mentioned earlier, the

independent variable (i.e., noncomparative vs. comparative challenge) had a significant influence on the degree of change in the postchallenge brand evaluation within the comparative initial conditions. Second, the independent variable had a significant effect on the putative mediator, perceived sufficiency ( $F(1, 97) = 11.48, p < 0.01$ ). Finally, when both the independent variable and the presumed mediator were included in the model, the time-by-challenge effect reduces to nonsignificance ( $F(1, 96) = 2.15, p > 0.15$ ); the mediator exerted a significant effect as indicated by the time  $\times$  sufficiency interaction ( $F(1, 96) = 7.32, p < 0.01$ ). Thus, within the comparative initial information conditions, the effect of the format of challenge on changes in evaluations was mediated by the perceived sufficiency of additional information. These results did not obtain for the noncomparative initial information condition.

## Discussion

The key finding of experiment 1, that the postchallenge evaluations depend on an interaction between the format of initial information and the format of additional, negative information, was replicated in this experiment. Moreover, we identified a process when the initial information was provided in a comparative format. Subjects who received both the initial information and challenge information in a comparative format were more likely to detect that the additional information was insufficient to revise their current evaluations, relative to those who received the initial information in a comparative format and the challenge in a noncomparative format. Note that for the former group, the comparisons pertain to a new set of attributes, and the comparison brand is also new. The mediational analysis highlights the role of perceived insufficiency in explaining the differences between the two groups in terms of evaluation change, and the results in the comparative initial cells are consistent with the predictions of the extended omission-detection model.

As predicted, there were no differences in perceived sufficiency of the time 2 information between the two challenge conditions for the subjects in the noncomparative initial conditions. Now, what explains the differences in evaluation updating between the two noncomparative initial information groups? Experiment 3 investigates the process that explains the results in the noncomparative initial information groups.

## EXPERIMENT 3

### The Mediational Role of Perceived Insufficiency of the Initial Information in Judgment Revision

As described earlier, our extension of the omission-detection model predicts that the difference between the time 2 evaluations of the two noncomparative initial groups should depend on the perceived sufficiency of the initial information (as measured at the second stage). The lesser

the perceived sufficiency, the greater the change in the evaluation. Of subjects in the noncomparative initial conditions, those who face a comparative challenge should consider the earlier information to be more insufficient than those who face a noncomparative challenge, since the presence of the comparison brand at time 2 cues the recognition that the initial information was provided in isolation. Thus, differences in the updated evaluations of the two noncomparative initial conditions should be mediated by the extent to which subjects perceived the earlier information to be insufficient.

## Method

We retained only the noncomparative initial information condition of experiments 1 and 2. Within this condition, we manipulated the type of challenge (either direct or comparative; the manipulation occurred at time 2). After subjects responded to the scales measuring evaluation at time 2, they answered a question pertaining to the sufficiency of the initial information. The question was, "How sufficient was the information about Omega 3 that you received at the beginning of this session to evaluate the pen accurately?" The response to this question was measured on a nine-point scale anchored by extremely insufficient and extremely sufficient. The remainder of the procedure was exactly same as in the noncomparative initial information cells of experiment 2. Eighty-seven subjects were randomly assigned to the two experimental groups.

## Results

The initial and revised evaluations were submitted to a mixed ANOVA with the type of challenge as a between-subjects factor and time as a repeated factor. The analysis revealed a significant time-by-challenge interaction ( $F(1, 85) = 4.78, p < 0.04$ ). As expected, the change was lesser in the noncomparative challenge condition (6.63 to 4.84) than in the comparative challenge condition (6.81 to 4.32). Perceived sufficiency (of the initial information) ratings were submitted to a single-factor ANOVA with the format of the challenge as a between-subjects factor. The effect of the information format on sufficiency approached significance ( $F(1, 85) = 3.72, p < 0.06$ ); the difference between the means was of the predicted sign ( $\bar{X}_{\text{direct challenge}} = 5.58$  and  $\bar{X}_{\text{comparative challenge}} = 4.70$ ). Finally, there is evidence that sufficiency of the earlier information mediates postchallenge evaluations. When sufficiency was included as a covariate in the model, the effect of the format of the additional information on postchallenge evaluation was not significant ( $F < 1$ ); the effect of sufficiency as a covariate was strong as suggested by time  $\times$  sufficiency interaction ( $F(1, 84) = 19.47, p < 0.001$ ). These results support our hypothesis that when the initial information is in a noncomparative format, the comparative challenge cues the recognition that the earlier information was inadequate, that the evaluation should have been more moderate at time 1, and hence leads to greater change in the new evaluation.

## EXPERIMENT 4

### Does Amount of Information Explain the Results?

As explained earlier, in experiments 1 and 2, the initial information conditions varied not only in terms of format (noncomparative and comparative) but also in terms of the number of attributes (six for the noncomparative format and three for the comparative format). We deliberately confounded the number and format in order to equate the initial evaluations in the two conditions. The present experiment aims to rule out the alternative explanation that the number rather than format caused the effects we obtained. In the present experiment, along with a six-attribute noncomparative initial condition and three-attribute comparative initial condition, we added a three-attribute noncomparative condition. If the magnitude of change in the new condition is greater than that of the comparative initial condition but is of the same magnitude as that of the six-attribute noncomparative condition, it is evidence against the alternative explanation of the amount of information driving the effect.

In the new condition, the target attribute is described in a noncomparative manner in terms of the same three attributes that were used in the comparative condition. All three groups face a comparative challenge at time 2. Perceived sufficiency of the initial information is measured in the same manner as in experiment 3. The remainder of the procedure is the same as that of previous experiments. Eighty-seven subjects were randomly assigned to one of three initial information format conditions.

### Results and Discussion

The data were submitted to a mixed ANOVA with time as a within-subjects factor and the initial information as a between-subjects factor at three levels (three-attribute noncomparative, six-attribute noncomparative, and comparative). The results revealed that the initial evaluations in three-attribute noncomparative condition (6.16) was significantly less than those of the six-attribute noncomparative condition (6.74;  $F(1, 84) = 3.92, p < 0.05$ ) or the comparative condition (6.93;  $F(1, 84) = 6.55, p < 0.02$ ). There was a significant time-by-initial-information interaction ( $F(1, 84) = 3.41, p < 0.04$ ). Table 4 presents the mean evaluations and sufficiency in different conditions. We performed planned contrasts between each of the noncomparative conditions and the comparative condition. The interaction between time and the noncomparative (three attributes) comparative contrast was significant ( $F(1, 84) = 5.59, p < 0.02$ ). Similarly, the interaction between time and the noncomparative (six attributes)-comparative contrast was significant ( $F(1, 84) = 4.69, p < 0.04$ ). The reductions in evaluation after a challenge were 2.11, 2.04, and 1.35 scale units in the three-attribute noncomparative, six-attribute noncomparative, and comparative conditions, respectively.

The results obtained in a single-factor between-subjects ANOVA for the dependent variable of sufficiency paral-

**TABLE 4**  
MEANS FOR EVALUATION AND SUFFICIENCY IN DIFFERENT CONDITIONS OF EXPERIMENT 4

Time 1 (+ve) manipulations	Time 1 evaluation	Time 2 unadjusted evaluation	Sufficiency of time 1 information
Three attributes-noncomparative	6.16	4.05	3.97
Six attributes-noncomparative	6.74	4.70	4.47
Comparative-Z	6.93	5.58	5.39

NOTE.—Z = Zebra brand pen, and E = Elegance brand pen. Challenge across conditions was in the form of comparative-E. +ve = positive information; -ve = negative information.

leled those for evaluation. A planned contrast between the noncomparative (three-attribute) and comparative conditions revealed that perceived sufficiency was significantly greater in the comparative condition ( $F(1, 84) = 11.55, p < 0.001$ ). Interestingly, despite receiving information about greater number of attributes, after facing a comparative form of challenge, subjects in the noncomparative (six-attribute) condition perceived the earlier information to be less sufficient than those in the comparative initial condition ( $F(1, 84) = 4.94, p < 0.03$ ).

To perform the mediational tests, we submitted time 1 and time 2 evaluations to a two-factor mixed ANOVA that included initial information format as a between-subjects factor at three levels, time as a repeated factor, and sufficiency as a covariate. The inclusion of perceived sufficiency in the model caused the previously significant interaction between time and the noncomparative (three attributes)/comparative contrast to become nonsignificant ( $F < 1$ ). Further, the previously significant interaction between time and the noncomparative (six attributes)/comparative contrast also reduced to nonsignificance. Thus, this experiment allows us to rule out explicitly the possibility that it was the amount of information rather than the format of the information presented that led to the results replicated in experiments 1 and 2.

## EXPERIMENT 5

### The Comparison Brand at Time 2 and Omission Detection

The objective of this experiment is to demonstrate that a comparative form of challenge need not always lead to a greater level of omission detection. We maintained the same information format at both time 1 and 2 (both were comparative) and only manipulated the comparison brand at time 2. In both conditions, at the first stage, subjects received information about the target brand, Omega 3, in comparative form against a brand, Zebra. At time 2, in one condition, the comparative challenge was about the same brand Zebra (old-brand condition). Although it fared worse than the target brand in comparisons at time 1, Zebra was

described in superior terms on a new set of attributes that were less important than the original attributes. In the other condition, the comparative challenge was described in terms of a new, superior brand, Elegance, as in experiments 1–4 (new-brand condition). The attributes used for comparison were the same across the two conditions.

In these situations, our extended omission-detection model predicts that relative to when the comparison at the second stage involves a new brand, when the comparison is in terms of the same brand as before, subjects will consider this set of information (about Omega 3 and Zebra) to be more complete. Because omission detection (about the information at the second stage) leads to moderation in evaluation updating, subjects in the old-brand condition, one that causes greater perceived sufficiency of the recent, negative information, will change their original evaluation more than those in the new-brand condition.

An additional objective of this experiment is to rule out another alternative explanation for the findings obtained in experiments 1 and 2. A simpler explanation behind the interactive pattern of results replicated in experiments 1 and 2 is that the change in the format of information presentation at time 1 and time 2 causes the more recent information to be more heavily weighted. In the noncomparative initial-comparative challenge and in the comparative initial-non-comparative challenge conditions, the latest information is presented in a different format from the earlier information. The greater salience of a novel information format (within the context of this study) may lead to more weight being placed on the negative information at time 2 and hence a greater change in judgment in these two conditions, a pattern consistent with our results on the main dependent variable. However, this pattern does not obtain with respect to the supplementary dependent variable of perceived sufficiency of time 2 information. In the present experiment, because the format of information in both stages is the same in both experimental conditions, the change in format argument cannot be offered as an alternative explanation.

## Method

The only factor manipulated in this experiment was the comparison brand used in the comparative challenge condition. Eighty-five subjects were assigned to one of two experimental groups. In both groups, subjects received the initial positive information about the target brand, Omega 3, in a comparative format (against brand Zebra). After about an hour, they received additional negative information, again in a comparative format. In one group, both the attributes of comparisons and the comparison brand (Elegance) were new. In the other group, the attributes of comparisons were new, but the comparison brand was the same that figured in the initial information (Zebra). After the postchallenge evaluations, subjects rated the sufficiency of the additional information.

## Results and Discussion

As predicted, brand of comparison had a significant effect on the perceived sufficiency of the additional information ( $F(1, 79) = 3.87, p < 0.05$ ); four subjects did not respond to the sufficiency question. When the comparison brand used in the challenge was the same brand that was used in the initial information, subjects perceived a greater sufficiency of the additional information ( $\bar{X} = 4.49$ ) than when the comparison brand was new ( $\bar{X} = 3.73$ ).

The initial and postchallenge evaluations were submitted to a mixed ANOVA in which brand of comparison and time served as a between-subjects factor and a repeated factor, respectively. As predicted, we obtained a significant brand of comparison  $\times$  time interaction ( $F(1, 83) = 10.66, p < 0.01$ ). Evaluation change was greater for the old comparison brand group (7.00 to 4.51) than the new comparison brand group (6.71 to 5.24). Finally, when sufficiency was included as a covariate in the model, the effect of brand-by-time interaction reduced considerably, although still significant ( $F(1, 73) = 4.3, p < 0.05$ ). There was almost an 80 percent reduction in the effect mean square. These results suggest that along with the format of information, the specific brand of comparison can also influence the degree of omission detection and thereby postchallenge brand evaluation.

## GENERAL DISCUSSION

In five experiments, we examined the effect of contextual factors—initial information and challenge in a noncomparative versus comparative format—on the degree of revision in an initial judgment. In experiment 1, we demonstrated that when the initial information is in a noncomparative form, a comparative challenge creates greater revision of initial evaluations. The pattern reverses when the initial information is in a comparative format. In experiment 2, we examined the potential mediational role of the perceived insufficiency of the new, negative information. We found that the perceived insufficiency of the information contained in the challenge mediates the effect obtained in the comparative initial conditions but not in the noncomparative initial conditions. Experiment 3 offered evidence for the mediational role of perceived insufficiency of the initial information in the effects obtained in the noncomparative initial conditions. Experiment 4 compared the noncomparative and comparative initial conditions under a comparative challenge and showed that the perceived insufficiency of the initial information as well as the change in evaluations were greater in the noncomparative initial condition. In addition, we ruled out an alternative explanation concerning an unequal amount of information in the initial conditions. Finally, experiment 5 sheds additional light on what causes the omission detection. The findings strengthen the omission-detection explanation for comparative challenges of different types. Although one could offer plausible rival explanations for our results, the omission-detection model seems to be the most parsimonious explanation of our



results. In the following section, we discuss why some important alternative interpretations of our experimental results are not supported by the pattern of results across the five experiments.

### Alternative Interpretations

First, can the results be explained by differences in the qualitative properties, (such as strength, persistence, etc.) of the initial evaluations caused by the two initial information conditions? We included explicit controls and analyses to reduce this possibility. In both experiments 1 and 2, evaluation scores (or attitude extremities) were the same across all four conditions at time 1. In experiment 1, confidence in the initial evaluation was also the same across all four conditions. The control groups in experiment 1 also demonstrated that in the absence of a challenge, the persistence of the evaluation did not differ across the two initial information conditions.

An important counterexplanation to be considered in any context-effect experiment in which the dependent variable is a rating is that the effects operate only on the response language (i.e., the way in which subjects anchor the rating scales) and that there is not a real context effect (Lynch, Chakravarti, and Mitra 1991; Parducci 1965). In our study, subjects in the comparative initial conditions may be particularly subject to this phenomenon. Subjects who see the information about Omega 3 at time 1 in the context of an inferior pen, Zebra, may give an inflated rating for Omega 3 at time 1. Conversely, the presence of a superior pen, Elegance, at time 2, may lead to a deflated rating for Omega 3 at time 2. Thus, if response language effects were the only factor driving the evaluations, we should expect that between the two comparative initial conditions the change in judgment should be higher in the comparative challenge condition relative to the noncomparative challenge condition. Instead we obtain the opposite result. The equal ratings at time 1 for all four conditions in both experiments 1 and 2 (i.e., no difference between the noncomparative initial and comparative initial conditions) further rule out the possibility that our results are caused by response language effects.

Another alternative explanation might be based on Anderson's (1981) information-integration model. This explanation might suggest that changing the formats between the initial and the new information may lead to greater weight being placed on the new information. For example, because of its salience, comparative additional information that follows the direct format of initial information may increase the weight of the new information. Thus, salience itself determines the weight assigned to old versus new information. However, the results are not entirely consistent with the salience explanation. For example, there is no difference between judgment revision in the noncomparative-noncomparative and the comparative-noncomparative conditions in experiment 2, while there is a difference between these two conditions in experiment 1. Further, as discussed earlier, experiment 5 provides a situation in which

the experimental conditions do not vary in terms of formats of initial information or challenge but still vary in terms of evaluation change because of an unequal amount of perceived sufficiency across conditions. Finally, the systematic mediational tests in experiments 2–5 offer further support for the process explanation based on the extended omission-detection model. Therefore, the evidence for the salience explanation is mixed, while there is consistent evidence favoring the extended omission-detection model.

Prior research has identified differences between non-comparative and comparative information in terms of how information is encoded. For example, Rose et al. (1993) suggest that comparisons create mental impressions that are relative rather than absolute (see also Meyers-Levy 1991). It is probable that these processing differences account for the greater likelihood of perceiving uncertainty (insufficiency detection) in the comparative challenge conditions. Future research should attempt to specify how exactly these processing differences account for differences in the degree of sensitivity to missing information.

### Extending the Omission-Detection Model

Considered together the results of the five experiments suggest that greater perceived insufficiency of the additional information causes lesser changes in the prior evaluations. Also, greater perceived insufficiency of the initial information leads to greater evaluation changes in the direction of the additional information. The format of the initial information, the challenging information, and the comparison brand can all influence the extent of perceived sufficiency, and hence the postchallenge brand evaluations. Thus, our work extends the omission-detection framework in three important ways. First, we adapt and extend the framework to a two-stage judgment context, and derive and test several predictions. Second, we suggest additional contextual factors (formats of information, object of comparison) other than the amount of information and the variables that create differences in processing capacity such as the number of alternatives presented and time pressure (Sanbonmatsu, Shavitt, and Gibson 1994) that can influence perceived sufficiency. Third, the results suggest that detection of omissions of different types (specifically, insufficiency of earlier vs. new information) leads to different patterns of evaluation updating.

The exact process through which the recognition of missing information leads to a moderate evaluation needs further research. Consumers may assign moderate or typical values to the missing attributes, and a moderate evaluation may reflect this process. On the contrary, consumers may discount either the initial or additional information once they perceive that certain aspects are missing and such discounting may not be mediated by inferences about attribute values (Simmons and Lynch 1991). Future research should attempt to tease apart different explanations for the effect of sensitivity to the missing information on judgment revisions. Future research should also investigate boundary conditions such as processing capacity, the order of positive

versus negative information, the message source, and the reliability of the message claims for the effects that we obtained.

### Implications for Theories of Persuasion

In both psychology and marketing, theories that examined the long-term impact of persuasion (such as persistence of evaluations and resistance to counterpersuasive messages) have been tested almost exclusively in noncomparative contexts. That is, both at the time of evaluation formulation and at the time of evaluation revision, consumers received information about the target brand in a noncomparative format, which probably induced an isolated mode of thinking. However, our findings suggest that the comparative mode of thinking produces a different pattern of judgment revision relative to an isolated mode of thinking. It will be useful to extend prior theories of attitudinal resistance so as to accommodate the effects of the contexts of evaluation.

### Implications for Advertising

We would like to reiterate that in our experiments subjects were informed that the message originated from a neutral source (consumer magazine). Hence, at this time, we can only speculate about the implications for advertising because noncomparative and comparative ads may vary in terms of source credibility and message believability (Grewal et al. 1997). Nonetheless, the real world abounds in examples that mirror our manipulations, where initial positive information on some attributes is countered with negative information on different attributes in the same or different format. For example, while the product-review sections of many magazines often provide information about a single brand, reviews in *Consumer Reports* compare several brands in a product category. A consumer may first receive positive information about a target brand provided in a noncomparative format in a product-review report. Later he or she may come across a comparative review in *Consumer Reports* that makes the weakness of the brand transparent. Similarly, one may come across positive information about the brand via a comparative report and later encounter negative information about the same brand in a noncomparative format from a product-review report. Controlling for source credibility and message believability, evaluations based on a noncomparative claim may be more resistant to a negative, noncomparative message than to a comparative message. An evaluation based on a comparative advertisement, on the contrary, is more vulnerable to a noncomparative attack than to a comparative attack. If these findings hold when the sources of the message are ads released by the target and competitor brands, the findings have powerful implications for advertisers and can shape the strategies for preemptive and defensive advertising.

## APPENDIX

**TABLE A1**  
THE ATTRIBUTES AND THEIR IMPORTANCE RATINGS

Attributes	Time and condition(s) in which it appeared	Importance rating
Special erasing feature that eliminates smudges	Noncomparative and comparative initial	8.66
The Benzene tip that facilitates smooth, no-skip writing	Noncomparative and comparative initial	9.38
The smear-proof, quick-drying ink that improves writing performance	Noncomparative and comparative initial	8.49
A special feature that helps a comfortable grip	Noncomparative initial	8.34
Sloped design and optimal balancing	Noncomparative initial	8.61
The special pressurized cartridge that allows for writing at any angle	Noncomparative initial	9.00
Performance on nonporous surfaces such as glass or ceramics	Challenge	7.92
Lasting beyond five years	Challenge	7.67
Ease (or difficulty) of opening the packages in which the pens were shipped	Challenge	7.90

[Received April 1998. Revised December 1998. Robert E. Burnkrant served as editor, and Richard F. Yalch served as associate editor for this article.]

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