

## A REPLY TO STIFF AND BOSTER

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IN the limited space accorded here we will address the major errors and misconceptions perpetuated by the Stiff and Boster response to our critique of Stiff's 1986 article. In keeping with the organization of the previous papers, our comments will focus first on the major conceptual and then the methodological issues on which Stiff continues to be confused.

### CONCEPTUAL ISSUES

Perhaps the most disturbing aspect of the reply by Stiff and Boster is the extent to which their paper ignores many of the conceptual points made in our initial critique and continues to provide inaccurate characterizations of our research and writing. As just one example of the latter, Stiff and Boster assert that "in no instance do they (Petty & Cacioppo) predict that both central and peripheral information influence attitudes" (p. 251). By this, they presumably mean to suggest that it would be inconsistent with the Elaboration Likelihood Model (ELM) if manipulations of both argument quality and peripheral cues had an impact on attitudes. This is not true. At any given level of elaboration likelihood, it is possible for both arguments and cues to have some absolute (statistically significant) impact on attitudes. The crucial ELM prediction is that as the elaboration likelihood is increased (e.g., via increased issue-relevance, personal responsibility, etc.), issue-relevant arguments should exert a greater impact on attitudes whereas peripheral cues should exert a smaller impact on attitudes. Conversely, as the elaboration likelihood is decreased (e.g., via increasing distraction), peripheral cues should exert a larger impact on attitudes, but the cogency of the message arguments should have a smaller impact (Petty & Cacioppo, 1981; 1986a).

Our research is consistent with this theorizing but is misrepresented by Stiff and Boster. For example, they describe the results of the Petty, Cacioppo, & Schumann (1983) study as finding that "product (argument) quality provided a central cue which only affected attitudes under high relevance conditions." (p. 251). It is not true that argument quality only affected attitudes under high relevance in this study—argument quality affected attitudes under low relevance as well. The important point is that consistent with the ELM, relevance and argument quality *interacted*. Although Stiff cites this study in both his original article and the reply to us, he apparently failed to read the following passage from the results section:

An Involvement X Arguments interaction [ $F(1,148) = 18.47, p < .0001$ ] revealed that although argument quality had an impact on product attitudes under both low involvement [ $F(1,148) = 5.4, p < .02$ ] and high involvement [ $F(1,148) = 71.36, p < .0001$ ] the impact of argument quality on attitudes was greater under high than low involvement (Petty et al., 1983, p. 141).<sup>1</sup>

If we cast aside the miscitations of our work, at the root of the two papers are still some fundamental misperceptions of both the general purpose and the specific

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postulates of the ELM (Petty & Cacioppo, 1981; 1986b). First and foremost, it is important to note that the ELM is (and has always been) a theory of *persuasion*. As such, the ELM makes predictions about when, why, and how variables have an impact on attitude change. Thus, for example, the ELM makes predictions about the role of "affect" in persuasion (e.g., Cacioppo & Petty, in press; Petty, Cacioppo, & Kasmer, 1987), but it does *not* make predictions about various persuasion-irrelevant features of affect. That is, the ELM does not address questions such as how many (and which) affective experiences are universal (e.g., Ekman, 1973) or whether the brain hemispheres differ in their processing of affect (e.g., Tucker, 1981). This should not be surprising since the ELM is a theory of attitude change, not a theory of emotion or brain functioning.

Similarly, the ELM makes explicit predictions about when simple cues rather than issue-relevant arguments have an impact on attitudes and it postulates different consequences for attitudes based on cues rather than arguments, but the ELM does *not* address questions such as whether the source, message, and other factors serving as cues and/or arguments are processed sequentially or in parallel, or in the right brain or left brain.<sup>2</sup> We think that it would be ludicrous to criticize Kahneman's (1973) elastic capacity model of information processing (the theory favored by Stiff) for failing to make explicit predictions about the resistance of attitude change to counterpropaganda, for example, since Kahneman did not propose a theory of persuasion. However, Stiff's attempt to apply Kahneman's model *can* be criticized for failing to make these and other persuasion-relevant predictions. Likewise, it is silly to criticize the ELM for failing to make predictions about hemispheric asymmetry, parallel processing, or the price of tea in China, since these have always been outside the domain of the theory.<sup>3</sup>

Second, despite the extended discussion of this in our initial reply, Stiff continues to misperceive the ELM as a theory postulating "central processing" of "central cues" and "peripheral processing" of "peripheral cues." Rather, the ELM outlines two general "routes" to persuasion. The *central route* is followed when people are motivated and able to engage in a careful scrutiny of the central merits of an attitude object. Under this route, attitudes are affected primarily by the quality or cogency of information (arguments) relevant to the perceived merits of the attitude object under consideration. The *peripheral route* is followed when motivation and/or ability to evaluate the central merits of an attitude object personally are low, and attitudes are affected primarily by simple cues in the persuasion environment. Importantly, the central and peripheral "routes" framework allows an integrative categorization of multiple processes of persuasion with similar antecedents and consequences (Petty & Cacioppo, 1981). That is, attitude changes under the peripheral route can result from a number of qualitatively distinct processes such as classical conditioning of affect (e.g., Staats & Staats, 1958) or the invocation of simple decision rules (e.g., Bem, 1967) or heuristics (e.g., Chaiken, 1980). These processes are postulated to occur when the "elaboration likelihood" is low, and result in attitudes that are relatively short lived, susceptible to counterpersuasion, and unpredictable of behavior. Likewise, several distinct attitudinal processes such as syllogistic reasoning (e.g., McGuire, 1981) and weighted averaging of issue-relevant arguments (e.g., Anderson, 1981) are possible under the central route umbrella. These processes are postulated to occur when the "elaboration likelihood" is high, and result in attitudes that are relatively long lasting, resistant to counterpersuasion, and predictive of

behavior. Other reviewers of the attitude change literature have not been confused by this issue (e.g., Aaker & Myers, 1987; Eagly & Chaiken, 1984), and it is not clear why Stiff continues to be.

Third, Stiff still does not appear to understand or appreciate the ELM notion that source, message, and other variables can serve in multiple roles. In our initial reply we provided an example of how "source attractiveness" could serve in different roles in different situations. Specifically, we argued and provided evidence for the view that source attractiveness could serve as a persuasive argument, a simple cue, or as a determinant of the extent of issue-relevant thinking in different situations. Contrary to Stiff's assertions, however, the "multiple role" feature of the ELM does not render it unable to make specific predictions because the ELM also specifies *when* variables serve in each of these roles, and it specifies testable consequences of a variable assuming these different roles. For example, if source attractiveness produced 3 units of attitude change by serving as a central argument for a beauty product under high elaboration conditions, this change should be more persistent, resistant, and predictive of behavior than an identical appearing 3 unit change induced under low elaboration conditions where source attractiveness served as a simple positive cue.

Importantly, in Figure 2 of our initial response to Stiff (see p. 246), we provided an example of the specific predictions made by the ELM for one variable (source credibility) serving in different roles across three levels of elaboration likelihood (Petty & Cacioppo, 1984b). We also graphed a subsequently published study (Moore et al., 1986) that provided support for these predictions. It is not that the ELM fails to make explicit predictions as Stiff charges, but rather, the predictions can be complex (see Figure 2). This needed complexity allows the ELM to account for more effects than previous models of persuasion. We view this as an important advantage of the ELM, not a flaw. Certainly, Stiff has failed to indicate how his preferred model of persuasion can account for (much less anticipate) the three-way interaction pattern depicted in Figure 2. Also, Stiff fails to indicate how his model of persuasion accounts for the numerous variables other than involvement that have been shown to have an impact on attitudes in the manner postulated by the ELM (e.g., personal responsibility, need for cognition, etc.). At present, the ELM explains the operation of many of the traditionally studied source, message, and context variables, and not only *predicts*, but provides a viable psychological account for complex interaction patterns such as the one depicted in Figure 2. The framework proposed by Stiff does not.

## METHODOLOGICAL ISSUES

*Evidence manipulations.* In our critique of Stiff's meta-analysis of message factors, we argued that the only unambiguous test of the view that argument processing increased with involvement came from studies that manipulated the *quality* of the arguments in a message. We argued and cited explicit evidence for the view that studies manipulating number of arguments or presence or absence of arguments could not be interpreted unambiguously because these manipulations were capable of affecting attitudes in two very different ways. That is, these manipulations could affect attitudes by serving as peripheral cues (i.e., inducing a simple inference—the more arguments the better) when the elaboration likelihood was low, but affect attitudes via diligent scrutiny of the merits of the arguments when

the elaboration likelihood was high (e.g., Petty & Cacioppo, 1984a; Wood, Kallgren, & Priesler, 1985). Stiff and Boster now present a reanalysis of their data in which studies are grouped by the specific message manipulations they employed. As we expected, studies manipulating argument quality showed the strongest effects—argument quality accounted for more variance in attitudes as involvement increased. Studies manipulating presence/absence of arguments and mere number of arguments showed a more mixed pattern. The presence/absence studies revealed no significant relationship across levels of involvement, whereas studies manipulating number of arguments showed an increasing effect size with increasing involvement. Importantly, this mixed pattern of results provides no evidence for the contention that “each of the three evidence manipulations is controlling the amount of central message information” (Stiff & Boster, 1987, p. 252). Our initial point, that message factors may serve in multiple roles, is not challenged by Stiff and Boster’s new analyses. As we noted in our initial reply, a manipulation of number of arguments may serve as a simple cue under low involvement and affect information processing under high involvement, but the arguments when processed may exert a greater effect on attitudes than when the mere number of arguments serves as a simple cue.

Furthermore, Stiff and Boster’s view that message factors cannot serve as peripheral cues is directly contradicted by the results of several empirical investigations noted in our earlier critique (i.e., Alba & Marmorstein, in press; Petty & Cacioppo, 1984a; Wood, Kallgren, & Priesler, 1985). How do they account for the results of these studies if they do not believe that message factors can serve as simple cues when the elaboration likelihood is low? In sum, Stiff and Boster ignore the evidence we presented which suggests that some message factors may serve as simple cues when the elaboration likelihood is low, and they offer no new evidence that is inconsistent with our initial critique.

*Involvement ratings.* Stiff and Boster argue that it is better to have 5 graduate students make ratings of involvement than 28 undergraduates. Although it is undoubtedly difficult for any judge to guess how involved another group of people was at another point in time, we believe our procedure is more justifiable than theirs on a number of grounds. First, 28 raters will produce a more reliable (less idiosyncratic) judgment than 5 raters. Second, our preference for *undergraduate* judges is based on the fact that undergraduates were the subjects in all of the studies comprising the meta-analysis. Thus, our judges would be at a similar level of psychological development as the subjects in the original studies and might better identify with the concerns of others in their own age group. Stiff and Boster argue that the graduate student judges are better because they “were familiar with both the topics and times at which the experiments were performed” (p. 253). Because the publication dates of the studies in question range from 1966 to 1983, a 30 year old judge in 1986 would have been only 10 when the earliest study was published, and 27 when the most recent study was reported. It is not clear how a 20 year old memory of one’s experiences at age 10 would lead to accurate prediction of how important various issues are for college students! In sum, we believe that there are grounds to prefer our rating procedure over that employed by Stiff.<sup>4</sup>

Importantly, by focusing on the people who served as the judges providing the involvement ratings, Stiff and Boster ignore a second reason why our classification of studies differed from theirs. That is, Stiff failed to have his judges rate studies that *manipulated* involvement, whereas we had judges rate *all* of the studies used in the analysis. Stiff’s procedure arbitrarily assumes that the designations “low” and

"high" involvement selected by the original investigators have a special meaning. It is more likely that these labels were chosen for convenience simply to designate two involvement levels, not to place these conditions on an overall continuum. The latter can be accomplished only by having a set of judges (whether graduate or undergraduate students) rate *all* of the studies along the same scales. Finally, it is important to note that by arguing over the involvement ratings *per se*, Stiff and Boster missed a major point of our critique—that the original meta-analysis of source effects was based on an "unacceptably small number of data points" (Petty et al., 1987, p. 241).

*Source credibility effects.* In our critique of Stiff's initial meta-analysis of source credibility, we argued that the analysis was based on an insufficient sample, and no statistical evidence was presented for the hypothesized curvilinear effect. Using a procedure recommended by Rosenthal (1984), we found that there was no significant curvilinear pattern to the effect sizes across levels of involvement. Stiff and Boster raise two objections to our analysis. First, they argue that our curvilinear contrast weights did not sum to zero. Stiff and Boster fail to consider that in the Rosenthal procedure each effect size is assigned its own weight in the contrast. Since there are five low involvement means ( $M_l$ ), three moderate involvement means ( $M_m$ ), and two high involvement means ( $M_h$ ), the curvilinear contrast becomes:  $(-6M_l - 6M_l - 6M_l - 6M_l - 6M_l) + (20M_m + 20M_m + 20M_m) + (-15M_h - 15M_h)$ . These contrast weights, of course, sum to zero.

Second, in response to our argument that no significant differences in effect size for credibility across involvement levels was reported in the initial paper, Stiff and Boster now report two new analyses. In the first, they perform a one-way ANOVA using involvement as the independent variable and effect size as the dependent variable. This analysis obtains a significant result. However, this ANOVA is based on an average of about 3 observations per cell. This small  $N$  analysis seriously violates the assumptions of the analysis of variance procedure and is therefore suspect. The second procedure employed by Stiff and Boster is to test the difference between correlations. Their analysis finds a significant difference between moderate and high and between moderate and low involvement conditions. Our analysis revealed only the former difference to be significant. One reason for this discrepancy appears to be that Stiff and Boster first averaged the correlations in each group and then tested for significance. In our analysis, separate contrast weights were applied to each study involved in the comparison. The latter meta-analytic procedure is recommended by Rosenthal (1984) because it takes into account the actual size of the sample employed in each of the studies involved in the comparison. The procedure used by Stiff and Boster does not.

Nevertheless, our major point was that it was unreasonable to conduct and draw conclusions from a meta-analysis that employed in Stiff's words, "a minute portion of the total credibility literature." (p. 85). Stiff and Boster do not address this criticism except to say that because the results were based on 1345 participants, the estimates were "affected very little by sampling error." (p. 255). This was not our concern, of course. Rather, we worried that when effect size estimates are based "on only two or three data points . . . the effects observed may be too dependent on factors *other than the variable of interest*" (p. 244). We pointed to factors such as the subjects' extent of knowledge on the topics and the magnitude of the credibility manipulations employed in the various studies that would contribute to the effect sizes observed *in addition to* subjects' level of involvement. Stiff and Boster make no

comment on these points. Our initial caution that it was premature to draw conclusions based on Stiff's small sample meta-analysis of source credibility remains in force.

## CONCLUSIONS

In their response to our critique of the Stiff paper, Stiff and Boster continue to misrepresent the ELM and attack straw-man positions. They ignore the specific ELM predictions described and depicted in our critique of the Stiff paper, and they fail to indicate how their preferred model of persuasion accounts for the various complexities observed in the existing attitude change literature that can be explained by the ELM. Furthermore, they continue to assert conclusions drawn from inadequate meta-analyses.

## NOTES

<sup>1</sup>In summarizing the results of the high involvement conditions of this study in Petty & Cacioppo (1986b), we wrote that "only argument quality affected attitudes in the high relevance conditions" (p. 156). That is, argument quality but not source likeability affected attitudes under high relevance. This sentence, of course, does not mean or imply that argument quality affected attitudes only under high involvement. Space limitations do not allow us to respond to all of the misinformation and mistaken inferences in the Stiff and Boster reply.

<sup>2</sup>The ELM, as a theory of persuasion, is of course not unique in failing to address the issues that are of particular interest to Stiff. Dissonance theory (Festinger, 1957), for example, takes no position on whether dissonant information is processed sequentially or in parallel, in the right brain or left brain, etc., but remains an important theory of attitude change because of its great explanatory power within its domain (Fazio & Cooper, 1984). Stiff is apparently confused by the ELM postulate that when the elaboration likelihood is high, arguments have a greater impact on attitudes than when the elaboration likelihood is low; however, the *reverse* holds for simple cues. Importantly, these relationships are postulated to hold regardless of whether the specific source, message, or other items of information serving as arguments or cues are processed sequentially or in parallel! Further details about setting up the conditions necessary to provide explicit tests of the ELM may be found in Petty & Cacioppo (1986a, Chapter 2).

<sup>3</sup>Although the ELM provides a theory of persuasion and not a general theory of cognitive functioning, elsewhere we have discussed the links between the ELM and some relevant theories of information processing from cognitive psychology. Interested readers should consult Petty & Cacioppo (1986a).

<sup>4</sup>Stiff and Boster also object to the order in which our involvement ratings were taken, but no evidence is provided to support their arguments.

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