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Effects of Involvement on Persuasion: A Meta-Analysis

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Defines involvement as a motivational state induced by an association between an activated attitude and the self-concept. Integration of the available research suggests that the effects of involvement on attitude change depended on the aspect of message recipients' self-concept that was activated to create involvement: (a) their enduring values (*value-relevant involvement*), (b) their ability to attain desirable outcomes (*outcome-relevant involvement*), or (c) the impression they make on others (*impression-relevant involvement*). Findings showed that (a) with value-relevant involvement, high-involvement subjects were less persuaded than low-involvement subjects; (b) with outcome-relevant involvement, high-involvement subjects were more persuaded than low-involvement subjects by strong arguments and (somewhat inconsistently) less persuaded by weak arguments; and (c) with impression-relevant involvement, high-involvement subjects were slightly less persuaded than low-involvement subjects.

To understand the conditions under which people are persuaded by others, researchers have often invoked the concept of involvement. Although this construct was popular prior to M. Sherif and Cantril's (1947) work (see A. G. Greenwald's, 1982, review), their proposal that highly involving attitudes be regarded as components of the self-concept or ego was seminal to theory about involvement's impact on attitude change. According to M. Sherif and Cantril (1947), such attitudes "have the characteristic of belonging to *me*, as being part of *me*, as psychologically experienced" (p. 93).

M. Sherif, C. W. Sherif, and their colleagues developed the implications of involvement (which they often called "ego involvement") for persuasion by giving it a major role in their social judgment-involvement approach, a theory of attitude change developed in the 1950s and early 1960s (Hovland, Harvey, & Sherif, 1957; C. W. Sherif, Sherif, & Nebergall, 1965; M. Sherif & Hovland, 1961). During this same period, Zimbardo (1960) introduced the concept of response involvement in order to predict attitude change in a social influence setting. In more

recent years, researchers concerned with the cognitive processes underlying attitude change have invoked involvement as a motivational variable that is presumed to affect persuasion because it instigates more thorough processing of persuasive messages (Chaiken, 1980; Petty & Cacioppo, 1979b, 1981a). In this article, we contend that across these three traditions of research, the operational definitions of involvement have differed sufficiently to require that three types of involvement be distinguished at a conceptual level. As we demonstrate via a meta-analytic review of the relevant studies, these three types of involvement have distinctively different effects on persuasion.

Value-Relevant Involvement

From an early point (e.g., M. Sherif & Cantril, 1947), social judgment-involvement theorists regarded highly involving attitudes as components of the ego or self-concept, that is, as aspects of the "self-picture—intimately felt and cherished" (C. W. Sherif et al., 1965, p. vi). Among the various attitude theorists who followed the Sherifs and their associates by defining involvement in terms of the embeddedness of highly involving attitudes in the self-structure, Ostrom and Brock (1968) provided an especially clear statement when they proposed that

the basic feature of an ego-involved attitude is its relation to the manner in which the individual defines himself. The individual defines himself primarily in terms of that "distinct constellation of social and personal values" he has acquired. The closer the relation between his attitude and these values and the more central these related values are, the higher the degree of attitudinal involvement. (p. 375)

Following Ostrom and Brock, we propose the term *value-relevant involvement* to refer to the psychological state that is created by the activation of attitudes that are linked to important values. Values are presumed to be aspects of the self that are especially important and enduring, consistent with Rokeach's (1968) definition of value as people's evaluations of general "modes of conduct and end-states of existence" (p. 159).

To develop specific predictions concerning the effects of

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value-relevant involvement (along with other variables) on attitude change, social judgment-involvement theorists proposed that an attitude provides an internal frame of reference for judging and reacting to stimuli related to the attitude (C. W. Sherif et al., 1965; M. Sherif & Hovland, 1961; M. Sherif & Sherif, 1967). In this tradition, the attitudinal continuum is divided into three ranges or *latitudes*: (a) the latitude of acceptance, containing a person's own stand and the other positions that he or she finds acceptable; (b) the latitude of rejection, containing the positions that are objectionable; and (c) the latitude of noncommitment, containing the positions that are neither acceptable nor unacceptable. The widths and locations of these latitudes then determine the persuasiveness of messages. Thus, for messages advocating positions located within message recipients' latitudes of acceptance, successful persuasion is likely to occur. For messages advocating positions located beyond the latitude of acceptance, persuasion becomes increasingly less likely the more discrepant these messages are from recipients' own stand, with very little persuasion produced by messages advocating positions located in the latitude of rejection.

Social judgment-involvement theorists assumed that value-relevant involvement affects persuasion via its influence on the widths of the latitudes. High involvement was represented on the attitudinal continuum by a relatively wide latitude of rejection and little or no latitude of noncommitment. In contrast, low involvement was assumed to produce a narrower latitude of rejection and a broader latitude of noncommitment (C. W. Sherif et al., 1965). The theory thus suggested that, to the extent that recipients are highly involved in the issue discussed in a counterattitudinal message, the position the message advocates is likely to fall in their latitude of rejection because this latitude covers a relatively great range of the attitudinal continuum. The prediction that less persuasion is produced by involving than by noninvolving messages is consistent with this reasoning about the latitude of rejection.

Researchers working in this theoretical tradition gave involvement a variety of operational definitions. In some studies (e.g., M. Sherif & Hovland, 1961), involved subjects were members of groups actively supporting a particular stand on an issue, and less-involved subjects were not members of such groups. Because group members' attitudinal positions usually differed from those of nonmembers, researchers sought operational definitions of involvement that were not so vulnerable to this confound. Identifying high- and low-involvement subjects by the relative widths of their latitudes then became popular (e.g., Letchworth, 1969; Sereno, 1968). Other operational definitions of value-relevant involvement also saw some use, in particular (a) the classification of subjects by their self-reports of the importance or level of involvement of issues (e.g., Powell, 1977) and (b) the presentation of messages on issues known to differ in level of involvement (e.g., C. W. Sherif, Kelly, Rodgers, Sarup, & Tittler, 1973).¹

Despite this multiplicity of operational definitions, highly involving attitudes were consistently viewed as more difficult to change than less-involving attitudes. Nonetheless, for experiments that crossed involvement with other variables (e.g., the size of the discrepancy between subjects' own position and the position advocated in the message), investigators produced more detailed predictions (e.g., that differences in the persua-

siveness of more- and less-involving messages would increase as message discrepancy increased; see Gorn, 1975; Rhine & Severance, 1970). Yet such predictions did not ordinarily include reversals of the tendency for value-relevant involvement to decrease attitude change but instead merely delineated conditions in which this tendency would be especially strong.

Impression-Relevant Involvement

The opposite prediction about involvement's effects was generated by Zimbardo (1960) within a cognitive dissonance framework. Following Festinger's (1957) claim that the magnitude of the dissonance created by the juxtaposition of inconsistent cognitive elements increases with the importance of the elements, Zimbardo argued that involvement should facilitate attitude change, provided that other methods of reducing dissonance are unavailable. However, as researchers refined dissonance theory by adding a number of conditions that must be present for the theory to predict attitude change, the link between Zimbardo's prediction and dissonance theory was severed because these special conditions were absent in Zimbardo's experiment. For example, Brehm and Cohen (1962) proposed that commitment was one of these conditions: People must commit themselves to a discrepant attitudinal position in order for dissonance to be created. In subsequent years, a variety of other conditions were also proposed (e.g., production of unwanted consequences from committing oneself to the attitudinal position; see Cooper & Fazio, 1984). Although the effects of involvement within experimental paradigms that genuinely produce dissonance (e.g., counterattitudinal advocacy) are of interest, in this article we confine our attention to studies in which subjects responded to a communicator's persuasive message and are not presumed to have experienced cognitive dissonance.

Despite the fact that Zimbardo's (1960) experiment became uninterpretable in terms of dissonance theory, the study is well known because other investigators designed involvement manipulations similar to Zimbardo's and often adopted Zimbardo's label of *response involvement* for this independent variable. These manipulations stressed the self-presentational consequences of the attitude that subjects anticipated they would

¹ Although manipulations of commitment (see Kiesler, 1971) have sometimes been regarded as similar to value-relevant involvement (e.g., by Leippe & Elkin, 1987), close inspection of those relatively few commitment studies that presented subjects with counterattitudinal persuasive messages suggested that these manipulations were dissimilar to those we have regarded as instances of value-relevant involvement. These commitment manipulations, which were administered prior to the persuasive message, generally made subjects' premessage positions public, for example, by promising that a statement of their position would be published in the campus newspaper (e.g., Pallak, Mueller, Dollar, & Pallak, 1972, Experiment 2) or by obtaining subjects' signatures on a proattitudinal petition (e.g., Kiesler, 1971, pp. 74-85). Another manipulation led subjects to expect future interaction with the source of the persuasive message but did not lead them to believe that they would discuss the issue considered in this message (Pallak et al., 1972, Experiment 1). Thus, it is not at all clear that the commitment manipulations used in persuasion studies influenced the extent to which the persuasive message activated subjects' values.

express after they received a communicator's viewpoint. Thus, Zimbardo's experimenter told subjects that their attitudinal position would reveal a great deal (high response involvement) or nothing (low response involvement) about themselves. Subjects made their position known after being exposed to a friend's position, which consisted of only a mark on an attitude scale. Although Zimbardo's experiment followed the tradition of conformity studies by presenting only the communicator's position on an issue, involvement manipulations modeled after Zimbardo's were subsequently used in persuasion studies, which presented subjects with complex messages consisting of an advocated position and supportive argumentation. For example, in studies by Chaiken (1980) and Leippe and Elkin (1987), high-involvement message recipients were informed that they would later be interviewed on and discuss the issue considered in the message.

Zimbardo's (1960) conceptual definition of response involvement as "the individual's concern with the consequences of his *response* or with the instrumental meaning of his opinion" (p. 87) was much broader than his operational definition, which mainly emphasized one particular consequence: the impression one makes on others. Because the Zimbardo manipulation and other manipulations that have been called response involvement probably make salient to subjects the self-presentational consequences of their postmessage positions, we suggest that Zimbardo's response involvement label is something of a misnomer. We propose instead that the more informative term *impression-relevant involvement* be used to refer to manipulations of this particular class.²

In agreement with Leippe and Elkin (1987), we assert that manipulations of this type establish a concern with holding an opinion that is socially acceptable to potential evaluators. As has been suggested by research on the effects of anticipated audiences on opinions (e.g., Cialdini, Levy, Herman, & Evenbeck, 1973; Cialdini, Levy, Herman, Kozlowski, & Petty, 1976; Cialdini & Petty, 1981), message recipients who anticipate public scrutiny of their views tend to advocate a flexible, moderate position on an issue, when the anticipated audience is not known to prefer a polarized position on the issue and the issue does not arouse other types of involvement. As Leippe and Elkin (1987) reasoned, such recipients may be attentive to the details of a persuasive message in order to become knowledgeable enough to win others' approval. However, they may hesitate to be greatly influenced, even by strong, cogent arguments, or to fully reject appeals based on weak, specious arguments, because of the self-presentational advantages of maintaining a flexible and nonpolarized position.

Outcome-Relevant Involvement

In recent years, cognitively oriented persuasion researchers have argued that involvement increases message recipients' motivation to process information about the issue discussed in a message. Petty and Cacioppo (1979a, 1979b) first provided this interpretation and suggested the term *issue involvement* for this type of involvement. They argued that issue involvement concerned "the extent to which the attitudinal *issue* under consideration is of personal importance" (Petty & Cacioppo, 1979b, p. 1915). Moreover, they regarded this type of involvement as

the same construct that had been examined by social judgment-involvement researchers (e.g., C. W. Sherif et al., 1965), although they distinguished it from Zimbardo's (1960) response involvement. We contend that, on the contrary, the operational definitions of involvement used by Petty and Cacioppo (e.g., 1979b, 1984) and investigators who have followed their example (e.g., Burnkrant & Howard, 1984) are sufficiently different from those used by social judgment-involvement researchers to justify a different involvement construct, which we label *outcome-relevant involvement*. We further suggest that, like the term *response involvement*, the term *issue involvement* is something of a misnomer because it implies a considerably broader set of operations than have in fact been used by investigators who invoke the term.³

Petty and Cacioppo (1979b) manipulated involvement in their first experiment by having the communicator of the high-involvement message advocate a policy change (that coeducational visitation hours in university dormitories be changed) for the college-student subjects' own university and by having the communicator of the low-involvement message advocate the change for another, relatively unknown college. In a second experiment, Petty and Cacioppo (1979b; see also Petty & Cacioppo, 1979a) had their high-involvement communicator advocate a different policy change (that undergraduate comprehensive examinations be instituted) for students at the subjects' own university versus a distant, relatively unknown university. Although this method of manipulating involvement was modeled on one used much earlier by Apsler and Sears (1968), the recent popularity of the manipulation appears to have stemmed from its repeated use by Petty, Cacioppo, and their colleagues (Petty & Cacioppo, 1979a, 1979b, 1981b, 1984; Petty, Cacioppo, & Goldman, 1981; Petty, Cacioppo, & Heesacker, 1981). In these studies, involvement was manipulated by having a recommended change take effect at the subjects' own university versus a distant university and by having the recommended change take effect soon (next year) versus in the distant future (in 10 years). Other investigators have followed this model quite closely (e.g., Axsom, Yates, & Chaiken, 1987; Burnkrant & Howard, 1984; Leippe & Elkin, 1987; Schul & Knapp, 1984; Sorrentino, Bobocel, Gitta, Olson, & Hewitt, 1988).

We suggest that the term *outcome-relevant involvement* be applied to these manipulations because they make salient to message recipients the relevance of an issue to their currently important goals or outcomes. For example, visitation hours im-

² Making subjects believe that their premessage position has important short-term consequences (e.g., Freedman, 1964) has occasionally been interpreted as response involvement (e.g., by Petty & Cacioppo, 1986a, p. 89). Such a manipulation does not follow the model of Zimbardo's (1960) experiment and in fact has more in common with manipulations of commitment (see Footnote 1) than with those of involvement. An additional reason that the Freedman (1964) study was not included in the meta-analysis was that its persuasive message consisted of a mere statement of a position on an issue, unaccompanied by argumentation.

³ The term *personal relevance*, which has recently been substituted for *issue involvement* by some researchers working in the cognitive response/elaboration likelihood tradition (e.g., Petty & Cacioppo, 1986a), entails the same difficulties of excessive breadth in relation to the operations used to define it.

pinge on college students' social lives and on their ability to balance academic and social pursuits, and comprehensive examinations impinge on students' ability to obtain a degree and on the quality of the education they receive. Although proposed changes on such issues would affect outcomes that are very important to students (provided, of course, that these changes would take effect relatively soon at the students' own university), the issues themselves are likely to be relatively unfamiliar to students and are unlikely to be closely linked to important values, in the manner that major social issues (e.g., abortion, arms control, pollution control) are linked to values. Instead, when the manipulation links the issues to anticipated outcomes, these issues raise strategic considerations in relation to message recipients' ability to achieve these outcomes. After the contention of James (1890) and Allport (1943) that "fighter for ends" is one facet of the self, one can view the goal-oriented responding elicited by this type of manipulation as stemming from the linkage of subjects' attitudinal position to the purposive aspects of the self.

Given these characteristics of manipulations of outcome-relevant involvement, it is not surprising that investigators argued that involvement increases message recipients' motivation to engage in message-relevant thinking (Chaiken, 1980; Petty & Cacioppo, 1979b). Motivation to process is crucial from the standpoint of the *cognitive response approach* to understanding persuasion (e.g., A. G. Greenwald, 1968; Petty, Ostrom, & Brock, 1981), the framework that provides the rationale for many of the predictions about the effects of outcome-relevant involvement. This approach (as well as the subsequent elaboration likelihood model; Petty & Cacioppo, 1986a, 1986b) regards persuasion as mediated by the quantity and valence of message recipients' thoughts (i.e., cognitive responses) relevant to the issue or message. Thus, for messages that elicit unfavorable thinking, increased message-relevant thinking should decrease persuasion, whereas for messages that elicit favorable thinking, this increased processing should increase persuasion. If involvement motivates people to engage in more message-relevant thinking, it should decrease persuasion for messages that elicit predominantly unfavorable thoughts and increase persuasion for messages that elicit predominantly favorable thoughts.

Given this rationale provided by cognitive response theory, predictions can be made about the effects of outcome-relevant involvement only if the valence of message recipients' thoughts is known. Earlier research by Petty, Wells, and Brock (1976) established that messages containing weak, specious arguments elicit primarily unfavorable thoughts and that messages containing strong, compelling arguments elicit predominantly favorable thoughts. Crossing level of outcome-relevant involvement and argument strength in a factorial design, Petty and Cacioppo (1979b, Experiment 2) then showed that when a counterattitudinal message contained weak, specious arguments, involvement enhanced the production of unfavorable thoughts and inhibited persuasion and when the message contained strong, compelling arguments, involvement enhanced the production of favorable thoughts and facilitated persuasion.

Because Petty and Cacioppo (1979b) linked their findings to those of the social judgment-involvement experiments, these results could be taken to imply that weakness of argumentation

explained why involvement had inhibited attitude change in this earlier work. Yet it seems unlikely that researchers would commonly have written persuasive communications to include predominantly weak, specious arguments. We think that the difference in findings instead stems from a major difference in the way issues were made involving in these two traditions of research: In social judgment-involvement experiments, subjects' involvement with the issue stemmed from the link between the issue and ingrained values, whereas in cognitive response experiments, the importance of the issue stemmed from its link to outcomes that subjects hoped to attain relatively soon. Although Petty and Cacioppo (1986a) stated that the involvement effects they predict might not occur "where personal interests are so intense, as when an issue is intimately associated with central values" (p. 87), we suggest that value-relevant involvement is not reducible to an extremely high level of outcome-relevant involvement but is instead a qualitatively different type of involvement that has persuasive effects distinct from those of outcome-relevant involvement.

Three Forms of Self-Relevance

That involvement has been studied in such different ways in the social influence settings of persuasion research should not be surprising in view of the varied uses of the self in social psychology (see A. G. Greenwald & Pratkanis, 1984). Although M. Sherif and Cantril's (1947) formulation of involvement was aptly focused on the self-concept, their discussion did not anticipate that the involvement construct would be used in such disparate ways in subsequent research on attitude change. Yet, consistent with their discussion, a general definition of involvement encompassing these varied uses of the term appropriately focuses on the self. We thus propose that involvement is the motivational state induced by an association between an activated attitude and some aspect of the self-concept. For value-relevant involvement, the pertinent aspect of the self is one's enduring values: The persuasive message activates an attitude that was linked to one's values prior to the experiment or that became linked during the experiment. For impression-relevant involvement, the pertinent aspect of the self is the public self or the impression one makes on others: The issue on which one expects to express an attitude after receiving a persuasive message is linked to the public self by the anticipation that this attitude will be known to an evaluative audience. For outcome-relevant involvement, the pertinent aspect of the self is one's ability to attain desirable outcomes: The information that the persuasive message provides and the attitude one forms on the basis of this information are made to appear relevant to the attainment of these outcomes. In its broadest interpretation, outcome-relevant involvement could be viewed as encompassing the other two types because both maintaining one's values and making a favorable impression on others are desirable outcomes. Nonetheless, because of the three distinct traditions of experimentation on involvement's effects on persuasion, we prefer to view outcomes more narrowly: An outcome is an explicit personal goal that one expects to obtain relatively soon mainly by one's own efforts and that directs aspects of one's behavior.

The communality of the three types of involvement is their activation of the self-concept. Their considerable differences lie

in the particular aspect of the self that is aroused. These differences are so important for predicting the persuasiveness of communications that it is perhaps unfortunate that the term *involvement* has been used for all three types. Nonetheless, to maintain continuity with traditional terminology, we favor continued use of the term *involvement*, but with the appropriate descriptor—value relevant, impression relevant, or outcome relevant—added when specific findings are discussed.

Design of Meta-Analysis

Setting boundaries for research on involvement and persuasion. To examine the effects of the three types of involvement, we endeavored to locate all studies that had manipulated or assessed message recipients' involvement and related this independent variable to the persuasion induced by a communication. The boundaries of this research literature are not clear-cut because some operational definitions of involvement were seriously confounded with other independent variables, other operationalizations that seemed unambiguous instances of one of our involvement types had not been labeled involvement by the author or authors of the study, and a few that were labeled involvement seemed unrelated to any of our three types.

We decided to exclude studies with obviously confounded manipulations.⁴ This decision meant that the early work of the Sherifs and their colleagues was not included (e.g., Hovland et al., 1957; C. W. Sherif et al., 1965; M. Sherif & Hovland, 1961) because these investigators compared groups differing in their initial stands on issues and then merely discussed the resulting findings in terms of involvement differences that they suggested were correlated with these stands. In addition, we decided to include studies with operational definitions that were suitable exemplars of involvement in all respects other than the fact that this variable was not labeled involvement (e.g., personal relevance in Sorrentino et al., 1988). Finally, because manipulations of involvement that did not fit into any of our three types seemed not to activate the self-concept, we excluded these studies as inconsistent with the usual understanding of involvement in social psychology. For example, in a condition labeled *high involvement*, Tsal (1985) instructed subjects to form an attitude toward the brand depicted in an advertising message (and he omitted this attitude-formation instruction in a condition labeled *low involvement*).⁵

The meta-analysis is also limited to studies in which message recipients were exposed to communications consisting of a position advocated by a communicator and one or more arguments designed to support the position. Studies were excluded if the message consisted of a mere statement of a communicator's position, without any support or argumentation, as is typical in conformity studies. One reason that our domain was limited in this way is that the theories underlying the recent interest in involvement research (e.g., Petty & Cacioppo, 1986a, 1986b) have been tailored to account for the persuasion that occurs when people are exposed to relatively complex messages. In addition, this limitation had the advantage of confining the meta-analysis to studies that are somewhat homogeneous methodologically and that therefore can be more readily compared.

Partitioning studies on message strength. In involvement studies, as in other research in experimental social psychology,

the variable of interest (involvement) has often been crossed with other variables (e.g., communicator credibility) in factorial designs. The meta-analyst can represent the effect of such a variable aggregated over these other variables (i.e., as a main effect). Alternatively, the meta-analyst can partition each study on each of these other variables and represent the effect of interest within levels of the other variables (i.e., as a simple main effect). The relative merits of these strategies depends on two considerations: (a) Have any of these other variables (e.g., communicator credibility) been crossed with the focal variable (i.e., involvement) frequently enough so that a fairly large subset of the studies can be similarly partitioned, and (b) are the other variables (e.g., communicator credibility) associated with reversals of the effects of the focal variable (i.e., involvement increases persuasion at one level but decreases it at another level)?

In the sample of studies, we found only one variable that was very commonly crossed with involvement in factorial designs: the strength of the persuasive message. This variable was typically manipulated by supporting the position advocated in the message with arguments preselected to be either quite weak or quite strong (e.g., Petty & Cacioppo, 1979b). Less commonly, the strength of the message was manipulated by presenting subjects with a smaller or larger number of arguments (e.g., Chaiken, 1980).⁶ As was explained earlier, the impact of out-

⁴ By *obviously confounded manipulations*, we mean confounds that were unambiguously established by the authors' own report of their data (e.g., the Hovland et al., 1957, confounding of involvement and subjects' initial attitudinal positions). In addition, we considered as obviously confounded those involvement manipulations that consisted of two or more separate procedures, one of which is ordinarily considered to manipulate another construct. For example, Gardner, Mitchell, and Russo (1978) told high-involvement subjects to examine advertisements "as though they were planning a purchase of the product class of the brand in the advertisement" and told low-involvement subjects to evaluate the advertisements on "the amount of otomotopia[sic], assonance, alliteration, rhyme, hyperbole in the copy and the number of times the words 'you' and 'your' appeared" (p. 585). The latter aspect of this involvement manipulation would ordinarily be considered a manipulation of distraction, a different construct. Confounds certainly may have occurred under other circumstances, especially when involvement was varied by classifying subjects according to their own responses (e.g., Powell, 1977) or was manipulated by presenting high- and low-involvement subjects with messages on different issues (e.g., Rhine & Severance, 1970). However, lacking proof from data or from separate operations, we cannot be certain that involvement was confounded with other variables in such designs, and these studies were retained in our sample.

⁵ Tsal's (1985) manipulation of involvement reflects the typical conceptualization of the variable in the consumer-behavior literature, in which information processing has been emphasized instead of an association between the attitude and the self, which has been central in social psychology. Integrating typical definitions of involvement by consumer psychologists, A. G. Greenwald and Leavitt (1984) defined the concept as "the allocation of attentional capacity to a message, as needed to analyze the message at one of a series of increasingly abstract levels" (p. 591). Consistent with this definition, consumer psychologists have manipulated involvement via a diverse set of treatments designed to influence information processing.

⁶ A larger number of arguments would increase message strength only if the quality of these arguments was relatively high. A larger number of low-quality arguments would decrease message strength. The effects of

come-relevant involvement on persuasion should be positive for strong messages and negative for weak messages. Thus, because of the popularity of crossing involvement with message strength and because of the reversals associated with this manipulation, studies were (whenever possible) partitioned on message strength (i.e., the quality or number of arguments), and involvement's effect was examined separately within weak and strong messages (as well as aggregated over message strength).

Aside from message strength, studies were not partitioned with respect to other variables (e.g., communicator credibility, message discrepancy, audience enthusiasm). Admittedly, these other variables have some importance in the theories of persuasion that spawned the research we review. For example, communicator variables can serve as peripheral cues in the elaboration likelihood model (Petty & Cacioppo, 1986a) and as a determinant of latitude width in the social judgment-involvement framework (C. W. Sherif et al., 1965). Nonetheless, each such variable was manipulated in such a small number of studies that there would be little or no gain from quantifying the effects of these variables. Moreover, these variables were not typically presumed to create reversals of involvement's effects. However, a few of these other variables established, for one level of the variable, a situation so atypical of persuasion research that the atypical condition was deleted from the meta-analysis. For example, Petty and Cacioppo (1979b, Experiment 1) presented half of the subjects with a proattitudinal communication (i.e., one that matched their premessage attitudes) and the other half with a counterattitudinal communication. Because the messages used in persuasion studies tend to be counterattitudinal (so that change toward the message can be assessed), the proattitudinal condition of this study was removed. Similarly, other studies established, for half of their subjects, an atypical set for receiving the message or responding to the attitudinal measure. For example, Apsler and Sears (1968) warned half of their subjects of the position the communicator was going to take, and Schul and Knapp (1984) presented the attitude measure to half of their subjects in a bogus pipeline format (see Jones & Sigall, 1971). Conditions establishing such unusual sets were also deleted.

Method

Sample of Studies

Computer-based information searches were conducted using the keyword *involvement* on the following data bases: *Psychological Abstracts* (PsycINFO: 1967 to July 1987); a compilation of newly published psychological research (PSYCALERT: July 1987); *Dissertation Abstracts International* (DAI: 1861 to July 1987); Educational Resources Information Center (ERIC: 1966 to December 1985); and a worldwide business and management data base (ABI/INFORM: 1971 to December 1985). The Social SciSearch data base was also searched to locate articles that cited Petty and Cacioppo (1979b) as of December 1985. We also searched through (a) the reference lists of numerous review articles, books, and chapters of books; (b) the reference lists of all located studies;

and (c) volumes of the journals with the largest number of involvement studies.

Criteria for including studies in the sample were that (a) subjects were adults or adolescents not sampled from abnormal populations; (b) subjects received a persuasive message; (c) subjects indicated their acceptance of the position advocated in the message; and (d) involvement (or a variable such as personal relevance that we deemed identical to involvement) was used in the analyses of persuasive effects. Studies were eliminated if involvement was operationalized in a manner that did not clearly vary the relevance of the issue considered in the message to subjects' self-concepts (e.g., Isaacson, 1974; Tsal, 1985). In addition, studies were eliminated if (a) the message subjects received consisted of a mere statement of a position on an issue, unaccompanied by argumentation (e.g., Eagly, 1967; Freedman, 1964; H. J. Greenwald, 1964; Zimbardo, 1960); (b) involvement was varied by classifying subjects on responses assessed after they received the persuasive message (e.g., Boyd, 1978; McGinnies, 1968, 1973); (c) involvement was varied in a manner that obviously confounded involvement with another variable (e.g., attitudinal position in Hovland et al., 1957, and C. W. Sherif et al., 1965; distraction in Gardner, Mitchell, & Russo, 1978, 1985); (d) in an after-only design in which involvement was manipulated by varying consumer products, a difference in attitudes toward the products probably existed prior to the experimental session and compromised interpretation of subjects' attitudes in terms of persuasion (e.g., Bowen & Chaffee, 1974; Chebat & Picard, 1985); (e) involvement was manipulated after subjects received the persuasive message (Pentony, 1986, 1987); (f) a check on the involvement independent variable failed to reach a marginal level of significance, $p < .10$ (e.g., Scileppi, 1973; Sorrentino et al., 1988, Study 1);⁷ and (g) the document reporting the study did not provide information sufficient for the computation of effect sizes (Huddleston, 1986; Schumann, Petty, & Cacioppo, 1984). Also excluded were studies or conditions within studies in which subjects received proattitudinal messages (e.g., Petty & Cacioppo, 1979b, Study 1; Stoltenberg, 1982) or in which an unusual set was established for receiving the persuasive message (e.g., the warning conditions of Petty & Cacioppo, 1979a; the rhetorical questions conditions of Petty, Cacioppo, & Heesacker, 1981) or for responding to the attitudinal measure (e.g., the bogus pipeline conditions of Schul & Knapp, 1984). Shechter's (1987/1988) conditions that presented partners holding favorable or unfavorable attitudes were removed because these were atypical of impression-relevant manipulations. Finally, Neises's (1988) conditions of extreme issue involvement and temporary response involvement were removed because these were atypical of outcome-relevant and impression-relevant manipulations, respectively.

Variables Coded From Each Study

The following information was coded from each report: (a) date of publication; (b) publication form (journal article, other published document, dissertation or master's thesis); (c) message length, in words (estimated in some instances); (d) amount of prior knowledge that subjects possessed about the issue discussed in the message (little or none: e.g., comprehensive exams for college seniors, novel brands of products; moderate: e.g., chest X-rays, university tuition increases; considerable: e.g., abortion, the Vietnam War; knowledge covaried with involvement: i.e., high- and low-involvement conditions used different issues, which differed in amount of prior knowledge); (e) message modality (print,

the number of arguments could also depend on how this cue is processed (see Petty & Cacioppo, 1984).

⁷ Included as manipulation checks were self-report measures of involvement (e.g., subjects' self-reports of involvement, importance, or concern) but not reports of subjects' memory for the details of the manipulation (e.g., Petty & Cacioppo, 1984, main experiment). Studies without manipulation checks were retained because there was no evidence that the variation of involvement was unsuccessful.

audio, video); (f) number of issues used per level of involvement; (g) message domain (college issues, social issues, consumer products, more than one domain); (h) type of involvement (value relevant, outcome relevant, impression relevant);⁸ (i) method of involvement variation (description of differing consequences for subjects of adopting the position advocated in message: e.g., the advocated policy would take effect next year versus 10 years hence; classification of subjects based on their judgments of an issue: e.g., latitudes of acceptance, rejection, or both; presentation of issues differing in level of involvement; e.g., appropriate male- and female-related career choices versus the postwar status of Paul von Hindenburg; other methods: e.g., making salient the relation between subjects' attitude and central or peripheral values); (j) status of involvement in the experimental design (between subjects, within subjects); (k) presence of involvement manipulation check (present, absent); (l) outcome of manipulation check (significant, mixed or marginal, unknown or check absent); (m) name given to involvement variation by the study author or authors (involvement, other name); (n) type of subject population (high school, college undergraduate); (o) quality of persuasion measure (single item; multi-item, unknown reliability; multi-item, high reliability, defined as $\alpha \geq .70$); and (p) metric for persuasion measure (posttest, including change scores based on single control group mean; covariance-adjusted posttest or change score based on differences from subjects' own pretest; change score based on differences from high- and low-involvement control groups). These variables were coded by the authors, with a median agreement of 100%.⁹ Disagreements were resolved by discussion or, in the case of message length, by averaging estimates, which were highly correlated, $r = .97$.

Argument Strength

The strength of the arguments supporting the position advocated in each available persuasive message was estimated using 182 undergraduate respondents who judged these arguments. Each argument was summarized in one or two sentences and presented to the respondents in questionnaire form. If more than three arguments were available for a message, the first and last were presented, and one was taken at random from the middle of the message. If three or fewer were available, all were used. The questionnaire instructed the respondents to read each argument carefully and to decide how strong an argument it made for the recommended position, which was displayed immediately below each argument. The questionnaire defined a strong argument as one that "you feel would be difficult to refute or argue against." Finally, respondents were told to try to disregard their own opinions about the policies when making their judgments and to make their judgment for each statement independently of their judgments of other statements. After reading two examples illustrating the task, subjects rated the arguments on 15-point scales anchored by *very weak* and *very strong*. Each respondent completed a version of the questionnaire containing one third of the arguments.

Ratings of arguments from the same message were averaged. As a check on the validity of respondents' judgments, the mean ratings of sets of arguments that researchers had manipulated to be strong or weak (e.g., Petty & Cacioppo, 1979b)⁶ were compared and found to differ significantly ($ps < .05$ or smaller) in the expected directions.

Computation and Analysis of Effect Sizes

The effect size calculated is g , the difference between the persuasion means of the high- and low-involvement groups, divided by the pooled standard deviation (see Hedges & Olkin, 1985). In this study, the computation of g was based on (a) F and t for 80.0% of the studies, (b) means and standard deviations or error terms for 17.5% of the studies, and (c) proportions of high- and low-involvement subjects who changed their attitudes for 2.5% of the studies. Two studies (Leippe & Elkin, 1987;

Neises, 1988) manipulated two types of involvement; in these cases, a separate effect size was computed for each manipulation.¹⁰ In studies that manipulated argument strength or number of arguments, separate within-study effect sizes were also computed within each level of the manipulation.¹¹ For the one study that crossed the strength and number of arguments in a factorial design (Petty & Cacioppo, 1984), effect sizes were computed within each of the resulting combinations of strength and number.

The pooled standard deviation that is the denominator of the effect size was estimated, whenever possible, only from the portion of each study's data entering into the effect size. For example, if an involvement effect size was calculated within the strong-arguments condition of a study, the pooled standard deviation was estimated from the standard deviations given for the strong-arguments subjects, if this information was available.

When the pooled standard deviation was estimated from the mean square error of an analysis of variance (ANOVA), this error term was sometimes reconstituted by adding into the sum of squares error all (available) between-groups sums of squares except that for involvement. By this procedure, recommended by Hedges and Becker (1986) and Glass, McGaw, and Smith (1981), one-way designs can be approximated. The procedure was followed for individual-difference variables that were crossed with involvement but not for manipulated variables, which in some experiments were quite powerful. Consequently, adding sums of squares for manipulated variables (e.g., argument strength, communicator credibility) to the sum of squares error would have had differing impact on these error terms, across the studies.

Glass et al. (1981) recommended that criterion measures reported in terms of gain scores or covariance-adjusted posttest scores rather than posttest scores should be converted to the metric of posttest scores, in order to ensure greater comparability between the effect sizes. This conversion was not performed because the correlation between the pretest and posttest scores, which is needed for the conversion, was not available in any of the studies that used change scores or covariance-adjusted posttest scores.

These effect sizes were computed independently by each of us, who then resolved any discrepancies. The g s were converted to d s by correcting them for bias (i.e., g 's overestimate of the population effect size, which occurs especially for small samples; see Hedges, 1981; Hedges & Olkin, 1985). Then the study outcomes were combined by averaging the

(text continues on page 301)

⁸ Our classification of the Rhine and Severance (1970) study as value relevant may be puzzling to readers because the issue used in the high-involvement condition, the desirability of increasing tuition at the University of California, may seem to be the type of issue that would have activated outcome-relevant concerns among the University of California, Riverside, students who served as subjects. Although the Rhine and Severance study may not be a clear-cut instance of value-relevant involvement, we believe that the issue aroused primarily value-relevant concerns because the study was conducted when "tuition was being actively discussed by the regents of the university, the Governor of the state, the press, the faculty, and the students. A student march on the State Capitol had been held to protest suggestions for increased tuition" (Rhine & Severance, 1970, p. 177).

⁹ Agreement was lowest (79%) for the outcome of the manipulation check.

¹⁰ For each type of involvement, the low-involvement mean was subtracted from the high-involvement mean within the low-involvement condition of the second type of involvement, and the resulting difference was divided by the pooled standard deviation.

¹¹ This procedure was not followed for one study (Chaiken, 1980, Study 2) because it confounded number of arguments with communicator likability.

Table 1
Summary of Study Characteristics

Variable and class	All studies (<i>n</i> = 38)	Value-relevant involvement studies (<i>n</i> = 15)	Outcome-relevant involvement studies (<i>n</i> = 20 ^a)	Impression-relevant involvement studies (<i>n</i> = 5)
Publication characteristics				
<i>Mdn</i> publication year	1979	1970	1984	1987
Publication form				
Journal or other published document	27	9	16	3
Dissertation or master's thesis	11	6	4	2
Message characteristics				
<i>M</i> argument strength ^b	8.27	8.51	8.13	8.33
<i>M</i> length of message in words ^c	562.31	625.33	554.20	539.80
Amount of subjects' knowledge				
Little or none	16	1	15	2
Moderate	10	2	5	3
Considerable	9	9	0	0
Knowledge covaried with involvement	3	3	0	0
Message modality				
Print	26	10	13	4
Audio	10	3	7	1
Video	2	2	0	0
Number of issues per level of involvement				
One	33	12	19	3
Two	5	3	1	2
Message domain				
College issues	16	0	16	2
Social policy issues	16	13	1	2
Consumer products	3	0	3	0
More than one	3	2	0	1
Involvement characteristics				
Method of involvement variation				
Differing consequences	23	0	20	5
Subject classification	10	10	0	0
Topics differing in level of involvement	3	3	0	0
Other methods	2	2	0	0
Status of involvement in experimental design				
Between subjects	36	13	20	5
Within subjects	2	2	0	0
Presence of involvement manipulation check				
Present	19	5	12	2
Absent	19	10	8	3
Outcome of manipulation check				
Significant	16	4	10	1
Mixed or marginal	4	0	2	1
Unknown or check absent	20	11	8	3
Other method characteristics				
Type of subject population				
High school	4	3	0	1
College undergraduate	34	12	20	4
<i>Mdn n</i> of subjects	116	101	80	148
Quality of persuasion measure				
Single item	8	2	5	1
Multi-item, unknown reliability	19	12	5	2
Multi-item, high reliability	11	1	10	2
Metric for persuasion measure				
Posttest	23	1	19	5
Covariance-adjusted posttest or change score based on subjects' pretest	13	12	1	0
Change score based on involvement control groups	2	2	0	0

Note. For categorical variables, numbers in table represent frequencies of studies in each class.

^a Two studies (Leippe & Elkin, 1987; Neises, 1988) are represented twice, once in outcome-relevant involvement and once in impression-relevant involvement.

^b Based on the mean for each study for which ratings were obtained; judgments are on a 1-to-15 scale in which higher numbers indicate greater strength.

^c Based on studies for which reports were available.

Table 2
Involvement Effect Sizes and Characteristics for Studies of Value-Relevant, Outcome-Relevant, and Impression-Relevant Involvement

Study	Effect size (d)	95% CI for d		Message characteristics ^a	Issue	Involvement characteristics ^b	Method characteristics ^c
		Lower	Upper				
Value-relevant involvement studies							
Aiello (1967)	-1.09	-1.32	-0.85	7.29/363/4/1/2/2	HI: Desirability of restrictions on teen driving; fairness of Jack Ruby's jail sentence LI: New comedian's odds for success on TV; the postwar status of Paul von Hindenburg	3/2/1/3	1/2/2
Eagly & Telaar (1972)	-1.02	-1.64	-0.41	8.04/138/3/1/1/2	Morality and desirability of birth control	2/1/2/3	2/2/2
Gantt (1970)	0.00	-0.57	0.57	10.01/1855/3/2/1/2	Morality of the Vietnam War	2/1/2/3	2/3/2
Gorn (1975)	-0.13	-0.40	0.13	8.40/352/3/1/1/2	Desirability of the secession of Quebec from Canada	2/1/1/1	2/2/3
Letchworth (1969)	-0.32	-0.60	-0.04	8.30/618/3/1/1/2	The possibility that religious people are bigoted and dogmatic	2/1/2/3	2/2/2
Miller (1965)	-0.75	-1.39	-0.11	9.44/750/2/2/1/2	The value of public water supply fluoridation	4/1/2/3	1/2/2
Ostrom & Brock (1968)	-0.22	-0.56	0.12	8.11/450/1/1/1/2	Greenland's participation in the Pan-American Bank	4/1/2/3	2/1/1
Powell (1977)	-0.55	-0.96	-0.13	8.77/547/2/3/1/2	The value of congressional politicians	2/1/2/3	2/2/2
Rand (1967)	-0.17	-0.39	0.05	8.94/472/3/1/2/4	Collegiate athletics; the possibility that religious people are bigoted	2/1/1/1	2/2/2
Rhine & Severance (1970)	-0.79	-1.12	-0.45	-/625/4/1/1/4	HI: College tuition increases LI: Park size increases in distant city	3/1/1/1	2/1/3
Rupnow (1970)	-0.69	-1.10	-0.29	8.96/1050/3/2/1/2	Morality of the Vietnam War	2/1/2/3	2/2/2
Rupnow (1975)	-0.22	-0.95	0.51	7.93/-/3/3/1/2	Opportunities for women in the United States	2/1/2/3	1/2/2
Sereno (1968)	-1.73	-2.31	-1.16	-/3/1/1/2	Desirability of unmarried women using contraceptive pill	2/1/2/3	2/2/2
Sereno & Bodaken (1972)	0.05	-0.58	0.68	-/3/1/1/2	Desirability of draft deferments for college students	2/1/2/3	2/2/2
C. W. Sherif, Kelly, Rodgers, Sarup, & Tittler (1973, Study 4)	-0.44	-0.69	-0.19	7.89/284/4/1/2/2	HI: Appropriate male- and female-related career choices LI: New comedian's odds for success on TV; the postwar status of Paul von Hindenburg	3/2/1/1	2/2/2
Outcome-relevant involvement studies							
Appler & Sears (1968; warning condition removed)	0.64	0.00	1.27	-/500/2/1/1/1	Desirability of replacing college professors with teaching assistants	1/1/1/2	2/1/2
Axson, Yates, & Chaiken (1987)	0.18	-0.16	0.52	8.83/646/2/2/1/2	Desirability of using probation instead of imprisonment for criminals	1/1/1/1	2/2/1
Strong arguments	0.35	-0.14	0.83	9.28/648			
Weak arguments	0.01	-0.47	0.49	8.38/643			
Burnkrant & Howard (1984; rhetorical questions condition removed)	0.07	-0.36	0.51	7.76/671/1/1/1/1	Institution of comprehensive exams for college seniors	1/1/1/1	2/3/1
Strong arguments	0.22	-0.40	0.84	9.01/665			
Weak arguments	-0.07	-0.69	0.55	6.51/677			
Chaiken (1980, Study 2)	-0.05	-0.48	0.39	8.60/298/1/1/1/1	Change from semesters to trimesters	1/1/1/1	2/1/1
Fredericks (1988)	0.28	0.03	0.53	7.76/400/1/1/1/1	Institution of comprehensive exams for college seniors	1/1/1/1	2/3/1
Strong arguments	0.21	-0.15	0.57	9.01/400			
Weak arguments	0.35	-0.01	0.71	6.51/400			

Table 2 (continued)

Study	Effect size (<i>d</i>)	95% CI for <i>d</i>		Message characteristics ^a	Issue	Involvement characteristics ^b	Method characteristics ^c
		Lower	Upper				
Outcome-relevant involvement studies (continued)							
Homer (1987)	0.30	0.00	0.60	7.73/47/1/1/1/3	Stereo components	1/1/1/1	2/3/1
Strong arguments	0.41	-0.02	0.84	9.41/49			
Weak arguments	0.19	-0.23	0.62	6.05/45			
Leippe & Eilkin (1987)	-0.15	-0.67	0.38	8.12/806/1/2/2/1	Institution of comprehensive exams for college seniors; parking fees for undergraduates	1/1/1/1	2/3/1
Strong arguments	1.79	0.91	2.66	9.05/821			
Weak arguments	-2.57	-3.57	-1.57	7.18/791	Desirability of essay exams instead of multiple-choice exams	1/1/1/1	2/3/1
Liberman (1988)	-0.36	-0.59	-0.13	8.69/448/2/1/1/1			
Strong arguments	-0.35	-0.67	-0.02	11.66/443			
Weak arguments	-0.37	-0.70	-0.05	7.72/453			
Neises (1988; extreme issue involvement conditions removed)	-0.34	-1.04	0.35	9.01/800/1/1/1/1	Institution of comprehensive exams for college seniors	1/1/1/1	2/3/1
Petty & Cacioppo (1979a; warned conditions removed)	0.50	-0.04	1.04	9.01/888/1/2/1/1	Institution of comprehensive exams for college seniors	1/1/2/3	2/1/1
Petty & Cacioppo (1979b, Study 1; proattitudinal message conditions removed)	-1.40	-2.66	-0.14	8.15/500/2/2/1/1	Appropriate coeducational dormitory visitation hours	1/1/1/1	2/2/1
Petty & Cacioppo (1979b, Study 2)	0.05	-0.41	0.51	7.76/1000/1/2/1/1	Institution of comprehensive exams for college seniors	1/1/1/2	2/2/1
Strong arguments	0.59	-0.08	1.26	9.01/1000			
Weak arguments	-0.50	-1.16	0.16	6.51/1000			
Petty & Cacioppo (1981b, Study 2)	0.00	-0.25	0.25	7.98/60/1/1/1/3	Vilance shampoo	1/1/2/3	2/2/1
Strong arguments	0.42	0.06	0.78	10.35/60			
Weak arguments	-0.42	-0.78	-0.06	5.61/60	College tuition increases	1/1/2/3	2/1/1
Petty & Cacioppo (1984, pilot study)	-0.05	-0.63	0.53	8.33/136/2/1/1/1			
Strong arguments	0.80	-0.24	1.84	9.58/99			
Weak arguments	-0.27	-1.27	0.74	7.08/105			
Both strong & weak arguments	0.71	-0.33	1.74	8.33/204			
Petty & Cacioppo (1984, main experiment)	-0.05	-0.35	0.26	7.76/706/1/1/1/1	Institution of comprehensive exams for college seniors	1/1/2/3	2/3/1
Nine strong arguments	0.49	-0.12	1.11	9.01/1050			
Three strong arguments	0.66	0.03	1.28	9.01/350			
Three weak arguments	-0.06	-0.67	0.54	6.51/356			
Nine weak arguments	-1.08	-1.73	-0.44	6.51/1068			
Petty, Cacioppo, & Goldman (1981)	0.09	-0.23	0.42	7.76/739/1/2/1/1	Institution of comprehensive exams for college seniors	1/1/2/3	2/2/1
Strong arguments	0.50	0.03	0.96	9.01/754			
Weak arguments	-0.31	-0.77	0.15	6.51/724			
Petty, Cacioppo, & Hessecker (1981; rhetorical questions conditions removed)	0.09	-0.36	0.53	7.76/739/1/2/1/1	Institution of comprehensive exams for college seniors	1/1/2/3	2/3/1
Strong arguments	0.81	0.16	1.46	9.01/754			
Weak arguments	-0.64	-1.28	0.00	6.51/724			
Petty, Cacioppo, & Schumann (1983)	-0.41	-0.73	-0.09	7.96/58/1/1/1/3	Edge disposable razors	1/1/2/3	2/3/1
Strong arguments	0.34	-0.10	0.79	9.87/58			
Weak arguments	-0.94	-1.41	-0.47	6.04/58			
Schul & Knapp (1984; bogus pipeline conditions removed)	0.22	-0.55	0.98	9.01/894/1/1/1/1	Institution of comprehensive exams for college seniors	1/1/2/3	2/1/1

(table continues)

Table 2 (continued)

Study	Effect size (<i>d</i>)	95% CI for <i>d</i>		Message characteristics ^a	Issue	Involvement characteristics ^b	Method characteristics ^c
		Lower	Upper				
Outcome-relevant involvement studies (continued)							
Sorrentino, Bobocel, Citta, Olson, & Hewitt (1988, Study 2)	0.16	-0.12	0.45	7.76/748/1/1/1/1	Institution of comprehensive exams for college seniors	1/1/1/1	2/3/1
Strong arguments	0.30	-0.09	0.69	9.01/742			
Weak arguments	0.03	-0.39	0.44	6.51/754			
Impression-relevant involvement studies							
Chaiken (1980, Study 1)	0.09	-0.20	0.38	8.71/368/2/1/2/4	The amount of sleep that is optimal; change from semesters to trimesters	1/1/1/2	2/1/1
Six arguments	0.22	-0.19	0.63	8.61/482			
Two arguments	-0.04	-0.45	0.37	8.81/254			
Johnson & Scitelli (1969)	-0.28	-0.56	0.00	6.87/350/2/1/1/2	Desirability of chest X rays to detect tuberculosis	1/1/2/3	1/2/1
Plausible arguments	-0.41	-0.81	-0.02	9.57/350			
Implausible arguments	-0.15	-0.54	0.24	4.17/350			
Leippe & Elkin (1987)	-0.34	-0.87	0.19	8.12/806/1/2/2/1	Institution of comprehensive exams for college seniors; parking fees for undergraduates	1/1/1/1	2/3/1
Strong arguments	-0.37	-1.11	0.38	9.05/821			
Weak arguments	-0.30	-1.04	0.45	7.18/791			
Neises (1988; temporary response involvement conditions removed)	-0.74	-1.45	-0.02	9.01/800/1/1/1/1	Institution of comprehensive exams for college seniors	1/1/2/3	2/3/1
Shechter (1987/1988; favorable and unfavorable partner attitude conditions removed)	-0.17	-0.50	0.15	8.93/375/2/1/1/2	Desirability of a large amount of media coverage of hijacking events	1/1/2/3	2/1/1
Strong arguments	-0.21	-0.67	0.26	10.61/375			
Weak arguments	-0.14	-0.60	0.32	7.24/375			

Note. Effect sizes are positive for differences in the high-involvement direction and negative for differences in the low-involvement direction. CI = confidence interval; HI = high-involvement issue; LI = low-involvement issue.

^a The first variable is mean argument strength judgments on a 1-to-15 scale on which higher numbers indicate greater strength; the second variable is message length in words; the third variable is subjects' prior knowledge about the issue (1 = little or none, 2 = moderate, 3 = considerable, 4 = knowledge covered with involvement); the fourth variable is message modality (1 = print, 2 = audio, 3 = video); the fifth variable is number of issues used per level of involvement; and the sixth variable is message domain (1 = college issues, 2 = social policy issues, 3 = consumer products, 4 = more than one). ^b The first variable is method of involvement variation (1 = differing consequences, 2 = subject classification, 3 = issues differing in involvingsness, 4 = other methods); the second variable is status of involvement in the experimental design (1 = between subjects, 2 = within subjects); the third variable is presence of involvement manipulation check (1 = present, 2 = absent); and the fourth variable is outcome of involvement manipulation check (1 = high school, 2 = college undergraduate). ^c The first variable is type of subject population (1 = high school, 2 = college undergraduate); the second variable is quality of persuasion measure (1 = single-item, 2 = multi-item, unknown reliability, 3 = multi-item, high reliability); and the third variable is metric for persuasion measure (1 = posttest, 2 = covariance-adjusted posttest or change score based on subjects' pretest, 3 = change score based on involvement control groups).

ds. The homogeneity of each set of *ds* was examined to determine whether the studies shared a common effect size (Hedges, 1981; Hedges & Olkin, 1985). In the absence of homogeneity, we accounted for variability in heterogeneous effect sizes by relating them to the attributes of the studies. To determine the relation between these study characteristics and the magnitude of the effect sizes, both categorical and continuous models were tested (Hedges, 1982a, 1982b; Hedges & Olkin, 1985). Categorical models, which are analogous to ANOVAs, may show that heterogeneous effect sizes are homogeneous within the subgroups established by dividing studies into classes based on study characteristics. Similarly, continuous models, which are analogous to regression models, are regarded as correctly specified when the systematic variability in the effect sizes is explained by the study attributes used as predictors. If homogeneity is not achieved within the classes when implementing categorical models and correctly specified models are not achieved when implementing continuous models, the results of these analyses cannot be interpreted as confidently as they otherwise would be.

As an alternative analysis to predicting effect sizes using categorical and continuous models, we attained homogeneity by identifying outliers among the effect sizes and sequentially removing those that reduced the homogeneity statistic by the largest amount (see Hedges & Olkin, 1985). Using such a procedure, Hedges (1987) found for several meta-analyses on psychological topics that the removal of up to 20% of the outliers in a group of heterogeneous effect sizes usually resulted in a high degree of homogeneity. Studies yielding effect sizes identified as outliers can then be examined after the fact to determine if they appear to differ methodologically from the other studies. Inspection of the percentage of effect sizes removed to attain homogeneity allows one to determine whether the effect sizes are homogeneous aside from the presence of relatively few aberrant values. Under such circumstances, the mean attained after removal of such outliers may better represent the distribution of effect sizes than would the mean based on all of the effect sizes.

Results

Characteristics of Studies

Before considering the effects of involvement on attitude change, we examine the characteristics of the studies from which conclusions about this research are drawn. Table 1 shows these study characteristics aggregated over all of the studies, as well as summarized separately within the classes of value-, outcome-, and impression-relevant involvement. Table 2 presents each study's involvement effect size (*d*) along with its 95% confidence interval (CI), study attributes, and a brief description of the issue used in each message. For studies that manipulated argument strength or number of arguments, effect sizes and confidence intervals are also presented within each level of these variables, along with the study characteristics that differed between these levels.

Across all types of involvement, as is shown by the central tendencies of the variables in Table 1, studies (a) were either value or outcome relevant; (b) were published relatively recently; (c) were published in journals; (d) presented messages with argument strength at approximately the midpoint of the scale used for obtaining respondents' judgments; (e) presented messages of moderate length (about two double-spaced typed pages or 2 min of speech); (f) used issues for which subjects had little (or no), moderate, or considerable prior knowledge; (g) presented messages via the print modality; (h) presented only one issue per level of involvement; (i) included or omitted manipulation checks; (j) obtained a significant manipulation

check when a check was present; (k) sampled subjects from college undergraduate populations; (l) used a moderate number of subjects; (m) assessed persuasion via multiple-item measures of unknown reliability; and (o) used either posttest or change score metrics.¹² Within the classes of involvement studies, notable exceptions to these overall patterns are that (a) value-relevant studies were published earlier than studies on the other two types of involvement; (b) outcome-relevant studies had smaller sample sizes than the other two types of studies, a trend that reflects our exclusion of some of the experimental conditions of several of the outcome-relevant studies; (c) subjects were more knowledgeable on the issues used in value-relevant studies than on the issues used in outcome-relevant studies; (d) value-relevant studies used social policy issues to a greater extent than did outcome-relevant studies, which typically used college issues; (e) value-relevant studies usually varied involvement by subject classification methods, whereas outcome- and impression-relevant studies manipulated involvement by describing differing consequences to the subjects; and (f) value-relevant studies typically presented findings in a change-score metric, whereas outcome- and impression-relevant studies presented them in a posttest metric.

Study Effect Sizes

With each study contributing a single effect size, a mean was computed with each of the effect sizes weighted by the reciprocal of its variance (Hedges & Olkin, 1985). This weighting procedure gives greater weight to effect sizes that are more reliably estimated. The resulting mean was -0.21 , indicating greater persuasion with low involvement than with high involvement. The 95% confidence interval for this mean, $CI = -0.27$ to -0.15 , shows that it differed significantly from the 0.00 value that indicates exactly no effect. Calculation of a homogeneity statistic, Q , which has an approximate chi-square distribution with $k - 1$ degrees of freedom, where k is the number of effect sizes (Hedges, 1982a; Hedges & Olkin, 1985), indicated that the hypothesis of homogeneity was rejected, $Q(39) = 190.83$, $p < .001$. Therefore, study attributes were used to account for variability in the involvement effect sizes.

Categorical models. Categorical models were fitted to the effect sizes following Hedges and Olkin's (1985) statistical procedures. These techniques provide a between-classes effect (analogous to a main effect in an ANOVA) and a test of the homogeneity of the effect sizes within each class. The between-classes effect is estimated by Q_B , which has an approximate chi-square distribution with $p - 1$ degrees of freedom, where p is the number of classes. The homogeneity of the effect sizes within each class is estimated by Q_{W_i} , which has an approximate chi-square distribution with $m - 1$ degrees of freedom, where m is the number of effect sizes in the class. The tables reporting tests of categorical models also include (a) the mean weighted effect size for each class, calculated with each effect size weighted by the reciprocal of its variance, and (b) the 95% confidence interval for each mean (Tables 3, 4, and 7).

¹² All of the studies in the sample assessed persuasion via a questionnaire measure of attitudes or beliefs administered soon after the persuasive message. Delayed measures were not used for this meta-analysis.

Table 3
Test of Categorical Model for Type of Involvement

Variable and class	Between-classes effect (Q_B)	n	Mean weighted effect size (d_{i+})	95% CI for d_{i+}		Homogeneity within each class (Q_{wi}) ^a
				Lower	Upper	
Type of involvement	68.68**					
Value relevant		15	-0.48	-0.57	-0.40	73.17**
Outcome relevant		20	0.02	-0.06	0.10	40.68*
Impression relevant		5	-0.17	-0.33	-0.01	6.57

Note. Effect sizes are positive for differences in the high-involvement direction and negative for differences in the low-involvement direction. CI = confidence interval.

^a Significance indicates rejection of the hypothesis of homogeneity.

* $p < .01$. ** $p < .001$.

Categorical model for type of involvement. Consistent with the significant between-classes effect for type of involvement shown in Table 3, a priori comparisons among the mean weighted effect sizes for the three classes of involvement (Hedges & Becker, 1986; Hedges & Olkin, 1985) showed that the mean for the value-relevant class differed significantly from the means for both the outcome- and impression-relevant classes, $\chi^2(1) = 68.45$, and $\chi^2(1) = 11.73$, respectively, $ps < .001$. As is shown by the 95% confidence intervals computed for these classes, low-involvement subjects were significantly more persuaded than high-involvement subjects in the value-relevant and impression-relevant studies, whereas involvement had no significant overall effect in the outcome-relevant studies. These results were not unexpected because for outcome-relevant studies, involvement's effects should depend on the strength of the persuasive message, which is not taken into account in this study-level analysis.

As is shown in Table 3 by the homogeneity statistic (Q_w) for each class, only the impression-relevant class was homogeneous.¹³ In the value-relevant class, homogeneity was attained after the removal of three effect sizes (20%) identified as outliers, $Q_w(11) = 18.36$, $p = .07$. In order of decreasing reduction of homogeneity, the removed studies were Aiello (1967), Sereno (1968), and Rhine and Severance (1970). The resulting mean weighted effect size was -0.32 (CI = -0.42 to -0.21). We did not identify outliers among the outcome-relevant studies until they were partitioned on argument strength (see next section).

Study and Within-Study Effect Sizes

Categorical models for argument strength within type of involvement. In order to test the hypothesis that involvement interacts with the strength of the persuasive message to affect persuasion, we applied categorical models to the 60 study and within-study effect sizes that we had available after the studies that manipulated the strength or number of persuasive arguments were partitioned on this basis. For the categorical models, classes were formed on the basis of the strength of the arguments, and the number of arguments was taken into account by treating message length as a predictor in the continuous models presented in the next subsection. In the categorical models, all studies that did not manipulate argument strength were assigned to the weak or strong category on the basis of the mean

judgments we obtained for these studies' persuasive arguments. Weak arguments were defined as those with mean judgments lower than the upper boundary of the mean judgments obtained for arguments labeled weak by study authors. Studies for which persuasive arguments were unavailable were omitted from this analysis.

Table 4 presents the results of tests of categorical models for argument strength within each of the classes of involvement effect sizes. As is indicated by the mean argument strength ratings associated with the strong and weak classes, the weak arguments used in the outcome- and impression-relevant studies were weaker than those used in the value-relevant studies. Thus, the relatively weak argumentation was not comparable across the three types of involvement studies, whereas the relatively strong argumentation was quite comparable.

Within the value-relevant group of effect sizes, the between-classes effect and the means and confidence intervals for the strong- and weak-argument classes show that high-involvement subjects were less persuaded than low-involvement subjects, but this difference was smaller with strong than with weak arguments. The strong-arguments class was homogeneous, and the weak-arguments class became homogeneous with the removal of one study (Aiello, 1967), $Q_w(4) = 5.28$, $p = .26$. The resulting mean effect size was -0.38 (CI = -0.54 to -0.23).

With outcome-relevant involvement, high-involvement subjects were more persuaded by strong arguments and less persuaded by weak arguments than were low-involvement subjects, but neither the strong- nor the weak-arguments class was homogeneous. Removal of one study (5%; Liberman, 1988) from the strong-arguments class established homogeneity, $Q_w(16) = 19.52$, $p = .24$. The resulting mean effect size was 0.41 (CI = 0.28 to 0.53). Homogeneity was not attained for the weak-arguments class until a relatively large proportion of effect sizes ($n = 4$; 24%) were removed, $Q_w(12) = 17.51$, $p = .13$. In order of decreasing reduction of homogeneity, the removed studies were Leippe and Elkin (1987), Fredericks (1988), Petty, Cacioppo, and Schumann (1983), and Petty and Cacioppo (1984, nine-

¹³ When interpreting the finding that the hypothesis of homogeneity was not rejected for this and other groups of effect sizes, readers should keep in mind that the relatively small number of effect sizes we had available limited our power to detect deviations from homogeneity.

Table 4
Tests of Argument-Strength Categorical Model for Studies of Value-Relevant, Outcome-Relevant and Impression-Relevant Involvement

Variable and class	Mean argument strength judgments ^a	Between classes effect (Q_B)	n	Mean weighted effect size (d_{i+})	95% CI for d_{i+}		Homogeneity within each class (Q_{wi}) ^b
					Lower	Upper	
Value-relevant involvement							
Argument strength		9.80*					
Strong	9.09		6	-0.28	-0.42	-0.14	10.74
Weak	7.93		6	-0.58	-0.71	-0.45	27.93**
Outcome-relevant involvement							
Argument strength		43.15**					
Strong	9.35		18	0.31	0.19	0.43	37.49*
Weak	6.86		17	-0.26	-0.39	-0.14	63.54**
Impression-relevant involvement							
Argument strength		0.00					
Strong	9.28		6	-0.17	-0.37	0.02	8.02
Weak	6.20		3	-0.17	-0.44	0.11	0.14

Note. Effect sizes are positive for differences in the high-involvement direction and negative for differences in the low-involvement direction. The values are based on the available messages. CI = confidence interval.

^a On a 1-to-15 scale in which higher numbers indicate greater strength.

^b Significance indicates rejection of the hypothesis of homogeneity.

* $p < .01$. ** $p < .001$.

arguments condition). The resulting mean effect size was -0.21 (CI = -0.35 to -0.07). The between-classes effect for argument strength was not significant within the impression-relevant group, which was homogeneous overall, $Q(8) = 8.16$, $p = .42$.¹⁴

Continuous models for argument strength and message length within type of involvement. Continuous models were used in order to test the effects of argument strength and message length on the magnitude of the involvement effect sizes available after partitioning the studies on the strength and number of arguments. These analyses are appropriate only within the classes of value- and outcome-relevant studies because the impression-relevant class contained only nine effect sizes, which were homogeneous.¹⁵ Although it would have been preferable to treat number of arguments (rather than message length) as an independent variable in the continuous models, information on length was much more frequently included in the reports, and length can be assumed to covary with the number of arguments. Finally, to assess the simultaneous impact of argument strength and message length, a multiple regression model including both of these variables was assessed.¹⁶

The continuous models are least squares regressions, calculated with each effect size weighted by the reciprocal of its variance. Each such model yields a test of the significance of each predictor as well as a test of model specification, which evaluates whether significant systematic variation remains unexplained in the regression model (Hedges, 1982b; Hedges & Olkin, 1985). The sum of squares error statistic, Q_E , which provides this test of model specification, has an approximate chi-square distribution with $k - p - 1$ degrees of freedom,

where k is the number of effect sizes and p is the number of predictors (not including the intercept).

Shown in Table 5 are the results for the continuous models for the value-relevant involvement effect sizes. In both the simple linear regression and the multiple regression, strength related positively to the magnitude of effect sizes. Thus, consistent with the categorical model for argument strength (see Table 4), as the strength of the argumentation increased, the tendency for low-involvement subjects to be more persuaded than high-involvement subjects was weakened. Finally, although message length did not relate to the effect sizes in the simple linear regression, it related negatively to them when analyzed in conjunction with argument strength. Thus, as the length of the messages in-

¹⁴ In this categorical model, both the six-arguments condition and the two-arguments condition of Chaiken (1980, Study 1) appear in the strong-arguments category. When the categories were defined instead as the strong-message and weak-message categories and Chaiken's six-arguments condition was therefore classified as strong and her two-arguments condition as weak (but the other studies' conditions remained classified as weak or strong on the basis of argument strength), the between-classes effect remained nonsignificant.

¹⁵ The continuous models were initially estimated without the studies for which message length or argument strength were unknown. Because the results were essentially unchanged by assigning to these studies (or to conditions within the studies) values based on the means for their respective classes, only the models that substituted means for missing values are presented.

¹⁶ These models do not include the interaction of message length and argument strength because this term proved to be nonsignificant.

Table 5
Tests of Argument-Strength and Message-Length Continuous Models for Value-Relevant Involvement Studies

Predictor or outcome	Simple linear regression		Multiple regression	
	<i>b</i>	<i>b</i> *	<i>b</i>	<i>b</i> *
Argument strength	0.30**	.47	0.45**	.70
Message length	0.00	.11	0.00**	-.35
Additive constant			-4.02	
Multiple <i>R</i>			.54	
<i>SE</i> of estimate			.33	
Q_E^b			51.76**	

Note. Models are weighted least squares regressions calculated with weights equal to the reciprocal of the variance for each effect size. In the multiple regression, the predictors were entered simultaneously; *b* = unstandardized regression coefficient; *b** = standardized regression coefficient. Effect sizes are positive for differences in the high-involvement direction and negative for differences in the low-involvement direction; *n* = 15.

* $b = -0.00049$; $SE(b) = .00022$. ^b Significance indicates model not correctly specified.

* $p < .05$. ** $p < .001$.

creased, the tendency for low-involvement subjects to be more persuaded than high-involvement subjects was strengthened. As is reflected in the multiple *R* of .54, this model was moderately successful in accounting for variability in the magnitude of the effect sizes, although the test of model specification (Q_E) showed that it cannot be regarded as correctly specified.

The same models were assessed within the outcome-relevant involvement class. As is shown in Table 6 under Simple linear regression and Multiple regression A, only argument strength was found to significantly influence the magnitude of the effect sizes in these models. Consistent with the categorical model for argument strength (see Table 4), with strong arguments, high-involvement subjects were more persuaded than low-involvement subjects, and with weak arguments the opposite pattern was obtained. Yet the multiple *R* of .37 indicated that argument strength was only somewhat successful in accounting for variability in the magnitude of the outcome-relevant effect sizes. Not surprisingly, this model cannot be regarded as correctly specified; the Q_E statistic was again highly significant.

Other predictors of outcome-relevant involvement effect sizes. In view of the surprising finding that argument strength accounted for more variability in the value-relevant effect sizes than in the outcome-relevant effect sizes, we decided to search for other study characteristics that could account for this disparity. Aside from the variation in argument strength and message length that we have already examined, the outcome-relevant studies were methodologically quite similar. However, perusal of the outcome-relevant effect sizes suggested that the studies authored by Petty, Cacioppo, and their colleagues obtained the predicted effects of argument strength, whereas other researchers obtained them more weakly or not at all. An exception to this pattern is the study by Leippe and Elkin (1987), which produced much larger effects than were typically obtained by the Petty and Cacioppo group. Because Leippe, like Petty and Cacioppo, obtained the PhD from Ohio State Univer-

sity in the late 1970s, the expected pattern of results might be described as obtained only by a particular group of researchers. Therefore, we classified the studies according to research group, placing the Leippe and Elkin study with the Petty and Cacioppo studies in a class that we have labeled, merely for convenience, the *Ohio State researchers* and placing the remaining studies in a class we labeled *other researchers*.¹⁷

To examine the effect of argument strength within each research group, we estimated a simple linear regression model with argument strength as the predictor of the involvement effect sizes. Within the Ohio State group, argument strength was a substantial predictor of the effect sizes ($b = 0.30$, $b^* = .72$, $p < .001$; $Q_E(19) = 58.08$, $p < .001$), whereas among the other researchers, argument strength failed to predict the effect sizes ($b = -0.04$, $b^* = -.23$, $p = .23$; $Q_E(13) = 25.24$, $p < .05$). To fully represent this interaction between research group and the effects of argument strength, we used the effect sizes of both groups and estimated a model that included as predictors argument strength, research group, and their interaction (see Table 6, under Multiple regression B). The results showed that this interaction was indeed significant and that this model was considerably more successful in accounting for variability in the effect sizes, as is shown by its multiple *R* of .66. Because this model was not well specified, we proceeded to calculate categorical models that took research group into account, in order to determine whether the lack of homogeneity was confined to certain subgroups of studies. These models, which are presented in Table 7, show that research group was a highly significant predictor of effect size magnitude, within both the strong- and weak-arguments classes. With strong arguments, the Ohio State group obtained the predicted positive mean effect size, whereas the other researchers found a mean effect size that did not differ from zero.¹⁸ Similarly, with weak arguments, the Ohio State group obtained the predicted negative mean effect size, whereas the other researchers found a mean effect size that did not differ from zero. Two of the four subclasses lacked homogeneity: strong arguments subclass for other researchers and weak arguments subclass for Ohio State researchers.

Among the many possible reasons that the Ohio State group obtained stronger effects of argument strength is that their manipulations of this variable were more impactful. We tested this hypothesis by performing a Research Group (Ohio State vs. other) \times Argument Strength Label (weak vs. strong) ANOVA on the studies' mean argument strength ratings. Although this analysis found the expected large effect for argument strength, $F(1, 32) = 92.27$, $p < .001$, neither the main effect of group nor the interaction proved significant ($F_s < 1$). Thus, this hypothesis failed to be confirmed by this analysis or, for that matter, by Multiple regression B, which controlled the interaction for the

¹⁷ Of course, the term *Ohio State researchers* should not be taken to imply that the environment or training provided by Ohio State University is causally related to obtaining these effects. The models that included research group (see text), were estimated with the Leippe and Elkin (1987) study being categorized in the *other researchers* group as well as being excluded from the analyses. Our results were essentially unchanged by these alternative classifications.

¹⁸ This model remained significant when the one outlying effect size (Lieberman, 1988) was deleted, $Q_B(1) = 4.94$, $p < .05$.

effects of argument strength.¹⁹ Our search for additional study characteristics that might account for the disparity between the Ohio State researchers' findings and those of other researchers was not successful.²⁰

Discussion

In this review, we have defined involvement as a motivational state induced by an association between an activated attitude and the self-concept. The findings we obtained are largely congruent with our proposal that involvement has taken three distinct forms in attitude-change research, depending on whether the aspect of the self-concept that was activated is one's enduring values (establishing value-relevant involvement), one's ability to obtain desirable outcomes (establishing outcome-relevant involvement), or the impression one makes on others (establishing impression-relevant involvement).

We found that (a) with value-relevant involvement, high-involvement subjects were less persuaded than low-involvement subjects; (b) with outcome-relevant involvement, high-involvement subjects were more persuaded than low-involvement subjects by strong arguments and less persuaded by weak arguments; and (c) with impression-relevant involvement, high-involvement subjects were slightly less persuaded than low-involvement subjects. Although there are some important qualifications to these generalizations, these results confirm our view that the effects of involvement on attitude change cannot be described in an informative way without using a label denoting the aspect of the self-concept from which involvement derives.

Value-Relevant Involvement

The findings for value-relevant involvement are reasonably clear: Involvement of this type typically inhibits attitude change. The mean weighted effect size was -0.48 , which corresponds to a correlation of $-.23$, a moderately strong effect. However, the effect sizes aggregated into this mean were not homogeneous across the studies. Removal of 20% of the studies in this class via outlier-removal techniques resulted in a homogeneous set of effect sizes.²¹ When all of the value-relevant effect sizes were analyzed, message characteristics accounted for a portion (29%) of the variability in the effect sizes. Because these analyses showed that the tendency for involvement to inhibit persuasion was weakened by strong arguments, the resistance to persuasion conferred by value-relevant involvement can apparently be overcome to some extent by cogent argumentation. Yet, as Table 4 shows, even with strong arguments, value-relevant involvement inhibited persuasion.

The models predicting value-relevant effect sizes were not correctly specified, but this result is understandable in view of the many different methods investigators have used to operationalize this type of involvement. Although most of these studies classified subjects via their pretest judgments of the issue discussed in the persuasive message, the type of data that provided the basis for this classification differed greatly across the studies (e.g., widths of latitudes of rejection and acceptance, ratings of issue importance), as did the specific criteria used to divide the samples (e.g., median splits, extreme groups). These

differences in the ways that subjects were classified via their own responses were too varied and complex to take into account in the meta-analysis, given the relatively few studies that are available. Moreover, three studies presented subjects with issues that differed in level of involvement, and two (classified as *other methods*) used experimental manipulations that can be viewed as linking the issue to the high-involvement subjects' values. It would indeed be surprising if such disparate methods of varying involvement had a consistent impact on persuasion.

Although diversity in the methods of varying value-relevant involvement may pose difficulties when attempting to account for variability in the effect sizes, this variety can be considered an advantage in another respect. This advantage accrues when considering whether a contaminating variable that covaried

¹⁹ Although our findings greatly reduce the plausibility of explaining the divergent findings in terms of the argument strength manipulations, subjects in the original experiments might have perceived the arguments somewhat differently from the students who served as judges for our argument-rating task. Unfortunately, it was not possible to compare studies' argument-strength manipulations in terms of their manipulation-check findings. Although all investigators who reported checks on their argument-strength manipulations found them highly significant, the measures that they interpreted as checks were somewhat diverse (e.g., judgments of argument strength, various indices based on the valence of thoughts reported by subjects), precluding unambiguous comparisons across studies. Also, it was not possible to compare most studies in terms of the proportions of positive and negative thoughts elicited by the persuasive messages. These proportions are consequential for the elaboration likelihood model in view of Petty and Cacioppo's (1986a, p. 32) operational definition of strong messages as those that elicit predominantly positive thoughts and that of weak messages as those that elicit predominantly negative thoughts.

²⁰ For example, we examined whether the other researchers used issues about which subjects were more knowledgeable. However, we had coded 15 of the 20 outcome-relevant studies as using low-knowledgeability issues; among the 5 studies using higher-knowledgeability issues, 2 were conducted by the Ohio State researchers and 3 by the other researchers. Nonetheless, we caution readers that many classes of explanations for the instability of the effects of outcome-relevant involvement cannot be tested by our meta-analytic methods. For example, the Ohio State researchers may have had more impactful involvement manipulations, even though the manipulations were procedurally very similar. Unfortunately, many studies (40%) lacked an appropriate manipulation check on involvement, making it impossible for us to adequately test this possibility. Another possibility is that the Ohio State researchers may have avoided publishing weaker findings, whereas other researchers, who are presumably less committed to Petty and Cacioppo's predictions, may have been more than willing to report such findings. Finally, we remind readers that our meta-analysis is not concerned with testing the elaboration likelihood model but with exploring the utility of distinguishing three types of involvement. Had we wished to test the elaboration likelihood model, we would have included the effect sizes for the impact of argument strength within levels of involvement, the contrasts that Petty and Cacioppo (1986a, 1986b) have considered more crucial to their theory than the effects of involvement within levels of argument strength (see Footnote 25).

²¹ Two of these outliers (Aiello, 1967; Rhine & Severance, 1970), which obtained large effects in the predicted direction, manipulated involvement by the presentation messages on issues varying in level of involvement. Manipulations of this type are particularly vulnerable to confounding in terms of knowledgeability about the issues and possibly other factors (see discussion of confounding later in this section).

with involvement produced the resistance to persuasion documented by our meta-analysis. Although we removed from our sample all studies with clear-cut evidence of confounding, we suspect that involvement was sometimes correlated to some extent with variables that could be considered confounds (e.g., knowledgeability about issues, confidence in own attitudinal position, accessibility of counterarguments), especially in the studies that varied involvement by classifying subjects or varying the issue of the persuasive message. Yet, because this research used diverse methods of varying involvement, the exact nature of any confounding would have differed across the studies, rendering less plausible any argument that a single confound explains the effects of value-relevant involvement. Thus, a critic who desires to dismiss the effects of value-relevant involvement on the basis of presumed confounding must construct a series of special-purpose hypotheses for each of the several types of manipulations used in this research and furthermore argue that these confounds were the main determinant of persuasion rather than the value-relevant involvement postulated by the researchers who produced the studies. Although the confounding issue cannot be satisfactorily resolved without relevant primary research, we doubt that the persuasion-inhibiting effect of value-relevant involvement can be dismissed simply on the basis of presumed confounding.

Other predictors not included in our meta-analysis might have explained additional variance in the magnitude of the value-relevant effect sizes. In particular, when considering this type of involvement, theorists have sometimes taken into account the discrepancy between the message and subjects' positions (e.g., C. W. Sherif et al., 1965). We did not include this variable in this meta-analysis because preliminary analyses indicated that partitioning the findings according to investigators' labeling of their discrepancy conditions did not improve prediction of the effect sizes. Nonetheless, the implications of this result remain ambiguous because discrepancy may not have been manipulated with similar enough operations to allow meaningful comparisons across the studies.

Outcome-Relevant Involvement

As was expected, outcome-relevant involvement did not influence persuasion as a main effect but instead interacted with the strength of the argumentation contained in messages to facilitate persuasion with strong arguments and inhibit it with weak arguments. However, these trends were qualified by the fact that the effect sizes were not homogeneous within either the strong- or the weak-arguments class. The mean effect size for the strong-arguments class was 0.32, slightly smaller in magnitude than the mean effect size found for the value-relevant class. The removal of only one outlier (5%) produced homogeneity and a mean effect size of 0.42.²² Because our analysis revealed only one outlier, this class may be reasonably described as sharing a common mean effect size.

The mean effect size for the weak-arguments class was negative and somewhat smaller in magnitude, $d_+ = -0.26$. The removal of four outliers (24%) resulted in homogeneity and a slight decrease in the magnitude of the mean effect size, $d_+ = -0.21$.²³ Because homogeneity was not attained until a relatively large proportion of effect sizes were removed, this class of

effect sizes may be considered particularly unstable. The fact that researchers have not consistently found that outcome-relevant involvement inhibits the persuasion induced by weak argumentation suggests that the conditions necessary to produce this effect may be complex. In addition to experimental procedures, these conditions could include characteristics rarely described in experimental research on persuasion such as particular distributions of prior attitudes and other individual-difference variables (see Sorrentino et al., 1988), study characteristics that we were unable to code and control in our analyses.

Despite the instability of results found for weak-arguments conditions, we believe that the force of the evidence obtained from studies of outcome-relevant involvement provides substantial support for our contention that this type of involvement is distinct from value-relevant involvement. Specifically, this support derives from the tendency for outcome-relevant involvement to increase persuasion with strong arguments, an effect that was relatively consistent across the studies. Whereas outcome-relevant involvement *facilitates* persuasion with strong arguments, value-relevant involvement *inhibits* persuasion, regardless of argument strength.

Moreover, if other evidence regarding human information processing is made known, the idea that outcome-relevant involvement typically affects persuasion negatively with weak ar-

²² The Liberman (1988) study, which produced a reversal of the predicted positive effect, used an issue (the desirability of essay exams instead of multiple-choice exams) about which the subjects were knowledgeable on the basis of personal experience; perhaps subjects did not revise their attitudes, even when confronted by arguments that they acknowledged were strong, because personal experience outweighed any and all arguments about the general impact of exams on students.

²³ The Leippe and Elkin (1987) study, which produced an extremely large effect size in the predicted direction, was atypical in its use of a student as the source of the persuasive message. Other studies typically used a more prestigious source or at least ascribed the position advocated in the message to a prestigious source (e.g., the president of the university or a faculty advisory committee). Perhaps a student source was very easily compromised by specious arguments when the subjects processed these arguments carefully. In addition, Leippe and Elkin ran their subjects individually (i.e., one per session), whereas other researchers typically ran subjects in groups. Perhaps individual administration of the experimental materials made the manipulations more impactful. The Fredericks (1988) study, which produced a reversal of the predicted effect, was atypical in its projection of the persuasive message onto a movie screen from which the subjects read. Also, subjects were videotaped while they read the message. Petty et al.'s (1983) experiment, which produced a large effect size in the predicted direction, was among the three studies presenting advertisements on consumer products. The product (a disposable razor) differed from those used in the other studies in that many of the subjects probably took little interest in this product class (i.e., subjects who used electric razors or who did not shave). Perhaps close scrutiny of weak justifications for a product's quality more easily lowered subjects' evaluations when they had little reason to maintain their interest in the product. The nine-arguments condition of Petty and Cacioppo's (1984) experiment, which produced a large effect size in the predicted direction, was unusual in its use of a large number of arguments. These arguments may have increased persuasion with low involvement (because the large number of arguments functioned as a persuasion-inducing peripheral cue) and decreased it with high involvement (because the large amount of weak argumentation was processed via the central route).

Table 6
*Tests of Argument-Strength and Message-Length Continuous Models
 for Outcome-Relevant Involvement Studies*

Variable	Simple linear regression		Multiple regression A		Multiple regression B ^a	
	<i>b</i>	<i>b</i> *	<i>b</i>	<i>b</i> *	<i>b</i>	<i>b</i> *
Individual predictors						
Argument strength ^b	0.11*	.37	0.11*	.37		
Message length	0.00	.01	0.00	.00		
Research group ^c	-0.07	-.06				
Interaction term (Argument Strength × Research Group)						
Additive constant			0.04		0.34*	.74
Multiple <i>R</i>			.37			.66
<i>SE</i> of estimate			.49			.39
<i>Q_E</i> ^d			127.35*			83.31*

Note. Models are weighted least squares regressions calculated with weights equal to the reciprocal of the variance for each effect size. In each multiple regression model, the predictors were entered simultaneously; *b* = unstandardized regression coefficient; *b** = standardized regression coefficient. Effect sizes are positive for differences in the high-involvement direction and negative for differences in the low-involvement direction; *n* = 36.

^a Regression equation was $d = 0.091 - 0.042$ (Argument strength) $- 0.016$ (Research group) $+ 0.345$ (Group × Strength), where argument strength has been adjusted (see Footnote b); when this model was assessed without the adjustment, the equation was $0.435 - 0.042$ (Argument strength) $- 2.836$ (Research group) $+ 0.345$ (Group × Strength).

^b This term was represented as the study strength rating minus the mean strength rating for the outcome-relevant class, in order to remove extreme multicollinearity among the predictors in Multiple Regression B (see Cohen & Cohen, 1983).

^c 1 = Ohio State University researchers; 0 = other researchers.

^d Significance indicates model not correctly specified.

* $p < .001$.

guments may be viewed as somewhat unlikely. Thus, if outcome-relevant involvement increases recipients' motivation to process information about the issue discussed in a message, with strong arguments the thorough processing that results may

indeed provide recipients with sufficient justification for revising their stand on the issue. In contrast, weak arguments may provide a less definitive guide for involved recipients' attitudinal positions, especially in view of people's demonstrated

Table 7
*Tests of Research-Group Categorical Model for Outcome-Relevant Involvement
 Studies for Each Condition of Argument Strength*

Variable and class	Between-classes effect (<i>Q_B</i>)	<i>n</i>	Mean weighted effect size (<i>d_{i+}</i>)	95% CI for <i>d_{i+}</i>		Homogeneity within each class (<i>Q_{wi}</i>) ^a
				Lower	Upper	
Strong arguments						
Research group	12.46**					
Ohio State		10	0.56	0.38	0.73	10.06
Other		9	0.14	-0.02	0.29	15.96*
Weak arguments						
Research group	21.38**					
Ohio State		11	-0.58	-0.76	-0.39	32.68**
Other		6	0.01	-0.16	0.18	9.48

Note. Effect sizes are positive for differences in the high-involvement direction and negative for differences in the low-involvement direction. CI = confidence interval.

^a Significance indicates rejection of the hypothesis of homogeneity.

* $p < .05$. ** $p < .001$.

difficulty in processing negative information (e.g., Newman, Wolff, & Hearst, 1980; Nisbett & Ross, 1980). This reasoning implies that weak arguments are analogous to the disconfirmations of hypotheses that have been shown to be difficult to process. This analogy is supported by the tendency for weak arguments to be more poorly recalled than strong arguments, a finding sometimes reported in persuasion studies (e.g., Homer, 1987; Petty, Cacioppo, & Goldman, 1981). Therefore, if involved recipients' thorough processing of weak argumentation provides an insufficient basis for revising their position on the issue, they may reserve judgment and react no differently than the less-involved recipients, who presumably processed the arguments less thoroughly. Involved recipients may await or seek more definitive information (see Chaiken, Liberman, & Eagly's [in press] discussion of the sufficiency principle). This logic would predict little or no effect of involvement with weak argumentation and a positive effect with strong argumentation.

Another limitation of outcome-relevant involvement studies is a lack of variety in manipulations of this variable. In contrast to the array of methods for varying involvement in the value-relevant studies, outcome-relevant involvement has been varied only by making salient to high-involvement subjects (but not to low-involvement subjects) that their ability to attain their personal goals might be affected if the policy change advocated in the message were instituted. In most of the studies, the message advocated a change in university policy, which was said to take effect at the subject's own (vs. a distant) university or take effect soon (vs. in the remote future). In the studies in which the persuasive message was an advertisement (Homer, 1987; Petty & Cacioppo, 1981b, Study 2; Petty et al., 1983), the advertised product was described as soon available in the subjects' city (vs. elsewhere), and in two of these studies, subjects also could (vs. could not) acquire the product through their participation in the experiment. The fact that persuasion researchers have devised so few methods for linking subjects' attitudes to their current goals or outcomes raises questions about how confidently we should claim that outcome-relevant involvement would generally have the impact that has been observed in the available studies.

Another possible restriction of the generalizability of findings on outcome-relevant involvement stems from the use of college issues in 16 of the 20 studies and the use of a single college issue, comprehensive exams for college students, in 11 of these 16 studies. Three of the remaining studies presented advertisements about consumer products, and only one study used a social policy issue (Axsom et al., 1987). Because of this focus on college issues, it remains an open question whether outcome-relevant involvement would have similar impact for issues in other domains.²⁴

Yet another limitation to the generalizability of our findings for outcome-relevant studies stems from the fact that involvement was often crossed with another variable (e.g., a communicator characteristic) in addition to the quality or number of the arguments in the messages. By aggregating the data over these other variables, we reduced some of the variability potentially present in the effect sizes. Although our strategy is defensible given the relatively small number of studies available and the variety of other variables we encountered, future reviewers

might be able to explore more fully how involvement may interact with contextual features of persuasion settings.

Impression-Relevant Involvement

Subjects whose involvement stemmed from the anticipation of public scrutiny of their positions were expected to maintain relatively neutral and defensible positions, even in response to especially strong or weak argumentation. The weak tendency for subjects who expected such scrutiny to be less persuaded than subjects who had no such expectation may reflect this desire to maintain a neutral, nonpolarized position. More compelling is our finding that this slight resistance to persuasion was not weakened by strong arguments. Indeed, it is this finding that differentiates impression-relevant involvement from outcome-relevant involvement. Yet because the impression-relevant class consisted of only five studies, conclusions about this class are especially tentative. However, confidence that these two types of involvement affect persuasion somewhat differently is greater than it would ordinarily be on the basis of five studies because one of these studies, Leippe and Elkin (1987), manipulated both outcome-relevant and impression-relevant involvement in the same experiment and obtained evidence of the differing effects they (and we) postulated. Yet Neises's (1988) experiment, which also manipulated these two types of involvement, obtained less distinct effects.

Another reason to be cautious about predicting the effects of impression-relevant involvement is that research on anticipatory attitude change (see review by Cialdini & Petty, 1981) suggests that manipulations that are superficially similar to those that we have labeled impression-relevant involvement may produce quite different effects on attitudes. For example, Cialdini et al. (1973) showed that merely expecting to listen to someone give an opinion is not sufficient to produce attitudinal moderation, which we argued may underlie the tendency for involved subjects not to react differentially to weak and strong arguments. Also, the research of Cialdini and his colleagues as well as other investigators (e.g., Hass, 1975) suggests that letting people know the position of a discussion partner or communicator to some extent produces anticipatory conformity to that position rather than moderation. Furthermore, as Cialdini et al. (1976) demonstrated, attitudinal moderation does not occur in response to an anticipated discussion when the issue itself elicits other types of involvement or when the anticipated discussion is not expected to occur relatively soon. Most importantly, these same studies have shown that canceling an anticipated discussion or communication causes attitudes to "snap back" to ap-

²⁴ Another concern in evaluating manipulations of outcome-relevant involvement is whether these manipulations may have confounded involvement with subjects' premessage attitudes. Thus, because of self-interest considerations, subjects who learned that a counterattitudinal policy change might be introduced at their own university may well have formed more negative attitudes toward the policy change than subjects who learned that this same policy change might be introduced at another university. Although the hypothesis that the involvement manipulation affects premessage attitudes was examined and not confirmed in one study (Petty, Cacioppo, & Goldman, 1981), this test was based on only 18 subjects and thus had low power to reject the null hypothesis.

proximately their prior level, suggesting that anticipatory shifts represent self-presentational accommodation to situational pressures rather than internalized attitude change. In fact, Neises's (1988) finding in his "temporary response involvement" condition that the tendency for impression-relevant involvement to decrease persuasion was nullified by the cancellation of the anticipated discussion suggests that this type of involvement may have only a superficial effect on persuasion.

In view of the complexity of findings in this related literature on anticipatory attitude change, we maintain that having subjects think that their attitudes will be under scrutiny can arouse a variety of motives that have differing kinds of impact on the attitudes they present to an evaluative audience. Nonetheless, only one variant of this family of manipulations has been featured in the impression-relevant studies included in this meta-analysis, namely, the anticipation of revealing one's attitude on a topic low in value-relevant involvement to an evaluative audience whose attitudes are unknown. We assert that these particular circumstances cause subjects to be primarily concerned about the impression this attitude will make on the audience.

Confounding Between Study Attributes and the Three Types of Involvement

Because the experiments on value-relevant and outcome-relevant involvement were carried out within different theoretical frameworks and were intended to produce quite different attitudinal effects, it is not surprising that they are methodologically somewhat different. Yet the resulting confounding of type of involvement with attributes of the studies is a barrier to unambiguous interpretation of our meta-analytic findings. Of particular concern is our demonstration (see Table 1) that subjects were more knowledgeable on issues used in value-relevant studies than they were on the issues used in outcome-relevant studies and the correlated finding that the value-relevant studies used social policy issues, whereas the outcome-relevant studies typically used college issues. Because of the correlational nature of meta-analysis, we cannot resolve whether the differing effects on persuasion demonstrated by this meta-analysis result from the type of involvement activated or from differences in knowledgeability and type of issue. We suggest that this matter be addressed in primary research.

The correlation in our meta-analytic data between type of involvement and the two study characteristics of knowledgeability and type of issue may reflect a confound that exists in natural settings. People's values are ordinarily at stake only when they are knowledgeable about an issue and often when the issue pertains to a current social policy debate on which important reference groups have taken stands. Consequently, researchers working in the social judgment-involvement tradition may well have considered correlations of involvement with knowledgeability (and with other variables) as part of the phenomenon rather than as an undesirable confound. In contrast, investigators of outcome-relevant involvement have maintained a more strictly motivational definition of involvement and have regarded correlations of involvement with cognitive variables such as knowledgeability as an undesirable confound. The issues chosen by these investigators seem carefully selected to avoid activation of subjects' values, a tactic that wisely pre-

vents arousing value-relevant involvement in studies oriented to understanding outcome-relevant involvement. However, outcome-relevant involvement may well not have the effects shown in this meta-analysis when subjects are knowledgeable about issues or when these issues link to their reference groups or values. We suspect that the outcome-relevant effects that investigators have obtained would be overwhelmed by the attitude-defensive processes that are elicited by persuasive communications that impinge on people's core values (see next section). Therefore, the effects of outcome-relevant involvement (and impression-relevant involvement as well) may essentially be limited to situations in which attitudes are formed rather than changed. Under such circumstances, subjects may in fact have to indicate their attitudes on a premessage questionnaire but in fact have a prior attitude only to a minimal extent, a psychological state that Converse (1970) has termed a *nonattitude*.

Psychological Processes Mediating Effects of Involvement

Our meta-analytic findings are not directly informative concerning the psychological processes underlying subjects' attitudinal responses. Nevertheless, the findings are worthy of discussion in terms of process issues, in the context of the differing ways that the authors of the studies have dealt with process. Not surprisingly, the authors of the value-relevant and outcome-relevant studies discussed the processes that mediate attitude change in very different terms because they conducted these studies in different periods and under the influence of different theoretical frameworks. In the social judgment-involvement approach, recipients' perception of the communicator's position was presumed to mediate persuasion (see C. W. Sherif et al., 1965; M. Sherif & Hovland, 1961). Recipients were assumed to displace such positions perceptually by assimilating positions that were relatively similar to their own attitude and by contrasting positions that were relatively different. In addition, message recipients were presumed to interpret message content in a biased fashion, by judging messages that were similar to their own attitude as fair and objective and by judging messages that were different from their own attitude as unfair and propagandistic. Although assimilation and contrast as well as biased evaluation of message content could lessen pressure to change toward messages, the exact relation between these reactions and attitude change remained somewhat ambiguous in social judgment-involvement theory (see Kiesler, Collins, & Miller, 1969), and few of the value-relevant studies in our sample assessed these reactions or examined their potential mediational role in relation to persuasion.

The experiments on outcome-relevant involvement, conducted more recently mainly within the cognitive response and elaboration likelihood frameworks, usually assessed the cognitive responses that are presumed to mediate persuasion. Indeed, several studies demonstrated that subjects' cognitive responses (i.e., their issue- and message-relevant thoughts) were more favorable with strong than with weak argumentation and that the tendency to think favorably with strong arguments and unfavorably with weak arguments was enhanced by outcome-relevant involvement (e.g., Leippe & Elkin, 1987; Petty & Cacioppo, 1979b, 1984). These trends suggest that with outcome-relevant

involvement, information processing is relatively unbiased and open minded: The cogency of strong arguments and the flaws of weak arguments may become more apparent as message recipients devote more cognitive resources to processing message content.

Because subjects in the value-relevant studies were less persuaded to the extent that they were involved (even when they received very strong arguments), they must have engaged in relatively closed-minded processing that enabled them to defend their initial attitudes. Yet, consistent with Petty and Cacioppo's (1986a, 1986b) speculation that high-quality argumentation may limit this tendency to think about the message in a biased manner, our meta-analysis found that value-relevant involvement's negative effect on persuasion weakened as the strength of argumentation increased. Nonetheless, it is important for persuasion researchers to clarify the nature of those processes that message recipients deploy primarily to defend their initial attitudes. Although little attention has been given to the details of biased processing of this sort, an increase in interest can be detected in some recent discussions (see Chaiken et al., in press; Petty & Cacioppo, 1986a, 1986b).²⁵

Relatively little is known about the cognitive processing underlying recipients' attitudinal responses when they are involved on an impression-relevant basis. Yet, to probe these mediational issues, Leippe and Elkin (1987) examined message recipients' thoughts and their private behavior (i.e., responses to a voluntary essay-writing task) related to their attitudes. Findings were complex and suggested that the effects of impression-relevant involvement on attitudes may have been mediated by (a) compliance with self-presentational demands, (b) cognitive responses biased toward moderation and possibly toward gaining information rather than evaluating message validity, or both (see also Neises's, 1988, and Shechter's, 1987/1988, discussions of these issues).

Related Analyses

In providing an account of some of the motivations underlying attitude change, our analysis of the involvement construct in persuasion research helps fill a void that has developed in contemporary treatments of persuasion, which have emphasized cognition considerably more than motivation (see Eagly & Chaiken, 1984). To the extent that there is an assumption about motivation in modern persuasion theories, this assumption appears to be that recipients are motivated to process information in a relatively unbiased way to attain valid opinions that are in line with the relevant facts. However, in very recent years, more recognition that recipients are often otherwise motivated has begun to emerge once again in discussions of attitude change. In addition to the possibility of biased processing that follows from recipients' desire to maintain their existing attitudes (Chaiken et al., in press; Petty & Cacioppo, 1986a, 1986b), various attitude researchers have explored attitudinal constructs (e.g., conviction, centrality, importance) with motivational implications (see Abelson, 1988; Judd & Krosnick, 1982; Krosnick, 1988; Raden, 1985).

Exploring the motivational sources of persuasion and other social behaviors in some detail, A. G. Greenwald (1982) provided an analysis of the ego-involvement construct that resem-

bles our own analysis to some extent. Greenwald noted three major meanings that social psychologists have ascribed to involvement, namely, concern about evaluation by others, concern about self-evaluation, and concern about maintaining one's values. This analysis is much broader than our own because these types were intended to represent social psychologists' uses of involvement and a variety of other constructs (e.g., evaluation apprehension, dissonance arousal, public and private self-consciousness). Still, application of Greenwald's analysis to the specifics of research on persuasion suggests that his concepts of concern about value maintenance and concern about evaluation by others encompass the operational definitions of involvement that we have labeled *value relevant* and *impression relevant*, respectively. Greenwald's third type, concern about self-evaluation, has not been examined as a form of involvement in persuasion research. Finally, although outcome-relevant involvement, the type of involvement most common in persuasion research of the 1980s, did not appear in Greenwald's analysis, he presented his framework as an open-ended system compatible with the addition of other types.

Our analysis in terms of value-relevant, outcome-relevant, and impression-relevant involvement is also related to the functional analyses of attitudes proposed by Katz (1960) and Smith, Bruner, and White (1956). According to these functional theorists, attitudes serve various functions in the personality and thus have different motivational bases. Because involvement is a motivational construct in attitude-change research, the match to these constructs is reasonably exact. Thus, our construct of value-relevant involvement corresponds to Katz's value-expressive function, which recognizes that people are motivated to maintain their values. Our construct of outcome-relevant involvement corresponds to Katz's instrumental or utilitarian function, which recognizes that people are motivated to attain goals they regard as rewarding. Our construct of impression-relevant involvement corresponds most closely to Smith et al.'s social-adjustive function, which recognizes that people are motivated to maintain positive relationships with other people. Showing the renewed importance that such functional concepts have gained in recent research on attitudes, both Herek (1986) and Prentice (1987) emphasized that attitudes have both instrumental functions, by which they directly express benefits and

²⁵ We acknowledge that the elaboration likelihood model's biased processing postulate (e.g., Petty & Cacioppo, 1986b, p. 163) provides a potential means for reducing value-relevant and outcome-relevant involvement to a single construct, if the effects of value-relevant involvement that we have documented are ascribed to biased processing stemming from knowledge, prior attitudes, and other extraneous variables. Indeed, other ways of combining our three types of involvement have been suggested to us. Yet we prefer to maintain our distinction between three qualitatively different types of involvement because we have located three distinct bodies of research that reflect three different ways that researchers have thought about involvement. Combinations of two or more of these research traditions in terms of a single construct are entirely premature because they would require accepting untested assumptions about underlying processes. Although we favor the idea that the three clusters of studies produced different persuasion findings because they operationalized qualitatively different types of involvement, our views remain provisional and could be modified by relevant primary research.

costs, and expressive or symbolic functions, by which they express personal values and core aspects of self-identity. These concepts reflect the earlier functional distinctions of Katz and of Smith et al. and are in harmony with our distinction between outcome-relevant and value-relevant involvement.

Both A. G. Greenwald's (1982) ego-task analysis and the earlier functional analyses of Katz (1960) and Smith et al. (1956) represent efforts to develop motivational constructs adequate for representing the variety of motives that commonly underlie social behavior in general and reactions in social influence settings more specifically. As we have shown for persuasion research, these motivational distinctions are essential for understanding the differing ways in which persuasive communications affect attitudes.

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(Appendix follows on next page)

Appendix

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