

Chapter 12

Conclusion

In the first part of this book we distinguished four major classes of variables: beliefs, attitudes, intentions, and behaviors. In contrast to most contemporary theory and research in the attitudes area, we have argued that these distinctions are an essential prerequisite for a systematic analysis of research findings in this area. In Chapters 3 and 4 we saw that it is possible to operationally distinguish between beliefs, attitudes, intentions, and behaviors, and to obtain reliable and valid measures of each of these constructs. We have also tried to show that the four major classes of variables are systematically related to one another. Toward the end of Chapter 1 we outlined a conceptual framework dealing with these interrelations. An analysis of contemporary theories of attitude formation and change in Chapter 2 showed that most of these theories are concerned primarily with the relations among beliefs and with the relations between beliefs and attitudes. At least at the conceptual level, relatively little attention has been paid to intentions and overt behavior.

Part II considered the determinants of beliefs, attitudes, intentions, and behaviors. We began our discussion in Chapter 5 with a consideration of processes underlying belief formation. Some beliefs are formed on the basis of direct observation, but most beliefs involve some inference on the part of the individual. This implies that most beliefs are based on or influenced by prior information available to the individual. Empirical evidence suggests that these inference processes reflect probabilistic rather than evaluative relationships among the beliefs a person holds. His inferences appear to be based on rational considerations of these relationships and can be described quite adequately by formal probability models. For example, some inferential beliefs seem to be formed in accordance with laws of syllogistic reasoning, and others can be described by Bayes's theorem. The common denominator of these models is their suggestion that there are systematic relations among beliefs and that formation of new inferential beliefs is

determined by the nature of these relationships. We reviewed studies of trait inferences, cue utilization, syllogistic reasoning, and formal probability models, which provided considerable evidence for the existence of such systematic and relatively stable relations among beliefs. In this context we showed that causal attributions to self and others are consistent with a Bayesian model of human inference processes.

Beliefs are the basic building blocks in our conceptual framework. In Chapter 6 we discussed the influence of a person's beliefs on his attitudes. A review of empirical research supported our conceptual framework by showing that a person's attitude toward any object, issue, behavior, or event is determined by his salient beliefs linking the object to various attributes and by his evaluations of those attributes. An expectancy-value formulation was found to account for research findings in such diverse areas as impression formation, interpersonal attraction, and classical conditioning of attitude. Also consistent with our general conceptual framework, a person's attitude was found to be related to the totality of his beliefs but not necessarily to any particular belief he holds. Similarly, in Chapter 7 we saw that a person's attitude is related to the totality of his intentions (weighted for their evaluative implications) but not necessarily to any given intention. We discussed a theoretical model for the prediction of intentions and presented evidence in support of this model. We found that intention to perform a given behavior is related to particular kinds of attitudes and beliefs, namely, attitudes toward the behavior and subjective norms concerning performance of the behavior.

Chapter 8 showed that when appropriately assessed (and barring unforeseen events), intentions serve as the primary determinants of overt behavior. Again we showed that although a person's attitude toward an object is related to the totality of his behaviors with respect to the object (a multiple-act criterion), it is not necessarily related to any given behavior (a single-act or repeated-observation criterion). This conclusion is in contrast to the recently emerging view that attitudes are unrelated to behavior. Our analysis suggests that the strength of this relation depends on the appropriateness of the attitudinal predictor to the behavioral criterion.

Part II demonstrated a sequence of systematic relations linking beliefs to attitudes, attitudes to intentions, and intentions to behaviors. We can now complete the cycle by realizing that in the final analysis a person can form new beliefs only by performing some behavior. To gain new information, he may read books, observe events, interact with other people, watch television, etc., and these activities provide the basis for the formation of descriptive and inferential beliefs. In a social interaction situation, for example, a person's behavior (and that of the other people involved) leads to the formation of beliefs about the attributes of objects in the environment, about the characteristics and reactions of other people, and about the consequences of the behavior. These beliefs provide the basis for the formation of attitudes toward the objects in the situation, toward the other people, and toward the behavior. In addition, they may also lead to the formation of normative beliefs concerning the behavior. Attitude toward the behavior and sub-

jective norms in turn determine the person's intention to perform the behavior in the future, and this intention leads to performance or nonperformance of the behavior.

Part III was concerned with the ways in which beliefs, attitudes, intentions, and behaviors can be changed. Consistent with the discussion above, Chapter 9 showed that changes in these variables are initiated by exposing a person to information which produces changes in some of his beliefs. We reviewed research demonstrating that changing beliefs can produce changes in other beliefs as well as changes in attitudes, that changes in attitude toward a behavior or in subjective norms can lead to changes in intentions, and that changes in intentions can produce behavioral change.

Chapters 10 and 11 dealt with the two major strategies of change: active participation and persuasive communication. In each strategy, subjects are exposed to certain items of information which may produce changes in corresponding proximal beliefs and may also have an impact on external beliefs. The success of the influence attempt depends on the relation between these beliefs and the dependent variable. In Chapter 10 we showed how these considerations further our understanding of the diverse findings obtained in studies of interpersonal contact, role playing, counterattitudinal behavior, and decisions between alternatives. Chapter 11 presented a model for the acceptance of informational items provided by an outside source and change in corresponding proximal beliefs. According to the model, probability of acceptance decreases with discrepancy between source and proximal belief and increases with overall facilitation. Change in the advocated direction is predicted to be a function of probability of acceptance and potential change. Along with our general principles of change, this model allowed a systematic analysis of research on persuasive communication. A review of research on the effects of discrepancy, communicator credibility, and fear appeals showed that the results were generally consistent with the model. In contrast to the traditional approach to communication and persuasion, our analysis emphasizes the importance of distinguishing between beliefs, attitudes, intentions, and behaviors. Further, it suggests that experimental manipulations cannot be expected to have systematic effects on any one of these dependent variables. To understand the effects of experimental manipulations, it is necessary to examine their effects on proximal and external beliefs and to consider the relations of these beliefs to the dependent variable.

RESEARCH TRADITIONS IN THE ATTITUDE AREA

Much of the research on processes of change discussed in Chapters 10 and 11 can be classified as falling into two major research traditions, one associated with the Yale program of research on communication and persuasion under the direction of Carl Hovland, the other with Leon Festinger and his theory of cognitive dissonance. The explicit goal of the *Yale approach* was to identify the factors that influence persuasion. Its general research strategy was to manipulate independent variables that would influence the amount of "attitude" change. Since much of

the research under the Yale approach has dealt with the effects of persuasive communication, the independent variables have included source credibility, type of appeal, order of arguments in the message, etc. Manipulations of these factors were assumed to influence amount of change to the extent that they affected two major intervening variables: reception (attention and comprehension) and acceptance or yielding. Thus the Yale approach was interested primarily in studying the ways in which "attitude" change can be brought about, and intervening processes were postulated in an attempt to understand the persuasion process.

In contrast, the *dissonance approach* concentrated not on an empirical phenomenon but rather on a hypothetical construct (dissonance) and proposed a broad theoretical hypothesis or principle, which can be stated as follows: Any treatment variable that produces dissonance between two cognitions will lead to attempts to reduce the dissonance. Much of the research has been conducted in order to support this general principle by looking at treatment variables that might produce dissonance and at dependent variables that might reflect the subject's attempt to reduce his dissonance. There was little interest in the effects of any particular independent variable on any specific dependent variable; that is, investigators were free to select any independent and dependent variables that allowed a test of the dissonance principle. Dissonance theory has been viewed as a theory dealing with attitude change, primarily because one class of dependent measures that are expected to reflect dissonance reduction are measures of attitudes or opinions.

Thus the Yale approach can be distinguished from the dissonance theory approach in terms of the phenomena that serve as the focus of research. Whereas the Yale program was interested in attitude change per se, research within the dissonance framework has studied attitude change primarily in an attempt to test the dissonance principle. One important implication of this difference in focus is that repeated failure to confirm a given experimental hypothesis in the Yale approach serves merely as evidence against the hypothesis and does not necessitate a change in research focus. In contrast, repeated failure to confirm an experimental hypothesis derived from the dissonance model raises doubt about the dissonance principle itself.¹

EVALUATION OF ATTITUDE RESEARCH

At this stage it may be worth reflecting on the relevance of this research and examining the degree to which it has produced a cumulative, systematically integrated body of knowledge. According to Katz (1972),

Of the thousands of experimental studies published in social psychology in the past 20 years, the number that supplies new information to a cumulative body of knowledge is surprisingly small. (p. 557)

1. This should not be taken as criticism of theory-based research; in such research, disconfirmation of a prediction may result merely in a modification of the theory. Research on dissonance "theory," however, has been concerned primarily with a single general principle.

An outside view is provided by the German psychologist Klaus Holzkamp (1970), who stated,

When one considers the contents of experimental psychological research, rather than its methodology it can be argued that from a broader perspective, the kinds of questions being asked are more or less meaningless or trivial.²

Similar views of social-psychological research have been expressed by several American psychologists. For example, Lackenmeyer (1970) not only agreed with “the argument that many . . . social psychologists are investigating minutia and irrelevant problems” (p. 621); he also pointed out that the majority of this research is “used to verify isolated hypotheses: hypotheses not deduced from some general, deductive theory” (p. 620). In order to evaluate these criticisms, it is necessary to take a closer look at the research that has been conducted in the attitude area and, particularly, the research generated by the two major research traditions.

Dissonance approach. Although the dissonance approach started out with a general theoretical orientation, much of the research it has generated has tested hypotheses that have little theoretical significance. First, recall that the main focus of dissonance research is not a given empirical phenomenon but rather a single general dissonance principle.

Instead of exploring the antecedent conditions that affected some standardized, reliable, and socially important dependent variable, experimenters in the Festinger tradition typically asked simple, direct, tailor-made questions . . . [which] were almost a casual afterthought. (Jones and Sigall, 1971, p. 350)

Initial studies attempted to test hypotheses derived from dissonance theory by employing some manipulation that was expected to produce different degrees of dissonance and then using the “tailor-made” questions to assess attempts at dissonance reduction. It soon became apparent that it was difficult to consistently demonstrate the operation of the dissonance principle, and studies with conflicting findings began to accumulate. Since most investigators were convinced of the validity of the dissonance principle, negative findings led not to the rejection of the hypothesis but rather to criticisms of the experimental procedures (see McGuire, 1973). Indeed, Aronson and Carlsmith (1968, p. 79) argued that “an experimenter can usually see weaknesses in his experiment; by making a few changes he can frequently strengthen his procedure and increase the probability of confirming a *true hypothesis*” (italics ours).

Studies disconfirming the dissonance hypothesis were carefully examined to discover procedural weaknesses in the manipulation of independent variables. Although research in the dissonance theory tradition has frequently been credited with “lavish[ing] great care on the independent variables being manipulated” (Jones and Sigall, 1971, p. 349; see also McGuire, 1969), it is apparent that the

2. Translated from the German article (Holzkamp, 1970, p. 6).

concern was really for the mechanics of manipulation rather than for the variable being manipulated. Perhaps it is characteristic of this state of affairs that Aronson and Carlsmith's (1968) "Experimentation in Social Psychology" places heavy emphasis on ways of deceiving and manipulating subjects. In that chapter we are told that some of the essential ingredients of a social-psychological experiment are of an "artistic, intuitive, ephemeral nature" (p. 1), and that "part of being a good experimental social psychologist involves learning to say 'whoops' convincingly" (p. 45). We are further told that "it is extremely important for all subjects to be brought to the identical point by the manipulation of the independent variable" (p. 46), even if different procedures are necessary to accomplish this.

Unfortunately, this approach to experimentation is quite characteristic of research in the dissonance tradition. It implies that instead of being a scientific undertaking, the social-psychological experiment is an artistic endeavor comparable to staging a theatrical drama. Like that of a director in the theater, the investigator's role is seen as one of moving and manipulating actors, settings, and props until he is intuitively satisfied with the effects of these manipulations. "Heavy attention is paid to dramatizing the experiment. This correctly reflects the imagination and ingenuity of the members of this school who seem at times to be frustrated playwrights." (Katz, 1971, p. 277). Needless to say, this approach is not only unscientific; it is actually detrimental to scientific progress since it violates one of the cardinal rules of scientific research, namely, that procedures used by one investigator be replicable by other investigators. Clearly, this can be achieved only if the manipulations employed are objective and communicable rather than "artistic, intuitive, and ephemeral."

This emphasis on the *mechanics* of independent variable manipulations has come at the expense of paying attention to what is being manipulated. Instead of studying the extent to which dissonance theory could account for some interesting phenomenon, investigators shifted the central focus of dissonance research to the effects of frequently minute and trivial variations in some specific manipulation. Thus, instead of being oriented toward furthering our understanding of some phenomenon, much dissonance research began to study quasi-methodological issues concerning the manipulation of some specific variable whose theoretical and practical significance did not go beyond the confines of a particular experimental situation.

Meehl (1967, p. 114) has characterized this state of affairs and its implications as follows:

... there exists among psychologists a fairly widespread tendency to report experimental findings with a liberal use of *ad hoc* explanations for those that didn't "pan out." This methodological sin is especially tempting in the "soft" fields of (personality and social) psychology, where the profession highly rewards a kind of "cuteness" or "cleverness" in experimental design, such as a hitherto untried method for inducing a desired emotional state, or a particularly "subtle" gimmick for detecting its influence upon behavioral output. The methodological price paid for this highly-valued "cuteness" is, of course, an unusual ease of escape

from [accepting negative findings and refuting the theory]. For, the logical structure of the "cute" component typically involves use of complex and rather dubious auxiliary assumptions, which are required to mediate the original prediction and are therefore readily available as (genuinely) plausible "outs" when the prediction fails. It is not unusual that this *ad hoc* challenging of auxiliary hypotheses is repeated in the course of a series of related experiments, in which the auxiliary hypothesis involved in Experiment 1 . . . becomes the focus of interest in Experiment 2, which in turn utilizes further plausible but easily challenged auxiliary hypotheses, and so forth. In this fashion a zealous and clever investigator can slowly wend his way through a tenuous nomological network, performing a long series of related experiments which appear to the uncritical reader as a fine example of "an integrated research program," *without ever once refuting or corroborating so much as a single strand of the network*. Meanwhile our eager-beaver researcher, undismayed by logic-of-science considerations and relying blissfully on the "exactitude" of modern statistical hypothesis-testing, has produced a long publication list and been promoted to a full professorship. In terms of his contribution to the enduring body of psychological knowledge, he has done hardly anything. His true position is that of a potent-but-sterile intellectual rake, who leaves in his merry path a long train of ravished maidens but no viable scientific offspring.³

Clearly, if attitude research is to reach the goal of providing a cumulative and systematically integrated body of knowledge concerning attitude phenomena, research must be more than quasi-methodological treatments of pseudo-problems created by conceptual ambiguities or procedural weaknesses. Another characteristic feature of research in the dissonance tradition has been its encouragement of counterintuitive or unexpected hypotheses. Despite our criticism of the intuitive approach, we are convinced that intuition and common sense play an important role in the formulation of hypotheses and the development of a theory. In fact, many commonsense hypotheses have found considerable support. For example, the notions that similarity leads to attraction and that a person's information about an object determines his attitude toward that object are clearly based on commonsense observations.

Our criticism is directed not so much at investigators who rely on common sense as at a particular use of intuition to *defeat* common-sense notions. For some

3. "Lest the reader wonder (quite appropriately) whether these impressions of the psychological literature ought perhaps to be dismissed as mere 'sour grapes' from an embittered, low-publication psychologist *manqué*, it may be stated that the author (a past president of the American Psychological Association) has published over 70 technical books or articles in both 'hard' and 'soft' fields of psychology, is a recipient of the Association's Distinguished Scientific Contributor Award, also of the Distinguished Contributor Award of the Division of Clinical Psychology, has been elected to Fellowship in the American Academy of Arts and Sciences, and is actively engaged in both theoretical and empirical research at the present time. He's not mad at anybody—but he is a bit distressed at the state of psychology" (Meehl, 1967, p. 114).

reason, many investigators seem to place a premium on being able to demonstrate the nonobvious. They select some reasonably well-established relationship (which is consistent with common sense) and then attempt to find some situation in which the relationship will not hold. It is almost as though an investigator says to himself, "Everybody thinks that if a person performs a commendable deed, liking for the person will increase. Wouldn't it be interesting if I could figure out some way to show that performing a commendable deed can actually *reduce* attraction?" The investigator then proceeds to search for some unique circumstances under which his counterintuitive hypothesis might be supported. At the same time, he speculates about possible intervening processes that can explain the counterintuitive phenomenon if it is substantiated. The theoretical relevance of the phenomenon and of the proposed intervening processes is of minor concern. Even if the investigator's intuition is supported, the results for the most part will be trivial, albeit unexpected. This should not be taken to mean that it is unimportant to identify the limits of a given phenomenon. What we are criticizing is the exclusive search for unexpected counterintuitive limitations of phenomena, limitations that can be demonstrated only under very special circumstances. As we have noted, most of these counterintuitive hypotheses have found little empirical support and have merely generated a wealth of inconsistent and inconclusive results.

It is unfortunate not only that research based on counterintuitive hypotheses has become acceptable but, as Meehl (1967) and Lackemeyer (1970) have pointed out, that a premium is increasingly placed on the "cuteness" of experimental hypotheses and the sophistication and ingenuity of experimental procedures at the expense of theoretical and empirical relevance. This trend is illustrated by a passage from Aronson and Carlsmith's (1968) chapter on experimentation in social psychology. In discussing sources for hypotheses they state,

Where the idea comes from is not terribly important. Indeed, ideas—even interesting ones—are cheap in social psychology. The important and difficult feat involves translating a conceptual notion into a tight, workable, credible, meaningful set of experimental operations." (p. 37)

To summarize briefly, there appear to be at least two reasons for the failure of the dissonance approach to generate a cumulative body of knowledge. First, in response to negative findings, much dissonance research has been distracted by tangential methodological issues that are of little relevance to the dissonance hypothesis. Second, the dissonance approach encouraged investigators to formulate counterintuitive and isolated hypotheses that do not contribute to a cumulative body of knowledge. It is unfortunate that this approach to experimentation appears to have been adopted by many investigators in the attitude area. For example, much of the currently fashionable research on interpersonal attraction (see Chapter 6) is characterized by "cute" intuitive hypotheses, inappropriate concern with methodological details, and a lack of theoretical perspective.

It has sometimes been suggested that the tendency to study relatively trivial issues and methodological details might be overcome by investigating socially relevant problems in field settings (McGuire, 1967; Katz, 1972). As noted in Chapter 4, however, there is no necessary relation between the way in which research is conducted and the setting in which these hypotheses are tested. It is just as easy to arrive at trivial ideas about socially relevant problems as it is to develop counterintuitive notions in laboratory settings. Indeed, a review of the literature shows that current field research is hardly more promising or fruitful than laboratory investigations. In a recent paper, McGuire (1973) reversed his position on the advantages of field research: "Recently I have come to recognize that this flight from the laboratory manipulative experiment to the field study, which I myself helped to instigate, is a tactical evasion which fails to meet the basic problem" (p. 449).

Yale approach. Research in the Yale tradition can be criticized neither for an excessive concern with pseudo-methodological problems nor for the testing of isolated, counterintuitive, or trivial hypotheses. The Yale approach has dealt for the most part with persuasion, an issue of considerable social relevance. Most hypotheses have relied on commonsense notions about the effects of source, message, and audience factors on the amount of "attitude" change produced by a persuasive communication. Nevertheless, this approach has also failed to produce a cumulative body of knowledge. The large volume of research generated by the Yale approach has not been able to demonstrate consistent effects of independent variable manipulations on persuasion.

In our opinion, this state of affairs is due in large part to the neglect of the nature of the dependent variable, a practice which the Yale approach shares with most other lines of research in the attitude area. Throughout this book we have demonstrated the importance of distinguishing between beliefs, attitudes, intentions, and behaviors. The major thrust of our argument has been that these variables have different determinants. When they are all treated as instances of a single construct, namely "attitude," the implicit assumption is made that they are all determined by the same antecedent variables. In fact, the Yale approach has assumed that persuasive effects, assessed by changes in beliefs, attitudes, intentions, or behaviors, are always a function of reception of message content and yielding to what is received. The fallacy of this approach becomes apparent whenever the same persuasive communication has different effects on different kinds of dependent variables. According to the Yale approach, reception and yielding occur with respect to the message. Since these processes are assumed to occur prior to the measurement of "attitude" change, they cannot be influenced by the nature of the particular measure used to assess such change. This implies that the same results should be obtained whether beliefs, attitudes, intentions, or behaviors serve as the dependent variable. The fact that different results are often obtained demonstrates that the conceptualization suggested by the Yale approach does not provide an adequate account of the persuasion process.

The conceptualization proposed by Hovland and his associates has a deceptive elegance. Since the yielding mediator is never directly assessed, it is always possible to account for any obtained result by making suitable post hoc assumptions about this mediator. The crucial test of this theory rests on the investigator's ability to demonstrate the consistency and validity of his assumptions concerning the yielding parameter. These problems are seen most clearly in McGuire's (1968) formal two-factor model:

$$p(O) = p(R)p(Y).$$

Probability of opinion change is viewed as a function of probability of reception and probability of yielding. If opinion change and reception are measured, it becomes possible to solve this equation and obtain a value for yielding. This procedure, however, adds little to our understanding of persuasion unless the estimated value is consistent with psychological theory.

Our discussion above concerning inconsistent results with respect to different dependent variables provides one example in which estimates of the yielding parameter would be incompatible with psychological theory. Specifically, such findings are inconsistent with the assumption that yielding is invariant with respect to the kind of dependent variable under consideration. A similar problem exists in relation to the effects of various independent variable manipulations. For example, to account for the inconsistent effects of high- and low-fear appeals, one would have to argue that fear arousal sometimes increases yielding and at other times reduces yielding. This argument is inconsistent with the assumption that a given manipulation, such as fear arousal, should have consistent effects on yielding.

These considerations apply whenever a theory is tested by estimating one or more of its major variables or parameters. Although such an approach is perfectly legitimate, it will contribute to a cumulative body of knowledge only if it is accompanied by a psychological theory that allows the investigator to derive testable hypotheses about the theory's parameters. The psychological assumptions linking independent variables to the parameters should not be changed arbitrarily from one study to another or changed to account post hoc for a given set of experimental findings. We have discussed some of these problems with respect to the application of Anderson's weighted averaging model to studies of information integration (see Chapters 5 and 6).

TOWARD A CUMULATIVE BODY OF KNOWLEDGE

We have adopted in this book an approach based on our conceptual framework which we hope will facilitate the accumulation of knowledge in the attitude area. This approach makes a clear distinction between beliefs, attitudes, intentions, and behaviors; it indicates how these variables can be measured; and it specifies the relations among them. This set of concepts and their specified interrelations provide a conceptual framework which can be used to analyze various phenomena

and lines of research in a systematic and consistent fashion. We have seen that this approach leads to the formulation of hypotheses that are consistent with a wide array of empirical findings. At the same time, it provides explanations for the apparently inconsistent findings in different areas of investigation.

Most important, our approach permits the investigator to test his hypotheses about theoretical processes directly by obtaining measures of his explanatory constructs. Most studies are designed to test a hypothesis about the effects of some manipulation on a dependent variable. According to our approach, the influence of the manipulation on the dependent variable is mediated by its effects on certain beliefs and the direct or indirect relations of these beliefs to the dependent variable. Hypotheses about the effects of the manipulation on a given set of beliefs and about the exact nature of the relation between these beliefs and the dependent variable can be directly tested. This is done by first measuring the beliefs in question and examining whether the manipulation had the hypothesized effects. The links intervening between these beliefs and the dependent variable can also be assessed, allowing a test of the hypothesized relations.

Stated somewhat differently, adoption of our conceptual framework forces the investigator to make explicit assumptions about the processes intervening between his manipulation and the dependent variable. Moreover, our conceptual framework specifies the intervening processes appropriate for a given dependent variable. On the basis of this approach, an investigator studying the effects of communicator credibility on behavioral change might make the following assumptions: (1) Communicator credibility will influence acceptance of the statements contained in the message. (2) Differential acceptance of source beliefs will produce different amounts of change in the corresponding proximal beliefs. (3) Differential changes in proximal beliefs will influence amount of change in beliefs about performance of the behavior. (4) These changes will produce corresponding changes in attitude toward the behavior. (5) As a result, communicator credibility will affect intentions to perform the behavior. (6) The corresponding behavior will also be affected. Each variable in this sequence can be directly measured, and the hypothesized effects of communicator credibility can be tested. When communicator credibility fails to influence behavioral change, it is possible to discover where in this chain of effects the investigator made inappropriate assumptions. For example, the assumption that changes in beliefs about the consequences of performing the behavior will lead to change in attitude toward that behavior may not have been supported in this particular study.

We have seen that many of the contradictory and inconclusive findings in the attitude area appear to be attributable to such inappropriate assumptions about the relations between different kinds of variables. One clear example of such a fallacious assumption is the hypothesis that attitude toward an object is related to specific behaviors with respect to that object. It is our hope that by distinguishing between beliefs, attitudes, intentions, and behaviors and by specifying the determinants of these variables, our conceptual framework will contribute to the development of a cumulative body of knowledge in the attitude area.