CHAPTER 3

Discriminative Validity

Norman Miller
William C. Pedersen
Vicki E. Pollock

Donald Campbell has made many contributions of broad methodological, epistemological, and practical importance to psychology as a science. Within his discussions of methodological issues, validity was a prominent theme. This chapter discusses the more specific notion of discriminative validity.

Among hindrances to the progress of scientific psychology, *inventing new names for old concepts* is one of the more troublesome (Miller & Pollock, 1994). In many areas of psychology, researchers have attached their own idiosyncratic term to a concept, thereby making it more distinctive (and self-referring). Giving a previous well-studied concept, process, or idea, a new label is akin to "reinventing the wheel." Rosenthal (1994), with a deft turn of phrase, introduced the term *concept capture* to describe this not uncommon tendency. There may well be positive features of this practice, such as calling attention to a concept or process, which in turn stimulates research on it and thereby increases scientific understanding. At the same time, however, it clutters the field with unnecessary terms and is antithetical to the scientific ideals of parsimony and conceptual integration. *Put bluntly, instances of concept capture amount to a false assertion*

AUTHORS' NOTE: Preparation of this chapter was facilitated by National Science Foundation Grant SBR 931-9752 and a University of Southern California Pre-Doctoral Fellowship to William C. Pedersen.

of discriminative construct validity, or one made with an absence of confirming evidence.

Our general discussion of the notion of discriminative validity will start with Campbell's fundamental notion of trait validation (Campbell & Fiske, 1959). Trait validation is then distinguished from nomological or construct validity, as Cronbach and Meehl (1955) used the latter term. We will use the terms *trait validity* and *nomological validity* to distinguish the concerns of Campbell and Fiske from those of Cronbach and Meehl. The latter used the terms *nomological validity* and *construct validity* interchangeably. Here, we consistently use *nomological validity* to refer to Cronbach and Meehl's approach, reserving the latter's term *construct validity* as a more overarching term that refers to the issue of concern in a more abstract or general manner.

We then consider the relation between discriminative trait validity and discriminative nomological validity. Next, we extend these notions to discriminative process validity and discriminative theoretical validity. Throughout, we give examples of contemporary instances of questionable attainment of each. Finally, in a brief concluding section, we make some constructive recommendations.

How would Donald Campbell have viewed this chapter? In buttressing our argument, we give examples that seemingly chide researchers for exhibiting questionable parsimony by failing to provide adequate evidence for the discriminative construct validity of the terms they introduce. We suspect that this aspect of the chapter might have rubbed Don Campbell the wrong way. His approach was always constructive, never critical. As graduate students, the first author of this chapter and fellow graduate student in sociology Kiyoshi Ikeda once approached Don for advice about an idea we had for writing a critical article on the literature concerned with worker morale and productivity. We had believed that much of this literature inadvertently (or uncritically) confounded morale with status. Don, never directly telling us to avoid an article that in its critical analysis would disparage the work of others, urged us, instead, to do some research on the issue.

Separate from the degree to which this anecdote illustrates features of Don's characterological makeup, his broader epistemological view also emphasizes a perspective that downplays the importance of the instances of questionable discriminative construct validity that we cite throughout the chapter. That is, in accord with his constructive, noncritical interpersonal approach, an upbeat optimism also characterized his epistemological view. It emphasized the self-corrective features of science—aspects of the peer review process and the critical interchange among scholars that might function to expose instances of questionable discriminative construct validity (Campbell, 1986). In citing instances of such scholarly exchange, in which authors directly attempt to deal with questions raised about the discriminative construct validity of concepts they have

introduced (e.g., Eagly & Chaiken, 1993; Neuberg, Judice, & West, 1997; Petty, 1994; Webster & Kruglanski, 1994), we provide evidence that Campbell would have taken as supportive of his epistemological view. Without belaboring our own perspective, however, we believe that the downside of inadequate demonstration of discriminative construct validity is substantial—that it outweighs such ancillary benefits as drawing renewed attention to an abandoned area of research and/or stimulating new research. Instead, it promotes an overdifferentiated depiction of knowledge that obfuscates the bearing of previously established empirical relationships on the new work that is being presented. It slows development of integrated, broader theoretical organization. Additionally, however, we think the lack of adequate evidence of discriminative construct validity is more efficiently dealt with prior to publication rather than after published instances of its occurrence.

Finally, despite his fondness for neologisms, in his own research Don seemed to emphasize integration, synthesis, and overarching inclusiveness in preference to differentiation, distinctiveness, and analysis. In his empirical work on social projection (Campbell, Miller, Lubetsky, & O'Connell, 1964), conceptually related to our discussion below concerning the discriminative construct validity of *assumed similarity*, *false consensus*, and other related traits, he had hoped to find an empirical basis for integrating behavioristic, Gestalt, and clinical psychology traditions regarding the understanding of human behavior. Thus, to the degree that we are correct in seeing slippage or shortfall regarding Don's optimistic belief that the self-corrective features of science will overcome the consequences of the routine failure by scientists to provide evidence of discriminative construct validity of new terms that they introduce, Don's penchant for and admiration of efforts to integrate knowledge is undermined.

DEVELOPING VALID SCIENTIFIC CONCEPTS

Trait Validation

Campbell and his associates (e.g., Campbell & Fiske, 1959; Cook & Campbell, 1979) were interested in the fit between operations and conceptual definitions. In their general discussion of trait (or concept) validity, Campbell and Fiske (1959) advised that it is necessary to achieve agreement among multiple measures that represent diverse approaches to assessment. They saw heterogeneity among the methods used to assess a trait as critical. They argued that to ensure that common method variance does not mistakenly contribute to the obtained convergence between measures of what was thought to be a single concept or trait, one must assess a trait with methods that are relatively distinct from one another (Campbell & Fiske, 1959). If similar methods of measurement are used to assess a trait, positive correlations will arise, at least in part, as a conse-

quence of their shared method variance. In such a case, distinct concepts may mistakenly be viewed as reflecting the same latent variable. Thus, contrary to Duncan (1984), who in his erudite essays on measurement in the social sciences came to question the wisdom of combining *any* measures, Campbell and his colleagues advocated multiple, maximally heterogeneous assessments to measure an underlying, conceptually single entity or trait. They labeled their approach *trait validation* (Campbell, 1960).

Trait validation, as developed by Campbell and Fiske, shares conceptual correspondence with internal consistency reliability, as reflected in Cronbach's alpha (Cronbach, 1951) or the Kuder-Richardson reliability formula #20 (Kuder & Richardson, 1937; Richardson & Kuder, 1939), which is a special case of alpha. Internal consistency reliability (or single-factoredness) reflects the interrelation among items on a single test. By contrast, trait validation considers diverse tests (items) designed to measure a single concept. The explicit identification of method variance is the key conceptual insight of Campbell and Fiske that separates internal consistency among diverse trait measures (or latent trait measurement) from internal consistency as assessed by application of Cronbach's alpha to a set of items composing a single measure designed to assess X. As typically applied, the latter will almost invariably produce internal consistency estimates that are inflated by a shared format among the items (including their shared response scale), the spatial/temporal adjacency of subsets of items, and other meta-cues for a consistency in responding. For instance, important among such cues in the typical implementation of experimental social psychology is the presence of another set of differently formatted questions that are presented separately to the participant on a previous page—thereby implicitly indicating something distinctively shared by those among the second (and first) set. As an example, a set of mood manipulation check items might appear on one page and a set of attitude measurement items on a second page.

At the same time, Campbell and Fiske (1959) explicitly recognized the fact that *discriminative* construct validity (discriminative trait validation) goes hand in hand with construct validity. "One cannot define without implying distinctions, and the verification of these distinctions is an important part of the validation process" (Campbell, 1988, p. 40). Thus, measures of construct A must be examined in the context of measures of other distinct concepts. That is, it makes sense to argue that within the context of any single type of measurement situation, A should correlate more highly with other measures of A than with measures of B, C, D, and E.[1]

Nomological Validity

Separate from the manner in which Campbell and Fiske deal with adequate identification of a trait or scientific concept in their 1959 paper is the confirma-

tion of theoretically important and hypothesized relationships between the construct (the trait, or the scientific concept) of interest and other constructs. Cronbach and Meehl (1955) viewed such nomological relationships—the frequency with which a construct (concept) exhibited lawful relationships with other constructs—as evidence that a construct had scientific validity. Hence, to them, construct validity meant evidence of predictable, meaningful, and replicable relationships with other concepts.

Decades ago, the cephalic index was used widely by physical anthropologists to assess the head size of racial/ethnic and national groups. It was thought that the measure reflected brain size. Measurements of dimensions of head size by means of physical measurement with a caliper can be highly accurate and reliable. Using such measures, Scots, for instance, were shown to have on average a small head size. Were head size also assessed by means of amount of water displaced by immersing the head in a bucket of water, and were the two indices shown to correlate highly (as they undoubtedly would), there would be evidence of trait validity in the sense prescribed by Campbell and Fiske. Furthermore, were such measures shown to be relatively less well related to similar measures of hand or foot size than they were to each other, they would simultaneously manifest evidence of discriminative trait validity. Despite the high likelihood of obtaining such confirmations, as best as is known, measures of the cephalic index have never been shown to be related to anything else. Thus, valid measurement of a trait or construct in the sense of Campbell and Fiske is not sufficient.[2] To be an important contribution to science, a concept must also be shown to have nomological validity—that is, construct validity in the sense of Cronbach and Meehl. In their discussion of trait validation, Campbell and Fiske (1959) gave little consideration to nomological validity as discussed by Cronbach and Meehl (1955).

Broader Treatments of Concept Validity

Twenty years later, Cook and Campbell (1979) discussed the link between construct validity and the idea of confounding. Here, their presentation incorporates aspects of both trait validation (Campbell & Fiske, 1959) and nomological validity (Cronbach & Meehl, 1955) under the single term *construct validity.*[3] Specifically,

> Construct validity is what experimental psychologists are concerned with when they worry about "confounding." This refers to the possibility that the operations which are meant to represent a particular cause or effect construct can be construed in terms of more than one construct. . . Confounding means that what one investigator interprets as a causal relationship between theoretical constructs labeled A

> and B, another investigator might interpret as a causal relationship between constructs A and Y or between X and B or even between X and Y. (p. 59)

These "reinterpretations" of the causal mechanisms at work are not mere translations of the same concept into slightly different terminology but instead represent conceptually different and rival explanations of the "facts."

Why is construct validity important to the scientific researcher? Cook and Campbell (1979) provide several reasons. First, on a very general level, "researchers would like to be able to give their presumed cause and effect operations names which refer to theoretical constructs" (p. 38). Although they concede that this desire might be stronger for those working on basic theoretical issues, they note that applied researchers also want to define their variables in a more abstract manner, even while wishing to avoid the relatively burdensome task of giving precise and technical operational definitions.

Beyond this basic concern, there are practical reasons why it is crucial. Specifically, they concern issues of replication, efficiency, and the avoidance of "irrelevant" research. With respect to replication, Cook and Campbell (1979) point out that experimental treatments in applied settings often involve a multitude of variables that do not necessarily reflect one single construct. Consider, for instance, the meaning of *school desegregation* as an experimentally introduced social reform. By comparison with control conditions, the situations in which it was introduced varied in size of unit (class, school, cluster of grade levels, or district), voluntary versus court-ordered implementation, one-way versus two-way busing of students, level of racial balance within classrooms, two-group versus multiple-group racial/ethnic mixing, level of parental involvement in schools, the amount of community conflict that preceded it, the degree to which increased monetary resources were coordinated with the implementation of the desegregation plan, teacher/student ratios, and other factors. Replication of research will be difficult if the relevant individual components in a multicomponent treatment are not clearly delineated and specified ahead of time. Second, efficiency dictates that if a subset of all the components is actually responsible for the overall effect, one would want to concentrate on that subset—in both future research and remedial intervention—rather than attempting to reproduce the larger overall treatment. Finally, having a tight fit between operationalizations and the constructs they reflect allows one to avoid irrelevant research.

Although it is clear that construct validity is important, it is less clear how it is achieved. Cook and Campbell (1979) first advocate rigorous attention to defining the constructs concisely and doing so in a manner that is clearly understood by the relevant scientific audience. Second, in terms of data analysis, the evidence must clearly show that the independent variable(s) affected the dependent measure(s) in the predicted manner (nomological validity). In addition, it is

important to assess whether the independent variable covaries with related but conceptually distinct constructs (trait validation). Specifically, one wants to ascertain that the experimental impact on the dependent measure is due to the construct under manipulation and not some other unforeseen variable. Obviously, the trait validity of the dependent measure needs to be considered as well.

Claims of Discriminative Trait Validity

In the next sections, we present some illustrative instances in which researchers implicitly seem to claim discriminative trait validity in the absence of adequate empirical assessment.

Dissonance and Related Constructs

Festinger (1957) introduced the term *cognitive dissonance* to refer to the state produced by the holding of two salient cognitions, one of which is the obverse of the other. For example, if I am aware that the surgeon general's report says smoking causes cancer and I am a smoker, I should experience cognitive dissonance. With the possible exception of attribution theory, no other theoretical development within modern social psychology has stimulated as much research as Festinger's theory of cognitive dissonance (Bagby, Parker, & Bury, 1990).

Aronson (1969) noted the importance of the self-concept with respect to the obverse cognitions involved in a state of dissonance, thereby arguing that the dissonant state required three cognitions: "I believe the task is dull"; "I told someone that the task was interesting"; and "I am a decent, truthful human being" (Festinger & Carlsmith, 1959). Scher and Cooper (1989) argue that the arousal involved in cognitive dissonance is not simply due to the presence of obverse cognitions but instead arises because one's actions have aversive outcomes for others. They describe the internal state produced by these circumstances as consisting of a sense of *impaired self-efficacy.* This impaired self-efficacy reflects the psychological disparity implicit in the second and third cognitions, along with the additional cognitive ramifications of these thoughts. Specifically, the disparity produces a new cognition of self-dissatisfaction (or the momentary cognitive representation of self as having impaired self-efficacy). *Self affirmation*, as described by Steele in his self-affirmation theory (Steele, Spencer, & Lynch, 1993), appears to be the converse of impaired self-efficacy. It is the restoration of that which was damaged. Is *self-affirmation* (Steele et al., 1993) distinct from dissonance reduction? Is there discriminative validity among *impaired self-efficacy* and the states described by other common terms, such as *responsibility*, *guilt*, or *feeling bad*? These questions remain unexamined.

Did the initial introduction of the term *cognitive dissonance*, conceptualized as a state that emerged as a consequence of obverse cognitions, indeed represent a new theoretical concept that had not previously been studied? Balance theory (Cartwright & Harary, 1956) is concerned with the affective relations among elements of a triad: self, object, and other. In the notation of Jordan (1953), it examines the affective relations between p-o (I to other), p-x (I to object), and o-x (my perception of other to object). This notation can be applied to the situations studied to test fundamental hypotheses within dissonance theory. Consider again the classic dissonance experiment in which participants were induced to lie about a boring task to a fellow student (Festinger & Carlsmith, 1959). Incorporating Aronson's notion about the self, its dissonance-producing ingredients are these: (a) I like my roommate (+ relation of I to other), (b) I say I like the boring task (+ relation of I to object), and (c) I believe that my roommate will dislike this boring task (– relation of other to object). Among the eight permutations of signs linking these three components, this ++– triad is one of the four viewed as unbalanced (unpleasant) by balance theory researchers. Does cognitive dissonance (an unpleasant state) differ from the arrangements of the triadic components in balance theory that are experienced as unpleasant? To our knowledge, dissonance theorists have never addressed this issue.[4]

Assumed Similarity and the False Consensus Effect

Assumed similarity refers to a judgmental bias in which the similarity between self and others is exaggerated. As shown by meta-analytic evidence, it is a consistent and reliable finding (e.g., Gross & Miller, 1997; Mullen & Hu, 1988). It applies to attitudes, personality traits, interests, and values. It was noted by Francis Bacon (1620/1853), who, in discussing its various manifestations as a conspicuous bias in human social perception, mentioned in particular the tendency to project one's own worldview onto others. Freud's (1937) discussion of paranoid projection provides an instance from the domain of personality trait attribution. Within scientific psychology, the history of research on assumed similarity is so extensive that incisive methodological suggestions concerning its quantitative analysis were raised more than three decades ago (e.g., Cronbach, 1955).

The *false consensus effect* (FCE) is defined as the difference in consensus estimates by those agreeing with and opposing an opinion position (Ross, Greene, & House, 1977). The data that constitute the FCE and the data concerning the magnitude by which consensus estimates exhibit bias from reality (viz. data on assumed similarity) are inextricably linked, being facets of the same data set (Gross & Miller, 1997). More than 150 references to work on assumed similarity had been published prior to the introduction of the new term, FCE (Miller &

Pollock, 1994). Some of these (e.g., Travers, 1941; Wallen, 1943) had used a paradigm identical to that employed specifically for assessing the magnitude of the FCE. To describe the entirety of antecedent work related to the FCE, however, its originators mention only five references.

Should the term *FCE* be singled out as a unique instance of concept capture within this specific research domain? It hardly appears to be. Instead, more than 15 other distinct labels previously had been used by various researchers to describe what is seemingly a single underlying concept (Miller & Pollock, 1994). More important, there has been little attempt to present evidence on whether any of these 15-plus labels for this judgmental bias obey laws that differentiate one member of this family of terms from another. In other words, evidence for discriminative construct validity appears to be lacking.

Need for Closure

The *need for closure* is an important theoretical construct in Kruglanski's lay epistemic theory (1989, 1990a).[5] Moreover, it provides an instance from contemporary social psychology in which both the major proponent of the importance and distinctiveness of a concept, as well as others, have examined its discriminative trait validity.

In constructing an individual difference measure to assess need for closure, Webster and Kruglanski (1994) described five separate aspects or constructs that captured the broad scope of the underlying theory: order, predictability, decisiveness, ambiguity, and closed-mindedness (Kruglanski, 1989, 1990a, 1990b). The five items that constitute the first of these constructs, preference for order and structure, were taken from a previously published scale (Thompson, Naccarato, Parker, & Moskowitz, 1993) called the Personal Need for Structure (PNS) Scale. In addition, three of the items that composed the third construct, urgency of striving for closure in judgment and decision making, were taken from the Personal Fear of Invalidity (PFI) Scale—also previously published by Thompson, Naccarato, Parker, and Moskowitz (1993). Finally, an additional three items from the fourth construct of the Need for Closure Scale (NFCS), dealing with predictability, were also taken from Thompson et al.'s (1993) Personal Need for Structure Scale.

To provide evidence of the discriminative trait validity of the NFCS, Webster and Kruglanski (1994) examined the relation between participants' scores on the NFCS with other previously developed measures that fell into three distinct conceptual categories. The first category contained scales hypothesized a priori to assess overinclusiveness: the F Scale (Sanford, Adorno, Frenkel-Brunswik, & Levinson, 1950), measuring authoritarianism; the Dogmatism Scale (Rokeach, 1960); the Intolerance of Ambiguity Scale (Eysenck, 1954); the Bieri REP Test

(Bieri, 1966), designed to measure cognitive complexity; and the MPQ Control Subscale (Tellegen, 1982), which assesses impulsivity. The second category of scales were viewed as assessing exclusiveness: the Personal Need for Structure (PNS) Scale (Neuberg & Newsom, 1993; Thompson et al., 1993) and the Personal Fear of Invalidity (PFI) Scale (Thompson et al., 1993).

Although both these sets of measures were viewed as tapping conceptual aspects of the NFCS, they also were seen as measuring other conceptual ingredients thought to be unrelated to the key conceptual aspects of lay epistemic theory. Finally, in the spirit of trait validation as discussed by Campbell and Fiske, the third category contained scales assumed to measure constructs unrelated to the NFCS: the Social Desirability Scale (Crowne & Marlowe, 1964), the Need for Cognition Scale (Cacioppo & Petty, 1982), and the Quick Test of Intelligence (Ammons & Ammons, 1962).

When Webster and Kruglanski (1994, Table 4, p. 1054) correlated each of these scales with the NFCS (both as a unitary scale and separately for its five subscales), they obtained low correlations. They report no comparisons showing differential average intra- and intercategory correlations among measures composing their three categories; nevertheless, they argue that the NFCS is conceptually distinct from the concepts measured by this host of other scales and that it "appears to possess acceptable discriminative and convergent validity with respect to other relevant psychological measures" (p. 1056).[6]

Neuberg and his colleagues provided independent evidence on these issues (Neuberg, Judice, et al., 1997; Neuberg, West, Judice, & Thompson, 1997). In all six of their samples, they obtained substantial overlap between the NFCS (when used as a unitary scale) and the PNS Scale (median r = .79). Neuberg, Judice, and West (1997) concluded that "the evidence is overwhelming that when used in a unidimensional manner, the NFCS is operationally redundant with the PNS Scale" (pp. 1403-1404). Moreover,

> When the NFCS is used more appropriately as a multifactorial scale, three of the five NFCS subfacets also fail to demonstrate discriminant construct validity from preexisting measures. The two strongest NFCS subfactors are highly redundant with the two PNS Scale subfactors (Neuberg & Newsom, 1993), with correlations in the .80 range. And, again despite Webster and Kruglanski's position to the contrary, a third NFCS subfactor is highly redundant with the Personal Fear of Invalidity (PFI) Scale (Thompson, Naccarato, & Parker, 1989), with correlations between the two in the .75 range. Our analyses reveal, then, that the NFCS possesses little, if any, discriminant validity. (p. 1397).

Moreover, in more recent work, Webster and Kruglanski (1994) found a similar relation between the NFCS and the Personal Need for Structure Scale (mean r =.72).

Bogen (1975) lists more than 35 sets of dichotomous terms that theorists have used to differentiate individual differences in cognitive styles reflecting a propensity for being focused, narrow-minded, and desirous of cognitive structure, as opposed to being general and open-minded. Do the differences described by these 35 pairs of terms also correspond to individual differences in need for closure?

Having discussed issues concerning the development of valid scientific concepts and, conversely, having presented examples of the conceptual repackaging of "old wine in new bottles" from the areas of social and personality psychology, we now broaden our scope. Specifically, we next consider the extension of our concerns to distinctive lawfulness.

DISCRIMINATIVE NOMOLOGICAL VALIDITY

When does one have evidence of a new law? Cronbach and Meehl skirted the issue of *discriminative* nomological validity, as did Cook and Campbell. The Campbellian notions of discriminative trait validity, however, can be applied to the problem of distinct lawful relationships—lawful relationships whose uniqueness amounts to more than instances of concept capture. If a researcher claims to have discovered a new law, the claim may rest on (a) a new independent variable that is linked to a well-established dependent variable, (b) a new dependent variable that has never before been associated with a well-studied independent variable, or (c) a relationship that involves both new independent and new dependent variables. Consequently, distinctive nomological lawfulness can be compromised when either the A variable, the B variable, or both variables lack discriminative trait validity in the sense of Campbell and Fiske (1959).

Within the context of experimental social psychology, the steps to be taken to address questionable discriminative construct validity are less problematic when a dependent variable, as opposed to an independent variable, is the source of concern. The reason for this is that dependent variables manifestly consist of a measure of a trait, state, or entity. Specifically, dependent measures consist of questionnaire items, codings of observed behavior, or other measures that operationalize a trait or concept. Of course, in line with the Campbell and Fiske program, for the comparative examination of intra- and intertrait relationships it is necessary that there be more than a single item for assessing both the key dependent concept of interest (B) and other potentially relevant but conceptually distinct dependent variables.

When independent variables ("A variables") are manipulated variables, as in experimental social psychology, establishing their discriminative validity in a lawful relationship (if A, then B) does not on first thought fall as readily into the Campbell and Fiske mold. How might one establish discriminative trait (or state) validity for manipulated variables? One approach is to develop trait

(or state) measures that correspond to the manipulated variables. Such measures, termed manipulation checks, in fact are commonly incorporated into good experimentation. Here, however, it may make sense to draw a distinction between manipulation checks that merely assess knowledge of a manifest event from those that assess successful (temporary) induction of an internal state. The former might include (a) knowledge that the experimenter said "Your task performance will be *evaluated*," (b) awareness that one was asked to read a passage on "X," or (c) acknowledgment that one listened to a tape recording of "steady rainfall." The latter might include measures, respectively, of (a) skin conductance, (b) memory of a specific substantive content, or (c) a subjective state of boredom or relaxation.

Clearly, measures of the second type correspond more closely to the conceptual variable that a researcher might hope to have manipulated. Here, there are two important points. First, to allow assessment of discriminative trait validity, and hence assessment of discriminative nomological validity as well, one needs not only two or more items that assess a (or b, or c) but also two or more items that assess allegedly distinct but relevant rival concepts, so as to justify the scientist's claim of discriminative nomological validity for a new law relating A to B. In addition, however, in accord with the distinction drawn above, the manipulation check measures must assess whether the underlying state (or process) and other related states (or processes) have been induced, rather than there merely being knowledge of the experimenter's behavior or instructions.

Claims of Discriminative Nomological Validity

The literature on *terror management* (Greenberg, Pyszczynski, & Solomon, 1986; Solomon, Greenberg, & Pyszczynski, 1991) provides a contemporary instance in which discriminative nomological validity has been claimed. Its key idea is that reminding a person of death (viz., mortality salience) increases the strength of one's faith in one's own particular cultural worldview and that, as a consequence, it not only increases their rejection or negative evaluation of those who violate their cultural norms or beliefs but also augments positive evaluation of those who support them (e.g., Greenberg et al., 1990; Rosenblatt, Greenberg, Solomon, Pyszczynski, & Lyon, 1989). Thus, the research examines the relationship between mortality salience as an independent variable and evaluative bias as a dependent variable.

To what degree does this posited relationship exhibit discriminative nomological validity? The first thing to note is that Greenberg and his colleagues do not claim any uniqueness with respect to their conceptual dependent variable—typically, out-group derogation, but sometimes the positivity of in-group evaluation. They assess out-group negativity by one of several specific evaluative measures of target persons who threaten (or support) the actor's

worldview. Their three principal operationalizations are (a) setting a monetary value of a bail bond to be required from a deviant (viz., a prostitute) to gain release from jail, pending trial (e.g., Rosenblatt et al., 1989, Study 1); (b) trait evaluations of a target's personality (e.g., Greenberg et al., 1990, Study 1); and (c) completion of the Interpersonal Judgment Scale (Byrne, 1971), which asks participants to rate a target person's intelligence, knowledge of current events, desirability as a work partner, and other characteristics.

The independent variable, mortality salience, typically has been manipulated by asking participants to write a response to the following prompts: "Please briefly describe the emotions that the thought of your own death arouses in you" and "Jot down, as specifically as you can, what you think will happen to you as you physically die and once you are physically dead." The control condition typically required participants to respond to similar prompts concerning emotional reactions to watching television. Other less frequent manipulations of mortality salience included requiring participants to respond to a Death Anxiety Questionnaire (Conte, Weiner, & Plutchik, 1982), to view a film of a gory accident, and to complete key dependent measures while adjacent to a funeral home.

It seems clear that in much of their initial work, their independent variable—mortality salience—was conceptualized as a specific form of anxiety that is distinct from other forms of anxiety, or even more generally, distinct from other forms of negative affect. Consistent with this interpretation, Greenberg, Solomon, and Pyszczynski (1997) state that "mortality salience effects are engendered specifically by concerns about one's own mortality rather than in response to *any* anxiety-provoking or self-threatening event" (p. 98, emphasis added). By contrast, however, a substantial amount of research within the large literature on intergroup relations links the rejection of out-groups to antecedent inductions of either integral anxiety—anxiety that is elicited directly by the salient out-group (e.g., Stephan & Stephan, 1996)—or incidental anxiety or general negative affect—anxiety or negative affect that is produced by an extraneous source (e.g., Urban & Miller, 1998; Wilder, 1993). This larger literature challenges the alleged uniqueness or specificity of mortality salience (death anxiety) in producing out-group rejection. That is, it questions the discriminative nomological validity of the relation between mortality salience, as a specific form of anxiety, and inductions of other types of anxiety that are empirically linked to the rejection of out-groups or those who challenge one's worldview.

To their credit, Greenberg and colleagues have attempted to rule out such alternative interpretations and show that mortality salience effects are indeed distinct from those produced by general anxiety. First, each of their studies has included an affect measure that typically contains a specific subscale for measuring anxiety or fear. Results generally have shown no difference in negative affect or anxiety between the mortality salience and control conditions, and, in

instances in which such differences arose, their use of affect or anxiety level as a covariate has not altered their overall finding on their main dependent variable. Second, worldview defense effects have not emerged in additional control conditions in which participants were asked to think about aversive events other than their own death (e.g., giving a public speech, experiencing intense physical pain, taking an exam). For instance, Greenberg, Pyszczynski, Solomon, Simon, and Breus (1994) showed that whereas mortality salience elicited derogation of a target who wrote an anti-U.S. speech compared to a pro-U.S. speech, inductions of other types of anxiety such as thinking about the possibility of experiencing intense physical pain or giving a talk in front of a large audience did not. Similarly, they have shown that remembering past failures, or currently experiencing failure, did not produce outgroup bias (Greenberg, Simon, Pyszczynski, & Solomon, 1996).[7] Finally, the fact that mortality salience increased negative evaluations of a deviant but did not manifest itself on a measure of negative affect, whereas another anxiety-provoking manipulation unrelated to death had just the opposite effect (i.e., no increase of derogation but an increase in negative affect), was seen as further evidence of discriminative trait validity of mortality salience and, hence, nomological validity for its relation to outgroup bias (Greenberg et al., 1995).

Although we applaud these efforts, we wonder where they take us with respect to our concerns about discriminative nomological validity. What does it mean if mortality salience conditions fail to evidence effects that differ from those of control conditions on manipulation check measures of anxiety or negative affect? Not only are these specific comparisons irrelevant to our concern with discriminative nomological validity, but also, because the measures that they have used as manipulation checks (e.g., PANAS, MAACL) have been well validated in the past, the outcomes of such comparisons lead one instead to question whether *any* form of anxiety has been manipulated. One response to such absence of differential effects on manipulation check measures is to postulate that experimental subjects defended against expressing the death anxiety elicited by mortality salience inductions and thereby masked differential effects on manipulation checks. More recently, terror management theorists have in fact postulated that such defense does indeed occur (e.g., Greenberg et al., 1995). If so, assessment of discriminative nomological validity requires comparative examination of the effects of other types of anxiety (e.g., sexual arousal) that also elicit defense on manipulation checks. Additionally, the induction of such other defense-arousing anxieties needs to be matched in strength with the induction of mortality salience (e.g., as judged by external judges).

Their second point, that manipulations of anxiety other than the induction of mortality salience have failed to elicit rejection of out-groups (whereas mortality salience does), seems on first thought to support the contention of

discriminative nomological validity of mortality salience. With these comparisons, it is incumbent on these researchers to explain why their own outcomes for incidental negative affect do not yield out-group rejection, whereas other researchers typically do find evidence that out-group rejection is increased by the arousal of negative incidental affect (e.g., Forgas, 1995; Forgas & Fiedler, 1996; Wilder & Shapiro, 1989, 1991; Wilder & Simon, 1996). Moreover, to the degree that these comparisons show greater out-group rejection for mortality salience than do other anxiety inductions, there are two other considerations that are important and need to be controlled. First, the differential importance of death by comparison with other potential worries, such as failing an exam, needs to be considered.

Second, other research suggests that suppression can produce intensified rebound effects (Wegner, 1994). As suggested in the previous paragraph, if the anxiety elicited by mortality salience is suppressed and controlled, its effects on out-group rejection are likely to be intensified. Consequently, the appropriate comparison condition for demonstrating discriminative nomological validity is not the induction of an anxiety (other than mortality salience) that is not suppressed but rather the induction of one that is.

Finally, another issue concerning discriminative nomological validity of terror management theory is the specificity of the findings to thinking about one's own death and not about death in general. Greenberg and colleagues state that "participants writing about the death of a loved one exhibited worldview defense only to the extent that this reminded them of their own mortality" (Greenberg et al., 1997, p. 98). This was interpreted as showing that not only is mortality salience the key component but also that it is specifically one's own death (and not death in general) that underlies their findings. The absence of relevant manipulation checks touching on this point remains a problem. Specifically, with respect to their comparison of the effects of writing about one's own death versus that of a loved one, they provide no evidence suggesting that the two manipulations differentially reminded participants of their personal mortality. Moreover, although own mortality salience induced stronger worldview defense than did writing about the death of a loved one, the latter also differed from the control. This specific finding differs from that seen in other research by Greenberg and colleagues wherein the anxiety-provoking conditions that were not related to the participant's own death never produced outcomes that differed from those in the control condition. Why should thinking about the death of a loved one be different from contemplating an impending exam or the receipt of mild electric shock? Although Greenberg et al. (1994) suggest that thinking about the death of a loved one might remind participants of their own mortality, there are no direct assessments of this possibility.

Our point in this section is not to diminish the accumulated work on mortality salience and worldview defense. Greenberg, Pyszczynski, Solomon, and colleagues are to be commended for their systematic work on the effects of mortality salience on worldview defense and their good efforts toward a process understanding of this relationship. Instead, our purpose was to use this body of research to illustrate issues with respect to discriminative nomological validity.

ASSESSMENT OF DISCRIMINATIVE PROCESS VALIDITY

Separate from trait and nomological validity is the issue of process distinctiveness. Researchers have sometimes claimed that distinct processes underlie the events that occur in response to one or another type of stimulus or setting or in the responses of one or another type of person, or group. For instance, Moscovici (1980) has argued that distinct processes account for influence by numerical minorities and majorities. Taking another example, some research suggests a discontinuity between effects at the individual level, by comparison with the group level (Brown & Turner, 1981; Schopler & Insko, 1992), suggesting separate processes. The notion that there are separate routes of persuasion (e.g., Chaiken, 1980, 1987; Petty & Cacioppo, 1986) also suggests distinct processes. Parsimony, however, requires that postulation of multiple distinct explanatory processes be eschewed when a single process model can be shown to provide adequate explanation (e.g., Kruglanski & Mackie, 1990; Kruglanski & Thompson, 1999). In this vein, researchers have frequently urged against invocation of motivational explanations when it appears that cognitive processes are sufficient for explanation (e.g., Kreuger & Clement, 1997).

The issue of discriminative process validity is more subtle and complex than is discriminative construct validity. On one hand, process assessment requires that one establish a relationship not only between (a) the situation, as operationalized, and the outcome but also between (b) the situation and the process and between (c) the process and the outcome. At the same time, it requires that one rule out to a reasonable degree (a) the influence of prior or concurrent conditions that correlate with the situation as operationally defined, (b) concurrent conditions induced inadvertently by the manipulations, and (c) alternative processes. Seven procedures have been viewed as relevant to, or useful for, assessment of process uniformity or distinctiveness (Harrington & Miller, 1993). They differ in their diagnostic strength. Two, ecological validity and experimentation, are too weak to be considered useful for the task. Both, in their bald form—that is, when implemented without the addition of process measures or manipulations—solely examine differences in outcomes. The usefulness of a

third, examination of statistical interactions, requires a modification of prior interpretation (Kruglanski & Mackie, 1990) of its usefulness.

Rejected Procedures for Establishing Process Distinctiveness

Ecological Validity

Ecological validity, or natural covariation, is the most primitive approach for inferring process distinctiveness. As indicated, some have proposed that different principles apply to intergroup and interpersonal behavior (e.g., Brown & Turner, 1981). In daily life, people meet as individuals (perhaps to exchange personal information), or as members of two or more groups (perhaps to resolve a dispute). One could observe the array of behaviors emitted by the actors in the two settings and ask whether their relative frequencies differ. Observed differences in competitiveness, for instance, might seemingly support the idea of process distinctiveness between interpersonal and intergroup settings. Alternatively, however, they may reflect selection effects among those who enter each setting, as well as different motives in the same individual when entering each setting. Thus, although different frequencies of competitive behavior within each setting *may* reflect distinct underlying processes, nevertheless, empirically observing the differential occurrence of such covariation cannot provide confirming evidence of process distinctiveness.

Experimentation

Experiments provide circumstances for stronger inference, but they too ordinarily do not speak strongly on process distinctiveness. Returning to our previous example, individuals experimentally assigned to interact as a member of a dyad (interpersonal behavior) exhibit lower rates of competitiveness than those assigned to one of two groups (Schopler & Insko, 1992), an effect consistent with Brown and Turner's (1981) contention of different underlying processes in the two conditions. The experience of differential threat within the two settings, however, may provide a single-process explanation that, if controlled, will eliminate the effect.

Interactions

The previous approach can be extended by experimentally examining interactions to assess whether the effect of relevant independent variables on behav-

ior differs across settings. One may be inclined to infer process uniformity if a variable has similar effects in different contexts. By contrast, when independent variables interact with context features, one may be inclined to infer process distinctiveness. Consider, for instance, settings that vary in cognitive overload, operationalized perhaps by a secondary task such as digit counting or memorization (e.g., Gilbert & Hixon, 1991; Gilbert & Osborn, 1989). Cognitive overload typically is viewed as interfering with encoding and retrieval of information relevant to a primary task because it reduces capacity within working memory. On first thought, its manipulation appears to have little connection with experimental inductions of negative affect (e.g., instructing participants to recall an extremely sad personal experience; Baker & Guttfreund, 1993). Further consideration, however, suggests overlap. Specifically, the heightened effort required by cognitive overload may induce negative affect (Marco & Suls, 1993; Repetti, 1993). If so, the two variables, ordinarily believed to be conceptually distinct, may yield parallel effects because they induce the shared underlying state of negative mood.

On the other hand, at least some forms of negative mood appear to induce more careful and accurate processing (Pacini, Muir, & Epstein, 1998). By contrast, cognitive overload, by taxing cognitive resources, induces a reliance on stereotypes and categorical judgments as compensatory strategies for the insufficient capacity available for careful processing—and thereby is likely to reduce accurate depiction of exemplars that deviate substantially from their category prototype. Seemingly, experiments could be designed to examine the interaction between the two variables in a context in which their potentially confounded effects were separated. Perhaps this could be achieved, for instance, by using an overload task that required processing of words sufficiently associated with positive affect to counter the negative affect induced by the increased task difficulty of the high-load condition. If, under these circumstances, a crossover interaction was obtained, such that increased induction of negative affect produced a different direction of effect than did increased load, the case for process distinctiveness would be supported.

To take another example by returning to the alleged distinction between intergroup and interpersonal contexts, levels of social status, power, and interdependence are seen as affecting intergroup behavior (Brown & Turner, 1981). If these factors (status, power, and interdependence) similarly affect interpersonal relations, that would support process uniformity for the two allegedly distinct settings. By contrast, if they produce opposing directions of effects in the two settings, that would support process distinctiveness.

In an attempt to assess Moscovici's (1980) claim that distinct processes underlie numerical majority and minority influence, Kruglanski and Mackie (1990) reviewed the relevant literature. They not only invoked the first two methods, ecological validity and difference in experimental effects, as evidence

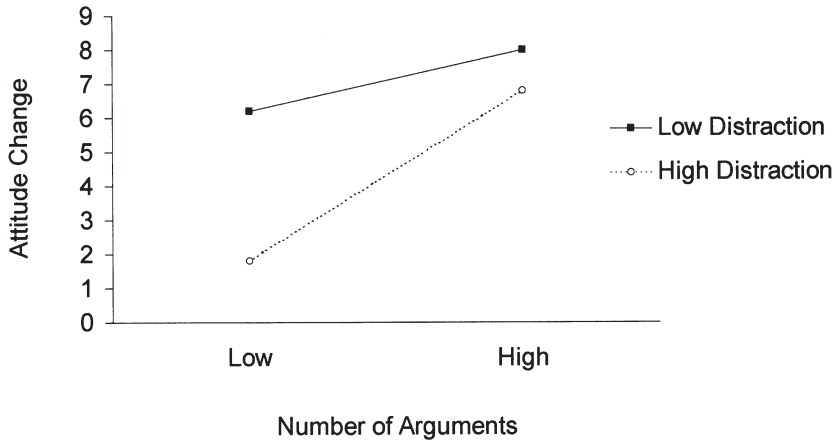

Figure 3.1. A Hypothetical Interaction Effect Between Distraction and Number of Arguments That Need Not Be Interpreted as Reflecting Distinct Distraction Processes When Arguments Are Few Versus Many

for process distinctiveness but also invoked the presence or absence of statistical interactions to thereby form an ordered scale of "strength of evidence." Specifically, they argued that an interaction between variable "X" and minority or majority group status would offer the strongest evidence for process distinctiveness.[8] Thus, absence of even a correlational relation and presence of an interaction between majority/minority status and another variable formed the weak- and strong-evidence endpoints of their scale.

In their interpretive logic, however, they failed to note the differential diagnostic power of ordinal and disordinal interactions. Aspects of measurement scales or differences in the magnitude by which subjective experience is altered by "equivalent" increments of a manipulated variable can create ordinal interactions that do not require the postulation of distinct underlying processes for their explanation. Noncrossover interactions therefore cannot be viewed as diagnostic of discriminative process validity.[9]

For example, imagine an experiment that manipulated distraction (high versus low) and the number of persuasive arguments, with 10 in the "low" and 20 in the "high" condition, respectively. Assume further that, in the context of two main effects, a significant ordinal (i.e., noncrossover) interaction was obtained such that distraction strongly affected persuasion in the low but not the high argument-number condition (see Figure 3.1).

Given their stated reasoning, Kruglanski and Mackie (1990) would take this interaction as evidence of process distinctiveness under low and high numbers of arguments because distraction affected the two argument conditions differ-

ently, causing a large decrease in persuasion in one case but not the other. One can also argue, however, that there is no difference in the underlying process that mediates the effect of number of arguments on attitude. Instead, the "effect" is due simply to the diminishing return of increased stimulus intensity and as such is similar to Weber's, Fechner's, or Steven's laws regarding the perception of physical stimuli. For these psychophysical laws, the essential idea is that an equivalent increase in subjective stimulus intensity requires ever increasing absolute intensities of the physical stimulus. Increases in the value of a physical stimulus therefore are more "impactful" in terms of their effect on perception when the intensity of the stimulus is already low and therefore near the absolute threshold.

Returning to the preceding example, assume that the number of arguments corresponds to a stimulus intensity dimension—strength of persuasive message. Further assume that the effect of high distraction is to halve the number of arguments processed, irrespective of whether the number of arguments is high or low. Taking these assumptions and tying them to the psychophysical principle stated above, under the high argument-number condition the difference between the 20 and 10 arguments respectively processed by participants subjected to low (zero) and high distraction is not as impactful (in terms of its effect of decreasing persuasion) as is the decrease from 10 to 5 arguments that will be produced by these same levels of distraction in the low number-of-arguments condition.

Direct Examination of Process and Process Distinctiveness

Correlational Analysis of Process

Stronger approaches to establishment of process distinctiveness will link the process both to antecedent and to consequent effects. Specifically, one can examine (a) the relation between key independent variables and process events (as dependent variables) and (b) the relation between the alleged process and the key dependent measures of interest. In line with our discussion of discriminative nomological validity, measures of relevant and irrelevant rival concepts are needed for the components of (a) and (b). Although better than no measurement of the alleged process, this procedure is weak in its failure to provide any evidence confirming the postulated temporal ordering of the three ingredients—independent variable, process, and final dependent variable.

Combined Correlational and Experimental Analysis

Statistical mediational assessment (Baron & Kenny, 1986; Judd & Kenny, 1981) is another approach now routinely used (Sigall & Mills, 1998) to assess

mediational processes. The well-established tendency among two interacting groups for the numerically smaller one to exhibit stronger in-group identification and favoritism has been attributed to greater self-focus of the smaller group (Mullen, Brown, & Smith, 1992). Demonstration of process mediation with statistical mediational procedures (in addition to showing that manipulated numerosity affects the magnitude of bias) requires that self-focus be affected by numerosity, that self-focus be correlated with bias, and that when the effect of numerosity on self-focus is controlled (via covariance or regression analysis), its effect on bias disappears.

A problem here, as with the previously discussed procedures, is that it does not rule out other possible mediators (Sigall & Mills, 1998). Moreover, as with the preceding procedures, statistical mediational assessment cannot provide direct evidence of a causal connection between the alleged mediator and the key dependent variable in that it does not establish the temporal ordering implied by the alleged process explanation. The situational manipulation designed to assess the process may *simultaneously* affect both the key dependent measure and the alleged measure of process, leading one to mistakenly impute a process role for the latter. For example, cooperation and competition, conceptually and operationally defined by the structure of outcomes, may elicit different motives. Cooperation may mean, to participants, "talk to each other." Competition may mean "focus on the task" (cf. Bettencourt, Brewer, Croak, & Miller, 1992). On first thought, one might assume that these differences in meanings mediate the effects of cooperation and competition on a key measure (e.g., attraction). Instead, both the differences in meaning (talking vs. task focus) and the differences in liking or attraction toward one's coactors may be simultaneous effects that are consequences of (unmeasured) differences in categorization caused by the manipulation of goal structures. Cooperation may induce a superordinate one-group perception, whereas competition causes a two-group or multiple-group perception (Gaertner, Dovidio, Anastasio, Bachman, & Rust, 1993). Such categorization effects may simultaneously affect focus (interpersonal vs. task) and degree of liking, and if unmeasured, they will never be diagnosed as the critical underlying process event.

Experimental Analysis of Process

The approach described in the preceding section experimentally examines the effect of the antecedent situation on the process. It can be further strengthened by applying experimentation to all steps of the causal chain. This requires examination of three experimental components: the effect of the antecedent situation on the process, the effect of the antecedent situation on the key dependent variable, and a direct manipulation of the process so as to experimentally (rather

than correlationally) examine its effect on the key dependent variable. Both nomological and process distinctiveness will be confirmed by application of the principles for trait validation to all three components of the chain.

Word, Zanna, and Cooper (1974) provide an instructive illustration. In Study 1, white interviewers met with confederates posing as applicants for a job. The race of the applicant was manipulated (black vs. white). The dependent measures included nonverbal behaviors (e.g., physical distance between the interviewer and the interviewee, eye contact with the confederate), the length of the interview, and the speech error rate of the subject (e.g., stutters, sentence incompletions, repetitions). In the second study, all subjects were white applicants who were interviewed by confederates. Some of the dependent measures used in Study 1, namely, "immediacy behaviors," were manipulated. In the immediate condition, the interviewer sat closer to the subject, made fewer speech errors, and conducted a longer interview, by comparison with the nonimmediate condition. Dependent variables were (a) judges' ratings of the subject's interview performance, (b) judges' ratings of reciprocated immediacy behaviors by the subjects, and (c) subjects' ratings of their postinterview mood state and attitudes toward the interviewer (confederate).

The results showed that black applicants were treated with less immediacy than whites (Study 1) and that this differential treatment impeded the performance and negatively affected the attitudes of job applicants (Study 2). In terms of our analysis of the needed components, the strength of Word et al.'s (1974) article lies in their completion of parts 1 and 3. Specifically, they test the effect of the antecedent situation (i.e., race) on the process (i.e., immediacy behaviors) (part 1), and they directly manipulate the process to examine its effect on the key dependent measure (i.e., applicant performance) (part 3). Thus, the research has strong analytical features. Nevertheless, an assessment of the effect of the antecedent situation (viz., race) on the key dependent variable (performance) is missing. Of greater importance, there are no assessments of rival process measures, those other than immediacy. For instance, one unassessed rival process explanation is that black applicants may elicit more anxiety in white interviewers than do white applicants (Stephan & Stephan, 1985). In turn, the anxiety of the white interviewers not only may depress black performance (had performance of black and white applicants been measured) but also may have produced the effects found on the measures of immediacy. Moreover, even though the differential effects of the manipulation of immediacy in Study 2 may indeed have the effects experimentally shown by Word et al. (1974), these effects may be ancillary. They may be sufficient to produce the effects observed but not be a necessary ingredient of the underlying process. That is, even if immediacy behaviors were controlled, it is possible that differential anxiety on the part of the interviewers (had it been manipulated in Study 2 instead of immediacy) would have produced the same effects found in Word et al's. version of Study 2.

Clearly, it is important to measure and manipulate rival process variables to assess their comparative explanatory value.

Meta-Analytic Process Analysis

The use of meta-analytic synthesis of experimental procedures that incorporate the approaches described in the three preceding procedures will provide the strongest evidence on process uniformity or distinctiveness (see Driskell and Mullen, 1990, for a meta-analytic approximation of statistical mediational assessment). As has been suggested previously for the preceding three approaches, however, a most important addition to this meta-analytic amalgam is the inclusion (for comparative purposes) of measures that assess rival, as well as the hypothesized explanatory, processes. At the same time, it is important to note that differential reliability and validity of the measures used to assess rival explanatory processes will contribute to differential statistical confirmation of their explanatory strength, as assessed by statistical mediation procedures. In turn, this can lead to erroneous inferences concerning the relative explanatory power of the (rival) processes they are assumed to tap.

EXTENSION OF DISCRIMINATIVE VALIDITY TO THE LEVEL OF THEORIES

In the preceding sections, we dealt with discriminative validity in terms of both constructs and processes. This analysis can now be extended to a more abstract or superordinate level—the discriminative validity of rival theoretical accounts. Are two theoretical explanations distinct, or do they amount to a compounding of multiple instances of concept capture?

In the arena of persuasion, the two theories that have dominated research during the last decade, the elaboration likelihood model (ELM) (Petty & Cacioppo, 1986) and heuristic-systematic model (HSM) (Chaiken, 1980, 1987), provide a useful illustration. Both hypothesize a dual-process feature in which there are two qualitatively distinct routes to persuasion, the central or systematic route for the processing of message arguments and the peripheral or heuristic route for the processing of cue information. Attitudes formed through the first route are longer lasting and more resistant to change than are those affected by the second. Here we ask if the distinctions between the two—other than the models being named, respectively, by selecting labels from opposing endpoints of a continuum that describes thoroughness of message or informational processing—are in any way theoretically important.

The proponents of each approach note differences between the two models. There are three issues on which either one or the other proponent attempts to

delineate differences that distinguish the models from each other. For instance, Petty (1994) states that the ELM views heuristic processing as a subcategory of peripheral processing. Eagly and Chaiken (1993) concur, seeing heuristic processing as more narrow and refined in scope than peripheral processing because the heuristic processing mode implies that individuals use learned knowledge structures, simple schemas, or rules to make judgments. Unlike the ELM's treatment of peripheral processing, the HSM emphasizes underlying principles of cognitive processing that affect heuristic processing (Eagly & Chaiken, 1993). Some of these principles include availability, referring to the storage of a structure in memory for potential subsequent use (Higgins, King, & Mavin, 1982); accessibility, referring to the activation of a structure from memory (e.g., Chaiken & Eagly, 1983; Roskos-Ewoldsen & Fazio, 1992); the processing goal (e.g., motivation induced by concerns about accuracy, defense, or impression management); and reliability or strength, as manipulated for instance as the likelihood of receiving good advice from a good friend (Chaiken, 1987).

Second, Petty (1994) explains that the ELM hypothesizes a trade-off between the central and peripheral routes of processing whereas the HSM allows for the possibility of increased impact of both systematic and heuristic processing as elaboration likelihood is enhanced. Thus, the ELM more clearly views the processing as consisting of a continuum, ranging from central to peripheral processing, than does the HSM. Petty does not believe, however, that the central and peripheral routes are mutually exclusive. In fact, bringing convergence rather than divergence to the two models, he states that these processes often co-occur and potentially may have a collective impact on attitudes. The trade-off hypothesis simply indicates that a variable has a decreased likelihood of affecting attitudes via a peripheral process as the elaboration likelihood increases. Echoing this point of view, Eagly and Chaiken (1993) mention a "concurrent processing assumption" (p. 328) that allows the HSM to propose "that heuristic and systematic processing can exert both independent (i.e., additive) and interdependent (i.e., interactive) effects on judgment" (p. 328).

Third, with respect to motivation, both the ELM and the HSM assume that people are inclined to adopt correct attitudes. Unlike the ELM, however, the HSM goes on to specify other possible motivations that people might have. These include "defense-motivation" (e.g., Giner-Sorolla & Chaiken, 1997) and "impression-motivation" (e.g., Chen, Shechter, & Chaiken, 1996). As such, the HSM hypothesizes a broader scope of potential motivations (Eagly & Chaiken, 1993). Finally, in discussing a "sufficiency threshold," the HSM hypothesizes a mediator to explain when certain variables will enhance the processing and elaboration that is seen in the central or systematic routes. The ELM lacks this theoretical mediator (Eagly & Chaiken, 1993).

As interesting and important as these distinctions may be, however, we know of no research that validates the distinctiveness of the two theories. Evidence concerning substantive arenas in which one or the other of the two theories makes (subsequently confirmed) predictions that are not made by the other, while constituting theoretical advance, does not invalidate the other theory or provide discriminative theoretical validity in a strong sense. The latter requires instead evidence in support of one of two competing predictions generated by each theory. Obviously, the alleged theoretical distinctiveness will acquire meaning only when such evidence is produced.[10]

CONCLUSION AND RECOMMENDATIONS

We have argued here that science evidences a needless and dysfunctional proliferation of conceptual baggage, whether considered at the abstract level of theory development or constrained to the more specific level of individual concepts. Although Don Campbell's interests were primarily methodological and epistemological, his substantive research was concerned with issues in social psychology. Consequently, it is appropriate that the specific illustrative examples of questionable discriminative validity that we have presented were taken from subareas of social psychology. At the same time, however, one can ask whether in addressing an issue that seems to be an appropriate area of concern for those in social psychology, we are guilty of having generalized a local problem into a worldview by implying that it characterizes psychology or social science in general.

In large part, we drew our examples from areas of social psychology because we are more familiar with research in it; however, we believe that other areas are no less likely to exhibit these same problems. Sometimes, instances of the general issue arise much like differences between languages for the name of an object. Thus, when distinct scientific disciplines encounter evidence of the same phenomenological experience among a set of persons, they may assign distinctive labels to that which they observe. For instance, psychiatry and neurology repeatedly use different terms to describe fundamentally similar manifestations of language disorders. Specifically, in semantic disorders of aphasia, *driveling* is used in psychiatry, whereas *jargon augmentation* is used in neurology to refer to speech that is devoid of meaningful content yet characterized by tightly linked associations that are accompanied by a preservation of syntax. In nominal disorders, *word approximation* is used in psychiatry and *verbal paraphasia* is used in neurology to refer to instances in which words are used without reference to their precise meaning. In phonemic disorders, *clanging* is used in psychiatry and *literal phonemic paraphasia* is used in neurology to refer to situations in which associations between words occur on the basis of their sound, rather than their meaning.

Similar problems arise in the field of mental illness. That is, discriminative validity is an issue in differential diagnosis of psychopathology. For example, in defining categories of depression, the *Diagnostic and Statistical Manual of Mental Disorders* (third revision; DSM-III) differentiates major depression from dysthymia, even though their respective symptoms overlap to a substantial degree (van Praag, 1993). Mild cases of major depression are virtually indistinguishable from dysthymia. Similarly, it is unclear that severe dysthymia can be distinguished from major depressive episodes.

Taking another example, modern psychopathological nomenclature used in the DSM-IV (and the DSM-III as well) avoids use of the term *neurosis*—a term with a long history of use that started in the 18th century with respect to conditions such as hysteria, as well as types of depression. (Today, these same symptoms are considered to be anxiety disorders.) Later, Freud used the term *neurosis* to conceptualize and describe these, as well as other, related anxiety symptoms and traits. Despite the long-standing use of the terms *neurosis* and *neurotic* to characterize persons who suffer from fundamentally similar symptomatology indicative of anxiety (including heart palpitations, malaise, and neurasthenia), the term *neurosis* has been abandoned for use with anxiety disorders in DSM-IV. It is now replaced with a plethora of discrete diagnostic categories: distinct *phobias*, including social, simple, and agoraphobia; *panic disorder*, subtyped as occurring with or without agoraphobia; *obsessive-compulsive disorder*; and *post-traumatic stress disorder*. In assessing the need for such differentiation, one must ask whether the underlying processes that account for the development of distinct phobias and the conceptually related anxiety disorders mentioned above do indeed differ. Similarly, one must ask whether the optimal approaches for their treatment will differ. Given van Praag's (1993) documentation of countless other instances of such dubious differentiation among diagnostic categories in psychopathology, we have little reason to think that the problem of compromised discriminative construct validity is constrained to social psychology.

At the same time, the preceding discussion calls for some remedial recommendations. For whatever form of discriminative validity is of concern—trait, nomological, process, or theoretical—measures of rival, potentially similar, and potentially dissimilar concepts are needed within the context of individual studies. To provide evidence of the discriminative construct validity of the primary dependent measure, the results of the study should show that the manipulation of independent variables affects the measure of interest but not the theoretically similar, but allegedly distinct, secondary measures. Likewise, for experimental social psychology, manipulations of independent variables not only should be confirmed with relevant manipulation checks that assess the presence of the subjective state intended to be induced by the manipulation

but also should be accompanied by other manipulation check measures that assess rival states judged to be theoretically unrelated to the experimental manipulation. In an analogous manner, when dealing with discriminative process validity, researchers should include measurements of theoretically similar but rival process variables to show that when such variables serve as covariates, they do not affect the primary dependent measure to the same extent as the hypothesized process variable of interest. Such changes in practice need to become normative. For this to happen, they must be espoused, if not routinely required, by dissertation advisers, editors, and professional associations.

NOTES

1. Campbell and Fiske went on to argue that when trait A is assessed with any one type of measure, its average correlation with four other types of measures of A should exceed the correlation found among traits A, B, C, D, and E as measured with any single type of measure. Here, however (without meaning to diminish the importance of discriminative trait validity as evidenced by within-measure differences in the magnitude of within-trait versus cross-trait correlations), we question the general principle that is implicit in this specific stipulation. The issue here is the comparative importance of trait versus state variables. If traits are conceptualized as individual difference measures, as opposed to situational (or acute, or temporary) inductions of high or low levels of a state, then this latter requirement assumes that social psychological variables (situational variance) are less powerful or important than personality variables (individual difference variance). Taking instead the perspective of experimental social psychology, both trait and method can be conceptualized as consequences of experimenter action. That is, both can be operationalized as situational manipulations. From this perspective, then, why should the former "on average" be a stronger source of variance than the latter, when they both are the same thing—a consequence of situational variation? Even while retaining the conceptual distinction between personality and situation, however, there seems to be little logical or empirical justification for assuming that personality is a more potent source of explained variance than is situation. For instance, a striking feature of the work related to Milgram's obedience studies was the failure to find personality moderators of the basic situational effect that Milgram studied (Brown, 1986, p. 5).

2. We do not mean to imply here that they thought it was indeed sufficient. See, for instance, the next paragraph.

3. Specifically, Cook and Campbell (1979) discussed four separate types of validity: internal, external, statistical conclusion, and construct (specifically, construct validity of causes or effects).

4. We recognize that to whatever degree we are correct in identifying overlap between *cognitive dissonance* and *unbalanced triads*, Festinger's theory of cognitive dissonance nevertheless was broader than balance theory in that it elaborated specific antecedent conditions for producing the dissonant or unbalanced state (cf. Zajonc, 1968) that had

not been identified within balance theory. Moreover, it also specified alternative modes for reducing dissonance or unbalance.

5. The following discussion does not attempt to evaluate Kruglanski's lay epistemic theory. Instead, it is constrained solely to a consideration of individual measures (trait validation) of the need for closure. There is a substantial body of empirical research on issues related to the broader theory. (For recent discussions and summaries, see Cratylus, 1995; DeGrada, Kruglanski, Mannetti, Pierro, & Webster, 1996; Jamieson & Zanna, 1989; Kruglanski & Webster, 1996). In numerous studies, need for closure has been experimentally manipulated as an independent variable. Research has examined its relation to the correspondence bias (Webster, 1993), impression primacy effects (Webster & Kruglanski, 1994), persuasion (Kruglanski, Webster, & Klem, 1993), and stereotypical judgments (Dijksterhuis, van Knippenberg, Kruglanski, & Schaper, 1996).

6. Curiously, however, there was little relation between the NFCS and both the Personal Need for Structure Scale (r = .24) and the Personal Fear of Invalidity Scale (r = –.21), even though (a) several items from each of these scales (particularly the PNS Scale) had been adopted for use on the NFCS and (b) all three scales are supposed to be based on lay epistemic theory (Kruglanski, 1989).

7. The sex of participants in this unpublished study is not reported. Meta-analytic evidence shows that females respond to failure by turning it inward and expressing self-disparagement, rather than responding with hostility toward others (Bettencourt & Miller, 1996). Do these outcomes reflect results obtained (primarily) with female participants?

8. Paralleling our own ordered criteria (but omitting the subsequent approaches we present), they noted that necessary covariation (experimental evidence) provides weaker confirmatory evidence for process distinctiveness, with natural covariation (correlational evidence) the weakest evidence. After imposing their logical (or intuitive) analysis, they concluded that all but one of the 21 variables that they considered, at best, only naturally covary with minority/majority source status (or have no relationship at all). They argued, therefore, that process uniformity underlies minority and majority influence.

9. It is important to note that the point we make here could not affect their conclusion because their application of a logical analysis of the "likely effects" of relevant variables on minority/majority influence failed to yield a single instance of an interaction among the 21 variables that they considered.

10. Although not bearing directly on the issue of discriminative theoretical validity, Kruglanski and Thompson (1999) question whether the two types of process invoked by both theories should be considered as distinct processes. They contend instead that both messages and cues should be subsumed under the broader category of persuasive evidence. Thus, they question the discriminative process validity of the two modes/routes of persuasion by proposing that once differences on persuasively relevant informational parameters are controlled, cue-based and message-based persuasion should be affected similarly by relevant processing variables (e.g., motivation and cognitive capacity). They offer evidence for this contention in a series of studies. The advantage of this view in terms of parsimony is obvious.

REFERENCES

Ammons, C. H., & Ammons, R. B. (1962). *The Quick Test (QT).* Missoula, MT: Psychological Test Specialists.

Aronson, E. (1969). The theory of cognitive dissonance: A current perspective. In L. Berkowitz (Ed.), *Advances in experimental social psychology* (Vol. 4, pp. 1-34). New York: Academic Press.

Bacon, F. (1853). *The physical and metaphysical works of Lord Bacon* (J. Dewey, Trans.). London: Bohn. (Original work published 1620)

Bagby, R. M., Parker, J.D.A., & Bury, A. S. (1990). A comparative citation analysis of attribution theory and the theory of cognitive dissonance. *Personality and Social Psychology Bulletin*, *16*(2), 274-283.

Baker, R. C., & Guttfreund, D. G. (1993). The effects of written autobiographical recollection induction procedures on mood. *Journal of Clinical Psychology*, *49*(4), 563-568.

Baron, R. M., & Kenny, D. A. (1986). The moderator-mediator variable distinction in social psychological research: Conceptual, strategic, and statistical considerations. *Journal of Personality and Social Psychology*, *51*, 1173-1182.

Bettencourt, B. A., Brewer, M. B., Croak, M. R., & Miller, N. (1992). Cooperation and the reduction of intergroup bias: The role of reward structure and social orientation. *Journal of Experimental Social Psychology*, *28*, 301-319.

Bettencourt, B. A., & Miller, N. (1996). Gender differences in aggression as a function of provocation: A meta-analysis. *Psychological Bulletin*, *119*, 422-447.

Bieri, J. (1966). Cognitive complexity and personality development. In O. J. Harvey (Ed.), *Experience, structure and adaptability* (pp. 13-37). New York: Springer.

Bogen, J. E. (1975). Some educational aspects of hemispheric specialization. *UCLA Educator*, *17*, 24-32.

Brown, R. (1986). *Social psychology* (2nd ed.). New York: Free Press.

Brown, R. J., & Turner, J. C. (1981). Interpersonal and intergroup behavior. In J. C. Turner & H. Giles (Eds.), *Intergroup behaviour* (pp. 33-65). Oxford, UK: Blackwell.

Byrne, D. (1971). *The attraction paradigm.* New York: Academic Press.

Cacioppo, J. T., & Petty, R. E. (1982). The need for cognition. *Journal of Personality and Social Psychology*, *42*, 116-131.

Campbell, D. T. (1960). Recommendations for APA test standards regarding construct, trait, or discriminant validity. *American Psychologist*, *15*, 546-553.

Campbell, D. T. (1986). Science's social system of validity-enhancing collective belief change and the problems of the social sciences. In D. W. Fiske & R. A. Shweder (Eds.), *Metatheory in social science: Pluralism and subjectivities* (pp. 108-135). Chicago: University of Chicago Press.

Campbell, D. T. (1988). Convergent and discriminant validation by the multitrait-multimethod matrix. In E. S. Overman (Ed.), *Methodology and epistemology for social science: Selected papers* (pp. 37-61). Chicago: University of Chicago Press.

Campbell, D. T., & Fiske, D. W. (1959). Convergent and discriminant validation by the multitrait-multimethod matrix. *Psychological Bulletin*, *56*(2), 81-105.

Campbell, D. T., Miller, N., Lubetsky, J. & O'Connell, E. J. (1964). Varieties of projection in trait attribution. *Psychological Monographs*, *78*(15, Whole No. 592).

Cartwright, D., & Harary, D. (1956). Structural balance: A generalization of Heider's theory. *Psychological Review, 63*, 277-293.

Chaiken, S. (1980). Heuristic versus systematic information processing and the use of source versus message cues in persuasion. *Journal of Personality and Social Psychology, 39*, 752-756.

Chaiken, S. (1987). The heuristic model of persuasion. In M. P. Zanna, J. M. Olson, & C. P. Herman (Eds.), *Social influence: The Ontario symposium* (Vol. 5, pp. 3-39). Hillsdale, NJ: Erlbaum.

Chaiken, S., & Eagly, A. H. (1983). Communication modality as a determinant of persuasion: The role of communicator salience. *Journal of Personality and Social Psychology, 45*, 241-256.

Chen, S., Shechter, D., & Chaiken, S. (1996). Getting the truth or getting along: Accuracy- versus impression-motivated heuristic and systematic processing. *Journal of Personality and Social Psychology*, *71*, 262-275.

Conte, H. R., Weiner, M. B., & Plutchik, R. (1982). Measuring death anxiety: Conceptual, psychometric, and factor-analytic aspects. *Journal of Personality and Social Psychology, 43*, 775-785.

Cook, T. D., & Campbell, D. T. (1979). *Quasi-experimentation: Design and analysis issues for field settings.* Chicago: Rand McNally.

Cratylus. (1995). De Nederlandse Need for Closure schaal [The Netherlands Need for Closure Scale]. *Nederlands Tijdschrift Voor de Psychologie*, *50*, 231-232.

Cronbach, L. J. (1951). Coefficient alpha and the internal structure of tests. *Psychometrika, 16*, 297-334.

Cronbach, L. J. (1955). Processes affecting scores on "understanding of others" and "assumed similarity." *Psychological Bulletin*, *52*, 177-193.

Cronbach, L. J., & Meehl, P. E. (1955). Construct validity in psychological tests. *Psychological Bulletin, 52*, 281-302.

Crowne, D. P., & Marlowe, D. (1964). *The approval motive.* New York: Wiley.

DeGrada, E., Kruglanski, A. W., Mannetti, L., Pierro, A., & Webster, D. M. (1996). Un'analisi strutterale comparativa delle versioni USA e italiana della scala di "Bisogno di chiusura cognitiva" di Webster e Kruglanski [A comparative structural analysis of the U.S. and Italian versions of the "Need for Closure Scale" of Webster and Kruglanski]. *Testing, Psicometria, metodologia*, *3*, 5-18.

Dijksterhuis, A., van Knippenberg, A., Kruglanski, A. W., & Schaper, C. (1996). Motivated social cognition: Need for closure effects on memory and judgment. *Journal of Experimental Social Psychology, 32*, 254-270.

Driskell, J. E., & Mullen, B. (1990). Status, expectations, and behavior: A meta-analytic review and test of the theory. *Personality and Social Psychology Bulletin*, *16*, 541-553.

Duncan, O. T. (1984). *Notes on social measurement: Historical and critical.* New York: Russell Sage Foundation.

Eagly, A. H., & Chaiken, S. (1993). *The psychology of attitudes.* Fort Worth, TX: Harcourt Brace Jovanovich.

Eysenck, H. J. (1954). *The psychology of politics.* New York: Praeger.

Festinger, L. (1957). *A theory of cognitive dissonance.* Stanford, CA: Stanford University Press.

Festinger, L., & Carlsmith, J. M. (1959). Cognitive consequences of forced compliance. *Journal of Abnormal and Social Psychology, 58,* 203-210.

Forgas, J. P. (1995). Mood and judgment: The affect infusion model (AIM). *Psychological Bulletin, 117,* 39-66.

Forgas, J. P., & Fiedler, K. (1996). Us and them: Mood effects on intergroup discrimination. *Journal of Personality and Social Psychology, 70,* 28-40.

Freud, S. (1937). *The ego and mechanisms of defense.* London: Hogarth.

Gaertner, S. L., Dovidio, J. F., Anastasio, P. A., Bachman, B. A., & Rust, M. C. (1993). The common ingroup identity model: Recategorization and the reduction of intergroup bias. In W. Stroebe & M. Hewstone (Eds.), *European review of social psychology* (Vol. 4, pp. 1-26). New York: John Wiley & Sons.

Gilbert, D. T., & Hixon, J. G. (1991). The trouble of thinking: Activation and application of stereotypes. *Journal of Personality and Social Psychology, 60,* 509-517.

Gilbert, D. T., & Osborn, R. E. (1989). Thinking backwards: Some curable and incurable consequences of cognitive business. *Journal of Personality and Social Psychology, 57,* 940-949.

Giner-Sorolla, R., & Chaiken, S. (1997). Selective use of heuristic and systematic processing under defense motivation. *Personality and Social Psychology Bulletin, 23,* 84-97.

Greenberg, J., Pyszczynski, T., & Solomon, S. (1986). The causes and consequences of the need for self-esteem: A terror management theory. In R. F. Baumeister (Ed.), *Public self and private self* (pp. 189-212). New York: Springer-Verlag.

Greenberg, J., Pyszczynski, T., & Solomon, S., Rosenblatt, A., Veeder, M., Kirkland, S., & Lyon, D. (1990). Evidence for terror management theory II: The effects of mortality salience reactions to those who threaten or bolster the cultural worldview. *Journal of Personality and Social Psychology, 58,* 308-318.

Greenberg, J., Pyszczynski, T., Solomon, S., Simon, L., & Breus, M. (1994). The role of consciousness and accessibility of death-related thoughts in mortality salience effects. *Journal of Personality and Social Psychology, 67,* 627-637.

Greenberg, J., Simon, L., Harmon-Jones, E., Solomon, S., Pyszczynski, T., & Lyon, D. (1995). Testing alternative explanations for mortality salience effects: Terror management, value accessibility, or worrisome thoughts? *European Journal of Social Psychology, 45,* 417-433.

Greenberg, J., Simon, L., Pyszczynski, T., & Solomon, S. (1996). *Are mortality salience effects specific to reminders of mortality?: Actual and imagined failure versus mortality salience.* Unpublished manuscript, University of Arizona, Tucson.

Greenberg, J., Solomon, S., & Pyszczynski, T. (1997). Terror management theory of self-esteem and cultural worldviews: Empirical assessments and conceptual refinements. In M.E.P. Zanna (Ed.), *Advances in experimental social psychology* (Vol. 29, pp. 61-139). San Diego: Academic Press.

Gross, S. R., & Miller, N. (1997). The "golden section" and bias in perceptions of consensus. *Personality and Social Psychology Review, 1*(3), 241-271.

Harrington, H., & Miller, N. (1993). Do group motives differ from individual motives? Considerations regarding process distinctiveness. In M. A. Hogg & D. Abrams (Eds.), *Group motivation: Social psychological perspectives* (pp. 149-172). New York: Harvester Wheatsheaf.

Higgins, E. T., King, G. A., & Mavin, G. H. (1982). Individual construct accessibility and subjective impressions and recall. *Journal of Personality and Social Psychology*, *43*, 35-47.

Jamieson, D. W., & Zanna, M. P. (1989). Need for structure in attitude formation and expression. In A. Pratkanis, S. Breckler, & A. G. Greenwald (Eds.), *Attitude structure and function* (pp. 46-68). Hillsdale, NJ: Erlbaum.

Jordan, N. (1953). Behavioral forces that are a function of attitude and of cognitive organization. *Human Relations*, *6*, 273-287.

Judd, C. M., & Kenny, D. A. (1981). *Estimating the effects of social interventions*. New York: Cambridge University Press.

Kreuger, J., & Clement, R. W. (1997). Estimates of social consensus by majorities and minorities: The case for social projection. *Personality and Social Psychology Review*, *1*, 299-313.

Kruglanski, A. W. (1989). *Lay epistemics and human knowledge: Cognitive and motivational bases*. New York: Plenum.

Kruglanski, A. W. (1990a). Lay epistemic theory in social-cognitive psychology. *Psychological Inquiry, 1*, 181-197.

Kruglanski, A. W. (1990b). Motivations for judging and knowing: Implications for causal attribution. In E. T. Higgins & R. M. Sorrentino (Eds.), *The handbook of motivation and cognition: Foundations of social behavior* (Vol. 2, pp. 333-368). New York: Guilford.

Kruglanski, A. W., & Mackie, D. M. (1990). Majority and minority influence: A judgmental process analysis. In W. Stroebe & M. Hewstone (Eds.), *European review of social psychology* (Vol. 1, pp. 229-261). Chichester, UK: Wiley.

Kruglanski, A. W., & Thompson, E. P. (1999). Persuasion by a single route: A view from the unimodel. *Psychological Inquiry*, *10*(2), 83-109.

Kruglanski, A. W., & Webster, D. M. (1996). Motivated closing of the mind: "Seizing" and "freezing." *Psychological Review*, *103*, 263-283.

Kruglanski, A. W., Webster, D. M., & Klem, A. (1993). Motivated resistance to openness to persuasion in the presence or absence of prior information. *Journal of Personality and Social Psychology, 65*, 861-876.

Kuder, G. F., & Richardson, M. W. (1937). The theory of the estimation of test reliability. *Psychometrika*, *2*, 151-160.

Marco, C. A., & Suls, J. (1993). Daily stress and the trajectory of mood: Spillover, response assimilation, contrast, and chronic negativity. *Journal of Personality and Social Psychology, 64*, 1053-1063.

Miller, N., & Pollock, V. E. (1994). Meta-analysis and some science-compromising problems in social psychology. In W. R. Shadish & S. Fuller (Eds.), *The social psychology of science* (pp. 230-261). New York: Guilford.

Moscovici, S. (1980). Toward a theory of conversion behavior. In L. Berkowitz (Ed.), *Advances in experimental psychology* (Vol. 13, pp. 209-239). New York: Academic Press.

Mullen, B., Brown, R., & Smith, C. (1992). Intergroup bias as a function of salience, relevance, and status: An integration. *European Journal of Social Psychology*, *22*, 103-122.

Mullen, B., & Hu, L-T. (1988). Social projection as a function of cognitive mechanisms: Two meta-analytic integrations. *British Journal of Social Psychology*, *130*, 333-356.

Neuberg, S. L., Judice, T. N., & West, S. G. (1997). What the Need for Closure Scale measures and what it does not: Toward differentiating among related epistemic motives. *Journal of Personality and Social Psychology*, *72*, 1396-1412.

Neuberg, S. L., & Newsom, J. (1993). Individual differences in chronic motivation to simplify: Personal need for structure and social-cognitive processing. *Journal of Personality and Social Psychology*, *65*, 113-131.

Neuberg, S. L., West, S. G., Judice, T. N., & Thompson, M. M. (1997). On dimensionality, discriminant validity, and the role of psychometric analyses in personality theory and measurement: Reply to Kruglanski et al.'s (1997) defense of the Need for Closure Scale. *Journal of Personality and Social Psychology*, *73*, 1017-1029.

Pacini, R., Muir, F., & Epstein, S. (1998). Depressive realism from the perspective of cognitive-experimental self-theory. *Journal of Personality and Social Psychology*, *74*, 1056-1068.

Petty, R. E. (1994). Two routes to persuasion: State of the art. In G. d'Ydewalle, P. Eelen, & P. Berteleson (Eds.), *International perspectives on psychological science* (Vol. 2, pp. 229-247). Hillsdale, NJ: Erlbaum.

Petty, R. E., & Cacioppo, J. T. (1986). The elaboration likelihood model of persuasion. In L. Berkowitz (Ed.), *Advances in experimental social psychology* (Vol. 19, pp. 123-205). San Diego: Academic Press.

Repetti, R. L. (1993). Short-term effects of occupational stressors on daily mood and health complaints. *Health Psychology*, *12*, 125-131.

Richardson, M. W., & Kuder, G. F. (1939). The calculation of test-reliability coefficients based upon the method of rational equivalence. *Journal of Educational Psychology*, *30*, 681-687.

Rokeach, M. (1960). *The open and closed mind.* New York: Basic Books.

Rosenblatt, A., Greenberg, J., Solomon, S., Pyszczynski, T., & Lyon, D. (1989). Evidence for terror management theory I: The effects of mortality salience on reactions to those who violate or uphold cultural values. *Journal of Personality and Social Psychology*, *57*, 681-690.

Rosenthal, R. (1994). On being one's own case study: Experimenter effects in behavioral research—30 years later. In W. R. Shadish & S. Fuller (Eds.), *The social psychology of science* (pp. 214-229). New York: Guilford.

Roskos-Ewoldsen, D. R., & Fazio, R. H. (1992). The accessibility of source likability as a determinant of persuasion. *Personality and Social Psychology Bulletin*, *18*, 19-25.

Ross, L., Greene, D., & House, P. (1977). The "false consensus effect": An egocentric bias in social perception and attributional processes. *Journal of Experimental Social Psychology*, *13*, 279-301.

Sanford, R. N., Adorno, E., Frenkel-Brunswik, E., & Levinson, D. J. (1950). The measurement of implicit antidemocratic trends. In E. Adorno, E. Frenkel-Brunswik, D. J. Levinson, & R. N. Sanford (Eds.), *The authoritarian personality* (pp. 222-279). New York: Harper & Row.

Scher, S. J., & Cooper, J. (1989). Motivational basis of dissonance: The singular role of behavioral consequences. *Journal of Personality and Social Psychology*, *56*, 899-906.

Schopler, J., & Insko, A. (1992). The discontinuity effect in interpersonal and intergroup relations: Generality and mediation. In W. Stroebe & M. Hewstone (Eds.), *European review of social psychology* (Vol. 3, pp. 121-152). New York: John Wiley & Sons.

Sigall, H., & Mills, J. (1998). Measures of independent variables and mediators are useful in social psychology experiments: But are they necessary? *Personality and Social Psychology Review*, *2*, 218-226.

Solomon, S., Greenberg, J., & Pyszczynski, T. (1991). A terror management theory of social behavior: The psychological functions of self-esteem and cultural worldviews. In M.E.P. Zanna (Ed.), *Advances in experimental social psychology* (Vol. 23, pp. 91-159). San Diego: Academic Press.

Steele, C. M., Spencer, S. J., & Lynch, M. (1993). Self-image resilience and dissonance: The role of affirmational resources. *Journal of Personality and Social Psychology*, *64*, 885-896.

Stephan, W. G., & Stephan, C. W. (1985). Intergroup anxiety. *Journal of Social Issues*, *41*, 157-175.

Stephan, W. G., & Stephan, C. W. (1996). *Intergroup relations.* Dubuque, IA: Brown & Benchmark.

Tellegen, A. (1982). *Brief manual for the multidimensional personality questionnaire.* Unpublished manuscript.

Thompson, M. M., Naccarato, M. E., & Parker, K. E. (1989, June). *Assessing cognitive need: The development of the Personal Need for Structure and Personal Fear of In - validity scales.* Paper presented at the annual meeting of the Canadian Psychological Association, Halifax, Nova Scotia, Canada.

Thompson, M. M., Naccarato, M. E., Parker, K. E., & Moskowitz, G. (1993). *The development and validation of the Personal Need for Structure (PNS) and Personal Fear of Invalidity (PFI) measures.* Unpublished manuscript, New York University.

Travers, R.M.W. (1941). A study in judging the opinions of groups. *Archives of Psychology* (Whole No. 266).

Urban, L. M., & Miller, N. (1998). A theoretical analysis of crossed categorization effects: A meta-analysis. *Journal of Personality and Social Psychology*, *74*, 894-908.

van Praag, H. M. (1993). *"Make-believes" in psychiatry or the perils of progress.* New York: Brunner/Mazel.

Wallen, R. (1943). Individuals' estimates of group opinion. *Journal of Social Psychology*, *17*, 269-274.

Webster, D. M. (1993). Motivated augmentation and reduction of the overattribution bias. *Journal of Personality and Social Psychology*, *65*, 261-271.

Webster, D. M., & Kruglanski, A. W. (1994). Individual differences in need for cognitive closure. *Journal of Personality and Social Psychology*, *67*, 1049-1062.

Wegner, D. M. (1994). Ironic processes of mental control. *Psychological Review, 101*, 34-52.

Wilder, D. A. (1993). The role of anxiety in facilitating stereotypic judgments of outgroup behavior. In D. M. Mackie & D. L. Hamilton (Eds.), *Affect, cognition, and*

stereotyping: Interactive processes in group perception (pp. 87-109). San Diego: Academic Press.

Wilder, D. A., & Shapiro, P. (1989). Role of competition-induced anxiety in limiting the beneficial impact of positive behavior by an outgroup member. *Journal of Personality and Social Psychology, 56*, 60-69.

Wilder, D. A., & Shapiro, P. (1991). Facilitation of outgroup stereotypes by enhanced ingroup identity. *Journal of Experimental Social Psychology, 27*, 431-452.

Wilder, D. A., & Simon, A. F. (1996). Incidental and integral affect as triggers of stereotyping. In R. M. Sorrentino & E. T. Higgins (Eds.), *Motivation and cognition: Vol. 3. The interpersonal context* (pp. 397-422). New York: Guilford.

Word, C. O., Zanna, M. P., & Cooper, J. (1974). The nonverbal mediation of self-fulfilling prophecies in interracial interaction. *Journal of Experimental Social Psychology, 10*, 109-120.

Zajonc, R. B. (1968). Cognitive theories in social psychology. In G. Lindzey & E. Aronson (Eds.), *The handbook of social psychology* (Vol. 1, pp. 320-412). Reading, MA: Addison-Wesley.