Assessing Process Distinctiveness

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In considering impediments to scientific progress in contemporary social psychology, Miller and Pollock (1994) discussed six major issues: ignoring or rejecting theoretically obvious findings, substantively selective citation, positive bias in hypothesis and theory confirmation, and journal publication policies. First on the list, however, was inventing new names for old concepts. Psychologists appear prone to "rediscovering the wheel," examining something well-studied in the past, and then attaching an idiosyncratic label to it so as to give it a more distinctive (and self-referring) quality than it might otherwise have had. In the same volume, Rosenthal (1994), perhaps somewhat ironically (but certainly with a nice "turn of phrase"), applied the new term *concept capture* to discuss this issue. The technical term for the achievement of valid conceptual differentiation is *discriminative construct validity* (Campbell & Fiske, 1959).

The Problem of Concept Capture

Contemporary social psychology is rife with implicit, but unsubstantiated, claims of discriminative construct validity. For instance, *assumed similarity,* the tendency to exaggerate similarity between self and others in attitudes, personality traits, interests, and values, is such a reliable main effect (e.g., Gross & Miller, 1997; Mullen & Hu, 1988) that one can routinely count on it in classroom demonstrations. Francis Bacon (1620/1853), describing prominent biases in human social perception, spoke of projecting one's own worldview onto others. In his discussion of paranoia and the defense mechanism of projection, Freud (1937), too provided an instance from the domain of personality trait attribution. Within scientific psychology, research on it is so extensive that incisive suggestions concerning its quantitative analysis were raised over three decades ago (e.g., Cronbach, 1955).

In the late 1970s Ross, Greene, and House (1977) introduced the *false consensus effect* (FCE), defined as the difference in consensus estimates by those agreeing with and opposing an opinion position. Elsewhere, Gross and Miller (1997) showed that data on the FCE and data on the difference between estimated and true consensus (viz., data on assumed similarity) are inextricably linked, being facets of the same data set. Yet, in referring to previous work related to the FCE, its entirety is mentioned once by Ross et al. (1977) as follows:

> References to egocentric attribution (Heider, 1958; Jones & Nisbett, 1970) to attributive projection (Holmes, 1968) and to specific findings and phenomena related to false consensus biases have appeared sporadically in the social perception and attributive literatures (cf. Katz & Allport, 1931; Kelley & Stahelski, 1970).

To put this acknowledgment of prior relevant work into perspective, Miller and Pollock (1994) listed over 150 references that were concerned with this bias and had been published prior to 1977, some of which (e.g., Travers, 1941; Wallen, 1943) had used an experimental paradigm identical to that for the FCE. Within this extensive literature, more than 15 distinct labels were used for discussing what is apparently a single underlying concept—in that no data have been presented by Ross, or anyone else, to suggest that FCE, assumed similarity, or any of the other 15 or more labels obey laws that differentiate one member of this family of terms from another.

Is this an isolated example? No. In the area of clinical and cognitive psychology Erdelyi (1990) noted that theorists have used various terms interchangeable with *repression.* Bogen (1975) listed 39 scholars, each of whom has used a distinct pair of labels for individual differences in narrow- versus broad-minded (focused vs. general) predispostions for processing experience.

Within social psychology, the distinction between cognitive dissonance and other terms more common in ordinary parlance remains unclear. For instance, Scher and Cooper (1989) claimed that cognitive dissonance is aroused when one causes an event that has aversive outcomes for others, claiming it consists of a sense of *impaired self-efficacy.* How this state differs from those described by terms such as *responsibility, guilt,* or *feeling bad* has not been examined. Moreover, *self-affirmation,* as described by Steele (1988), may be merely one among the several distinct means by which actors reduce dissonance (Festinger, 1957).

Aggression researchers have argued that verbal and physical aggression are conceptually distinct (e.g., Tedeschi, 1983; Tedeschi & Quigley, 1996; Tedeschi, Smith, & Brown, 1974). However, meta-analytic evidence showed that for each of four well-established precursors of aggressive behavior, verbal and behavioral indexes of aggression exhibited parallel functional relations (Carlson, Marcus-Newhall, & Miller, 1989). This argues against discriminative construct validity for the two forms of aggression (Giancola & Chermack, 1998). In the related area of alcohol and ag-

gression, Steele and Southwick (1985) proposed the notion of *conflict inhibition,* arguing that alcohol increases aggression by augmenting conflict inhibition. Yet, within a meta-analysis that examined moderators of the effect of alcohol consumption on aggression, conflict inhibition correlated with anxiety to the limits of their respective reliabilities (Ito, Miller, & Pollock, 1996)!

Assessing Discriminative Process Validity

Additional examples can readily be given. We turn instead to the related concern of Kruglanski and Thompson, discriminative process validity. Specifically, they ask whether the Elaboration Likelihood Model (ELM; Petty & Cacioppo, 1986) and the Heuristic Systematic Model (HSM; Chaiken, 1980, 1987) models of persuasion are correct in their assertion that two distinct processes underlie persuasion effects—that the effects of source cues and message attributes reflect different underlying processes. They argue instead that the two types of variables, source and message, rely on the same processes to produce their effects. Although concept capture may have the positive features of calling attention to a research area, inspiring research on it, and thereby increasing understanding, nevertheless, its occurrence constitutes excess conceptual baggage. Likewise, although an amplified differentiation of processes underlying a theoretical account may typically be a genuine intellectual attempt to provide conceptual understanding, in the absence of confirming evidence of discriminative process validity it too constitutes excess conceptual baggage. More generally, raising concern about these issues calls attention to the stronger focus of contemporary scientific social psychology on differentiation among concepts or principles than on their integration. Whereas analysis and differentiation is indeed an appropriate and important goal for scientific progress, false differentiation is not.

The issue of discriminative process validity is more subtle and complex than is discriminative construct validity. On the one hand, process assessment requires that one establish the relations between (a) the situation, as operationalized, and the outcome; (b) the situation and the process; and (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. The first three, being based on differences in outcomes and not including process measures or manipulations, are weak.

Differences in Outcome

1. Ecological validity. Ecological validity, or natural covariation, is the most primitive approach. To illustrate, 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 a member 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, it 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 process, it cannot strongly confirm it.

2. Experimentation. Experiments provide circumstances for stronger inference, but 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. However, the experience of differential threat within the two settings may provide a single-process explanation that, if controlled, will eliminate the effect.

3. Interactions. The previous approach can be extended by experimentally examining interactions to assess whether the effect of relevant independent variables on behavior 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, cognitive overload may induce negative affect (Marco & Suls, 1993; Repetti, 1993). If so, the two variables, ordinarily believed to be conceptually distinct, will yield parallel effects because they induce a shared underlying state.

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, it would support process uniformity for the two allegedly distinct settings. By contrast, if they produce opposing directions of effects in the two settings, it would support process distinctiveness. However, because the scaling characteristics of levels of an experimental induction or a measure can affect the slope of a relation, the case for process distinctiveness is strong only when a statistical interaction that is disordinal (a crossover interaction) is obtained.

Kruglanski and Mackie (1990) invoked the presence or absence of statistical interactions to assess Moscovici's (1980) claim that distinct processes underlie numerical majority and minority influence. Specifically, they argued that an interaction between Variable *X* and minority or majority group status would offer the strongest evidence for process distinctiveness.[1] However, although it 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), nevertheless, in their interpretive logic 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. Therefore, noncrossover interactions cannot be viewed as diagnostic of discriminative process validity.

For example, imagine an experiment that manipulated distraction (high vs. low) and the number of persuasive arguments, with 10 in the "low" and 20 in the "high" condition, respectively. Assume further that 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 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 impacted the two argument conditions differently, causing a large decrease in persuasion in one case but not the other. However, one can also argue 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. As such, increases in the value of a physical stimulus 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 number of arguments is high or low. Taking these assumptions and tying them to the psychophysical principle stated earlier, under the high argument-number condition the difference between the 20 and 10 arguments processed by participants sub-

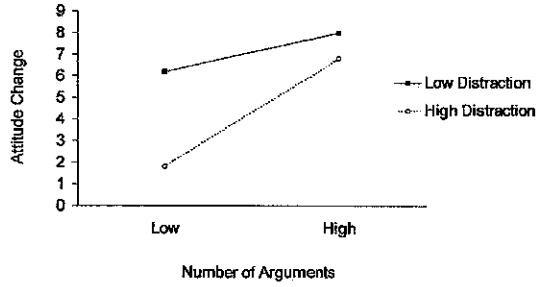

Figure 1. *A hypothetical interaction effect between distraction and number of arguments that need not be interpreted as reflecting distinct distraction processes when arguments are few versus many.*

[1] Paralleling our own ordered criteria (but omitting the approaches we present in the Direct Examination of Process section) 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 1 of the 21 variables that they considered, at best, only naturally covary with minority–majority source status (or have no relation at all). Therefore, they argued that process uniformity underlies minority and majority influence.

jected to low (zero) and high distraction, respectively, is not as consequential (in terms of its effect of decreasing persuasion) as is the decrease from 10 to 5 arguments that will be produced in the low argument-number condition by these same levels of distraction.

Direct Examination of Process

4. Correlational analysis of process. Stronger approaches to establishment of process distinctiveness will link process both to antecedent and 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 dependent measures of interest.

5. Combined correlational and experimental analysis. Statistical mediational assessment (Baron & Kenny, 1986; Judd & Kenny, 1981) is another approach now routinely used 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 the 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. Moreover, 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 (described earlier) may simultaneously affect both the key dependent measure as well as the alleged measure of process. 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- 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, will never be diagnosed as the critical underlying process event.

6. Experimental analysis of process. The previous approach 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 three experimental components: the effect of the antecedent situation on the process; the effect of the antecedent situation on the key dependent variable; and additionally, a direct manipulation of the process so as to experimentally (rather than correlationally) examine its effect on the key dependent variable.

7. Meta-analytic process analysis. The use of meta-analytic synthesis of experimental procedures that incorporate the approaches described under the previous two headings will provide the strongest evidence on process uniformity or distinctiveness (see Driskell & Mullen, 1990, for a meta-analytic approximation of the Combined Correlational and Experimental Analysis section). A most important addition to this meta-analytic amalgam, however, and (as has been suggested) one that is imperative for aforementioned approaches, as well, 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 each explanatory processes will contribute to differential statistical confirmation of their explanatory strength, as assessed by the Kenny and Judd (1981) procedures. In turn, this can lead to erroneous inferences concerning the relative explanatory power of the (rival) processes they are assumed to tap.

Message and Source Processes Underlying Persuasion

Having discussed approaches to assessing discriminant process validity, we can now examine the strength of Kruglanski and Thompson's argument. As indicated, they make a fundamental criticism of the dual-process feature of the ELM (Petty & Cacioppo,

1986) and HSM (Chaiken, 1980, 1987) models of persuasion by disputing their qualitative distinction between message arguments (central/systematic route) and cues (peripheral/heuristic route). They argue instead that both messages and cues should be subsumed under the broader category of persuasive evidence. Thus, they question the discriminant process validity of the two modes or routes of persuasion by proposing that once differences on persuasively relevant informational parameters are controlled, cue-based and message-based persuasion will be impacted similarly by relevant processing variables (e.g., motivation and cognitive capacity).

Specifically, in a series of four studies, they examined the effect of expertise and involvement (Study 1); expertise and cognitive capacity or load (Study 2); expertise, load, and length of cue information (Study 3); and involvement combined with the persuasive strength of both brief initial arguments and more detailed subsequent arguments (Study 4). They concluded that when informational length and complexity are controlled, thereby removing confounds, theoretically relevant variables such as motivation and cognitive capacity will interact with cues in a manner similar to their exhibited interaction with message arguments in past research.

Design Strength

We think Kruglanski and Thompson provide a provocative and cleverly insightful reconceptualization. From our perspective, scientific parsimony (and hence, their parallel processes model for source and message characteristics) clearly should be the default position—to be abandoned only when forced to by convincing data. Nevertheless, we can raise some criticisms of the specific work that they present. For instance, Petty, Cacioppo, and Goldman (1981), which they cite as the pinnacle of confirmatory evidence for the dual-process model, showed that argument quality (i.e., the message) more strongly affected persuasion under conditions of high relevance (involvement), whereas expertise (i.e., the cue) had greater persuasive effect in the low-relevance condition. Kruglanski and Thompson argue that this occurred both because the expertise information was presented first and, more important, was easier to process. By contrast, because the argument information was more complex and difficult to process, it only became persuasive under conditions of high involvement. Despite the support for their view provided by their four studies, if complexity is of central importance, a more direct test will examine persuasive effects when the expertise information is made more complex than the arguments (and the argument information perhaps is presented first). Under these conditions, if the expertise information were shown to be the key component in the high-involvement condition, whereas the argument quality was more important under low involvement, their parallel processes model would more strongly be supported.

Likewise, they make the well-taken argument that the typical confounding of cue information with early positioning increases its likelihood of being processed under low involvement (whereas under high involvement, processing remains likely despite its late positioning). Yet, none of their studies manipulated the ordinal position of cues and arguments.

Strength of Process Assessment

Such criticisms of "omission," however, should not be viewed as strong evidence against their common-processes perspective. At the same time, although the outcomes of their four studies are consistent with their perspective, it is important to consider the strength of their evidence in terms of the approaches to discriminative process validity outlined earlier. Taking their four studies as a group, they use a combination of portions of the approaches discussed earlier. As suggested in our prior discussion, a reasonably strong assessment of discriminative process validity will involve separate sets of analyses wherein the proper comparisons needed for discriminant process analysis can be examined. The first stage is to ascertain the impact of antecedent variables on what are considered to be process variables (as well as the key dependent variable), whereas the second is to assess experimentally the impact of these process variables on the outcome measure (i.e., the key dependent variable). To use Study 2 as an example, the authors performed the second stage, which is the manipulation of a process variable, by investigating the interaction between (a) distraction or cognitive capacity (i.e., the process variable) and (b) lengthy cues that indicated either high or low source expertise. Note, however, that their "examination of interactions," is only implicit. That is, the needed parallel set of conditions (i.e., high- vs. low-quality arguments that in length and complexity equaled that of the cues) were omitted from the design. Even had they been included, however, it may be troublesome that support for their common-process view rests on confirmation of the null hypothesis—the absence of the (implicit) interaction—requiring assurance of adequate power.

Separate from the manipulation of the process (so as to examine its causal effect on the dependent measure of interest) is examination of the first step of the causal chain by developing dependent measures that assess the process, so as to directly determine how key independent variables within the theory affect it. Although some might quibble with the degree to which it is an ideal measure of the underlying process variable, their

subjective self-report manipulation checks (Studies 2, 3, and 4) on the distraction manipulation do qualify as a process measure of cognitive capacity. Again, however, the relevant comparison (and its expected null effect) is only implicit in that the confirmation of diminished capacity under the lengthy cue information of Study 2, for instance, is not directly shown. That is, there is no comparison condition of short cue information under which the distraction manipulation check measure is shown to have no effect. Again we are left with an incomplete picture.

In summary, in their inclusion of manipulation checks of cognitive capacity they have used a process measure, but their designs and analyses preclude assessment of the differential process alleged by the ELM and HSM models with respect to cues versus message information. Moreover, this omission prevents use of the statistical mediational procedures discussed earlier.

Ecological Considerations

A final issue is ecological validity. Kruglanski and Thompson assume that heuristic cues and message arguments need not differ in their complexity, difficulty, or length. Moreover, they argue that (a) it is difficult to ascertain whether or not they do outside of the lab; and (b) were such assessment possible, there is little reason to believe that such differences will be evidenced. Finally, they maintain that cues might in some cases be more complex than arguments.

It is certainly reasonable to assume that on a continuum of complexity, the distributions of cues and arguments will overlap. A relevant question for real-world environments, however, is whether arguments do tend on average to be more complex than cues. Reliance on expertise as a major "cue variable" is a choice that is very amenable to manipulations of length and complexity that parallel complexity of message information. However, many other cue variables, such as group size, ethnicity, race, gender, language accent, religion (as cued by the wearing of religious icons such as a cross), and physical attractiveness, are manifestly apparent from the initial sight of, or a short exposure to a potential source of influence, and hence, cannot as readily be made more complex. From this perspective, whatever the merits of Kruglanski and Thompson's argument, the ELM and HSM distinction between cue and message variables is also valid.

Conclusion

We applaud Kruglanski and Thompson for their insightful theoretical contribution. From the point of view of parsimony, we think it an advancement worthy of continued exploration and experimentation. At the same time, we believe that attention to the concerns raised herein will further strengthen their argument.

Note

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