On The Thoughtfulness of Cognitive Psychologists

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To take an example, if the so-called cognitive approaches are mistaken — *and they are* — the experimental analyst of behavior should be sufficiently well equipped to know (i) what such approaches entail, (ii) why they have been so influential in recent years, (iii) what are the scientific problems they address, (iv) *why they are mistaken,* and (v) how the experimental analysis of behavior can better address those questions. (EAHB Programs, 1983, p. 4, emphasis added)

The above quotation was taken from a description of the experimental program in the Auburn University psychology department that was published in the *Experimental Analysis of Human Behavior Bulletin.* The hubris of this statement will probably serve, depending on their temperaments, to either amuse or irritate those who refer to themselves as "cognitive psychologists." However, this quotation, and our reactions to it, illustrate the essential incompatibility that many "cognitive" and "behavioral" psychologists perceive between their respective approaches. The series of papers by Tageson, Rychlak, Slife, Williams, and Lamiell and Durbeck has, therefore, undoubtedly elicited a reaction of its own — from both groups. For the central message of these papers is that cognitive psychology, as presently practiced, is not fundamentally different from behavioral psychology. In this comment I will present two responses. First, I will argue that these authors view the field of cognitive psychology too narrowly and that much of the work in this field does represent a radical departure from behavioral approaches. Second, I will explore some of the methodological issues raised by these papers, with particular reference to Lamiell and Durbeck.

Are Cognitive Psychologists Closet Behaviorists?

It is clear that the authors intend their critique to apply very broadly to the field of cognitive psychology. For example, in introducing these papers Tageson implies that they concern "expectations, attributions, personal constructs, cognitive schemata, learned strategies, and the like." Likewise, Rychlak refers generally

to schemata and scripts. However, with the exception of Lamiell and Durbeck's concern with the role of cognitive prototypes in impression formation, the papers, in fact, are more narrowly focused on what might be called traditional experimental cognitive psychology. Thus, Rychlak is concerned with the shortcomings of models based on artificial intelligence, Williams focuses on the theories of Broadbent (1958), Kahneman (1973), Norman (1968), and Shiffrin and Atkinson (1969), and Slife discusses "metacognitive" control functions in terms of cybernetic models.

The contention that these approaches are based on the same mechanistic model that underlies behaviorism and that, as Slife has stated, "Cognitive psychology has merely succeeded in elongating the causal chain of traditional behaviorism" is an important and well-taken point. However, it probably is a point that is not very troubling to many experimental cognitive psychologists because, as even these papers acknowledge, extending the causal chain into the "black box" is not trivial. Moreover, it does represent a substantial departure from traditional behaviorism (see Anderson and Bower, 1973, Chapter 2) even though it may not, in Williams's terms, be a "radical" one.

What is troubling is the implication that this characterization applies to all of what is called cognitive psychology. In fact, many cognitive approaches, especially those that have been labeled "social cognition," do reflect dialectical and teleological principles. Specifically, it has long been recognized that individuals actively construct their reality. More recently, there has been a growing consensus that these constructions are consequential, and that these constructions may be based, at least in part, on imagined criteria.

The idea that individuals construct their reality is, to pick one example, central to George Kelly's (1955) personal construct theory. The central tenet of this theory is that the same empirical observations may be interpreted very differently by observers, depending on their idiosyncratic personal experiences. In addition, this philosophy of constructive alternativism can be found in Mischel's (1973) "cognitive-behavioral" reconceptualization of personality, in which he argued that the most important individual difference variables are the subjective perceptions, expectations, interpretations, and values that effect people's reactions to events. More recently, Bem (1983) has argued that, "The traditional inference of [behavioral] inconsistency is not an inference about individuals, but a statement about a disagreement between a group of individuals and an investigator over which behaviors and situations may be properly classified into common equivalence classes" (p. 568).

There is also substantial empirical evidence that individuals construe events differently. Anecdotally, in their classic study of cross-situational consistency, Bem and Allen (1974) had to eliminate one of their measures because their intuition that keeping one's room clean was indicative of conscientiousness was not shared by their subjects. More compelling evidence comes from Buss and Craik's (1983) work on the act-frequency model of personality description. In

this study they asked subjects to rate the meaningfulness (prototypicality) of 100 behavioral acts as indicants of each of six traits. Overall, the consistency of the ratings was quite high. The alpha index of internal consistency for the aggregate ratings ranged from .77 for the trait of agreeable to .97 for the trait of aloof. However, the average degree of agreement between any two raters was much lower, ranging from .12 for agreeable to .44 for quarrelsome. Studies of both verbal (e.g., Bartlett, 1932) and object (e.g., Wulff, 1922) memory have also repeatedly documented the constructive nature of these cognitive activities.

Accepting that cognitive activity is constructive leads to the conclusion that individuals are not passive recorders of environmental stimuli. Instead they give meaning to their perceptions, at least in part, on the basis of internal criteria. However, Rychlak argues that such internal criteria are typically viewed as the product of past associations. That is, constructive activity can, and has (e.g., Brown and McNeill, 1966), been explained by referring to cognitive networks that are based on the frequency or intensity of the links among past experiences. Thus, constructivist theories are not necessarily at odds with the mechanistic component of behaviorism. However, much work in cognitive psychology also suggests that such constructions are consequential. The importance of this realization is that it provides a rationale for purposive (teleological) constructive activity.

In 1978 Golding wrote, "The so-called objective-behavioral criteria against which we have tested most personality-social-clinical hypotheses are either questionably relevant or, more modestly, deserving of considerably less respect than they are typically afforded. Constructs about personality-social-clinical phenomena entail psychological criteria that are inextricably bound to the psychological reality of the perceiver and are not easily reducible to objective-behavioral criteria" (p. 71). In a similar vein, Nisbett and Ross (1980) wrote, "Harmonious social interaction often may depend more on consensus about the world than on accuracy. It does not matter whether both of us call a spade a spade or both of us call it a shovel or a spoon, as long as each knows what the other means and agrees to use the implement in the same way" (p. 267). Both of these quotations clearly emphasize that the manner in which people construct reality may be at least as important as reality itself.

Evidence is now accumulating that supports this belief. Consider, for example, the pervasive finding that depressed individuals tend to view themselves and their actions "realistically," whereas the views of nondepressed individuals are unrealistically positive (Alloy and Abramson, 1979; DeMonbreun and Craighead, 1977; Lewinsohn, Mischel, Chaplin, and Barton, 1980; Nelson and Craighead, 1977; Tennen and Herzberger, 1987). Not only does this research demonstrate the consequential nature of an individual's construal styles, but it also documents a situation in which it is the distortion of reality that leads to psychological well-being. It is hard to explain such distortions without resorting to teleological principles. Other domains in which the construction of reality is recognized as

consequential are self and other personality description (Hampson, 1982), anxiety (Alden, 1987), reciprocity in friendships (Rook, 1987), and self-efficacy (Bandura, 1977).

All of this work emphasizes that the manner in which individuals process information may impact their psychological well-being, motivation, and interpersonal behavior. It is difficult to reconcile these views with behavioristic principles. Indeed, it is by postulating a purpose that one can, for example, understand the distortions of non-depressed individuals or the sometimes inaccurate perceptions of reciprocity in friend pairs. However, it is still possible that these consequential constructions result entirely from demonstrative comparisons. If so, then there is still a residue of behaviorism left in these theories, although I do not see how any behaviorist could endorse them. Nonetheless, there are now a number of examples of cognitive theories and research that clearly reflect dialectical reasoning and imagined standards. The existence of such work is certainly contrary to the authors' contention that cognitive psychology is behaviorism one step removed.

One example is the recent article by Markus and Nurius (1986) in which they introduce the concept of "possible selves." Such selves "derive from representations of the self in the past and they include representations of the self *in the future*" (p. 954). These future selves, "represent specific individually significant hopes, fears, and *fantasies*. I am now a psychologist, but could be a restaurant owner, a marathon runner, a journalist, or the parent of a handicapped child" (p. 954, emphasis added).

A second example is Kahneman and Tversky's (1982) "simulation heuristic," which, although expressed in the traditional language of cognitive psychology, clearly reflects dialectical principles. "Our starting point is a common introspection: there appear to be many situations in which questions about events are answered by an operation that resembles the running of a simulation model. The simulation can be constrained and controlled in several ways: the starting conditions for a 'run' can be left at their realistic default values *or modified to assume some special contingency* . . ." (p. 201, emphasis added). Kahneman and Tversky suggest that this heuristic may be used in a variety of judgmental contexts, including prediction, assessing the probability of a specified event, assessing conditional probabilities, counterfactual assessments, and assessments of causality. Their work on the simulation heuristic has thus far focused on judgments of an event that "was close to happening" or "nearly occurred." "The spatial metaphor is compelling and has been adopted in many philosophical investigations: it appears reasonable to speak of the distance between reality *and some once-possible but unrealized world*" (p. 203, emphasis added).

A third example is Shepard's (1975, 1978) work on mental transformations. "The evolution of a mental faculty for the simulation of spatial transformations would seem inexplicable if it had no purpose other than an idle, ephemeral, and epiphenomenal play of sensory shadows for one's purely private delectation. In fact, however, situations of practical moment or even urgency do arise in

which one can avoid unnecessary effort . . . by mentally trying out a proposed rearrangement . . . before undertaking the arrangement in physical reality" (Shepard and Cooper, 1982, pp. 1-2). In introducing their work Shepard and Cooper note that the 19th century empiricists did have some appreciation for the mind's ability to create imaginary forms from its contents. "To join incongruous shapes and appearances costs the imagination no more trouble than to conceive the most natural and familiar objects" (Hume, 1748/1907, Section II, p. 15). "[E]quipped with the awareness of the physical form of an object, we can clearly imagine all the perspective images which we may expect upon viewing from this or that side, *and we are immediately disturbed when such an image does not correspond to our expectations*" (Helmholtz, 1894; translated in Warren and Warren, 1968, pp. 254, emphasis added).[1]

Other examples of cognitive theories that have dialectical components are Ellis' (1974) rational-emotive theory, which postulates irrational goals and beliefs, Higgins, Strauman, and Klein's (1986) multiple stage theory of self-evaluation, which includes imagined or fantasized standards, and Loftus' (1979) model of eyewitness testimony that includes a mechanism for the actual modification of material in memory.
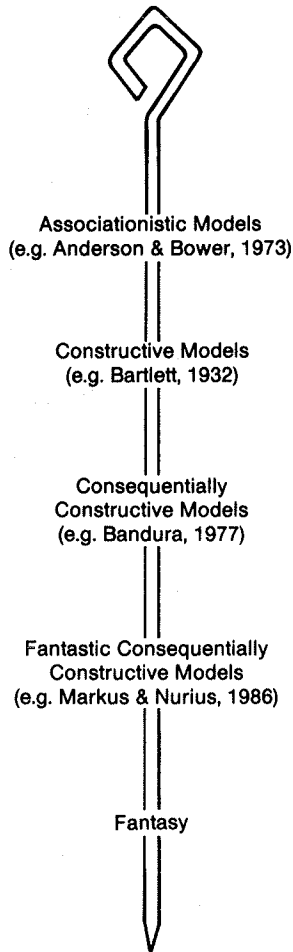
In summary, I agree with the authors of these papers that cognitive theories need to postulate purposive and dialectical mechanisms. However, the authors' critique of cognitive psychology focuses on a narrow range of theories and thus strikes me as largely a strawperson argument. In this section I have pointed to a great deal of work in cognitive psychology that is radically different from behavioral approaches. Another way of summarizing my position is to note that the extent to which cognitions are thought to be tied to experience is a matter of degree. The authors have focused their critique on cognitive models for whom the link is reasonably direct, and have offered instead models in which the link is quite indirect. Between these two extremes are a variety of models, most of which the authors have overlooked. Figure 1 presents a pointed illustration of this continuum and my classification of some of the approaches discussed in this section. More succinctly: the authors have more friends out here than they seem to think, although this situation may change if they continue to characterize us as closet behaviorists.

Methodological Issues

Once we propose cognitive models based on dialectical and teleological principles we need to consider how to evaluate them. The difficulty is that we

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[1]Lest I set Hume and Helmholtz spinning like lathes, I want to emphasize that I do not intend to imply that they believed that the contents of the mind were the product of anything but experience. However, these quotations do suggest that these empiricists were willing to accept the mind's ability to act on its contents, once the contents were stored. Although they are mute about the fate of these mental creations, if these creations could also be written on the *tabula*, then the extreme empirical position with which they are credited seems undermined.

Figure 1. Cognitive kebabs on a behavioral skewer.

Associationistic Models
(e.g. Anderson & Bower, 1973)

Constructive Models
(e.g. Bartlett, 1932)

Consequentially
Constructive Models
(e.g. Bandura, 1977)

Fantastic Consequentially
Constructive Models
(e.g. Markus & Nurius, 1986)

Fantasy

Note: Even Fantasy has been skewered, though only just, and there is some space between the handle and the Associationistic Models, though not much.

must devise methods that do not presume an isomorphic relationship between stimuli and cognitions. Nor can we appeal to external standards of accuracy because such standards may not be able to differentiate between demonstrative and dialectical models. For example, in a series of papers Lamiell (1981, 1982, 1986) has proposed that personality psychologists adopt an "idiothetic"

measurement model that is based on dialectical principles. Unfortunately, measures based on his models are essentially linear transformations of measures based on the traditional normative or ipsative measurement models (Paunonen and Jackson, 1986). Thus, the different models can not be fruitfully compared using the traditional psychometric standards of validity coefficients and predictive utility.

Fortunately, judgment and decision scientists have provided us with a set of methods for testing models of how people think (e.g., Goldberg, 1968, 1970, 1971; Hoffman, 1960; Wiggins, 1973) regardless of the empirical accuracy of those thoughts (Funder, 1987). One method, which has been adopted by Lamiell in his work (Lamiell, Foss, Larsen, and Hempel, 1983; Lamiell, Foss, Trierweiler, and Leffel, 1983), is to present some information to a group of subjects and to submit that same information to a set of equations derived from different, preferably competing, theories about how the subjects will process that information. The equation whose outputs most closely match the subjects' responses is declared the winner. The present paper by Lamiell and Durbeck illustrates this approach.

The advantage of this method is that the investigator has control over the information the subject receives. Thus, the comparison of the models is unconfounded with the stimuli on which they act. Of course, this advantage has some costs. First, the investigator must be able to explicitly translate his or her model into an equation. More important, it also requires that the equations and the subjects use identical information. This assumption is not appropriate in those (relatively common) situations in which the subjects may have available information beyond that given in the experiment. For example, in the study reported by Lamiell and Durbeck the subjects could be comparing the targets not only to each other but also to people they actually know, including themselves. This, in turn, could account for the relatively poorer fit between the normative model and the subjects' responses.

A more naturalistic method (e.g., Chaplin and Buckner, 1987) is to ask subjects to respond to a set of stimuli and then to ask them to respond to that same set of stimuli according to rules or instructions derived from the competing theories. That theory whose instructions lead to responses that best fit the naturalistic responses is the winner. This method does not require that the information available to the subjects be controlled. Thus, it is appropriate for evaluating cognitive processes *in vivo*. For example, in our investigation of the internal standards that underlie self-ratings of personality (Chaplin and Buckner, 1987), we first asked subjects to rate themselves on a set of adjectives, using standard rating instructions. Subsequently they were asked to rate themselves according to explicit normative, ipsative, and idiothetic standards. We found that the ratings based on ipsative and idiothetic standards were consistently more similar to the "uninstructed" ratings than were ratings based on normative standards. However, this approach requires the assumption that the subjects

understand and respond to the instructions in a manner consistent with the theory from which they were derived. Depending on the circumstances, this assumption may (McArthur, 1972) or may not (Nisbett and Wilson, 1977) be justified.

Although they differ on the dimension of experimental control these approaches have in common their use of the subject's responses as the criteria against which the different cognitive models are assessed. Thus, they are appropriate for assessing theories that take seriously the dialectical nature of human thought. Moreover, these methods can complement each other because each compensates for some of the other's weaknesses. Fortunately, there is already some indication that results obtained with these two methods converge (Chaplin and Buckner, 1987).

One issue that is fundamental to both these approaches is how the similarity between the subject's responses and the output of the various models should be assessed. The problem is that similarity can be assessed with respect to a variety of different features of the subject's responses, and a given model may have outputs that are very similar to some features of a subject's responses and very dissimilar to others. Moreover, the outputs of different models may be more or less similar to different aspects of the responses. Unfortunately, although establishing how to interpret different types of similarity is crucial for understanding the results of our comparisons, little explicit attention has been paid to this issue.

By far the most popular index of similarity is the correlation coefficient. Unfortunately both Lamiell's equations and our instructions tend to yield outputs that are highly linearly related. Thus, the correlation coefficient is not very diagnostic for distinguishing among the various models. For example, the correlation between the outputs from the normative and "interactive" models for Attribute 1 reported by Lamiell and Durbeck in their Table 3 is .66.

An alternative measure of similarity, one that is employed by both Lamiell and ourselves, is a Euclidean distance index (Cronbach and Gleser, 1953). This index defines the similarity between two sets of responses as a function of the difference (distance) between them on a given response scale. One useful feature of this index is that, in addition to providing a global index of similarity, it can be broken down into the three components that contribute to that global index. These components have been labeled "elevation," which reflects differences between the means of two sets of responses, "scatter," which reflects differences between the variances of the responses, and "shape," which reflects differences between the patterns of the responses. The shape index is, essentially, the correlation between the two sets of responses.

In much of his work Lamiell has not reported the values for the separate components of similarity, yet those values are important for a complete understanding of the differences in the similarity between the outputs of the models and the subject's responses. For example, in our work (Chaplin and Buckner, 1987) we found that it was the scatter component that accounted for

the greater similarity (relative to the normative model) between the outputs of the ipsative and idiothetic models and the subjects' responses. This led us to the finding that normative instructions led to less extreme responses than the other instructions, which, in turn, can be understood in terms of a "false consensus bias" (Ross, Greene, and House, 1977). Although we do not claim to have "explained" the phenomena we do submit that we have obtained some potentially useful information as a result of examining the different components of similarity and considering their implications.

Finally, it is important to be cautious when drawing conclusions based on *differences* between the similarities of the models and responses. In their discussion, Lamiell and Durbeck state, "In light of the findings we have reported, however, a normative/mediational conception of the judgement process seems difficult indeed to defend. In our view, those findings strongly suggest that the negations with reference to which the subjects judged the targets were not of the standard logical sort *at all*, but were instead dialectical . . ." (emphasis added). Demonstrating that the outputs of a dialectical equation are a bit more similar to the subjects' responses than the outputs of a normative equation does not mean that the normative outputs are completely unrelated to the subjects' responses. Indeed, it is clear from Lamiell and Durbeck's results that the Normative outputs were proportionately more similar (.33 on a 1.00 scale of proportional dissimilarity) to the subjects' responses than they were dissimilar. Thus, although the dialectical model provided a relatively better fit to a subject's responses, both models fit the responses reasonably well. In our work (Chaplin and Buckner, 1987) we also found that instructions based on several different models led to ratings that are more similar than dissimilar to subjects' responses.

Conclusion

If the goal is to demonstrate that one model is correct and others are incorrect the foregoing results are frustrating. However, such a goal is probably unreasonable. More realistic (and dialectic!) goals would involve synthesizing these different views. For example, we could seek to specify the conditions under which dialectical and demonstrative processes operate, determine how these processes interact and complement each other, or trace how these processes develop together. After all, a model need not be perfect in order for it to be useful. And, two imperfect models may be better than one. Actually, I do not think that the authors of these papers would be unsympathetic to these goals, although I enjoy fantasizing about their possible other reactions.

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