FURTHER REMARKS ON THE ROLE OF COGNITION IN THE COMPARATIVE ANALYSIS OF BEHAVIOR

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Recent interest in comparative psychology has stimulated much research and debate concerning cognitive processes in animal behavior. The present paper relates to this general area by treating particular issues in the analysis of comparative cognition: specifically, how cognition is inferred from animal behavior; whether the postulation of intervening cognitive processes furthers our understanding of behavior; and how rival approaches help advance the science of behavior.

Key words: comparative psychology, cognitive processes

My recent (1981) review of Hulse, Fowler, and Honig's 1978 book, *Cognitive Processes in Animal Behavior*, provided me with an opportunity to place the current upsurge of interest in comparative cognitive psychology in broad historical perspective. Far from being an isolated fad or peculiarity, concern with the cognitive capabilities of animals dates back to the evolutionary theories of Spencer and Darwin. Indeed, comparative cognitive psychology is celebrating its first centennial, if we mark the birth of the field with publication of Romanes' *Animal Intelligence* in 1882.

The present occasion affords me the chance to expand and clarify a number of points raised in that review. The three I will consider are: (a) the methods of inferring cognition in the behavior of others; (b) the usefulness of positing intervening cognitive processes in the analysis of behavior; and (c) the role of rival approaches in advancing the science of behavior.

Inferring Cognition

We are all observers of behavior. The behavior we observe is sometimes our own and sometimes that of others. Our behavior may be overt and observable by ourselves and others; or it may be covert and observable only by ourselves.

Concern with private behavior was an early preoccupation of psychologists, perhaps because of the uniqueness and immediacy of the

data at issue, or because of the importance assigned to private behavior by many influential philosophers and theologians. Whereas early psychologists interested in human behavior used the readily available channel of language to communicate to others the contents of their own inner behavioral worlds (introspection), psychologists interested in animal behavior had to rely upon the overt, nonverbal behavior of their subjects. They reasoned by analogy that, if external forces provoke in themselves certain overt and covert activities, then when those same forces elicit in others the same overt activities, they too will be accompanied by the same covert activities (anthropomorphism). Such analogical inferences (called subjective inferences by Mackenzie, 1977) are generally of two sorts, cognitive and motivational, and are said to be more or less justified depending upon the phylogenetic relatedness of the species under study to humans. The key point is that the private behavior of others is not directly observable; it must be inferred from overt verbal and nonverbal responses. In Romanes' words, overt actions are the "ambassadors" of others' minds.

Given that we are denied direct access to another's inner world, how should we deal with the subjective behavioral data that we know in ourselves and presume in others? One approach is traceable to Watson (1913). Confronted with the often inconsistent and idiosyncratic data of consciousness gathered by introspective methods, Watson wrote that "the plans which I most favor for psychology lead practically to the ignoring of consciousness in the sense that that term is used by psy-

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chologists today. I have virtually denied that this realm of psychics is open to experimental investigation" (p. 175). A science of behavior then need not address the private data of consciousness and might proceed more expeditiously without reference to such subjective phenomena. Many today still adhere to this position.

A second approach to covert behavior is also attributable to Watson (1913). Here, Watson declined to deal with consciousness as something unique, preferring instead a more objective orientation. He said that, "if you will grant the behaviorist the right to use consciousness in the same way that other natural scientists employ it—that is, without making consciousness a special object of observation—you have granted all that my thesis requires" (p. 175).

These words of Watson are pregnant with interpretations. One reading is that of Skinner (1964). His point seems to be that an experimental analysis of behavior is equally applicable to public and private behavior, and it should comprise both (also see Lashley, 1923a, 1923b). Thus, Skinner writes that, "an adequate science of behavior must consider events taking place within the skin of the organism . . . as part of behavior itself. It can deal with these events without assuming that they have any special nature or must be known in any special way. The skin is not that important as a boundary" (p. 84).

Just where Skinner's position will lead is not clear. To date, it has generated some interesting analyses of verbal behavior in scientists (Moore, 1975, 1980, 1981). Implications for comparative psychology have yet to be carefully delineated.

Another, more prominent interpretation of Watson's words is tied to the trend in psychology toward objective definitions of cognitive processes. Watson actually had no particular problem with events that were knowable in the private world, so long as they were also knowable in the public world; then, "we shall have at hand tangible phenomena which may be studied as behavior material" (p. 174). Thus, cognitive processes such as perception, attention, memory, and association may have both subjective and objective behavioral referents. From a purely objective standpoint, however, our inferring the presence of these phenomena in behavior need not commit us

to projecting onto the behaving organism our own private behavior; inferring cognition in another's behavior may be made purely on the basis of empirical environment-behavior relationships (called objective inference by Mackenzie, 1977). Once again, we infer, but cannot observe, cognition.

An example illustrating both methods of inferring cognition may prove instructive. Consider Morgan's (1894) account of association formation by birds. The following empirical evidence led Morgan to infer (objectively) that young chicks and ducks were capable of forming associations:

A chick, about eighteen hours old, pecked at its own excrement rapidly thrice in succession, and then shook its head and wiped its bill on the ground. Ten minutes later it began to peck, but checked the action before reaching the excrement and wiped its bill. A little later it came near, looked at the material, and then walked away. (1896, pp. 85-86)

Reasoning by analogy from his own private behavior under related conditions, Morgan inferred (subjectively) the contents of the chick's inner world that accompanied its final performance: "A visual impression and a taste impression had become associated, and the recurrence of the former suggested a representation of the latter as an idea" (1896, p. 86).

Most psychological terms such as "association" have both objective and subjective meanings, thereby inviting debate and confusion. Workers generally agree that a comparative psychology of cognition is better predicated on the objective capabilities of animals than on the private accompaniments of their intelligent actions (see especially Jennings, 1976, pp. 328-337). Analysis of their private behavior is in no way precluded, but it presents far more challenging methodological and theoretical problems (see Griffin, 1978).

Intervening Variables

Tolman made a significant contribution to experimental psychology by formalizing the intervening variable and by making it the cornerstone of his scientific approach—operational behaviorism. As the intervening variable is a key concept in cognitive psychology, it may be useful to examine Tolman's ideas with some care.

According to Tolman (1936/1961), operational behaviorism comprises the following principles:

(a) It asserts that the ultimate interest of psychology is solely the prediction and control of behavior. (b) It asserts that psychological concepts, i.e., the mental capacities and mental events—may be conceived as objectively defined intervening variables. And it asserts that these intervening variables are to be defined wholly operationally—that is, in terms of the actual experimental operations whereby their presences or absences and their relations to the controlling independent variables and to the final dependent behavior are determined. (1961, p. 129)

Tolman provided us with a host of objectively inferred intervening variables, of both the cognitive and motivational varieties. One example that may be of considerable contemporary interest is that of “memory” (see, for instance, the recent interchange between Branch, 1977, and Shimp, 1976).

According to Tolman (1925/1961), memory may be conceived . . . as a purely empirical aspect of . . . behavior. To say that an animal remembers “such and such” is merely another way of saying that . . . behavior is then and there a function of a now absent object. But this tempero-functional dependence of behavior is a purely objective fact and all that need be meant by memory. (1961, p. 35)

Thus far, the term *memory* merely names a functional relationship between an independent variable and a dependent variable (see Feigl, 1945); Tolman (1938/1961) called such empirical relationships f_1 functions.

There are of course, many empirical relationships that might be said to define or to involve memory. If they all could be subsumed in some way with a single theoretical concept, we might greatly simplify a wealth of isolated facts. A recent theory of animal memory attempted to do just that.

Roberts and Grant (1976) proposed that the concept of the “memory trace” might be able to comprise many empirical relationships found with delayed matching-to-sample procedures. Two such relations are: (a) the longer the sample stimulus is presented, the better is discriminative test performance, and (b) the

longer the time since offset of the sample stimulus, the poorer is discriminative test performance. The memory trace, argued Roberts and Grant, grows with sample exposure and decays after sample offset. Because delayed matching-to-sample procedures necessarily present and remove the sample stimulus prior to the discrimination test, trace theory provides the potential of embracing two sets of empirical relationships with one theoretical notion.

Unlike Tolman’s earlier definition of memory that simply named an empirical relationship, the memory trace of Roberts and Grant truly intervenes between the objective procedural and behavioral variables. Roberts and Grant hypothesized precise quantitative relationships between sample exposure and trace strength and between delay length and trace strength; such relationships between the independent and the intervening variable Tolman called f_2 functions. Although unspecified by Roberts and Grant, there must also be a precise quantitative relationship between the intervening and the dependent variable, called by Tolman an f_3 function.

By using the intervening variable as a theoretical mediating device, then, workers endeavor to reduce the number of factors they must entertain in order to predict and control behavior. Who could reasonably object to this? Skinner, for one.

Skinner (1964, 1977) notes a number of problems with postulating intervening cognitive variables in the analysis of behavior. First, Skinner believes that the use of intervening variables encourages individuals to treat these ideas as “mental way stations,” in which environmental variables induce certain subjective states that then are causally linked to behavior. Such a concern is, of course, not directly relevant to objectively inferred cognitive processes and capabilities, such as memory as just discussed; the term *memory* was the overt behavior of psychologists, not the covert behavior of their subjects (Moore, 1981). Nor is Skinner’s concern necessarily relevant to subjectively inferred cognitions and emotions; covert responses may accompany overt responses without causing them (Moore, 1980).

A second danger noted by Skinner is that cognitive theories may seduce workers into stopping at the way station. Having identified environmental variables of which the memory trace is a function and having mathematically

specified the relationships between these variables and the strength of the memory trace, Roberts and Grant did, indeed, fail to specify precisely how discriminative responding is related to the strength of the memory trace. Cognitive theorists would then be well advised to take seriously this concern of Skinner.

A third problem is that the very ease of inventing intervening cognitive processes will deter researchers from the important activity of carefully delineating the environmental variables of which behavior is a function. This concern is a very real one for Skinner and others who are committed to applying the principles of behavior to human and environmental problems. What is largely at issue here is the extent to which the activities of psychologists are directed toward data and theory. More of that later. For now, I would simply say that I am unconvinced that the activities of contemporary comparative and cognitive psychologists (such as those who contributed to the Hulse, Fowler, and Honig 1978 volume) are failing to broaden our understanding of environment-behavior interrelations. On the contrary, I would suggest that interest and research in comparative psychology have never been greater. Skinner himself has been drawn into this arena and has reported the results of thought-provoking research on animal "self-awareness" (Epstein, Lanza, & Skinner, 1981) and "symbolic communication" (Epstein, Lanza, & Skinner, 1980), largely in response to those espousing a more cognitive orientation. I believe that the science of behavior has benefited from the work of all concerned.

Rival Approaches

A very real tension exists between two kinds of scientific behavior. On the one hand, we strive to organize a chaotic array of seemingly unrelated empirical facts, aided by concepts that themselves are of a theoretical, nonempirical sort. On the other hand, we try to invoke as few of these theoretical terms as possible, fearing that the very ease of their invention will lead to rampant conceptual superfluity and to a reduced concern with the empirical data of interest.

Speaking to the need for theory, we have Loeb (1915):

We may say it is the task of science to visualize completely and correctly the phenomena

of nature, of which our senses give us only very fragmentary and disconnected perceptions. We must try to visualize the numerous hidden processes and conditions connecting the disconnected phenomena we perceive. (pp. 768-769)

Arguing against uncontrolled theoretical speculation, we have Woodrow (1942):

Many appear to take the view that while all our measurements deal with environment and behavior, what psychologists should be most interested in is neither, but intermediate events occurring within the organism. . . . Since it is difficult for the psychologist to observe the interior of his subjects, the sort of events which occur in the region between stimulus and response must be left largely to the imagination. And our imaginations have not failed us. The things we have stuck within the organism in the hope thereby of explaining behavior are almost without limit in number and variety. (p. 3)

Among other things, the current debate between cognitive psychologists (e.g., Ellis, 1977; Griffin, 1978; Haugeland, 1978; Mahoney, 1977) and radical behaviorists (e.g., Rachlin, 1977a, 1977b; Skinner, 1977) revolves around this natural tension between theory-oriented and data-oriented approaches. I find the debate a very stimulating one, largely because I feel that both sides have important points to make concerning the analysis of behavior. My own efforts to explore the role of memory in operant behavior (Grayson & Wasserman, 1979; Wasserman, Deich, & Cox, in press; Wasserman, Nelson, & Larew, 1980) indicate how I have tried to incorporate the merits of both positions.

I believe that the debate between these two rival approaches can be a mutually constructive one. Cognitive psychologists can press their behaviorist adversaries for answers to such questions as: What is the theoretical status of terms like reinforcement history, discriminative stimulus, and operant response? What psychological processes are actually involved in the control of behavior by schedules of reinforcement? When you identify the endpoint of scientific analysis as the prediction and control of behavior, will not theoretical terms have to be invoked to effect such prediction and control? And, are not prediction and

control inherently cognitive ideas in the first place? Behaviorists may respond in kind by demanding to know of their cognitive opponents: Does the invention of intervening cognitive processes really increase our understanding of behavioral processes or is such theoretical activity simply superstitious? How do you determine when adding another cognitive step or process is truly justified? Can the cognitive processes that you have identified as being involved in one functional relationship be independently verified in another? What safeguards are there for preventing cognitive psychology from regressing to the earlier days of introspection and anthropomorphism? Cognitive psychology and radical behaviorism seem to be natural counterweights for preventing excesses in each other and for moving the analysis of behavior in a forward direction.

Less productive have been the adamant attitudes of a few advocates of each persuasion, with individuals occasionally engaging in needless derision and ad hominem argument. We can ill afford to trap ourselves into intransigent positions. No less than other responses, our scientific behavior should be subject to its consequences. When old responses prove less productive than new ones, they should extinguish and be replaced by more reinforcing alternatives. Variability in behavior is not without its virtues; stereotypy is not without its vices.

A final remark is in order concerning one further negative feature of the debate between cognitive psychologists and radical behaviorists. To a large degree, defenders of each position have cited successes in very different empirical domains. Thus, radical behaviorists claim as their prime achievement a description of response rates under various schedules of reinforcement; cognitive psychologists lay principal claim to a systematic, stagewise account of reaction times between stimulus and response. Such "victories" as these are hollow ones, indeed, if workers of each persuasion fail to encompass the data of their adversaries.

Seventy years ago, Watson (1913) complained that, "in psychology we are . . . in that stage of development where we feel that we must select our material. We have a general place of discard for processes, which we anathematize so far as their value for psychology is concerned" (p. 162). I agree with Skinner (1964, p. 96) that we can no longer treat ourselves

to the luxury of such selective analyses of behavior. We must broaden the range of phenomena to which we apply our scientific behavior. Here again, I applaud the contributors to the Hulse, Fowler, and Honig volume for their willingness to expand the contents and paradigms of their investigations. If we truly aspire to become a complete science of behavior, then we will have to remain receptive to incorporating new data and methods into our explanatory systems. Our failure to do so will serve no positive end and instead will only reinforce our critics who reject the very premise of a science of behavior.

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