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Comments on Cognitive Science in the Experimental Analysis of Behavior

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Arguments are increasingly being made for the inclusion of cognitive science in the experimental analysis of behavior (TEAB). These arguments are described, and a critical analysis of them is presented, especially in regards to the logic of objective inference and the renewed use of cognitive intervening variables. In addition, one particular defining feature of cognitive processes (i.e., the absence of an immediate controlling stimulus) is described, along with alternative points of view stressing molar-molecular levels of analysis and historical causation. Finally, comments are made on the use of cognitive concepts and language in the behavioral sciences. On all of these issues, counter-arguments are based on available material in behavior analysis metatheory, concepts, and experimental practices.

During the past ten years, cognition has become an increasingly dominant theme in animal learning (Bower & Hilgard, 1981). Cognition, of course, is nothing new. It has always dominated research and theory on animal behavior, as the work of Hull, Tolman, and others clearly attests. B. F. Skinner, however, departed from this tradition and successfully established an approach that has come to be known as the experimental analysis of behavior (TEAB). In making this departure, Skinner separated his approach from those of his contemporaries on the basis of metatheory (Skinner, 1938), methodology (Skinner, 1956), and practice (Skinner, 1957a). Through the years, TEAB has studiously avoided cognitive constructs and language (see Skinner, 1972), but now this seems to be changing—cognition is emerging in TEAB. Even though this movement is not *yet very* substantial, it does represent a potentially bold new change in the field—a change upon which we would like to comment.

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The emergence of cognitive constructs and language within TEAB might best be dated from the publication of Shimp's 1976 article on organization in memory and behavior published in the *Journal of the Experimental Analysis of Behavior (JEAB)*. In that article, Shimp argued for adopting the term "memory" as a useful and perhaps necessary theoretical concept in the analysis of behavior. Such a concept, he said, would allow for greater precision and generality in accounting for behavior-environment relationships than had theretofore been achieved by noncognitive approaches. This move of Shimp's, however, appeared to have little direct or immediate impact in TEAB at that time, perhaps because it was not organized around any one systematic point of view and was met with cogent and well-reasoned critiques (e.g., Branch, 1977; see also Skinner, 1977).

The current movement towards cognitive perspectives, however, has gradually taken a new tact. It has aligned itself, either wittingly or unwittingly, with recent work in the contemporary philosophy of mind called "cognitive science." This approach to cognition makes extensive use of the computational model of intellectual functioning, or what is commonly referred to as information-processing theory (see Bower & Hilgard, 1981, p. 353-415), both of which are tied to research and theory on artificial intelligence. That TEAB would adopt such a cognitive perspective might have been unthinkable several years ago. Today, however, arguments against radical behaviorism and for the necessity of cognitive science in the analysis of behavior are appearing in prestigious scientific journals (Fodor, 1981) and the popular press (e.g., Searle, 1982; Solomon, 1982), as well as in movements towards cognitive science by individuals who are or were at least once nominally aligned with TEAB (e.g., Estes, 1969; Hearst, 1978; Honig, 1978a; Premack, 1978). Further evidence of the influence of cognitive science is the acceptance (Nevin, 1982, p. 2) and increased use of cognitive language in the primary TEAB journal—*JEAB* (cf. Branch & Malagodi, 1980, p. 33). This is easily documented by counting the number of cognitive keyword descriptors used in *JEAB*. In the 18 years of *JEAB* publication before 1976, the descriptors, memory, cognition, intelligence, and information processing (from the 1980 *JEAB* index), appeared but five times ($\bar{x}$ = 0.28 per year); however, in the six years between 1976 and 1981, they have appeared 26 times ($\bar{x}$ = 5.2 per year), and at an ever-increasing rate. Of course, not all of these descriptors are being used to make a case for cognitive science; some are being used in rebuttals to this position (e.g., Branch, 1977). Nonetheless, this trend reflects to us the emerging influence of cognitive science in TEAB. Finally, the most obvious indication of this influence is Wasserman's 1981 *JEAB* position-paper-qua-review of Hulse, Fowler, and Honig's (1978) text, *Cognitive Processes in Animal Behavior* in which he argued strongly for the inclusion of cognitive terms and concepts in the analysis of behavior.

Despite the trend towards cognitive science, the arguments for this move are not beyond critical analysis; indeed, some of them are misleading in both logic and fact. We would like to respond to these arguments with both logical analyses and descriptions of relevant experimental research. First, we briefly describe contemporary cognitive science and the form in which it is manifested in TEAB. Second, we criticize this position with respect to its use of the logic of objective inference, and describe the proper domain of objective inference in the analysis of behavior. Third, we examine how intervening variables are being misused in the analysis of behavior. Fourth, we review a defining dimension of cognitive processes (i.e., lack of an immediately controlling stimulus) and offer some behavioral alternatives for the use of cognition in this context. And fifth, we discuss the role of cognitive language and concepts in the behavioral sciences.

Before starting, however, two comments are in order. First, although this paper is directed primarily at cognitive science in TEAB, it is also pertinent to the use of cognitive science in applied behavior analysis (e.g., cognitive behavior modification); hence, our arguments are relevant to both domains of contemporary behaviorism. Second, the organization of this paper moves, in general, from the more philosophical to the more empirical because we must initially describe the conceptual and logical behavior of cognitive scientists and behavior analysts before presenting concrete empirically-based arguments against the necessity of cognition in TEAB. This said, we now turn to a description of contemporary cognitive science.

COGNITIVE SCIENCE

Cognitive Science in the Philosophy of Mind

Cognitive science is difficult to define because it is no one thing and because many cognitive scientists do not always agree on a number of important issues (e.g., materialism, reductionism, representation, and imagery). In addition, cognitive science is not actually a theory of the mind per se. Rather, it is a collection of research programs on topics drawn from several fields, most notably, experimental psychology, philosophy, linguistics, and computer science. Basic to most cognitive science, though, is the computational model of human cognitive activity in which this activity is likened to the artificial intelligence of a computer. Cognitive science, then, represents a complex interplay between philosophy and psychology.

As incorporated into the philosophy of

mind, cognitive science seeks an account of human mental activity that is neither solely in terms of the common-sense psychology of ordinary language nor solely in terms of the neurophysiology of the brain. According to cognitive science, common-sense psychology is effective for daily discourse, but is not scientific; the neurophysiological study of the brain is scientific, but is too limited yet for an adequate explanation of the mind (Searle, 1982). The means for overcoming these two limitations is said to be found in the computational model in which the mind is viewed as an abstract system of logic whose function is to process information. The essence of such a formal model is that it matters not so much what the system is made of—whether "meat" (human neurophysiology) or "metal" (computer hardware)—but rather the formal or logical operation of the system, that is, how it functions and how its components function with respect to one another. In other words, the computational model, in conjunction with the "functionalist" philosophy of mind (see Fodor, 1981), is concerned with the software or program functions of the computing system which directs how information is processed; it is not concerned with the physical structure of the computer hardware. On the basis of this approach to cognition, the mind is to be studied on the analogy that if the output or behavior of computers and humans (or other organisms) is similar, then their information-processing programs or minds may be said to function or process information in the same way. The purported value of this approach is that psychologists can perhaps better understand the minds of humans, which are said to be "opaque" or unobservable, by understanding the minds of computers, which are said to be "transparent" or knowable.

This ends our brief characterization of cognitive science in contemporary philosophy of mind. For more complete accounts, see Block (1980, 1981) and Haugland (1981). We turn now to the arguments for the inclusion of cognitive science perspectives in TEAB.

Cognitive Science in TEAB

Although the current move to include cognition in TEAB does not always explicitly ascribe to cognitive science, its logic and language are for the most part consonant with this approach, and work in computer science is often referred to as a source of important ideas. For example, Shimp (1976, p. 120) made a brief but explicit note of how his suggestions were closely related to mathematical and computer-simulation work in cognitive psychology. Riley and Roiblat (1978) have described the pigeon as an "information-processing system with a limited channel capacity" (p. 274) and have spoken of the benefits in the comparative examination of information-processing mechanisms in animals and humans. Premack (1978) has discussed how pigeons "assign weight" (p. 448) to absolute and relational stimulus factors, and then how judgments are made in either a digital or analogue fashion. Finally, Hulse, Fowler, and Honig (1978) have pointed out how animal psychologists are making increased use of "the techniques and models of [their] colleagues in the field of human cognition and information procesing" (p. xi), especially computer technology, which is viewed as a useful tool and as "a ready-made model of human thought" (p. xii).

Perhaps the best examples of the arguments for adopting a cognitive science approach in TEAB come from Wasserman's (1981) review of Hulse, Fowler, and Honig (1978). Wasserman argued that through "reasoning by function from the intelligent action of humans and their wondrous mechanical creations [computers]" (p. 250), objective inferences can be derived about mental processes. A particularly clear illustration of how closely this argument is aligned with cognitive science is Wasserman's (1981) use of a quote from Lachman, Lachman, and Butterfield (1979, p. 109):

> When we draw the computer analogy, it is not to parts of the brain, but to human competence and capacities of the mind. We do not refer to the hardware from which the computer is built, or its electrical parts, but to certain of its functions. The analogy is between functions created by appropriate

programming and human cognitive processes. (p. 250)

Finally, Wasserman noted that "future work in comparative psychology will surely explore the usefulness of computer models of performance and cognition" (p. 251). On the basis of these and other comments, it appears that the arguments of some cognitivists within TEAB are in accord with those of the cognitive scientists in the contemporary philosophy of mind. Let us now describe these arguments more fully.

Objective inference. Central to cognitive science in the analysis of behavior is the strategy of objective inference (cf. Mackenzie, 1977, p. 63-73) and the argument that the root of many objections to the study of cognition in animals lies in the failure to distinguish between objective and subjective inference (e.g., Wasserman, 1981, p. 248). A common illustration of subjective inference is Romanes' (1882) method of reasoning by analogy from human minds to animal minds, in which what an observer subjectively knows and feels is projected onto animals in order to explain the latters' behavior. Subjective inference of this sort is generally rejected by cognitive scientists because it is mentalistic (Wasserman, 1981, p. 248). More specifically, subjective inference is rejected by cognitive scientists because, first, introspection has yet to provide a sufficient enough understanding of human minds to make the analogy valid and, second, even if the minds of humans were sufficiently understood, it cannot be assumed that the minds of animals are similar enough to produce analogous subjective states (Mackenzie, 1977, p. 57-63).

Instead of using subjective inference, cognitive scientists propose that animal minds can be known on the basis of objective inference. Using objective inference, the presence or absence of certain cognitive capacities or activities, such as associative memory, perception, and expectation, can be inferred on the basis of an animal's behavioral adjustment, or lack thereof, to the circumstances of its environment. Research on cognitive processes, then, follows the logic that if an experimental manipulation produces similar behavioral adjustments within or across species, then the inference can be made that similar or functionally equivalent cognitive processes were operating. If the adjustments are different, however, then the inference can be made that qualitatively different processes were in effect (see Mackenzie, 1977, p. 64). In both cases, the inference is based on objective criteria, typically on an animal's ability to learn. It is not based on subjective criteria.

OBJECTIVE INFERENCE

As just described, cognitive scientists adopt the strategy of objective inference in the development of cognitive theories of behavior. We do not want to object that this form of logic is somehow inappropriate, for it is a dominant form of reasoning in science. We do, though, have two primary reservations about its use, one with respect to violations of the law of equifinality by cognitive scientists and the other regarding the proper domain of objective inference in the analysis of behavior.

The Law of Equifinality

Our first reservation about the use of objective inference in cognitive science is that it can easily lead to violations of the law of equifinality. That is, it can lead to the logical fallacy of affirming the consequent when assertions are made that a particular consequent *must* follow from a particular antecedent. The law of equifinality states that many different effects can be produced by formally identical causes (cf. McKearney, 1977). Applied to objective inference within cognitive science, equifinality means that similar cognitive capabilities or cognitive processes cannot be asserted to exist on the basis of similar behavioral outputs, nor can different capabilities and processes be asserted to exist when the outputs are different.

One example of how the law of equifinality can be violated in cognitive approaches to TEAB is provided by Wasserman's (1981) answer to his own question about whether animals can "learn to perform in accordance with specific situational demands deemed necessary to evidence some 'psychical faculty,' such as association formation, abstraction, or ideation" (p. 247). Wasserman answered that if animals do perform this way, "then they *must* [italics ours] possess that mental capability" (p. 247). "Must," however, is either a tautological explanation, that is, true merely by the meanings of its component terms, or it violates the law of equifinality by affirming the consequent through asserting the necessity of the antecedent. In objective inference (or inductive logic), the *necessity* of a relationship can never be established, only varying degrees of *sufficiency* can be inferred. In order to strengthen these degrees of sufficiency, the scientist seeks to eliminate alternative explanations, to affirm additional consequents, and to employ the methods of converging operations and other logical rules for the construction of evidence. In sum, objective inference allows for varying degrees of "may be," but never "must."

Our argument here is made difficult by the cognitive scientists' concurrent and vascillating use of descriptive *dispositional* terms, such as "capacity," "competence," and "adjustment," with explanatory or causal *process* terms, such as cognitive "activities" and "operations," as though the two sets were interchangable. The former, however, are merely summary descriptions of organism-environment relationships, while the latter are inferences about underlying, controlling cognitive variables.

If inferences are made only about dispositions, which refer to nothing more than capabilities, then "must" does not violate the law of equifinality or affirm the consequent. That is, if an animal can perform to certain standards or can adjust to certain circumstances, then it *must* have the capability to do so. This assertion, though, adds nothing to the understanding of behavior and, as mentioned before, any argument that these capabilities explain behavior is merely tautological.[1] If, however, inferences are made about processes, such as controlling cognitive activities and/or operations, and not just about capabilities, then equifinality will be violated and consequents unjustifiably affirmed with any assertion of "must." That is, although an animal does perform to certain standards or adjust to certain circumstances, no objective inference can be made that the animal must possess some underlying controlling cognitive process or psychical faculty, such as association formation (e.g., Fowler, 1978), memory (e.g., Honig, 1978a), abstract perceptual categories (e.g., Premack, 1978), cognitive maps (e.g., Menzel, 1978), or expectancies (e.g., Wagner, 1978).

Thus, if an animal adjusts to certain circumstances, then we can make objective inferences about its capabilities because these statements will be summary descriptions of observed organism-environment interactions. But just because we can make strong inferences about capabilities does not mean that we can make them about supposed underlying, controlling cognitive processes. The distinctions between the descriptive and explanatory functions that cognitive terms can serve have not always been made explicit by cognitive scientists. Indeed, cognitive terms are used sometimes as descriptive or summary concepts, and at other times as hypothetical explanatory constructs which often refer to states or processes which, in part, determine behavior (see Honig, 1978b, p. 1-3). The lack of clarity about how cognitive terms and concepts are used stems in part from their multiple and sometimes conflicting functions.

The Domain of Objective Inference

Our second reservation about objective inference has to do with the proper domain of its use in behavioral science. One might construe from reading the cognitive science literature that the use of objective inference is only meant for inferring *cognitive* dispositions and processes. This

[1]An example of such tautological explanation may be gleaned from Riley and Roiblat's (1978) discussion of a pigeon's matching-to-sample ability. They stated that "when the animal's information-processing capacity is exceeded, errors occur" (p. 274).

is not so. Objective inference is used throughout all science, behavior analysis included (Sidman, 1960, p. 127-137). The difference between its use in behavior analysis and cognitive science, however, is that within behavior analysis objective inference is restricted to functional relationships between behavior and the environment, whereas in cognitive science objective inferences are made about processes supposedly occurring at another level of analysis. We propose that, in a science of behavior, objective inference be applied to behavior-environment relationships. Here, objective inferences take different forms depending on the nature of that which is being inferred. Let us describe various forms of these inferences.

Empirical constructs: Response classes and behavioral capabilities. First, behavior analysts make objective inferences about several types of empirical constructs, most typically about response classes and behavioral capabilities. With respect to response classes (e.g., respondent, operant, and adjunctive), inferences are made about various behavior-environment relationships on the basis of observed interactions. These inferences allow the categorization of these classes and predictions about their controlling relationships (e.g., eliciting, reinforcing/discriminative, and schedule-induced). Objective inferences are also made about the presence, kind, and extent of an organism's behavioral capabilities (e.g., its discriminative ability), that is, about the types of behavior an organism can engage in with respect to its environment. What is important in both cases is that the objective inferences are based on observable behavior-environment relationships. As such, response classes and behavioral capabilities are empirical constructs (or dispositional terms).

Controlling relationships. A second set of inferences made in behavior analysis is about controlling relationships, such as reinforcement, stimulus control, and elicitation. For example, with respect to stimulus control, Sidman (1979) has pointed out that:

> Stimulus control is always an inference. We can, of course, observe and measure a single instance of any stimulus, but we can never know except by inference whether we are actually observing a particular controlling relation between two stimuli or between a stimulus and a response. Unlike individual stimuli and responses, controlling relations are not directly observable. (p. 123)

We should note here that the strength of inferences about controlling relationships is governed stringently by experimental demonstrations and proof. Although this evidence must be compelling, standards do change with new findings. In other words, objective inferences are subject to the self-corrective nature of science which may alter previous inferences on the basis of new discoveries.

Because objective inference in the analysis of behavior is most often practiced in the domain of controlling relationships, let us present an example of how it operates and how inferences remain open to experimental analysis; for additional examples, see Sidman (1960, p. 128-135). In 1961, Reynolds published an important early study on stimulus control. In his research, he reinforced pigeons' responses to a lighted disk which displayed a triangle on a red background—the S+—and extinguished responses when a circle on a green background—the S-—was projected. After a stability criterion was met, the individual stimuli of the S+ and S- complexes were presented under extinction conditions. The results showed that the pigeons responded to only one element of the S+ complex, and Reynolds made the inference that this element was the only one that functionally controlled behavior. The results of subsequent research, however, conflicted with this inference. In 1976, Wilkie and Masson replicated Reynolds' experiments and his results. In addition, they also assessed stimulus control using a resistance-to-reinforcement test instead of extinction test and found that both the S+ elements actually acquired control. This, then, is an example of how science ought to progress: objective references are to be based on testable organism-environment relationships.

Interpretation. A third type of objective inference in the analysis of behavior refers to the extension of behavior analysis principles and concepts to experimentally unanalyzed behavior-environment relationships, that is, to relationships in which the controlling variables have not yet been investigated or are for practical purposes unavailable for analysis. This sort of inference is often referred to as "interpretation" (cf. Day, 1976, p. 95-100). Here, behavior analysts go beyond the facts at hand to make inferences about controlling relationships in everyday behavior-environment interactions, as opposed to inferences about controlling relationships in experimentally-based analyses such as those described in our discussion above. Skinner's books *Science and Human Behavior* (1953) and *Verbal Behavior* (1957b), for example, are interpretations of human behavior in terms of the basic principles of behavior analysis. Interpretations such as these are based on the assumption that the behaviors observed may be accounted for by already-understood behavioral laws, and that these laws operate in everyday human behavior-environment relationships. Such inferences or interpretations can set the occasion for important scientific inquiry and practical action. They are, however, difficult to accept into the basic facts and empirical relationships of a science of behavior, or as true explanations, because no experimental analysis has been conducted.

Covert activity. The fourth type of objective inference is actually another form of interpretation. Interpretation, here, is about the inaccessible behavior-environment relationships that are called covert or private (cf. Schnaitter, 1978, p. 11). For example, Skinner (1957b) has argued that "when someone solves a problem in 'mental arithmatic,' the initial statement of the problem and the final overt answer can often be related only by inferring covert events" (p. 434). Autoclitic behavior and self-editing (Skinner, 1957b, p. 311-330, 369-401) are other examples of objectively inferred covert behavioral activity. This covert activity is not qualitatively different from overt activity; it is only inaccessible. Although private activity is accepted in principle as proper subject matter for a science of behavior (Skinner, 1953, p. 257-282; see Moore, 1980), either as cause or effect (cf. Zuriff, 1980), inferences about it are difficult to accept as scientifically valid unless it can be manipulated and evaluated, either directly or under tightly controlled training and with deductive inferences fitting a natural science of behavior (Johnston & Pennypacker, 1980, p. 171-188).

Conclusions. If behavior is the subject matter of the behavioral sciences, then the use of objective inference should be restricted to this domain. Inferences about supposed theoretical cognitive processes occurring at other levels, or in other dimensions, create problems for the logic of science. Sidman (1960) speaks directly to this issue:

> The usefulness of the method [of affirming the consequent] as a test of theory is limited by the specificity of the theoretical statements in question. It is folly to evaluate theory by affirming the consequent when the basic statements of the theory are subject to equivocation . . . The loose theorizing to which so many of us have been addicted has thus served to conceal a major strength of the technique of affirming the consequent. When this technique of systematic replication is applied successfully, one gains confidence not only in the reliability and generality of the data, but also in one's understanding of the data. (p. 136)

Within properly conducted behavior analysis, as opposed to cognitive science, inaccurate inferences are easily dispelled because they are about behavior-environment relationships that can be objectively analyzed. Errors in inferences about theoretical cognitive processes, however, are almost impossible to dispell because of the ephemeral and unobservable nature of those processes—hence, perhaps, their proliferation.

INTERVENING VARIABLES

The use of intervening variables and hypothetical constructs in psychology, and debates over their ontological status, have aroused considerable controversy throughout the history of the field (Marx, 1951; Turner, 1967, p. 258-261). TEAB, of course, has traditionally rejected both terms and their connotative meanings. The term "intervening variable," however, is central to the emergence of cognitive science in TEAB, for it is intervening variables that are being objectively inferred (cf. Wasserman, 1981). In light of this, we think it is necessary to spell out (a) the logic used for including intervening variables in TEAB, (b) the shift that can occur from the use of intervening variables to the positing of hypothetical constructs, and (c) the physiological status often attributed to hypothetical constructs.

Intervening Variables as Empirical Constructs

The current interest in cognitive animal psychology is said to be due to a rekindling of the debate over the Darwinian thesis of mental or cognitive continuity between animal and man. Such theorizing about cognitive dispositions and processes, however, has been objected to by many psychologists, most notably Skinner (e.g., Skinner, 1977), because it invokes the existence of homunculi and other inner causes denoting actual entities. Objections such as Skinner's, though, are viewed as misdirected by cognitive scientists because no distinction is made between subjective and objective inference—subjective inference is said to be mentalistic, while objective inference is not (cf. Wasserman, 1981).

To argue against mentalism and for the objective inference of cognitive dispositions and processes, cognitive scientists apparently rely heavily on the early work of Tolman (1932) who is credited with the development and proper use of intervening variables. According to MacCorquodale and Meehl (1948), Tolman (1938) used intervening variables as a convenient, shorthand method for abstracting or grouping terms and empirical laws. Thus construed, intervening variables contain no meaning beyond the terms or laws they summarize, and are directly reducible to them. In MacCorquodale and Meehl's words, intervening variables "involve no hypothesis as to the existence of non-observed entities or the occurrence of unobserved processes" (1948, p. 103). Intervening variables, then, are to be contrasted with hypothetical constructs, which do have surplus meaning and which typically involve the positing of "an entity, process, or event which is not itself observed" (MacCorquodale & Meehl, 1948, p. 95-96). Wasserman (1981) apparently concurs:

> Tolman's main justification for inventing intervening variables was to help summarize the limitless combinations of independent variables that might have orderly effects on behavior. . . . Tolman's cognitive theorizing simply never sought access to the private contents of an animal's consciousness, nor did it move the study of behavior into the skulls of animals. So long as contemporary workers infer cognitive processes objectively and restrict their theorizing to intervening cognitive variables, comparative psychology will be well served and will be immune to the sorts of criticisms made by Skinner. (p. 248-249)

If intervening variables are no more than useful abstractions or empirical constructs, then our objections to them are minor, though the term "intervening variable" seems somehow unnecessary and rife with possible misunderstanding. Maze (1954) has been particularly clear on this point:

> It does not seem to me that the term "intervening variable" is necessarily suitable for referring to [empirical, mathematical relations], partly because of other explicit meanings that have accrued to it, and partly because the words themselves inevitably suggest some state-like thing that *intervenes between* stimulus and response. A relation is not "between" its terms even in the most neutral way; one should say rather, that [its terms] have, or stand in, that relation. (p. 233)

Nonetheless, if intervening variables are akin to the descriptive dispositional concepts described earlier (see above p. 113) (cf. Maze, 1954, p. 229), then, as empirical constructs, their validity "cannot be called into question except by an actual denial of the empirical facts" (MacCorquodale & Meehl, 1948, p. 104). As mentioned before, however, behavior analysts use empirical constructs (e.g., behavioral capabilities and response classes, see above) for the same reason as Tolman—economy of description and summarization (cf. Skinner, 1938, p. 1). Skinner (1956) himself has argued that the development of behavioral science will ultimately culminate in the derivation of these higher-order concepts and statements from lower-order empirical relationships.

Intervening Variables as Hypothetical Constructs

Given that intervening variables are no more than empirical concepts, then little reason exists to describe them with a cognitive vocabulary. On their face, descriptive cognitive terms (e.g., association, attribution, expectation, information processing, internal representation, memory, perception, and strategy) seem no more logical or necessary than noncognitive terms. To their detriment, though, the history of psychology shows that cognitive terms easily evolve into invalid explanatory and mentalistic mechanisms (Branch, 1977; Moore, 1975; Zuriff, 1979). MacCorquodale and Meehl (1948), in fact, discussed how such an evolution can occur with terms that are introduced as intervening variables, but that later become explanatory hypothetical constructs.

> First . . . there is the failure explicity to announce the postulates concerning existential properties, so that these are introduced more or less surreptitiously and *ad hoc* as occasion demands. Secondly, by this device there is subtly achieved a transition from admissible intervening variables to inadmissable hypothetical constructs. These hypothetical constructs, unlike intervening variables, are inadmissible because they require the existance of entities and the occurrence of processes which cannot be seriously believed because of other knowledge. (p. 136)

The historical evolution of Tolman's position—a position that is a major exemplar of the safe and rational use of intervening variables (Wasserman, 1981)—demonstrates just such a shift. While it is often contended that Tolman held strongly to the use of intervening variables as opposed to hypothetical constructs (e.g., Wasserman, 1981), cognitive scientists sometimes fail to acknowledge that Tolman quite clearly put aside the former in favor of the latter (Marx, 1951; cf. Tolman, 1959, p. 97). Tolman (1949), himself, was quite straightforward in making this change:

> I am now convinced that "intervening variables" to which we attempt to give merely operational meaning by tying them through empirically grounded functions either to the stimulus variables, on the one hand, or to response variables, on the other, really can give us no help unless we can also imbed them in a model from whose attributed properties we can deduce new relationships to be looked for. That is, to use Meehl and MacCorquodale's distinction, I would abandon what they call pure "intervening variables" for what they call "hypothetical constructs," and insist that hypothetical constructs be parts of a more general hypothesized model or substrate. (p. 49)

Not only did Tolman alter his position to one viewed as objectionable by cognitive scientists, cognitive scientists themselves seem to vascillate on the use of intervening variables. While they may argue for intervening variables as shorthand descriptive dispositional concepts (e.g., capacities, competencies, and adjustments), they also use them as hypothetical explanatory or causal constructs (e.g., processes, activities, and operations). This point relates to the issue we raised earlier (p. 113) about the failure of cognitive scientists to distinguish clearly between descriptive dispositional terms and explanatory or causal process terms. While Tolman's (1932) original use of intervening variables was directed to the development of empirical constructs, the term has come to have numerous, less than empirical connotations (Maze, 1954; Turner, 1967). Turner (1967) is particularly forthright on this point:

> The more important distinction [between intervening variables and hypothetical constructs] seems to be that the surplus of presumptive meaning of hypothetical constructs is embedded in models and their positive analogy, whereas the surplus meaning for intervening variables is left unreified and downright suppressed. (p. 261)

Numerous examples of how intervening variables evolve into hypothetical constructs appear among the chapters in Hulse, Fowler, and Honig (1978). One of the most illustrative comes from Church's (1978) description of how his research and theorizing on internal clocks—mediators of temporal control—evolved over the years. As he describes it:

> When we began research on timing behavior of rats several years ago, the concept of internal clock was, for us, simply a metaphor. As our research progressed, however, we found ourselves searching for the properties of the internal clock. After we discovered some characteristics of the internal clock, our attitude toward it gradually began to change. The concept was no longer a metaphor; we began to believe that the clock actually exists. (p. 284)

Other cognitivists seem to vascillate

more on the use of intervening variables and hypothetical constructs. For example, Wasserman (1981) said that cognition refers to "processes and activities . . . that may intervene between changes in the environment and changes in overt behavior" (p. 243). Later, he said that for the cognitive animal psychologist, "how animals represent and interrelate the events in their environments is the primary concern of the researcher. How that knowledge is later translated into overt action is a secondary concern; these actions are, after all, merely 'ambassadors' of the mind" (p. 253). The positions of Wasserman and of Church are not limited to empirical relationships and do not seem "immune to the sorts of criticisms made by Skinner" (Wasserman, 1981, p. 249). Apparently, objective inference can be just as mentalistic as subjective inference.

Intervening Variables and Physiological Reductionism

Cognitive scientists are not always explicit about physiological reductionism as the ultimate basis of cognition, but their remarks are sometimes suggestive. On the one hand, we find statements that physiological reductionism poses difficult problems for those who adhere to Tolman's (1932) early use of intervening variables: such reductionism often generates surplus physiological or mentalistic meaning (e.g., Wasserman, 1981, p. 249). On the other hand, though, we find that Tolman (1959) later adopted a reductionistic thesis. For instance, he stated: "I assume that the different readiness or beliefs (dispositions) are stored up together (*in the nervous system*)" [italics ours] (p. 114).

More recently, Wasserman (1981, p. 248) cited Church's (1978) concept of the internal clock as an example of an intervening variable, though he later referred to it as a hypothetical construct. We would contest Wasserman's labelling an internal clock as an intervening variable when, by MacCorquodale and Meehl's (1948) definitions, it is a hypothetical construct. In any case, Wasserman appeared to accept Church's pragmatic argument that such theoretical terms are useful because they may represent "physiological reality" (Church, 1978, p. 284). In Church's (1978) words: "Many facts of temporal discrimination are consistent with the simple notion that an animal advances an internal or biological clock at a fixed rate, and reads the clock when necessary" (p. 308). Wasserman (1981, p. 249) later acknowledged that hypothetical constructs typically do refer to physiological processes. According to MacCorquodale and Meehl (1948), this concurrence between hypothetical constructs and physiological reductionism is called for because neurology must eventually become relevant to any hypothetical construct. If hypothetical constructs are ever to be more than metaphors, they state, then it is legitimate "to require of a hypothetical construct that it should not be manifestly unreal in the sense that it assumes inner events that cannot conceivably occur" (MacCorquodale & Meehl, 1948, p. 105). In summary, then, it would be difficult to say that cognitive scientists are as a group anti-reductionistic.

In contradistinction to what may be the position of the cognitive scientists, many philosophers and psychologists strongly oppose physiological reductionism on both logical and empirical grounds and hold that psychology stands as an independent natural science (Kantor, 1947; Skinner, 1938). This is not to say, however, that a behavioral account of organism-environment relationships will not be made more complete by an increased understanding of physiological processes. Indeed, a strong physiological science is necessary for the behavioral sciences because physiology surely participates in all behavior. Watson's (1929) comments of over 50 years ago echo our point—indeed, they are quite up to date, except for the nature of the machinery:

> We need in psychology all the available facts neurologists can give us, but we can very well leave out of consideration those ingeneous puzzle pictures that compose the action of the central nervous system with a series of pipes and valves, sponges, electric switchboards, and the like. (p. 19)

Our point here is simply that the complexity of behavioral phenomena can not be explained solely by reducing those phenomena to neurophysiological and biochemical activities (Delprato, 1979; McKearney, 1977). For behavior analysts, "mental" activity is no more than the public and private material activity of an organism under certain conditions, both of which set the occasion for us to describe (or "tact") that activity as "mental." The explanation or meaning of this "mental" activity, however, is not to be found in its material, that is, in the physical structure of behavior, whether in the physiological activity of the brain or in any other behavioral activity. Rather, the meaning of this "mental" activity is to be found in the historical development of organism-environment interactions as expressed by current functional relationships between behavior and the environment.

THE DEFINITION OF COGNITIVE PROCESSES

These comments on objective inference and on intervening variables bring us to a critical point. If cognitive processes are to serve an explanatory role, then how are they to be defined? Interestingly, a positive answer is not always available; often the answer is stated negatively (cf. Ryle, 1949, p. 20). Honig (1978b), who has adopted a cognitivist stance, states the case explicitly:

> Cognitive processes seem more clearly defined by the exclusion of *immediate* [italics ours] control of behavior by environmental conditions than by the inclusion of the "meat" or "essence" of cognitive functioning. A positive description of cognitive mechanisms or processes common to the defining observations still seems to be lacking. (p. 11)

Other, related comments supportive of cognition in the TEAB literature also focus on "immediacy" in the inference of cognitive processes. For instance, Wasserman (1981) said that within stimulus-response time-spans ("reaction times") "inferred cognitive processes occur that serve as the essential theoretical entities of the information processing approach" (p. 250). We may also return to Shimp (1976, p. 118-119) who argued that radical behaviorism encourages analyses of behavior-environment relationships "in terms of *immediate* observables and heavily discourages an analysis in terms of theoretical quantities that are *not immediately* observable" (p. 118) [italics ours]. He then went on to argue that theoretical cognitive variables can serve important and necessary roles in advancing a science of behavior that a restricted focus on immediate behavior-environment relationships can not.

In order to explain behavior that does not appear to be caused by immediate environmental conditions, however, we need not infer the presence of mediating processes operating at another level or in another dimensional system (i.e., physiological or cognitive), or even infer the presence of a controlling relationship with private or covert behavioral activity. Rather, we should consider two alternatives: (a) examining the molar and molecular levels at which behavior is being analyzed and/or (b) examining the role of historical causation. These two approaches probably reside along a single continuum; hence, distinctions between them are difficult to make. By molar-molecular, however, we are referring to controlling operations that are occurring in situations adjacent to the activity in question (e.g., in multiple schedules); by historical causation, we are referring to controlling relationships that were established at an earlier time and that continue to exist without any intervening mediation or maintenance (e.g., schedule history).

Molar-Molecular Levels of Analysis

With respect to the level of analysis, a view of the subject matter that is too molar or too molecular can obscure or make unobservable controlling relationships that actually exist (see Branch, 1977; Wahler & Fox, 1981). Just as the power of a microscope must be adjusted as a function of the phenomenon under study, so too does the level of behavior analysis need to be adjusted to the functional unit of behavior-environment interaction. To be specific, when order is not apparent at a molar level, a more molecular analysis may be necessary (cf. Moore, 1982). Conversely, if one fails to find an immediate stimulus that controls a response, perhaps the response is only an element of a larger functional unit which is controlled by currently operating variables not immediately attendant to that element.

While it is difficult to determine what length of time cognitive scientists deem as more than "immediate," the TEAB literature repeatedly reveals the complex nature of functional relationships between behavior and the environment and how such relationships can at times only be discernable over large, nonimmediate units of time. Research by Herrnstein and Hineline (1966) on avoidance conditioning, for example, has demonstrated that responding can be engendered and maintained under conditions where no single response avoids a single electric shock, but, rather, where there is a relation between the *rate* of responding and the *rate* of electric shock delivery; such relations between rates were only manifested over an extended period of time. Other research shows that rats will press a bar, the consequence for which is to present an immediate electric shock; if the same response also avoids five shocks of the same intensity latter in time (Lambert, Bersh, Hineline, & Smith, 1973). Rachlin (1978) has discussed these studies and others in his "molar" analysis of self-control. Taste aversion conditioning has also provided compelling examples of the effects of controlling variables that do not operate in the "immediate" temporal interval (cf. Logue, 1979. Finally, research in behavioral pharmacology has demonstrated that drug effects on behavior are determined not only by the interaction of the pharmacological agent and the immediate behavior-environment context, but also by contingencies in effect in other cotemporaneous conditions, such as in another component of a multiple schedule (e.g., McKearney & Barrett, 1975).

Historical Causation

With respect to historical causation, objective inferences about historical variables, as opposed to objective inferences about co-temporary cognitive processes, should serve as the basis for proper scientific inquiry (see McKearney, 1977; Sidman, 1976). It is through the history of organism-environment interactions that functional relationships between behavior and environment develop (cf. Bijou & Baer, 1978). Without an appreciation for historical causation, specifying the factors that produce these controlling interrelationships can be difficult, and predicting the effects of future interactions is often severely hampered. When objective inference leads one to ignore the historical development of behavior-environment relationships, an approach to science has been taken in which effects are first discovered and cotemporaneous processes then hypothesized. Instead of attempting to prove the existence of such hypothetical processes, though, organism-environment relationships should be manipulated and experimentally analyzed in order to discover how such effects are produced in the first place. To be specific, histories of reinforcement, stimulus-response relationships, and stimulus-stimulus relationships should be examined to test the plausibility of objective inferences about behavior and its controlling variables.

Numerous experimental examples demonstrate that behaviors observed and labelled by others as indicative of cognitive dispositions and processes can often be explained by the history of organism-environment interactions. For example, Sidman (1971; see also Sidman & Tailby, 1982) demonstrated that, by arranging appropriate histories, previously untrained behavior can be made to emerge. Using a matching-to-sample format, he showed that matching of a stimulus A to a nonidentical stimulus B and then matching the stimulus B to a nonidentical stimulus C results in the correct matching of stimulus A to C, even though this last relation was never trained. In the area of drug abuse, the experimental analysis of drug-seeking behavior has demonstrated that much of what traditionally has been attributed to personality "types" may be accounted for by reinforcement schedule histories (see Weiner, 1981). Finally, Skinner and his colleagues (Epstein, Lanza, & Skinner, 1980, 1981) have demonstrated with pigeons that specific histories will result in novel behavior, such as "communication" and "self-awareness," which typically engenders explanations couched in cognitive terms.

Conclusions

It should be clear then that an environmental event need not be temporally contiguous with behavior to be its "cause." Causation is to be understood as correlations among classes of events, for instance, among stimuli and responses (Skinner, 1935; Staddon, 1973), and not as a mechanical transfer of energy. Just as physicists have learned that they need not ascribe to hook-and-eye mediators to explain action-at-a-distance, psychologists must learn to avoid hook-and-eye cognitive and physiological mediators to explain behavior that occurs in the absence of apparently compelling stimuli. One of the most important jobs of a behavior analyst is to investigate behavior-environment relationships. The apparent absence of immediate control in those relationships should promote scientific investigation, not speculation about supposed cognitive processes.

THE LANGUAGE OF BEHAVIORAL SCIENCE

We have responded to cognitive science in TEAB because it represents the emergence of a trend that we think is not in the best interests of behavioral science. Perhaps, though, we have overreacted, especially in regard to the use of mentalistic language (see p. 117); thus, let us make a few comments about language use in the behavioral sciences.

Cognitive scientists in TEAB argue that the use of cognitive terms and concepts neither implies any mentalistic preconceptions nor subsequently exerts mentalistic influence over psychological theory (cf. Shimp, 1976, p. 118; Wasserman, 1981, p. 249). In addition, they make the point that these terms and concepts are attractive for their heuristic value in raising interesting questions about behavior (e.g., Shimp, 1976, p. 121; Wasserman, 1981, p. 249, 253). We object to both these arguments—neither is necessarily so.

Scientific Language

First, cognitive terms are not neutral terms. They typically have an extensive history of use as mentalistic terms (Honig, 1978, p. 3). From a radical behavioral perspective a scientist's language and logic are verbal behavior and as such are susceptible to the variables that control behavior in general (Hineline, 1980; Moore, 1981; Skinner, 1957). Hence, because we all share a common mentalistic cultural heritage, cognitive terms cannot help but be controlled by variables other than the crude data and empirical relationships encountered in scientific practice. In turn, these terms may control behavior (e.g., tautological and mentalistic explanations) other than that which should occur in scientific practice (cf. Moore, 1981, p. 60-62). Thus, because cognitive processes are vague and often negatively defined, it seems unlikely that we can use cognitive terms or make judgments about their epistemological status without being influenced by mentalism. To quote from Maze (1954):

> It may for the most part be true that the actual verbal forms are nothing but ways of expressing the observed connections, but the mere fact of offering "intervening variable" statements makes it vaguely seem that our knowledge is being extended, and that we are "getting to understand the facts better" in the sense of seeing how they are produced. The attribution of events to occult forces is rarely explicit because then it is so blatantly unscientific; it creeps in unacknowledged and gains its influence by default, as it were—by our failing to look for the *actual* causes. (p. 234)

In light of this point about language use, let us reason by analogy. In the area of scientific quantification, which is a form of verbal behavior (Schnaitter, 1980, p. 154), behavioral scientists have taken rigorous methodological safeguards against the contamination and confounding of their data. If the quantification of data is influenced by variables other than those specified by experimental designs and procedures, then those data are considered to be unreliable and lacking in validity. In the same vein, the scientific community should be similarly intolerant of conceptual terms that permit extra-scientific factors to affect scientific practice. Unfortunately, behavioral scientists have not been as careful in avoiding contamination and confounding of their conceptual analyses. Perhaps this is true because the construction of mentalistic, common-sense explanations couched in cognitive language is strongly reinforced by listeners for whom similar explanations are at considerable strength due to the control exerted by long-standing and shared extra-scientific concepts of human activity. This is a behavioral trap into which behavior analysts must not fall. By speaking in ways that are not contaminated by mentalistic concepts, these extra-scientific influences can be minimized and verbal practices developed that are more strongly under the control of the scientific subject matter.[2]

Heuristics

As for heuristics, cognitivists argue that cognitive terms are attractive for their value in raising interesting questions about behavior that might not otherwise be asked—most notably about competence and knowledge. To quote Wasserman (1981): "Cognitive ideas provide a fertile source of investigable hypotheses whose experimental evaluation may tell us much about the process of animal behavior" (p. 253). While cognitive terms and concepts may prompt the asking of new questions, they may also preclude the asking of others—for instance those of the sort described in our sections on molar-molecular analyses and historical causation (p. 118-121). Moreover, there is no guarantee that the cognitive questions are useful or that cognitive answers promote the science of behavior. Even when such questions and answers may be useful, they are often so far removed from the level of direct behavior-environment interactions that they require several layers of translation and interpretation before they can be integrated with the known facts of behavior-environment relationships.

Beyond this, it is inaccurate for cognitive scientists to state that a behavioral psychology can not pose interesting questions and provide provocative analyses for complex problems. When analyzing knowledge, for example, behavioral analysts need not use mentalistic terms or invoke the existence of non-temporal, non-spatial events as explanations. Behavioral accounts of knowledge are also available. At a conceptual level, these accounts emphasize the effects of stimulus control and stimulus-stimulus relationships on behavior (cf. Sidman, 1978; Skinner, 1968). At an empirical level, experimental analyses of stimulus control and stimulus-stimulus relationships have demonstrated powerful effects and have been fruitful in providing accounts of what is called knowledge and competence without involving cognitive constructs or processes (e.g., Dixon, 1977; Sidman, 1971). In summary, the behavioral perspective is sufficiently heuristic and allows for the pursuit of research in areas typically left by default to cognitive psychology (Kantor, 1970; Sidman, 1979).

CONCLUSION

Cognitive scientists state that the movement of psychologists towards cognitive theory is a scientific necessity and a matter of heuristics. We, however, think not. We think it is more likely due to the *inherent difficulty of the subject matter* (Skinner, 1938); to the ease with which words can be manipulated, and in turn can manipulate, as opposed to the difficulty of conducting rigorous experimental analyses (Ferster, 1978); and to the influence of a lengthy and powerful cultural history (Kantor, 1963, 1969). Behavioral science cannot afford to be led astray, and it is the responsibility of behavior analysts to see that it is not. This can be done by

[2] An analogous issue is raised in contemporary feminism. The supposedly neutral language one uses to describe people (e.g., "he" and "man") and their activities (e.g., "his") does affect how we behave in regards to them (see Moulton, Robinson, & Elias, 1978). This process is damaging to objective descriptions and analyses of our social environments, and our behavior within those environments. Another example of how a supposedly neutral term can control what should be scientific behavior is the use in this paper of the acronym "TEAB" for "the experimental analysis of behavior." For some readers, "TEAB" may be paired with negative aspects of Kantor's 1970 *JEAB* article in which he criticized several aspects of the field. If "TEAB" sets the occasion for or elicits a negative reaction, then our point that supposedly neutral terms can have powerful effects is made.

maintaining verbal practices strongly under the control of behavior-environment relationships, by seeking empirical answers to difficult and complex questions, and by ascribing to a truly behavioral metatheory.

The reader might quite reasonably ask at this point about the value of papers such as this which present merely verbal arguments and which contribute no experimental proof to an empirical science. Science, however, is more than just research. Behavior analysis, of which TEAB is an integral part, takes its character, as Day (1980, p. 20) says, in terms of the contrasts it makes with other professional perspectives, of which cognitive science is one. Thus, commentaries on how TEAB is defined as a research enterprise and on how it differs from other perspectives can set the occasion for examining scientific practice and theory.

We do not assert, however, that what we have presented here is the "truth" about how TEAB must continue to progress, though we are strongly inclined to such a position. We have merely sought to illustrate that TEAB need not evolve in the directions urged by cognitive scientists in order to deal with complex issues in the analysis of behavior. Perhaps, as Kuhn (1970) has put it, the value of this sort of critique is not only to make distinctions between differing perspectives, but also to suggest or advise caution to those who might be tempted into new views that have yet to provide a superior theoretical and empirical account of the subject matter. When these new views are mentalistic in orientation, as is cognitive science, behavioral scientists should be especially careful of the dangers in adopting them. Much of their everyday, nonscientific behavior, as well as the field of psychology in general, is already controlled by mentalistic assumptions, and hence behavioral scientists are especially susceptible to the superficial ease and elegance of cognitive theory. From our perspective, cognitive science offers little that is new that is not also dangerous.

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