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On the Relation Between Behaviorism and Cognitive Psychology

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Cognitive psychology and behaviorism are often held to be competing, mutually exclusive paradigms in contemporary psychology. The present paper argues that cognitive psychology is actually quite compatible with the most widely recognized version of behaviorism, here designated as mediational S–O–R neobehaviorism. The paper argues this case by suggesting that neobehaviorist theoretical terms have tended to be interpreted as "hypothetical constructs." Such an interpretation permits neobehaviorist theoretical terms to refer to a wide variety of nonbehavioral acts, states, mechanisms, and processes, with extensive "surplus meaning." Consequently, an interpretation of neobehaviorist theoretical terms as hypothetical constructs can readily accommodate the kind of mental entities postulated by cognitive psychology.

Even casual observers of the contemporary scene can scarcely miss the tension between cognitive psychology and behaviorism, as a sort of paradigm clash (see Moore, 1983; Schnaitter, 1987). For example, Haugeland (1981) suggests that "Cognitivism in psychology and philosophy is roughly the position that intelligent behavior can (only) be explained by appeal to internal 'cognitive processes'" (p. 243). In contrast, Denny (1986) suggests that "The breath of fresh air provided by the S–R, behavioristic tradition, if overthrown, could set back an objective, scientific view of behavior many years . . . . We simply cannot afford a regression to dualism before we've even shed its remnants" (pp. 35–36). Clearly, strong feelings exist on both sides of the debate.

The purpose of the present paper is to critically analyze historical and conceptual relations between behaviorism and cognitive psychology (see also Moore, 1995). By so doing, we hope to further understand the similarities

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and differences between the two positions. We will begin with a brief review of the development of both behaviorism and cognitive psychology, starting with the "behavioral revolution."

The Behavioral Revolution

Texts in the history of psychology typically identify the first quarter of the twentieth century as the period of "the behavioral revolution." During this period, a more practical interest in analyzing behavior and specifying its determinants through experimentation replaced an interest in analyzing experience and specifying the contents of consciousness through introspection. Although Leahey (1992) has recently questioned whether these events should actually be called a "revolution," most historians accept that the years witnessed a significant reorientation in the subject matter and methods of psychology (e.g., "For a while in the 1920s it seemed as if all America had gone behaviorist," Boring, 1950, p. 645).

Indeed, retrospective analysis suggests that important events also took place during the second quarter of the twentieth century (Koch, 1964). Thus, if we want to talk of a "behavioral revolution," we may consider the relevant events as actually taking place in two successive phases.

Watson's Classical S–R Behaviorism and the First Phase of the Behavioral Revolution

The first phase began with the publication of Watson's (1913) famous behavioral manifesto. (Schneider and Morris, 1987, p. 28, have pointed out that Watson was apparently the first to use the term "behaviorism," as well as such cognate terms as "behaviorist" and "behavioristic.") Watson's behaviorism is generally designated as classical S–R behaviorism, to distinguish it at least chronologically from the various other forms that followed. As Koch (1954, 1964) has noted, classical behaviorism was "objective," it emphasized S–R associations and learning, it emphasized environmentalism over nativism, and it emphasized peripheral, rather than "centrally initiated" processes (Watson, 1913, p. 174). For that matter, said Watson, most phenomena that contemporary society thought were important were nothing more than "the result of old wives' tales and monks' lore, of the teachings of medicinemen and priests" (Heidbreder, 1933, p. 235).

Most scholars eventually judged classical S–R behaviorism as inadequate to account for the whole range of human behavior, notably because responses were not always correlated with eliciting stimuli in the way that classical behaviorism required. Thus, the second phase of the behavioral revolution began during the late 1920s and early 1930s.

Mediational S–O–R Neobehaviorism and the Second Phase of the Behavioral Revolution

The significant event during the second phase of the behavioral revolution was the rise of mediational S–O–R neobehaviorism. Mediational S–O–R neobehaviorism was characterized by the attempt to include mediating "organismic" variables that intervened between stimulus and response. By so doing, mediational neobehaviorists hoped to account for the difficult problems that classical behaviorism could not satisfactorily explain (see discussion in Koch, 1964).

One of the first mediational neobehaviorists was R.S. Woodworth (e.g, Woodworth, 1929), who explicitly proposed an S–O–R formulation. The "O" was meant precisely to accommodate such organismic variables as motives, response tendencies, and purposes, which were presumed to determine the effects of other stimuli. Shortly thereafter, Tolman formalized the introduction of "intervening variables" into psychology (see Smith, 1986, pp. 116 ff.). Still later, such other learning theorists as Clark Hull and Kenneth Spence followed with an ever-expanding set of "intervening variables," no longer necessarily related to Woodworth's original sense of "organic states" (Smith, 1986, chapter 7).

The mature form of mediational neobehaviorism emphasized the following principles: (a) that only publicly observable techniques were acceptable for securing and expressing data; (b) that the relevant variables were independent (stimulus) variables, dependent (response) variables, and mediating (intervening/hypothetical) variables; (c) that causation was to be accommodated according to a linear chain model: S ==> O ==> R; and (d) that psychological knowledge consists in theoretical inferences about the mediating/intervening acts, states, mechanisms, or processes going on in some other dimension, at some other level of observation, described in different terms, where behavior is used as evidence to validate the inferences. The process by which these principles evolved is in part the subject of the remainder of this paper. In any case, this general approach to behaviorism is widespread in contemporary psychology, although it takes many different guises.

Cognitive Psychology

Cognitive psychology is a relatively recent development in the field, at least when compared with behaviorism. For example, Gardner (1985, p. 28) identifies September 11, 1956, the date of the Symposium on Information Theory at MIT, as cognitive psychology's birthday. Historically, most cognitive psychologists suggest the roots of cognitive psychology lie in information and communication theory, cybernetics, mathematics and computer technol-

ogy, and verbal learning traditions (for historical overviews of the development of cognitive psychology, see Baars, 1986; Gardner, 1985; Knapp, 1986).

To be sure, the term "cognitive" applies to a broad range of positions in contemporary psychology. For present purposes, cognitive psychology is regarded as an attempt to understand and explain behavior in terms of the underlying mental acts, states, mechanisms, and processes through which environmental factors exert any influence, and which are therefore presumed to be responsible for the behavior. Thus, cognitive psychology characterizes itself as a discipline that seeks to specify in an abstract fashion the functional properties of internal phenomena that enable organisms to behave as they do in a given context. In contrast to some forms of behaviorism, most forms of cognitive psychology de-emphasize affect, context, culture, history, and a concern with overt performance qua performance. The position bears the imprint of Kantian rationalism and the a priori. It works from a top–down approach, mixing the "design stance" with the "intentional stance" (Dennett, 1978).

In many cases, the computer and the principles of "information processing" are regarded as offering a suitable metaphor to the working of what are presumed to be underlying mental states and processes. The appeal to the computer metaphor is predicated on the idea of internal states and representations that are then operated on according to various instructions, principles, or rules (see Norman, 1981; Palmer and Kimchi, 1986).

Importantly, the internal states are defined functionally, rather than in terms of any "stuff" that realizes their function. Continuing with the computer metaphor, we can say that the internal functional states of an Apple Macintosh or an IBM PC are realized in different ways in their various components (e.g., Motorola vs. Intel central processing units, disk drives, hard drives, random–access/read–only memory), although in the final analysis the same function may be accomplished with each set of hardware. Similarly, the internal functional states of the mind are realized in different ways for different individuals, although in the final analysis the same function is accomplished by those individuals.

The philosophy of mind that is most influential among cognitive psychologists is philosophical functionalism (see Block, 1980; Dennett, 1978; Fodor, 1968). Philosophical functionalism regards itself as a coherent materialist philosophy of mind. It is concerned with the functional design features of the mechanisms that accomplish the psychologically interesting work of human organisms. The functional design is independent of whatever specific hardware may realize that design, although appeal to various metaphors may be used to give the design some context. The principal feature is the appeal to causal "mental states" that are definable in terms which do not mention physics or chemistry, as would presumably be required by physicalistic defini-

tions of operationism and logical positivism. On this view, cognitive psychology is at once intentional, agent-oriented, and intensional. Indeed, cognitive psychology unselfconsciously prides itself on its mentalism, precisely because mentalism is to be distinguished from behaviorism, which cognitive psychology vigorously rejects.

Three Common Assumptions about the Relation Between Behaviorism and Cognitive Psychology

Baars (1986), Flanagan (1984), and Gardner (1985) have expertly described what they see as the relation between behaviorism and cognitive psychology. To provide the basis for the present exposition, we have formulated certain of their key points and expressed them as three "assumptions." Let us now assess the validity of each of these three assumptions. We will use the most widely recognized form of behaviorism, mediational neobehaviorism, as the exemplar.

Assumption 1: Behaviorism is Concerned Solely With Publicly Observable Phenomena, Whereas Cognitive Psychology is Concerned With Unobservable Underlying Phenomena

According to Assumption 1, behaviorism is primarily concerned with publicly observable phenomena, because of some link with logical positivism. In contrast, the committment to functionalism means that cognitive psychology is free from the restrictions of logical positivism. Consequently, it is free to offer explanations involving unobservable underlying mental phenomena.

The rejoinder to Assumption 1 is straightforward. Smith (1986) notes that the relation between mediational neobehaviorism and logical positivism is complex; the two are not necessarily isomorphic. In any case, even if a close relation between mediational neobehaviorism and logical positivism is assumed, logical positivism was intensely and explicitly concerned with phenomena that were not publicly observable, such as those in relativity theory and quantum mechanics. Thus, logical positivism was forged in the crucible of dealing with unobservables.

Similarly, mediational neobehaviorism was always intensely and explicitly concerned with phenomena that were not publicly observable. Readers will recall that Hull (1943) appealed to an "oscillation factor" (pp. 304 ff.) and "afferent neural interactions" (pp. 349 ff.), and Tolman (1948) to "cognitive maps," none of which is merely a small-scale facsimile of publicly observable behavior. Thus, this assumption simply does not apply to mediational neobehaviorism.

Indeed, consider the following passages from two neobehaviorists. The first is from Kimble (1985):

> In a general way, the operational point of view did nothing more than insist that terms designating unobservables be defined in ways that relate them to observables . . . . Obviously, there is nothing in this formula to exclude mentalistic concepts. In fact, the whole point of it is to admit unobservables. (p. 316)

The second is from Amsel (1989):

> It has never been debatable — certainly not among neobehaviorists — that explanations should involve constructs [representing nonbehavioral states and processes that go on inside organisms] . . . . And it is really not debatable either that stimulus–response theory refers, as it did in Hull's 21 papers in *Psychological Review* . . . , as well as his *Principles of Behavior* (1943), to hypothetical states and processes that "go on inside organisms." . . . [T]he fact is that for the present S–R theorist, as I think for Hull and certainly for Spence, the mediating machinery defined as hypothetical Ss and Rs are no more or no less permissible, and no more or no less observable, than are the cognitive constructs the "emergent behaviorists" are now willing to permit . . . . It is an essential contradiction to refer to models of observables; and as I indicated earlier, such a characterization of S–R models does not fit the neobehaviorism of Hull, Spence, Miller, or Mowrer — or any other version of neobehaviorism, including my own. (pp. 50–51; 71)

To be sure, the use of the passages above to support the present point is not in keeping with either Kimble's or Amsel's agenda. In particular, Amsel argues that the neobehavioristic approach to dealing with unobservables is preferable to the cognitive approach because the concepts of the neobehavioristic approach are derived from physiology (p. 41) and classical conditioning (pp. 72–73), whereas the concepts of the cognitive approach are not so derived. On Amsel's view, these differences contribute to the fundamental difference between the two approaches. Nevertheless, the explicit focus on the matter of unobservables, evidenced in the quotations above from Kimble and Amsel, readily illustrates the present point by showing that neobehaviorism was always concerned with appealing to unobservable, nonbehavioral processes in explanations of behavior, in contrast to the usual understanding of behaviorism.

Assumption 2: Behaviorism is Merely Observational and Descriptive, Whereas Cognitive Psychology is Theoretical and Explanatory

According to Assumption 2, behaviorism is purportedly "limited" to descriptive endeavors because of its concern with publicly observable phenomena. In contrast, cognitive psychology is more theoretical, and consequently able to secure adequate explanations of behavior, precisely because of its functional theoretical stance.

The following passage from Baars (1986) illustrates Assumption 2:

> Cognitive psychologists also have a claim about the domain of scientific psychology — essentially, it is that psychologists observe behavior in order to make inferences about underlying factors that can explain the behavior. They agree with behaviorists that the data of psychology must be public, but the purpose of gathering this data is to generate theories about unobservable constructs, such as "purposes" and "ideas," which can summarize, predict, and explain the data . . . . A psychological theory is a network of such constructs, serving to summarize empirical observations, predict new results, and explain them in an economical way. Like behaviorism, cognitive psychology is primarily a metatheory for psychology, one that simply encourages psychologists to do theory . . . . No longer is it thought necessary for theoretical constructs to resemble visible stimuli and responses, or to adhere to rigid conceptions of theoretical parsimony . . . . By the same token cognitive psychology is an act of imagination that permits wider latitude in imagining explanations for behavior. Whereas behaviorism taught psychologists to respect empirical evidence, the cognitive metatheory may make it possible to do good theory. (pp. 7; 144–145)

The rejoinder to Assumption 2 is similarly straightforward. As noted above, mediational neobehaviorism made liberal use of theoretical constructs that related to underlying, unobservable processes. The whole history of "learning theory" from Tolman to Hull to Spence is ample testimony to this liberal use. Thus, despite Baars' (1986) insights, Assumption 2 simply does not apply.

Assumption 3: Attempts by Behaviorists to Incorporate Theoretical Terms are Generally Inadequate Because the Theoretical Terms are Defined With Respect to Publicly Observable Phenomena, Whereas Cognitive Theoretical Terms are Superior Because They are More Liberally Defined

According to Assumption 3, neobehaviorists regard any terms that do not refer to observables as theoretical terms, which are then operationally defined with respect to some publicly observable phenomenon, such as a disposition to engage in publicly observable behavior or a brain state. This generalization holds true for "mental" terms as well. In contrast, cognitive psychologists do not define mental terms with respect to publicly observable phenomena, which means that such terms can be used more freely and prolifically than in behaviorism. As a consequence, cognitive psychology enjoys greater explanatory power.

Three specific criticisms in the literature of functionalism/cognitive psychology evidence Assumption 3. The first is generally attributed to the philosopher Hilary Putnam, and is called the "perfect actor" counterexample (see Putnam, 1980). Suppose that talk of pain is held to be meaningful only when there is some publicly observable pain-related behavior. Now suppose that there is a world populated by a race of particularly stalwart Spartans, who when they have pains do not act like it; instead of moaning and groaning, they smile and sing. Analogous cases might be actors who act like they are in pain when they really are not, or those who are in pain but are para-

lyzed and cannot act at all. In such cases, the presence or absence of pain is not systematically related to the presence or absence of observable pain behavior. Thus, mentalists argue that behaviorism is incorrect when it asserts that talk of pain is meaningful only in the context of some publicly observable, pain-related behavior.

The second criticism is generally attributed to the philosopher Roderick Chisholm (1957). According to this criticism, treating mental terms as dispositions to engage in publicly observable behavior only creates an endless chain of such dispositions, without providing a causal explanation of the behavior. For example, suppose that being in pain from a headache is taken to mean nothing but having a disposition to take an aspirin. The next question is, Why is one disposed to take an aspirin? To answer this question, one must then say that one takes the aspirin because one also has the desire to get rid of the headache, the belief that the aspirin exists, the belief that taking aspirin reduces headaches, and so on. The upshot is that in order to explain what causes the behavior, one must always posit a causal sequence of mental events (see Fodor, 1981a). Otherwise, the behavior in question never gets explained. Thus, mentalists argue against troublesome regressions to observable behavior, as in (they presume) the dispositional analyses in which any kind of behaviorism engages. Instead, mentalists advocate direct appeals to the causal efficacy of underlying mental states, as in cognitive psychology.

The third criticism concerns type vs. token physicalism, and the interpretation of mental phenomena as brain states. Suppose one interprets mental phenomena as neurophysiological brain states. On this view, to say that individuals are in pain means that one set of fibers in their brains is firing, to say that individuals believe aspirins will cure headaches means that another set of fibers of their brains is firing, and so on. Mental items are identical with states and events occurring in individuals' central nervous systems, such that every mental event is identical with and reducible to some corresponding neurophysiological event.

Cognitive psychologists argue that although tokens (instances) of mental states are surely physical, types (classes) of mental states can surely be realized in multiple forms, not one and only one form. Types of mental states are therefore differentiated by their function, rather than by their physical properties. Cognitive psychologists further argue that when behaviorists interpret mental states in terms of brain states, behaviorists commit themselves to distinguishing types of mental phenomena by their physical properties, which is demonstrably false. Therefore, cognitive psychologists argue, behaviorism is demonstrably false (see Block, 1980).

Further Analysis of the Third Assumption

As suggested above, Assumption 3 and its correlated criticisms have garnered extensive attention in the literature. However, as sophisticated and appealing as the features of Assumption 3 are presumed to be, they actually do not pertain to neobehaviorism. To see why they do not, let us trace some developments in the history of logical positivism and neobehaviorism.

The Distinction Between Observational and Theoretical Terms

A traditional distinction in philosophy of science, generally attributed to logical positivism, is between observational and theoretical terms. According to this distinction, observational terms are first-order terms relating to directly measurable physical properties of objects, events, or situations. Theoretical terms are higher-order terms that are logical constructions from observational terms. For present purposes, such other terms as logical constructs, theoretical constructs, logical terms, theoretical concepts, mediating concepts, inferred constructs, and other permutations and combinations are regarded as synonymous. At least originally, these theoretical terms were thought to be exhaustively reducible to observational terms. That is, the meaning of any theoretical term was completely determined by its relation to observational terms, for example, as expressed in an equation.

Operationism

As suggested above, by the mid-1930s mediational neobehaviorists began to treat mediating variables in their explanations as theoretical terms, and referred to them as "intervening variables." In doing so, they faced the problem of how to remain scientifically respectable in their formulations. P.W. Bridgman (1928) had earlier proposed the principle of "operationism" in physics, and this principle seemed to offer some promise to psychologists (see Boring, 1950, pp. 653–659).

As Bridgman (1928, p. 5) originally proposed it, operationism was the position that scientific concepts are synonymous with the set of operations by which they are measured. The purpose of operationism was to generate agreement about the concept in question, resolve disputes, and allow scientists to understand each other. In addition, operationism would further ensure that efforts were directed toward resolving genuine scientific problems, and not "metaphysical" pseudoproblems.

Accordingly, the operational definition of a concept provided an exhaustive meaning of a term. The concept was entirely reducible to the measurement operation involved, with no remainder. The length (of a table) and distance

(to the sun) were regarded as different concepts because different measurement operations were involved, say via a tape measure vs. triangulation.

Some representative samples of scientific verbal behavior of the time will illustrate the positions of the neobehaviorists involved, and their commitment to operationism. First, consider the position of E.C. Tolman (1936/1951):

$$B = f_2^x (I_a, I_b, I_c, I_f \ldots H, T, A)$$

> It is such intervening I's, whether simply or complicatedly related to the independent variables and to one another, which are all that my operational behaviorism finds in the way of mental processes. These I's are "demands," discriminanda," "manipulanda," "means–ends fields," "traits," "capacities," and the like. They are objective entities defined in terms of the f_2 functions which connect them to the S's, P's, H's, T's, and A's, on the one hand, and to the final B, on the other . . . . [Operational behaviorism] 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 variable are determined. (pp. 117–118; 129)

C.L. Hull (1943) adopted a similar position:

> At bottom this is because the presence and amount of such hypothetical factors must always be determined indirectly. But once (1) the dynamic relationship existing between the amount of the hypothetical entity (X) and some antecedent determining condition (A) which can be directly observed, and (2) the dynamic relationship of the hypothetical entity to some third consequent phenomenon or event (B) which can also be directly observed, become fairly well known, the scientific hazard largely disappears . . . . When a hypothetical dynamic entity, or even a chain of such entities each functionally related to the one logically preceeding and following it, is thus securely anchored on both sides to observable and measurable conditions or events (A and B), the main theoretical danger vanishes. This is at bottom because under the assumed circumstances no ambiguity can exist as to when, and how much of, B should follow A. (p. 22)

Kenneth W. Spence (1944) was also a central figure:

> According to Hull and Tolman, theoretical constructs, or intervening variables, have to be introduced into psychology either when we do not know all the important variables entering into a set of experimental events, or the precise nature of the interrelating function is not known . . . . The task of the psychologist . . . is to discover the precise nature of the interrelations holding within this set of variables . . . . But in such a situation, involving as it does a large number of variables, the function relating the dependent and independent variables is so complicated that we are unable to conceive of it directly. It is necessary, say Hull and Tolman, to proceed by conceiving of it as broken down into successive sets of simpler component functions. These component functions begin by introducing new intervening constructs defined in terms of the independent variables. Further intervening variables are then introduced by stating them as functions of the first set of intervening constructs, until finally the dependent behavior variable is postulated to be a function of one or more of the intervening variables . . . . (p. 59)

Spence (1936) had anticipated this instrumentalist orientation in an earlier paper:

> It is not . . . the theoretical concepts themselves and their hypothetical relations (principles) that must necessarily coincide or agree with the facts of experience, but it is the logical consequences or deductions that follow from the theories. The test of the adequacy of any theoretical structure is that the logical consequences that flow from it coincide with the events of experience, i.e., the learning behavior of the animal in this instance. The descriptive characteristics of the facts of behavior do not need to be found in the basic theoretical concepts and principles and conversely, the failure to find anything in learning behavior descriptively resembling the theoretical concepts is no disproof of their adequacy as a scientific explanation. Finally, the question as to whether the theoretical concepts do or do not correspond with reality is a problem not for science but for philosophy. The scientist can justify such ideal constructs wholly from the pragmatic standpoint that they serve as an aid to the integration and comprehension of the observed phenomena. He is under no obligation to imply nor yet deny the possibility of their correspondence with reality. (pp. 446–447)

Such locutions as "wholly" defined (Tolman) and "securely anchored" (Hull), as well as the assertion that the descriptive facts of behavior do not need to correspond to the theoretical structures (Spence), imply that these views all take the intervening variables involved in theoretical explanations to be exhaustively defined with respect to their locus in an equation, with the accompanying publicly observable measures. These views were consistent with Bridgman's (1928) original position on operationism.

Partial Definitions and the Rise of Logical Empiricism

Let us now return to philosophy. Recall that in the late 1920s and early 1930s, logical positivists exhaustively defined theoretical terms. However, by the mid–1930s, Carnap realized that exhaustive physicalistic definitions would not work. One problem revolved around the logical status of the inferred entity when the test conditions were not in effect (for further discussion of the problem of the "counterfactual conditional," see Zuriff, 1985, pp. 59 ff.). A second problem was that scientific concepts were flexible and probabilistic, not static in the sense implied by exhaustive definitions. A third problem was that if the mediating theoretical terms were exhaustively reducible to publicly observable operations, with no remainder, then they added nothing, and were logically superfluous. This third problem came to be known as the "theoretician's dilemma" (Hempel, 1958).

In recognition of these problems, Carnap (1936, 1937) worked out "partial definitions" and "reductive chains." These moves freed the interpretation of theoretical terms from exhaustive definitions, and required simply that theoretical terms be logically derived from public observables. Hence, surplus meaning was explicitly allowed. (Smith, 1986, p. 28, suggests the entire

movement after this time be known as "logical empiricism," to officially acknowledge the significant shift in position.)

Readers will recall that logical positivism is often regarded as intimately supporting behaviorism because of some common reliance on public observability. Such critics as Fodor (1968) explicitly condemn purported attempts to translate mental events into publicly observable behavior on the part of logical positivism and behaviorism. Consider now the following passage from the writing of Herbert Feigl (1963), a distinguished logical positivist, in which he explicitly addresses the relation between "mental" events and publicly observable measures:

> Statements about mental events are not translatable into statements about (actual or possible) overt behavior . . . . The meaning of statements (at least in one very important sense of "meaning") is to be identified with their factual reference, and not with their evidential basis. The slogans of early logical positivism and of ultra-operationism about meaning and verification — while helpful in the repudiation of transcendent metaphysics — despite their imprecision were far too restrictive to do justice to the actual conceptual structure of knowledge. Given this general outlook it becomes obvious that the naive peripheralistic forms of behaviorism must be repudiated and their shortcomings remedied by the admission of central states and processes as the genuine referents of psychological terms . . . . Concepts such as memory trace may be taken to refer to (as yet very incompletely specified) central conditions. (pp. 247–248; 252)

Clearly, logical positivist philosophers had retreated from the formerly held principle of exhaustive definitions in terms of publicly observable measures.

Intervening Variables and Hypothetical Constructs in Neobehaviorism

In any case, mediational neobehaviorists (and particularly those who embraced the conventional interpretation of operationism) faced similar issues in their theorizing. For example, Hull (1943) did not restrict his concept of habit strength to just the number of reinforced trials. Rather, he freely speculated about possible underlying neurophysiology:

> [I]t is important to note that habit strength cannot be determined by direct observation, since it exists as an organization as yet largely unknown, hidden within the complex structure of the nervous system. (p. 102)

By referring to an unknown organization, hidden in the nervous system, Hull opened the door to partial definitions, with surplus meaning. Spence (1944) tried heroically to clarify Hull's usage:

> Quite in contrast to [Tolman's] approach, Hull has ventured to make guesses as to the precise nature of the functions introducing the intervening variables in his theoretical formulations . . . . Despite the neurophysiological tone of some of the terms that Hull

> employs to designate these constructs, the mistake should not be made of interpreting them as physiological concepts. Their scientific meaning is given only by the equations introducing them, and in this respect they are strictly comparable to many similar, abstract, mathematical constructs employed by the physicist in his theorizing. The use of neurophysiological terms and such additional statements as Hull sometimes makes as to their possible locus in the nervous system merely serve the purpose of providing experimental hints to persons interested in such matters. It may or may not turn out that they represent actual neurophysiological states or conditions that will some day be measurable by independent neurophysiological procedures. (pp. 60–61)

Nevertheless, confusion reigned in the mid–1940s as to the nature of theoretical concepts in psychology, particularly with regard to those of the most influential neobehaviorist, Clark Hull. Were theoretical terms exhaustively reducible to observables?

In recognition of this problem, MacCorquodale and Meehl (1948) proposed a linguistic convention in an effort to calm the epistemologically troubled waters. They proposed that theorists recognize they used two sorts of theoretical terms, and implicitly suggested that *either* sort of theoretical term was acceptable. They then proposed the identifying characteristics of each sort. The first sort involved theoretical terms that were exhaustively reducible to a set of publicly observable, manipulable variables. These terms involved no hypothesis as to the existence of unobserved entities or the occurrence of unobserved processes. The terms had no surplus meaning, or meaning beyond the immediate observations from which they are derived. MacCorquodale and Meehl proposed that this first sort be called "intervening variables." This treatment was consistent with the original sense of operationism, and the original sense certainly of Tolman and Spence, although Hull was equivocal, notwithstanding Spence's attempts to clarify Hull's practices. (Note that some writers, including MacCorquodale and Meehl, may also use "intervening variable" to refer to any theoretical term; to avoid terminological confusion, we use "theoretical term" as the overarching, generic term; we then use "intervening variable" as the first of two specific sorts of theoretical terms.)

MacCorquodale and Meehl then proposed a second sort of theoretical term. These terms referred to a possibly existing, but at the moment unobserved entity or process. If the existence of a process or entity was entertained, then presumably the process or entity has another property as well; this property might be observed at some time in the future. Thus, because such terms are thought to refer to processes or entities that possibly existed, these terms do allow surplus meaning, or meaning beyond the set of publicly observable operations from which they are derived. MacCorquodale and Meehl proposed that this second sort be called "hypothetical constructs." (Again, to avoid terminological confusion, we refer to a "hypothetical construct" as the second of two specific sorts of theoretical terms.)

MacCorquodale and Meehl regarded appeals to both intervening variables and hypothetical constructs as permissible in theoretical statements, so long as the usage was consistent. (The distinction has always been controversial; for example, Turner, 1967, p. 259, organized 40 references into five groups, with different ways of distinguishing between intervening variables and hypothetical constructs; see also Zuriff, 1985, chapter 4 and p. 290.)

Whether MacCorquodale and Meehl's (1948) proposal was essentially consistent with Carnap's (1936, 1937) moves of 12 years earlier is a matter of interpretation (see Carnap, 1956). In any case, what is clear is that MacCorquodale and Meehl's proposal liberalized the principle of operationism substantially, and theorists once again felt reassured their verbal–theoretical practices did not conflict with their actual experimental practices (although Koch, 1954, was manifestly unconvinced). Tolman (1949), who introduced theoretical terms to psychology, quite explicitly abandoned his original intervening variable interpretation and embraced the hypothetical construct interpretation:

> 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 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 look 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)

Let us now bring some closure to this part of the story. The criticisms of neobehaviorist theoretical terms may possibly be justified given an intervening variable interpretation of those theoretical terms. *However, the dominant interpretation is a hypothetical construct interpretation, which allows for surplus meaning, including the kind of meaning imparted in cognitive psychology.* Consequently, an assumption that mediational neobehaviorism and cognitive psychology differ by virtue of their respective treatments of theoretical terms is not supported.

What then about the specific interpretations of theoretical/mental terms (a) as dispositions to engage in publicly observable behavior, and (b) as brain states? Let us first consider the matter of interpreting theoretical terms as behavioral dispositions.

Neobehaviorism and the Interpretation of Mental Terms as Dispositions to Engage in Publicly Observable Behavior

Quine (1974, p. 8) suggests that a disposition is some physical property, inherent in an object, by virtue of which a given set of circumstances is

likely to cause some event to take place concerning that object. For example, one might attribute the disposition of "solubility" to a sugar cube when placing the sugar cube in water causes the sugar cube to dissolve, and the disposition of "brittleness" to glass when throwing a rock at the glass causes the glass to break. Looked at one way, dispositions are physical states of affairs, such as specific though probably unspecified arrangements in the microstructure (Quine, 1974, p. 13). Looked at another way, dispositions are conditional probabilities, that is, publicly observable symptoms of the property that are liable to obtain in particular circumstances (Hocutt, 1985, p. 93).

Dispositional analyses have a long history in philosophical psychology. Early logical positivist philosophers, such as Carl Hempel (1935/1949), attempted to make sense out of the "mental," subjective language that prevailed in psychology during the early part of the century. They believed they could best do so by relating mental language to "dispositions," which were then defined in terms of publicly observable phenomena. The movement was given the name "logical behaviorism." On this view, to attribute "mental" properties to a state of mind or trait of character was unacceptable in a scientific statement because the mental properties were not publicly observable and therefore were not scientific. Logical behaviorists argued instead that to attribute a state of mind or trait of character to individuals is to say that their bodies are in a condition that disposes them to behave in a particular way. The condition of the body could then be detected, at least in principle, through such publicly observable measures as pointer or meter readings. Thus, logical positivists such as Hempel argued that statements invoking states of mind or traits of character could be translated without remainder into statements about physical conditions that prevail within the body, which are then correlated with subsequent behavior (Hocutt, 1985, p. 88). The question of what it meant to say an individual was in pain was usually regarded as a question of whether the individual could be described as disposed to engage in pain-related behavior or language. (Readers will doubtlessly recognize that dispositional analyses are intimately associated with analytic philosophy, particularly the work of Ryle, 1949, and Wittgenstein, 1953; analytic philosophy is concerned with a "conceptual analysis" of language in use, and of the precise circumstances in which particular combinations of words are uttered, rather than pointer or meter readings; however, dealing with the complexities of dispositional analyses in analytic philosophy is beyond the scope of the present paper.)

In any case, recall that cognitive psychologists argue most vehemently against the adequacy of behaviorism by questioning the interpretation of mental states as "dispositions," which are then related to publicly observable behavior (e.g., Chisholm, 1957). However, neobehaviorists interpret most theoretical terms as hypothetical constructs, rather than as intervening vari-

ables. If dispositions are regarded as theoretical terms, interpreting dispositions as hypothetical constructs admits surplus meaning. Indeed, readers will recall that Feigl (1963) suggested behavior should be regarded as the evidence for using the term, not the exclusive and exhaustive referent of the term. Similarly, Carnap (1956) framed the problem as follows:

> In a way similar to the philosophical tendencies of empiricism and operationism, the psychological movement of Behaviorism had, on the one hand, a very healthful influence because of its emphasis on the observation of behavior as an intersubjective and reliable basis for psychological investigations, while, on the other hand, it imposed too narrow restrictions. First, its total rejection of introspection was unwarranted . . . . . Secondly, Behaviorism in combination with the philosophical tendencies mentioned led often to the requirement that all psychological concepts must be defined in terms of behavior . . . . [T]he interpretation of a psychological concept as a theoretical concept, although it may accept the same behavioristic test procedure based on S and R, does not identify the concept (the state or trait) with the pure disposition . . . .
>
> The distinction between intervening variables and theoretical constructs, often discussed since the article by MacCorquodale and Meehl, seems essentially the same or closely related to our distinction between pure dispositions and theoretical terms. "Theoretical construct" means certainly the same here as "theoretical term", viz., a term which cannot be explicitly defined even in an extended observation language, but which is introduced by postulates and not completely interpreted. (pp. 70–71; 73)

Carnap's terminology differs slightly from that used by MacCorquodale and Meehl, and may be the cause of some confusion. When Carnap (1956) uses "pure disposition," he is referring to the original intervening variable interpretation of theoretical terms, where a theoretical term is exhaustively defined with reference to publicly observable measures and no surplus meaning is involved. In contrast, when he uses "theoretical term" or "theoretical construct," he is referring to a hypothetical construct interpretation, where a theoretical term is only partially interpreted with reference to publicly observable measures and surplus meaning is involved (e.g., in Carnap's passage above, "a term which cannot be explicitly defined even in an extended observation language, but which is introduced by postulates and not completely interpreted"). Nevertheless, allowing for these terminological differences, we can see that the term disposition, when interpreted as a hypothetical construct, can "mean" (i.e., refer to) virtually anything, not simply something that is publicly observable. Thus, a criticism that dispositions refer to nothing but publicly observable phenomena, such as bodily states, is off the mark.

In addition, consider Hocutt's (1985) treatment of the relation between dispositions and publicly observable behavior. Hocutt distinguishes between intensional, connotative meaning on the one hand and extensional, denotative meaning on the other. He suggests that if dispositions are taken simply to denote behavior, e.g., symptoms, then a given person need only respond

differently when in pain than when not in pain (p. 89). Consequently, dispositional analyses mean simply that different states of mind require different forms of behavior (p. 90). Again, a claim by cognitive psychology that behaviorism is not adequate because it engages in dispositional analyses is not necessarily damaging.

Neobehaviorism and the Interpretation of Mental Terms as Brain States

Let us now consider the interpretation of mental terms as brain states, and the matter of type vs. token physicalism. Using Hocutt's (1985) analysis above, we can say that if being in pain implies being in one brain state and having a belief implies being in another brain state, a given individual's central nervous system need only be different when the individual is in pain as compared to when the individual has a belief. There is no necessary comparison to other individuals.

To be sure, an analysis stating that pain was for every individual in every instance the firing of a particular set of fibers at a particular set of stereotaxic coordinates, and that belief was for every individual in every instance the firing of another particular set of fibers at another particular set of stereotaxic coordinates, would be a stronger analysis. However, the absence of this sort of specific connection does not invalidate the argument. In principle, all that is necessary is for one set of fibers to be active when the individual is said to be in one state and a different set of fibers to be active when the individual is said to be in another state. As before, a claim by cognitive psychology that behaviorism is not adequate because it interprets mental states as brain states, and thereby confuses types and tokens, is not necessarily damaging.

The Question of Mediation

Given that neobehaviorism is characterized as mediational, questions can be raised as to whether contemporary cognitive psychology can be characterized as mediational in a related sense. Indeed, most cognitive psychologists see themselves as thankfully liberated from mediational, mechanistic, associative, linear chaining models of behavior; that was the whole point of the movement. In short, what is the basis for the present argument that cognitive psychology is nevertheless a mediational approach at heart?

At issue is what is meant by the term "mediational." To be sure, there are numerous reasons to question the adequacy of *linear* mediation mechanisms involving "single–stage" Watsonian associative S–R principles in explanations of complex behavior. Lashley's (1951) classic paper on the problem of serial order in behavior reviewed the evidence against these sorts of associative mechanisms in some detail. A simple example is that the rapid finger

movement in typing exceeds the capability of the relevant neurophysiology to give the requisite feedback.

The development of psycholinguistics in the 1960s and 1970s, led by cognitive psychologists, provides further evidence challenging the adequacy of single–stage mediational approaches. For example, Fodor (1965) argued that if mediational theory is to have any explanatory force (Fodor chose Mowrer's, 1960, mediational theory as the exemplar), each mediating response can be tied to one and only one overt response. However, this one-to-one correspondence is difficult to accept if the mediators are the broad, affective sorts of reactions Mowrer presumed them to be. Ultimately, Fodor concluded that an account based on this interpretation of mediation is seriously deficient (see also Fodor, 1981b, and Fodor, 1983, pp. 22–38, for related analyses).

Interviews with G.A. Miller of Harvard and J.J. Jenkins of the University of Minnesota in Baars (1986, pp. 198–223; 237–252) give additional insight into the development of psycholinguistics, and the accompanying rejection of the mediational approaches of the time. The focal importance of experimental data in rejections of a mediational approach cannot be underestimated (e.g., Bever, Fodor, and Garrett, 1968; Bransford and Franks, 1971; Fodor and Bever, 1965). Finally, Chomsky's arguments (e.g., 1959, and as summarized in Chomsky, 1990) are often cited as devastating to any behavioristic approach to language that involves mediation.

However, the cognitive rejection of mediation mentioned above appears to be based on a single–stage, intervening variable interpretation of the mediating entities and processes. As mentioned earlier, intervening variables require exhaustive definition, whereas hypothetical constructs do not. To be sure, the mediating theoretical terms of *some* neobehaviorists were intervening variables. However, given that most of the theoretical terms deployed in mediational neobehaviorism were hypothetical constructs, rather than intervening variables, the general cognitive rejection of neobehaviorism because the latter appeals to mediational concepts seems to miss the mark. Given a hypothetical construct interpretation of the mediational terms employed by mediational neobehaviorists, we can argue that no qualitative inconsistency exists between the theoretical approach taken by mediational neobehaviorists and the theoretical approach taken by cognitive psychologists, which involved hypothetical "representations and rules." The internal phenomena of the cognitive psychologists are generally orders of magnitude more complex than most of the hypothetical constructs of the neobehaviorists. Nevertheless, the two approaches differ only in degree, not in kind.

Reconsidering the Relation

To recapitulate, the present argument is that the relation between neobehaviorism and cognitive psychology differs appreciably from that portrayed in much of the contemporary literature by both cognitivists (e.g., Baars, 1986; Flanagan, 1984; Gardner, 1985) and neobehaviorists (e.g., Amsel, 1989). More specifically, given that theoretical terms are interpreted as hypothetical constructs, rather than as intervening variables, we can offer the following two conclusions:

1. The theoretical terms of mediational neobehaviorism are compatible with the mental acts, states, mechanisms, and processes of cognitive psychology.

2. Cognitive psychology is compatible with mediational neobehaviorism, and is not its revolutionary replacement.

On this view, cognitive psychology may legitimately be considered as compatible with the mediational neobehaviorism of Tolman and Hull. In fact, the "advantages" that cognitivists claim for their position over neobehaviorism are not even genuine differences, let alone "advantages." Similarly, the criticisms of cognitive psychology offered by neobehaviorists (e.g., as in Amsel, 1989) are not selective; ironically, they could apply equally well in principle to neobehaviorism. Perhaps cognitivists employ more hypothetical constructs, as opposed to intervening variables, than do neobehaviorists; perhaps cognitivists use a higher proportion of hypothetical constructs among their theoretical terms than do neobehaviorists; almost certainly the internal phenomena invoked by cognitivists are much more sophisticated than those of the neobehaviorists. Notwithstanding these possibilities, both mediational neobehaviorism and cognitive psychology seem to employ mediating constructs that in principle seem quite consistent with each other, despite the traditional argument to the contrary.

The literature does contain occasional references to the compatibility between neobehaviorism and cognitive psychology, although the compatibility is not always analyzed extensively. For example, Simon (1992), a cognitive psychologist, observed that the

> "cognitive revolution" . . . did not destroy either behaviorism or Gestalt psychology. It drew liberally upon both of them, both for experimental data and for concepts. The productions of information processing psychology are natural descendants of the familiar stimulus–response links of behaviorism (though not identical with them). (pp. 150–151)

Similarly, the eminent cognitive psychologist Patrick Suppes (1975) surveyed developments in the discipline of professional psychology and concluded as follows:

> [I]n neobehaviorism as opposed to classical behaviorism it is quite appropriate to postulate a full range of internal structures, ranging from memory hierarchies to language production and language comprehension devices that cannot be, from the standpoint of the theory, directly observed . . . . It is my view that the approach of cognitive psychologists or of psychologists interested in complex problem solving or information processing (Newell and Simon, 1972, is a good example) could be fit within a neobehaviorist framework if a proper amount of structure is assumed and not mastered from scratch . . . . There is not a formal inconsistency between the two viewpoints. (pp. 270; 279–280)

One analysis that is a bit more extensive is that of the historian of psychology Thomas Leahey. A key component of Leahey's analysis is the concept of "behavioralism," which Leahey (1994, p. 138) defines as the attempt to predict, control, explain, or model behavior, in which one may or may not refer to conscious or unconscious mental processes. Behavioralism is aimed at behavior; consciousness — the mind — is not the object of study, although it may be called on to explain behavior. According to Leahey (1994),

> While it was a major — perhaps the major — theoretical position in the 1950s, mediational behaviorism mainly proved to be a bridge linking the inferential behaviorism of the 1930s and 1940s to the inferential behavioralism of the 1980s: cognitive psychology . . . . The mediationalists' commitment to internalizing S–R language resulted primarily from their desire to perserve rigor and avoid the unscientific character of "junkshop psychology." In essence, they lacked any other language with which to discuss the mental processes in a clear and disciplined fashion, and took the only course they saw open to them. However, when a new language of power, rigor, and precision came along — the language of computer programming — it proved easy for mediational psychologists to abandon their r–s life raft for the ocean liner of information processing. (pp. 274–275)

Leahey (1994) continues:

> Information-processing psychology is a form of behavioralism. It represents a continuing conceptual evolution in the psychology of adaptation . . . . Perhaps to those involved, the revolt against S–R psychology was a scientific revolution; but viewed against the broader framework of history, the revolt is a period of rapid evolutionary change, not a revolutionary jump. (p. 317)

Leahey argues for compatibility between mediational neobehaviorism and cognitive psychology on the basis of formal similarity. The present approach goes farther and argues that the basis for compatibility is the interpretation of neobehaviorist theoretical terms as hypothetical constructs, which is what provides the formal similarity between neobehaviorism and cognitive psychology.

To be sure, the relation between cognitive psychology and behaviorism is complex. All positions to which the term "behaviorism" is conventionally applied are not compatible with cognitive psychology. For example, as Moore (1983, 1990, 1992), Schnaitter (1987), and Skinner (1985) point out, the "radical behaviorism" of B.F. Skinner is clearly not. However, if at

least one form is — mediational neobehaviorism — then analysts may have to take greater care in lumping neobehaviorism together with other positions under the common heading of behaviorism, as if they are all exemplars of a common disciplinary matrix, and in comparing any of those positions with cognitive psychology.

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