CLARK HULL, ROBERT CUMMINS, AND FUNCTIONAL ANALYSIS*

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Robert Cummins has recently used the program of Clark Hull to illustrate the effects of logical positivist epistemology upon psychological theory. On Cummins's account, Hull's theory is best understood as a functional analysis, rather than a nomological subsumption. Hull's commitment to the logical positivist view of explanation is said to have blinded him to this aspect of his theory, and thus restricted its scope. We will argue that this interpretation of Hull's epistemology, though common, is mistaken. Hull's epistemological views were developed independently of, and in considerable contrast to, the principles of logical positivism.

Robert Cummins (1983a) presents an account of functional analysis and its central role in psychological explanation. (Except where noted, all references to Cummins are to 1983a.) In this account, psychological phenomena are explained not as state transitions subsumed under laws, but as manifestations of capacities that are functionally analyzed. Cummins argues that psychological explanations make little sense construed as subsumptions, notwithstanding the great influence that the subsumptionist "Received Doctrine" has had in psychology. To illustrate this incongruence between theory and epistemology, Cummins chooses the case of Clark Hull. Cummins gives a functional/analytic interpretation of the theory expressed in Hull's *Principles of Behavior* (1943a), argues that this interpretation was unavailable under Hull's epistemological commitments, and suggests that this theory/epistemology mismatch accounts for the failure of Hullian theory to address certain important facts about behavioral capacities.

The epistemology Cummins attributes to Hull is, of course, the logical positivist "Received Doctrine." This attribution occurs so regularly in commentaries on Hull that it deserves to be called the "Received Doctrine

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of Hullian Epistemology" (Wolman 1960, p. 106; Taylor 1967, p. 518; Koch 1954, pp. 25, 41; Leahey 1980, p. 356). Nonetheless, the attribution is mistaken. We will attempt to show in this paper that the historical Hull did not endorse the epistemological views assigned to him by Cummins (among others) and that he made good use of the techniques described by Cummins as "functional analysis." This will imply that certain of Cummins's criticisms of the Hullian program are misdirected. We do not intend to challenge Cummins's insightful account of psychological explanation. Indeed, we will conclude by noting an endorsement of Cummins's account from a most unexpected source.

1. The Status of Intervening Variables. Cummins attributes to Hull the view that intervening variables (psychology's theoretical terms) are in principle eliminable from scientific discourse. In support of this attribution, Cummins mentions only Hull's claim that such variables must be "anchored at both ends" (Cummins, p. 103). While he does not cite the source of this statement, similar uses of the "anchoring" metaphor occur in Hull's *Principles* (the only work cited by Cummins) on pages 29 and 382. In the latter passage, Hull describes intervening variables as "represent[ing] presumptive intervening processes not directly subject to observation." In the earlier passage we find the following account of intervening variables:

> These symbols or X's represent entities or processes which, if existent, would account for certain events in the observable molar world. Examples of such postulated entities in the field of the physical sciences are electrons, protons, positrons, etc. A closely parallel concept in the field of behavior familiar to everyone is that of habit as distinguished from habitual behavior. The habit presumably exists as an invisible condition of the nervous system quite as much when it is not mediating action as when habitual action is occurring. . . . (Hull 1943a, p. 29)

Elsewhere Hull includes molecules and atoms in his list of physical "intervening variables" (1943b, p. 277) consistent with his interest in hierarchies discussed below. In the face of Hull's talk of postulated physical structures, the use of the "anchoring" metaphor is slim grounds on which to attribute to Hull the eliminability of theoretical terms.

Concerning the (purported) eliminability of theoretical terms, Cummins asks himself (and, rhetorically, Hull) "So why have them?" (p. 104). The eliminativist answer comes not from Hull, but from Neal Miller (1959). The answer is that, even though intervening variables do not refer to real (though unobserved) states of the organism, they do make for theoretical economy—they simplify calculation. Cummins gives this argument the brief consideration it deserves, apparently assuming that Miller's genuinely eliminativist move would be Hull's, if Hull had thought to address the question. Cummins goes on to consider the virtues of rejecting Hull's (read "Miller's") eliminativism in favor of a functional view of Hull's theoretical apparatus. Now, it must be admitted that Hull does not address the issue of eliminability in the *Principles*. But he does elsewhere, as for example in a paper published the same year. In that paper Hull responds to the "rock-ribbed positivist" who would ask Hull: "If you have secure equational linkage extending from the antecedent observable conditions through to the consequent observable conditions, why . . . use several equations [the ones linking the intervening variables] when one would do?" (1943b, p. 284). Hull admits that his structure of interrelated constructs could, in mathematical principle, be expressed as one complex functional relation between dependent and independent variables, but opposes this move for two reasons. First, the equation eliminating reference to intervening variables would be nothing other than the mathematical expression "telescoping" Hull's internal functions into one expression. Second, various of the independent variables differ in their temporal, and thus their causal, relations with the dependent variables. "While it is perfectly possible to put into a single equation the values of events which occur at very different times, it is hard to believe that an event such as a stimulation in a remote learning situation can be causally active long after it has ceased to act on the receptors" (p. 285). Intervening variables exist not merely to facilitate prediction, but to describe the structure of the causal dependencies which exist within the organism. If these dependencies are described rightly by Hullian theory, then the "telescoping" of them all into one grand equation would (1) gain nothing, as the internal dependencies would determine the form of the grand equation, and (2) misdescribe (or at least fail correctly to describe) the causal dependencies which actually exist within the organism. Hull's intervening variables were intended not simply to make the theorizer's life easier, but to represent accurately the causal hierarchy within the organism which mediates its behavior. He did not endorse the eliminability of such terms.

The first extended collaboration between a behaviorist and a logical positivist was between Kenneth Spence (a student of Hull) and Gustav Bergmann, at the University of Iowa beginning about 1940. This, together with Spence's growing influence in psychology, led to a great stiffening of the empiricism of behaviorism. Under the influence of a more genuinely eliminativist epistemology, Spence and his coworkers were embarrassed by Hull's speculations regarding physiological correlates for his intervening variables (Spence 1950, p. 10). The Hullian concept Spence found most fruitful was the "mediating goal response" (or 'r_g'). Hull had conceived of this as an actual physiologically occurring partial response to a stimulus, which then served as an internal stimulus for the classical conditioning of further observed or unobserved responses. After Spence, neobehaviorists were forced to concede that even this internal conditioned response "need not really occur at all" in order for the explanations in which it was used to serve their scientific purpose (Goldstein et al. 1965, p. 3). In fact, draft versions of the *Principles* seem to have been full of physiological interpretations of Hull's intervening variables. Howard Kendler reports that Spence, during the writing of Hull's *Principles,* assumed "the combined role of a critic and contributor with devoted unselfishness . . . eliminating unnecessary material, such as the physiologizing, which tended to confuse the theoretical issue" (Kendler and Spence 1971, p. 5). Hull's interest in identifying physiological correlates of his intervening variables was recognized by Spence (but not by Cummins) as inconsistent with the eliminability of theoretical terms. Further evidence of Hull's physiological realism can be seen in the following passage, written just prior to Spence's "unselfish" critiques:

> The concept *stimulus trace* has substantially the status of a symbolic or logical construct. While there are physiological indications that the expression represents an entity which may ultimately be observable in some indirect manner, for the present purposes it may be regarded as an unobservable. The existence of this hypothetical entity is explicitly assumed by Postulate 1. (Hull et al. 1940, p. 23)

2. *Functional Explanation and Deductivism.* In contrast to Cummins's marginal documentation of Hull's eliminativism, Hull's endorsement of deductive-nomological (D-N) explanation is clearly expressed, with Hull parroting the standard positivist line: "A natural event is explained when it can be derived as a theorem by a process of reasoning from (1) a knowledge of the relevant natural conditions antedating it, and (2) one or more relevant principles called postulates" (1943a, p. 14). But does this endorsement warrant Cummins's conclusion that Hull's commitment to D-N explanation blinded him to the possibility and utility of functional analysis? A detailed examination of the historical record (Smith forthcoming, chap. 6–8) indicates that the answer is no. Hull's early work on learning theory was quite similar to the kind of functional analysis practiced by modern cognitive psychology, at least regarding its conformity to Cummins's account of explanatory strategies. Only after logical positivism was imported into the United States during the mid-to-late 1930s did Hull graft logical positivist language onto his already conceived view of functional explanation. A brief review of Hull's intellectual development will clarify this point, and illustrate the nature of his 'deductivist' intuitions.

From the outset of his career, Hull was interested in machines and machine simulations of intellectual abilities. As an undergraduate engineering major, he built a logic machine as a project for a course under Roy Wood Sellars (see Hull 1962, p. 151; Gardner 1958, p. 124). Later during the 1920s, he designed and built the first machine for calculating correlation coefficients (Hull 1952, p. 151). His success with these devices convinced him that organismic capacities for intelligent behavior, including the highest mental capacities, could be understood in terms of suitably arranged mechanical functions. As early as 1926 Hull formulated the rationale for automated simulations of psychological processes:

> The human organism is one of the most extraordinary machines—and yet a machine . . . [S]o far as the thinking processes go, a machine could be built which would do every essential thing that the body does . . . [T]o think through the essentials of such a mechanism would probably be the best way of analyzing out the essential requirements of thinking, responding to abstract relations among things, and so on. (Hull 1962, p. 828)

Significantly, Hull had by this time formulated the notion that mechanical simulations of psychological functioning would have to employ some sort of hierarchical control system if they were to exhibit the flexibility and variety characteristic of intelligent functioning (Hull 1962, p. 821). In effect, Hull had given an early and clear expression of the rationale for cybernetics, thereby anticipating the founding of that field by more than a decade (as is occasionally recognized—see Gunderson 1967, p. 281).

Under the influence of Pavlov's work, Hull came to regard the conditioned reflex as basic to learning, and to the analysis of learning. From 1930 to 1935 he produced a series of theoretical papers interpreting such phenomena as higher-order conditioning, purposive behavior, and insight learning in terms of classical conditioning (Hull 1930a, 1930b, 1931, 1934, 1935b). These papers addressed just such questions as "Why are organisms subjects to the Law of Effect?" which heads Cummins's list of "striking behavioral capacities" unaddressed by Hullian theory. Hull introduced the "pure stimulus act" (similar to the later "mediating goal response") in these papers. This "act" was an internal classically conditioned response which could be interoceptively perceived, and thus serve as an internal stimulus for subsequent internal or overt responses. By these means, Hull attempted to analyze higher forms of learning into the structured development of classically conditioned internal events. In Cummins's apt terms (p. 100), these papers provided *analyses of* complex learning *capacities into subcapacities* (the production and classical conditioning of sets of pure stimulus acts) *such that the exercise of* complex learning *is reduced to the programmed exercise of* internal classically conditioned reflexes. Thus Hull's later "physiologizing" was not merely an interest in physiological events which happened to be functionally characterizable (which in itself would make him a sort of functionalist). Rather, Hull was interested in the instantiation (see Cummins, p. 110) of the specific subcapacities into which he had *already* analyzed complex forms of learning. This is the kind of functionalism proposed by Cummins. As Cummins points out, a functional analysis has force to the extent that the analyzing capacities are simpler than, and different in kind from, the analyzed capacities. Classical conditioning has often been regarded to differ in just these ways from complex learning.

The 1930–1935 papers emphasized the importance of machine simulation as a constraint on proposed functional analyses. Each paper contained some statement to the effect that the theoretical derivation in question could be realized in the form of an intelligent mechanism. For example: "if the type of explanation put forward above be really a sound deduction, it would be a matter of no great difficulty to construct parallel inanimate mechanisms . . . which will genuinely manifest the qualities of intelligence, insight, and purpose . . ." (Hull 1930b, p. 256). What is not often realized—and is even denied by Cummins (p. 108)—is that Hull and his coworkers actually constructed several such machines (Baernstein and Hull 1931; Ellson 1935; Hull and Baernstein 1929; Hull and Krueger 1931), the first published account of one appearing in *Science* in 1929.

In sum, Hull cannot be said to have been blinded by his commitment to the D-N model to the fact that his own theoretical system represented a kind of explanation by functional analysis. Quite the contrary, he was an early and avid practitioner of analytic explanatory strategies (and not without some historical consequences—one of the figures who participated in his Yale seminars was the young Warren McCulloch). If readers of Hull's mature theoretical systems have been largely oblivious to this crucial aspect of his thinking, it is not without reason. By 1940 Hull was publicly downplaying the role of machine design in his thinking. One important reason was that, by this time, American psychologists were rapidly coming to accept logical positivism as a guiding conception of science (Smith 1981), and Hull bowed to this trend by adopting the jargon of the movement. Logical positivism made little room for analogy and metaphor in science, and Hull's robot-metaphor, which had occupied an entire chapter in early drafts of the *Principles* (see Koch 1954, p. 16), was given only cursory treatment in the published version (Hull 1943a, pp. 27–28).

3. *Metaphysics*. Hull's adoption of logical positivist terminology and his sympathy with the philosophical movement were to some extent matters of expedience. But there were also intellectual reasons for his receptivity to the ideas. Hull had practiced deductive methods of theorizing in the

early 1930s, prior to his contact with logical positivism. His "Idea Books" (his intellectual diary, interpreted at greater length in Smith forthcoming, chap. 8) reveals the source of Hull's deductivism. First, Hull was a determinist. Second, he viewed organisms as machines, and machines as hierarchical systems. Finally, he viewed theories as symbolic habit sequences that operate in parallel with those systems in the world that they describe. From these ideas, Hull concluded that theories of organismic functioning must parallel the hierarchical organization of the behavior that they aim to explain. Since a hierarchical structure was seen as a desideratum for theories of behavior, Hull turned to the deductive systems he knew—geometry and logic—for systematizing his theories (Hull 1935a; 1937; Hull et al. 1940). Had other hierarchical modes of expression been available to him—such as modern programming languages with their subroutines and iterative loops—he no doubt would have found them more suitable for his enterprise.

Hull's approach will strike modern ears as odd, and not particularly compelling. Habit structures seem less susceptible to symbolic or representational analysis than do inferential systems, the preferred model of modern cognitivists. But the above sketch of Hull's metaphysical views does account for his commitment to the epistemological views we have documented. His functionalism follows from his belief in hierarchical structure, together with his representational realism. His denial of the eliminability of theoretical terms follows from his mechanism and realism. His deductive strategies of explanation result from the above, together with his mechanistic determinism. It is important to note that Hull's deductivism resulted from his view of the internal structure of the organism—the organism was subject to causal determinism. His motive was not a positivist *Angst* regarding the ontological status of unobservables. For Hull, the deducibility of dependent from independent variables by way of the theory gave a confirmation of a postulated real internal structure, analyzed functionally, which (it was hoped) would receive further confirmation from physiological research. In any case, deducibility was not intended to provide eliminative semantic translations of the terms used to describe internal structure.

4. Philosophy and the History of Psychology. The key to the reinterpretation of Hull that we are advancing is the recognition of his metaphysical views regarding machines, organisms, and the relation between scientific theories and the world. Cummins was right about Hull's deductivism, but not about the sources of Hull's commitment to the deductivist strategy. Cummins is mistaken about Hull's antipathy to functional analysis, although Hull's (at least verbal) endorsement of positivist views of explanation in the *Principles* might explain this misunderstanding. Cummins is also mistaken in attributing an eliminativist view on intervening variables to Hull, and here a reasonable source of misunderstanding is harder to find. The vaguely positivist-sounding references to "anchoring" are far from adequate to justify this important attribution. Hull's fellow neobehaviorist E. C. Tolman made much more insistent use of operationalist/positivist jargon than did Hull; like Hull, Tolman is now commonly used to illustrate the (regrettable) influences of positivism on behaviorist psychology. However, a careful reading of Tolman shows that the epistemology he *expressed* with the use of positivist jargon was quite inconsistent with that intended by those who introduced the jargon (see Amundson 1983). Like Tolman, Hull's fashionable use of positivist terminology has served to obscure his actual epistemology and suppress his more original contributions. For neither psychologist was positivism or operationalism a significant source of methodology.

One is tempted to say that it was bad luck for Cummins to have chosen Hull's *Principles* on which to base his explication of Hullian epistemology. The robot metaphor and the physiological interpretation of intervening variables were crucial to Hull's concept of psychology; both were largely removed from the *Principles* under positivist influence. But in this case Cummins made his own bad luck. Historically reputable accounts of a scientist's epistemology are not to be based on a few comments in a few major publications (let alone only one such publication). A bare minimum is a broad reading of published materials; a deeper understanding (as is sketched in the preceding section) requires archival work as well.

Contemporary philosophy of the natural sciences has shown an increasing interest in the history of science. Biased or "Whiggish" historical accounts, especially of a scientist's methodological views, can lead to biased philosophical accounts of science. Contemporary philosophers of psychology do not seem to share this historical interest, especially with regard to behaviorism. Many philosophers seem to have a stereotype of the All-Purpose Behaviorist, a construction formed out of bits of Skinner, Spence, and perhaps Ryle (and then attributed to Hull, Tolman, etc.). The All-Purpose Behaviorist is, of course, a logical positivist or operationalist, and this epistemology is most of what is wrong with behaviorist theory. Some features of Cummins's discussion of Hull suggest that Cummins has just this casual attitude towards the epistemology of behaviorists. His slapdash documentation of Hullian epistemology leads one to think that Hull's D-N positivism was a foregone conclusion. The treatment of the "striking behavioral capacities" (pp. 106–7) is also puzzling. The list of striking capacities begins with "Why are organisms subject to the Law of Effect?", the sort of question for which Hull had a fully functionalist answer, as shown above. The list ends with the following anachronistic pair: "Why does the complexity of an English description of a color predict the memorability of colors for non-English speakers with no color vocabulary?"; "Why is it easier to learn concepts of the form (A & B) than of the equivalent form $-(-A \vee -B)$?" The former of this pair refers to a *discovery* of the 1970s (Heider 1972), and modern psychology's interest in the latter cannot be much older. While it is worthwhile to study the impact of epistemology on scientific debate (for example, on the "place versus response" debates of 1945–55 [Amundson forthcoming]), it is a hazardous enterprise to blame epistemology for the failure of a 1943 theory to approximate the interests of today's psychologists, *even* when the 1943 epistemology is adequately understood. This foreshortened historical perspective (which is by no means Cummins's alone) may explain Cummins's recent assertion that cognitive psychology is "now less than thirty years old" (1983b, p. 52). While the fact is not widely acknowledged, cognitive psychology's history predates 1950, and even includes some (shudder) behaviorists (Leahey 1980, pp. 374–76; Leahey 1981).

In other ways, Cummins shows a quite progressive attitude towards understanding behaviorist epistemology. He does not blame epistemology for substantive features of Hullian theory, but only for its narrowness of scope. In fact the failure of Hullian explanation is blamed on other sources altogether (p. 110). There is much to be said for Cummins's interpretation of substantive Hullian theory, and for his account of the nature of explanations of capacities. Indeed, Cummins's misunderstanding of Hullian epistemology has a serendipitous effect on the validation of his broader point concerning the importance of functional analysis in psychology. Cummins's point receives *independent* endorsement from a surprising source: Clark Hull himself.

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