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Requisition for a pexgo.

Bindra's points about the flaws in his predecessors are well taken - and could be taken farther - but his own view, for all its apparent novelty, has something naggingly familiar about it. What is it exactly that makes the proposed theory look so old-fashioned (to me)? I think the answer can be seen if we reflect on a trend discernible in the history of behaviorism. As Bindra points out, such phenomena as latent learning and response substitution soon showed behaviorists that the laws and mechanisms proposed were insufficient; animals (even rats) were too smart to be explicable by the simple mechanistic models of reinforcement and conditioning then available. Skinner's response, as Bindra notes, was to retreat from models of conditioning mechanisms and settle for an analysis of "functional relations," and, as Bindra observes, this evaded "the fundamental problem of how adaptive behavior is put together." For those who did not follow Skinner's lead, but continued to grapple with the "fundamental problem," the result was an inexorable if reluctant migration of behavioristic theory toward more "cognitive" characterizations of the theoretical entities posited. (See Broadbent, 1961; Taylor, 1964 and BBS 1:2; and Dennett, 1975, for overviews of this trend from different perspectives.) The rationale for this trend can be seen more clearly in retrospect than it could be at the time, especially now that cognitive or intentional (Dennett, 1971; Haugeland, this volume, next issue, forthcoming) formulations are no longer viewed with horror. It was usually easy enough for the behaviorist to say (in the privacy of his own room) what the shortcomings of his particular theoretical entities were: they did not vary, but remained constant (as they ought to have) in rough concomitance with what one would call the perceivable meaning of the situation, if one could only permit oneself such locutions. One had hoped for laws of nature, on the model of the laws of chemical combination for instance, but when the laws that suggested themselves turned out not to handle the data, a subtle and relatively unrecognized (one might even say repressed) shift in tactic occurred: the search for laws of nature relating presumably simple theoretical entities (e.g., stimuli and responses) was displaced by what in retrospect can be seen to have been an effort to design theoretical entities of some complexity with the requisite powers to accommodate the data. It was a shift, in Haugeland's terms, (this volume, next issue, forthcoming) from a derivational-nomological enterprise to a systematic enterprise, but without abandoning the trappings and preconceptions of the former style of science. Thus a premium was still placed on framing laws to relate the theoretical entities and on equations to characterize their interaction, even though what was being done was essentially system-designing at ever more cognitive, less mechanistic levels of abstraction. As Bindra says: "The general thrust of such concepts (e.g., cognition, expectancy emotion, etc.) is that the motor output comprising a response is separated from current sensory inflow by the intervention of some central system of knowledge or motivation, and it is this system, not sensory-motor associations, that determines what the response will be. It is assumed that such a central system somehow flexibly adjusts the motor output to the ever-changing stimulus and organism conditions." Somehow, but how? The question invites speculation about systems: "what if we tried to do it with some parts with functions A.B,C, ?" What Bindra offers us is a fairly advanced set of design proposals in this tradition, but still couched in vestigial "laws of nature" talk. What this tends to conceal, from theorist and onlooker alike, is that the design proposals are not full-fledged structural blueprints (the end-product of a solved design problem), but an elaborate collection of functional specifications of sub-system parts, specifications that set the problem for the designer, rather than solve it. For instance, Bindra says, "According to the incentive-motivation hypothesis…a pexgo of a particular stimulus (say, S_2) generates a pexgo of a hedonic stimulus (S^H) and thereby generates the same motivation (central motive state or cms) as is normally generated by the hedonic stimulus (S^H) itself; this central motive state, in combination with the detailed sensory-spatial features of the situation, then determines what response will emerge." This is presented as a hypothesis (a sketch of a law to be tested), when in fact it can only be viewed as a sketch of a design proposal the feasibility of which is utterly uncertain because of the semi-disguised intentionalistic characterization of the sub-system parts and functions.

The disguises come off when one asks what the equivalence classes are and what the individuating characteristics of the entity-types are. What makes a pexgo a pexgo of a stimulus S_2 (i.e., a stimulus of type S_2- -a solution to the problem of stimulus generalization or pattern recognition is presupposed from the outset)? Not merely that the pexgo was simply caused or triggered by an S_2 type stimulus, for pexgos are representations of stimuli or stimulus situations. (Direction to the engineer: make me some pexgos to represent each important type of stimulus, please.) Next, this pexgo "generates" a pexgo of the hedonic stimulus (S^H). (Note to engineer: do not forget to build in a conditionable hedonic-stimulus-pexgo generator as part of each pexgo.) And this thereby ("thereby" because that is the way it ought to work) "generates the same motivation (central motive state or cms) as is normally generated by the hedonic stimulus (S^H) itself " What counts as same central motive state? Our equivalence class here is undisguisedly intentional: a particular cms is a token of a particular cms-type if and only if that token has the function of determining "in combination with the detailed sensory-spatial features of the situation" a response appropriate to that particular cms type. (See Dennett, 1971, on intentionality and appeals to appropriateness.)

Is it possible to design parts with these features and powers and then get them to work together in the ways proposed? Who knows? One might get some hint of the answer by turning Bindra's proposal over to the Artificial Intelligence (A.I.) types (see Pylyshyn et al, this issue), whose stock in trade is taking such sketchy design proposals and trying to come up with ways of implementing them. But I suspect we will not have to wait long for an answer. The sorts of suggestions Bindra makes seem to me to propose structures much less sophisticated than structures already known by the A.I community not to be powerful enough to model very intelligent behavior. I do not expect that they will believe Bindra when he claims that "'cognitions' postulated to account for complex forms of perception, meaning, recall, and problem solving are ultimately constellations of overlapping and nested contingency organizations, each developed by the observation of stimulus-stimulus relations in a simple learning experiment."

There is an abundance of reasons for viewing askance the freewheeling system-designing of the A.I. school, but one makes a mistake if one contrasts that enterprise with the apparently "more scientific," apparently law-seeking enterprise instanced here by Bindra, for the two are really the same: both schools are trying to design systems powerful enough to explain the data, but the latter school suffers from the great disadvantage of thinking it is doing something else.

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