*THE SECOND OFFSPRING OF GENERAL PROCESS
LEARNING THEORY: OVERT BEHAVIOR AS THE
AMBASSADOR OF THE MIND*

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Wasserman suggested in a recent book review that the study of intervening cognitive processes represents a current focus of interest in animal learning and that this has led to a revitalization of comparative psychology. An examination of the volume reviewed suggests that he may have overstated the case. Most of the authors to whom he refers expressed dissatisfaction with traditional stimulus-response associationism but few argued for the extreme (information processing) sort of cognitive approach described by Wasserman.

Key words: general process learning theory, associationism, biological boundaries, cognitive processes, radical behaviorism, comparative psychology

As is well known to most readers, a recent rebellion continues, carried on by those who emphasize the importance of species-specific influences on animal learning. Their target has been "General Process Learning Theory," a view derived from the distillations of Pavlov, Skinner, and others by textbook writers and several derivative theorists. According to the typical General Process view, learning consists of the formation of S-R (or other) associations and all stimuli, responses, reinforcers, and organisms are pretty much alike. But, as this "Biological Boundaries" protest group emphasizes exceptions to the "laws" of learning, its members and the General Process defenders seem more and more to be cut from the same cloth. The Biological Boundaries group also tends to favor an S-R associationism and to have greater respect than seems justified for the traditional textbook conditioning paradigms, as has been pointed out by Malone (1973, 1975) and, at length, by Schwartz (1974).

General Process Learning Theory has spawned once again, this time simultaneously in behavior therapy (e.g., Bandura, 1977) and in animal conditioning and learning (e.g., Wasserman, 1981). Where the first born saw the emphasis on biological endowment as the alternative to its parent, the second turns to cognitive models. As was true of the first child,

the latest arrival criticizes the influences of "radical behaviorism" (read as "extreme" behaviorism, not as Skinner's usage), which has exerted a stifling influence. It has prevented the use of intervening variables, robbed us of the richness of cognitive processes, militated against the use of computer simulations of animal cognition, and blinded us to the recognition that overt behavior is merely the ambassador of the mind.

The case for cognitive models in animal behavior was recently made by Wasserman (1981) in what was in part a review of Hulse, Fowler, and Honig's *Cognitive Processes in Animal Behavior* (1978). One easily gets the impression from Wasserman's review that this volume ushers cognitive science into animal learning; this means information processing mechanisms, computer simulations, mental chronometry, and all of the "richness and diversity" that cognitive processes bring to any subject matter. Having earlier seen the book in question, I rushed to it for further study and concluded that Wasserman may have overstated the case a bit. I hope that this conclusion was correct.

Many contributors to the book did express a preference for "more flexible interpretations" (as Hearst put it in his chapter) than typically have appeared (presumably, in the leading journals). That is, complaints were frequently made against the perceived forced adherence to a rigid S-R associationism. Other types of associationism (S-S or R-S) were advocated by several authors and this advocacy had

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been made known years ago. But a search for rich and diverse cognitive mechanisms, for the "mind and its ambassadors," turned up very little. After all, criticism of S-R associative models need not make one a cognitivist and, since the *Journal of the Experimental Analysis of Behavior* has never dealt with rigid S-R associationism, critics of such a view should evoke little interest.

What makes one a cognitivist? Surely a defining attribute is a concern with "underlying processes," the neural or hypothetical mechanisms of which overt behavior is presumed to be a manifestation. Pavlov was a cognitivist in this sense, as is anyone concerned with the problem of "what is learned." However, it is fair to say that today cognitive psychology is nearly synonymous with a very specific interpretation of underlying processes, based on the information processing/computer analogy approach. Wasserman accepts such a definition of cognitive psychology and goes so far as to suggest that there now exists a "comparative psychology," involving human and computer! Is the information processing approach really gaining great favor in animal learning or are many workers simply reacting to the strictures of the past by rejecting a particular associationistic behaviorism and emphasizing underlying-processes-in-general as an apparent alternative? Such a shift in emphasis may well lead one to call oneself a cognitivist but that in itself does not mean an endorsement of the extreme information processing approach that Wasserman emphasizes.

Looking at the individual contributions to the volume, we find Rescorla proposing a more flexible definition of classical conditioning than has often appeared in textbooks and showing an appreciation for why Pavlov frequently described a CS as a "signal," but his chapter shows no sign of a new penchant for information processing mechanisms. Bolles, Mackintosh, and Honig still favor associationism, but Bolles stresses R-S relations, Mackintosh stresses selective attention (as do Riley and Roitblat), and Honig proposes three kinds of memory in pigeons. With the exception of Honig, there is no real cognitive emphasis of the sort Wasserman describes. Hulse and Fowler seem similarly noncognitive, as do the chapters by Menzel and by Premack. Menzel simply argues that chimpanzees learn relationships (though he does want them stored in the

brain, which may make his view minimally cognitive), and Premack shows that chimps more readily learn relational concepts than do pigeons. Both Wagner and Olton do propose information processing models to deal with short-term memory for CSs and spatial memory, respectively, but this information processing emphasis makes their chapters exceptional.

Aside from Wagner's and Olton's models and Honig's variety of pigeon memories, the "animal cognition" revolution seems quite tame. I concede that each of the authors may well consider himself a true cognitive psychologist, but there seems by no means a consensus that overt behavior is the "ambassador of the mind"; there is no mental chronometry, no simulation of cognitive processes, and little reference to information processing mechanisms. It is true that some have gone so far as to time the reaction of a pigeon to a rotated "R" in a matching-to-sample task, but such work is nowhere in evidence in this volume.

What is in the volume is an often expressed dissatisfaction with what the authors perceive as the current and recent state of affairs in the conditioning and learning establishment. A number of the contributors have themselves long been salient members of that establishment and have aided in perpetuating that state of affairs. This state of affairs ("radical behaviorism," as Wasserman calls it) is really radical S-R associationism, and the cognitive animal movement seems actually a fifth column rebellion against it. As most readers know, S-R associationism has never characterized Skinner's views, nor those of the *Journal of the Experimental Analysis of Behavior*. Anyone may discover Skinner's position on radical behaviorism in Skinner (1964); it will be clear that this position has nothing in common with rigid S-R associationism and that, far from excluding the study of cognition, its study could be of great service to those who really wish to understand cognitive processes.

This is the heart of the issue. Wasserman stresses the role of comparative psychology in determining to what extent animals share the cognitive capacities of humans, a pursuit popular since the pre-Darwinian nineteenth century. The issue is not whether animals do share such capacities but in what way behavior, whether overt or covert (thinking, seeing, hoping, etc.), may be most profitably understood. Cognitive phenomena may be viewed

as activities, not necessarily precedent to nor correlated with overt activities; they are thus in need of explanation as much as are overt activities. Alternately, cognitive activity may be viewed as different in kind, as the "secrets behind the phenomena," with overt behavior left as Romanes' "ambassador of the mind." This has long been the more popular way of construing things and its current representative, the information processing view, is undeniably the most popular approach in experimental psychology at this time (e.g., Hearst, 1979). But this Journal has long espoused the alternative view which surely includes the study of cognitive processes, but which insists that accounts not be cast in terms of intervening variables and hypothetical constructs, whether cognitive or not.

It is difficult to see the virtues of adopting intervening cognitive variables and in following Romanes, Morgan, and Herbert Simon in regarding overt behavior as the "ambassador of the mind." Doing so assumes underlying mechanisms of which behavior is a manifestation; for Wasserman and for some of the authors in the reviewed volume, this amounts to stressing the distinction between knowledge and performance. But "knowledge" is still inferred from the observation that experience affects behavior in ways that may not be immediately demonstrated in overt behavior. If the change in behavior is never manifested, then it is difficult to see why it is of any interest. If the change in behavior appears only under certain conditions, then the specification of those conditions seems enough; the inclusion of intervening cognitive processes, however "varied and rich," is gratuitous. Obviously, the simplest natural event ultimately depends upon extremely complicated "underlying mechanisms," but surely we have learned that the identification of order in the world does not require the specification of all of them.

I was puzzled by Wasserman's emphasis of the virtues of "objective inference" and the impropriety of "subjective inference," a point that was also made by Hulse in the reviewed volume (the usage here is, of course, unlike Romanes' distinction between objective and subjective methods). Objective inference refers to the positing of cognitive processes (I suppose that one could say their "discovery") through the observation of behavior. Success-

ful delayed-matching-to-sample thus allows us to posit memory mechanisms and the catching of a ball implies an internal trajectory-calculating mechanism. On the other hand, subjective inference, as Wasserman presents it, refers to the introspection by analogy of nineteenth-century psychology, whereby inferences of subjective experience are based upon observations of behavior. The implication is that subjective inference is generally improper, whether applied to human or to animal.

However, it is objective and not subjective inference that may be objectionable. Private experience is certainly of interest to us, and there is no reason to restrict the use of what Wasserman calls subjective inference. We daily infer subjective experience in others based on our observations of their behavior (Romanes' objective method), just as we infer much of what we call our own private experience from our observations of our own overt behavior (as in, I was hungrier than I thought). Part of the behavior we observe in others consists of their verbalizations which, if properly construed, are certainly useful. Thorndike, Watson, Guthrie, and Skinner all included private behavior as part of the phenomena with which our accounts must deal. If the reader has any doubts regarding Watson's views on this matter, see his explicit endorsement of subjective inference (Watson, 1924, p. 229). I trust that no references are required for the other three. It seems that only Hull and his associates, the epitome of rigid S-R associationism, would agree with Wasserman's conclusion (but see also Tolman!). The other theorists simply (and rightly) insisted only that private behavior not be treated as different in kind from that behavior which is more readily observable.

One may easily train a pigeon to respond in the presence of a faint red key and to not respond when the key is white or green. The key is then illuminated by a deeply saturated green, which abruptly changes to white. The pigeon pecks the white key and as its response rate falls with time since the offset of green, it seems reasonable to assume that the bird is responding to a (private) stimulus, the negative chromatic afterimage. In the case of humans we can also ask for verbal corroboration of such inferences, and it seems odd not to feel justified in doing so. Needless to say, care must be taken in the making of such inferences; private events are less accessible, both

to the observer and to the subject. But if private experiencing is a part of the behavior to be dealt with by a science of behavior, then subjective inference is essential. Indeed, the acceptance of subjective inference merely means that subjective experience is a legitimate object of study.

Objective inference, on the other hand, leads to speculation about the mechanisms and powers that logic tells us must be necessary to allow our subjects to behave as they do, whether this behavior be public or private-made-public. If past experience is any guide, such inferences seem inevitably to lead to simplistic mechanical analogies, such as the digital computer. But, as was pointed out above, such is rarely the case in Hulse, Fowler, and Honig.

There seems a perennial debate in psychology concerning the relative merits of research based upon logic or upon observation. One may choose a phenomenon for study, whether it be the learning of language by children, the forming of discriminations by pigeons, or the way in which dogs catch balls. One may observe behavior as conditions are restricted and manipulated or under unrestricted conditions and seek out the orderly relations between the world and behavior. Skinner (1931, 1935) clearly specified how we may know when we have discovered such orderly relations, and this strategy is by no means expressible only in the language of operants and reinforcers. On the other hand, one may begin with a logical analysis of what must be necessary and, after a cursory examination of behavior, posit a language-acquisition device, templates and buffer storage, and trajectory calculators. The latter strategy has always had greater appeal and it does make it easier to describe our research to other scientists and to the lay public. But I trust that the reader needs no argument against the latter and for the former approach; if an especially clear argument is needed, see Skinner (1953 or 1964).

In fairness to most of the authors in *Cognitive Processes in Animal Behavior*, whatever cognitive mechanisms are proposed were the result of data that seemed to them to force the inclusion of cognitive factors. I suspect that most would not view their research as attempts to "divulge cognitive processes." But most of them, as Hearst admits, did begin with a fairly narrow and rigid (S-R) behavioristic view and have come recently to seek some-

thing more flexible. The "something" need not be something as extreme as the information processing view of cognition, as Wasserman describes it.

If all that Wasserman advocated were the value of intervening variables, it would be easier to sympathize with him. My mentor, J. E. R. Staddon, once described me as a cognitive operant conditioner, since I favored Pavlov's (cognitive) model and because I attempted to include the intervening variable "value" in a 1971 manuscript submitted to the *Journal of the Experimental Analysis of Behavior*. I pointed out the virtues of such an intervening variable (essentially as did Church in Hulse, Fowler, and Honig) and added that other intervening variables, such as "aversiveness" and "response strength," had proven useful. This view was not shared by the reviewers, one of whom wrote that the use of those two intervening variables had set back the analysis of behavior 20 years. "Value" did not appear in that paper.

What seemed to me to be innocuous intervening variables such as value, response strength, and the like seemed a far cry from the sensory copies, surrogate behavior, and mental machinations referred to by "representations," "memories," "rehearsals," and other intervening cognitive variables which Wasserman favors. But if value were acceptable, why are encoding and template matching not acceptable? Why not go a step further and suggest that value is determined by cognitive counters that increment and decrement with the delivery of food and shock? The reviewer who seemed so stodgy ten years ago was correct; despite the appeal of intervening variables, their use tends to encourage the abandoning of the study of behavior (both overt and private) as the datum to be explained and leaves it as the "ambassador of the mind." Where the reviewer really was stodgy (and mistaken) was in his belief that the analysis of behavior is possible only in terms of operants and reinforcers.

Though Wasserman suggested that our knowledge is so meager that we cannot foreclose any approach, he was at least partly mistaken; must we follow the lead of the workers in human memory who traded an older for a newer associationistic approach, couched in information processing terms? The computer analogy has not done so well in fos-

tering the understanding of human cognition that the analysis of behavior should be abandoned. Our knowledge may be lacking, but is it really so very limited that we cannot foreclose any approach?

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