*A REVIEW OF M. J. MAHONEY'S Cognition and Behavior Modification*¹HOWARD RACHLIN²

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This is a book on application of current cognitive psychology to behavior modification. It is of interest to the readers of the *Journal of Applied Behavior Analysis* because the author, Mahoney, explicitly rejects a wholly behavioristic psychology in favor of the "cognitive approach". At various points Mahoney rejects the philosophical, the scientific, and the clinical aspects of behaviorism. Mahoney's arguments are worth noting because he claims to be riding the first swell of a new wave of psychology. The book contains a large amount of Mahoney's own intellectual history, which perhaps symbolizes the direction in which he sees psychology developing. Mahoney was an undergraduate at Arizona State University in the days when the faculty was dominated by operant conditioners. There he learned how to experiment with animal subjects and became a strong, perhaps even rigid, Skinnerian adherent. Mahoney remains loyal to Skinner personally: he speaks with pride of several conversations with him about "cognitive" topics and takes pains to quote from Skinner's works in support of psychological inference, the existence of internal events, and the right of the scientist to interpret and speculate beyond the data given.

Mahoney's Ph.D. is from Stanford University. It was there, presumably, that he learned cognitive psychology from Richard Atkinson and

Gordon Bower and its application to behavior therapy from Albert Bandura and Walter Mischel. If behavior therapy is indeed to "go cognitive", Mahoney is the man to lead the way.

The first four chapters of the book discuss the inadequacies of a nonmediational model. The rest of the book presents several alternative cognitive models and discusses several cognitive therapies. Mahoney's argument against behaviorism can be summarized as follows: (1) Behaviorism ignores private events. (2) There is evidence that private events occur and are useful in explaining behavior. (3) Therefore, behaviorism results in analyses of behavior which are at best incomplete and at worst wrong. We must learn to consider those variables that mediate between the outside world and our reactions to it. According to Mahoney: "An individual responds—not to some *real* environment—but to a *perceived* environment. The frightened airline passenger reacts not to a purely external stimulus (loud noises after take-off) but to his perception (*i.e.*, labelling) of those stimuli ('My God, we've lost an engine!')" (p. 5). The job of the experimental psychologist then is to characterize these internal mediational events, and the job of the behavior therapist is to modify them when they result in dysfunctional behavior. The book suggests several characterizations of internal events. The first is a sort of miniature external world inside the skin. The internal version of an operant is a coverant and there are internal discriminative stimuli and internal reinforcers and punishers. Thus, within a man sitting perfectly still in a dark room, an entire operant laboratory full of stimuli, responses, and reinforcements can be

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found if we only look for it. This is simply an operant version of the little r_g-s_g connections that Hull and his followers found so convenient as mediators. The difficulties with this position have been well known since 1896, when John Dewey attacked S-R theory. Since stimuli, responses, and reinforcements have been initially defined as *interactions* between a system (organism) and its environment, the postulation of wholly internal stimuli, responses and reinforcers can be at best a rough analogy to what is going on at the boundary of the system. Certainly internal events, such as nervous discharges occur. But is a given nervous discharge the stimulus to the next discharge, the response to the previous discharge, or a reinforcer for some other discharge in the nervous system? After abandoning the system (the intact organism) that gave definition to these concepts, there is no way to tell. It is, of course, conceivable that events within the nervous system follow the same dynamic laws as events outside it, but to expect such a coincidence is putting a great deal of faith in homogeneity. When we open a rock we do find little rocks, but the whole point of an organism is that its various parts are not homogeneous. Why should the parts of a person behave like little persons? It's like expecting the carburetor of an automobile to behave like a little automobile.

These objections do bother Mahoney. But for him, any mediational concepts, no matter how bad, are better than none. He says, about this theory: "Although its logical status is problematical, it stands in good company with an infinite array of other 'formally illogical' scientific premises (*e.g.*, falsifiability, induction, *etc.*). Fortunately, the contemporary scientist seldom allows logic to impede his progress." Fortunate indeed, if this is his version of logic. But the covert conditioning model refers not to the actions of scientists as falsifiability and induction do, but to the behavior of their object of study. No one expects to arrive at a given mediational model by logical means. But homogeneity, aside from its lack of logical necessity, seems also highly

improbable, given what we know of the behavior of complex devices of any kind, let alone human organisms.

Unlikely or not, is there any evidence for homogeneity? (which Mahoney calls "continuity", but this word might be too easily confused with phylogenetic continuity). Immediately, a problem appears. How does one examine internal events? Mahoney, Thoreson, and Danaher (1972) offer a clever solution. One could simply ask a person to indicate a given thought (say, the image of an elephant) and reward the person whenever the thought was indicated. But this procedure would be open to the objection that the overt indication, rather than the covert thought, was being reinforced. Mahoney *et al.* carried this process one step further. They gave subjects a series of alternate instructions on how to do a given task and then rewarded them for indicating when they thought of a given instruction. They found, not only increases in reinforced indications, but also changes in performances on the task in accordance with the instructions. The task was paired noun associations, the instructions were to form the elements into an image (I) to repeat the elements (R), or make them into a sentence (S). The indication was writing down "I", "R", "S", or "O" (for other) and the rewards were dimes. Subjects not only increased reports of rewarded thoughts but also performed correspondingly on the task. That is, they did better when "I" was reinforced than when "R" was reinforced. This experiment shows that instructions influence performance, but the function of the rewards is not clear. Were thoughts really being reinforced by the dimes or were the subjects just taught to repeat overtly a discriminative stimulus and then behave in accordance with a previously learned discrimination? A person may be taught to go left when he hears the word "black" and to go right when he hears the word "white", and then be rewarded for saying black. If the person then goes left is this equivalent to rewarding thought? One would think not. Any discriminative stimulus could equally be called a thought. Mahoney *et al.* are

already assuming what they set out to prove—that the best way to describe a complex pattern of behavior is in terms of a set of internal events.

But philosophical and experimental questions may be irrelevant to the usefulness of a concept in behavior therapy. Therapists have been ignoring such questions in developing their techniques, and rightly so. As with any application of theory, the proof of the pudding is in the eating (or in the case of weight control, not eating). How does covert conditioning do on that score? The pattern with the techniques described here (systematic desensitization, thought stopping, reinforcing and punishing thoughts, *etc.*) is one that occurs repeatedly throughout the book. The techniques are interesting, the early experiments show them to be effective, but later analyses and subsequent experiments show the early experiments to be faulty. Here is Mahoney summing up the research on thought stopping, a procedure to treat obsessives whereby the therapist shouts STOP! when the client indicates an obsessive thought is occurring. Then, clients are taught to say STOP! to themselves.

"Besides using unstandardized treatment conditions, most [thought-stopping studies] have failed to meet the methodological requirements of a controlled empirical case study. Poor operational definitions, the absence of within-subject control procedures, and lack of follow-up measurements are only a few of the methodological problems which have characterized many of the case study applications of thought stopping." (p. 84.)

Mahoney adds:

"The theoretical rationale for thought stopping as a "conditioning" procedure is not entirely clear. Few rats or pigeons have experienced an analogue version of the strategy." (p. 84.)

But if this is a criterion, and it is a good one because it forces the therapist's attention on conditioning variables, few of the techniques in this book would qualify.

After two chapters devoted to reviewing covert conditioning studies, Mahoney concludes: ". . . the empirical evidence on the effectiveness of procedures derived from the covert model tentatively suggests that its clinical utility may be problem-specific and summarily modest." (p. 22.) But if covert therapy is problem specific, in what way is it superior to the overt therapies it was supposed to replace?

Next, Mahoney takes up the information-processing model. This is the currently popular cognitive model, whereby internal events are described in computer language as being stored, retrieved, encoded, *etc.* The model is well-developed, accounts for much of what we know of human memory, and is buttressed by a host of well-designed experiments. The problem is that there do not seem to be any therapies based on it. It is conceivable that dysfunctional storage or encoding mechanisms may be responsible for dysfunctional behavior, but Mahoney cites no studies that attempt to modify these mediational mechanisms.

So much for the first two models. One is illogical and unsupported, despite many attempted therapeutic applications; the other is logical but completely unapplied. But we are only halfway through the book at this point. What about the models to come? Unfortunately, there are no more models. The rest of the book (the last 150 pages) is spent describing several techniques that could be analyzed with more or less cognitive language—attention, thought, belief, *etc.* Since no model is presented, these descriptions are just vague references to vaguely defined behavioral patterns. Given the lack of cognitive models, Mahoney can hardly be blamed for trying to stretch a point. He describes what he calls a third model, Mediational Model III: The Cognitive Learning Model. Here is Mahoney's description of this "model":

"Man is viewed as a complex organism capable of impressive adaptation. He is in a continuous reciprocity relationship with his environment, a relationship which

might be analogized as a cybernetic feedback loop. Behavior changes are influenced by the current psychological state of the organism, his past learning history, the existing environmental situation, and a variety of interdependent cognitive processes (*e.g.*, selective attention, anticipated consequences, etc.). Part of man's awesome complexity derives from his extensive evolutionary differentiation. Three interdependent nervous systems (central, somatic, and autonomic) affect the organism's adaptive performance. Under many circumstances the autonomic nervous system—which is most closely associated with basic life support functions—appears to override and dominate man's actions. However, we now know that the three systems interact in complex and reciprocal fashion." (p. 145.)

But this is no model. It is not even a sketch of a model. Who could disagree that man's adaptation is impressive, his complexity awesome, his nervous systems interdependent? At this point, Mahoney seems to have given up on describing coherent mediational models and the rest of the book is devoted to describing a series of therapeutic procedures in loosely cognitive terms. Some of the therapies seem to be effective when tested. For instance, Meichenbaum has effectively modified the verbal behavior of schizophrenics by rewarding them for various patterns of speech, and this modification has transferred to several other, largely verbal, tasks. A traditional behavior therapist would describe such a procedure in terms of the actual behavior modified and the extent of its transfer to other behavior. It does little harm, I suppose, to claim, as both Meichenbaum and Mahoney do, that by modifying a schizophrenic's speech they have changed his view of the world, but, of course, no scientific evidence for such a change is presented.

As an example of the completely pointless attribution of cognitive terms to what is essentially a behavioral procedure, let us take the oldest of the cognitive therapies, Ellis's Rational

Emotive Therapy (also called, cognitive restructuring). The therapy, in brief, consists of the following steps:

1. The client is interviewed and behavior observed to determine which of several irrational ideas are causing trouble. An example of an irrational idea is: "The idea that it is a dire necessity for an adult to be loved by everyone for everything he does."
2. The client is told of this irrational idea and presented with alternatives that are more rational.
3. The client is assigned various tasks, "homework assignments", and these are later analyzed during therapy sessions for application of the more rational ideas. The book contains a transcript of a therapy session. While discussing a young man's homework assignment with him the therapist says, "Good . . . you're getting pretty 'insightful.' Now let's see how well you've done your cognitive homework. Give me a brief summary of what happened and why you may have felt depressed."

There are two interesting things about this therapy:

1. The irrational ideas are rarely, if ever, expressed as such by the client. Rather, they are inferred by the therapist from the client's behavior, including verbal behavior. In other words, the therapist uses the idea as a way of describing the client's behavior. It is the *therapist* who has the idea, if anyone can be said to have it. Of course, after the therapist has expressed the idea the client may be taught to express it as well, and to apply it to instances of self-behavior.
2. The therapy does not stop after the irrational idea is uncovered and the rational idea presented. If ideas themselves could control behavior, as Ellis and Mahoney assert, then one would expect a change of ideas to result simply in a change of behav-

ior, as when a general orders his army to advance or retreat. But far from ending after the client's ideas are changed, therapy barely begins at that point. The therapy consists of homework assignments that to all intents and purposes are identical with the sorts of things noncognitive behavior therapists do as a matter of course.

Thus, in Rational Emotive Therapy, the "irrational" and "rational" ideas are ways that the therapist has of characterizing the client's behavior. They are concise and convenient descriptions of behavior, but there is no evidence that they have an existence apart from that behavior in the subject's mind or brain or anywhere else. Furthermore, the actual therapy itself (protestations of the therapists to the contrary notwithstanding) presumes no such separate existence for ideas.

Unfortunately, as in so many cases described in this book Mahoney concludes: ". . . the experimental research evaluating the efficacy of cognitive restructuring has been sparse, methodologically poor, and summarily modest in its implications." (p. 182.)

At this point, we may well turn and ask what was wrong with the nonmediational theory in the first place. In the face of the lack of support for so many of the cognitive therapies, in the face of the fact that evidence for one mediational model is, perforce, evidence against *all* the others, the nonmediational model comes off pretty well. What was wrong with it that made Mahoney reject its apparent virtues (observability, parsimony, simplicity, theoretical consistency) in favor of unsupported crypto-mythological models of events inside the head? Mahoney argues that much current behavior cannot be explained in terms of current stimuli, responses, and reinforcements. Consequently, one must make inferences, and according to Mahoney: "Inference involves a 'leap' from observed to unobserved phenomena." True enough. But let us look before we leap. Given that one observes a current behavior unexplained by observed cur-

rent events, what sort of inferences should one make about it? Two possibilities suggest themselves. First, we might infer that current events are influencing current behavior, even though we cannot see them. For the lack of a better place, we may assume that these events take place in the mind or the nervous system, (but what we know about the nervous system fits so poorly with what we know about behavior that cognitive psychologists profess only a distant hope of relating their models to actual neurological events). This sort of leap has the advantage of preserving temporal contiguity between cause and effect, as well as conforming to current linguistic convention. But if, after these many many years of treating behavioral disorders as if they were mental disorders, the best we can do are the therapies described in this book, it might be advisable to find somewhere else to leap. The other alternative is to abandon the requirement that a cause be temporally contiguous with its effects, and to infer events in the past when we are confronted with events in the present that have no apparent present cause. If observed events in the past are usually correlated in certain ways with events in the present, it should be sufficient to infer such events even when they are not observed. If a subject in an experiment is instructed on Day 1 to make response X on Day 2, the cause of response X when it is observed, need not be attributed to anything inside the subject's head, but directly to events on the previous day. There is nothing logically wrong with supposing that events on Day 1 directly cause events on Day 2 without supposing a chain of intermediary events (in the mind or the nervous system or elsewhere). In physics, when Faraday first suggested that physical forces could act at a distance, the response of other physicists was to infer a series of imaginary hooks bridging the gap between a magnet and an iron filing. But as time has gone on, the notion of action at a spatial distance has come to be generally accepted without the intermediary hooks. Similarly, action at a temporal distance ought to be accepted without the intermediary r_g - s_g 's or coverants, or neural

circuits, or cognitions that are usually invoked to bridge the gap. Operant conditioners are often accused of over-reliance on models from physics to justify their scientific behavior. The models in this book do not represent freedom from physics, but *rather* attachment to physics—of the nineteenth century! To return to the frightened airplane passenger who according to Mahoney, “. . . reacts not to the *real* environment but to the *perceived* environment”, we should rather infer that the passenger reacts not *only* to the present environment (loud noises) but to events in his past, such as stories he has heard about engines falling off airplanes. Two equally frightened passengers are as likely to have had similar experiences in the past as to have similar current “perceptions”. Inferences about past experiences may be as speculative as inferences about present cognitions, but at least they are potentially observable and suggest methods of observation and modification short of brain surgery. Such an attitude might seem to be mere semantics, mere translation of current cognition to past event, but semantic issues are not necessarily trivial. It is only a semantic issue whether the earth goes around the sun or the sun, around the earth, but the implications of this issue for the nature of man and his place in the world are nonetheless important. Similarly, whether we regard present behavior as caused by present events in the mind or past interactions between behavioral and environmental events has important implications for the sorts of therapies we devise. This was pointed out by Skinner in 1955 and deserves to be re-emphasized in the light of current versions of molar behaviorism (Baum 1973; Herrnstein, 1969; Rachlin, 1970; Staddon, 1973). We *can* have inference without mediation, however much this goes against the

dictates of “common sense” and the current direction of behavior therapy as characterized in this book.

Finally, I should not want the reader to suppose, from the above remarks, that “cognitive” therapies never work. At the end of the book, Mahoney describes what he calls “an emerging paradigm: the personal scientist”. This paradigm (largely unrelated to the cognitive models previously described) seems eminently sensible from a behavioral viewpoint.

Clients are instructed in how to treat themselves as subjects, how to monitor, record, and manipulate their own behavior through modeling, reinforcement, and self-evaluation. All of these techniques may be effective, but they are perfectly describable in terms of discriminative stimuli, responses, reinforcements, and the correlations among them—in short, the behavioral language of this journal.

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