Book Reviews

Behaving Man

Behavior. The Control of Perception. WILLIAM T. POWERS. Aldine, Chicago, 1973. xiv, 296 pp., illus. $8.95.

Many a psychologist spends so much time on the details of specialized problems that he forgets the point. For this reason, among many, the appearance of a book proposing a global model of mind, brain, or behavior is both noteworthy and salutary. Wiener's *Cybernetics* (1948), Hebb's *The Organization of Behavior* (1949), Ashby's *Design for a Brain* (1952), Von Neumann's *The Computer and the Brain* (1955), and Miller, Galanter, and Pribram's *Plans and the Structure of Behavior* (1960) are premier examples of such catholic endeavors. Powers's book is another.

It is always difficult to assess the contribution of books of this genre, even years later, because their impact is subtle and diffuse. Nonetheless, most would agree they are of signal importance even though the very scope of the enterprise undertaken means that each lacks the level of specificity on which others can most comfortably build. Powers's book shares with the others many qualities: lucid writing, an admirable combination of novel approach and clear precedent, reserve in the polemical portions. However, his work differs in important ways. He sees his theory not as an extension of traditional models of man, but as a paradigm shift which he hopes can bridge the gap between the mechanistic view of man as automaton and the humanistic view of man as autonomous. The model he presents is even broader in scope than those of his precursors, and he draws ecumenical conclusions from it.

Powers's model of the individual human comprises a hierarchy of feedback control systems. His major points are that the essence, if not the detail, of this model goes beyond all others in accounting for the crucial data of psychology, experience; that the stimulus-causes-response (SR) model of behaviorism and conventional scientific wisdom is both different from the feedback control model and fundamentally incorrect; and that the acceptance of the SR model has led to consequences ranging from waste of laboratory time to social unrest.

Many, by now perhaps most, psychologists have recognized limitations in the SR model and have proposed other concepts, but few serious scientific psychologists have argued that it is fundamentally incorrect. (None, to my knowledge, believes with Powers that it is the root of all evil.) On first reading, many will dismiss Powers's interpretation of behaviorism as antiquated and see his model as fitting within the modern behaviorist framework (*1*); psychologists do not talk *only* of stimulus and response. Confronted with Powers's model, many will see him unwittingly riding the SR horse while beating it to death. Those who see the horse alive will think the whipping foolish; those who see the horse as dead will think the ride is futile.

Closer examination reveals that Powers is critical of a more abstract model of causation, of which stimulus-response associationism is but one prominent example. Graduate students are still admonished to specify and distinguish independent variables and dependent variables; even humanist psychologists make statements such as "stress causes anxiety." To Powers these are but SR models in different guises. Once this is understood, the model will be seen as differing in kind from SR models which, though not universal (Powers notwithstanding), are still legion. A close look at the distinction is therefore in order.

At least as far back as Thorndike's earliest work (*2*), psychologists were aware of a difficulty with describing behavior as specific stimulus-response causal connections that get "stamped in" by experience. Thorndike noticed, and wondered at, the behavior of cats attempting to escape from puzzle boxes by manipulating various levers and strings. He observed that even after the task was solved the cat's behavior in repeated escapes was not stereotyped. This is a central quandary for psychology: How can one explain the variety of means an organism uses to achieve the same end? Monkeys use a variety of motions to press bars for food; rats that have learned to run a maze proceed to swim it correctly when it is flooded. Through the years this issue has continued to simmer if not boil. Behaviorists have ignored it; after all, they were getting results by reinforcing SR connections, weren't they? Clearly each bar-press is some sequence of movements that *could* be analyzed in SR terms if anyone were of a mind to do it, and cognitive theorists offered no better account.

Powers correctly sees, as others have seen before him, that the SR explanation is wrong. The stimulus-response-stimulus-response sequence is a will-o'-the-wisp. Behavior is too variable and novel to permit such an analysis, much less to be accounted for on the basis of previous learning-by-doing. We can explain the successes of the behaviorists' analysis, though it is fundamentally wrong, if we note that behaviorists do not observe behavior but the *results* of behavior. They do not measure a totality of muscle positions and tensions, they record the closing of a relay which is the result of an animal's movements. All along they have been recording the achievement of goals. Let's call a goal a goal, argues Powers. If we do we will see that what is invariant is the organism's aim, not his trajectory. Thus regularity is not to be found in behavior, but in perception. On this view negative feedback replaces positive reinforcement as the guiding principle of behavioral organization.

To illustrate feedback control, consider a man holding his arm out, parallel to the ground. His nervous system sends signals to his muscles, which respond by tensing, and this results in the feedback of signals to the nervous system. These signals are compared (algebraically, in a "comparator") to a reference level, and the difference, or error, is the signal sent to the muscles. What the man controls, in the sense of attempting to match it to a criterion, is his *perception* of arm

position. He does not control his muscles, in this sense, for tensions may vary greatly in counteracting disturbances. This closed loop of continuous (analog) signals cannot be reduced to a linear cause-effect chain, nor is there any point in doing so, since it is a well-understood mechanism itself. The puzzle is how this analysis can account for more complex goal achievement, like walking to the store, or graduating from college, or achieving peace with honor. Powers suggests it can, and the bulk of the book sketches out how.

The proposed hierarchical organization of control systems has the reference level of one system provided by an output from a control system in the next higher order in the hierarchy. The model, importantly, is based on the assumption that the signals which provide outputs, perceptions, and errors are simply quantities (firing rates of nerves, he suggests), not complex "messages." In addition to feeding back to a comparator, perceptions also feed up to higher and higher orders and may provide inputs to all orders. It is the totality of these perceptions that is our experience, but each component is simply a particular magnitude at a given moment. To account for the complexity of human experience must require an enormous number of levels, right? Wrong. To get all the way up to experiences of logical thinking, moral principles, and religion (beyond which Powers wisely gives up) requires only nine levels. It sounds too simple to be true, and of course it is not true, as Powers noted before I did. It *is* a suggestion worth examination.

Not surprisingly there is a decrease in specificity, number of neural hypotheses, and extent of discussion as the narrative ascends the hierarchy. In particular there is a sharp break in credibility at about level 6, the first five levels perhaps being able to stand alone as a model of a coherent segment of behavior. The model engages the world at the sensory nerve endings. Those endings that terminate in the organism's effectors are inputs to order 1 control systems, which control the perceived intensity of these kinesthetic inputs. Each order comprises large numbers of control systems, so that the model makes up in breadth what it lacks in depth. There are some 800 order 1 systems. Each controls the intensity of perceptions resulting from effector state by feeding them to subtractive comparators, where they are compared with reference levels supplied by order 2 systems. These perceived intensities also provide inputs to order 2 systems; other order 2 inputs are supplied by stimulation of sense organs of sight, pressure, and so forth which are not functions of effector state. Order 2 systems thus receive and control combinations of intensities, which Powers identifies as sensations (for example, the taste of steak, temperature). The hierarchy proceeds in this fashion through the next three levels, which control in turn configurations of sensations (for example, postures, phonemes, objects), changes in configurations (for example, motion), and sequences of changes, or events (for example, walking). Suggestions are made as to neurological sites of the components of these levels, and compelling arguments for this structure are offered. Here the model is on its firmest ground, although it is not described in sufficient detail to convince me that such an organization could actually accomplish the functions claimed for it. One would like to see a walking robot constructed according to this blueprint, and a mathematical analysis of the sort Minsky and Papert so cogently applied to the Perceptron model (*3*). Short of that no definitive claims can be made for the model's sufficiency.

Orders 6 through 9 become much more speculative but in some ways more interesting. Powers describes the break as a change from "classes of perception that can be seen as exterior to ourselves to those which seem to be inside ourselves—from the world of 'physical reality' to the world of 'subjective reality.' " The arguments for hierarchical organization and feedback control remain interesting, but the functions which combine and compare neural signals take on new complexity. In fact, even Powers seems more comfortable with a computer program metaphor, and makes no serious commitment to the simple model of perceptual-signal/reference-level comparator. By the time we get to order 8 (control of principles, for example, honesty) and order 9 (control of systems concepts, such as the government, physics, the Los Angeles Dodgers), neural signals and subtractive comparators cease to be mentioned.

Powers's expressed aim is to achieve a complete model before testing its correctness. The nine levels presented encompass a range of experience sufficiently broad to provide a potential home for the major phenomena of psychology, yet two important additions are needed and supplied. The hierarchy described thus far offers an account of the behavior of a static, fully developed system unchanged by its history. Powers must add memory and a means of organizing and reorganizing, that is, learning.

The discussion of memory shows Powers at his best. What memory must do and how it must do it are neatly laid out, and a remarkably simple scheme is proposed which bears on imagination, hallucination, dreaming, and learning by imitation, as well as remembering.

The more important component, at least as far as Powers's ultimate purposes are concerned, is learning. Unfortunately this is the least enlightened portion of the book. The question posed is how the nine-level organization comes about. The first approximation to an answer is that perennial favorite, random change guided by the effects of the new organization on a few critical variables (such as hunger, pain) which the organism must keep within bounds. To appreciate the emptiness of this model one might consider how many ways 10^{11} neurons can be organized into nine levels of feedback control, and what proportion of these are viable (*4*). The second, and final, approximation is the adoption of a suggestion of Wiener's, namely the insertion of known test signals into control systems and the determination of the "appropriateness" of their effects. The sole criterion offered for appropriateness is stability, and this will not suffice. The learning hypotheses thus give us no clue to even the basic facts needed: the time course of reorganization and the limits of stability of the entire system. Thus they do not begin to show us how reorganization promotes the achievement of higher goals.

How does all of this account for the world's continuing history of individual anxiety and social tumult? The argument runs as follows. We are bent on controlling one another, since the behavior of others is frequently at odds with our own goals. The only means of changing another's behavior is to change his internal reference levels, through reorganization. Since we do not and cannot know the total organization of anyone, this in general results in conflicts between his goals and

the imposed ones. Stability will be lost and attempts to overcome the conflict will ensue. This will be true even of attempts at self-control, since the individual is unaware of his own internal organization. Finally, although reinforcement is the only way to exert external control, humans will discover who metes out rewards and punishments and will use cunning or force to circumvent the process.

The trouble with Skinner's program, claims Powers, is not that the world will not accept his view, but that it already has accepted it, since there is no other means of control and control is what we demand. But constant diddling with control systems leads inevitably to conflict and ultimately to revolt. Salvation can be found only in stopping all attempts at control and influence.

Powers's hope was to reconcile the mechanistic and the humanistic conceptions of man. Mechanists (some of them) will like the model and humanists (some of them) will like the conclusions, but I am afraid that the rapprochement has not been achieved. There are more assumptions than those embodied in the model lying between it and the final message. What these are is not made transparently clear, but they involve, at least, assumptions about the reorganizing principles and about man's perception of other men as special entities. I must confess that I do not even understand what Powers means when he politely suggests that all attempts at control be stopped. The only alternative he offers is cooperation, but the line between the two has never been made clear. Nor do I understand why animate controllers are more troublesome than inanimate ones, or how we will alter a control-crazy society without influencing it. Perhaps my control system is caught on a local stability plateau from which I cannot see clearly. Others may do better and I hope they will try. Not urge or suggest or demand, I suppose; but hope.

ROBERT K. LINDSAY
Mental Health Research Institute, University of Michigan, Ann Arbor

References and Notes

1. W. M. Baum, *Science* **181**, 1114 (1973); H. W. Reese, *ibid.*, p. 1114.
2. E. L. Thorndike, "Animal intelligence: An experimental study of the associative processes in animals," *Psychol. Monogr. No. 8* (1898).
3. M. Minsky and S. Papert, *Perceptrons: An Introduction to Computational Geometry* (MIT Press, Cambridge, Mass., 1969).
4. For an elaboration on this topic, see R. K. Lindsay, *Contemp. Psychol.* **13**, 113 (1968).

Restorative Processes

The Functions of Sleep. ERNEST L. HARTMANN. Yale University Press, New Haven, Conn., 1973. x, 198 pp., illus. Cloth, $8.50; paper, $2.95.

Why do we sleep? Although researchers have been busy describing how we sleep, many regard it as improper then to inquire whether sleep should have purpose or functions, physiological or psychological. In this book, Hartmann argues (and I agree with him) that there is a requirement for sleep and that we must therefore ask what needs it serves. He draws on the available observational evidence in theorizing about different functions for the two kinds of sleep, usually termed REM and NREM sleep in the United States. He prefers his individual terminology of S (synchronized) and D (desynchronized) for these, mentioning the synonyms only in a footnote.

Hartmann writes clearly and certainly without excess of personal diffidence. A reader could be excused for thinking that Hartmann alone has suggested that S sleep must be linked with general restorative anabolic processes and D more specifically with brain synthetic processes. What is original in this book is its attempt to explain why S always *precedes* D. Hartmann proposes that macromolecules synthesized in the brain during S are, in a second step, utilized "in processes of restoration and reconnection" of the cerebrum during D. In a speculative field some inconsistency between chapters is allowable: at one time Hartmann appears to think that S is mainly related to general bodily synthesis but elsewhere says that with S increased synthesis probably occurs especially in the brain. However, he does not refer to the cerebral blood flow, which is actually much greater during D. This would be compatible with increased oxidative metabolism during D, hence with D's being more important for cerebral restoration than S.

It is Hartmann's claim, based largely on his own research, that intellectual and emotional tiredness cause longer sleep and, specifically, that more D is a response to greater cognitive or learning activity (he does not mention the work indicating that perceptual deprivation enhances D). He first describes the evidence for his claim as "suggestive" but later allows himself a "probable" when marshaling support for the proposal that in D new structural or enzymatic protein is laid down in order especially to restore brain catecholaminergic receptors or presynaptic axonal endings exhausted during waking effort. It is in his review of catecholamines and sleep, and in his arguments therefrom, that I find him at his most persuasive.

Few sleep researchers have been biochemists, and when Hartmann writes that the duration of S and D is roughly the amount of time necessary for brain macromolecular synthesis and structural change I can imagine biochemists stirring uneasily. Yet these remarkably regular durations exist: Hartmann has not hesitated to face the challenge, nor has he shunned paths that angels might fear to tread.

IAN OSWALD
Department of Psychiatry, University of Edinburgh, Edinburgh, Scotland

Origins of Life

Molecular Evolution. Prebiological and Biological. DUANE L. ROHLFING and A. I. OPARIN, Eds. Plenum, New York, 1972. xx, 482 pp., illus. $24.

This book is a collection of 35 papers dealing with diverse topics related to the general question of chemical evolution which were contributed in honor of S. W. Fox on his 60th birthday. The papers are organized into the following groupings: History and Scope, Thermodynamic and Philosophical Considerations, Micromolecules, Macromolecules, Protocells and Cells, Academic Aspects, and finally Man and Evolution. The section on macromolecules is the longest (about 35 percent of the book), while that headed Man and Evolution is only three pages long. The scope and the style of the contributions are varied. Some authors concern themselves with an investigation of one particular point and have contributed papers such as might have appeared in a journal. Others are reviews of their authors' own research over an extended time period. A third group are general discourses on topics related to chemical evolution by scientists who are interested in the subject but are not actively carrying out research in it.

One advantage of a compilation of this type is that it allows the inclusion of a broad spectrum of contributions which do not necessarily fit together in a logical array. For example, I was sur-