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The comments by Baum and Reese on my control-system approach to understanding behavior are the most balanced I have received from behaviorists. I thank them for trying to find a place for my work within behaviorism, an attempt that reflects generosity, but not understanding, of what I said (or tried to say). The conceptual basis of control-system theory is so alien to behavioralist thought that there can be no such easy reconciliation. The best we can hope for is a constructive confrontation.

Baum says that a scientific psychology must deal with the observable, which to him means behavior. Behavior, however, is not something self-evident that anyone can see just by looking. What is the behavior of a man walking? Is he really tensing his leg muscles, moving his legs, walking, going to buy a paper, on his way to work, making a living for his family, or maintaining his self-respect? The point of view of the observer defines the behavior he sees. The actual behavior of the nervous system consists only of sending neural signals to muscles and glands; that is the last event that truly reflects the system's output. From that point outward, the results of that output become more and more mixed with properties of any events in the external physical environment, so that even such elementary behavior as a "movement" no longer is a unique indicator of a particular activity in the nervous system. Thus, while Baum's pronouncement seems reasonable on the surface, it ignores one of the deepest conceptual dilemmas of behaviorism.

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Concept of the controlled quantity. To find the proper definition of the controlled quantity, the observer must recognize that his own point of view determines the behavior he will observe, and he must find an objective way to discover the right point of view—namely, that of the behaving system. The observer must try to find out which of the infinity of potential controlled quantities is the one that the behaving system is actually sensing and controlling. Only when the controlled quantity has been correctly identified can the observer see that the system's outputs are always such as to counter the effects which environmental disturbances would otherwise have on the controlled quantity. In my article I presented an experimental paradigm, new to psychology, for testing hypotheses concerning the controlled quantity and its reference level.

In the section on controlled quantities in my article, there appears an approximation, \( g(d) \approx -h(o) \), which says that the cause-effect relationships that can be observed between stimulus events and consequences of nervous system outputs—responses—are expressible wholly in terms of the physics of the local environment, containing almost no information about the behaving system at all. I see no way in which behaviorism can survive a full understanding of the derivation and significance of this harmless expression. If control-system theory does indeed describe correctly the relationship between organisms and their environments, behaviorism has been in the grip of a powerful illusion since its conceptual bases were laid.

It is therefore not possible that behaviorism already contains an adequate treatment of feedback phenomena; if it did, a behaviorist would have discovered this illusion already. Many behaviorists have observed feedback phenomena, but they have tried to deal with them by translating the terminology of control-system theory in such a way that well-accepted behavioristic principles would remain undisturbed. That is why “purpose” has lost its original meaning of inner purpose or intentionality, and has been redefined as consequences. That redefinition was necessitated by the fact that early behaviorists knew of no physical system that could contain inner purposes—their telephone-switchboard model had no place for them, and control-system theory then lay far in the foreseeable future.

In control-system terms, a purpose...
is not a consequence of behavior, but a model inside the organism for what it wants the perceptual consequences of its outputs (modified by environmental disturbances or not) to be. When I bowl, my inner purpose is to perceive all the pins falling on the first ball. What I perceive is generally something different. I am still doing my best to alter my outputs in such a way as to reduce the error between what I generally perceive and what I intend to perceive. Another observer can discover that intended perception by manipulating my environment until he finds the state where I cease to alter my outputs in opposition to the changes he causes. There is nothing metaphysical or conjectural about this process. But it does not make any sense in behavioristic terms, because it is designed around rigorous laws of feedback, not around the imprecise usages of the term feedback that are found in behaviorism.

There seems to be a general impression that feedback is analyzable (in Reese's terms) by following a "unidirectional, linear causal chain" around and around a closed circle (I trust that Reese noticed that the circle is closed). That approach to feedback, often expressed as taking into account the effects of a response on subsequent stimuli, is the natural one, but, as every beginning control-system engineer soon discovers, it leads to totally incorrect predictions of the behavior of the system being modeled. The qualitative chain-of-events approach leaves out the crucial factor of system dynamics; when that is properly taken into account, through use of a physical analysis of the system and its environment and application of differential equations or transform methods, a very different and surprising picture emerges. If the control system one wants to model is free of spontaneous, self-sustained oscillations (as normal behavioral systems are), time lags in the system can safely be ignored, and the behavior of the whole system can be seen quite correctly as occurring simultaneously with disturbances. The output changes along with the disturbance (a normal, slowly varying disturbance), and the input variable being monitored continually tracks the inner reference signal, if a variable inner reference signal exists. There are no loopholes in this analysis; if organisms are in the negative feedback relationship with their environments, this is how they behave. To arrive at a different conclusion, one would have to show that the bases of

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control-system theory are wrong, and he would have a lot of engineers who use it every day to convince.

Thus, the attempts by behaviorists to bring feedback phenomena into the scope of their conceptual scheme have involved only a superficial adoption of certain terms and loose qualitative observations, the true beauty and power of control-system concepts having been left behind. The distortions of feedback theory that occurred in the adoption of the terminology were precisely those which would prevent change in the basic conceptual scheme of behaviorism (this should not surprise control-theory fans, since all organisms manipulate their own perceptions to keep them in the desired state).

There is no “reference signal that prohibits operant conditioning,” as Reese puts it while guessing wrong about what I meant. Operant conditioning is a fact; in my model, it is a portion of a control process whereby organisms modify their own inner structure of control systems as a means of keeping certain critical variables (W. R. Ashby’s term, as I noted), at genetically established reference levels. I was talking about the feasibility of people deliberately trying to control the behavior of other people through deliberate application of operant conditioning.

In order to control another person, one must establish contingencies or schedules of reinforcement. Whatever one chooses to use as a reward, he must make sure (i) that the subject needs or wants the reward and (ii) that the only way the subject can obtain the reward is by doing what the experimenter wants to perceive him doing. The experimenter, of course, is trying to control his own perceptions relative to his own inner purposes, using the subject as his means.

The establishment of contingencies, therefore, requires that the experimenter already be the sole source of something the subject wants, and establishing that situation is where operant conditioning will fail as a way of controlling behavior—as it has failed throughout recorded history. An experimenter trying to control people rather than laboratory animals cannot conceal the fact that he has what the subject wants, and is withholding it until the subject does what the experimenter demands. If one person can establish a contingency, another person can see that he has done so, and can decide to “unestablish” it. If the act that the experimenter wants to see performed in any way inconveniences the subject, the subject will be forced by his own nature to find a way to circumvent the contingency. He can operate properly only on the basis of his own inner purposes, not on the basis of the experimenter’s. Only a god capable of seeing a person’s entire structure of inner goals could establish contingencies for that person without creating conflicts that would lead to a direct and violent confrontation. Even then, the god would be constrained to controlling the person in ways that created no uncorrectable errors in that person’s control hierarchy.

Operant conditioning is only a modern term for what people have been trying to do to each other since civilization started. Everyone knows that people seek rewards and will change their behavior, within limits and as necessary, to get those rewards. But rewarding always implies withholding, and withholding what people need is a sure way to create violent and bloody conflict. An excellent case can be made for the statement that the present state of the world is the direct result of people trying to set up contingencies of reward for each other. It is time we realized that this principle of social interaction is the cause of, not the solution to, our most serious human problems.

Finally, I want to acknowledge the justice of some of the criticisms of my work. I know that I have overgeneralized in speaking of “behaviorists” when I really should have said, “some behaviorists.” My aim is to find ways to effect a transition from what I believe is an outmoded view of the nature of human nature—and animal nature—to what seems a vastly more productive and humane point of view. My attitude toward what I see as the basic errors of behaviorism is not one of irritation or superiority. My model is only a feeble step in the right general direction. It is instead that there is an enormously difficult task ahead—but, considering what I see as the possible results of success, worth all the effort. I hope that Baum and Reese and other behaviorists will come to see it this way after careful consideration. I know their task is harder than mine, and it would be even harder if this clash of ideas were set up so that someone had to win, and someone had to lose.

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(Continued from page 1169)

The Child in His Family. The Impact
of Disease and Death. E. James Anthony
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York, 1973. xxiv, 510 pp. $15.95. Year-
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9, Addition and Elimination Reactions of
Aliphatic Compounds. C. H. Bamford
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A Concise Introduction to Organic
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Constraints on Learning. Limitations
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R. A. Hinde and J. Stevenson-Hinde,
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Cours d’Automatique. Vol. 1. Systèmes
Asservis Continus. J. Mainguenaud. Mas-
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A Course in APL/360 with Applications.
Louis D. Ges. Addison-Wesley, Reading,
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The Development of Freud’s Thought.
From the Beginning (1886–1900) through
Id Psychology (1900–1914) to Ego Psychol-
yogy (1914–1939). Reuben Fine. Aronson,
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The Impact of Parasitic Diseases in St.
Lucia. Burton A. Weissbord, Ralph L.
Andrano, Robert E. Baldwin, Erwin H. Ep-
sin, and Allen C. Kelley with the assist-
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Viruses. Papers by Robert W. Leader
and 15 others. MIT. MIT Press, Cambridge,
Massachusetts, 1973. xvi, 162 pp., illus.
$14.

Dominique. Analysis of an Adolescent.
Françoise Dolto. Translated from the

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Friction. An Introduction to Tribology. Frank Philip Bowden and David Tabor. Anchor ( Doubleday), Garden City, N.Y., 1973. xiv, 178 pp., illus. $5.95; paper, $2.50. Science Study Series.

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**An Introduction to Food Rheology.** H. G. Muller. Crane, Russak, New York, 1973. xii, 148 pp., illus. $9.25.


**Let's Talk About the New World of Medicine.** M. W. Martin. David, Middle Village, N.Y., 1973. iv, 76 pp., illus. $4.95.


**Men in a Developing Society.** Geographic and Social Mobility in Monterey, Mexico. Jorge Balan, Harley L. Browning, and Elizabeth Jelin with the assistance of Waltraut Feindt. Published for the Institute of Latin American Studies by the University of Texas Press, Austin, 1973. xx, 384 pp., illus. $11.50. Latin American Monographs, No. 30.


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**Responses of Fish to Environmental Changes** edited by Walter Chavin, Wayne State Univ., Detroit, Michigan. (23 Contributors) '73, about 480 pp., 166 ill., 39 tables

**Fish Chromosome Methodology** by Thomas E. Denton, Samford Univ., Birmingham, Alabama. '73, about 172 pp., 26 ill., 1 table

**Ecology and the Quality of Life** edited by Sylvan J. Kaplan, and Evelyn Kirby-Rosenberg, Jersey City State College, New Jersey. (24 Contributors) '73, about 240 pp., 23 ill., 11 tables

**Mercury, Mercurials and Mercaptans** edited by Morton W. Miller and Thomas W. Clarkson, both of the Univ. of Rochester, New York. (33 Contributors) '73, 404 pp., 116 ill., 57 tables, $19.75


**Eugenic Sterilization** compiled and edited by Jonas Robitscher, Emory Univ. Schools of Law and Medicine, Atlanta, Georgia. (9 Contributors) '73, 152 pp., 1 ill., $7.95

**Diseases of Fishes** (3rd Ed.) by C. van Duijn, Jr., Zeist, The Netherlands. '73, 380 pp., 388 ill., $12.95

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xiv, 284 pp., illus. $12.95. Harper's Series in Modern Mathematics.


**The Nature of Multiple Sclerosis.** Papers by Heavey Rickham and 11 others. MSS Information Corporation, New York, 1973. 186 pp., illus. $15.


**New Methods of Automated Analysis of Protein Structures.** Papers by J. Daniel Lynn and 14 others. MSS Information Corporation, New York, 1973. 246 pp., illus. $17.50.


**Oral Contraceptives.** Psychological and Physiological Effects. MSS Information Corporation, New York, 1973. 178 pp., illus. $15.


**Parasites of Laboratory Animals.** Robert J. Flynn. Iowa State University Press, Ames, 1972, xvi, 884 pp., illus. $35.

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