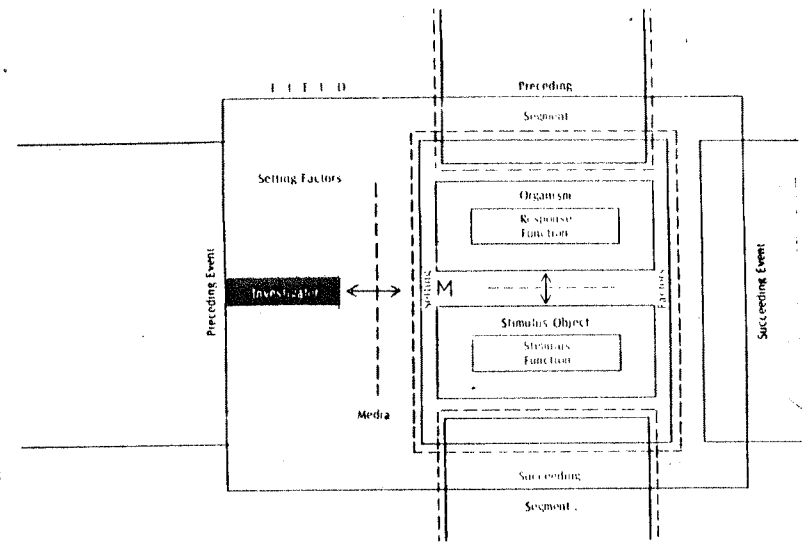


# THE I n t e r b e h a v i o r i s t



## A QUARTERLY NEWSLETTER OF INTERBEHAVIORAL PSYCHOLOGY

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The "control mechanism" view of culture begins with the assumption that human thought is basically both social and public---that its natural habitat is the house yard, the marketplace, and the town square. Thinking consists not of "happenings in the head" (though happenings there and elsewhere are necessary for it to occur) but of a traffic in what have been called, by G.H.Mead and others, significant symbols---words for the most part but also gestures, drawings, musical sounds, mechanical devices like clocks, or natural objects like jewels---anything, in fact, that is disengaged from its mere actuality and used to impose meaning upon experience.

C. Geertz, 1966. The Impact of the concept of culture on the concept of man. In J. Platt (Ed.), *New Views of the Nature of Man.*

THE INTERBEHAVIORIST

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The Agora

The conceptualization of "thinking" revealed in this issue's cover quote contrasts sharply in its naturalism with the view of "hearing" expressed in the following excerpt from a pamphlet entitled "How do you hear?", prepared by Warner/Chilcott Pharmaceuticals as a child patient aid:

In the cochlea, the vibrations are changed into nerve signals which go to the hearing center of the brain. And the brain says "Oh, I know that sound, it's a barking dog." Or, "That sound is a train whistle." And, that's how we hear!

In this case, a laudible attempt to provide a simple answer to a child's question "How do we hear?" fails because the brain is transformed from a participant bodily organ into an entity endowed with the capabilities for activity of whole organisms. Rather than answering the question posed, the purported explanation raises new, more puzzling, and insoluble questions: Who or what is the object in the hearing center that hears the barking dog and the train whistle? And by what process does that object hear? Such mystical treatments of perceiving and thinking far outnumber naturalistic analyses of the sort exemplified on the cover page, and as long as this is the case, children's efforts to learn about perceiving and thinking will more often leave them farther from than closer to the understanding of those even!

\* \* \*

A colleague at Kenyon College with no previous exposure to interbehaviorism read the Spring 1978 issue of The Interbehaviorist and subsequently responds by commenting that the publication seemed very "specialized" and questioning whether the label "underground publication" is an accurate one. Following the editor's reply, written after some hard thinking about who interbehavior is and what The Interbehaviorist is trying to do:

I appreciated your note concerning The Interbehaviorist and am very happy that you took the time to read it. I hope that your confusion with some of it is neither an index of overspecialization nor of a lack of clarity of expression. While the publication is clearly specialized in some senses, I believe that it has more to say to the general psychological community (and to a larger academic community, in fact) than most specialized publications I read. As for the term "underground publication," that was a label affixed to The Interbehaviorist by an editor of a kindred publication, Steven Brown of the

Kent State University political science department; his letter is excerpted in the issue you read. I must confess that I am both compelled and repulsed by the application of the term to our publication. "Underground" has an air of adventure and intrigue associated with it, and in that sense I like to think of us as doing something "underground." Also, it is fair to say that some premises of interbehaviorism (particularly the idea that physiological reductionism has no place in psychology) border on heresy in the larger psychological community. However, to the extent that the label "underground" implies that those with interbehaviorist leanings are a wild-eyed group of misfits brandishing a similarly misfit and anti-intellectual doctrine, the label is far off the mark. Interbehaviorism is rooted firmly in the soil of Aristotelian naturalism, and draws its sustenance from ideas that are respected both without and within the psychological community (though not so much within the more scientific psychological community). As for interbehaviorists, they are, for the most part, a not-so-wild-eyed but enthusiastic group of philosophically-oriented research psychologists committed to the belief that empirical psychology would derive many benefits from the adoption of an interactional mode of doing its business.

Your responses to the editor's characterization of our enterprise, whether in the form of emendations or elaborations, would be welcome.

\* \* \*

In the Summer 1978 issue of The Psychological Record, the following open letter appeared, reprinted here with the editor's permission:

To reach the age of 90 is in itself notable. To be professionally productive for more than 60 of the 99 years since the benchmark founding of Wundt's laboratory is especially remarkable. Two facets of Kantor's contributions characterize his work. First, with historical perspective he has inveighed against the pervasive and continuing spiritistic influence upon the sciences, particularly psychology. He has been insistent that in all scientific behavior the scientist needs to differentiate between construct and event and, rejecting prevailing animistic thinking, to derive the former from the observation of the latter. In his long list of papers and books he also originated and elaborated the interbehavioral viewpoint, a naturalistic approach offering a framework for studying and understanding the whole spectrum of psychological events. He avoided both dualism and the newer neurologizing while emphasizing the role of cultural conditions in the development of human behavior. He went beyond other critics among the behaviorists who seemed to concentrate on what he labeled "learnology" and who thus, with something like benign neglect, left certain areas minimally treated. Claiming to fill this void, many current writers seem openly to embrace mentalism or not to comprehend that their efforts mirror the persisting and perhaps burgeoning mysticism. In addition to his writing, Kantor also has had impact in the scientific world through The Principia Press which he still heads, founding of this journal, The Psychological Record, and teaching his many students. Dr. Kantor, we mark your day this August (8), congratulate and thank you, and wish you many happy returns!

Irvin S. Wolf

In the same issue of The Psychological Record are two additional items of interest to those who follow the distinguished career of Dr. Kantor: an article entitled "Cognition as events and as psychic constructions" based on a lecture he delivered at Wichita State University on April 13, 1978; and an unusually detailed review of his Psychological Linguistics book by David Herman.

\* \* \*

Two other items relating to Dr. Kantor:

The third volume of Krawiec's series of autobiographical sketches (The Psychologists) contains a reference to Kantor in an unlikely place: the joint autobiography of J. B. and E. Louisa Rhine. They note that while they were graduate students in biology at the University of Chicago in the 1920s, Louisa Rhine took a course under Dr. Kantor and they were both impressed by behaviorism and more generally by attempts to develop "an all-inclusive mechanistic philosophy of life" (p. 185) devoid of any supernatural elements. It is easy to see how Kantor's developing system would have impressed the Rhines in this regard; we might speculate that, in addition, interbehaviorism's message that a psychologist must be open to events unencumbered by presuppositions and constructs was recalled by them in their subsequent pioneering work in parapsychology, when they bravely and steadfastly pursued objectivity in a "taboo" area.

Cedric A. Larson provides the following comments about his acquaintance with Dr. Kantor:

I am working on the biography of the late Dr. John B. Watson (1878-1958) and this is the centennial year of his birth. I have been on this project for a number of years, but am now devoting most of my time to it. Several years ago (I think it was in the year 1964), and I have the interview in my files, I had an interview with Dr. Kantor about John B. Watson when he was teaching a summer school course in a college near Washington. He was most cooperative and gave me a long interview. He had once taken a summer school course under Watson early in the century at the University of Chicago. I have five or six of his books, and he has sent me many reprints of his articles. I recognized him immediately at the 1977 A.P.A. convention in San Francisco at the Hilton Hotel, and talked briefly with him. His nephew had brought him there, and although he was close to 90, his conversation and his mind were alert. I wrote him later in Chicago and he sent me more reprints. He is a remarkable man.

\* \* \*

Since Rosenthal's pioneering experimental work in the early 1960's, much attention has been given to the problem of bias in psychological research. Recently, T. X. Barber (1976), in his book Pitfalls in Human Research, has criticized much of this earlier research on grounds that it takes a too simplistic and narrow view of the problem of bias in empirical investigations. He argues that the researcher is intertwined with the events being observed at all stages of the investigatory process; thus, instead of focussing attention solely upon "experimenter effects" (biases introduced while the experimental procedure is being carried out), the investigator must be equally attentive to potential trouble spots earlier (the initial conceptualization and designing of the study) and later (data recording and analysis) in the investigatory sequence. Those familiar with the interbehavioral approach may recognize that the naïve attitude

about investigatory bias that Barber wishes to overcome derives from an unsatisfactory model of the researcher's relationship to the psychological events being studied, one wherein the researcher is viewed as set apart from the events being observed. In contrast, the interbehavioral conception is that the researcher is an inextricable part of the sequence of investigatory field events constituting the planning, implementation, and data manipulation of an experiment (see cover design). Thus, adoption by psychological investigators of an interbehavioral view would serve as the best antidote to the sort of inattentiveness to bias that concerns Barber in his book.

\* \* \*

This issue features contributions from Noel W. Smith and Edward Blewitt. Our former editor's contribution is a chart that shows the chapter correspondences between two texts that survey the domain of psychology from an interbehavioral perspective: Kantor and Smith's, The science of psychology: an interbehavioral survey and Pronko's Panorama of psychology. Smith writes that "the two texts harmonize very well and some readers may want to try using them together." Blewitt is a Ph.D. candidate at University College, Cardiff, Wales, U.K. who describes himself as "carrying out research into the descriptive analysis of behaviour-environment relationships with developmentally retarded persons." In this issue appears the first installment of a paper he wrote while an undergraduate at the University of Wales at Bangor.

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The Necessity of Social Control:  
A Critical Review of B. F. Skinner's Science and Human Behavior

Edward Blewitt  
University College, Cardiff

If one looks at Skinner's publishing history it will be seen that his first book, The Behavior of Organisms (1938), is one of empirical research, while his second, Walden Two (1948), is a utopian novel. This contrast gives us a good indication of the aspects of psychology that have interested Skinner, and which he finds important. Skinner's interests lie not only in carrying out psychological research but also in applying the principles formulated in the laboratory to the problems of modern society. Indeed, as the years have passed the social implications of psychological research -- developing a technology of behaviour change -- have occupied an increasingly important part of Skinner's published works. It is mainly through the social applications that the "experimental analysis of behavior" (Skinner's not particularly descriptive term for his approach) came to the public's attention, at least in the United States. Unfortunately, Skinner's work is still largely unknown to the lay public in the U.K., a sad indictment of British psychology, which seems reluctant to emerge from academic isolationism and enter the arena of social reality.

It is in Science and human behavior (1953) that the synthesis of laboratory research and its social implications is best represented. I will deal with only two aspects of this synthesis: (1) the application of a scientific approach to human behaviour; that is, the problem of determinism in human behavior; (2) the application of the findings of a scientific psychology to the problem of social control.

THE POSSIBILITY OF A SCIENCE OF HUMAN BEHAVIOUR

One of the most influential explanations of human behaviour is that each individual is totally free to decide whether to follow the ways of God or the ways of Satan. The modern assault on the concept of free will came as a consequence of the application of the methodology of the natural sciences to behavioural phenomena. The extension of scientific methods has not been without its critics: the resultant conflict is an example of the more general conflict existing between Aristotelian and Galilean modes of thought (see Lewin, 1931).

The Aristotelian mode<sup>1</sup> tends to be anthropomorphic and inexact, abounding in normative concepts taken from ethics. For example, the "high est" forms of motions occur only in heavenly movements, such as the stars, whereas the earthly world is endowed with motion of inferior types. When Galilean and post-Galilean physics disposed of the distinction between heavenly and earthly, and thereby extended the domain of natural law, it was not due only to the exclusion of value concepts but also to a different interpretation of classification. For Aristotelian physics the membership of an object to a given class was of critical importance because the class defined the essential nature of, and determined the likely behaviour of, the object. For the Aristotelian the environment played a part only in so far as it may give rise to disturbances or modifications of the motion which follows from the

<sup>1</sup>It is important to distinguish the Aristotelianism of Aristotle from the Aristotelianism of the Christian interpretation of Aristotle. In the following we speak of the latter, not the former.

nature of the object itself. For Galilean physics, on the other hand, the motion of an object depends on the relation of the object to the environment. This does not mean that the nature of the object becomes insignificant, only that the contextual situation assumes as much importance in determining the motion as the nature of the object. It is only by examining the concrete whole, which comprises the object, the environment, and the mode of interaction, that an event can be understood.

Aristotelian classes were defined as the sum total of those characteristics a group of objects has in common. This definition largely determined the Aristotelian concept of lawfulness and chance. For the Aristotelian those things were lawful which occur without exception. Also, those were lawful which occur frequently. Excluded from the class of the conceptually intelligible, then, were those things occurring infrequently or only once. The source of this conception may be that for Aristotelian physics not all physical processes possessed the lawful character ascribed to them by post-Galilean physics. In post-Galilean physics, with the elimination of the distinction between lawful and chance events, the necessity also disappeared for proving that the process under consideration was lawful. Instead, the question to be asked became what are the particular lawful relationships between the various factors which make up the event. For Aristotelian physics, on the other hand, it was necessary to have a criterion to decide whether or not a given event was lawful. Because the regularity with which similar events occurred in nature was used as such a criterion, the domain of Aristotelian science was limited to those events that were certified by repetition, and thus Aristotelian science was limited in its ability to understand complex, chaotic, and unintelligible phenomena of nature. The Aristotelian conviction that it is impossible to wholly comprehend the individual case encouraged, in addition, a certain laxity in inquiry: a satisfaction with setting forth mere regularities, requiring validity "in general" or "on the average" only. The complexity of nature makes it unreasonable to require complete, exceptionless validity. The Aristotelian ascribed to the proverb that "the exception proves the rule": exceptions are not counter-arguments so long as their frequency is not too great. For post-Galilean physics, however, exceptions are not taken lightly and are valid disproofs, even though they are rare (cf. Popper's falsifiability criterion of science in his The Logic of Scientific Discovery, 1959). In summary, the Aristotelian concept of science was static, resulting in little scientific advancement. By way of contrast, there is no limit to the range of application of Galilean science and this led to a rapid development and accumulation of scientific knowledge.

The application of the Galilean concept of science to events other than psychological ones is today met with little resistance. This is far from being the situation when it is applied to the field of human activity. The proposal that human behaviour is determined to the same degree as non-human activity is still strongly opposed: the belief that the individual has a free will, and that human behaviour is therefore capricious, maintains its hold, both within and without the scientific community. In the battle against the free will concept, Skinner has been a particularly outspoken leader, viewing it as a relic of the prescientific mode of thought, as a concept concealing and perpetuating ignorance of the variables of which behaviour is a function. The position that determinism occupies in Skinner's approach to the explanation of human behaviour is shown when he writes in Science and human behavior:

Science is more than the mere description of events as they occur. It is an attempt to discover order, to show that certain events stand in lawful relations to other events . . . .

But order is not only a possible end product; it is a working assumption which must be adopted at the very start. We cannot apply the methods of science to a subject matter which is assumed to move about capriciously. Science not only describes, it predicts. It deals not only with the past but with the future. Nor is prediction the last word: to the extent that relevant conditions can be altered, or otherwise controlled, the future can be controlled. If we are to use the methods of science in the field of human affairs, we must assume that behavior is lawful and determined. We must expect to discover that what a man does is the result of specifiable conditions and that once these conditions have been discovered, we can anticipate and to some extent determine his actions. (Skinner, 1953, p. 6)

If human behaviour does not exhibit cause-effect sequences, then the scientific method is essentially irrelevant to the explanation of the nature of man, and scientific psychology and the other social sciences are permanently barred from achieving the status they desire. To deny determinism in regard to human behaviour is to assert that significant lessons cannot be drawn from the past and that man's future is capricious and elusive. In contrast, the assumption that causal laws are discoverable in human behaviour leads to enormous possibilities in the social control of man's social environment; rules for managing social behaviour can be based on causal laws which tell us that if such and such is done it is likely that the outcome will be this and this.

#### Some Objections to Determinism

##### (1) The Heisenberg Principle of Indeterminacy

The Heisenberg Principle of Indeterminacy states that there are circumstances under which it is not possible to simultaneously determine the velocity and position of subatomic particles. The reason for this is that any system used to observe the particle interacts with the particle. More specifically, there is an exchange of energy between the observing system and the object. Hence, the very act of observation alters the behaviour of the particle. This experimental fact is supposed to provide scientific evidence of the reality of free will, of the undetermined nature of behavioural events. The Heisenberg Principle, however, does not necessarily lead to this conclusion. That certain events appear to be unpredictable does not entail that these events are free or capricious. In the field of human behaviour, there may be acts to which the principle of indeterminacy applies, but it does not follow that human behaviour is free. It is only that it is "beyond the range of a predictive or controlling science" (Skinner, 1953, p. 17). It may be argued that the principle of indeterminacy is a testimonial only to experimental imperfection. Uncertainty is, at the present stage of development, inherent in the methods of observation and measurement, and not in nature itself. That certain events are unpredictable now does not mean that this will always be the case. A future technology may be capable of creating a means of observation that can eliminate the energy interchange between the object and the system of observation. That such an event will eventually occur is predicted by "Clarke's Law":

When a distinguished but elderly scientist states that something is possible, he is almost invariably right. When he states that something is impossible, he is very probably wrong.

(End of first installment)

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