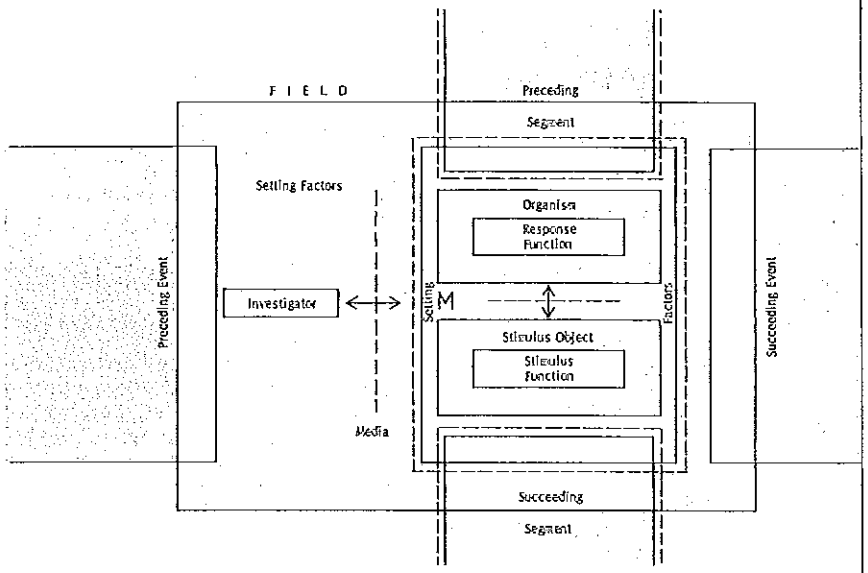
THE Interbehaviorist



A QUARTERLY NEWSLETTER OF INTERBEHAVIORAL PSYCHOLOGY

PUBLISHED AT HARTWICK COLLEGE
ONEONTA, NEW YORK

Volume 10, Number 1 Summer 1980

As the whole of conduct of life consists of things done, which do other things in their turn, just so our behaviour and its fruits are essentially one and continuous and persistent and unquenchable, . . .and so, among our innumerable acts, are not senseless separations. (1)

* * *

What is character but the determination of incident? . . .It is an incident for a woman to stand up with her hand resting on a table and look at you in a certain way. . .At the same time it is an expression of character. If you say you don't see it. . ., this is exactly what the artist. . .undertakes to show you. (2)

Henry James. (1) 1909. Preface to The Golden Bowl, Scribner's, 1909, p. xxiv. (2) 1900. The Art of the Novel, Penguin Classics, 1966, p.24.

THE INTERBEHAVIORIST

Editor:
Ronald G. Heyduk, Hartwick College

Associate Editors:
Donna M. Cone, Department of Mental Health, Retardation, and Hospitals, State of Rhode Island
Paul T. Mountjoy, Western Michigan University
Noel W. Smith, State University of New York, Plattsburgh

The Agora

We begin our tenth volume amidst a flurry of interbehaviorally-oriented papers presented at major national conventions. For example, Edward Morris of the University of Kansas sent a list of eight papers of interest to interbehaviorists that were read at this year's meeting of the Association for Behavior Analysis (only two such papers were read at the previous two meetings). The list is given below for those who may wish to send for reprints:

Delprato, D. Interbehavioral psychology as a palliative to the excesses of cognitionism.

Morris, E.K. The evolution of interbehavioral psychology and radical behaviorism.

Morris, E.K. Traits and situations: A behavior analysis.

Mountjoy, P.T. The interrelationship between interbehavioral psychology and radical behaviorism.

Parrott, L.J. Science and scientific philosophy.

Parrott, L.J. Radical- and interbehavioral perspectives on knowing.

Parrott, L.J. Beyond radical behaviorism: The interbehavioral alternative.

Verplanck, W.S. Talk: You can't get there from here.

* * *

Next week the forum for interbehaviorism will be at the Annual Convention of the American Psychological Association in Montreal, where Edward Morris has organized and will chair an interbehavioral symposium on Friday, September 5 at 1:00 p.m. in the Terrace Room of the Sheraton Mt. Royal:

Interbehavioral and Radical Behavioral Psychologies:
Is Behaviorism Reductionistic?

Paper 1. Edward K. Morris, Department of Human Development, University of Kansas, Lawrence, Kansas 66045.

Historical and Philosophical Evolution of Interbehaviorism and Radical Behaviorism.

Paper 2. Paul T. Mountjoy, Department of Psychology, Western Michigan University, Kalamazoo, Michigan 49008.

Conceptual Relationships between Interbehaviorism and Radical Behaviorism.

Paper 3. Dennis J. Delprato and Peter A. Holmes, Department of Psychology, Eastern Michigan University, Ypsilanti, Michigan 48197.

Interbehaviorism: A Non-Reductionistic Alternative to the Heredity-Environment Question.

Discussant: William S. Verplanck, Department of Psychology, University of Tennessee, Knoxville, Tennessee 37919.

Also regarding the upcoming convention, Dennis Delprato wrote:

"Interbehaviorists will be most encouraged if they attend a paper that will be presented at the upcoming APA convention by Thomas Z. Cassel, Ph.D., Plymouth Center for Human Development, Northville, MI 48167. The paper is entitled 'The Trajectory of Coupled Organisms: Reductionism vs. Systems of Constraint.' [Thursday 9/4, 2:00 p.m., Sheraton Mt. Royal, Quebec Room] The following views of Dr. Cassel show the congruity between his outlook and interbehaviorism:

'A notion central to the new currents in biology is that of coupled systems. Across all levels of biological organization emphasis is placed upon the manner in which a system is coupled to its surround. For example, it is both incorrect and misleading to state that DNA contains directive information. Outside of an appropriate aqueous medium, DNA macromolecules exhibit a denatured secondary sterochemical form. When placed in an appropriate aqueous medium, hydrophobic and hydrophilic couplings with the medium further twist the macromolecule into its active tertiary form. The medium and the coupling of DNA to it supplies essential configurational information.'

Dr. Cassel notes that the major paradigms for behavioral research are reductionistic in that they 'restrict their analysis to selected components of the overall behavioral

field, and seek to provide explanations by reduction to the isolated activities of these components. For example, behaviorism argues for a theoretical reductionism limited to isolated "behavioral" events emitted by an uncoupled entity. In addition, neither the morphological structure of the organism, nor the structure of its surround are given serious consideration. In a similar manner cognitive psychology argues for a theoretical reductionism to a subsystem of the organism (a cognitive system). As Flavell (1971) ingenuously notes, complex behavior is to be understood as "just a matter of the head doing its characteristic 'thing' ..., (p. 273)." This view represents nothing more than an infelicitous revival of sixteenth century reductionistic mentalism.'

"Dr. Cassel indicates that Kantor's writings did not influence his own because he did not find Kantor. After reading some of Kantor, he notes, 'clearly I must read more.' Dr. Cassel goes on, 'At the present, the only problem that I have is with the conceptual separation of the biological and the psychological, particularly the responsive-manipulative dichotomy. If we are dealing with complex performances across all levels, is this really a fruitful distinction?' "

* * *

Interbehaviorists planning to attend the upcoming A.P.A. Convention are encouraged to contact Associate Editor Noel W. Smith at his home or office by August 31, so that plans for a social gathering can be made. As an alternative, use the message board at A.P.A. to contact either Smith or Heyduk, and we shall inform you via return note of any plans that have materialized.

* * *

This issue's feature article is a book review by Paul T. Mountjoy of Daniel Robinson's An Intellectual History of Psychology. As The Interbehaviorist would seem to be a proper publication outlet for interbehaviorally-oriented book reviews, readers are encouraged to submit titles to the editor, who will request them from publishers.

* * *

A Review of Daniel N. Robinson's An Intellectual History of Psychology

Paul T. Mountjoy
Western Michigan University

Why add another to the plethora of texts concerning the history of psychology? Robinson's answer would be that the others fail to properly interpret that history. Make no mistake on this point: the interpretation presented is quite different from that found in the works which it is designed to replace. Is An Intellectual History destined to become a dominant influence in our field? My prediction (and my hope) is that it is not; yet the book is an expression of a significant movement in the Euro-American culture of which we historians are a subcultural group,

and consequently Robinson may impact significantly.

An Intellectual History of Psychology is divided into three major parts entitled Philosophical Psychology, From Philosophy to Psychology, and Scientific Psychology. Part One covers the period from the Ionian philosophers through the sixteenth century; Part Two includes the seventeenth and eighteenth centuries; Part Three discusses the nineteenth and twentieth centuries. Excluding the brief introductory chapter approximately 40% of the text is devoted to the earliest developments and about 30% is contained in each of the second and third parts. This distribution is commendable in that early, seminal developments are discussed in sufficient depth.

In what manner does Robinson depart from the usual scholarly interpretations of psychological history? The most significant deviations are his (1) insistence that psychology is not (and never can be) a science, and (2) emphasis upon the necessity of considering religious issues in understanding human behavior. Clearly he has performed a service by questioning conventional (and usually unquestioned) assumptions regarding the history of psychology as he developed these two interpretations. Anyone will profit from a careful reading of this literate and stimulating book, especially when disagreement concerning interpretation arises--as it most certainly will. I repeatedly found myself carrying on animated arguments with the author as I perused the text; that is, it stimulated me to become critical in turn, an important function for any work to perform.

Readers also will find many of Robinson's statements about the discipline of history to be compelling. For example:

> History is not simply a subject to be learned. It is a method by which we can attempt to explain ourselves and our world. Historical scholarship, properly conceived, is not an actuarial exercise leading to correct datings. It is misused when wielded in such a way as to populate a Hall of Fame (p. viii).

Given that Robinson raises stimulating questions, is undeniably literate, discusses early developments in sufficient depth, and correctly interprets the nature of the discipline of history, what aspects of the book lead to my overall negative reaction? In part, my reaction is a result of my belief that Robinson's scholarship could have been more careful and complete; but my major misgiving is that I believe that Robinson has made grievous errors in his interpretation of the nature and procedures of science.

Regarding Robinson's scholarship, as I read I drew up a list of 20 errors of fact and 23 errors of interpretation. Considerations of space and time constrain me to present three examples rather than an exhaustive list.

The statement is made that Frederick II"...experimented with falcons, proving their ability to locate prey by smell by blindfolding them...." (p. 160). The citation for this bit of nonsense is Haskins' Studies in the History of Medieval Science

(which is an excellent secondary source for Frederick II). For a moment I thought that Haskins had erred, but no--on p. 263, and again on p. 322 Haskins stated clearly that the Emperor sealed the eyes of vultures to determine whether they detected cadavers by sight or smell. The Emperor may well have been the greatest falconer of all times, and he knew full well from his widespread experience that the active avoidance of falcons by their live prey precluded any discovery by smell. Only vultures, which feed upon decaying cadavers, could possibly locate their food by smell--but, in fact, in his De Arte Venandi cum Avibus Frederick reports his conclusion that even vultures find their carrion food by vision and not by smell.

Page 185 contains this statement: "He [Leonardo] virtually discovered the geometric principles of perspective...." Actually it has long been known that in about 1425 the Florentine Brunelleschi painted according to the principles of linear perspective. Leonardo was not born until some 25 years after that event.

Page 293 reproduces an illustration from Descartes' L'Homme. In 1969 Pastore and Klibbe pointed out in the pages of The Journal of the History of the Behavioral Sciences that the diagram was not Descartes' and that it is actually misleading about Descartes' interpretation of vision.

This last example of Robinson's error-filled scholarship leads to an observation that may be of some significance. Robinson does not cite either The Journal of the History of the Behavioral Sciences or Isis, and for that matter, the sole citation of Boring's A History of Experimental Psychology is to a description of Breuer and Freud's concept of catharsis, which can hardly be regarded as definitive since the publication of Ellenberg's The Discovery of the Unconscious. Thus, Robinson's factual and interpretative foundation is weakened by his failure to exploit modern scholarship.

There are some specific cases of scholarly errors of omission worth noting. For example, Robinson's treatment of Fechner is limited to a half page (p. 330) which in spite of its brevity is essentially accurate. However, there is no mention of the motivation which guided Fechner's work in psychophysics despite the fact that Fechner himself is one of the best possible examples of Robinson's central thesis that scientists are pushed about by social forces rather than guided by their investigations. Fechner is treated as a scientist among scientists--but this is seriously misleading. Fechner adapted the scientific metaphor which Robinson inveighs against in order to support his theological notions. As Wundt expressed it:

> Das ganze Interesse Fechners gehort eben nicht der Psychologie als solcher an, sondern diese ist fur ihn nur ein Bestandtheil der Natur--und Religionsphilosophie. (Gustave Theodor Fechner: Rede zur Feir seines hundertjahrigen Geburtstages, p. 84).

Robinson also repeatedly reports (triumphantly) that behaviorism has only one law, the law of effect (p. 234, 241, 263-4, 404-5) and that is not really a law. Robinson should stop reading Skinner (his major example of behaviorism) and start reading The Journal of the Experimental Analysis of Behavior. Operant conditioning has progressed far beyond Skinner's notions. To be sure, the operant conditioners give Skinner a testimonial banquet each year but they then go ahead and do their own work. As an example of new behavioral laws I offer the matching law, first proposed in 1961 by Herrnstein. Perhaps it should be called a matching assumption, as it is usually properly perceived by operant conditioners to be a tentative proposal, one which will be rejected when it no longer predicts observations. Nevertheless, the so-called matching law does predict successfully that when organisms are placed on two concurrent variable interval schedules the ratio of response rates on those two schedules will equal the ratio of the reinforcement rates of the two schedules. In a sense the behavioral matching law resembles Ohm's Law (and the history of both is somewhat complex) since both generate families of theoretical curves that may be compared to empirical results. This sort of proposition is worthy of analysis by the historian of psychology.

As a final example of problems in scholarship, Sir Charles Bell is presented as the discoverer of the sensory and motor functions of the dorsal and ventral spinal roots and as a founder of physiological psychology, but no mention is made of Sir Charles' coauthorship of the Bridgewater Treatises, an eight volume set devoted to the demonstration of the "Power, Wisdom and Goodness of God, as manifested in the Creation." In this case, Robinson's omission is particularly unfortunate, for pointing out Sir Charles' association with the Bridgewater Treatises would have shown the oversimplicity of Robinson's presentation of the history of psychology as a battle between representatives of "good" Christianity and those of "evil" science.

Much more serious than Robinson's frequent lapses in scholarship are his misunderstandings of the terminology, methods, and purpose of the scientific enterprise that is the major villain of his text. As an example of terminology confusion, Robinson states:

> It is an undeniable law of physics that the mass and density of the human body are such, and that the viscosity of the earth's atmosphere is such, that no one will ever be able to escape the pull of gravity by beating his arms up and down (p. 410, italics added).

Robinson's confusion of a theorem with a law is a disservice to students of science. The laws of physics are mathematical expressions which summarize empirical observations. For example, Ohm's Law is in the form V=IR, where V is voltage, I is current, and R is resistance. Knowledge of two variables allows prediction of the other. More specifically: a conductor obeys Ohm's Law if the relationship V=IR is true for all voltages (i.e., resistance is independent of both voltage and current). Note the qualifying and limiting assumption: a far cry from flapping one's arms up and down.

A second gap in Robinson's understanding of science concerns hypothesis testing. The logical problems involved in accepting the null hypothesis are elementary enough to be included in

undergraduate curricula in psychology. Yet, on page 352 Robinson states: "The contemporary physicist does not devote his energies to the search for phlogiston because he has established that there is no such thing." This mistatement is not simply a momentary lapse since the same basic error reoccurs on page 357: "No experimental finding has made it clear that we lack a moral sense or a link with God or a love of beauty. I hesitate to state the obvious, and yet I must. Statements that deny the existence of an entity cannot be experimentally tested. It may be that some individual scientists have spoken carelessly (as I myself in the give and take of an argument), but the null hypothesis cannot be proven. That is, the existence of phlogiston, or the soul, or gods, cannot be disproven. Thus, when on page 279 Robinson states that "Science, after all, has not discovered that there is no God," he is stating the obvious, and is in no way providing support for the validity of theological concepts.

In the end, Robinson's attacks against science in the name of free will demonstrate a fundamental misunderstanding of the process whereby scientific propositions may be confirmed or disconfirmed. Science is, of course, concerned with the development of propositions that cannot be adequately evaluated except by the criterion of science itself. That criterion is the events which the propositions purport to describe. I submit that this criterion is involed by all human intellectual enterprises, but the scientist is set apart from other scholars by an exceptional faithfulness to the use of events as the ultimate criterion of the validity of his constructs. In fact, in no other field of scholarship has there been developed such a complex set of rules and procedures to insure that a description of events is accurate rather than misleading. A quotation best illustrates this point.

> Insofern sich die Satze der mathematik auf die Wirklichkeit beziehen sind sie nicht sicher, und insofern sie sicher sind, beziehen sie sich nicht auf die Wirklichkeit. (Einstein, Geometrie und Erfahrung, p. 3f.)

The point is that common sense is not a guide to the validity of scientific constructs. The notions of the man in the street that he has freedom of the will, as interesting and instructive as they may be, are of no greater value to science than the assertation of my garbage man the other day when we were having a philosophical discussion that "you gotta believe in something." The arbitrer of scientific validity is not the theologian nor the man in the street. Neither is the scientist the arbitrer of the validity of theological constructs--though he may sometimes act as if he were.

The free will versus determinism debate is a subtle trap into which Robinson blunders, thus following the lead of many of his contemporaries. That hoary debate has no place in a discussion of the nature of science since both free will and determinism are primarily theological doctrines. The debate in its modern form may be summarized by the St. Augustine-Pelagius controversy (in which Augustine held that grace was sufficient for salvation--free will being only an aid to grace--while Pelagius maintained that free will was necessary for salvation, and grace was only an aid to free will); and the Calvin-Arminius controversy (in

which Calvin argued that salvation was an absolute act of Jehovah's will--determinism--and Arminius proposed that salvation was a matter of grace). This brief summary should remind us all that both those who attack science in the name of religion because science is deterministic, and those who defend a doctrine of determinism in science are inheritors of a long tradition in Judeo-Christian culture. We should also remind ourselves that similar controversies have occurred in other religions as well.

Robinson did not invent the error of regarding determinism as a scientific assumption. He follows many scientists, among whom we must number Skinner. Historically speaking, what is the origen of the error? I suggest that scientists as a group have uncritically carried theological doctrines into science. I further propose that an historical analysis of the nature of the concept of causality itself would repay the effort expended. The simplistic nineteenth century conceptualization of a single cause and a single effect was dealt a death blow by the discovery of radio-activity and the development of quantum mechanics. However, the expansion of field concepts of causality from physics into biology and the behavioral sciences appears to me to have been an extraordinarily slow process. I, for one, would welcome a description of the cultural factors which have impeded the development of field theory especially in the behavioral sciences. That analysis would be significant to the historian of the behavioral sciences.

Of course, attacks such as Robinson's upon science in the name of religion are not new in history. Andrew White in his A History of the Warfare of Science with Theology documented the conflict and argued that such warfare was futile. This last quarter of the twentieth century is the locus of kaleidoscopic attacks by various sects upon science. These include the legislation of equal time for "creation" theories in public school science courses.

The history of science contains many conservative figures who attempted to proclaim the limits of scientific method. Frequently their proclaimations were motivated by an interest in preserving the authority of the church and preventing the spread of atheism. Yet there would seem to be little to fear: with the possible exception of Soviet Russia, and more recently the People's Republic of China, governments have continued to align themselves with conventional religious power structures and the great majority of all cultural populations have practiced some religious rites more or less regularly. Robinson is specifically concerned that behaviorism will have deleterious effects on religious behaviors. It appears to me that he has little to worry about. At the present time the majority of the population of the United States is religious, and a significant proportion is responsive to the most bizarre types of mysticism. For example, a recent book proclaiming that it explains how to harness the mystical power contained in the great pyramid is reported to have sold over 300,000 copies in English and to be in the process of translation into seven other languages. With an astrology column in almost every daily newspaper in these United States the historian need not seek far for evidence of a significant anti-scientific movement in our culture. In my opinion, it is unfortunate that An Intellectual History fits so neatly into that significant cultural

trend.

Robinson is misguided in his interpretations so often that I have been stimulated to compose several essays pointing out his errors. In my view that is the principle benefit of the book; it led me to examine and justify my own convictions. In this sense the book serves a very useful purpose--in contrast to the dreary parade of chronologies which have recently attempted to masquerade as histories of psychology. Read the book. I hope it will stimulate you to examine your own assumptions as it did me to examine mine.