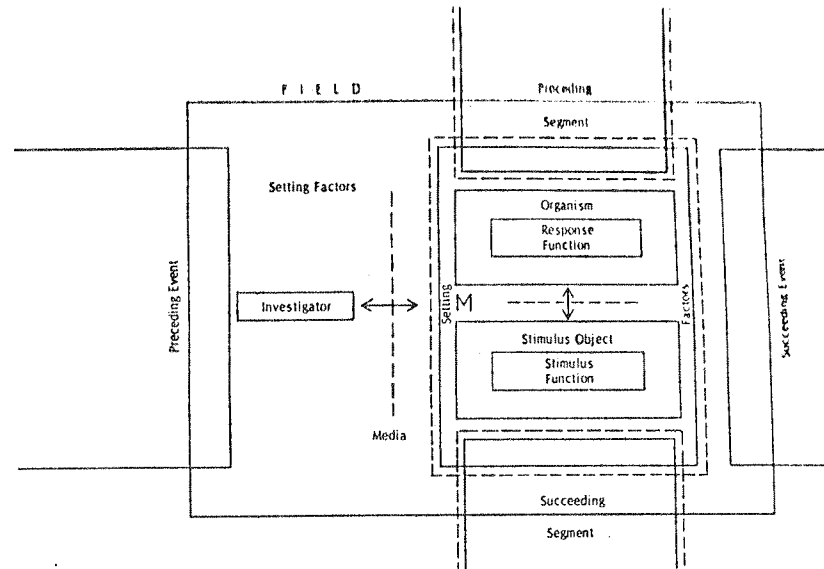


THE Interbehavioral Journal



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It is astonishing what havoc is wrought in psychology by admitting at the outset apparently innocent suppositions, that nevertheless contain a flaw. The bad consequences develop themselves later on, and are irremediable being woven through the whole texture of the work.

William James, 1890. The principles of psychology (Vol. 1). New York: Holt, p. 224.

THE INTERBEHAVIORIST

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The Agora

Dennis Delprato contributed the following comment and suggested reading:

If anyone was naive enough to assume that psychology had crossed the barrier of spiritism, a paper referred to by Grossberg (Comments about cognitive therapy and behavior therapy, Journal of Behavior Therapy and Experimental Psychiatry, 1981, 7, 25-33) will bring them back to earth. A. E. Bergin has published an article that could have come directly out of the Dark Ages in the Journal of Consulting and Clinical Psychology (Psychotherapy and Religious Values, 1980, 48, 95-105). The author calls to task those views of psychologists that "manifest a relative indifference to God, the relationship of human beings to God, and the possibility that spiritual factors influence behavior" (p. 98). Grossberg points out how cognitivism continues to foster spiritistic verbal behavior on the part of psychologists as seen in Bergin's statement that:

"The emergence of studies of consciousness and cognition, which grew out of disillusionment with mechanistic behaviorism and the growth of humanistic psychology, has set the stage for a new examination of the possibility that presently unobservable realities--namely, spiritual forces--are at work in human behavior." (p. 96)

Delprato also recommends an article by Stephen Wilcox and Stuart Katz (The ecological approach to development: an alternative to cognitivism, Journal of Experimental Child Psychology, 1981, 32, 247-263). The authors' criticisms of the cognitive approach and their recommended alternative should be appreciated by interbehaviorists.

* * *

The following note from Science, Volume 213, 31 July 1981, is reproduced without comment:

When Einstein died, on 18 April 1955, his body was cremated

but his brain, according to his specific direction, was removed to be used for research. It thereupon began a journey which, like the Flying Dutchman's, seems to have no clear end in sight.

The sage's cerebral remains disappeared for some years until Steven Levy, then a reporter with the New Jersey Monthly, set out to find what had become of them. He finally located the brain, or most of what was left of it, reposing in a Mason jar packed in a cardboard box marked COSTA CIDER, in an office in Wichita, Kansas (Science, 25 August 1978, p. 696).

The office belonged to Thomas S. Harvey, who had been entrusted with the brain as the pathologist at the Princeton Hospital where Einstein died. Harvey had had most of the brain sectioned and distributed to various specialists for study. He had not published any of the findings, as of August 1978, but hoped to do so in "perhaps a year."

Three years rolled by, the world has been presented with a work of science fiction, Einstein's Brain, by Mark Olshaker, but not with any scientific treatise on the neuroanatomy of the mind that shaped the foundations of modern physics. It seemed not unduly premature to inquire when the report would be ready.

Harvey has since moved from Wichita to the town of Weston, Missouri. He has not yet written up his study of the brain. He has no firm date for doing so. Asked what his article is likely to conclude, Harvey says he has "No concrete plans. I have my ideas about it but they have not solidified." The results from the specialists who studied sections of the brain show that everything is "perfectly within normal limits except for the changes due to age."

Harvey possesses "small fragments" of the brain but declines to say exactly where they are now stored. Einstein's estate, he says, has no interest in them.

* * *

In Volume 10, Number 3, Michael MacRoberts offered "A Challenge to the Interbehaviorists," to which the editor briefly replied in the same issue. Our disagreement centered on the issue of whether the energies of interbehaviorists would be better spent on "converting" our intellectual allies to the interbehavioral position, or on "building bridges" with them, even at the risk of losing our identity as interbehaviorists. MacRoberts' reply to my reaction follows:

I enjoyed your reply and really have little to add. I think it's fair and to the point. Our only difference is in the question of evolution versus revolution. I'm a revolutionist, you, an evolutionist, or better put, you would welcome a revolution but believe that things are going to change

more slowly. If this is what happens, let us work in such a way as to make it a rapid evolution: after all, a revolution is only a rapid evolution. As to building bridges, I don't think we disagree. I certainly don't advocate treating allies as enemies -- but my only question is, if we build bridges, who is it that is supposed to cross them?

* * *

Steven R. Brown, a political scientist who is a subscriber and occasional contributor, drew attention to his recently published book, which he suggests may be of "peripheral interest...especially the introductory and concluding chapters on operant subjectivity":

Brown, Steven R. Political subjectivity: applications of Q methodology in political science. New Haven: Yale University Press, 1980.

* * *

Robert W. Lundin of the University of the South, whose productive career as an interbehavioral psychologist is familiar to most of us, has recently been named to a prestigious chair, the William R. Kenan Professor of Psychology. The criteria were scholarship, research, and teaching ability. Congratulations!

* * *

Research Fellowship History of Psychology Foundation

The Research Fellowship funded by the History of Psychology Foundation, a private foundation affiliated with the University of Akron, promotes research in the history of psychology through the granting of stipends of up to \$750 to aid scholars wishing to utilize the primary resources of the Archives of the History of American Psychology, Bierce Library, University of Akron. The stipend is intended to help defray travel and living expenses and the recipient is expected to reside in Akron while using materials in the Archives.

Candidates should submit a prospectus of the work planned, a vita, and two letters of recommendation. It is particularly important that there is evidence that the Archives is the most suitable place for the research to be undertaken by the candidate. Advanced graduate students and recent doctorates are encouraged to apply.

Applications should be completed by March 1, 1982. The award will be announced not later than April 15th. Applications should be sent to the chairman of the University of Akron awards committee:

Mr. John V. Miller, Jr.
Director of Archival Services
University of Akron
Akron, Ohio 44325

* * *

Our feature article was contributed by "Commentator," an anonymous advocate of interbehaviorism. It looks at the "phenomenon" of Kantorian interbehaviorism from the somewhat detached but useful perspective of a science historian.

* * *

Priority and Interbehaviorism

The common occurrence of multiple independent discoveries in science has been the subject of special interest to historians and sociologists of science for two main reasons. First, multiples provide evidence that discoveries are context-dependent; that is, they occur when the time is right. If Darwin and Wallace had not independently hit on the idea of evolution by natural selection, someone else soon would have. Second, multiples often lead to priority disputes, which underlines strongly the importance of personal recognition in science. Darwin's agonies over being forestalled is evidence of this. The literature on multiple discoveries is quite extensive (see, for example, Ben-David & Sullivan, 1975; Gaston, 1973, 1978; Hagstrom, 1974; Merton, 1973).

The failure of scientists to recognize priority can be extremely irritating. Sorokin (1956) discusses this in his essay, "Amnesia and New Columbuses," in which he gives many examples of the "disease," as he calls it, wherein rediscoverers claim to be first, and asks why this occurs so frequently. He suggests two possibilities: it is due either to ignorance of previous work or it is due to the rediscoverer attempting to gain priority by knowingly ignoring previous work. While the latter occurs with some regularity, Sorokin believes that "the bulk [of cases] are probably due to ignorance."

Observer (1981) finds Kantor involved in a "priority dispute" in which it is evident that a rediscoverer or set of rediscoverers is clearly ignorant of previous work (see also Lichtenstein, 1980; Smith, 1980). In brief, sixty years ago Kantor began developing his "integrated field" ("ecological") approach and has been propounding it ever since (for a bibliography of Kantor's writings see Smith, 1976). But Kantor is not totally alone. Independently and over the same period, philosophers have been attempting to break from the thralldom of classical empiricism and to establish a realism, of which an attempt at a naturalistic theory of perception forms a central part (Brown, 1977; Hanson, 1958; Moore, 1922; Sellars, 1916; Woodbridge, 1965). Additionally, and again apparently independently, during the last two decades, psychologists have been moving rapidly toward a field perspective (see references in Observer,

1981). Certainly, the field or ecological perspective has been in the wind, with a series of discoverers or a discoverer and a series of rediscoverers. But the central figures seem to be largely unaware of each other.

What is of interest to us in this and other cases is why rediscoverers should be unaware of previous work. Kantor's situation, I think, provides a classic example of one type of case. Briefly, it would appear that Kantor is neglected by current researchers because he failed to receive attention from his contemporaries. And he failed to receive attention because he was (and is) ahead of his time. Because few scientists read anything that is more than a few years old (they apparently assume that anything of value from "ancient" times will have been assimilated in succeeding periods), Kantor's work has been effectively lost from the mainstream of psychology: or if it has been influential, it has been so without association with his name or school. For example, Observer points specifically to a 1920 paper and a 1933 book for Kantor's claim to priority regarding "ecological perception." But these two works, like all of Kantor's writings, have received almost no attention (see Smith, Note 1). Up to 1976, the 1920 paper has received only eight citations, of which five were in the early twenties, and the 1933 book has been cited only 54 times with citations peaking in the fifties. The vast majority of citations to this latter work are not to perception. Further, if we consider Kantor's entire production, up to 1976 (omitting self-citation) he has been cited in 626 works, an astonishingly low figure considering his high productivity (Smith, 1976; Note 1).

In this regard it is interesting that the three rediscoverers of particulate inheritance not only searched the literature but found Mendel's long-forgotten papers, and the only reason they were able to do so was that Mendel's papers were cited in Focke's 1881 *Die Pflanzen-Mischlinge* and in Bailey's 1892 article, "Cross breeding and hybrids." Kantor's work seems to have received less citation attention even than Mendel's, or put another way, Kantor's work has been lost in the literature deluge while Mendel's was not.

Other factors undoubtedly have contributed to the continued neglect of Kantor's work. One is that he is not always the clearest writer (Stevens & Stove, 1947), often leaving the reader dangling at the punch line. Another is that Kantor has not spelled out his system in a simple manner. He should have been in philosophy, not science, where the reading and writing customs are entirely different. One almost needs to begin with Kantor's earliest works and proceed to the present for his system to become evident. Contrast this with Skinner's *About Behaviorism*, which can be read by specialist and layman alike with complete comprehension. But behaviorism has been around for a long time, its terminology does not deviate substantially from common usage, and its ideas are not radically new, even when first put forward. The terminology of interbehaviorism, on the other hand, is as novel as its perspective,

and interbehaviorism appears "negative" because it trammels on sacred cultural and scientific beliefs, and this has not sat well with many psychologists. Little wonder, then, that Kantor finds himself in the unenviable position that Observer depicts.

What twenty or fifty years will bring is impossible to tell. Will Kantor emerge as a name to be reckoned with on a par with Skinner and Watson, will such luminaries ultimately be considered "mere schoolboys" to old Kantor, will psychologists simply rediscover piecemeal what Kantor has pioneered but not recognize his originality, or will psychology devolve into personalism and then into transcendentalism and the cycle repeat itself? It is all well and good for the handful of those who understand what Kantor has done to say in the pages of one quarterly publication with a relatively small circulation: "Wait a minute, Kantor said that in 1920." But will this send rediscoverers scurrying to the library where they will spend the next few years mastering the interbehavioral Canon? I doubt it. In my experience, even if they see Smith's review or Lichtenstein's paper or Observer's comments, at best they will, in future work, mention Kantor in a group citation with the one-liner: "Similar ideas have been developed by others (see Adams...Kantor...Zilch)." What reader of scientific literature is stimulated to explore the writings of those mentioned in group citations?

Whatever the fate of interbehaviorism, it will provide an interesting study for historians of science. While obviously no one is completely ahead of his time, some are enough ahead to cause themselves trouble, and Kantor is clearly a case in point. So far, he has been virtually ignored. If he is rediscovered *in toto* and not piecemeal, then the historian will be concerned with understanding that set of factors which prevented his emergence and then rediscovery. If, on the other hand, he is only reinvented piecemeal, the historians have another set of problems. What, for example, is the ultimate fate of a person's work if his contemporaries and immediate successors do not understand and/or cite him? This, of course involves a consideration of how scientists utilize scientific literature. Do they carefully comb the literature and abstracting services or do they pick up the latest "review" and consult its bibliography under the impression that the author has "pulled together" everything of value? Do scientists regularly study the work of the "ancients" or do they learn about the "ancients" from introductory textbooks, undergraduate courses, and traditional histories? DeVries, Correns, and Tschermak made a concerted effort to be thorough in their literature surveys, but has this practice changed in the last century?

The fate of interbehaviorism is in the balance, but with a little help from his friends, perhaps Kantor's work can become, as it should, something more than a group citation in psychology or an example of "the man ahead of his time" in the history of science.

Note

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