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The Journal of Mind and Behavior
Winter 1990, Volume 11, Number 1
Pages 105-110
ISSN 0271-0137

Paradigms in Behavior Therapy: Present and Promise. Daniel B. Fishman, Frederick Rotgers, and Cyril M. Franks (Editors). New York: Springer Publishing Company, 1988, 376 pages, \$42.95.

Reviewed by William O'Donohue, University of Maine

According to the editors the two purposes of this book were "to articulate the basic assumptions underlying modern behavior therapy within a philosophy-of-science context, and to sample and compare the views of systematically selected, prominent, exemplar behavior therapists with regard to the status of behavior therapy on various dimensions of the concept 'paradigm'" (p. 4). These goals are important for several reasons. First, the explication of the presuppositions of behavior therapy potentially can allow a deeper understanding of behavior therapy. Second, to the extent that criticism is essential to the growth of knowledge, such an explication can expose these buried assumptions to the light of criticism and perhaps aid in the growth of behavior therapy. Third, adopting a Kuhnian perspective is important because Kuhn's views have dominated meta-scientific analyses of psychology (Coleman and Salamon, 1988) and, if examined critically, such an analysis could provide important information regarding the value of the Kuhnian account of science.

The editors have certainly enlisted an impressive group of contributors (Krasner, Eysenck, Rachlin, Erwin, Kendall, Bacon, Woolfolk, Staats, and G.E. Schwartz) and have themselves published interesting meta-scientific analyses of various questions in behavior therapy. However, the major problem with this book is that the understanding of the views of Thomas Kuhn (1962, 1970a, 1970b, 1970c, 1974) is superficial. There is an indication of this in the quote above. For Kuhn certain instances of successful puzzle-solving are exemplars—not the individuals who solved the puzzles. "Exemplar behavior therapist" is an especially confused notion given Kuhn's emphasis on the community structure of science rather than on a "great person account."

Coleman and Salamon (1988) in a study of the psychological literature provided evidence that psychologists refer to Kuhn more than to any other philosopher of science and that these references are both favorable and superficial. Psychologists' obsession with Kuhn is unfortunate because it is not at all clear that Kuhn's views actually comprise a philosophy of science. Kuhn (1970a) has called himself an historian who examined the development of several of the physical sciences (and to a lesser extent the biological sciences). Thus, more precisely his views seem to concern the historiography of the physical sciences. Therefore, the task of this book involves an

extrapolation of Kuhn's views to a novel domain. The question becomes, to what extent are Kuhn's conclusions regarding the historical development of science, based on a study of some of the basic physical sciences, applicable to an applied social science, i.e., behavior therapy? That is, does the Kuhnian account adequately "save the phenomena" of the history of behavior therapy?

The question of extrapolation is not trivial for several reasons. First, even Kuhn thought that the social sciences differed on meta-scientific criteria from the physical sciences. In fact, Kuhn stated that he was led to the concept of paradigm when he compared the agreement over fundamentals in the physical sciences with the deep controversies endemic to the social sciences (Kuhn, 1970a, p. viii). Second, Kuhn thought that the applied sciences developed differently than the basic sciences (see below). Finally, Kuhn never tested his model by seeing how well it fit the history of the social sciences. However, this question of the generalizability of Kuhn's views unfortunately is not addressed or even recognized in this book.

Moreover, in this book criticisms of Kuhn's account of science are mentioned only in passing. This is particularly unfortunate given Gholson and Barker's (1985) excellent article which argues for the superiority of the philosophies of Lakatos (1970) and Laudan (1977) over Kuhn's for understanding the history of psychology. This article is mentioned only once in this entire book and, again, only in passing.

Below I will attempt to quickly summarize Kuhn's account of science and then I will briefly indicate what I see as the most glaring ambiguities in Kuhn's account of science. I will also summarize some of the criticisms that have been leveled against Kuhn. These are important to keep in mind when reading certain chapters in this book. Third, I will briefly examine Kuhn's views on the special status of applied science. Finally, I will discuss some implications of these points for the philosophy of behavior therapy.

Kuhn's Account of Science

According to Kuhn, a field is initially characterized by a lack of consensus concerning fundamentals such as what are legitimate problems, methods, and standards of solution. A field undergoes the transition from immaturity to maturity when a paradigm emerges that is capable both of providing a consensus about fundamentals and of supporting puzzle-solving. The field can now demonstrate cumulative progress.

However, paradigms also inevitably give rise to anomalies because some puzzles resist solution. At some point, often due to the press of anomalies, there is a loosening of normal procedures and a blurring of the paradigm. Sometimes a new paradigm is advanced that wins the allegiance of the community of scientists. Kuhn seems to state that the old and the revolutionary paradigm are incommensurable. This revolutionary paradigm then becomes the paradigm that allows normal, cumulative science to take place until the occurrence of the next revolution.

Major Ambiguities in Kuhn's Account

There are a number of points on which the Kuhnian account is unclear (O'Donohue, 1989). These include: (1) What is a paradigm?. Masterson (1970) found that Kuhn used this term in at least twenty-one different ways in *The Structure of Scientific Revolutions*. (2) When do revolutions take place? Kuhn states that there is no critical level of anomaly that will, or should, produce a revolution. Furthermore, he states that revolutions can occur due to other reasons, such as innovations in instrumentation in other fields.

(3) Are paradigms incommensurable? Kuhn often states that they are (e.g., Kuhn, 1970a, pp. 147-148). However, he appears to be inconsistent regarding this question in that he also states that paradigms can be compared on their problem-solving ability (Kuhn, 1970a, p. 153), and that they can be compared with each other and against the single standard of nature (Kuhn, 1970a, p. 77). Furthermore at times Kuhn states that science progresses. Progress seems to entail a favorable comparison of a more recent paradigm with an earlier one. (4) It is unclear what Kuhn regards as special about science or even if Kuhn regards science as special. Much of the philosophy of science has been an attempt to understand the seemingly unique growth of knowledge and problem-solving ability associated with science. It is not clear how Kuhn's views account for this special status. (5) Finally, it is not clear whether Kuhn's views are descriptive or normative. Kuhn takes them to be both (Kuhn, 1970c, p. 237). However, what regulative claims about science does his account support? If your field is immature, find a paradigm that can produce a consensus and that can solve puzzles? If your field is in a period of normal science and the level of anomaly is not too high, then continue to attempt to solve puzzles using the accepted paradigm? These seem to be little more than slightly jargonized platitudes.

Other Criticisms of Kuhn

These ambiguities are, of course, important criticisms of the Kuhnian account. One has the right to expect a writer to be clear and consistent. However, these are not the only criticisms that can be leveled against Kuhn. Given that most criticisms of Kuhn are not even mentioned in the book, they will be briefly summarized here (see O'Donohue, 1989).

Criticism 1: Kuhn's historiographical method is problematic. By what criteria did he choose the particular sample of historical episodes that he examined? By what method was this sample analyzed? Are his sample and method sufficient to support general conclusions about the development of all science?

Criticism 2: Some of his historical claims about the history of the physical science are false (Feyerabend, 1970).

Criticism 3: Although Kuhn claims that he has given a social psychology or sociology of science, in fact he has not. Williams (1970) claims that a proper analysis of the social structure of science has yet to be conducted.

Criticism 4: Science does not evolve through a progression of periods of normal science followed by revolutions. Rather, received views and dissenting views are always co-present (Lakatos, 1970; Laudan, 1977).

Criticism 5: Kuhn fails to provide an accurate demarcation criterion to distinguish science from nonscience. Feyerabend (1970) takes Kuhn's demarcation criterion to be puzzle-solving. However, Feyerabend suggests that if this is the demarcation criterion then organized criminals are scientists since they too solve puzzles.

Criticism 6: Kuhn exaggerates the agreement among scientists who hold a common paradigm. The degree of consensus he attributes to adherents of a paradigm is not found in science and would entail that all communities of scientists consist of only one individual.

Criticism 7: In taking paradigms to be incommensurable, Kuhn has exaggerated the difficulty of comparing two paradigms into an impossibility (Popper, 1970).

Criticism 8: Kuhn fails to give any account of the rational superiority of science over other methods and traditions.

Criticism 9: Kuhn's incommensurability thesis can be taken to entail that science cannot progress. However even Kuhn states that science does progress (see for example Kuhn, 1970b, p. 20).

Criticism 10: Kuhn apparently takes the narrow-minded and the dogmatic attitude of normal science to be an acceptable and even desirable state of affairs. Popper (1970) believes that this is a false normative view. According to Popper, scientists should be perpetually critical and should always attempt to falsify even their most cherished beliefs.

Criticism 11: Kuhn's view suffers from an internal contradiction. If the specification of problems and acceptable solutions are relative to a paradigm, then how can Kuhn's own views be taken to be true? Lakatos (1970) has called this the "scientific skeptics' dilemma": "[D]oes the popularity of Kuhn's philosophy indicate that people recognize its *truth*? In this case it would be refuted. Or does this popularity indicate that people regarded it as an attractive new fashion? In this case, it would be 'verified.' But would Kuhn like *this* verification?" (p. 115)

These criticisms and the above-mentioned ambiguities should be carefully considered before using the Kuhnian account in an attempt to understand a scientific discipline. Unfortunately, the editors of the book under review fail to do this. Thus, this book is constructed on shaky foundations.

The Special Status of Applied Science

Kuhn's view that applied sciences have a special status is also ignored in this book. For example, Kuhn (1970b) states:

Astrology was not a science . . . The parallels to an older medicine and to contemporary psychoanalysis are, I think, particularly close. In each of these fields shared theory was adequate only to establish the plausibility of the discipline and to provide a rationale for the various craft-rules which governed practice. These rules had proved their use in the past, but no practitioner supposed they were sufficient to prevent recurrent failure. A more articulated theory and more powerful rules were desired, but it would have been absurd to abandon a plausible and badly needed discipline with a tradition of success simply because these desiderata were not yet at hand. In their absence, however, neither the astrologer nor the doctor could do research. Though they had rules to apply, they had no puzzles to solve and therefore no science to practice. (pp. 8-9)

Kuhn apparently believes that the complexity of applied problems does not allow clear research questions to be formulated when puzzle-solving attempts have failed. The complexity of applied problems allows too many possibilities to plausibly account for failures at puzzle-solving, especially potential problems that are outside the applied scientist's control. Failures are therefore attributed to this complexity rather to either the investigator or to the paradigm.

According to Kuhn another reason for the special status of applied science is the lack of isolation of the applied scientific community from society (Kuhn 1970a). This lack of isolation results in research problems that are not given by what the paradigm seems most able to solve but rather by urgent social concerns. This also results in much slower progress in problem-solving. If there is to be an attempt to understand behavior therapy from a Kuhnian point of view, then his views concerning the special problems of an applied science should be considered.

Other Problems With the Book

Another troubling aspect of this book is the discrepancy between the type of psychology used by Kuhn and the type of psychology that is endorsed by the editors and chapter contributors. Kuhn states that he was influenced by Piaget, Gestalt psychology, and certain research in the psychology of perception (e.g., Bruner and Postman, 1949; Stratton, 1897). All of these are obviously not behavioral. Thus, there seems to be *prima facie* evidence that by adopting a Kuhnian account of the behavior of scientists and groups of scientists, these behaviorists have implicitly adopted an account of scientific behavior that is inconsistent with their general views concerning human behavior and proper ways to study behavior. Behavioral psychology is not consistent with the type of psychology espoused by Piaget or the Gestalt psychologists. These schools (paradigms?) have different ideas concerning what are the proper problems of psychology and what are the proper methods to study these. This incompatibility is not reduced one iota when the *explanandum* is the behavior of the scientist. For example, Kuhn relies heavily on the quasi-psychological concept of "Gestalt switch" and his physiological account of perception seems to rely on what Skinner would call "The Conceptual Nervous System." This is especially puzzling given that behaviorists have attempted to give behavioral accounts of science (e.g., Houts, 1988; Skinner, 1957). These behaviorists want to study *scientific* behavior with the same principles and methods which they use to study other behavior.

All of the preceding criticisms might be viewed as concerning subtleties of the Kuhnian account. However, this book also disappoints concerning what have to be the main features of the relationship between Kuhn's account and behavior therapy. Most of the authors seem to assume behavior therapy to be a mature science having a paradigm capable of successful puzzle-solving. However no argument is ever provided which supports this important claim. Obvious questions include: When did the transition from immaturity to maturity occur? What caused this? Have other revolutions occurred? What anomalies were associated with these revolutions? What are the exemplars of successful puzzle-solving for various communities of behavior therapists? What symbolic generalizations and models are held in common by various communities of scientists in behavior therapy? What are the current anomalies?

However, the larger question is whether the development of behavior therapy fits the pattern suggested by Kuhn. The editors, apparently without realizing it, even present evidence contrary to the notion that behavior therapy has developed according to the pattern suggested by Kuhn when in the last chapter they find that there is little consensus among their contributors concerning basic issues in behavior therapy. The editors also have failed to provide any evidence that there is a community of like-minded scientists within behavior therapy. This seems to imply three possibilities: (1) behavior therapy is an immature discipline because there is not agreement concerning fundamentals; (2) behavior therapy is multi-paradigmatic and there are communities of like-minded scientists (out there somewhere) that would rally around each of these points of view; or (3) the Kuhnian account of science is wrong, at least when applied to behavior therapy because it exaggerates the degree of consensus concerning fundamentals. An examination of these possibilities is not presented in the book.

Despite these problems, there are still some excellent chapters in the book. Rachlin provides a thought provoking and even entertaining review of molar behaviorism. This chapter should be read by anyone attempting to understand deficiencies in Skinner's account as well as by anyone wishing to gain a current understanding of contemporary behaviorism. Krasner's chapter is also praiseworthy. In it he provides some fascinating suggestions concerning the relationship between behaviorism and

the American world view. He also suggests that contemporary behavior therapy lost its paradigm when it went cognitive. Finally, Erwin's chapter is also worthy of praise. In this chapter he examines some of the epistemological assumptions underlying the cognitivist-behaviorist debate in behavior therapy. There are other worthwhile chapters in the book and the editors certainly have an impressive list of contributors.

In sum, this book is flawed in its rather uncritical and unfaithful reliance on the views of Thomas Kuhn. The ambiguities and problems of the Kuhnian account are not adequately explored. This is especially troublesome in that there seems to be a tendency for psychologists to be uncritical toward Kuhn and therefore to ignore other accounts of science. However, despite this fact there are some fascinating chapters in the book that are written by what are unquestionably some of the most influential behavior therapists.

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