

BEHAVIOURISM AND POSITIVISM

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The past few years have seen a mounting disillusionment with and rejection of behaviourism as a basis for psychology. This mounting disillusionment manifests itself in many different forms: the publication of increasingly trenchant conceptual and methodological critiques of behaviourism (11, 23, 31, 42), the intensification of philosophical debate on behaviourism (e.g., 46), the attrition in the ranks of both well-known and unknown behaviourists (e.g., Sigmund Koch, George Miller, O. H. Mowrer, D. S. Palermo), the strong and vigorous development of the so-called "third force in Psychology" of Rogers, Maslow, and others (26, 33, 34), and perhaps most importantly, a gradual change in the kind of articles published in the hitherto mainstream behaviouristic journals.¹

Behaviouristically oriented psychologists have, in some cases, been quick to sense and respond to the attack. Such works as Kantor's *Scientific evolution of psychology* (16), Esper's *A history of psychology* (9), Skinner's *Contingencies of reinforcement: A theoretical analysis* (39), and Smith's *Behavior and conscious experience* (40) all attempt to show that behaviourism, or something very like behaviourism, is the only possible scientific orientation for psychology, and that it can quite robustly serve as a guide to any and all psychological phenomena. But these works, too, are evidence in themselves of the decline of behaviourism. They all are defensive works, and they all promote brands of "behaviourism" which are nearly unrecognizable. Skinner's radical behaviourist credo (39), reprinted in part from an earlier work (38), was originally described as "so extraordinarily libertarian . . . that one begins to wonder what the actual defining characteristics of the behaviourist thesis or the behaviourist method might be in his particular case (S. Koch, quoted in 46, p. 98)." The other three books promote various forms of closer relationship between psychology and biology, in the avowed hope that such closer connections will cleanse behaviourism (and psychology) of the last traces of psychic fictions. It is taken as almost an article of faith that a new and indomitable behaviourism will emerge from this process.

The question arises: if behaviourism is surrendering its hegemony, however unwillingly, how has it come to be forced to do so? Palermo (30) suggests that the downfall of behaviourism began in earnest with a dissertation by Kuenne (20), in which she showed that, for any behaviourist theory, there were unaccountable differences in verbal transposition behaviour between younger and older children. Palermo's claim at first appears unfounded; accommodation to more anomalous phenomena than Kuenne's had cheerfully been made within the behaviourist framework before Kuenne published.

¹In a discussion of a symposium on imagery in children's learning recently published in the *Psychological Bulletin* (30), D. S. Palermo observes "Some 15 years ago, when I was a year from completing my graduate work, . . . proposing a symposium on imagery at a psychological convention might have been considered a joke. Most hard-nosed experimental psychologists probably would not even have set aside their copies of *Modern learning theory* . . . long enough to notice such a symposium."

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Palermo's claim makes more sense, however, in the light of the general historical analysis he provides. His article is of interest as one of the first explicitly "post-Kuhnian" analyses in psychology. T. S. Kuhn's theory of scientific progress (21) is by now well known; "normal" or everyday scientific activity in a given field is guided by a paradigm, or outstanding scientific achievement, which implicitly defines the methodology, conceptual structure, and problem areas of scientific research. This paradigm-based research is devoted to solving specific problems related to, and following from, the paradigm. Eventually one or more research problems of central importance to the paradigm proves intractable to paradigm-based research. These "anomalies" provoke a "crisis-state" in research; unsatisfactory attempts are made to resolve the anomalies by means of *ad hoc* additions to the paradigm-based theory, the paradigm comes to be severely questioned, and research proceeds on a relatively undirected basis for a time. Finally the anomaly is solved, at least in part by a non-paradigm-based piece of research; this achievement, or one following from it, forms the basis for the next paradigm.

Palermo has accepted Kuhn's analysis and applied it to psychology. Behaviourism is a paradigm-based research programme with classical conditioning (or, later, S-R conceptualizations) as its paradigmatic heart. Research was stimulated and guided by this paradigm into the familiar channels of S-R learning theories, drive-reduction theories of motivation, mechanical "mediating response" theories of memory and cognition, etc. The frontiers of behaviourist experimentation and theorizing were in the fields of complex human functioning, and ambitious attempts to extend behaviouristic theories to these fields were made by such pioneers as Miller and Dollard (8, 27), Rotter (36), Bandura and Walters (1), and Staats and Staats (41). Predictably, it was in these frontier areas that major experimental anomalies began to appear. The first was that of Kuenne (20), but Palermo goes on to specify others coming from the work of Harlow on curiosity and love (13, 14), Rock and Estes on one trial learning (10, 32), Olds on brain stimulation (29), etc. The work of Chomsky (e.g., 7) marks, according to Palermo, the first signs of the emergence of a new paradigm, one characterized by a "mentalistic and rationalistic orientation (30, p. 416)."

Palermo's analysis is simple, rational, and consistent with a growing trend to view the succession of scientific hegemonies as determined more by factors relating to the sociology of science than by those relating to scientific activity itself. The selection of Kuenne's dissertation as the beginning of the end for behaviourism makes more sense when viewed from the standpoint of Palermo's extension of Kuhn's theory. Kuenne was working within the behaviourist framework, at a behaviourist laboratory, and should have found good behaviourist answers. Instead, ". . . older children did not transpose in the same manner as the younger children. The latter behaved in much the same way as rats, as expected from the theory current at the time, but there was something peculiar and theoretically difficult to handle about the older children (30, p. 416)." If behaviourism is, in Kuhn's sense, a paradigm, then Kuenne's findings must be, in Kuhn's sense, an anomaly. The identification of Kuenne's work as a serious anomaly, that is, as the starting point of the crisis, cannot be made until after the crisis is in full progress of course, but this limitation on identification is customary.

THE CONTENT OF PARADIGMS

Is behaviourism, or the foundation of behaviourism, most appropriately viewed as a paradigm? At first sight it would seem so. Thorndike's puzzle-box studies and Pavlov's classical conditioning experiments appear to fit Kuhn's description of a paradigm as "one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice (21, p. 10)." They seem, furthermore, to have two characteristics Kuhn considers essential in a paradigm. "Their achievement was sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity. Simultaneously, it was sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to resolve (21, p. 10)."

Still, there is a fundamental difference in scientific status between Thorndike's and Pavlov's researches, and the research achievements that Kuhn provides as examples of paradigms. The difference has to do with the relative importance of the substantive and the methodological components of the scientific achievements. The significance of the paradigm as a scientific achievement is that it solves a problem: it answers particular questions about the world and is, primarily, a major substantive contribution to its field. Matters of methodology, conceptual orientation, and the definition of important problem areas are determined implicitly by the paradigm. Whether the proponents of the new paradigm attempt to legislate methodology or not, it is the paradigm itself that both stimulates and justifies any methodological development.

The paradigm, then, is first and foremost, a major substantive scientific achievement. By a process of implication and even of direct modelling, the paradigm has a major effect on the entire structure of the field. But each effect it has is justified by reference to the substantive significance of the paradigm achievement, a significance which necessarily antedates the effects justified by it. Paradigms, that is, are defined by their content, not by their methodology. This point deserves some emphasis, because it does not describe the way behaviourism developed.

It is questionable whether there was a crisis in psychological research when Thorndike published "Animal intelligence" in 1898 (43). There certainly was one by the time Watson published "Psychology as the behaviorist views it" in 1913 (47), and his adoption of Pavlovian conditioning principles in 1916 (48) was specifically intended to resolve this crisis. But neither Thorndike's researches, nor Pavlov's as Watson used them, answered any major questions or solved any substantive problems in psychology. Instead, they proclaimed that human and animal functioning *could* be understood in a particular way, and promised that the use of a correct methodology *would* make psychology into a genuine science. Thorndike's and Pavlov's results were never of principal importance (indeed, in the case of Pavlov, they have always been difficult to replicate); it was their techniques, and the principles that followed from their techniques, that became central.

The revolution that produced behaviourism was, in short, a methodological revolution. Behaviourism was not born from a solution, even a tentative solution, to a major problem. It was born rather of an uncompromising faith in a particular objective methodology, a faith that (as is well known) required the rejection and denial of those phenomena and foci of research which could not be made compatible with the methodology.

Legislation thus played a rather larger role in the development of behaviouristic psychology than in the development of those sciences it tried to imitate. There was no outstanding achievement to refer back to as proof of the worth of the behaviourist approach, and as a source of methodological principles. The methodology itself was the starting point, and was justified only by appeal to the future and to other "objective" sciences.

The same factors that made legislation necessary, however, also made it ineffective. The hegemony of behaviourism has always been rather loose, as well as geographically isolated. Debate over fundamentals was never resolved. The lack of justification for the extreme methodological tenets of Watson forced behaviourism to become "neo-" very swiftly. Tolman's first exposition of "purposive behaviourism"—in effect, teleological behaviourism—appeared in 1922 (44), just six years after Watson began to promote Pavlovian conditioning principles. Tolman's *Purposive behavior in animals and men* (45), ten years later, went most of the way towards introducing mentalism. In the same year, Cannon's *The wisdom of the body* (6) climaxed his refutation of Watson's peripheralism. Environmentalism is often thought to have remained central to behaviouristic formulations somewhat longer. Even so, Hull in his *Principles of behavior* (15) was forced to list a dozen autonomous drives, and to imply that his list might well be incomplete. A list of primary drives that encompasses hunger, thirst, sex, maternal drive, and drives resulting from needs for air, to avoid pain, to maintain body temperature, to defecate, micturate, rest, sleep, and be active (15, pp. 59-60), treads perilously close to instinctivism.

Environmentalism, peripheralism, the rejection of teleological explanation and mentalism—all these supposed fundamentals of behaviourism were abandoned or greatly modified long before behaviourism passed its heyday. Rejection of them took place within the behaviourist tradition, but on a piecemeal and *ad hoc* basis. They were abandoned, not through a process of growth and development of the behaviourist paradigm, but through a reluctant and gradual response to the inadequacy of a methodology that had never had significant substantive justification.

In summary, behaviourism was not in anything like Kuhn's sense, a paradigm. It did not have a paradigmatic base, and it did not have the power to settle fundamental issues that is essential for the practice of paradigm-based normal science.

THE FUNCTION AND DYSFUNCTION OF POSITIVISM IN THE DEVELOPMENT OF SCIENTIFIC SYSTEMS

If behaviourism was not based on a paradigm, then what was it based on, and how did it come to its predominant status? It is widely recognized that behaviourism did not at first succeed entirely on its own merits. It was "one of those ideas that are blessed at birth by the zeitgeist (2, p. 32)." "Psychology was all ready for behaviourism . . . the times were ripe for more objectivity in psychology, and Watson was the agent of the times (4, p. 642)." The essence of behaviourism was, as implied above, its adoption of objective methods and orientations avowedly analogous to those of the physical sciences. And those orientations, at the time, emphasized mathematical and provisional nature of physical constructs; under the spur of the collapse of the Newtonian world-view they rejected any reference of constructs to a "metaphysical" underlying reality. The orientation of physics at the time was,

in a word, highly positivistic, and it was the adoption of this positivism, which was becoming an immensely popular and influential philosophical and scientific movement, that greatly helped ensure behaviourism's success.

It was this adoption of positivism that gave behaviourism, not just its emphasis on observables, but its rejection of anything purporting to lie behind the observables. At first the fact that it was positivism that psychology was adopting from physics was little recognized; following the introduction of Bridgman's operationalism (5) the adoption of positivism became conscious and systematic. Operationalism has always been taken more seriously in psychology than in physics. It was of great assistance in the development of neo-behaviourism, since it seemed to permit, while keeping staunchly within an objective and positivist framework, the introduction of any level of concept so long as the concept could be "operationally defined". Both the success and the characteristics of the behaviourist programme are thus largely attributable to the acceptance within psychology of a natural-science-based positivism. Positivism in psychology, however, is still something very different from positivism in physics, and this difference is of central importance to an understanding of behaviourism.

The development of positivism as an internal process within a scientific discipline is a cyclical historical phenomenon with a determinable social function (cf. 19). Its central characteristic is a systematic suspension of judgment concerning the reality of a particular explanatory system. Its function seems to be that of easing the transition from one explanatory system to another which replaces it. Positivism may arise when an old system, which is accepted and believed, comes to have its validity questioned; it makes possible the response to criticism that the system under attack is justified as a scientific system by its pragmatic success (by successfully predicting phenomena) rather than by its metaphysical success (by accurately reflecting reality). Positivism flowers when a new system is proposed to replace the old one; the resistance to accepting a new explanatory schema with potentially revolutionary metaphysical implications is at least as great as the reluctance to abandon the old one. Whether the proponents of the new system are positivistically oriented or not seems to make little difference. The claim comes to be made for the new system that it, too, is justified pragmatically,² that it is a mistake to see it as requiring a change in world-view. As the new system becomes accepted and commonplace, its positivism is tacitly and gradually abandoned; its principles for describing and accounting for phenomena come to be taken as descriptions of real processes. The positivistic approach to a system may never entirely die out, even after the system has become widely accepted, and is always considered a respectable, if somewhat overly cautious, scientific attitude. Its incidence in science, however, is very low during the period that the system is fully accepted. Positivism has philosophical roots, of course, in skeptical epistemology, and may continue to stimulate activity in philosophy throughout the "realistic" period of acceptance of a scientific system.

Examples of positivism are readily available in the history of science. Both the Eudoxian (heavenly spheres) and the Ptolemaic (equants and epicycles) theories

²From the standpoint of Kuhn's theory, such a claim must always be unjustified. The old theory, at the time of its death, is always able to do more than the new theory at the time of its birth.

of astronomy were proposed simply as mathematical devices to "save the phenomena"—to enable prediction of the observed motions of the planets without regard to their hypothetical actual behaviour. This early positivism was given rational support by the argument that it was impossible to make astronomy a natural science since its objects were totally inaccessible to close observation and experiment; therefore, any statements concerning the "real" nature of the stars and their motions could not even in principle be justified. Nevertheless, an amalgam of the Eudoxian and the Ptolemaic theories gradually came to be considered true, rather than merely efficient. Copernicus, in his turn, believed in his heliocentric hypothesis, but much of the early defense of his system emphasized a positivistic justification similar to the ancient one (35). Newton was never satisfied with the account he gave of gravitation, and cautioned his readers to consider his formulation as merely a mathematical description. They did not do so, and gravity slowly became accepted as a fundamental property of matter.

The emergence of positivism towards the end of the nineteenth century displays a similar pattern. A few physicists, such as Ostwald and Priestley, had resolutely kept to a positivist conception of Dalton's atomic theory. In general, however, although the scientific temper of the period was aggressively tough-minded, it was a tough-mindedness of materialism rather than of positivism. A thoroughgoing positivism started to be widely acceptable only with the publication of Mach's *Science of mechanics* (24) in 1883 (Eng. tr. 1893). The acceptance of Mach's positivism and of the doctrines that followed from it was based on the gradual buildup of anomalies, and the gradual loss of faith, in the Newtonian scientific system. The theories—quantum mechanics and relativity theory—that followed from all these anomalies have continued frequently to be presented as positivistically based theories (although Einstein's own distaste for positivism is well known). Koyré, however, confidently predicts that the "positivist phase of renouncement" will once again give way to a realism (19). There are indications that he is right, that physicists are increasingly coming to assign greater realistic significance to their postulates (22, 28).

Psychological positivism, then, was adopted from a science wherein it currently appears as a symptom and concomitant of revolutionary change. But the revolutionary change is always from one world-system that has become inadequate to another developed to redress the inadequacies. In behaviouristic psychology this could have happened but did not. The Wundtian, elementaristic, associationistic, paradigm was proving inadequate and a Darwinian functionalist paradigm for a time seemed to be the replacement. However, the Darwinian emphasis on unconscious wellsprings of behaviour and on instinctual mechanisms was unacceptable to the dogmatic objectivism of developing behaviourism. Positivism lost, in its transition from physics, its function of masking and rationalizing underlying entities, and served instead to abolish them. The Darwinian paradigm was instead exploited by Freud and, much later, by the ethologists. The Darwinian approach did not disappear altogether in America of course, but was used only by those, such as McDougall and Yerkes, who eventually came to reject the behaviourist orthodoxy. Darwinism in American psychology thus never achieved the status of a paradigm and never realized its potential.

The mainstream of American psychology thus placed itself in the curious

position of adopting a methodology appropriate to paradigm shifts, while the manner of adoption entailed having no paradigm to shift to. This is the failure of behaviourism, that it had no world-view to grow up into, to guide research, to provide substance to its orthodoxy; it restricted itself instead to a methodology which is productive only when there is a world-view beneath it, waiting to emerge.

This is not to say that behaviourism was entirely without underlying, guiding, metaphysical principles. It had them, and they were the same elementaristic, associationistic ones derived from British empiricism as were present in the rejected Wundtian paradigm. However, the possible implications of these principles for a new model of the nature of psychological processes could not be realized, because they had become so widespread, vague, and generally disseminated throughout the scientific culture that their substance had been reduced to the general methodological maxim of "analyze everything into its components." This reduction of the metaphysical principles to the level of methodological maxims was reinforced, of course, by the growing positivism of the scientific culture. In addition, the kind of philosophical positivism which was starting to develop at the time, and which eventually became the logical positivism of the early Vienna circle, was itself inclined strongly towards the same kind of empiricist elementarism (3). The principles did influence much behaviourist research, but they were not essential to behaviourism, and could be tacitly abandoned when necessary, as the elementarism was abandoned in Tolman's theory. They served to give the appearance of content to the positivist orthodoxy, but they expressed no new principles or insights, and were never sufficiently strong or stimulating to initiate an emergence from the positivism.

POSITIVISM AS SCIENTIFIC ORTHODOXY

Serious consequences arise when positivism is institutionalized as the assumed content of a discipline. The development of the discipline is restricted, since the positivist orthodoxy, like all orthodoxies, resists change that is anything but a development of itself. Since positivism is substantively empty, a positivist orthodoxy resists any genuine development. It has two important defenses against change.

First, the orthodoxy has an effective criticism available to counter any proposed world-view. By the tenets of positivism, which are taken to be the tenets of science, any world-view is meaningless (unverifiable), and hence unscientific. This criticism of any non-positivistic position, that it is unscientific, is simple to apply and is sufficient to invalidate any such position in the judgment of all those who accept the orthodoxy.

Second, since a positivist orthodoxy has no substantive core, it is not falsifiable; that is, any empirically verifiable statement is consistent with it. The orthodoxy is thus totally pluralistic, and any finding that seems anomalous to a theory consistent with the orthodoxy can in fact be accommodated within it.

The combination of these two defense mechanisms provides a devastating defense against any scientific system that challenges the orthodoxy: the empirical findings of the challenging system can be accommodated within the orthodoxy²,

²It is very difficult for any position to be entirely content-free. In practice, therefore, an additional technique is employed of ignoring empirical findings that cannot be assimilated.

and the theoretical formulations, to the extent that they are not compatible with a positivistic orientation, can be dismissed as unscientific.

These defensive reactions are familiar occurrences within behaviourism. They are the basis for much of the behaviourist criticism of psychoanalytic theory (e.g., 37); the attempt of Dollard and Miller (8) to translate Freudian theory into S-R terms was explicitly designed so as to accommodate the range of Freudian findings within a behaviourist framework while rejecting the unique components of Freudian theory as unscientific. Maltzman provides an explicit version of this defense, or counter-attack, in discussing behaviourist vs. cognitive (i.e., mentalistic) treatments of awareness in verbal conditioning.

No cognitive psychologist has made any discoveries, obtained any empirical laws, uncovered new experimental variables which logically could not be treated within some behaviourist theory. . . . The difference between the cognitive psychologist and the behaviourist is in the logical status of their respective theories. For the behaviourist awareness is a defined concept. For the cognitive psychologist awareness is a primitive or undefined term, despite disclaimers about operational definitions and converging operations. . . . Psychology can manage without such a *ding an sich* (25, p. 162-3).

In this double-barreled, almost sophistic technique of reply to criticism, behaviourism is rather similar to its philosophical cousin linguistic philosophy, which could be identified as another modern case of positivism institutionalized as the pseudo-content of a discipline (cf. 12). It is the imperviousness to substantive criticism of behaviourism that gives significance to the remark of Koch: "I suspect that there is a class of positions that are wrong but not refutable and that behaviourism may be in such a class (17, p. 6)." Koch relates his remark separately to the metaphysical and to the methodological aspects of behaviourism. The analysis presented above, however, implies that the methodological and metaphysical components cannot be considered separately, or even be properly identified apart from each other, for behaviourism has succeeded in making its methodology into a kind of metaphysic.

The resistance of behaviourism to change and criticism has not, of course, made change impossible or criticism ineffective. What it has done is to delay change, and for a time to force criticism and change to be piecemeal. It has strongly hindered the development of a new paradigm. Its techniques for doing so, from the defensive viewpoint, were described above. From the viewpoint of the initiators of change, it has also done so by providing little specifically to react against. Behaviourism's anomalies are not Kuhn's anomalies; *ad hoc* additions designed to counter the anomalies are very difficult to distinguish from apparently genuine extensions of theory. Behaviourism has thus not only failed to provide an adequate systematization within psychology. It has even failed to present central problems for the attention of a possible successor.

The decline of behaviourism is not being ushered in by the growth of anomalies, by the appearance of a new paradigm that gives the anomalies a more central position.⁴ Instead, a growing awareness of the emptiness of the behaviourist pro-

⁴As was the case, for instance, in the development of gestalt psychology out of structuralism.

gramme is giving rise within the behaviourist ranks to a feeling of weariness and disillusionment. At the same time, the tentative programmes instituted by those working independently of the behaviourist orthodoxy are beginning to bear fruit and to attract more and more workers. Common to behaviourists and others is the feeling that behaviourism has simply failed to make good its promise.

It seems necessary, therefore, for American psychology to revert to an explicitly pre-paradigm position for a time. Behaviourism, during the period of its domination, prevented the development of a paradigm, and none of the recent competing schools is yet in a position to advance one. The conclusion of Koch (18), that the unity and scientific coherence produced by adherence to a paradigm are impossible within psychology, is obviously premature. His conclusion is based on the assumption that behaviourism, despite its fundamental inadequacies, is a scientific system, and that the failure of behaviourism to become systematically viable must therefore preclude scientific coherence and unification on the part of any successor. In contrast, the analysis presented here concludes that behaviourism is not a scientific system and has hitherto prevented one from developing.

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