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PROBLEMATIC PROGRESS: A REVIEW OF LAUDAN'S PROGRESS AND ITS PROBLEMS *AND* SCIENCE AND VALUES[1]

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The situation in psychology is troubling. Skinner, reflecting on recent trends, has declared, "Psychology as a science is, in fact, in shambles" (1983, p. 9). Concern with recent historical developments in psychology has been amply evident in many recent reviews in the *Journal of the Experimental Analysis of Behavior* that have explored historical and conceptual aspects of behavior analysis and examined relations to other perspectives. Collectively, the many strands of these reviews reflect a profound—albeit often indirect—effort to come to grips with the current fortunes of behaviorism. We believe the work of Laudan, a contemporary philosopher of science, can provide a unifying framework for directly addressing these issues and pulling together many of the distinct strands of the historical concerns of behavior analysts. Two aspects of Laudan's philosophy of science are of particular interest in this regard.

First, Laudan, along with other post-Kuhnian philosophers of science, provides a critique of the Kuhnians' account of scientific change and presents a "gradualist's" alternative to their revolutionary model. Like the Kuhnians, Laudan recognizes that conceptual issues play a particularly important role in the competition among broad theoretical perspectives, and he shares their concern with the role of such competition in historical developments in science. Thus, Laudan's account, like that of the Kuhnians, can provide a framework for examining the controversies between behavior analysts and their cognitivist or ethological critics. But Laudan strongly rejects the Kuhnians' contention that the meta-empirical assumptions that constitute a worldview are impervious to criticism from an alternative perspective. In particular, he argues that empirical considerations often play a role in debates over meta-empirical assumptions; therefore, meta-empirical differences can be meaningfully debated. Hence Laudan finds a role for meta-empirical factors while avoiding the scientific irrationalism of the Kuhnians and, in so doing, avoids the bleak implications of this irrationalism for behavior analysts concerned with the present state of psychology. Laudan's argument is in part historical. He claims the history of science is the history of the development of controversies and the emergence of consensus at both the empirical and meta-empirical levels. We will discuss this aspect of Laudan's position in the next section in which his *Science and Values* (1984) will be reviewed.

Laudan's criticism of the Kuhnians' revolutionary models of scientific change and his gradualist's alternative are characteristic of post-Kuhnian philosophy of science. The second reason why Laudan might be of interest to behavior analysts is that he provides a particularly broad and detailed framework for describing the conduct of science. In *Progress and Its Problems* (1977), Laudan develops a descriptive taxonomy with the resources for deal-

[1] Laudan, L. (1977). *Progress and its problems: Toward a theory of scientific growth.* Berkeley, CA: University of California Press; Laudan, L. (1984). *Science and values: The aims of science and their role in scientific debate.* Berkeley, CA: University of California Press.

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ing with the range of empirical, methodological, and evaluative issues that enter into behavior analysis and alternative perspectives. Competition is central to Laudan's approach to science; he stresses the importance of comparative evaluations in scientific change. The positivists' concern with formal analyses led them to focus on single theories taken in isolation. To the limited extent that they were concerned with comparative evaluations, the evaluating criteria were limited to the range of concerns that originated in the attempts to deal with single theories. Laudan's approach is the reverse; his apparatus for dealing with individual theories is largely determined by the issues that emerge from analyzing scientists' practices in comparatively evaluating theoretical alternatives. This stress on comparative evaluation plays a significant role in Laudan's description of the role of meta-empirical factors and strongly influences his descriptions of the role of empirical problems. The resulting taxonomy, Laudan claims, more accurately reflects the findings of the history of science. We suggest the result is a more realistic account of the conduct of science than that found in many formal models, and behavior analysts will recognize elements of their scientific behavior. Laudan's taxonomy will be presented in a review of *Progress and Its Problems* below.

Laudan's work provides an introduction to recent philosophy of science; his position broadly typifies the themes and techniques characteristic of the approaches of philosophers of science during the past decade. Because Laudan often uses earlier philosophies of science as foils in presenting his own position, a preliminary sketch of the major historical developments in 20th century philosophy of science will be a useful background for discussing the details of Laudan's views.

THE STRUCTURE OF SCIENTIFIC REFORMATIONS

The main thrust of the positivist program was to "rationally deconstruct" the role of empirical evidence in science by developing formal models of the logical structure of scientific theories in which observation sentences provided the foundation. Beset by internal problems and criticized from without (Kuhn, 1962), the program collapsed in the 1960s. The reaction that followed has been called one of "epistemological nihilism" (Quine, 1969). The postpositivists (Kuhnians), attempting to correct the failure of the positivists to attend to the actual conduct of science, laid great emphasis on the role of meta-empirical factors in science, which had largely been ignored by the positivists. The resulting models of science severely reduced the importance of empirical issues by making factual matters relative to particular worldviews; they were often interpreted as portraying the choice of worldview as akin to an act of faith. Scientific change was pictured as a series of revolutions in which scientists "converted" from one worldview to a new, incommensurable worldview.

Discontent with this view of science as irrational has led some to charge that the postpositivists went too far in efforts to find a role for meta-empirical factors (Gutting, 1980). Laudan's complaint is that the postpositivists did not go far enough in breaking with positivism. He claims postpositivism is the result of trying to graft meta-empirical issues onto the positivists' badly flawed account of scientific controversies. Laudan instead presents a gradualist's alternative to the postpositivists' revolutionary model of scientific change. According to Laudan, close attention to the historical details of episodes of rapid change in science reveals the process to be one of piecemeal reform rather than incommensurable "saltatations."

To outsiders, the sciences appear remarkable for the degree of agreement and shared views. Outsiders see those aspects of a field on which there is broad consensus—the verities of textbooks and of popular treatments. Those actively working within a field have a different perspective. A scientist familiar with the primary literature on a topic will be able to discuss at length the controversies and disagreements that form part of the cutting edge of any science.

Appreciation of both the broad agreements and detailed dissents is essential to understanding historical developments in science. In *Science and Values,* Laudan contrasts his position with positivism and postpositivism and strongly criticizes both for their opposite failures to recognize the roles of agreement and dissent.

Positivism is primarily concerned with explaining the high degree of consensus in science. Science was supposed to be culturally unique because of this consensus, and models of science were developed to demarcate science and nonscience by showing how science differed from other intellectual disciplines in which agreement is less pervasive. The positivists were overwhelmingly concerned with the role of empirical evidence. At their broadest, issues of scientific methodology are rules of evidence shared by all scientists regardless of specialty or theoretical orientation. Conflicts might occur over factual claims but these could be adjudicated by appeal to "the scientific method." At worst, a conflict might require additional, more discriminating empirical evidence. Coupled with the view that scientific debates were confined to empirical issues, the claim that all scientists shared a universal scientific method supported the positivists' picture of science as dominated by agreement; controversies could be easily resolved by appeal to the appropriate rules of evidence.

Criticizing positivism, postpositivists such as Kuhn pointed to the ubiquity of controversy in science. Despite the positivists' picture of pervasive consensus, the history of science is marked by recurring conflict. Laudan provides a number of examples of such debates: for example, Copernican versus Ptolemian astronomy, Newtonian versus Cartesian mechanics, and wave versus particle optics. Such conflicts, of course, are not confined to physics. During both its long past and short history, psychology has been marked by recurring controversies of which the mentalism versus behaviorism debate in its many manifestations is a particularly obvious example. Siding with the critics of positivism, Laudan agrees that the protracted nature of these controversies is inconsistent with the claim that scientific differences can be readily settled by applying universally accepted standards of evidence. But Laudan suggests that, in correcting the positivists' failure to address the role of disagreement in science, Kuhn and other critics have constructed models in which consensus is largely inexplicable. "Kuhn is scarcely unique among contemporary philosophers and sociologists of science in propounding an account of disagreement which leaves little or no scope for explaining agreement" (1984, p. 19).

The postpositivists recognized, as their predecessors had not, that scientific controversies extend well beyond simple disputes over factual matters. Scientific controversies are marked by profound differences concerning standards of evidence, appropriateness of alternate research strategies, and the goals and objectives of a given scientific discipline. Laudan strongly concurs with the postpositivists in recognizing a central role for such meta-empirical factors. He breaks with them, however, in viewing meta-empirical disputes as subject to rational adjudication. According to Laudan, the failure of the postpositivists to appreciate this led them to develop models of science in which consensus is anomalous. This point can be better understood by considering Laudan's account of Kuhn.

For Kuhn, protracted scientific controversies indicate that rival theorists are operating within different paradigms. The different paradigms are distinguished in part by incompatible methodologies and conflicting assumptions concerning aims and objectives. These meta-empirical differences ensure that simply appealing to empirical data will not settle the controversy; the quality and significance of the data will be viewed differently from the perspectives of the two paradigms. Hence, the incommensurability of different paradigms: The meta-empirical differences create a communication barrier that makes a meaningful exchange concerning differences impossible. Kuhn, obviously then, is in a position to explain protracted controversies. What he cannot do, claims Laudan, is explain an equally important fact about the history of science: that controversy eventually gives way to consensus as the leading scientists reach agreement about which paradigm is the most acceptable.

The positivists failed to explain disagreement. The postpositivists cannot deal adequately with consensus. Both, Laudan argues, must be dealt with in a comprehensive model: "Until we manage to account for a Janus-faced science, we cannot claim to have to have understood what we are about" (1984, p. 22).

In *Science and Values,* Laudan provides an account of disagreement and consensus by discussing an approach in which scientific claims are viewed as occurring on three levels: the axiological level, consisting of claims about aims and objectives; the methodological level, involving claims about the proper procedures for data collection and analysis; and the empirical level, including claims about theoretical entities as well as assertions about directly observable events. Laudan presents his own hierarchical model by criticizing a simpler version. The simple hierarchical model was initially developed by positivists, but Laudan claims—and this is a key point—that the model's major assumptions were adopted uncritically by the postpositivists despite their generally critical stance toward the positivists' position.

According to the simple hierarchical model, disputes at one level are resolved by appealing to the next level in the hierarchy. The relationship between the levels is unidirectional, with a higher level determining the adjudication of disputes at a lower level, but not vice versa. Disagreements over facts are resolved by moving one step up the hierarchy to the methodological level; in this model the methodological level constitutes a set of agreed-upon rules of evidential support. Even if these rules do not dictate an immediate resolution to a conflict, they indicate procedures for the collection of additional data capable of determining a definitive resolution. Sometimes, however, scientists disagree over the appropriate rules of evidence. Again, the simple model suggests that such disputes are resolved by an appeal to the next higher level—the axiological level.

The claim, shared by positivists and postpositivists, that disputes at one level are resolved by appealing to a higher level suggests that disputes at the axiological level—the highest—are irresolvable. This result has very different consequences for the two positions. Because positivists, with their unified science program, viewed axiological differences as largely nonexistent, their interpretation of the model predicted that disagreements would be of short duration. On the other hand, postpositivists argued, on historical grounds, for a multiplicity of axiological perspectives. The resulting view is as if the "unified science" model of the positivists were fragmented. Paradigms represent different axiological perspectives. The simple hierarchical model then would describe the internal functioning of each paradigm. Within paradigms, disputes will be quickly settled. The assumption that axiological disputes are irresolvable—carried over from the positivists' assumption of unidirectional influence between levels—guarantees that disputes between paradigms will be protracted.

Laudan presents an alternative model incorporating a number of changes that, he claims, allow him to capture the strengths of both positivism and postpositivism while avoiding their respective weaknesses. He argues, in short, that his model can explain both consensus and disagreement. Most important, he rejects unidirectional influence between levels, claiming the three levels mutually influence each other. For example, factual claims, contrary to the simple model, influence methodological decisions. It was the empirical finding that subject expectations could influence data in drug studies that led to single-blind procedures to eliminate placebo effects. Experimenter expectancy effects later led to adoption of double-blind procedures.

Laudan's view of the process of mutual adjustment among the three levels suggests that, although change might occasionally be rapid, the process is one of piecemeal reform rather than wholesale revolution. Given the frequent use of Kuhn's analysis to describe the situation in psychology, the difference between Laudan and Kuhn on this point is worth emphasizing. Kuhn's analysis has decidedly pessimistic implications for those unhappy with the reigning cognitive paradigm. If paradigms were indeed self-sustaining and isolated monoliths immune to external criticism, there would be little to do but wait and hope that the next revolution produces a more acceptable paradigm. At a time when Kuhn's views have been repudiated by philosophers and historians of science, it would be unfortunate if the continued use of his analysis by pyschologists led behavior analysts to acquiesce to the status quo.

Laudan's gradualist model of scientific change more accurately reflects the behavior of those who have actively responded to the situation in psychology. Skinner (1977, 1985, 1989), for example, has aggressively responded to cognitivism with arguments that Laudan would characterize as methodological or axiological. Recent behavior-analytic research concerning cognitive phenomena (e.g., *JEAB*, 52(3), November 1989) and verbal behavior (e.g., Lamarre & Holland, 1985) are empirical responses. The taxonomy of factors influencing competition in science that Laudan develops in *Progress and Its Problems* provides an organizing framework for discussing these and other responses that have been made to reform contemporary psychology.

LAUDAN'S FIELD GUIDE TO COMPETITION IN SCIENCE

A taxonomy can be judged on a number of points. In a well-developed scientific area, a descriptive taxonomy is expected to be congruent—or at least compatible—with the categories of the leading theories. Where there is no widely accepted theory or when the taxonomy is not for scientific purposes, probably the most pressing concern is to have a taxonomy rich enough to deal with the variability of the phenomena in question. The would-be amateur naturalist whose report on a day in the field was of "four little gray birds and seventeen other things that weren't" was probably guilty of using an unduly sparse taxonomy.

For much of this century, philosophers of science quite deliberately eschewed any effort to model the actual conduct of science (Reichenbach, 1938). They felt that certain general features of language allowed them to derive criteria for judging the empirical adequacy of scientific theories at a level of abstraction that did not require attention to the actual conduct of scientists. Sparse indeed was their taxonomy, which, on the basis of assumptions concerning the relation between theoretical and observational sentences, divided scientific theories into those that were empirically meaningful and those that were not.

Although still very much concerned with philosophical issues, Laudan attempts a much more realistic model, descriptive of actual scientific practice. In a sense, he provides a natural history of science, with historical cases and episodes providing the data. In broadening the philosopher's view of science, Laudan is particularly anxious to draw attention to the variety of problems dealt with by scientists. His development of a descriptive taxonomy of scientific problems is central to this effort. This taxonomy, developed in considerable detail in *Progress and Its Problems,* provides a rich descriptive framework for approaching historical developments in science. In this section we describe this taxonomy and demonstrate its applicability to the concerns of behavior analysts by using episodes from the history of psychology to illustrate Laudan's system. Table 1 provides a simplified summary of Laudan's taxonomy, in which are recognized two major categories of problems: empirical and conceptual. We discuss each in turn.

A preliminary caveat is in order. There is considerable room for differences of opinion as to where a particular problem should be placed in Laudan's scheme. This is not necessarily a weakness in the taxonomy. Some of the uncertainty over categories may be due to ambiguities or vagueness in the way categories are defined, but the model predicts that some ambiguity is unavoidable. The placement of problems within the system changes over time. This is something that Laudan, with his concern for the process of historical growth in science, is particularly interested in stressing. Laudan's system can also accommodate differences of opinion resulting from differences in theoretical perspectives. Given differences in background theories, one and the same empirical finding can be viewed by one person as a seriously debilitating anomaly and by another as an interesting if peripheral phenomenon worthy of additional research attention. In emphasizing the central role of problem evaluation in scientific conduct, Laudan is sensitive to the many factors leading to different assessments of the nature and importance of a problem.

Table 1

Laudan's taxonomy of scientific problems.

Empirical problems	Conceptual problems
Solved problems	Internal
Unsolved problems	External
	Normative
Anomalies	Intrascientific
Refuting	Worldview
Nonrefuting	

Empirical Problems

Empirical Anomalies

Laudan underscores the significance of including meta-empirical considerations in his model as a means of making clear his break with traditional accounts. This is not to say that he ignores empirical issues; empirical problems are treated at length and in considerable detail. Even when discussing empirical problems though, his treatment is a good deal broader than those of his predecessors. This difference is particularly evident in Laudan's account of anomalous problems.

In a broad sense, empirical anomalies occur whenever empirical findings raise doubts about a theory. Traditional concern with empirical anomalies was confined to those situations in which a theory's predictions were logically inconsistent with observations. Laudan, of course, recognizes this type of anomaly but claims that it plays a considerably more restricted part in actual scientific conduct than the all-important role suggested by traditional accounts. Analysis of historical cases shows that scientists often simply ignore what, on logical grounds, are refuting instances. Such an outcome is likely when there is doubt about the refuting experimental observations. It also occurs when a theory is retained in the face of anomalous data simply because it is the only or (despite its problems) best theory available.

Traditional accounts also err in overlooking empirical anomalies that occur without *logical inconsistency* between a theory and data. Laudan argues that these "nonrefuting anomalies" have played an important role in science:

> A careful look at the history of science makes it clear that a number of situations generate behavior similar to the kind of response which we have been led to expect [by traditional accounts] when an inconsistency between theory and observation arises. *One of the most important species of anomaly arises when a theory, although not inconsistent with observational results, is nonetheless incapable of explaining or solving those results (which have been solved by a competitor theory).* (1977, p. 29, his emphasis)

Nonrefuting anomalies, by definition, arise only where there are directly competing theories. As will be seen, a parallel conceptual problem can arise as the result of an empirical finding in one discipline that casts doubt on a theory in a different domain. The competition between learning theorists earlier in this century provides many examples of both refuting and nonrefuting anomalies. For example, Tolman's experiments on place learning produced findings explicable in terms of his theory but were either unpredicted or logically inconsistent with Hull's theory. Skinner's early work often produced nonrefuting anomalies. As Verplanck (1954) noted, precisely because the positions of Hull, Tolman, and Guthrie made no predictions about the effects of intermittent reinforcement, Skinner's exploration of intermittent schedules counted against their positions and provided support for Skinner's alternative.

Unsolved Problems

Laudan classifies as an unsolved problem an empirical finding not explained by any competitors in a domain. Unsolved problems are—like anomalies—a clear liability to a theory. Laudan points out that this category is more complicated than it might first appear. If an empirical finding is totally unexpected from the perspective of the reigning theories, the initial scientific response will often be one of skepticism that the finding is genuine. Hence, the response to an early report on taste aversion learning: "Those findings are no more likely than birdshit in a cuckoo clock" (reported by Seligman & Hager, 1972, p. 15). Even when an effect has been well documented, its status as a problem for a particular domain can remain unclear. Until a solution is at hand, it is often unclear to which scientific domain a problem belongs.

While a problem remains in this ambiguous state there is considerable room for debate among scientists as to who should be charged with the responsibility for addressing the problem. The reaction of behavior analysts and their critics to various findings often grouped under the rubric "constraints on learning" exemplifies this situation. Many critics (e.g., Bolles, 1985) have viewed the behavioral phenomena reported by Breland and Breland (1961), Garcia and Koelling (1966), and Staddon and Simmelhag (1971) as presenting a serious challenge to behavior analysts. Skinner (1983) addressed these findings and in each case found them compatible with behavior analysis. For example, in discussing taste aversion learning, Skinner concludes, "There is nothing in the Garcia Effect that contradicts any part of an operant analysis or throws into question any established facts" (p. 14). Skinner does not view "constraints on learning" phenomena as counting against operant theory because the findings do not contravene any explicit predictions; that is, they are not refuting anomalies. Some critics, on the other hand, view the findings as liabilities because they interpret the findings as problems that behavior analysts have ignored. Hinde, for example, states, "And at no point do I wish to imply that learning theorists cannot cope with the issues raised, only that they have been neglected" (1973, p. 4). Hence, critics view the findings as serious unsolved problems or nonrefuting anomalies for behavior analysts.

This disagreement is the result, in part, of differences between behavior analysts and critics in interpreting the intended scope and aims of behavior analysis. This is an important issue in its own right. Debates over the scope and aims of a given science are an important part of competition between theoretical perspectives. Such debates have figured prominently in the exchanges between behaviorists and their opponents. Closely related issues will be discussed below in connection with Laudan's views on the role of conceptual problems in science.

Solved Problems

Anomalies count against a theory. Unsolved problems, on occasion, can do so as well. Solved problems count in favor of a theory. Although this category is probably the most straightforward of the three species of empirical problems, it is nevertheless subject to additional clarification. Laudan notes that the standards by which solutions are evaluated generally become more demanding over time. What counts as an adequate solution at an early point in the development of a theory will often be considered inadequate later. Laudan points out that, despite what might be expected from the formal models of the relation between theories and observation, problem solutions are almost always only approximate; that is, the fit between prediction and observation is rarely exact. For example, the law of effect as used by Thorndike has been superseded by the quantitative law of effect as used in the matching literature. Qualitative prediction may be acceptable when competing theories are unable to manage equally accurate predictions. Thus, Skinner's early work on schedules of reinforcement was quickly accepted as an important advance. But, of course, the analysis of temporal control of responding has changed considerably since the work of Ferster and Skinner (1957), as seen in the work of Dews (1970), Church (1978), and Palya and Pevey (1987).

The Weighting of Empirical Problems

Laudan suggests that a realistic account of the conduct of science must also address the issue of the relative importance of different problems. Scientists view some problem solutions as more important than others and judge some anomalies to be more threatening than others. Therefore, in addition to his taxonomy of empirical problems, Laudan outlines some factors involved in weighting empirical problems. A few examples will illustrate Laudan's approach.

Clearly, solving an anomalous problem is a particularly significant activity. Such problem solving does double duty. Like any problem solution, solving an anomaly exhibits the problem-solving capacities of a theory. Solving an anomaly has the added benefit of simultaneously eliminating a liability of a theory. Although not explicit in Laudan's treatment, a corollary of the importance of solving anomalies is that, in a highly competitive situation, empirical studies will be selected with an eye toward generating anomalies for a competing theory. This is amply borne out by the history of the competitive interaction between followers of Tolman and Hull. For example, Tolman and others conducted experiments that simultaneously explored place learning while generating refuting anomalies for Hull's theory of the central importance of learned sequences of muscular movements. Many of the experiments on place learning were designed explicitly to serve this dual purpose (e.g., Tolman & Honzik, 1930; Tolman, Ritchie, & Kalish, 1946). The important role of anomalous problems in driving a research program is exemplified by Spence's (1937) efforts to develop a stimulus–response discrimination model to deal with the anomaly of transposition. The model was soon extended (Spence, 1942) to deal with the related anomaly of the intermediate size problem.

There are a number of factors that increase or decrease the importance of a solution to a problem. Laudan discusses the role of what he calls *archetypes* in this context. Many theories single out, from a much broader range of phenomena, certain empirical situations as archetypal because the theory suggests that these situations are primary or basic. Thus, Hull's theoretical assumption that drive reduction was basic in learning both emphasized experimental situations in which biological "needs" were involved and inflated the importance of studies directed at explaining learning where there was no apparent drive reduction. Anomalous findings, such as reinforcement by saccharin (Sheffield & Roby, 1950; Sheffield, Roby, & Campbell, 1954) led to alternative theories that posited reinforcing properties for some sensory events. This new theoretical assumption picked out a slightly different range of conditioning situations as basic, leading to a different weighting of problems.

The solutions of problems that extend the explanatory scope of a theory are of obvious importance: hence the significance of research by Fuller (1949) and Azrin and Lindsley (1956) that helped establish applied behavior analysis by extending the scope of behavior-analytic principles and techniques developed with animals in the laboratory to human behavior outside the laboratory. Unanticipated generality increases the importance of a set of problems. Claims that a common mechanism was involved that could explain the long-unsolved problem of specific hungers increased the significance of taste aversion studies (Rozin & Kalat, 1971).

Laudan also recognizes that "irrational" factors can influence the weighting of problems. The emphasis of federal funding agencies on certain areas of research may reflect political or practical concerns rather than purely theoretical considerations. How availability of funds influences priorities in behavior-analytic research is an example (Baer, 1975). Although not addressed by Laudan, a particular type of experimental situation or apparatus might, in some cases, constitute a nonrational factor in defining and weighting the "archetypal problem." It seems clear that, prior to Skinner's introduction of free-operant techniques, the choice of discrete-trial techniques reflected practical constraints rather than theoretical concerns. It was not as if the reigning theories required discrete-trial techniques; it was simply that free-operant techniques were unavailable. Nevertheless, even though selected for practical rather than theoretical reasons, discrete-trial techniques decisively influenced the choice of problems by limiting attention to a subset of the factors involved in behavior. Skinner's development of the necessary techniques significantly broadened the range of empirical problems. Free-operant techniques introduced empirical problems involving response rates and the analysis of behavior in temporally dynamic contexts. Of course, techniques adopted by behavior analysts can also constrain the choice of empirical problems, a point discussed by Killeen (1985) in his reflections on the use of the cumulative record.

Conceptual Problems

Laudan stresses his inclusion of conceptual problems in distinguishing his position from those of his predecessors. According to Laudan, models restricted to empirical problems are too impoverished to account for much of actual scientific conduct. It is not just a matter of Laudan finding some small role for conceptual issues. Instead, he claims that they are major determinants of scientific development: "It is vital to stress at the outset that a conceptual problem will, in general, be a *more* serious one than an empirical anomaly" (1977, p. 64).

The point of including conceptual problems within his taxonomy of the problems of science is parallel to the incorporation, in *Science and Values,* of mutual interactions among the three levels of his version of the hierarchical model. If Laudan's broadly elaborated account of the domain of empirical problems is implicitly a critical reworking of the positivists' account of the relation between theory and observation, then his account of conceptual problems can be viewed as an extended critique of the Kuhnians' failure to find a meaningful role for debate of meta-empirical issues within "normal" science. We believe Laudan's treatment of conceptual problems should be of particular interest to behavior analysts. Empirical problems, by their very nature, crop up most often in relation to a specific theory or among a set of closely related theories. Conceptual problems, on the other hand, are particularly evident in debates between broad theoretical perspectives. Thus, Laudan's account can provide a framework for examining recent controversies between behavior analysts and their cognitivist or ethological critics, a framework that does not assume that such controversies are necessarily intractable.

Internal Conceptual Problems

Laudan subdivides conceptual problems into internal and external problems. Internal conceptual problems arise when a theory exhibits internal inconsistencies, and external problems arise from conflict with another theory. Laudan devotes most of his attention to the latter, suggesting, on historical grounds, that internal conceptual problems are considerably less important in scientific controversies. It is often scientists working within a tradition rather than external critics who spot internal conceptual problems, because recognizing these problems requires a working familiarity with the details of a particular theory, and such scientists are usually more interested in refining rather than discarding the theory. External critics offering possible internal conceptual problems are often ignored because they have misunderstood or misrepresented essential portions of a theory, but external critics sufficiently well versed in a theory to offer informed criticism may not be especially forgiving. The detailed reviews of Hull's system by Miller (1959) and Logan (1959) are quite different in tone and overall conclusion from Koch's (1954). As active scientists within the Hullian tradition, both Miller and Logan largely accept the system and focus on smaller problems that Koch does. Instead, Koch takes Hull to task over fundamental issues of operational definitions, measurement, and function construction.

External Conceptual Problems

External conceptual problems arise from conflict between one theory and another and can be divided into three types, based on the nature of the external theory with which the problem-laden theory is in conflict. *Normative* difficulties occur when a theory conflicts with axiological and methodological assumptions. *Intrascientific* difficulties result from tension between two theories from different scientific domains. *Worldview* difficulties arise when a scientific theory is viewed as being incompatible with a nonscientific doctrine or body of beliefs.

Normative conceptual problems. Science, Laudan claims, is an activity with aims and goals; hence, assessment in science often involves questions concerning the means used to achieve goals and questions about the goals themselves. "These norms, which a scientist brings to bear in his assessment of theories, have been perhaps the single major source for most of the controversies in the history of science, and for the generation of many of the most acute conceptual problems with which scientists have had to cope" (1977, p. 58).

Certainly normative issues have often been paramount in behaviorists' controversies with other orientations. Watson's (1913) criticisms of introspection were based almost exclusively on normative issues. The paper opened with his famous declaration that the goal of psychology is the prediction and control of behavior. Introspectionists were faulted on the grounds that their theoretical orientation was incompatible with the objectivity of natural science. Their inability to resolve conflicts among different factions was cited as evidence of the bankrupt nature of their methodology. The problems posed by attempts to extend the introspectionists' descriptive categories to nonhumans was taken as evidence that, despite claims for universality, the scope of their theory was to narrow.

Normative issues have also been central to controversies among behaviorists. Skinner's (1944) criticism of Hull's (1943) *Principles of Behavior* was addressed entirely to conceptual issues. In part, Skinner based his criticism on detailed analyses of internal conceptual inconsistencies, but in general the criticism deals with Hull's failure to consistently adopt the techniques of a functional analysis.

Normative conceptual problems that arise as a result of the tension between a theory and methodological commitments are not always resolved in favor of the methodological strictures. Robinson, Baum, and Woodward[2] have suggested that the convergence of interests between ethologists and behavior analysts in optimal foraging theory has changed methodological assumptions among both ethologists and behavior analysts. Ethologists' interest in optimal foraging theories led to a change in emphasis from qualitative descriptions to quantitative models and to a newfound willingness to make use of laboratory studies—including operant research techniques. They suggest a parallel shift among behavior analysts concerned with foraging theory, a shift of interest from the cumulative record and molecular analysis to multiple schedules and relative response rates.

Intrascientific conceptual problems. During the 1930s and 1940s, the major controversies confronting behaviorists were intramural debates among learning theorists. Of late, the most pressing controversies are with theoretical perspectives of other domains. These current controversies often exemplify what Laudan calls intrascientific conceptual theories because of different basic assumptions rather than empirical differences. The emergence of transformational grammars in linguistics is an obvious example. Chomsky (1957, 1959) aggressively argued the view of language involved in transformational grammars was incompatible with any behaviorist account of

[2] Robinson, J. K., Baum, W. H., & Woodward, W. R. (1988, May). *The convergence of behavioral biology and operant psychology.* Paper presented at the annual meeting of the Association for Behavior Analysis, Philadelphia.

language. Although later there were claims of experimental findings empirically anomalous to the behaviorist position, the criticisms initially involved meta-empirical issues.

In Chomsky's case the claim was made that the new theory was logically incompatible with the older one. Laudan argues that there need not be strict, logical incompatibility for a new theory to raise conceptual difficulties for the older theory. His move here is parallel to his arguments in favor of broadening the notion of empirical anomaly. For example, conceptual problems can arise when a theory emerges that, in some sense, should strengthen an existing theory but fails to do so and is merely compatible with it. This is because assumptions about the interdisciplinary structure of science can generate expectations about how theories in different domains should be related. Ethology and behavior analysis are not logically inconsistent, and both make allowances for phenomena of interest to the other; for example, ethologists recognize a role for learning (Lorenz, 1965) and behavior analysts recognize a role for species-specific behavior (Skinner, 1966). Yet the growth of ethology is often portrayed by ethologists as being inimical to behavior analysis (Burghardt, 1984). Although there are substantive methodological issues at stake in the controversy, the problem also seems to be one of the failure of an emerging biological theory to strengthen behavior-analytic theory, whose roots lie nearly as much in biology as in psychology.

Worldview conceptual problems. The third type of external conceptual problem arises as a result of tension between a theory and what Laudan characterizes as a worldview. Worldview problems are similar to intrascientific problems, except that they are caused by nonscientific doctrines or assumptions. Although worldview difficulties arise from extrascientific assumptions, it is only as such concerns get filtered through the professional concerns of scientists that they play a significant role in scientific debates. Laudan's point is that such broad background assumptions do play a role in the evaluation of scientific theories even when they are not articulated explicitly.

Behavior analysis has often encountered resistance because of worldview problems. Rogers' (1964) reaction was typical of many psychologists who found behaviorism unacceptable because of conflict with traditional views of free will and the nature of the self. This example might suggest that worldview problems always represent a clash between outdated, unscientific ideologies and newer scientific theories, but Laudan includes the "common sense" of scientists among the worldviews that can conflict with particular theories. Scientists make broad assumptions about the physical universe, and these can play an important role in the way particular theories are assessed. Consider the reaction of many scientists to parapsychology. Parapsychology is frequently dismissed on the basis of very broad assumptions about the world rather than detailed refutation of specific claims by parapsychologists. For example, the claim that some "psi" phenomena supposedly require an effect to precede its cause is sufficient grounds for many to dismiss the matter without examination of empirical evidence.

Objections to behavior analysis based on doctrines of free will are usually obvious. Less obviously, the commitment of many psychologists to a worldview involving mechanistic reductionism may contribute to objections to radical behaviorism—which involves an alternative worldview (Hayes, Hayes, & Reese, 1988). In criticizing radical behaviorism in mechanistic and reductionistic terms, critics reflect their own worldview. This bias in favor of mechanistic reductionism might account for the unfortunate tendency, noted by Malone (1987) and others, to equate behaviorism with stimulus–response associationism.

Traditionally, behavior analysts' central objection to mentalism has been methodological. Mentalistic theoretical constructs are dangerous because they can easily lead to the development of elaborate theories at the expense of attention to the behavior (Skinner, 1950). Behaviorists have also objected to the new cognitivism on worldview grounds, charging that, like the old mentalism, it is committed to a mental-physical dualism inconsistent with the thoroughgoing physicalism of natural science. Many cognitive scientists would object to this charge. Dennett (1987), for example, argues that the new cognitivism is physicalistic and suggests that part of the significance of the computer metaphor is that computers demonstrate the usefulness of describing purely physical systems in cognitive terms.

Problems and Progress

Having elaborated a taxonomy of the empirical and conceptual problems of scientists, Laudan provides a sketch of how these play a role in judgments of progress. Accepting the truism that progress occurs when a theory is replaced by one that solves more problems, Laudan insists that this truism is historically accurate only if the concept of "problem" is broadened beyond the empirical problem issues of traditional philosophers of science. If philosophical accounts of scientific progress are based solely on solved empirical problems while anomalies and conceptual problems are not factored in, the picture of science that emerges fails to reflect the judgments of scientists about progress in their own disciplines.

CAN WE HAVE PHILOSOPHY AND SCIENCE?

Scientists disagree about data interpretation, theory construction and evaluation, weighting of problems, and many other details of scientific conduct. It is not surprising that they also disagree over the merits of specific philosophical models of scientific development or even argue about the usefulness of such models.

Skinner (1945, 1957) has suggested that a science of science may eventually emerge out of an analysis of language. Yet science is arguably one of the most complex classes of verbal behavior and may be one of the last to be adequately captured by thorough behavior analysis. It will be particularly difficult to subject scientific verbal behavior to the direct empirical manipulation that is the hallmark of behavior-analytic research.

Historically, the relation between the models used by scientists and those developed by philosophers of science is complex (Smith, 1986). Skinner, in developing criteria of empirical adequacy, was influenced by Mach and by the logical positivists to a much lesser extent, freely rejecting some aspects of positivism outright and adapting others to his own use. (Even a scientist who has never read in philosophy of science will, as a result of training and experience, develop at least an implicit "philosophy of science.") There is some consensus on a behavior-analytic meta-theory, as sketched by Skinner (1938, 1950) and Sidman (1960). This is probably the closest thing extant to a behavior-analytic philosophy of science. Agreement primarily concerns relatively narrow procedural issues such as the appropriate data language, rules of evidence, and standards of quantitative analyses. Broader issues about the nature of science have, of course, often been discussed by behavior analysts. For example, Skinner has discussed the direct experiential contact the scientist has with the natural contingencies of the phenomenon under study (Skinner, 1957). The scientist's behavior is shaped and maintained by the reinforcing effects of accurate prediction and control. This is related to Mach's view of science as an extension of skilled human activity; the relevant relation is between the behavior of the individual scientist and the contingencies offered by direct contact with the world (Smith, 1986). As Skinner (1957) has pointed out, a complete analysis requires the integration of the control exerted by the scientific verbal community with the direct contingencies offered by the phenomenon. In an important and tolerably accurate sense, Laudan's model can be understood as an attempt to deal with this social dimension of science through a description of the reinforcing practices of scientific communities that contribute to the shaping and maintenance of the behavior of their members.

Laudan is concerned with actual scientific practices; his move away from a *pre*scriptive toward a *de*scriptive philosophy of science would probably win the approval of many scientists. It was the presumption that philosophers could dictate scientific practice ("The practical scientist does the business but the philosopher keeps the books"; Goodman, 1972, p. 168) that earned philosophy the enmity reflected in the quip "scientists need philosophy of science like birds need ornithology." Zuriff (1985) made a similar point in describing how some claim philosophy of science to be an epiphenomenon of science—influenced by but having no substantial influence on science.

Laudan's suggestion that his model is empirical is debatable, however. Historical data, although useful, are obviously not collected under the controlled conditions that define *empirical* for many scientists. There are also difficulties even on less stringent grounds. There is some circularity involved in using the historical record to develop a taxonomy and then returning to the same record for data to test the taxonomy. If it seems odd to raise objections to a *philosophy* of science based on questions of sampling procedures, reliability, and the validity of the descriptive categories, it is so much the worse for Laudan's claim to be providing an empirical description of scientific practices. Coleman (1985) argued that the Skinner–Boring debate over Skinner's history of the reflex (Skinner, 1931) was at least partially driven by fundamental differences over the proper uses of history in science and what it implies about scientific growth and progress. Use of history as a critical tool to discuss current issues, rather than as a self-contained activity, comes close to uses of historical analysis in modern philosophy of science.

Although a reasonably complete empirical analysis of scientific behavior is currently unavailable, working scientists will probably continue to use models of science to discuss their own work and the work of others. Philosophers of science have probably tended to overestimate the importance of these models in shaping scientific practices, but such meta-empirical assumptions undoubtedly play some role in the professional behavior of scientists. For example, a scientist who has adopted views similar to Kuhn's on the problem of communicating across paradigms would probably spend little time in dealing with external critics or criticizing alternative paradigms. In Laudan's system, the active resolution of conceptual conflicts is seen as an important source of scientific progress. Presumably a scientist who has adopted such a perspective would be more likely to carefully consider and respond to criticisms raised by external critics and to criticize alternative theoretical viewpoints.

The lack of a truly empirical science of science can engender a sense of complacency about one's own philosophical assumptions about science. Granted that no extant philosophy of science is empirically adequate, still this does not mean that all philosophies of science are equally satisfactory. The important role of comparative evaluation in the history of science is a recurring theme in Laudan's work. It is probably fitting, then, to end by suggesting that the potential value of Laudan's position—despite misgivings about empirical adequacy—lies in providing an alternative perspective for critically examining and comparatively evaluating the more familiar views of philosophy of science.

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