The Paradigm Concept

The Structure of Scientific Revolutions. THOMAS S. KUHN. Second edition. University of Chicago Press, Chicago, 1970. xii, 210 pp. Cloth, $6; paper, $1.50. International Encyclopedia of Unified Science, vol. 2, No. 2.

Criticism and the Growth of Knowledge. Proceedings of the International Colloquium on the Philosophy of Science, London, July 1965, vol. 4. IMRE LAKATOS and ALAN MUSGRAVE, Eds. Cambridge University Press, New York, 1970. viii, 282 pp. Cloth, $11.50; paper, $3.45.

Since its publication in 1962, Thomas Kuhn's *The Structure of Scientific Revolutions* has become one of the most popular attempts of all time to interpret the nature of science. It has proved an important step in a movement away from the positivistic empiricism that has held sway, among both philosophers and working scientists, for well over two generations. Writers in many disciplines have adopted the book's fundamental notion of "paradigm" in analyses of their subject matter and controversies. The book has had an impact also on a wide body of laymen, even, on occasion, being cited as authority by spokesmen of the New Left.

The thesis of the original edition was that "particular coherent traditions of scientific research" (p. 10), which Kuhn called "normal science," are unified by and emerge from "paradigms." Paradigms are "universally recognizable scientific achievements that for a time provide model problems and solutions to a community of practitioners" (p. x). Kuhn conceived of a paradigm as not identifiable with any body of theory, being more "global" (p. 43) and generally incapable of complete formulation. He held it to include "law, theory, application, and instrumentation together" (p. 10), consisting of a "strong network of commitments, conceptual, theoretical, instrumental, and methodological" (p. 42), and even "quasi-metaphysical" (p. 41); it is, he claimed, "the source of the methods, problem-field, and standards of solution accepted by any mature scientific community at any given time" (p. 102), permitting "selection, evaluation, and criticism" (p. 17). "Normal science" consists of working within and in the light of the paradigm, making it more and more explicit and precise, actualizing its initial promise "by extending the knowledge of those facts that the paradigm displays as particularly revealing, by increasing the extent of the match between those facts and the paradigm's predictions, and by further articulation of the paradigm itself" (p. 24). In the course of such articulation, however, "anomalies" arise which, after repeated efforts to resolve them have failed, give birth to the kind of situation in which a scientific revolution can take place:

> Confronted with anomaly or with crisis, scientists take a different attitude toward existing paradigms, and the nature of their research changes accordingly. The proliferation of competing articulations, the willingness to try anything, the expression of explicit discontent, the recourse to philosophy and to debate over fundamentals, all these are symptoms of a transition from normal to extraordinary research. . . . Scientific revolutions are inaugurated by a growing sense . . . that an existing paradigm has ceased to function adequately in the exploration of an aspect of nature to which that paradigm itself had previously led the way [pp. 90–91].

New candidates for fundamental paradigm are introduced; ultimately one may become accepted, often necessitating "a redefinition of the corresponding science" (p. 102). Kuhn emphasized that scientific revolutions are "non-cumulative developmental episodes in which an older paradigm is replaced in whole or in part by an incompatible new one" (p. 91).

Kuhn's views diverge radically from those dominant since Mach and Ostwald, developed in the outlook of the Vienna Circle and its intellectual associates, and paralleled in the views of Bridgman and Frank and a host of more recent thinkers. Whereas those views tended, at least in their heydays, to separate sharply "fact" (or "observation" or "operation") from "interpretation"—thus claiming to preserve the "objectivity" of science—Kuhn emphasizes the dependence of what counts as a "fact," a "problem," and a "solution of a problem" on presuppositions, theoretical or otherwise, explicit or implicit. Likewise, he attacks traditional "development-by-accumulation" views of science—views according to which science progresses linearly by accumulation of theory-independent facts, older theories giving way successively to wider, more inclusive ones. In these respects, Kuhn's book has had an undeniably healthy influence on discussions of the nature of science, bringing them to a closer inspection of science and more in line with what recent scholarship has revealed about its history.

Despite these beneficial effects, however, Kuhn's views as expressed in the first edition have faced severe criticism. Two main types of objections have been raised. Those of the first type revolve around ambiguities in the notion of "paradigm." For that term, although at the outset it is applied to "a set of recurrent and quasi-standard illustrations of various theories," which are "revealed in . . . textbooks, lectures, and laboratory exercises" (p. 43), ultimately appears, as the reader may have gathered from the passages quoted above, to cover anything and everything that allows the scientist to do anything. The assertion that a scientific tradition is paradigm-governed then appears to become a tautology, and all the wealth of Kuhn's historical analysis becomes irrelevant. On the other hand, the term is so vague that, in particular cases, it is difficult to identify what is supposed to be the paradigm. (This problem is, of course, compounded by Kuhn's insistence that the paradigm is not, and in general cannot be, completely expressed.) Furthermore, the vagueness of the term makes the distinction between "normal" and "revolutionary" science seem a matter more of degree than of kind, as Kuhn claims: expression of explicit discontent, proliferation of competing articulations, debate over fundamentals are all more or less present throughout the development of science. And similarly for the distinction, drawn with uncompromising sharpness by Kuhn, between different "traditions" in science: far from there being such sharp discontinuities, there are always guiding factors which are more or less common, even among what are somewhat artificially classified as different "traditions." Finally, "commitment" to such guiding factors does not in general seem to be as rigid as Kuhn suggests.

The second major type of objection

against Kuhn's first-edition view has to do with the relativism in which it apparently eventuates. In emphasizing the determinative role of background paradigms, and attacking the notion of theory- (or paradigm-) independent "facts" (or any such independent factors or standards whatever), Kuhn appears to have denied the possibility of reasonable judgment, on objective grounds, in paradigm choice; there can be no good reason for accepting a new paradigm, for the very notion of a "good reason" has been made paradigm-dependent. And certainly, though in some passages Kuhn denied this implication of his view, in most he gloried in it: "the competition between paradigms is not the sort of battle that can be resolved by proofs" (p. 147), but is more like a "conversion experience" (p. 150); "What occurred [in a paradigm change] was neither a decline nor a raising of standards, but simply a change demanded by the adoption of a new paradigm" (p. 107); "In these matters neither proof nor error is at issue" (p. 150); "We may . . . have to relinquish the notion, explicit or implicit, that changes of paradigm carry scientists and those who learn from them closer and closer to the truth" (p. 169). Objectivity and progress, the pride of traditional interpretations of science, have both been abandoned. Indeed, Kuhn's relativism did not stop here: for not only is there no means of rationally assessing two competing paradigms; there is no way of comparing them at all, so different is the world as seen through them (or—in an alternative formulation that is in many ways more consonant with Kuhn's general thesis—so different are the worlds they define). "The normal-scientific tradition that emerges from a scientific revolution is not only incompatible but often actually incommensurable with that which has gone before" (p. 102). Kuhn carried this view to the point of holding that if the same terms continue to be used after a scientific revolution (like "mass" after the replacement of the Newtonian by the Einsteinian "paradigm") those terms have different meanings.

In this new edition, Kuhn has altered little of the original text; however, he has added a 36-page "Postscript" (p. 174 ff.) reviewing and attempting to meet criticisms that were made of the first edition. This discussion is supplemented by opening and closing essays by Kuhn in *Criticism and the Growth of Knowledge*, a collection of papers—the others are by Paul Feyerabend, Imre Lakatos, Margaret Masterman, Karl Popper, Stephen Toulmin, John Watkins, and L. P. Williams—discussing Kuhn's ideas in relation to those of Popper. (There is good reason to compare the two; for despite the differences that emerge from the discussions in that book, Popper's contention that there is no rationale in the introduction of new "conjectures" in science, but only in the exposure of such conjectures to tests potentially falsifying them, and Kuhn's insistence, at least in the first edition, and despite a number of contradictory statements, that there is no rationale in the introduction of a new paradigm, but only in the attempt to "articulate" the paradigm and make it deal successfully with "anomalies," are basically similar. There is room here to discuss only Kuhn's contributions to this volume; the paper by Lakatos, however, may be recommended as being particularly important and provocative.)

It is important to recognize the extent—and the significance—of Kuhn's withdrawal from his original position. With regard to the concept of paradigm, Kuhn now wishes to distinguish two different senses of the term.

> On the one hand, it stands for the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community. On the other, it denotes one sort of element in that constellation, the concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science [p. 173].

For the former, broader sense Kuhn suggests the name "disciplinary matrix," distinguishing four components of such matrices (pp. 182–86): "symbolic generalizations," "metaphysical paradigms," "values," and "exemplars," the "concrete puzzle-solutions" referred to above. All these elements were lumped together in the first edition; "they are, however, no longer to be discussed as though they were all of a piece" (p. 182). This distinction, however, is of little help to those who found the earlier concept of "paradigm" obscure. Contrary to Kuhn's complaint, few critics failed to see that the *primary* sense of "paradigm" had to do with the "concrete puzzle-solution." The difficulty was, rather, that Kuhn never adequately clarified how the remaining factors covered by that term were related to (embodied in) the concrete examples in such a way that the whole outlook ("paradigm" in the broader sense) of the tradition would be conveyed to students through such examples. Nor did he clarify the ways in which, through the concrete examples, this general paradigm determined the course of scientific research and judgment. Yet it was precisely the unity, and the controlling status, of paradigms that constituted the appeal and the challenge of Kuhn's original view: the contention that there was a coherent, unified viewpoint, a single overarching *Weltanschauung*, a disciplinary *Zeitgeist*, that determined the way scientists of a given tradition viewed and dealt with the world, that determined what they would consider to be a legitimate problem, a piece of evidence, a good reason, an acceptable solution, and so on. (The affinities of Kuhn's view with 19th-century Idealism run deep.) Does he now hold that this "constellation" that makes up the disciplinary matrix is just a loosely associated assemblage, each of whose components has its own separate and separable function? (And Kuhn offers precious little discussion of those functions.) Certainly Kuhn's emphasis here is on the distinction between the components rather than on any unity underlying them; but if this is his new view, then—especially when it is coupled with his apparent abandonment (to be discussed below) of the controlling status of the paradigm—Kuhn will have abandoned what was, however obscure, one of the most provocative and influential aspects of his earlier view. Perhaps this would be—if the remaining elements of his new position prove consistent with this view—for the best. For it could then be argued that he has moved in the direction of a salutary concern with the details of scientific reasoning—for example, with specific ways in which specific background presuppositions may influence scientific judgment and activity—rather than with sweeping but vague generalities that are ultimately tautological. But in any case it would not be the old Kuhn. (It should be remarked that Kuhn still, in spite of his critics' attacks, maintains the sharp distinction between "revolutionary" and "normal" science; indeed, the latter and its characteristic activity of "puzzlesolving"—a notion which Kuhn uses far too lightly—acquire in his essays in *Criticism and the Growth of Knowledge* an even more central role.)

But it is in his attempt to meet the charge of relativism that Kuhn's most striking retreats from his original extreme position occur. Now, what counts as a scientific problem is not determined, at least completely, by the paradigm: "Most of the puzzles of normal science are directly presented by nature, and all involve nature indirectly" (*Criticism*, p. 263); there is, apparently, a paradigm-independent objective world (nature) which presents problems that a paradigm must solve. Further, paradigms no longer, apparently, determine, at least completely, what counts as a good reason:

> It should be easy to design a list of criteria that would enable an uncommitted observer to distinguish the earlier from the most recent theory time after time. Among the most useful would be: accuracy of prediction, particularly of quantitative prediction; the balance between esoteric and everyday matter; and the number of different problems solved. . . . Those lists are not yet the ones required, but I have no doubt that they can be completed. If they can, then scientific development is, like biological, a unidirectional and irreversible process. Later scientific theories are better than earlier ones for solving puzzles in the often quite different environments to which they are applied. That is not a relativist's position, and it displays the sense in which I am a convinced believer in scientific progress [pp. 205–06].

No, that is not a relativist's position; but it is a far cry from Kuhn's first-edition attack on the view of scientific change as a linear process of ever-increasing knowledge (to say nothing of its view that there is no such thing as an "uncommitted observer"), and its defense of the view that what happens in a scientific revolution is "neither a decline nor a raising of standards, but simply a change demanded by the adoption of a new paradigm." It is, in fact, for better or for worse, a long step toward a more conventional position in the philosophy of science—one that makes a distinction between the "given" and the "interpretation" (or "theory") and holds that the latter are adequate to the extent that they account for the former.

It appears, then, that Kuhn now believes that the conceptual guiding factors in scientific research are more diverse and complicated in their functioning, and that there are objective factors that are independent of and exercise some constraint on them ("nature cannot be forced into an arbitrary set of conceptual boxes"—*Criticism*, p. 263). Such sober retrenchment is not, however, consistent with Kuhn's simultaneous adherence to many of his old views. Despite his claim that his view does not imply "either that there are no good reasons for being persuaded [in favor of a new paradigm] or that those reasons are not ultimately decisive for the group" (p. 179), he still tells us,

> What it should suggest, however, is that such reasons function as values and that they can thus be differently applied, individually and collectively, by men who concur in honoring them. If two men disagree, for example, about the relative fruitfulness of their theories, or if they agree about that but disagree about the relative importance of fruitfulness and, say, scope in reaching a choice, neither can be convicted of a mistake. Nor is either being unscientific [pp. 199–200].

But if there are, as Kuhn suggests here and elsewhere, no constraints on what one can assert in the name of "values," it seems gratuitous to speak of *reasons* in such contexts. And yet this seems to be the sort of thing Kuhn intends when he speaks of "good reasons" for adopting a new paradigm (for example, after telling us that his view does not imply "that the reasons for choice are different from those usually listed by philosophers of science: accuracy, simplicity, fruitfulness, and the like" [p. 199], he declares that "such reasons function as values" in the sense just discussed). It is a viewpoint as relativistic, as antirationalistic, as ever.

Particularly unhelpful is Kuhn's reply to the charge that his view of paradigm "incommensurability" implies that competition or communication between different paradigms is impossible. This is partly due to his residual ambiguity regarding the extent to which paradigms determine meanings and views of "nature": for in the absence of a clear idea of the extent of that determination, it is impossible to be clear about the extent to which meanings determined by one paradigm can be expressed in the language of another. This ambiguity in turn destroys the effectiveness of his suggestion that Quine's views on translation can help alleviate the difficulty: for Quine's views (briefly, that "radical" translation is indeterminate in that it depends on some "analytic hypothesis" which is highly arbitrary, though subject to some constraints) are not obviously consistent with Kuhn's first-edition view of paradigm determination of meanings, hypotheses, and standards. Finally, Kuhn's strange view of neural stimuli and processes and their relation to meanings and knowledge muddies the situation still further: on the one hand, we read that "people do not see stimuli; our knowledge of them is highly theoretical and abstract" (p. 192); but on the other hand—when he is trying to face the problem of incommensurability—he says that "the stimuli that impinge on [the adherents of two different paradigms] are the same" (p. 201). It is thus unclear whether what we consider to be stimuli is paradigm-independent or is relative to our paradigm (for Kuhn does call our knowledge of them "theoretical"). Beyond these contributions to confusion, Kuhn's discussion (pp. 202–04; *Criticism*, p. 266 ff.) fails utterly to come to grips with the issue. He now admits (in denial of complete paradigm-determination of meanings) to a great deal of overlap of meanings, and this, he claims, helps to circumscribe the areas of communication breakdown between adherents of different paradigms. But how are mutual understanding and comparison of adequacy to be achieved with regard to *those* areas, once located? His answer is simply that competing scientists proceed to observe one another and "may in time become very good predictors of each other's behavior. Each will have learned to translate the other's theory and its consequences into his own language and simultaneously to describe in his language the world to which that theory applies" (p. 202). But this begs the question, amounting merely to an assertion that such translation is possible. Kuhn has not succeeded in showing how he can retain paradigm incommensurability in the sense of the first edition while allowing cross-paradigm communication and comparison.

In summary, then, Kuhn appears to have retreated from his earlier position in just those respects in which it was most suggestive, important, and influential, and to have retained aspects which many have felt were the most objectionable features of his earlier view. Finally, the consistency of what he has retained with his apparent departures from his former view is certainly open to question. And it is far from being unambiguously clear what his current view really is. He seems to want to say that there are paradigm-independent considerations which constitute rational bases for introducing and accepting new paradigms; but his use of the term "reasons" is vitiated by his considering them to be "values," so that he seems not to have gotten beyond his former view after all. He seems to want to say that there is progress in science; but all grounds of assessment again apparently turn out to be "values," and we are left with the same old relativism. And he seems unwilling to abandon "incommensurability," while trying, unsuccessfully, to assert that communication and comparison are possible.

These issues come to a head in Kuhn's proposals as to what must be done if a complete understanding of science is to be obtained, and what the character of that understanding will be once obtained. For the fundamental question is, Do scientists (at least sometimes, even in "revolutionary" episodes) proceed as they do because there are objective reasons for doing so, or do we call those procedures "reasonable" merely because a certain group sanctions them? Despite the ambiguities and inconsistencies of many of his remarks, Kuhn's tendency is clearly toward the latter alternative. Though occasionally tentative ("Some of the principles deployed in my explanation of science are irreducibly sociological, at least at this time"—*Criticism*, p. 237), in most passages he asserts his view categorically: "The explanation [of scientific progress] must, in the final analysis, be psychological or sociological. . . . I doubt that there is another sort of answer to be found" (*Criticism*, p. 21). "Whatever scientific progress may be, we must account for it by examining the nature of the scientific group, discovering what it values, what it tolerates, and what it disdains. That position is intrinsically sociological" (*Criticism*, p. 238). We must study scientific communities not as one of several steps in clarifying the nature of science (in attempting, say, to separate the irrational from the rational components as a prelude to analyzing the latter); it is the *only* step. What the community says is rational, scientific, is so; beyond this, there is no answer to be found. An alternative to this view is to think of sociology as able to bring to our attention the kinds of biases which scientists should learn to avoid, as interferences, hindrances to good scientific judgment. For Kuhn, however, such biases are an integral, and indeed the central, aspect of science. The point I have tried to make is not merely that Kuhn's is a view which denies the objectivity and rationality of the scientific enterprise; I have tried to show that the arguments by which Kuhn arrives at his conclusion are unclear and unsatisfactory.

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The Underpinnings of the Chemical Revolution

Atoms and Powers. An Essay on Newtonian Matter-Theory and the Development of Chemistry. ARNOLD THACKRAY. Harvard University Press, Cambridge, Mass., 1970. xxvi, 326 pp., illus. $12. Harvard Monographs in the History of Science.

This stimulating and suggestive essay ranges over that most important century of chemical history, the 18th. Most of the significant and familiar figures are discussed—Stahl, Boerhaave, Hales, Black, Priestley, Maquer, Lavoisier, Dalton—but in what will be an unfamilar setting to most readers. The trials of phlogiston theory and the triumphs of pneumatic chemistry do not loom large in this book. Rather, the author has sought to delve into the more fundamental presuppositions about matter theory which 18th-century natural philosophers and chemists debated and which formed part of the theoretical underpinning for the more spectacular episodes of the chemical revolution. This then is not a positivistic account of chemical discovery, but an essay that seeks to illuminate the place of chemistry in some of the major scientific and intellectual currents of the 18th century. Such approaches to the history of chemistry have been and still are exceedingly rare, and one must therefore accord a special welcome to Thackray's book.

The aspect of 18th-century chemical theory that Thackray has sought to explore is the impact and influence of Sir Isaac Newton's speculations on the nature of matter. It is no easy task to unravel the strands of Newtonian matter theory, for in this area the master's legacy was far from definitive or even consistent. Newton's own views were subject to some modifications throughout his lifetime, dependent upon such varied factors as the state of his researches on light and colors, the Cartesian and Leibnitzian criticisms of his natural philosophy, and not least his own heterodox theological beliefs. This provided his faithful disciples with ample scope for individual interpretation of the canonical corpus. Out of their mélange of text and gloss, Thackray has isolated three beliefs as fundamental to orthodox Newtonian matter theory in the 18th century: first, that matter was inertially homogeneous and internally structured (that is, that matter was ultimately composed of particles of identical solid matter defined inertially and that the qualitative differences of bulk matter were due to the different spatial arrangement of these fundamental particles or atoms); second, the acceptance of attractive and repulsive forces as the proper categories of explanation in a discussion of chemical change; and third, a belief in an all-pervading ether. Although Thackray points up the importance of ethereal concepts in 18th-century chemistry from Boerhaave's "matter of fire" through Hales's "air" to Dalton's "caloric," it is the aspect of the Newtonian legacy he explores least, thereby depriving his book (and his reader) of a full discussion of one of the most important themes of 18th-century chemistry.

By contrast, Thackray devotes much of his book to the influence of the other aspects of Newton's matter theory, namely the belief in an atomic structure of matter and the acceptance of interparticle forces. He follows the fate of these views from the early eagerness of Newton's immediate disciples, most notably the Keill brothers and John Freind, to reduce chemistry to a set of laws for the short-range forces operative between the constituent particles of matter, to the much later attempts to quantify the forces of chemical affinity by a more empirical approach as exemplified in the work of such later French chemists as Macquer, Guyton de Morveau, Fourcroy, and Berthollet, perhaps the last of great Newtonian visionaries in chemistry. Thackray has some interesting sugges-