THE STRUCTURE OF SCIENTIFIC REVOLUTIONS

THIS important book[1] is a sustained attack on the prevailing image of scientific change as a linear process of ever-increasing knowledge, and an attempt to make us see that process of change in a different and, Kuhn suggests, more enlightening way. In attacking the "concept of development-by-accumulation," Kuhn presents numerous penetrating criticisms not only of histories of science written from that point of view, but also of certain philosophical doctrines (mainly Baconian and positivistic philosophies of science, particularly verification, falsification, and probabilistic views of the acceptance or rejection of scientific theories) which he convincingly argues are associated with that view of history. In this review, I will not deal with those criticisms or with the details of the valuable case studies with which Kuhn tries to support his views; rather, I will concentrate on certain concepts and doctrines which are fundamental to his own interpretation of the development and structure of science. His view, while original and richly suggestive, has much in common with some recent antipositivistic reactions among philosophers of science—most notably, Feyerabend, Hanson, and Toulmin—and inasmuch as it makes explicit, according to Kuhn, "some of the new historiography's implications" (p. 3), it is bound to exert a very wide influence among philosophers and historians of science alike. It is therefore a view which merits close examination.

Basic to Kuhn's interpretation of the history of science is his notion of a paradigm. Paradigms are "universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners" (p. x). Because a paradigm is "at the start largely a promise of success discoverable in selected and still incomplete examples" (pp. 23-24), it is "an object for further articulation and specification under new or more stringent conditions" (p. 23); hence from paradigms "spring particular coherent traditions of scientific research" (p. 10) which Kuhn calls "normal science." Normal science thus consists largely of "mopping-up operations" (p. 24) devoted to actualizing the initial promise of the paradigm "by

[1] *The Structure of Scientific Revolutions.* By Thomas S. Kuhn. (Chicago, University of Chicago Press, 1962. Pp. xiv, 172.) All page references, unless otherwise noted, are to this work.

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 this process of paradigm development lie both the strength and weakness of normal science: for though the paradigm provides "a criterion for choosing problems that, while the paradigm is taken for granted, can be assumed to have solutions" (p. 37), on the other hand those phenomena "that will not fit the box are often not seen at all" (p. 24). Normal science even "often suppresses fundamental novelties because they are necessarily subversive of its basic commitments. Nevertheless, so long as those commitments retain an element of the arbitrary, the very nature of normal research ensures that novelty shall not be suppressed for very long" (p. 5). Repeated failures of a normal-science tradition to solve a problem or other anomalies that develop in the course of paradigm articulation produce "the tradition-shattering complements to the tradition-bound activity of normal science" (p. 6).

The most pervasive of such tradition-shattering activities Kuhn calls "scientific revolutions."

> 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 [p. 90].
> 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 [p. 91].

The upshot of such crises is often the acceptance of a new paradigm:

> Scientific revolutions are here taken to be those non-cumulative developmental episodes in which an older paradigm is replaced in whole or in part by an incompatible new one [p. 91].

This interpretation of scientific development places a heavy burden indeed on the notion of a paradigm. Although in some passages we are led to believe that a community's paradigm is simply "a set of recurrent and quasi-standard illustrations of various theories," and that these are "revealed in its textbooks, lectures, and laboratory exercises" (p. 43), elsewhere we find that there is far more to the paradigm than is contained, at least explicitly, in such illustrations. These "accepted

examples of actual scientific practice . . . include law, theory, application, and instrumentation together" (p. 10). A paradigm consists of a "strong network of commitments—conceptual, theoretical, instrumental, and methodological" (p. 42); among these commitments are "quasi-metaphysical" ones (p. 41). A paradigm is, or at least includes, "some implicit body of intertwined theoretical and methodological belief that permits selection, evaluation, and criticism" (pp. 16-17). If such a body of beliefs is not implied by the collection of facts (and, according to Kuhn, it never is), "it must be externally supplied, perhaps by a current metaphysic, by another science, or by personal and historical accident" (p. 17). Sometimes paradigms seem to be patterns (sometimes in the sense of archetypes and sometimes in the sense of criteria or standards) upon which we model our theories or other work ("from them as models spring particular coherent traditions"); at other times they seem to be themselves vague theories which are to be refined and articulated. Most fundamentally, though, Kuhn considers them as not being rules, theories, or the like, or a mere sum thereof, but something more "global" (p. 43), from which rules, theories, and so forth are abstracted, but to which no mere statement of rules or theories or the like can do justice. The term "paradigm" thus covers a range of factors in scientific development including or somehow involving laws and theories, models, standards, and methods (both theoretical and instrumental), vague intuitions, explicit or implicit metaphysical beliefs (or prejudices). In short, anything that allows science to accomplish anything can be a part of (or somehow involved in) a paradigm.

Now, historical study does bear out the existence of guiding factors which are held in more or less similar form, to greater or less extent, by a multitude of scientists working in an area over a number of years. What must be asked is whether anything is gained by referring to such common factors as "paradigms," and whether such gains, if any, are offset by confusions that ensue because of such a way of speaking. At the very outset, the explanatory value of the notion of a paradigm is suspect: for the truth of the thesis that shared paradigms are (or are behind) the common factors guiding scientific research appears to be guaranteed, not so much by a close examination of actual historical cases, however scholarly, as by the breadth of definition of the term "paradigm." The suspicion that this notion plays a determinative role in shaping Kuhn's interpretation of history is strengthened by his frequent remarks about what *must* be the case with regard to science and its development: for example, "No natural history can be inter-

preted in the absence of at least some . . . belief" (pp. 16-17); "Once a first paradigm through which to view nature has been found, there is no such thing as research in the absence of any paradigm" (p. 79); "no experiment can be conceived without some sort of theory" (p. 87); "if, as I have already urged, there can be no scientifically or empirically neutral system of language or concepts, then the proposed construction of alternate tests and theories must proceed from within one or another paradigm-based tradition" (p. 145). Such views appear too strongly and confidently held to have been extracted from a mere investigation of how things *have* happened.

Still greater perplexities are generated by Kuhn's view that paradigms cannot, in general, be formulated adequately. According to him, when the historian tries to state the rules which scientists follow, he finds that "phrased in just that way, or in any other way he can imagine, they would almost certainly have been rejected by some members of the group he studies" (p. 44). Similarly, there may be many versions of the same theory. It would appear that, in Kuhn's eyes, the concepts, laws, theories, rules, and so forth that are common to a group are just not common enough to guarantee the coherence of the tradition; therefore he concludes that the paradigm, "the concrete scientific achievement" that is the source of that coherence, must not be identified with, but must be seen as "prior to the various concepts, laws, theories, and points of view that may be abstracted from it" (p. 11). (It is partly on the basis of this argument that Kuhn rejects the attempt by philosophers of science to formulate a "logic" of science in terms of precise rules.) Yet if it is true that all that can be said about paradigms and scientific development can and must be said only in terms of what are mere "abstractions" from paradigms, then it is difficult to see what is gained by appealing to the notion of a paradigm.

In Kuhn's view, however, the fact that paradigms cannot be described adequately in words does not hinder us from recognizing them: they are open to "direct inspection" (p. 44), and historians can "agree in their *identification* of a paradigm without agreeing on, or even attempting to produce, a full *interpretation* or *rationalization* of it" (p. 44). Yet the feasibility of a historical inquiry concerning paradigms is exactly what is brought into question by the scope of the term "paradigm" and the inaccessibility of particular paradigms to verbal formulation. For on the one hand, as we have seen, it is *too* easy to identify a paradigm; and on the other hand, it is not easy to determine, in particular cases treated by Kuhn, what the paradigm is supposed to have been in that case. In most of the cases he discusses, it is the theory

that is doing the job of posing problems, providing criteria for selection of data, being articulated, and so forth. But of course the theory is not the paradigm, and we might assume that Kuhn discusses the theory because it is as near as he can get in words to the inexpressible paradigm. This, however, only creates difficulties. In the case of "what is perhaps our fullest example of a scientific revolution" (p. 132), for instance, what was "assimilated" when Dalton's theory (paradigm) became accepted? Not merely the laws of combining proportions, presumably, but something "prior to" them. Was it, then, the picture of matter as constituted of atoms? But contrary to the impression Kuhn gives, that picture was never even nearly universally accepted: from Davy to Ostwald and beyond there was always a very strong faction which "regarded it with misgiving, or with positive dislike, or with a constant hope for an effective substitute" (J. C. Gregory, *A Short History of Atomism* [London, 1931], p. 93), some viewing atoms as convenient fictions, others eschewing the vocabulary of atoms entirely, preferring to talk in terms of "proportions" or "equivalents." (It is noteworthy that Dalton was presented with a Royal Medal, not unequivocally for his development of the atomic theory, but rather "for his development of the Theory of Definite Proportions, usually called the Atomic Theory of Chemistry"; award citation, quoted in Gregory, p. 84.) No, it was certainly not atoms to which the most creative chemists of the century were "committed"—unless (contrary to his general mode of expression) Kuhn means that they were "committed" to the atomic theory because they—most of them—used it even though they did not believe in its truth. Further, what else was "intertwined" in this behind-the-scenes paradigm? Did it include, for instance, some inexpressible Principle of Uniformity of Nature or Law of Causality? Is this question so easy to answer—a matter of "direct inspection"—after all these years of philosophical dispute? One begins to doubt that paradigms are open to "direct inspection," or else to be amazed at Professor Kuhn's eyesight. (And why is it that such historical facts should be open to direct inspection, whereas scientific facts must always be seen "through" a paradigm?) But if there are such difficulties, how can historians know that they agree in their identification of the paradigms present in historical episodes, and so determine that "the same" paradigm persists through a long sequence of such episodes? They cannot, by hypothesis, compare their formulations. Suppose they disagree: how is their dispute to be resolved?

On the other hand, where do we draw the line between different paradigms and different articulations of the same paradigm? It is

natural and common to say that Newton, d'Alembert, Lagrange, Hertz, Hamilton, Mach, and others formulated different versions of classical mechanics; yet certainly some of these formulations involved different "commitments"—for example, some to forces, others to energy, some to vectorial, others to variational principles. The distinction between paradigms and different articulations of a paradigm, and between scientific revolutions and normal science, is at best a matter of degree, as is commitment to a paradigm: expression of explicit discontent, proliferation of competing articulations, debate over fundamentals are all more or less present throughout the development of science; and there are always guiding elements which are more or less common, even among what are classified as different "traditions." This is one reason why, in particular cases, identification of "the paradigm" is so difficult: not just because it is hard to see, but because looking for the guiding elements in scientific activity is not like looking for a unitary entity that either is there or is not.

But furthermore, the very reasons for supposing that paradigms (nevertheless) exist are unconvincing. No doubt some theories are very similar—so similar that they can be considered to be "versions" or "different articulations" of one another (or of "the same subject"). But does this imply that there must be a common "paradigm" of which the similar theories are incomplete expressions and from which they are abstracted? No doubt, too, many expressions of methodological rules are not as accurate portrayals of scientific method as they are claimed to be; and it is possible that Kuhn is right in claiming that no such portrayal can be given in terms of any one set of precise rules. But such observations, even if true, do not compel us to adopt a *mystique* regarding a single paradigm which guides procedures, any more than our inability to give a single, simple definition of "game" means that we must have a unitary but inexpressible idea from which all our diverse uses of "game" are abstracted. It may be true that "The coherence displayed by the research tradition . . . may not imply even the existence of an underlying body of rules and assumptions" (p. 46); but neither does it imply the existence of an underlying "paradigm."

Finally, Kuhn's blanket use of the term "paradigm" to cover such a variety of activities and functions obscures important differences between those activities and functions. For example, Kuhn claims that "an apparently arbitrary element . . . is always a formative ingredient" (p. 4) of a paradigm; and, indeed, as we shall see shortly, this is a central aspect of his view of paradigms and scientific change. But is the acceptance or rejection of a scientific theory "arbitrary" in the same

sense that acceptance or rejection of a standard (to say nothing of a metaphysical belief) is? Again, Newtonian and Hertzian formulations of classical mechanics are similar to one another, as are the Einstein, Whitehead, Birkhoff, and Milne versions of relativity, and as are wave mechanics and matrix mechanics. But there are significant differences in the ways in and degrees to which these theories are "similar"—differences which are masked by viewing them all equally as different articulations of the same paradigm.

There are, however, deeper ways in which Kuhn's notion of a paradigm affects adversely his analysis of science; and it is in these ways that his view reflects widespread and important tendencies in both the history and philosophy of science today.

Because a paradigm is

> the source of the methods, problem-field, and standards of solution accepted by any mature scientific community at any given time, . . . the reception of a new paradigm often necessitates a redefinition of the corresponding science. . . . And as the problems change, so, often, does the standard that distinguishes a real scientific solution from a mere metaphysical speculation, word game, or mathematical play. 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].

Thus the paradigm change entails "changes in the standards governing permissible problems, concepts, and explanations" (p. 105). In connection with his view that concepts or meanings change from one theory (paradigm) to another despite the retention of the same terms, Kuhn offers an argument whose conclusion is both intrinsically important and crucial to much of his book. This argument is directed against the "positivistic" view that scientific advance is cumulative, and that therefore earlier sciences are derivable from later; the case he considers is the supposed deducibility of Newtonian from Einsteinian dynamics, subject to limiting conditions. After summarizing the usual derivation, Kuhn objects that

> the derivation is spurious, at least to this point. Though the [derived statements] are a special case of the laws of relativistic mechanics, they are not Newton's Laws. Or at least they are not unless those laws are reinterpreted in a way that would have been impossible until after Einstein's work. . . . The physical referents of these Einsteinian concepts are by no means identical with those of the Newtonian concepts that bear the same name. (Newtonian mass is conserved; Einsteinian is convertible with energy. Only at low relative velocities may the two be measured in the same way, and even then they must not be conceived to be the same.) . . . The argument has still not done what

it purported to do. It has not, that is, shown Newton's Laws to be a limiting case of Einstein's. For in the passage to the limit it is not only the forms of the laws that have changed. Simultaneously we have had to alter the fundamental structural elements of which the universe to which they apply is composed [pp. 100-101].

But Kuhn's argument amounts simply to an assertion that despite the derivability of expressions which are in every formal respect identical with Newton's Laws, there remain differences of "meaning." What saves this from begging the question at issue? His only attempt to support his contention comes in the parenthetical example of mass; but this point is far from decisive. For one might equally well be tempted to say that the "concept" of mass (the "meaning" of "mass") has remained the same (thus accounting for the deducibility) even though the *application* has changed. Similarly, rather than agree with Kuhn that "the Copernicans who denied its traditional title 'planet' to the sun . . . were changing the meaning of 'planet' " (p. 127), one might prefer to say that they changed only the application of the term. The real trouble with such arguments arises with regard to the cash difference between saying, in such cases, that the "meaning" has changed, as opposed to saying that the "meaning" has remained the same though the "application" has changed. Kuhn has offered us no clear analysis of "meaning" or, more specifically, no criterion of change of meaning; consequently it is not clear why he classifies such changes as changes of meaning rather than, for example, as changes of application. This is not to say that no such criterion could be formulated, or that a distinction between change of meaning and change of application could not be made, or that it might not be very profitable to do so for certain purposes. One might, for example, note that there are statements that can be made, questions that can be raised, views that may be suggested as possibly correct, within the context of Einsteinian physics that would not even have made sense—would have been self-contradictory —in the context of Newtonian physics. And such differences might (for certain purposes) be referred to with profit as changes of meaning, indicating, among other things, that there are differences between Einsteinian and Newtonian terms that are not brought out by the deduction of Newtonian-like statements from Einsteinian ones. But attributing such differences to alterations of "meaning" must not blind one to any resemblances there might be between the two sets of terms. Thus it is not so much Kuhn's conclusion that is objectionable as, first, the fact that it is based, not on any solid argument, but on the feature of meaning dependence which Kuhn has built into the term

"paradigm" (scientists see the world from different points of view, through different paradigms, and therefore see different things through different paradigms); and second, the fact that this feature leads him to a distorted portrayal of the relations between different scientific theories. For Kuhn's term "paradigm," incorporating as it does the view that statements of fact are (to use Hanson's expression) theory-laden, and as a consequence the notion of (in Feyerabend's words) meaning variance from one theory or paradigm to another, calls attention excessively to the differences between theories or paradigms, so that relations that evidently do exist between them are in fact passed over or denied.

The significance of this point emerges fully when we ask about the grounds for accepting one paradigm as better than another. For if "the differences between successive paradigms are both necessary and irreconcilable" (p. 102), and if those differences consist in the paradigms' being "incommensurable"—if they disagree as to what the facts are, and even as to the real problems to be faced and the standards which a successful theory must meet—then what are the two paradigms disagreeing about? And why does one win? There is little problem for Kuhn in analyzing the notion of progress within a paradigm tradition (and, indeed, he notes, such evolution is the source of the prevailing view of scientific advance as "linear"); but how can we say that "progress" is made when one paradigm replaces another? The logical tendency of Kuhn's position is clearly toward the conclusion that the replacement is not cumulative, but is mere change: being "incommensurable," two paradigms cannot be judged according to their ability to solve the same problems, or deal with the same facts, or meet the same standards. "If there were but one set of scientific problems, one world within which to work on them, and one set of standards for their solution, paradigm competition might be settled more or less routinely. . . . But . . . The proponents of competing paradigms are always at least slightly at cross-purposes" (pp. 146-147). Hence "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). In fact, in so far as one can compare the weights of evidence of two competing paradigms—and, on Kuhn's view that after a scientific revolution "the whole network of fact and theory . . . has shifted" (p. 140), one must wonder how this can be done at all—the weight of evidence is more often in favor of the older paradigm than the new (pp. 155-156). "What occurred 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 truth nor error is at issue" (p. 150); indeed, Kuhn's view of the history of science implies that "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).

Kuhn is well aware of the relativism implied by his view, and his common sense and feeling for history make him struggle mightily to soften the dismal conclusion. It is, for instance, only "often" that the reception of a new paradigm necessitates a redefinition of the corresponding science. Proponents of different paradigms are only "at least partially" at cross-purposes. Though they "see different things when they look from the same point in the same direction," this is "not to say that they can see anything they please. Both are looking at the world" (p. 149). It is only "in some areas" that "they see different things" (p. 149). But these qualifications are more the statement of the problems readers will find with Kuhn's views than the solutions of those problems. And it is small comfort to be told, in the closing pages of the book, that "a sort of progress will inevitably characterize the scientific enterprise" (p. 169), especially if that "progress," whether or not it is aimed toward final truth, is not at least an advance over past error. Nor will careful readers feel reassured when they are asked, rhetorically, "What better criterion [of scientific progress] than the decision of the scientific group could there be" (p. 169)? For Kuhn has already told us that the decision of a scientific group to adopt a new paradigm is not based on good reasons; on the contrary, what counts as a good reason is determined by the decision.

A view such as Kuhn's had, after all, to be expected sooner or later from someone versed in the contemporary treatment of the history of science. For the great advances in that subject since Duhem have shown how much more there was to theories that were supposedly overthrown and superseded than had been thought. Historians now find that "the more carefully they study, say, Aristotelian dynamics, phlogistic chemistry, or caloric thermodynamics, the more certain they feel that those once current views of nature were, as a whole, neither less scientific nor more the product of human idiosyncrasy than those current today" (p. 2). Yet perhaps that deep impression has effected too great a reaction; for that there is more to those theories than was once thought does not mean that they are immune to criticism —that there are not *good* reasons for their abandonment and replacement by others. And while Kuhn's book calls attention to many mistakes that have been made regarding the (good) reasons for scien-

tific change, it fails itself to illuminate those reasons, and even obscures the existence of such reasons. We must, as philosophers of science, shape our views of the development and structure of scientific thought in the light of what we learn from science and its history. But until historians of science achieve a more balanced approach to their subject—neither too positivistic nor too relativistic—philosophers must receive such presentations of evidence with extremely critical eyes.

Certainly there is a vast amount of positive value in Kuhn's book. Besides making many valid critical remarks, it does bring out, through a wealth of case studies, many common features of scientific thought and activities which make it possible and, for many purposes, revealing to speak of "traditions" in science; and it points out many significant differences between such traditions. But Kuhn, carried away by the logic of his notion of a paradigm, glosses over many important differences between scientific activities classified as being of the same tradition, as well as important continuities between successive traditions. He is thus led to deny, for example, that Einsteinian dynamics is an advance over Newtonian or Aristotelian dynamics in a sense more fundamental than can consistently be extracted from his conceptual apparatus. If one holds, without careful qualification, that the world is seen and interpreted "through" a paradigm, or that theories are "incommensurable," or that there is "meaning variance" between theories, or that all statements of fact are "theory-laden," then one may be led all too readily into relativism with regard to the development of science. Such a view is no more implied by historical facts than is the opposing view that scientific development consists solely of the removal of superstition, prejudice, and other obstacles to scientific progress in the form of purely incremental advances toward final truth. Rather, I have tried to show, such relativism, while it may seem to be suggested by a half-century of deeper study of discarded theories, is a *logical* outgrowth of conceptual confusions, in Kuhn's case owing primarily to the use of a blanket term. For his view is made to appear convincing only by inflating the definition of "paradigm" until that term becomes so vague and ambiguous that it cannot easily be withheld, so general that it cannot easily be applied, so mysterious that it cannot help explain, and so misleading that it is a positive hindrance to the understanding of some central aspects of science; and then, finally, these excesses must be counterbalanced by qualifications that simply contradict them. There are many other facets of Kuhn's book that deserve attention—especially his view that a paradigm "need not,

and in fact never does, explain all the facts with which it can be confronted" (p. 18), and his suggestion that no paradigm ever could be found which would do so. But the difficulties that have been discussed here indicate clearly that the expanded version of this book which Kuhn contemplates will require not so much further historical evidence (p. xi) as—at the very least—more careful scrutiny of his tools of analysis.

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