**Thomas S. Kuhn (1996) The Structure of Scientific Revolutions**
**Third Edition. The University of Chicago Press, Chicago and London.**

## XI. The Invisibility of Revolutions

We must still ask how scientific revolutions close. Before doing so, however, a last attempt to reinforce conviction about their existence and nature seems called for. I have so far tried to display revolutions by illustration, and the examples could be multiplied ad nauseam. But clearly, most of them, which were deliberately selected for their familiarity, have customarily been viewed not as revolutions but as additions to scientific knowledge. That same view could equally well be taken of any additional illustrations, and these would probably be ineffective. I suggest that there are excellent reasons why revolutions have proved to be so nearly invisible. Both scientists and laymen take much of their image of creative scientific activity from an authoritative source that systematically disguises—partly for important functional reasons—the existence and significance of scientific revolutions. Only when the nature of that authority is recognized and analyzed can one hope to make historical example fully effective. Furthermore, though the point can be fully developed only in my concluding section, the analysis now required will begin to indicate one of the aspects of scientific work that most clearly distinguishes it from every other creative pursuit except perhaps theology.

As the source of authority, I have in mind principally textbooks of science together with both the popularizations and the philosophical works modelled on them. All three of these categories—until recently no other significant sources of information about science have been available except through the practice of research—have one thing in common. They address themselves to an already articulated body of problems, data, and theory, most often to the particular set of paradigms to which the scientific community is committed at the time they are written. Textbooks themselves aim to communicate the vocabulary and syntax of a contemporary scientific language. Popularizations attempt to describe these same applications in a language closer to that of everyday life. And philosophy of science, particularly that of the English-speaking world, analyzes the logical structure of the same completed body of scientific knowledge. Though a fuller treatment would necessarily deal with the very real distinctions between these three genres, it is their similarities that most concern us here. All three record the stable outcome of past revolutions and thus display the bases of the current normal-scientific tradition. To fulfill their function they need not provide authentic information about the way in which those bases were first recognized and then embraced by the profession. In the case of textbooks, at least, there are even good reasons why, in these matters, they should be systematically misleading.

We noted in Section II that an increasing reliance on textbooks or their equivalent was an invariable concomitant of the emergence of a first paradigm in any field of science. The concluding section of this essay will argue that the domination of a mature science by such texts significantly differentiates its developmental pattern from that of other fields. For the moment let us simply take it for granted that, to an extent unprecedented in other fields, both the layman's and the practitioner's knowledge of science is based on textbooks and a few other types of literature derived from them. Textbooks, however, being pedagogic vehicles for the perpetuation of normal science, have to be rewritten in

whole or in part whenever the language, problem-structure, or standards of normal science change. In short, they have to be rewritten in the aftermath of each scientific revolution, and, once rewritten, they inevitably disguise not only the role but the very existence of the revolutions that produced them. Unless he has personally experienced a revolution in his own lifetime, the historical sense either of the working scientist or of the lay reader of textbook literature extends only to the outcome of the most recent revolutions in the field.

Textbooks thus begin by truncating the scientist's sense of his discipline's history and then proceed to supply a substitute for what they have eliminated. Characteristically, textbooks of science contain just a bit of history, either in an introductory chapter or, more often, in scattered references to the great heroes of an earlier age. From such references both students and professionals come to feel like participants in a long-standing historical tradition. Yet the textbook-derived tradition in which scientists come to sense their participation is one that, in fact, never existed. For reasons that are both obvious and highly functional, science textbooks (and too many of the older histories of science) refer only to that part of the work of past scientists that can easily be viewed as contributions to the statement and solution of the texts' paradigm problems. Partly by selection and partly by distortion, the scientists of earlier ages are implicitly represented as having worked upon the same set of fixed problems and in accordance with the same set of fixed canons that the most recent revolution in scientific theory and method has made seem scientific. No wonder that textbooks and the historical tradition they imply have to be rewritten after each scientific revolution. And no wonder that, as they are rewritten, science once again comes to seem largely cumulative.

Scientists are not, of course, the only group that tends to see its discipline's past developing linearly toward its present vantage. The temptation to write history backward is both omnipresent and perennial. But scientists are more affected by the temptation to rewrite history, partly because the results of scientific research show no obvious dependence upon the historical context of the inquiry, and partly because, except during crisis and revolution, the scientist's contemporary position seems so secure. More historical detail, whether of science's present or of its past, or more responsibility to the historical details that are presented, could only give artificial status to human idiosyncrasy, error, and confusion. Why dignify what science's best and most persistent efforts have made it possible to discard? The depreciation of historical fact is deeply, and probably functionally, ingrained in the ideology of the scientific profession, the same profession that places the highest of all values upon factual details of other sorts. Whitehead caught the unhistorical spirit of the scientific community when he wrote, "A science that hesitates to forget its founders is lost." Yet he was not quite right, for the sciences, like other professional enterprises, do need their heroes and do preserve their names. Fortunately, instead of forgetting these heroes, scientists have been able to forget or revise their works.

The result is a persistent tendency to make the history of science look linear or cumulative, a tendency that even affects scientists looking back at their own research. For example, all three of Dalton's incompatible accounts of the development of his chemical atomism make it appear that he was interested from an early date in just those chemical problems of combining proportions that he was later famous for having solved. Actually those problems seem only to have occurred to him with their

solutions, and then not until his own creative work was very nearly complete.[1] What all of Dalton's accounts omit are the revolutionary effects of applying to chemistry a set of questions and concepts previously restricted to physics and meteorology. That is what Dalton did, and the result was a reorientation toward the field, a reorientation that taught chemists to ask new questions about and to draw new conclusions from old data.

Or again, Newton wrote that Galileo had discovered that the constant force of gravity produces a motion proportional to the square of the time. In fact, Galileo's kinematic theorem does take that form when embedded in the matrix of Newton's own dynamical concepts. But Galileo said nothing of the sort. His discussion of falling bodies rarely alludes to forces, much less to a uniform gravitational force that causes bodies to fall.[2] By crediting to Galileo the answer to a question that Galileo's paradigms did not permit to be asked, Newton's account hides the effect of a small but revolutionary reformulation in the questions that scientists asked about motion as well as in the answers they felt able to accept. But it is just this sort of change in the formulation of questions and answers that accounts, far more than novel empirical discoveries, for the transition from Aristotelian to Galilean and from Galilean to Newtonian dynamics. By disguising such changes, the textbook tendency to make the development of science linear hides a process that lies at the heart of the most significant episodes of scientific development.

The preceding examples display, each within the context of a single revolution, the beginnings of a reconstruction of history that is regularly completed by post-revolutionary science texts. But in that completion more is involved than a multiplication of the historical misconstructions illustrated above. Those misconstructions render revolutions invisible; the arrangement of the still visible material in science texts implies a process that, if it existed, would deny revolutions a function. Because they aim quickly to acquaint the student with what the contemporary scientific community thinks it knows, textbooks treat the various experiments, concepts, laws, and theories of the current normal science as separately and as nearly seriatim as possible. As pedagogy this technique of presentation is unexceptionable. But when combined with the generally unhistorical air of science writing and with the occasional systematic misconstructions discussed above, one strong impression is overwhelmingly likely to follow: science has reached its present state by a series of individual discoveries and inventions that, when gathered together, constitute the modern body of technical knowledge. From the beginning of the scientific enterprise, a textbook presentation implies, scientists have striven for the particular objectives that are embodied in today's paradigms. One by one, in a process often compared to the addition of bricks to a building, scientists have added another fact, concept, law, or theory to the body of information supplied in the contemporary science text.

But that is not the way a science develops. Many of the puzzles of contemporary normal science did not exist until after the most recent scientific revolution. Very few of them can be traced back to the historic beginning of the science within which they now occur. Earlier generations pursued their own problems with their own instruments and their own canons of solution. Nor is it just the problems that have changed. Rather the whole network of fact and theory that the textbook paradigm fits to nature has shifted. Is the constancy of chemical composition, for example, a mere fact of experience that chemists could have discovered by experiment within any one of the worlds within which

chemists have practiced? Or is it rather one element—and an indubitable one, at that—in a new fabric of associated fact and theory that Dalton fitted to the earlier chemical experience as a whole, changing that experience in the process? Or by the same token, is the constant acceleration produced by a constant force a mere fact that students of dynamics have always sought, or is it rather the answer to a question that first arose only within Newtonian theory and that that theory could answer from the body of information available before the question was asked?

These questions are here asked about what appear as the piecemeal-discovered facts of a textbook presentation. But obviously, they have implications as well for what the text presents as theories. Those theories, of course, do "fit the facts," but only by transforming previously accessible information into facts that, for the preceding paradigm, had not existed at all. And that means that theories too do not evolve piecemeal to fit facts that were there all the time. Rather, they emerge together with the facts they fit from a revolutionary reformulation of the preceding scientific tradition, a tradition within which the knowledge-mediated relationship between the scientist and nature was not quite the same.

One last example may clarify this account of the impact of textbook presentation upon our image of scientific development. Every elementary chemistry text must discuss the concept of a chemical element. Almost always, when that notion is introduced, its origin is attributed to the seventeenth-century chemist, Robert Boyle, in whose *Sceptical Chymist* the attentive reader will find a definition of 'element' quite close to that in use today. Reference to Boyle's contribution helps to make the neophyte aware that chemistry did not begin with the sulfa drugs; in addition, it tells him that one of the scientist's traditional tasks is to invent concepts of this sort. As a part of the pedagogic arsenal that makes a man a scientist, the attribution is immensely successful. Never-the-less, it illustrates once more the pattern of historical mistakes that misleads both students and laymen about the nature of the scientific enterprise.

According to Boyle, who was quite right, his "definition" of an element was no more than a paraphrase of a traditional chemical concept; Boyle offered it only in order to argue that no such thing as a chemical element exists; as history, the textbook version of Boyle's contribution is quite mistaken.[3] That mistake, of course, is trivial, though no more so than any other misrepresentation of data. What is not trivial, however, is the impression of science fostered when this sort of mistake is first compounded and then built into the technical structure of the text. Like 'time,' 'energy,' 'force,' or 'particle,' the concept of an element is the sort of textbook ingredient that is often not invented or discovered at all. Boyle's definition, in particular, can be traced back at least to Aristotle and forward through Lavoisier into modern texts. Yet that is not to say that science has possessed the modern concept of an element since antiquity. Verbal definitions like Boyle's have little scientific content when considered by themselves. They are not full logical specifications of meaning (if there are such), but more nearly pedagogic aids. The scientific concepts to which they point gain full significance only when related, within a text or other systematic presentation, to other scientific concepts, to manipulative procedures, and to paradigm applications. It follows that concepts like that of an element can scarcely be invented independent of context. Furthermore, given the context, they rarely require invention because they are already at hand. Both Boyle and Lavoisier changed the

chemical significance of 'element' in important ways. But they did not invent the notion or even change the verbal formula that serves as its definition. Nor, as we have seen, did Einstein have to invent or even explicitly redefine 'space' and 'time' in order to give them new meaning within the context of his work.

What then was Boyle's historical function in that part of his work that includes the famous "definition"? He was a leader of a scientific revolution that, by changing the relation of 'element' to chemical manipulation and chemical theory, transformcd the notion into a tool quite different from what it had been before and transformed both chemistry and the chemist's world in the process.[4] Other revolutions, including the one that centers around Lavoisier, were required to give the concept its modern form and function. But Boyle provides a typical example both of the process involved at each of these stages and of what happens to that process when existing knowledge is embodied in a textbook. More than any other single aspect of science, that pedagogic form has determined our image of the nature of science and of the role of discovery and invention in its advance.

**Thomas Kuhn's Footnotes**

1. L. K. Nash, "The Origins of Dalton's Chemical Atomic Theory," Isis, XLVII (1956), 101-16.
2. For Newton's remark, see Florian Cajori (ed.), Sir Isaac Newton's Mathematical Principles of Natural Philosophy and His System of the World (Berkeley, Calif., 1946), p. 21. The passage should be compared with Galileo's own discussion in his Dialogues concerning Two New Sciences, trans. H. Crew and A. de Salvio (Evanston, Ill., 1946), pp. 154-76.
3. T. S. Kuhn, "Robert Boyle and Structural Chemistry in the Seventeenth Century," Isis, XLIII (1952), 26-29.
4. Marie Boas, in her Robert Boyle and Seventeenth-Century Chemistry (Cambridge, 1958), deals in many places with Boyle's positive contributions to the evolution of the concept of a chemical element.

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## XII. The Resolution of Revolutions

The textbooks we have just been discussing are produced only in the aftermath of a scientific revolution. They are the bases for a new tradition of normal science. In taking up the question of their structure we have clearly missed a step. What is the process by which a new candidate for paradigm replaces its predecessor? Any new interpretation of nature, whether a discovery or a theory, emerges first in the mind of one or a few individuals. It is they who first learn to see science and the world differently, and their ability to make the transition is facilitated by two circumstances that are not common to most other members of their profession. Invariably their attention has been intensely concentrated upon the crisis-provoking problems; usually, in addition, they are men so young or so new to the crisis-ridden field that practice has committed them less deeply than most of their contemporaries to the world view and rules determined by the old paradigm. How are they able, what must they do, to convert the entire profession or the relevant professional subgroup to their way of seeing science and the world? What causes the group to abandon one tradition of normal research in favor of another?

To see the urgency of those questions, remember that they are the only reconstructions the historian can supply for the philosopher's inquiry about the testing, verification, or falsification of established scientific theories. In so far as he is engaged in normal science, the research worker is a solver of puzzles, not a tester of paradigms. Though he may, during the search for a particular puzzle's solution, try out a number of alternative approaches, rejecting those that fail to yield the desired result, he is not testing the paradigm when he does so. Instead he is like the chess player who, with a problem stated and the board physically or mentally before him, tries out various alternative moves in the search for a solution. These trial attempts, whether by the chess player or by thc scientist, are trials only of themselves, not of the rules of the game. They are possible only so long as the paradigm itself is taken for granted. Therefore, paradigm-testing occurs only after persistent failure to solve a noteworthy puzzle has given rise to crisis. And even then it occurs only after the sense of crisis has evoked an alternate candidate for paradigm. In the sciences the testing situation never consists, as puzzle-solving does, simply in the comparison of a single paradigm with nature. Instead, testing occurs as part of the competition between two rival paradigms for the allegiance of the scientific community.

Closely examined, this formulation displays unexpected and probably significant parallels to two of the most popular contemporary philosophical theories about verification. Few philosophers of science still seek absolute criteria for the verification of scientific theories. Noting that no theory can ever be exposed to all possible relevant tests, they ask not whether a theory has been verified but rather about its probability in the light of the evidence that actually exists. And to answer that question one important school is driven to compare the ability of different theories to explain the evidence at hand. That insistence on comparing theories also characterizes the historical situation in which a new theory is accepted. Very probably it points one of the directions in which future discussions of verification should go.

In their most usual forms, however, probabilistic verification theories all have recourse to one or another of the pure or neutral observation-languages discussed in Section X. One probabilistic theory asks that we compare the given scientific theory with all others that might be imagined to fit the same collection of observed data. Another demands the construction in imagination of all the tests that the given scientific theory might conceivably be asked to pass.[1] Apparently some such construction is necessary for the computation of specific probabilities, absolute or relative, and it is hard to see how such a construction can possibly be achieved. 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. Thus restricted it would have no access to all possible experiences or to all possible theories. As a result, probabilistic theories disguise the verification situation as much as they illuminate it. Though that situation does, as they insist, depend upon the comparison of theories and of much widespread evidence, the theories and observations at issue are always closely related to ones already in existence. Verification is like natural selection: it picks out the most viable among the actual alternatives in a particular historical situation. Whether that choice is the best that could have been

made if still other alternatives had been available or if the data had been of another sort is not a question that can usefully be asked. There are no tools to employ in seeking answers to it.

A very different approach to this whole network of problems has been developed by Karl R. Popper who denies the existence of any verification procedures at all.[2] Instead, he emphasizes the importance of falsification, i.e., of the test that, because its outcome is negative, necessitates the rejection of an established theory. Clearly, the role thus attributed to falsification is much like the one this essay assigns to anomalous experiences, i.e., to experiences that, by evoking crisis, prepare the way for a new theory. Nevertheless, anomalous experiences may not be identified with falsifying ones. Indeed, I doubt that the latter exist. As has repeatedly been emphasized before, no theory ever solves all the puzzles with which it is confronted at a given time; nor are the solutions already achieved often perfect. On the contrary, it is just the incompleteness and imperfection of the existing data-theory fit that, at any time, define many of the puzzles that characterize normal science. If any and every failure to fit were ground for theory rejection, all theories ought to be rejected at all times. On the other hand, if only severe failure to fit justifies theory rejection, then the Popperians will require some criterion of "improbability" or of "degree of falsification." In developing one they will almost certainly encounter the same network of difficulties that has haunted the advocates of the various probabilistic verification theories.

Many of the preceding difficulties can be avoided by recognizing that both of these prevalent and opposed views about the underlying logic of scientific inquiry have tried to compress two largely separate processes into one. Popper's anomalous experience is important to science because it evokes competitors for an existing paradigm. But falsification, though it surely occurs, does not happen with, or simply because of, the emergence of an anomaly or falsifying instance. Instead, it is a subsequent and separate process that might equally well be called verification since it consists in the triumph of a new paradigm over the old one. Furthermore, it is in that joint verification-falsification process that the probabilist's comparison of theories plays a central role. Such a two-stage formulation has, I think, the virtue of great verisimilitude, and it may also enable us to begin explicating the role of agreement (or disagreement) between fact and theory in the verification process. To the historian, at least, it makes little sense to suggest that verification is establishing the agreement of fact with theory. All historically significant theories have agreed with the facts, but only more or less. There is no more precise answer to the question whether or how well an individual theory fits the facts. But questions much like that can be asked when theories are taken collectively or even in pairs. It makes a great deal of sense to ask which of two actual and competing theories fits the facts better. Though neither Priestley's nor Lavoisier's theory, for example, agreed precisely with existing observations, few contemporaries hesitated more than a decade in concluding that Lavoisier's theory provided the better fit of the two.

This formulation, however, makes the task of choosing between paradigms look both easier and more familiar than it is. 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 by some process like counting the number of problems solved by each. But, in fact, these conditions are never met completely. The proponents of competing paradigms are always at least

slightly at cross-purposes. Neither side will grant all the non-empirical assumptions that the other needs in order to make its case. Like Proust and Berthollet arguing about the composition of chemical compounds, they are bound partly to talk through each other. Though each may hope to convert the other to his way of seeing his science and its problems, neither may hope to prove his case. The competition between paradigms is not the sort of battle that can be resolved by proofs.

We have already seen several reasons why the proponents of competing paradigms must fail to make complete contact with each other's viewpoints. Collectively these reasons have been described as the incommensurability of the pre- and post-revolutionary normal-scientific traditions, and we need only recapitulate them briefly here. In the first place, the proponents of competing paradigms will often disagree about the list of problems that any candidate for paradigm must resolve. Their standards or their definitions of science are not the same. Must a theory of motion explain the cause of the attractive forces between particles of matter or may it simply note the existence of such forces? Newton's dynamics was widely rejected because, unlike both Aristotle's and Descartes's theories, it implied the latter answer to the question. When Newton's theory had been accepted, a question was therefore banished from science. That question, however, was one that general relativity may proudly claim to have solved. Or again, as disseminated in the nineteenth century, Lavoisier's chemical theory inhibited chemists from asking why the metals were so much alike, a question that phlogistic chemistry had both asked and answered. The transition to Lavoisier's paradigm had, like the transition to Newton's, meant a loss not only of a permissible question but of an achieved solution. That loss was not, however, permanent either. In the twentieth century questions about the qualities of chemical substances have entered science again, together with some answers to them.

More is involved, however, than the incommensurability of standards. Since new paradigms are born from old ones, they ordinarily incorporate much of the vocabulary and apparatus, both conceptual and manipulative, that the traditional paradigm had previously employed. But they seldom employ these borrowed elements in quite the traditional way. Within the new paradigm, old terms, concepts, and experiments fall into new relationships one with the other. The inevitable result is what we must call, though the term is not quite right, a misunderstanding between the two competing schools. The laymen who scoffed at Einstein's general theory of relativity because space could not be "curved"—it was not that sort of thing—were not simply wrong or mistaken. Nor were the mathematicians, physicists, and philosophers who tried to develop a Euclidean version of Einstein's theory.[3] What had previously been meant by space was necessarily flat, homogeneous, isotropic, and unaffected by the presence of matter. If it had not been, Newtonian physics would not have worked. To make the transition to Einstein's universe, the whole conceptual web whose strands are space, time, matter, force, and so on, had to be shifted and laid down again on nature whole. Only men who had together undergone or failed to undergo that transformation would be able to discover precisely what they agreed or disagreed about. Communication across the revolutionary divide is inevitably partial.

Consider, for another example, the men who called Copernicus mad because he proclaimed that the earth moved. They were not either just wrong or quite wrong. Part of what they meant by 'earth' was fixed position. Their earth, at least, could not be moved. Correspondingly, Copernicus' innovation was not simply to move the earth. Rather, it was a whole new way of regarding the problems of

physics and astronomy, one that necessarily changed the meaning of both 'earth' and 'motion.'[4] Without those changes the concept of a moving earth was mad. On the other hand, once they had been made and understood, both Descartes and Huyghens could realize that the earth's motion was a question with no content for science.[5]

These examples point to the third and most fundamental aspect of the incommensurability of competing paradigms. In a sense that I am unable to explicate further, the proponents of competing paradigms practice their trades in different worlds. One contains constrained bodies that fall slowly, the other pendulums that repeat their motions again and again. In one, solutions are compounds, in the other mixtures. One is embedded in a flat, the other in a curved, matrix of space. Practicing in different worlds, the two groups of scientists see different things when they look from the same point in the same direction. Again, that is not to say that they can see anything they please. Both are looking at the world, and what they look at has not changed. But in some areas they see different things, and they see them in different relations one to the other. That is why a law that cannot even be demonstrated to one group of scientists may occasionally seem intuitively obvious to another. Equally, it is why, before they can hope to communicate fully, one group or the other must experience the conversion that we have been calling a paradigm shift. Just because it is a transition between incommensurables, the transition between competing paradigms cannot be made a step at a time, forced by logic and neutral experience. Like the gestalt switch, it must occur all at once (though not necessarily in an instant) or not at all.

How, then, are scientists brought to make this transposition? Part of the answer is that they are very often not. Copernicanism made few converts for almost a century after Copernicus' death. Newton's work was not generally accepted, particularly on the Continent, for more than half a century after the Principia appeared.[6] Priestley never accepted the oxygen theory, nor Lord Kelvin the electromagnetic theory, and so on. The difficulties of conversion have often been noted by scientists themselves. Darwin, in a particularly perceptive passage at the end of his Origin of Species, wrote: "Although I am fully convinced of the truth of the views given in this volume . . ., I by no means expect to convince experienced naturalists whose minds are stocked with a multitude of facts all viewed, during a long course of years, from a point of view directly opposite to mine. . . . [B]ut I look with confidence to the future,—to young and rising naturalists, who will be able to view both sides of the question with impartiality."[7] And Max Planck, surveying his own career in his Scientific Autobiography, sadly remarked that "a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it."[8]

These facts and others like them are too commonly known to need further emphasis. But they do need re-evaluation. In the past they have most often been taken to indicate that scientists being only human, cannot always admit their errors, even when confronted with strict proof. I would argue, rather, that in these matters neither proof nor error is at issue. The transfer of allegiance from paradigm to paradigm is a conversion experience that cannot be forced. Lifelong resistance, particularly from those whose productive careers have committed them to an older tradition of normal science, is not a violation of scientific standards but an index to the nature of scientific research itself. The source of

resistance is the assurance that the older paradigm will ultimately solve all its problems, that nature can be shoved into the box the paradigm provides. Inevitably, at times of revolution, that assurance seems stubborn and pigheaded as indeed it sometimes becomes. But it is also something more. That same assurance is what makes normal or puzzle-solving science possible. And it is only through normal science that the professional community of scientists succeeds, first, in exploiting the potential scope and precision of the older paradigm and, then, in isolating the difficulty through the study of which a new paradigm may emerge.

Still, to say that resistance is inevitable and legitimate, that paradigm change cannot be justified by proof, is not to say that no arguments are relevant or that scientists cannot be persuaded to change their minds. Though a generation is sometimes required to effect the change, scientific communities have again and again been converted to new paradigms. Furthermore, these conversions occur not despite the fact that scientists are human but because they are. Though some scientists, particularly the older and more experienced ones, may resist indefinitely, most of them can be reached in one way or another. Conversions will occur a few at a time until, after the last hold-outs have died, the whole profession will again be practicing under a single, but now a different, paradigm. We must therefore ask how conversion is induced and how resisted.

What sort of answer to that question may we expect? Just because it is asked about techniques of persuasion, or about argument and counter-argument in a situation in which there can be no proof, our question is a new one, demanding a sort of study that has not previously been undertaken. We shall have to settle for a very partial and impressionistic survey. In addition, what has already been said combines with the result of that survey to suggest that, when asked about persuasion rather than proof, the question of the nature of scientific argument has no single or uniform answer. Individual scientists embrace a new paradigm for all sorts of reasons and usually for several at once.. Some of these reasons—for example, the sun worship that helped make Kepler a Copernican—lie outside the apparent sphere of science entirely.[9] Others must depend upon idiosyncrasies of autobiography and personality. Even the nationality or the prior reputation of the innovator and his teachers can sometimes play a significant role.[10] Ultimately, therefore, we must learn to ask this question differently. Our concern will not then be with the arguments that in fact convert one or another individual, but rather with the sort of community that always sooner or later re-forms as a single group. That problem, however, I postpone to the final section, examining meanwhile some of the sorts of argument that prove particularly effective in the battles over paradigm change.

Probably the single most prevalent claim advanced by the proponents of a new paradigm is that they can solve the problems that have led the old one to a crisis. When it can legitimately be made, this claim is often the most effective one possible. In the area for which it is advanced the paradigm is known to be in trouble. That trouble has repeatedly been explored, and attempts to remove it have again and again proved vain. "Crucial experiments"—those able to discriminate particularly sharply between the two paradigms—have been recognized and attested before the new paradigm was even invented. Copernicus thus claimed that he had solved the long-vexing problem of the length of the calendar year, Newton that he had reconciled terrestrial and celestial mechanics, Lavoisier that he had

solved the problems of gas-identity and of weight relations, and Einstein that he had made electrodynamics compatible with a revised science of motion.

Claims of this sort are particularly likely to succeed if the new paradigm displays a quantitative precision strikingly better than its older competitor. The quantitative superiority of Kepler's Rudolphine tables to all those computed from the Ptolemaic theory was a major factor in the conversion of astronomers to Copernicanism. Newton's success in predicting quantitative astronomical observations was probably the single most important reason for his theory's triumph over its more reasonable but uniformly qualitative competitors. And in this century the striking quantitative success of both Planck's radiation law and the Bohr atom quickly persuaded many physicists to adopt them even though, viewing physical science as a whole, both these contributions created many more problems than they solved.[11]

The claim to have solved the crisis-provoking problems is, however, rarely sufflcient by itself. Nor can it always legitimately be made. In fact, Copernicus' theory was not more accurate than Ptolemy's and did not lead directly to any improvement in the calendar. Or again, the wave theory of light was not, for some years after it was first announced, even as successful as its corpuscular rival in resolving the polarization effects that were a principal cause of the optical crisis. Sometimes the looser practice that characterizes extraordinary research will produce a candidate for paradigm that initially helps not at all with the problems that have evoked crisis. When that occurs, evidence must be drawn from other parts of the field as it often is anyway. In those other areas particularly persuasive arguments can be developed if the new paradigm permits the prediction of phenomena that had been entirely unsuspected while the old one prevailed.

Copernicus' theory, for example, suggested that planets should be like the earth, that Venus should show phases, and that the universe must be vastly larger than had previously been supposed. As a result, when sixty years after his death the telescope suddenly displayed mountains on the moon, the phases of Venus, and an immense number of previously unsuspected stars, those observations brought the new theory a great many converts, particularly among non-astronomers.[l2] In the case of the wave theory, one main source of professional conversions was even more dramatic. French resistance collapsed suddenly and relatively completely when Fresnel was able to demonstrate the existence of a white spot at the center of the shadow of a circular disk. That was an effect that not even he had anticipated but that Poisson, initially one of his opponents, had shown to be a necessary if absurd consequence of Fresnel's theory.[l3] Because of their shock value and because they have so obviously not been "built into" the new theory from the start, arguments like these prove especially persuasive. And sometimes that extra strength can be exploited even though the phenomenon in question had been observed long before the theory that accounts for it was first introduced. Einstein, for example, seems not to have anticipated that general relativity would account with precision for the well-known anomaly in the motion of Mercury's perihelion, and he experienced a corresponding triumph when it did so.[14]

All the arguments for a new paradigm discussed so far have been based upon the competitors' comparative ability to solve problems. To scientists those arguments are ordinarily the most significant and persuasive. The preceding examples should leave no doubt about the source of their

immense appeal. But, for reasons to which we shall shortly revert, they are neither individually nor collectively compelling. Fortunately, there is also another sort of consideration that can lead scientists to reject an old paradigm in favor of a new. These are the arguments, rarely made entirely explicit, that appeal to the individual's sense of the appropriate or the aesthetic—the new theory is said to be "neater," "more suitable," or "simpler" than the old. Probably such arguments are less effective in the sciences than in mathematics. The early versions of most new paradigms are crude. By the time their full aesthetic appeal can be developed, most of the community has been persuaded by other means. Nevertheless, the importance of aesthetic considerations can sometimes be decisive. Though they often attract only a few scientists to a new theory, it is upon those few that its ultimate triumph may depend. If they had not quickly taken it up for highly individual reasons, the new candidate for paradigm might never have been sufficiently developed to attract the allegiance of the scientific community as a whole.

To see the reason for the importance of these more subjective and aesthetic considerations, remember what a paradigm debate is about. When a new candidate for paradigm is first proposed, it has seldom solved more than a few of the problems that confront it, and most of those solutions are still far from perfect. Until Kepler, the Copernican theory scarcely improved upon the predictions of planetary position made by Ptolemy. When Lavoisier saw oxygen as "the air itself entire," his new theory could cope not at all with the problems presented by the proliferation of new gases, a point that Priestley made with great success in his counter-attack. Cases like Fresnel's white spot are extremely rare. Ordinarily, it is only much later, after the new paradigm has been developed, accepted, and exploited that apparently decisive arguments—the Foucault pendulum to demonstrate the rotation of the earth or the Fizeau experiment to show that light moves faster in air than in water—are developed. Producing them is part of normal science, and their role is not in paradigm debate but in post-revolutionary texts. Before those texts are written, while the debate goes on, the situation is very different. Usually the opponents of a new paradigm can legitimately claim that even in the area of crisis it is little superior to its traditional rival. Of course, it handles some problems better, has disclosed some new regularities. But the older paradigm can presumably be articulated to meet these challenges as it has met others before. Both Tycho Brahe's earth-centered astronomical system and the later versions of the phlogiston theory were responses to challenges posed by a new candidate for paradigm, and both were quite successful.[15] In addition, the defenders of traditional theory and procedure can almost always point to problems that its new rival has not solved but that for their view are no problems at all. Until the discovery of the composition of water, the combustion of hydrogen was a strong argument for the phlogiston theory and against Lavoisier's. And after the oxygen theory had triumphed, it could still not explain the preparation of a combustible gas from carbon, a phenomenon to which the phlogistonists had pointed as strong support for their view.[16] Even in the area of crisis, the balance of argument and counter-argument can sometimes be very close indeed. And outside that area the balance will often decisively favor the tradition. Copernicus destroyed a time-honored explanation of terrestrial motion without replacing it; Newton did the same for an older explanation of gravity, Lavoisier for the common properties of metals, and so on. In short, if a new candidate for paradigm had to be judged from the start by hard-headed people who examined only relative

problem-solving ability, the sciences would experience very few major revolutions. Add the counter-arguments generated by what we previously called the incommensurability of paradigms, and the sciences might experience no revolutions at all.

But paradigm debates are not really about relative problem-solving ability, though for good reasons they are usually couched in those terms. Instead, the issue is which paradigm should in the future guide research on problems many of which neither competitor can yet claim to resolve completely. A decision between alternate ways of practicing science is called for, and in the circumstances that decision must be based less on past achievement than on future promise. The man who embraces a new paradigm at an early stage must often do so in defiance of the evidence provided by problem-solving. He must, that is, have faith that the new paradigm will succeed with the many large problems that confront it, knowing only that the older paradigm has failed with a few. A decision of that kind can only be made on faith.

That is one of the reasons why prior crisis proves so important. Scientists who have not experienced it will seldom renounce the hard evidence of problem-solving to follow what may easily prove and will be widely regarded as a will-o'-the-wisp. But crisis alone is not enough. There must also be a basis, though it need be neither rational nor ultimately correct, for faith in the particular candidate chosen. Something must make at least a few scientists feel that the new proposal is on the right track, and sometimes it is only personal and inarticulate aesthetic considerations that can do that. Men have been converted by them at times when most of the articulable technical arguments pointed the other way. When first introduced, neither Copernicus' astronomical theory nor De Broglie's theory of matter had many other significant grounds of appeal. Even today Einstein's general theory attracts men principally on aesthetic grounds, an appeal that few people outside of mathematics have been able to feel.

This is not to suggest that new paradigms triumph ultimately through some mystical aesthetic. On the contrary, very few men desert a tradition for these reasons alone. Often those who do turn out to have been misled. But if a paradigm is ever to triumph it must gain some first supporters, men who will develop it to the point where hard-headed arguments can be produced and multiplied. And even those arguments, when they come, are not individually decisive. Because scientists are reasonable men, one or another argument will ultimately persuade many of them. But there is no single argument that can or should persuade them all. Rather than a single group conversion, what occurs is an increasing shift in the distribution of professional allegiances.

At the start a new candidate for paradigm may have few supporters, and on occasions the supporters' motives may be suspect. Nevertheless, if they are competent, they will improve it, explore its possibilities, and show what it would be like to belong to the community guided by it. And as that goes on, if the paradigm is one destined to win its fight, the number and strength of the persuasive arguments in its favor will increase. More scientists will then be converted, and the exploration of the new paradigm will go on. Gradually the number of experiments, instruments, articles, and books based upon the paradigm will multiply. Still more men, convinced of the new view's fruitfulness, will adopt the new mode of practicing normal science, until at last only a few elderly hold-outs remain. And even they, we cannot say, are wrong. Though the historian can always find men—

Priestley, for instance—who were unreasonable to resist for as long as they did, he will not find a point at which resistance becomes illogical or unscientific. At most he may wish to say that the man who continues to resist after his whole profession has been converted has *ipso facto* ceased to be a scientist.

**Thomas Kuhn's Footnotes**

1. For a brief sketch of the main routes to probabilistic verification theories see Ernest Nagel, Principles of the Theory of Probability, Vol. 1, No. 6, of International Encyclopedia of Unified Science, pp. 60-75.
2. K. R. Popper, The Logic of Scientific Discovery (New York, 1959), esp. chaps. i-iv.
3. For lay reactions to the concept of curved space, see Philipp Frank, Einstein, His Lffe and Times, trans. and ed. G. Rosen and S. Kusaka (New York, 1947), pp. 142-46. For a few of the attempts to preserve the gains of general relativity within a Euclidean space, see C. Nordmann, Einstein and the Universe, trans. J. McCabe (New York, 1922), chap. ix.
4. T. S. Kuhn, The Copernican Revolution (Cambridge, Mass., l957), chaps. iii, iv, and vii. The extent to which heliocentrism was more than a strictly astronomical issue is a major theme of the entire book.
5. Max Jammer, Concepts of Space (Cambridge, Mass., 1954), pp. 118-24.
6. I. B. Cohen, Franklin and Newton: An Inquiry into Speculative Newtonian Experimental Science and Franklin's Work in Electricity as an Example Thereof (Philadelphia, 1956), pp. 93-94.
7. Charles Darwin, On the Origin of Species . . . . (authorized edition from 6th English ed.; New York, 1889), II, 295-96.
8. Max Planck, Scientific Autobiography and Other Papers, trans. F. Gaynor (New York, 1949), pp. 33-34.
9. For the role of sun worship in Kepler's thought, see E. A. Burtt, The Metaphysical Foundations of Modern Physical Science (rev. ed.; New York, 1932), pp.44-49
10. For the role of reputation, consider the following: Lord Rayleigh, at a time when his reputation was established, submitted to the British Association a paper on some paradoxes of electrodynamics. His name was inadvertently omitted when the paper was first sent, and the paper itself was at first rejected as the work of some "paradoxer." Shortly afterwards, with the author's name in place, the paper was accepted with profuse apologies (R. J. Strutt, 4th Baron Rayleigh, John William Strutt, Third Baron Rayleigh [New York, 1924], p. 228).
11. For the problems created by the quantum theory, see F. Reiche, The Quantum Theory (London, 1922), chaps. ii, vi-ix. For the other examples in this paragraph, see the earlier references in this section.
12. Kuhn, op. cit., pp. 219-25.
13. E. T. Whittaker, A History of the Theories of Aether and Electricity, I (2nd ed.; London, 1951), 108.

14. See ibid., II (1953), 151-80, for the development of general relativity. For Einstein's reaction to the precise agreement of the theory with the observed motion of Mercury's perihelion, see the letter quoted in P. A. Schilpp (ed.) Albert Einstein, Philosopher-Scientist (Evanston, Ill., 1949), p. 101.
15. For Brahe's system which was geometrically entirely equivalent to Copernicus', see J. L. E. Dreyer, A History of Astronomy from Thales to Kepler (2nd ed., New York, 1953), pp. 359-71. For the last versions of the phlogiston theory and their success, see J. R. Partington and D. McKie, "Historical Studies of the Phlogiston Theory," Annals of Science, IV (1989), 113-49.
16. For the problem presented by hydrogen, see J. R. Partington, A Short History of Chemistry (2nd ed.; London, 1951), p. 134. For carbon monoxide, see H. Kopp, Geschichte der Chemie, III (Braunschweig, 1845), 294-96.

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## XIII. Progress through Revolutions

The preceding pages have carried my schematic description of scientific development as far as it can go in this essay. Nevertheless, they cannot quite provide a conclusion. If this description has at all caught the essential structure of a science's continuing evolution, it will simultaneously have posed a special problem: Why should the enterprise sketched above move steadily ahead in ways that, say, art, political theory, or philosophy does not? Why is progress a perquisite reserved almost exclusively for the activities we call science? The most usual answers to that question have been denied in the body of this essay. We must conclude it by asking whether substitutes can be found.

Notice immediately that part of the question is entirely semantic. To a very great extent the term 'science' is reserved for fields that do progress in obvious ways. Nowhere does this show more clearly than in the recurrent debates about whether one or another of the contemporary social sciences is really a science. These debates have parallels in the pre-paradigm periods of fields that are today unhesitatingly labeled science. Their ostensible issue throughout is a definition of that vexing term. Men argue that psychology, for example, is a science because it possesses such and such characteristics. Others counter that those characteristics are either unnecessary or not sufficient to make a field a science. Often great energy is invested, great passion aroused, and the outsider is at a loss to know why. Can very much depend upon a definition of 'science'? Can a definition tell a man whether he is a scientist or not? If so, why do not natural scientists or artists worry about the definition of the term? Inevitably one suspects that the issue is more fundamental. Probably questions like the following are really being asked: Why does my field fail to move ahead in the way that, say, physics does? What changes in technique or method or ideology would enable it to do so? These are not, however, questions that could respond to an agreement on definition. Furthermore, if precedent from the natural sciences serves, they will cease to be a source of concern not when a definition is found, but when the groups that now doubt their own status achieve consensus about their past and present accomplishments. It may, for example, be significant that economists argue less about whether their field is a science than do practitioners of some other fields of social science. Is that because economists know what science is? Or is it rather economics about which they agree?

That point has a converse that, though no longer simply semantic, may help to display the inextricable connections between our notions of science and of progress. For many centuries, both in antiquity

and again in early modern Europe, painting was regarded as the cumulative discipline. During those years the artist's goal was assumed to be representation. Critics and historians, like Pliny and Vasari, then recorded with veneration the series of inventions from foreshortening through chiaroscuro that had made possible successively more perfect representations of nature.[1] But those are also the years, particularly during the Renaissance, when little cleavage was felt between the sciences and the arts. Leonardo was only one of many men who passed freely back and forth between fields that only later became categorically distinct.[2] Furthermore, even after that steady exchange had ceased, the term 'art' continued to apply as much to technology and the crafts, which were also seen as progressive, as to painting and sculpture. Only when the latter unequivocally renounced representation as their goal and began to learn again from primitive models did the cleavage we now take for granted assume anything like its present depth. And even today, to switch fields once more, part of our difficulty in seeing the profound differences between science and technology must relate to the fact that progress is an obvious attribute of both fields.

It can, however, only clarify, not solve, our present difficulty to recognize that we tend to see as science any field in which progress is marked. There remains the problem of understanding why progress should be so noteworthy a characteristic of an enterprise conducted with the techniques and goals this essay has described. That question proves to be several in one, and we shall have to consider each of them separately. In all cases but the last, however, their resolution will depend in part upon an inversion of our normal view of the relation between scientific activity and the community that practices it. We must learn to recognize as causes what have ordinarily been taken to be effects. If we can do that, the phrases 'scientific progress' and even 'scientific objectivity' may come to seem in part redundant. In fact, one aspect of the redundancy has just been illustrated. Does a field make progress because it is a science, or is it a science because it makes progress?

Ask now why an enterprise like normal science should progress, and begin by recalling a few of its most salient characteristics. Normally, the members of a mature scientific community work from a single paradigm or from a closely related set. Very rarely do different scientific communities investigate the same problems. In those exceptional cases the groups hold several major paradigms in common. Viewed from within any single community, however, whether of scientists or of non-scientists, the result of successful creative work is progress. How could it possibly be anything else? We have, for example, just noted that while artists aimed at representation as their goal, both critics and historians chronicled the progress of the apparently united group. Other creative fields display progress of the same sort. The theologian who articulates dogma or the philosopher who refines the Kantian imperatives contributes to progress, if only to that of the group that shares his premises. No creative school recognizes a category of work that is, on the one hand, a creative success, but is not, on the other, an addition to the collective achievement of the group. If we doubt, as many do, that nonscientific fields make progress, that cannot be because individual schools make none. Rather, it must be because there are always competing schools, each of which constantly questions the very foundations of the others. The man who argues that philosophy, for example, has made no progress emphasizes that there are still Aristotelians, not that Aristotelianism has failed to progress.

These doubts about progress arise, however, in the sciences too. Throughout the pre-paradigm period when there is a multiplicity of competing schools, evidence of progress, except within schools, is very hard to find. This is the period described in Section II as one during which individuals practice science, but in which the results of their enterprise do not add up to science as we know it. And again, during periods of revolution when the fundamental tenets of a field are once more at issue, doubts are repeatedly expressed about the very possibility of continued progress if one or another of the opposed paradigms is adopted. Those who rejected Newtonianism proclaimed that its reliance upon innate forces would return science to the Dark Ages. Those who opposed Lavoisier's chemistry held that the rejection of chemical "principles" in favor of laboratory elements was the rejection of achieved chemical explanation by those who would take refuge in a mere name, A similar, though more moderately expressed, feeling seems to underlie the opposition of Einstein, Bohm, and others, to the dominant probabilistic interpretation of quantum mechanics. In short, it is only during periods of normal science that progress seems both obvious and assured. During those periods, however, the scientific community could view the fruits of its work in no other way.

With respect to normal science, then, part of the answer to the problem of progress lies simply in the eye of the beholder. Scientific progress is not different in kind from progress in other fields, but the absence at most times of competing schools that question each other's aims and standards makes the progress of a normal-scientific community far easier to see. That, however, is only part of the answer and by no means the most important part. We have, for example, already noted that once the reception of a common paradigm has freed the scientific community from the need constantly to re-examine its first principles, the members of that community can concentrate exclusively upon the subtlest and most esoteric of the phenomena that concem it. Inevitably, that does increase both the effectiveness and the efficiency with which the group as a whole solves new problems. Other aspects of professional life in the sciences enhance this very special efficiency still further.

Some of these are consequences of the unparalleled insulation of mature scientific communities from the demands of the laity and of everyday life. That insulation has never been complete—we are now discussing matters of degree. Nevertheless, there are no other professional communities in which individual creative work is so exclusively addressed to and evaluated by other members of the profession. The most esoteric of poets or the most abstract of theologians is far more concerned than the scientist with lay approbation of his creative work, though he may be even less concerned with approbation in general. That difference proves consequential. Just because he is working only for an audience of colleagues, an audience that shares his own values and beliefs, the scientist can take a single set of standards for granted. He need not worry about what some other group or school will think and can therefore dispose of one problem and get on to the next more quickly than those who work for a more heterodox group. Even more important, the insulation of the scientific community from society permits the individual scientist to concentrate his attention upon problems that he has good reason to believe he will be able to solve. Unlike the engineer, and many doctors, and most theologians, the scientist need not choose problems because they urgently need solution and without regard for the tools available to solve them. In this respect, also, the contrast between natural scientists and many social scientists proves instructive. The latter often tend, as the former almost

never do, to defend their choice of a research problem— e.g., the effects of racial discrimination or the causes of the business cycle—chiefly in terms of the social importance of achieving a solution. Which group would one then expect to solve problems at a more rapid rate?

The effects of insulation from the larger society are greatly intensified by another characteristic of the professional scientific community, the nature of its educational initiation. In music, the graphic arts, and literature, the practitioner gains his education by exposure to the works of other artists, principally earlier artists. Textbooks, except compendia of or handbooks to original creations, have only a secondary role. In history, philosophy, and the social sciences, textbook literature has a greater significance. But even in these fields the elementary college course employs parallel readings in original sources, some of them the "classics" of the field, others the contemporary research reports that practitioners write for each other. As a result, the student in any one of these disciplines is constantly made aware of the immense variety of problems that the members of his future group have, in the course of time, attempted to solve. Even more important, he has constantly before him a number of competing and incommensurable solutions to these problems, solutions that he must ultimately evaluate for himself.

Contrast this situation with that in at least the contemporary natural sciences. In these fields the student relies mainly on textbooks until, in his third or fourth year of graduate work, he begins his own research. Many science curricula do not ask even graduate students to read in works not written specially for students. The few that do assign supplementary reading in research papers and monographs restrict such assignments to the most advanced courses and to materials that take up more or less where the available texts leave off. Until the very last stages in the education of a scientist, textbooks are systematically substituted for the creative scientific literature that made them possible. Given the confidence in their paradigms, which makes this educational technique possible, few scientists would wish to change it. Why, after all, should the student of physics, for example, read the works of Newton, Faraday, Einstein, or Schrödinger, when everything he needs to know about these works is recapitulated in a far briefer, more precise, and more systematic form in a number of up-to-date textbooks?

Without wishing to defend the excessive lengths to which this type of education has occasionally been carried, one cannot help but notice that in general it has been immensely effective. Of course, it is a narrow and rigid education, probably more so than any other except perhaps in orthodox theology. But for normal-scientific work, for puzzle-solving within the tradition that the textbooks define, the scientist is almost perfectly equipped. Furthermore, he is well equipped for another task as well—the generation through normal science of significant crises. When they arise, the scientist is not, of course, equally well prepared. Even though prolonged crises are probably reflected in less rigid educational practice, scientific training is not well designed to produce the man who will easily discover a fresh approach. But so long as somebody appears with a new candidate for paradigm—usually a young man or one new to the field—the loss due to rigidity accrues only to the individual. Given a generation in which to effect the change, individual rigidity is compatible with a community that can switch from paradigm to paradigm when the occasion demands. Particularly, it is compatible

when that very rigidity provides the community with a sensitive indicator that something has gone wrong.

In its normal state, then, a scientific community is an immensely efficient instrument for solving the problems or puzzles that its paradigms define. Furthermore, the result of solving those problems must inevitably be progress. There is no problem here. Seeing that much, however, only highlights the second main part of the problem of progress in the sciences. Let us therefore turn to it and ask about progress through extraordinary science. Why should progress also be the apparently universal concomitant of scientific revolutions? Once again, there is much to be learned by asking what else the result of a revolution could be. Revolutions close with a total victory for one of the two opposing camps. Will that group ever say that the result of its victory has been something less than progress? That would be rather like admitting that they had been wrong and their opponents right. To them, at least, the outcome of revolution must be progress, and they are in an excellent position to make certain that future members of their community will see past history in the same way. Section XI described in detail the techniques by which this is accomplished, and we have just recurred to a closely related aspect of professional scientific life. When it repudiates a past paradigm, a scientific community simultaneously renounces, as a fit subject for professional scrutiny, most of the books and articles in which that paradigm had been embodied. Scientific education makes use of no equivalent for the art museum or the library of classics, and the result is a sometimes drastic distortion in the scientist's perception of his discipline's past. More than the practitioners of other creative fields, he comes to see it as leading in a straight line to the discipline's present vantage. In short, he comes to see it as progress. No alternative is available to him while he remains in the field.

Inevitably those remarks will suggest that the member of a mature scientific community is, like the typical character of Orwell's 1984, the victim of a history rewritten by the powers that be. Furthermore, that suggestion is not altogether inappropriate. There are losses as well as gains in scientific revolutions, and scientists tend to be peculiarly blind to the former.[3] On the other hand, no explanation of progress through revolutions may stop at this point. To do so would be to imply that in the sciences might makes right, a formulation which would again not be entirely wrong if it did not suppress the nature of the process and of the authority by which the choice between paradigms is made. If authority alone, and particularly if nonprofessional authority, were the arbiter of paradigm debates, the outcome of those debates might still be revolution, but it would not be scientific revolution. The very existence of science depends upon vesting the power to choose between paradigms in the members of a special kind of community. Just how special that community must be if science is to survive and grow may be indicated by the very tenuousness of humanity's hold on the scientific enterprise. Every civilization of which we have records has possessed a technology, an art, a religion, a political system, laws, and so on. In many cases those facets of civilization have been as developed as our own. But only the civilizations that descend from Hellenic Greece have possessed more than the most rudimentary science. The bulk of scientific knowledge is a product of Europe in the last four centuries. No other place and time has supported the very special communities from which scientific productivity comes.

What are the essential characteristics of these communities? Obviously, they need vastly more study. In this area only the most tentative generalizations are possible. Nevertheless, a number of requisites for membership in a professional scientific group must already be strikingly clear. The scientist must, for example, be concerned to solve problems about the behavior of nature. In addition, though his concern with nature may be global in its extent, the problems on which he works must be problems of detail. More important, the solutions that satisfy him may not be merely personal but must instead be accepted as solutions by many. The group that shares them may not, however, be drawn at random from society as a whole, but is rather the well-defined community of the scientist's professional compeers. One of the strongest, if still unwritten, rules of scientific life is the prohibition of appeals to heads of state or to the populace at large in matters scientific. Recognition of the existence of a uniquely competent professional group and acceptance of its role as the exclusive arbiter of professional achievement has further implications. The group's members, as individuals and by virtue of their shared training and experience, must be seen as the sole possessors of the rules of the game or of some equivalent basis for unequivocal judgments. To doubt that they shared some such basis for evaluations would be to admit the existence of incompatible standards of scientific achievement. That admission would inevitably raise the question whether truth in the sciences can be one.

This small list of characteristics common to scientific communities has been drawn entirely from the practice of normal science, and it should have been. That is the activity for which the scientist is ordinarily trained. Note, however, that despite its small size the list is already sufficient to set such communities apart from all other professional groups. And note, in addition, that despite its source in normal science the list accounts for many special features of the group's response during revolutions and particularly during paradigm debates. We have already observed that a group of this sort must see a paradigm change as progress. Now we may recognize that the perception is, in important respects, self-fulfilling. The scientific community is a supremely efficient instrument for maximizing the number and precision of the problem solved through paradigm change.

Because the unit of scientific achievement is the solved problem and because the group knows well which problems have already been solved, few scientists will easily be persuaded to adopt a viewpoint that again opens to question many problems that had previously been solved. Nature itself must first undermine professional security by making prior achievements seem problematic. Furthermore, even when that has occurred and a new candidate for paradigm has been evoked, scientists will be reluctant to embrace it unless convinced that two all-important conditions are being met. First, the new candidate must seem to resolve some outstanding and generally recognized problem that can be met in no other way. Second, the new paradigm must promise to preserve a relatively large part of the concrete problem-solving ability that has accrued to science through its predecessors. Novelty for its own sake is not a desideratum in the sciences as it is in so many other creative fields. As a result though new paradigms seldom or never possess all the capabilities of their predecessors, they usually preserve a great deal of the most concrete parts of past achievement and they always permit additional concrete problem-solutions besides.

To say this much is not to suggest that the ability to solve problems is either the unique or an unequivocal basis for paradigm choice. We have already noted many reasons why there can be no

criterion of that sort. But it does suggest that a community of scientific specialists will do all that it can to ensure the continuing growth of the assembled data that it can treat with precision and detail. In the process the community will sustain losses. Often some old problems must be banished. Frequently, in addition, revolution narrows the scope of the community's professional concerns, increases the extent of its specialization, and attenuates its communication with other groups, both scientific and lay. Though science surely grows in depth, it may not grow in breadth as well. If it does so, that breadth is manifest mainly in the proliferation of scientific specialties, not in the scope of any single specialty alone. Yet despite these and other losses to the individual communities, the nature of such communities provides a virtual guarantee that both the list of problems solved by science and the precision of individual problem-soluffons will grow and grow. At least, the nature of the community provides such a guarantee if there is any way at all in which it can be provided. What better criterion than the decision of the scientific group could there be?

These last paragraphs point the directions in which I believe a more refined solution of the problem of progress in the sciences must be sought. Perhaps they indicate that scientific progress is not quite what we had taken it to be. But they simultaneously show that a sort of progress will inevitably characterize the scientific enterprise so long as such an enterprise survives. In the sciences there need not be progress of another sort. We may, to be more precise, 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.

It is now time to notice that until the last very few pages the term 'truth' had entered this essay only in a quotation from Francis Bacon. And even in those pages it entered only as a source for the scientist's conviction that incompatible rules for doing science cannot coexist except during revolutions when the profession's main task is to eliminate all sets but one. The developmental process described in this essay has been a process of evolution *from* primitive beginnings—a process whose successive stages are characterized by an increasingly detailed and refined understanding of nature. But nothing that has been or will be said makes it a process of evolution *toward* anything. Inevitably that lacuna will have disturbed many readers. We are all deeply accustomed to seeing science as the one enterprise that draws constantly nearer to some goal set by nature in advance.

But need there be any such goal? Can we not account for both science's existence and its success in terms of evolution from the community's state of knowledge at any given time? Does it really help to imagine that there is some one full, objective, true account of nature and that the proper measure of scientific achievement is the extent to which it brings us closer to that ultimate goal? If we can learn to substitute evolu-from-what-we-do-know for evolution-toward-what-we-wish-to-know, a number of vexing problems may vanish in the process. Somewhere in this maze, for example, must lie the problem of induction.

I cannot yet specify in any detail the consequences of this alternate view of scientific advance. But it helps to recognize that the conceptual transposition here recommended is very close to one that the West undertook just a century ago. It is particularly helpful because in both cases the main obstacle to transposition is the same. When Darwin first published his theory of evolution by natural selection in 1859, what most bothered many professionals was neither the notion of species change nor the

possible descent of man from apes. The evidence pointing to evolution, including the evolution of man, had been accumulating for decades, and the idea of evolution had been suggested and widely disseminated before. Though evolution, as such, did encounter resistance, particularly from some religious groups, it was by no means the greatest of the difficulties the Darwinians faced. That difficulty stemmed from an idea that was more nearly Darwin's own. All the well-known pre-Darwinian evolutionary theories—those of Lamarck, Chambers, Spencer, and the German Naturphilosophen—had taken evolution to be a goal-directed process. The "idea" of man and of the contemporary flora and fauna was thought to have been present from the first creation of life, perhaps in the mind of God. That idea or plan had provided the direction and the guiding force to the entire evolutionary process. Each new stage of evolutionary development was a more perfect realization of a plan that had been present from the start.[4]

For many men the abolition of that teleological kind of evolution was the most significant and least palatable of Darwin's suggestions.[5] The Origin of Species recognized no goal set either by God or nature. Instead, natural selection, operating in the given environment and with the actual organisms presently at hand, was responsible for the gradual but steady emergence of more elaborate, further articulated, and vastly more specialized organisms. Even such marvelously adapted organs as the eye and hand of man—organs whose design had previously provided powerful arguments for the existence of a supreme artificer and an advance plan—were products of a process that moved steadily from primitive beginnings but toward no goal. The belief that natural selection, resulting from mere competition between organisms for survival, could have produced man together with the higher animals and plants was the most difficult and disturbing aspect of Darwin's theory. What could ‘evolution,' ‘development,' and 'progress' mean in the absence of a specified goal? To many people, such terms suddenly seemed self-contradictory.

The analogy that relates the evolution of organisms to the evolution of scientific ideas can easily be pushed too far. But with respect to the issues of this closing section it is very nearly perfect. The process described in Section XIl as the resolution of revolutions is the selection by conflict within the scientific community of the fittest way to practice future science. The net result of a sequence of such revolutionary selections, separated by periods of normal research, is the wonderfully adapted set of instruments we call modern scientific knowledge. Successive stages in that developmental process are marked by an increase in articulation and specialization. And the entire process may have occurred, as we now suppose biological evolution did, without benefit of a set goal, a permanent fixed scientific truth, of which each stage in the development of scientific knowledge is a better exemplar.

Anyone who has followed the argument this far will never-the-less feel the need to ask why the evolutionary process should work. What must nature, including man, be like in order that science be possible at all? Why should scientific communities be able to reach a firm consensus unattainable in other fields? Why should consensus endure across one paradigm change after another? And why should paradigm change invariably produce an instrument more perfect in any sense than those known before? From one point of view those questions, excepting the first, have already been answered. But from another they are as open as they were when this essay began. It is not only the scientific community that must be special. The world of which that community is a part must also

possess quite special characteristics, and we are no closer than we were at the start to knowing what these must be. That problem—What must the world be like in order that man may know it?—was not, however, created by this essay. On the contrary, it is as old as science itself, and it remains unanswered. But it need not be answered in this place. Any conception of nature compatible with the growth of science by proof is compatible with the evolutionary view of science developed here. Since this view is also compatible with close observation of scientific life, there are strong arguments for employing it in attempts to solve the host of problems that still remain.

**Thomas Kuhn's Footnotes**

1. E. H. Gombrich, Art and Illusion: A Study in the Psychology of Pictorial Representation (New York, 1960), pp. 11-12.
2. Ibid. p. 97 and Giorgio de Santillana, "The Role of Art in the Scientific Renaissance," in Critical Problems in the History of Science, ed. M. Clagett (Madison, Wis., 1959), pp. 33-65.
3. Historians of science often encounter this blindness in a particularly striking form. The group of students who come to them from the sciences is very often the most rewarding group they teach. But it is also usually the most frustrating at the start. Because science students "know the right answers," it is particularly difficult to make them analyze an older science in its own terms.
4. Loren Eiseley, Darwin's Century: Evolution and the Men Who Dlscovered It (New York, 1958), chaps. ii, iv-v.
5. For a particularly acute account of one prominent Darwinian's struggle with this problem, see A. Hunter Dupree, Asa Gray, 1810-1888 (Cambridge, Mass.,1959), pp. 295-306, 355-83.