THOMAS S. KUHN

The Function of Dogma in Scientific Research[1]

At some point in his or her career every member of this Symposium has, I feel sure, been exposed to the image of the scientist as the uncommitted searcher after truth. He is the explorer of nature—the man who rejects prejudice at the threshold of his laboratory, who collects and examines the bare and objective facts, and whose allegiance is to such facts and to them alone. These are the characteristics which make the testimony of scientists so valuable when advertising proprietary products in the United States. Even for an international audience, they should require no further elaboration. To be scientific is, among other things, to be objective and open-minded.

Probably none of us believes that in practice the real-life scientist quite succeeds in fulfilling this ideal. Personal acquaintance, the novels of Sir Charles Snow, or a cursory reading of the history of science provides too much counterevidence. Though the scientific enterprise may be open-minded, whatever this application of that phrase may mean, the individual scientist is very often not. Whether his work is predominantly theoretical or experimental, he usually seems to know, before his research project is even well under way, all but the most intimate details of the result which that project will achieve. If the result is quickly forthcoming, well and good. If not, he will struggle with his apparatus and with his equations until, if at all possible, they yield results which conform to the sort of pattern which he has foreseen from the start. Nor is it only through his own research that the scientist displays his firm convictions about the phenomena which nature can yield and about the ways in which these may be fitted to theory. Often the same convictions show even more clearly in his response to the work produced by others. From Galileo's reception of Kepler's research to Nägeli's reception of Mendel's, from Dalton's rejection of Gay Lussac's results to Kelvin's rejection of Maxwell's, unexpected novelties of fact and

Reprinted from *Scientific Change*, Alistair C. Crombie, Ed. (1963), pp. 347–369.

theory have characteristically been resisted and have often been rejected by many of the most creative members of the professional scientific community. The historian, at least, scarcely needs Planck to remind him that "A new scientific truth is not usually presented in a way that convinces its opponents . . . ; rather they gradually die off, and a rising generation is familiarized with the truth from the start."[2]

Familiar facts like these—and they could easily be multiplied—do not seem to bespeak an enterprise whose practitioners are notably open-minded. Can they all be reconciled with our usual image of productive scientific research? If such a reconciliation has not seemed to present fundamental problems in the past, that is probably because resistance and preconception have usually been viewed as extraneous to science. They are, we have often been told, no more than the product of inevitable *human* limitations; a proper scientific method has no place for them; and that method is powerful enough so that no mere human idiosyncrasy can impede its success for very long. On this view, examples of a scientific *parti pris* are reduced to the status of anecdotes, and it is that evaluation of their significance that this essay aims to challenge. Verisimilitude, alone, suggests that such a challenge is required. Preconception and resistance seem the rule rather than the exception in mature scientific development. Furthermore, under normal circumstances they characterize the very best and most creative research as well as the more routine. Nor can there be much question where they come from. Rather than being characteristics of the aberrant individual, they are community characteristics with deep roots in the procedures through which scientists are trained for work in their profession. Strongly held convictions that are prior to research often seem to be a precondition for success in the sciences.

Obviously I am already ahead of my story, but in getting there I have perhaps indicated its principal theme. Though preconception and resistance to innovation could very easily choke off scientific progress, their omnipresence is nonetheless symptomatic of characteristics upon which the continuing vitality of research depends. Those characteristics I shall collectively call the dogmatism of mature science, and in the pages to come I shall try to make the following points about them. Scientific education inculcates what the scientific community had previously with difficulty gained—a deep commitment to a particular way of viewing the world and of practicing science in it. That commitment can be, and from time to time is, replaced by another, but it cannot be merely given up. And, while it continues to characterize the community of professional practitioners, it proves in two respects fundamental to productive research. By defining for the individual scientist both the problems available for pursuit and the nature of acceptable solutions to them, the commitment is actually constitutive of research. Normally the scientist is a puzzle-solver like the chess player, and the commitment induced by education is what provides him with the rules of the game being played in his time. In its absence he would not be a physicist, chemist, or whatever he has been trained to be.

In addition, commitment has a second and largely incompatible research role. Its very strength and the unanimity with which the professional group subscribes to it provides the individual scientist with an immensely sensitive detector of the trouble spots from which significant innovations of fact and theory are almost inevitably educed. In the sciences most discoveries of unexpected fact and all fundamental innovations of theory are responses to a prior breakdown in the rules of the previously established game. Therefore, though a quasi-dogmatic commitment is, on the one hand, a source of resistance and controversy, it is also instrumental in making the sciences the most consistently revolutionary of all human activities. One need make neither resistance nor dogma a virtue to recognize that no mature science could exist without them. Before examining further the nature and effects of scientific dogma, consider the pattern of education through which it is transmitted from one generation of practitioners to the next. Scientists are not, of course, the only professional community that acquires from education a set of standards, tools, and techniques which they later deploy in their own creative work. Yet even a cursory inspection of scientific pedagogy suggests that it is far more likely to induce professional rigidity than education in other fields, excepting, perhaps, systematic theology. Admittedly the following epitome is biased toward the American pattern, which I know best. The contrasts at which it aims must, however, be visible, if muted, in European and British education as well.

Perhaps the most striking feature of scientific education is that, to an extent quite unknown in other creative fields, it is conducted through textbooks, works written especially for students. Until he is ready, or very nearly ready, to begin his own dissertation, the student of chemistry, physics, astronomy, geology, or biology is seldom either asked to attempt trial research projects or exposed to the immediate products of research done by others—to, that is, the professional communications that scientists write for their peers. Collections of "source readings" play a negligible role in *scientific* education. Nor is the science student encouraged to read the historical classics of his field—works in which he might encounter other ways of regarding the questions discussed in his text, but in which he would also meet problems, concepts and standards of solution that his future profession had long since discarded and replaced.[3] Whitehead somewhere caught this quite special feature of the sciences when he wrote, "A science that hesitates to forget its founders is lost."

An almost exclusive reliance on textbooks is not all that distinguishes scientific education. Students in other fields are, after all, also exposed to such books, though seldom beyond the second year of college and even in those early years not exclusively. But in the sciences different textbooks display different subject matters rather than, as in the humanities and many social sciences, exemplifying different approaches to a single problem field. Even books that compete for adoption in a single science course differ mainly in level and pedagogic detail, not in substance or conceptual structure. One can scarcely imagine a physicist's or chemist's saying that he had been forced to begin the education of his third-year class almost from first principles because its previous exposure to the field had been through books that consistently violated his conception of the discipline. Remarks of that sort are not by any means unprecedented in several of the social sciences. Apparently scientists agree about what it is that every student of the field must know. That is why, in the design of a pre-professional curriculum, they can use textbooks instead of eclectic samples of research.

Nor is the characteristic technique of textbook presentation altogether the same in the sciences as elsewhere. Except in the occasional introductions that students seldom read, science texts make little attempt to describe the *sorts* of problems that the professional may be asked to solve or to discuss the *variety* of techniques that experience has made available for their solution. Instead, these books exhibit, from the very start, concrete problem-solutions that the profession has come to accept as paradigms, and they then ask the student, either with a pencil and paper or in the laboratory, to solve for himself problems closely modelled in method and substance upon those through which the text has led him. Only in elementary language instruction or in training a musical instrumentalist is so large or essential a use made of "finger exercises." And those are just the fields in which the object of instruction is to produce with maximum rapidity strong "mental sets" or *Einstellungen*. In the sciences, I suggest, the effect of these techniques is much the same. Though scientific development is particularly productive of consequential novelties, scientific education remains a relatively dogmatic initiation into a pre-established problem-solving tradition that the student is neither invited nor equipped to evaluate.

The pattern of systematic textbook education just described existed in no place and in no science (except perhaps elementary mathematics) until the early nineteenth century. But before that date a number of the more developed sciences clearly displayed the special characteristics indicated above, and in a few cases had done so for a very long time. Where there were no textbooks there had often been universally received paradigms for the practice of individual sciences. These were scientific achievements reported in books that all the practitioners of a given field knew intimately and admired, achievements upon which they modelled their own research and which provided them with a measure of their own accomplishment. Aristotle's *Physica*, Ptolemy's *Almagest*, Newton's *Principia* and *Opticks*, Franklin's *Electricity*, Lavoisier's *Chemistry*, and Lyell's *Geology*—these works and many others all served for a time implicitly to define the legitimate problems and methods of a research field for succeeding generations of practitioners. In their day each of these books, together with others modelled closely upon them, did for its field much of what textbooks now do for these same fields and for others besides.

All of the works named above are, of course, classics of science. As such their role may be thought to resemble that of the main classics in other creative fields, for example the works of a Shakespeare, a Rembrandt, or an Adam Smith. But by calling these works, or the achievements which lie behind them, paradigms rather than classics, I mean to suggest that there is something else special about them, something which sets them apart both from some other classics of science and from all the classics of other creative fields.

Part of this "something else" is what I shall call the exclusiveness of paradigms. At any time the practitioners of a given specialty may recognize numerous classics, some of them—like the works of Ptolemy and Copernicus or Newton and Descartes—quite incompatible one with the other. But that same group, if it has a paradigm at all, can have only one. Unlike the community of artists—which can draw simultaneous inspiration from the works of, say, Rembrandt *and* Cézanne and which therefore studies both—the community of astronomers had no alternative to choosing *between* the competing models of scientific activity supplied by Copernicus and Ptolemy. Furthermore, having made their choice, astronomers could thereafter neglect the work which they had rejected. Since the sixteenth century there have been only two full editions of the *Almagest*, both produced in the nineteenth century and directed exclusively to scholars. In the mature sciences there is no apparent function for the equivalent of an art museum or a library of classics. Scientists know when books, and even journals, are out of date. Though they do not then destroy them, they do, as any historian of science can testify, transfer them from the active departmental library to desuetude in the general university depository. Up-to-date works have taken their place, and they are all that the further progress of science requires.

This characteristic of paradigms is closely related to another, and one that has a particular relevance to my selection of the term. In receiving a paradigm the scientific community commits itself, consciously or not, to the view that the fundamental problems there resolved have, in fact, been solved once and for all. That is what Lagrange meant when he said of Newton: "There is but one universe, and it can happen to but one man in the world's history to be the interpreter of its laws."[4] The example of either Aristotle or Einstein proves Lagrange wrong, but that does not make the fact of his commitment less consequential to scientific development. Believing that what Newton had done need not be done again, Lagrange was not tempted to fundamental reinterpretations of nature. Instead, he could take up where the men who shared his Newtonian paradigm had left off, striving both for neater formulations of that paradigm and for an articulation that would bring it into closer and closer agreement with observations of nature. That sort of work is undertaken only by those who feel that the model they have chose is entirely secure. There is nothing quite like it in the arts, and the parallels in the social sciences are at best partial. Paradigms determine a developmental pattern for the mature sciences that is unlike the one familiar in other fields.

That difference could be illustrated by comparing the development of a paradigm-based science with that of, say, philosophy or literature. But the same effect can be achieved more economically by contrasting the early developmental pattern of almost any science with the pattern characteristic of the same field in its maturity. I cannot here avoid putting the point too starkly, but what I have in mind is this. Excepting in those fields which, like biochemistry, originated in the combination of existing specialties, paradigms are a relatively late acquisition in the course of scientific development. During its early years a science proceeds without them, or at least without any so unequivocal and so binding as those named illustratively above. Physical optics before Newton or the study of heat before Black and Lavoisier exemplifies the pre-paradigm developmental pattern that I shall immediately examine in the history of electricity. While it continues, until, that is, a first paradigm is reached, the development of a science resembles that of the arts and of most social sciences more closely than it resembles the pattern which astronomy, say, had already acquired in antiquity and which all the natural sciences make familiar today.

To catch the difference between pre- and post-paradigm scientific development, consider a single example. In the early eighteenth century, as in the seventeenth and earlier, there were almost as many views about the nature of electricity as there were important electrical experimenters, men like Hauksbee, Gray, Desaguliers, Du Fay, Nollet, Watson, and Franklin. All their numerous concepts of electricity had something in common—they were partially derived from experiment and observation and partially from one or another version of the mechanico-corpuscular philosophy that guided all scientific research of the day. Yet these common elements gave their work no more than a family resemblance. We are forced to recognize the existence of several competing schools and sub-schools, each deriving strength from its relation to a particular version (Cartesian or Newtonian) of the corpuscular metaphysics, and each emphasizing the particular cluster of electrical phenomena which its own theory could do most to explain. Other observations were dealt with by *ad hoc* elaborations or remained as outstanding problems for further research.[5]

One early group of electricians followed seventeenth-century practice, and thus took attraction and frictional generation as the fundamental electrical phenomena. They tended to treat repulsion as a secondary effect (in the seventeenth century it had been attributed to some sort of mechanical rebounding) and also to postpone for as long as possible both discussion and systematic research on Gray's newly discovered effect, electrical conduction. Another closely related group regarded repulsion as the fundamental effect, while still another took attraction and repulsion together to be equally elementary manifestations of electricity. Each of these groups modified its theory and research accordingly, but they then had as much difficulty as the first in accounting for any but the simplest conduction effects. Those effects provided the starting point for still a third group, one which tended to speak of electricity as a "fluid" that ran through conductors rather than as an "effluvium" that emanated from non-conductors. This group, in its turn, had difficulty reconciling its theory with a number of attractive and repulsive effects.[6]

At various times all these schools made significant contributions to the body of concepts, phenomena, and techniques from which Franklin drew the first paradigm for electrical science. Any definition of the scientist that excludes the members of these schools will exclude their modern successors as well. Yet anyone surveying the development of electricity before Franklin may well conclude that, though the field's practitioners were scientists, the immediate result of their activity was something less than science. Because the body of belief he could take for granted was very small, each electrical experimenter felt forced to begin by building his field anew from its foundations. In doing so his choice of supporting observation and experiment was relatively free, for the set of standard methods and phenomena that every electrician must employ and explain was extraordinarily small. As a result, throughout the first half of the century, electrical investigations tended to circle back over the same ground again and again. New effects were repeatedly discovered, but many of them were rapidly lost again. Among those lost were many effects due to what we should now describe as inductive charging and also Du Fay's famous discovery of the two sorts of electrification. Franklin and Kinnersley were surprised when, some fifteen years later, the latter discovered that a charged ball which was repelled by rubbed glass would be attracted by rubbed sealing-wax or amber.[7] In the absence of a well-articulated and widely received theory (a desideratum which no science possesses from its very beginning and which few if any of the social sciences have achieved today), the situation could hardly have been otherwise. During the first half of the eighteenth century there was no way for electricians to distinguish consistently between electrical and non-electrical effects, between laboratory accidents and essential novelties, or between striking demonstration and experiments which revealed the essential nature of electricity.

This is the state of affairs which Franklin changed.[8] His theory explained so many—though not all—of the electrical effects recognized by the various earlier schools that within a generation all electricians had been converted to some view very like it. Though it did not resolve quite all disagreements, Franklin's theory was electricity's first paradigm, and its existence gives a new tone and flavor to the electrical researches of the last decades of the eighteenth century. The end of inter-school debate ended the constant reiteration of fundamentals; confidence that they were on the right track encouraged electricians to undertake more precise, esoteric, and consuming sorts of work. Freed from concern with any and all electrical phenomena, the newly united group could pursue selected phenomena in far more detail, designing much special equipment for the task and employing it more stubbornly and systematically than electricians had ever done before. In the hands of a Cavendish, a Coulomb, or a Volta the collection of electrical facts and the articulation of electrical theory were, for the first time, highly directed activities. As a result the efficiency and effectiveness of electrical research increased immensely, providing evidence for a societal version of Francis Bacon's acute methodological dictum: "Truth emerges more readily from error than from confusion."

Obviously I exaggerate both the speed and the completeness with which the transition to a paradigm occurs. But that does not make the phenomenon itself less real. The maturation of electricity as a science is not coextensive with the entire development of the field. Writers on electricity during the first four decades of the eighteenth century possessed far more information about electrical phenomena than had their sixteenth- and seventeenth-century predecessors. During the half-century after 1745 very few new sorts of electrical phenomena were added to their lists. Nevertheless, in important respects the electrical writings of the last two decades of the century seemed further removed from those of Gray, Du Fay, and even Franklin than are the writings of these early eighteenth-century electricians from those of their predecessors a hundred years before. Some time between 1740 and 1780 electricians, as a group, gained what astronomers had achieved in antiquity, students of motion in the Middle Ages, of physical optics in the late seventeenth century, and of historical geology in the early nineteenth. They had, that is, achieved a paradigm, possession of which enabled them to take the foundation of their field for granted and to push on to more concrete and recondite problems.[9] Except with the advantage of hindsight, it is hard to find another criterion that so clearly proclaims a field of science.

These remarks should begin to clarify what I take a paradigm to be. It is, in the first place, a fundamental scientific achievement and one which includes both a theory and some exemplary applications to the results of experiment and observation. More important, it is an open-ended achievement, one which leaves all sorts of research still to be done. And, finally, it is an accepted achievement in the sense that it is received by a group whose members no longer try to rival it or to create alternates for it. Instead, they attempt to extend and exploit it in a variety of ways to which I shall shortly turn. That discussion of the work that paradigms leave to be done will make both their role and the reasons for their special efficacy clearer still. But first there is one rather different point to be made about them. Though the reception of a paradigm seems historically prerequisite to the most effective sorts of scientific research, the paradigms which enhance research effectiveness need not be and usually are not permanent. On the contrary, the developmental pattern of mature science is usually from paradigm to paradigm. It differs from the pattern characteristic of the early or pre-paradigm period not by the total elimination of debate over fundamentals, but by the drastic restriction of such debate to occasional periods of paradigm change.

Ptolemy's *Almagest* was not, for example, any less a paradigm because the research tradition that descended from it had ultimately to be replaced by an incompatible one derived from the work of Copernicus and Kepler. Nor was Newton's *Opticks* less a paradigm for eighteenth-century students of light because it was later replaced by the ether-wave theory of Young and Fresnel, a paradigm which in its turn gave way to the electromagnetic displacement theory that descends from Maxwell. Undoubtedly the research work that any given paradigm permits results in lasting contributions to the body of scientific knowledge and technique, but paradigms themselves are very often swept aside and replaced by others that are quite incompatible with them. We can have no recourse to notions like the "truth" or "validity" of paradigms in our attempt to understand the special efficacy of the research which their reception permits.

On the contrary, the historian can often recognize that in declaring an older paradigm out of date or in rejecting the approach of some one of the pre-paradigm schools a scientific community has rejected the embryo of an important scientific perception to which it would later be forced to return. But it is very far from clear that the profession delayed scientific development by doing so. Would quantum mechanics have been born sooner if nineteenth-century scientists had been more willing to admit that Newton's corpuscular view of light might still have something significant to teach them about nature? I think not, although in the arts, the humanities, and many social sciences that less doctrinaire view is very often adopted toward classic achievements of the past. Or would astronomy and dynamics have advanced more rapidly if scientists had recognized that Ptolemy and Copernicus had chosen equally legitimate means to describe the earth's position? That view was, in fact, suggested during the seventeenth century. But in the interim it was firmly rejected together with Ptolemaic astronomy, emerging again only in the very late nineteenth century when, for the first time, it had concrete relevance to unsolved problems generated by the continuing practice of non-relativistic physics. One could argue, as indeed by implication I shall, that close eighteenth- and nineteenth-century attention either to the work of Ptolemy or to the relativistic views of Descartes, Huygens, and Leibniz would have delayed rather than accelerated the revolution in physics with which the twentieth century began. Advance from paradigm to paradigm rather than through the continuing competition between recognized classics may be a functional as well as a factual characteristic of mature scientific development.

Much that has been said so far is intended to indicate that—except during occasional extraordinary periods to be discussed in the last section of this paper—the practitioners of a mature scientific specialty are deeply committed to some one paradigm-based way of regarding and investigating nature. Their paradigm tells them about the sorts of entities with which the universe is populated and about the way the members of that population behave; in addition, it informs them of the questions that may legitimately be asked about nature and of the techniques that can properly be used in the search for answers to them. In fact, a paradigm tells scientists so much that the questions it leaves for research seldom have great intrinsic interest to those outside the profession. Though educated men as a group may be fascinated to hear about the spectrum of fundamental particles or about the processes of molecular replication, their interest is usually quickly exhausted by an account of the beliefs that already underlie research on these problems. The outcome of the individual research project is indifferent to them, and their interest is unlikely to awaken again until, as with parity nonconservation, research unexpectedly leads to paradigm-change and to a consequent alteration in the beliefs which guide research. That, no doubt, is why both historians and popularizers have devoted so much of their attention to the revolutionary episodes which result in change of paradigm and have so largely neglected the sort of work that even the greatest scientists necessarily do most of the time.

My point will become clearer if I now ask what it is that the existence of a paradigm leaves for the scientific community to do. The answer—as obvious as the related existence of resistance to innovation and as often brushed under the carpet—is that scientists, given a paradigm, strive with all their might and skill to bring it into closer and closer agreement with nature. Much of their effort, particularly in the early stages of a paradigm's development, is directed to articulating the paradigm, rendering it more precise in areas where the original formulation has inevitably been vague. For example, knowing that electricity was a fluid whose individual particles act upon one another at a distance, electricians after Franklin could attempt to determine the quantitative law of force between particles of electricity. Others could seek the mutual interdependence of spark length, electroscope deflection, quantity of electricity, and conductor-configuration. These were the sorts of problems upon which Coulomb, Cavendish, and Volta worked in the last decades of the eighteenth century, and they have many parallels in the development of every other mature science. Contemporary attempts to determine the quantum mechanical forces governing the interactions of nucleons fall precisely in this same category, paradigm-articulation.

That sort of problem is not the only challenge which a paradigm sets for the community that embraces it. There are always many areas in which a paradigm is assumed to work but to which it has not, in fact, yet been applied. Matching the paradigm to nature in these areas often engages much of the best scientific talent in any generation. The eighteenth-century attempts to develop a Newtonian theory of vibrating strings provide one significant example, and the current work on a quantum mechanical theory of solids provides another. In addition, there is always much fascinating work to be done in improving the match between a paradigm and nature in an area where at least limited agreement has already been demonstrated. Theoretical work on problems like these is illustrated by eighteenth-century research on the perturbations that cause planets to deviate from their Keplerian orbits as well as by the elaborate twentieth-century theory of the spectra of complex atoms and molecules. And accompanying all these problems and still others besides is a recurring series of instrumental hurdles. Special apparatus had to be invented and built to permit Coulomb's determination of the electrical force law. New sorts of telescopes were required for the observations that, when completed, demanded an improved Newtonian perturbation theory. The design and construction of more flexible and more powerful accelerators is a continuing desideratum in the attempt to articulate more powerful theories of nuclear forces. These are the sorts of work on which almost all scientists spend almost all of their time.[10]

Probably this epitome of normal scientific research requires no elaboration in this place, but there are two points that must now be made about it. First, all of the problems mentioned above were paradigm-dependent, often in several ways. Some—for example, the derivation of perturbation terms in Newtonian planetary theory—could not even have been stated in the absence of an appropriate paradigm. With the transition from Newtonian to relativity theory a few of them became different problems and not all of these have yet been solved. Other problems—for example, the attempt to determine a law of electric forces—could be and were at least vaguely stated before the emergence of the paradigm with which they were ultimately solved. But in that older form they proved intractable. The men who described electrical attractions and repulsions in terms of effluvia attempted to measure the resulting forces by placing a charged disc at a measured distance beneath one pan of a balance. Under those circumstances no consistent or interpretable results were obtained. The prerequisite for success proved to be a paradigm that reduced electrical action to a gravity-like action between point particles at a distance. After Franklin electricians thought of electrical action in those terms; both Coulomb and Cavendish designed their apparatus accordingly. Finally, in both these cases and in all the others as well a commitment to the paradigm was needed simply to provide adequate motivation. Who would design and build elaborate special-purpose apparatus, or who would spend months trying to solve a particular differential equation, without a quite firm guarantee that his effort, if successful, would yield the anticipated fruit?

This reference to the anticipated outcome of a research project points to the second striking characteristic of what I am now calling normal, or paradigm-based, research. The scientist engaged in it does not at all fit the prevalent image of the scientist as explorer or as inventor of brand new theories which permit striking and unexpected predictions. On the contrary, in all the problems discussed above everything but the detail of the outcome was known in advance. No scientist who accepted Franklin's paradigm could doubt that there was a law of attraction between small particles of electricity, and they could reasonably suppose that it would take a simple algebraic form. Some of them had even guessed that it would prove to be an inverse square law. Nor did Newtonian astronomers and physicists doubt that Newton's laws of motion and of gravitation could ultimately be made to yield the observed motions of the moon and planets even though, for over a century, the complexity of the requisite mathematics prevented good agreements being uniformly obtained. In all these problems, as in most others that scientists undertake, the challenge is not to uncover the unknown but to obtain the known. Their fascination lies not in what success may be expected to disclose but in the difficulty of obtaining success at all. Rather than resembling exploration, normal research seems like the effort to assemble a Chinese cube whose finished outline is known from the start.

Those are the characteristics of normal research that I had in mind when, at the start of this essay, I described the man engaged in it as a puzzle-solver, like the chess player. The paradigm he has acquired through prior training provides him with the rules of the game, describes the pieces with which it must be played, and indicates the nature of the required outcome. His task is to manipulate those pieces within the rules in such a way that the required outcome is produced. If he fails, as most scientists do in at least their first attacks upon any given problem, that failure speaks only to his lack of skill. It cannot call into question the rules that his paradigm has supplied, for without those rules there would have been no puzzle with which to wrestle in the first place. No wonder, then, that the problems (or puzzles) which the practitioner of a mature science normally undertakes presuppose a deep commitment to a paradigm. And how fortunate it is that that commitment is not lightly given up. Experience shows that, in almost all cases, the reiterated efforts, either of the individual or of the professional group, do at last succeed in producing within the paradigm a solution to even the most stubborn problems. That is one of the ways in which science advances. Under those circumstances can we be surprised that scientists resist paradigm-change? What they are defending is, after all, neither more nor less than the basis of their professional way of life.

By now one principal advantage of what I began by calling scientific dogmatism should be apparent. As a glance at any Baconian natural history or a survey of the pre-paradigm development of any science will show, nature is vastly too complex to be explored even approximately at random. Something must tell the scientist where to look and what to look for, and that something, though it may not last beyond his generation, is the paradigm with which his education as a scientist has supplied him. Given that paradigm and the requisite confidence in it, the scientist largely ceases to be an explorer at all, or at least to be an explorer of the unknown. Instead, he struggles to articulate and concretize the known, designing much special-purpose apparatus and many special-purpose adaptations of theory for that task. From those puzzles of design and adaptation he gets his pleasure. Unless he is extraordinarily lucky, it is upon his success with them that his reputation will depend. Inevitably the enterprise which engages him is characterized, at any one time, by drastically restricted vision. But within the region upon which vision is focused the continuing attempt to match paradigms to nature results in a knowledge and understanding of esoteric detail that could not have been achieved in any other way. From Copernicus and the problem of precession to Einstein and the photo-electric effect, the progress of science has again and again depended upon just such esoterica. One great virtue of commitment to paradigms is that it frees scientists to engage themselves with tiny puzzles.

Nevertheless, this image of scientific research as puzzle-solving or paradigm-matching must be, at the very least, thoroughly incomplete. Though the scientist may not be an explorer, scientists do again and again discover new and unexpected sorts of phenomena. Or again, though the scientist does not normally strive to invent new sorts of basic theories, such theories have repeatedly emerged from the continuing practice of research. But neither of these types of innovation would arise if the enterprise I have been calling normal science were always successful. In fact, the man engaged in puzzle-solving very often resists substantive novelty, and he does so for good reason. To him it is a change in the rules of the game and any change of rules is intrinsically subversive. That subversive element is, of course, most apparent in major theoretical innovations like those associated with the names of Copernicus, Lavoisier, or Einstein. But the discovery of an unanticipated phenomenon can have the same destructive effects, although usually on a smaller group and for a far shorter time. Once he had performed his first follow-up experiments, Roentgen's glowing screen demonstrated that previously standard cathode ray equipment was behaving in ways for which no one had made allowance. There was an unanticipated variable to be controlled; earlier researches, already on their way to becoming paradigms, would require re-evaluation; old puzzles would have to be solved again under a somewhat different set of rules. Even so readily assimilable a discovery as that of X rays can violate a paradigm that has previously guided research. It follows that, if the normal puzzle-solving activity were altogether successful, the development of science could lead to no fundamental innovations at all.

But of course normal science is not always successful, and in recognizing that fact we encounter what I take to be the second great advantage of paradigm-based research. Unlike many of the early electricians, the practitioner of a mature science knows with considerable precision what sort of result he should gain from his research. As a consequence he is in a particularly favorable position to recognize when a research problem has gone astray. Perhaps, like Galvani or Roentgen, he encounters an effect that he knows ought not to occur. Or perhaps, like Copernicus, Planck, or Einstein, he concludes that the reiterated failures of his predecessors in matching a paradigm to nature is presumptive evidence of the need to change the rules under which a match is to be sought. Or perhaps, like Franklin or Lavoisier, he decides after repeated attempts that no existing theory can be articulated to account for some newly discovered effect. In all of these ways and in others besides the practice of normal puzzle-solving science can and inevitably does lead to the isolation and recognition of anomaly. That recognition proves, I think, prerequisite for almost all discoveries of new sorts of phenomena and for all fundamental innovations in scientific theory. After a first paradigm has been achieved, a breakdown in the rules of the pre-established game is the usual prelude to significant scientific innovation.

Examine the case of discoveries first. Many of them, like Coulomb's law or a new element to fill an empty spot in the periodic table, present no problem. They were not "new sorts of phenomena" but discoveries anticipated through a paradigm and achieved by expert puzzle-solvers: That sort of discovery is a natural product of what I have been calling normal science. But not all discoveries are of that sort: Many could not have been anticipated by any extrapolation from the known; in a sense they had to be made "by accident." On the other hand the accident through which they emerged could not ordinarily have occurred to a man just looking around. In the mature sciences discovery demands much special equipment, both conceptual and instrumental, and that special equipment has invariably been developed and deployed for the pursuit of the puzzles of normal research. Discovery results when that equipment fails to function as it should. Furthermore, since some sort of at least temporary failure occurs during almost every research project, discovery results only when the failure is particularly stubborn or striking and only when it seems to raise questions about accepted beliefs and procedures. Established paradigms are thus often doubly prerequisite to discoveries. Without them the project that goes astray would not have been undertaken. And even when the project has gone astray, as most do for a while, the paradigm can help to determine whether the failure is worth pursuing. The usual and proper response to a failure in puzzle-solving is to blame one's talents or one's tools and to turn next to another problem. If he is not to waste time, the scientist must be able to discriminate essential anomaly from mere failure.

That pattern—discovery through an anomaly that calls established techniques and beliefs in doubt—has been repeated again and again in the course of scientific development. Newton discovered the composition of white light when he was unable to reconcile measured dispersion with that predicted by Snell's recently discovered law of refraction.[11] The electric battery was discovered when existing detectors of static charges failed to behave as Franklin's paradigm said they should.[12] The planet Neptune was discovered through an effort to account for recognized anomalies in the orbit of Uranus.[13] The element chlorine and the compound carbon monoxide emerged during attempts to reconcile Lavoisier's new chemistry with laboratory observations.[14] The so-called noble gases were the products of a long series of investigations initiated by a small but persistent anomaly in the measured density of atmospheric nitrogen.[15] The electron was posited to explain some anomalous properties of electrical conduction through gases, and its spin was suggested to account for other sorts of anomalies observed in atomic spectra.[16] Both the neutron and the neutrino provide other examples, and the list could be extended almost indefinitely.[17] In the mature sciences unexpected novelties are discovered principally after something has gone wrong.

If, however, anomaly is significant in preparing the way for new discoveries, it plays a still larger role in the invention of new theories. Contrary to a prevalent, though by no means universal, belief, new theories are not invented to account for observations that have not previously been ordered by theory at all. Rather, at almost all times in the development of any advanced science, all the facts whose relevance is admitted seem either to fit existing theory well or to be in the process of conforming. Making them conform better provides many of the standard problems of normal science. And almost always committed scientists succeed in solving them. But they do not always succeed, and, when they fail repeatedly and in increasing numbers, then their sector of the scientific community encounters what I am elsewhere calling "crisis." Recognizing that something is fundamentally wrong with the theory upon which their work is based, scientists will attempt more fundamental articulations of theory than those which were admissible before. (Characteristically, at times of crisis, one encounters numerous different versions of the paradigm theory.[18]) Simultaneously they will often begin more nearly random experimentation within the area of difficulty, hoping to discover some effect that will suggest a way to set the situation right. Only under circumstances like these, I suggest, is a fundamental innovation in scientific theory both invented and accepted.

The state of Ptolemaic astronomy was, for example, a recognized scandal before Copernicus proposed a basic change in astronomical theory, and the preface in which Copernicus described his reasons for innovation provides a classic description of the crisis state.[19] Galileo's contributions to the study of motion took their point of departure from recognized difficulties with medieval theory, and Newton reconciled Galileo's mechanics with Copernicanism.[20] Lavoisier's new chemistry was a product of the anomalies created jointly by the proliferation of new gases and the first systematic studies of weight relations.[21] The wave theory of light was developed amid growing concern about anomalies in the relation of diffraction and polarization effects to Newton's corpuscular theory.[22] Thermodynamics, which later came to seem a superstructure for existing sciences, was established only at the price of rejecting the previously paradigmatic caloric theory.[23] Quantum mechanics was born from a variety of difficulties surrounding black-body radiation, specific heat, and the photo-electric effect.[24] Again the list could be extended, but the point should already be clear. New theories arise from work conducted under old ones, and they do so only when something is observed to have gone wrong. Their prelude is widely recognized anomaly, and that recognition can come only to a group that knows very well what it would mean to have things go right.

Because limitations of space and time force me to stop at this point, my case for dogmatism must remain schematic. I shall not here even attempt to deal with the fine structure that scientific development exhibits at all times. But there is another more positive qualification of my thesis, and it requires one closing comment. Though successful research demands a deep commitment to the status quo, innovation remains at the heart of the enterprise. Scientists are *trained* to operate as puzzle-solvers from established rules, but they are also *taught* to regard themselves as explorer and inventors who know no rules except those dictated by nature itself. The result is an acquired tension, partly within the individual and partly within the community, between professional skills on the one hand and professional ideology on the other. Almost certainly that tension and the ability to sustain it are important to science's success. Insofar as I have dealt exclusively with the dependence of research upon tradition, my discussion is inevitably one-sided. On this whole subject there is a great deal more to be said.

But to be one-sided is not necessarily to be wrong, and it may be an essential preliminary to a more penetrating examination of the requisites for successful scientific life. Almost no one, perhaps no one at all, needs to be told that the vitality of science depends on the continuation of occasional tradition-shattering innovations. But the apparently contrary dependence of research upon a deep commitment to established tools and beliefs receives the very minimum of attention. I urge that it be given more. Until that is done, some of the most striking characteristics of scientific education and development will remain extraordinarily difficult to understand.

Notes

1. The ideas developed in this paper have been abstracted, in a drastically condensed form, from the first third of my monograph, *The Structure of Scientific Revolutions*, published during 1962 by the University of Chicago Press. Some of them were also partially developed in an earlier essay, "The Essential Tension: Tradition and Innovation in Scientific Research," which appeared in Calvin W. Taylor (ed.), *The Third (1959) University of Utah Research Conference on the Identification of Creative Scientific Talent* (Salt Lake City 1959).

   On this whole subject see also I. B. Cohen, "Orthodoxy and Scientific Progress," *Proceedings of the American Philosophical Society*, XCVI (1952) pp. 505–12, and Bernard Barber, "Resistance by Scientists to Scientific Discovery," *Science*, CXXXIV (1961) pp. 596–602. I am indebted to Mr. Barber for an advance copy of that helpful paper. Above all, those concerned with the importance of quasi-dogmatic commitments as a requisite for productive scientific research should see the works of Michael Polanyi, particularly his *Personal Knowledge* (Chicago, 1958) and *The Logic of Liberty* (London, 1951). The discussion which follows this paper will indicate that Mr. Polanyi and I differ somewhat about what scientists are committed to, but that should not disguise the very great extent of our agreement about the issues discussed explicitly below.
2. *Wissenschaftliche Selbstbiographie* (Leipzig, 1948) 22, my translation.
3. The individual sciences display some variation in these respects. Students in the newer and also in the less theoretical sciences—e.g., parts of biology, geology, and medical science—are more likely to encounter both contemporary and historical source materials than those in, say, astronomy, mathematics, or physics.
4. Quoted in this form by S. F. Mason, *Main Currents of Scientific Thought* (New York, 1956) 254. The original, which is identical inspirit but not in words, seems to derie from Delambre's contemporary éloge, *Memoires de . . . l'Institut . . . , année 1812*, 2nd part (Paris, 1816) p. xlvi.
5. Much documentation for this account of electrical development can be retrieved from Duane Roller and Duane H. D. Roller, *The Development of the Concept of Electric Charge: Electricity form the Greeks to Coulomb* (Harvard Case Histories in Experimental Science, VIII, Cambridge, Mass., 1954) and from 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). For analytic detail I am, however, very much indebted to a still unpublished paper by my student, John L. Heilbron, who has also assisted in the preparation of the three notes that follow.

THOMAS KUHN

The Nature and Necessity of Scientific Revolutions

. . . What are scientific revolutions, and what is their function in scientific development? . . . [S]cientific 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. There is more to be said, however, and an essential part of it can be introduced by asking one further question. Why should a change of paradigm be called a revolution? In the face of the vast and essential differences between political and scientific development, what parallelism can justify the metaphor that finds revolutions in both?

One aspect of the parallelism must already be apparent. Political revolutions are inaugurated by a growing sense, often restricted to a segment of the political community, that existing institutions have ceased adequately to meet the problems posed by an environment that they have in part created. In much the same way, scientific revolutions are inaugurated by a growing sense, again often restricted to a narrow subdivision of the scientific community, 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. In both political and scientific development the sense of malfunction that can lead to crisis is prerequisite to revolution. Furthermore, though it admittedly strains the metaphor, that parallelism holds not only for the major paradigm changes, like those attributable to Copernicus and Lavoisier, but also for the far smaller ones associated with the assimilation of a new sort of phenomenon, like oxygen or X rays. Scientific revolutions . . . need seem revolutionary only to those whose paradigms are affected by them. To outsiders they may, like the Balkan revolutions of the

Reprinted from Chapter IX of *The Structure of Scientific Revolutions* by Thomas Kuhn, 2nd edition (1970), pp. 92–110.

early twentieth century, seem normal parts of the developmental process. Astronomers, for example, could accept X rays as a mere addition to knowledge, for their paradigms were unaffected by the existence of the new radiation. But for men like Kelvin, Crookes, and Roentgen, whose research dealt with radiation theory or with cathode ray tubes, the emergence of X rays necessarily violated one paradigm as it created another. That is why these rays could be discovered only through something's first going wrong with normal research.

This genetic aspect of the parallel between political and scientific development should no longer be open to doubt. The parallel has, however, a second and more profound aspect upon which the significance of the first depends. Political revolutions aim to change political institutions in ways that those institutions themselves prohibit. Their success therefore necessitates the partial relinquishment of one set of institutions in favor of another, and in the interim, society is not fully governed by institutions at all. Initially it is crisis alone that attenuates the role of political institutions as we have already seen it attenuate the role of paradigms. In increasing numbers individuals become increasingly estranged from political life and behave more and more eccentrically within it. Then, as the crisis deepens, many of these individuals commit themselves to some concrete proposal for the reconstruction of society in a new institutional framework. At that point the society is divided into competing camps or parties, one seeking to defend the old institutional constellation, the others seeking to institute some new one. And, once that polarization has occurred, *political recourse fails*. Because they differ about the institutional matrix within which political change is to be achieved and evaluated, because they acknowledge no supra-institutional framework for the adjudication of revolutionary difference, the parties to a revolutionary conflict must finally resort to the techniques of mass persuasion, often including force. Though revolutions have had a vital role in the evolution of political institutions, that role depends upon their being partially extrapolitical or extrainstitutional events.

The remainder of this essay aims to demonstrate that the historical study of paradigm change reveals very similar characteristics in the evolution of the sciences. Like the choice between competing political institutions, that between competing paradigms proves to be a choice between incompatible modes of community life. Because it has that character, the choice is not and cannot be determined merely by the evaluative procedures characteristic of normal science, for these depend in part upon a particular paradigm, and that paradigm is at issue. When paradigms enter, as they must, into a debate about paradigm choice, their role is necessarily circular. Each group uses its own paradigm to argue in that paradigm's defense.

The resulting circularity does not, of course, make the arguments wrong or even ineffectual. The man who premises a paradigm when arguing in its defense can nonetheless provide a clear exhibit of what scientific practice will be like for those who adopt the new view of nature. That exhibit can be immensely persuasive, often compellingly so. Yet, whatever its force, the status of the circular argument is only that of persuasion. It cannot be made logically or even probabilistically compelling for those who refuse to step into the circle. The premises and values shared by the two parties to a debate over paradigms are not sufficiently extensive for that. As in political revolutions, so in paradigm choice—there is no standard higher than the assent of the relevant community. To discover how scientific revolutions are effected, we shall therefore have to examine not only the impact of nature and of logic, but also the techniques of persuasive argumentation effective within the quite special groups that constitute the community of scientists.

To discover why this issue of paradigm choice can never be unequivocally settled by logic and experiment alone, we must shortly examine the nature of the differences that separate the proponents of a traditional paradigm from their revolutionary successors. . . . We have, however, already noted numerous examples of such differences, and no one will doubt that history can supply many others. What is more likely to be doubted than their existence—and what must therefore be considered first—is that such examples provide essential information about the nature of science. Granting that paradigm rejection has been a historic fact, does it illuminate more than human credulity and confusion? Are there intrinsic reasons why the assimilation of either a new sort of phenomenon or a new scientific theory must demand the rejection of an older paradigm?

First notice that if there are such reasons, they do not derive from the logical structure of scientific knowledge. In principle, a new phenomenon might emerge without reflecting destructively upon any part of past scientific practice. Though discovering life on the moon would today be destructive of existing paradigms (these tell us things about the moon that seem incompatible with life's existence there), discovering life in some less well-known part of the galaxy would not. By the same token, a new theory does not have to conflict with any of its predecessors. It might deal exclusively with phenomena not previously known, as the quantum theory deals (but, significantly, not exclusively) with subatomic phenomena unknown before the twentieth century. Or again, the new theory might be simply a higher level theory than those known before, one that linked together a whole group of lower level theories without substantially changing any. Today, the theory of energy conservation provides just such links between dynamics, chemistry, electricity, optics, thermal theory, and so on. Still other compatible relationships between old and new theories can be conceived. Any and all of them might be exemplified by the historical process through which science has developed. If they were, scientific development would be genuinely cumulative. New sorts of phenomena would simply disclose order in an aspect of nature where none had been seen before. In the evolution of science new knowledge would replace ignorance rather than replace knowledge of another and incompatible sort.

Of course, science (or some other enterprise, perhaps less effective) might have developed in that fully cumulative manner. Many people have believed that it did so, and most still seem to suppose that cumulation is at least the ideal that historical development would display if only it had not so often been distorted by human idiosyncrasy. There are important reasons for that belief. . . . Nevertheless, despite the immense plausibility of that ideal image, there is increasing reason to wonder whether it can possibly be an image of *science*. After the pre-paradigm period the assimilation of all new theories and of almost all new sorts of phenomena has in fact demanded the destruction of a prior paradigm and a consequent conflict between competing schools of scientific thought. Cumulative acquisition of unanticipated novelties proves to be an almost non-existent exception to the rule of scientific development. The man who takes historic fact seriously must suspect that science does not tend toward the ideal that our image of its cumulativeness has suggested. Perhaps it is another sort of enterprise.

If, however, resistant facts can carry us that far, then a second look at the ground we have already covered may suggest that cumulative acquisition of novelty is not only rare in fact but improbable in principle. Normal research, which *is* cumulative, owes its success to the ability of scientists regularly to select problems that can be solved with conceptual and instrumental techniques close to those already in existence. (That is why an excessive concern with useful problems, regardless of their relation to existing knowledge and technique, can so easily inhibit scientific development.) The man who is striving to solve a problem defined by existing knowledge and technique is not, however, just looking around. He knows what he wants to achieve, and he designs his instruments and directs his thoughts accordingly. Unanticipated novelty, the new discovery, can emerge only to the extent that his anticipations about nature and his instruments prove wrong. Often the importance of the resulting discovery will itself be proportional to the extent and stubbornness of the anomaly that foreshadowed it. Obviously, then, there must be a conflict between the paradigm that discloses anomaly and the one that later renders the anomaly lawlike. . . .

The same argument applies even more clearly to the invention of new theories. There are, in principle, only three types of phenomena about which a new theory might be developed. The first consists of phenomena already well explained by existing paradigms, and these seldom provide either motive or point of departure for theory construction. When they do . . . the theories that result are seldom accepted, because nature provides no ground for discrimination. A second class of phenomena consists of those whose nature is indicated by existing paradigms but whose details can be understood only through further theory articulation. These are the phenomena to which scientists direct their research much of the time, but that research aims at the articulation of existing paradigms rather than at the invention of new ones. Only when these attempts at articulation fail do scientists encounter the third type of phenomena, the recognized anomalies whose characteristic feature is their stubborn refusal to be assimilated to existing paradigms. This type alone gives rise to new theories. Paradigms provide all phenomena except anomalies with a theory-determined place in the scientist's field of vision.

But if new theories are called forth to resolve anomalies in the relation of an existing theory to nature, then the successful new theory must somewhere permit predictions that are different from those derived from its predecessor. That difference could not occur if the two were logically compatible. In the process of being assimilated, the second must displace the first. Even a theory like energy conservation, which today seems a logical superstructure that relates to nature only through independently established theories, did not develop historically without paradigm destruction. Instead, it emerged from a crisis in which an essential ingredient was the incompatibility between Newtonian dynamics and some recently formulated consequences of the caloric theory of heat. Only after the caloric theory had been rejected could energy conservation become part of science.[1] And only after it had been part of science for some time could it come to seem a theory of a logically higher type, one not in conflict with its predecessors. It is hard to see how new theories could arise without these destructive changes in beliefs about nature. Though logical inclusiveness remains a permissible view of the relation between successive scientific theories, it is a historical implausibility.

A century ago it would, I think, have been possible to let the case for the necessity of revolutions rest at this point. But today, unfortunately, that cannot be done because the view of the subject developed above cannot be maintained if the most prevalent contemporary interpretation of the nature and function of scientific theory is accepted. That interpretation, closely associated with early logical positivism and not categorically rejected by its successors, would restrict the range and meaning of an accepted theory so that it would not possibly conflict with any later theory that made predictions about some of the same natural phenomena. The best-known and the strongest case for this restricted conception of a scientific theory emerges in discussions of the relation between contemporary Einsteinian dynamics and the older dynamical equations that descend from Newton's *Principia*. From the viewpoint of this essay these two theories are fundamentally incompatible in the sense illustrated by the relation of Copernican to Ptolemaic astronomy: Einstein's theory can be accepted only with the recognition that Newton's was wrong. Today this remains a minority view.[2] We must therefore examine the most prevalent objections to it.

The gist of these objections can be developed as follows. Relativistic dynamics cannot have shown Newtonian dynamics to be wrong, for Newtonian dynamics is still used with great success by most engineers and, in selected applications, by many physicists. Furthermore, the propriety of this use of the older theory can be proved from the very theory that has, in other applications, replaced it. Einstein's theory can be used to show that predictions from Newton's equations will be as good as our measuring instruments in all applications that satisfy a small number of restrictive conditions. For example, if Newtonian theory is to provide a good approximate solution, the relative velocities of the bodies considered must be small compared with the velocity of light. Subject to this condition and a few others, Newtonian theory seems to be derivable from Einsteinian, of which it is therefore a special case.

But, the objection continues, no theory can possibly conflict with one of its special cases. If Einsteinian science seems to make Newtonian dynamics wrong, that is only because some Newtonians were so incautious as to claim that Newtonian theory yielded entirely precise results or that it was valid at very high relative velocities. Since they could not have had any evidence for such claims, they betrayed the standards of science when they made them. In so far as Newtonian theory was ever a truly scientific theory supported by valid evidence, it still is. Only extravagant claims for the theory—claims that were never properly parts of science—can have been shown by Einstein to be wrong. Purged of these merely human extravagances, Newtonian theory has never been challenged and cannot be.

Some variant of this argument is quite sufficient to make any theory ever used by a significant group of competent scientists immune to attack. The much-maligned phlogiston theory, for example, gave order to a large number of physical and chemical phenomena. It explained why bodies burned—they were rich in phlogiston—and why metals had so many more properties in common than did their ores. The metals were all compounded from different elementary earths combined with phlogiston, and the latter, common to all metals, produced common properties. In addition, the phlogiston theory accounted for a number of reactions in which acids were formed by the combustion of substances like carbon and sulphur. Also, it explained the decrease of volume when combustion occurs in a confined volume of air—the phlogiston released by combustion "spoils" the elasticity of the air that absorbed it, just as fire "spoils" the elasticity of a steel spring.[3] If these were the only phenomena that the phlogiston theorists had claimed for their theory, that theory could never have been challenged. A similar argument will suffice for any theory that has ever been successfully applied to any range of phenomena at all.

But to save theories in this way, their range of application must be restricted to those phenomena and to that precision of observation with which the experimental evidence in hand already deals.[4] Carried just a step further (and the step can scarcely be avoided once the first is taken), such a limitation prohibits the scientist from claiming to speak "scientifically" about any phenomenon not already observed. Even in its present form the restriction forbids the scientist to rely upon a theory in his own research whenever that research enters an area or seeks a degree of precision for which past practice with the theory offers no precedent. These prohibitions are logically unexceptionable. But the result of accepting them would be the end of the research through which science may develop further.

By now that point too is virtually a tautology. Without commitment to a paradigm there could be no normal science. Furthermore, that commitment must extend to areas and to degrees of precision for which there is no full precedent. If it did not, the paradigm could provide no puzzles that had not already been solved. Besides, it is not only normal science that depends upon commitment to a paradigm. If existing theory binds the scientist only with respect to existing applications, then there can be no surprises, anomalies, or crises. But these are just the signposts that point the way to extraordinary science. If positivistic restrictions on the range of a theory's legitimate applicability are taken literally, the mechanism that tells the scientific community what problems may lead to fundamental change must cease to function. And when that occurs, the community will inevitably return to something much like its pre-paradigm state, a condition in which all members practice science but in which their gross product scarcely resembles science at all. Is it really any wonder that the price of significant scientific advance is a commitment that runs the risk of being wrong?

More important, there is a revealing logical lacuna in the positivist's argument, one that will reintroduce us immediately to the nature of revolutionary change. Can Newtonian dynamics really be *derived* from relativistic dynamics? What would such a derivation look like? Imagine a set of statements, $E_1, E_2, \ldots, E_n$, which together embody the laws of relativity theory. These statements contain variables and parameters representing spatial position, time, rest mass, etc. From them, together with the apparatus of logic and mathematics, is deducible a whole set of further statements, including some that can be checked by observation. To prove the adequacy of Newtonian dynamics as a special case, we must add to the E_i's additional statements like $(v/c)^2 < 1$, restricting the range of the parameters and variables. This enlarged set of statements is then manipulated to yield a new set, $N_1, N_2, \ldots, N_m$, which is identical in form with Newton's laws of motion, the law of gravity, and so on. Apparently Newtonian dynamics has been derived from Einsteinian, subject to a few limiting conditions.

Yet the derivation is spurious, at least to this point. Though the N_i's 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 variables and parameters that in the Einsteinian E_i's represented spatial position, time, mass, etc., still occur in the N_i's; and they there still represent Einsteinian space, time, and mass. But 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.) Unless we change the definitions of the variables in the N_i's, the statements we have derived are not Newtonian. If we do change them, we cannot properly be said to have *derived* Newton's Laws, at least not in any sense of "derive" now generally recognized. Our argument has, of course, explained why Newton's Laws ever seemed to work. In doing so it has justified, say, an automobile driver in acting as though he lived in a Newtonian universe. An argument of the same type is used to justify teaching earth-centered astronomy to surveyors. But 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.

This need to change the meaning of established and familiar concepts is central to the revolutionary impact of Einstein's theory. Though subtler than the changes from geocentrism to heliocentrism, from phlogiston to oxygen, or from corpuscles to waves, the resulting conceptual transformation is no less decisively destructive of a previously established paradigm. We may even come to see it as a prototype for revolutionary reorientations in the sciences. Just because it did not involve the introduction of additional objects or concepts, the transition from Newtonian to Einsteinian mechanics illustrates with particular clarity the scientific revolution as a displacement of the conceptual network through which scientists view the world.

These remarks should suffice to show what might, in another philosophical climate, have been taken for granted. At least for scientists, most of the apparent differences between a discarded scientific theory and its successor are real. Though an out-of-date theory can always be viewed as a special case of its up-to-date successor, it must be transformed for the purpose. And the transformation is one that can be undertaken only with the advantages of hindsight, the explicit guidance of the more recent theory. Furthermore, even if that transformation were a legitimate device to employ in interpreting the older theory, the result of its application would be a theory so restricted that it could only restate what was already known. Because of its economy, that restatement would have utility, but it could not suffice for the guidance of research.

Let us, therefore, now take it for granted that the differences between successive paradigms are both necessary and irreconcilable. Can we then say more explicitly what sorts of differences these are? The most apparent type has already been illustrated repeatedly. Successive paradigms tell us different things about the population of the universe and about that population's behavior. They differ, that is, about such questions as the existence of subatomic particles, the materiality of light, and the conservation of heat or of energy. These are the substantive differences between successive paradigms, and they require no further illustration. But paradigms differ in more than substance, for they are directed not only to nature but also back upon the science that produced them. They are the source of the methods, problem-field, and standards of solution accepted by any mature scientific community at any given time. As a result, the reception of a new paradigm often necessitates a redefinition of the corresponding science. Some old problems may be relegated to another science or declared entirely "unscientific." Others that were previously non-existent or trivial may, with a new paradigm, become the very archetypes of significant scientific achievement. 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.

The impact of Newton's work upon the normal seventeenth-century tradition of scientific practice provides a striking example of these subtler effects of paradigm shift. Before Newton was born the "new science" of the century had at last succeeded in rejecting Aristotelian and scholastic explanations expressed in terms of the essences of material bodies. To say that a stone fell because its "nature" drove it toward the center of the universe had been made to look a mere tautological wordplay, something it had not previously been. Henceforth the entire flux of sensory appearances, including color, taste, and even weight, was to be explained in terms of the size, shape, position, and motion of the elementary corpuscles of base matter. The attribution of other qualities to the elementary atoms was a resort to the occult and therefore out of bounds for science. Molière caught the new spirit precisely when he ridiculed the doctor who explained opium's efficacy as a soporific by attributing to it a dormitive potency. During the last half of the seventeenth century many scientists preferred to say that the round shape of the opium particles enabled them to soothe the nerves about which they moved.[5]

In an earlier period explanations in terms of occult qualities had been an integral part of productive scientific work. Nevertheless, the seventeenth century's new commitment to mechanico-corpuscular explanation proved immensely fruitful for a number of sciences, ridding them of problems that had defied generally accepted solution and suggesting others to replace them. In dynamics, for example, Newton's three laws of motion are less a product of novel experiments than of the attempt to reinterpret well-known observations in terms of the motions and interactions of primary neutral corpuscles. Consider just one concrete illustration. Since neutral corpuscles could act on each other only by contact, the mechanico-corpuscular view of nature directed scientific attention to a brand-new subject of study, the alteration of particulate motions by collisions. Descartes announced the problem and provided its first putative solution. Huyghens, Wren, and Wallis carried it still further, partly by experimenting with colliding pendulum bobs, but mostly by applying previously well-known characteristics of motion to the new problem. And Newton embedded their results in his laws of motion. The equal "action" and "reaction" of the third law are the changes in quantity of motion experienced by the two parties to a collision. The same change of motion supplies the definition of dynamical force implicit in the second law. In this case, as in many others during the seventeenth century, the corpuscular paradigm bred both a new problem and a large part of that problem's solution.[6]

Yet, though much of Newton's work was directed to problems and embodied standards derived from the mechanico-corpuscular world view, the effect of the paradigm that resulted from his work was a further and partially destructive change in the problems and standards legitimate for science. Gravity, interpreted as an innate attraction between every pair of particles of matter, was an occult quality in the same sense as the scholastics' "tendency to fall" had been. Therefore, while the standards of corpuscularism remained in effect, the search for a mechanical explanation of gravity was one of the most challenging problems for those who accepted the *Principia* as paradigm. Newton devoted much attention to it and so did many of his eighteenth-century successors. The only apparent option was to reject Newton's theory for its failure to explain gravity, and that alternative, too, was widely adopted. Yet neither of these views ultimately triumphed. Unable either to practice science without the *Principia* or to make that work conform to the corpuscular standards of the seventeenth century, scientists gradually accepted the view that gravity was indeed innate. By the mid-eighteenth century that interpretation had been almost universally accepted, and the result was a genuine reversion (which is not the same as a retrogression) to a scholastic standard. Innate attractions and repulsions joined size, shape, position, and motion as physically irreducible primary properties of matter.[7]

The resulting change in the standards and problem-field of physical science was once again consequential. By the 1740s, for example, electricians could speak of the attractive "virtue" of the electrical fluid without thereby inviting the ridicule that had greeted Molière's doctor a century before. As they did so, electrical phenomena increasingly displayed an order different from the one they had shown when viewed as the effects of a mechanical effluvium that could act only by contact. In particular, when electrical action-at-a-distance became a subject for study in its own right, the phenomenon we now call charging by induction could be recognized as one of its effects. Previously, when seen at all, it had been attributed to the direct action of electrical "atmospheres" or to the leakages inevitable in any electrical laboratory. The new view of inductive effects was, in turn, the key to Franklin's analysis of the Leyden jar and thus to the emergence of a new and Newtonian paradigm for electricity. Nor were dynamics and electricity the only scientific fields affected by the legitimization of the search for forces innate to matter. The large body of eighteenth-century literature on chemical affinities and replacement series also derives from this supramechanical aspect of Newtonianism. Chemists who believed in these differential attractions between the various chemical species set up previously unimagined experiments and searched for new sorts of reactions. Without the data and the chemical concepts developed in that process, the later work of Lavoisier and, more particularly, of Dalton would be incomprehensible.[8] Changes in the standards governing permissible problems, concepts, and explanations can transform a science. . . .

Other examples of these nonsubstantive differences between successive paradigms can be retrieved from the history of any science in almost any period of its development. For the moment let us be content with just two other and far briefer illustrations. Before the chemical revolution, one of the acknowledged tasks of chemistry was to account for the qualities of chemical substances and for the changes these qualities underwent during chemical reactions. With the aid of a small number of elementary "principles"—of which phlogiston was one—the chemist was to explain why some substances are acidic, others metalline, combustible, and so forth. Some success in this direction had been achieved. We have already noted that phlogiston explained why the metals were so much alike, and we could have developed a similar argument for the acids. Lavoisier's reform, however, ultimately did away with chemical "principles," and thus ended by depriving chemistry of some actual and much potential explanatory power. To compensate for this loss, a change in standards was required. During much of the nineteenth-century failure to explain the qualities of compounds was no indictment of a chemical theory.[9]

Or again, Clerk Maxwell shared with other nineteenth-century proponents of the wave theory of light the conviction that light waves must be propagated through a material ether. Designing a mechanical medium to support such waves was a standard problem for many of his ablest contemporaries. His own theory, however, the electromagnetic theory of light, gave no account at all of a medium able to support light waves, and it clearly made such an account harder to provide than it had seemed before. Initially, Maxwell's theory was widely rejected for those reasons. But, like Newton's theory, Maxwell's proved difficult to dispense with, and as it achieved the status of a paradigm, the community's attitude toward it changed. In the early decades of the twentieth century Maxwell's insistence upon the existence of a mechanical ether looked more and more like lip service, which it emphatically had not been, and the attempts to design such an ethereal medium were abandoned. Scientists no longer thought it unscientific to speak of an electrical "displacement" without specifying what was being displaced. The result, again, was a new set of problems and standards, one which, in the event, had much to do with the emergence of relativity theory.[10]

These characteristic shifts in the scientific community's conception of its legitimate problems and standards would have less significance to this essay's thesis if one could suppose that they always occurred from some methodologically lower to some higher type. In that case their effects, too, would seem cumulative. No wonder that some historians have argued that the history of science records a continuing increase in the maturity and refinement of man's conception of the nature of science.[11] Yet the case for cumulative development of science's problems and standards is even harder to make than the case of cumulation of theories. The attempt to explain gravity, though fruitfully abandoned by most eighteenth-century scientists, was not directed to an intrinsically illegitimate problem; the objections to innate forces were neither inherently unscientific nor metaphysical in some pejorative sense. There are no external standards to permit a judgment of that sort. What occurred was neither a decline nor a raising of standards, but simply a change demanded by the adoption of a new paradigm. Furthermore, that change has since been reversed and could be again. In the twentieth century Einstein succeeded in explaining gravitational attractions, and that explanation has returned science to a set of canons and problems that are, in this particular respect, more like those of Newton's predecessors than of his successors. Or again, the development of quantum mechanics has reversed the methodological prohibition that originated in the chemical revolution. Chemists now attempt, and with great success, to explain the color, state of aggregation, and other qualities of the substances used and produced in their laboratories. A similar reversal may even be underway in electromagnetic theory. Space, in contemporary physics, is not the inert and homogenous substratum employed in both Newton's and Maxwell's theories; some of its new properties are not unlike those once attributed to the ether; we may someday come to know what an electric displacement is.

By shifting emphasis from the cognitive to the normative functions of paradigms, the preceding examples enlarge our understanding of the ways in which paradigms give form to the scientific life. Previously, we had principally examined the paradigm's role as a vehicle for scientific theory. In that role it functions by telling the scientist about the entities that nature does and does not contain and about the ways in which those entities behave. That information provides a map whose details are elucidated by mature scientific research. And since nature is too complex and varied to be explored at random, that map is as essential as observation and experiment to science's continuing development. Through the theories they embody, paradigms prove to be constitutive of the research activity. They are also, however, constitutive of science in other respects, and that is now the point. In particular, our most recent examples show that paradigms provide scientists not only with a map but also with some of the directions essential for map-making. In learning a paradigm the scientist acquires theory, methods, and standards together, usually in an inextricable mixture. Therefore, when paradigms change, there are usually significant shifts in the criteria determining the legitimacy both of problems and of proposed solutions.

That observation returns us to the point from which this section began, for it provides our first explicit indication of why the choice between competing paradigms regularly raises questions that cannot be resolved by the criteria of normal science. To the extent, as significant as it is incomplete, that two scientific schools disagree about what is a problem and what a solution, they will inevitably talk through each other when debating the relative merits of their respective paradigms. In the partially circular arguments that regularly result, each paradigm will be shown to satisfy more or less the criteria that it dictates for itself and to fall short of a few of those dictated by its opponent. There are other reasons, too, for the incompleteness of logical contact that consistently characterizes paradigm debates. For example, since no paradigm ever solves all the problems it defines and since no two paradigms leave all the same problems unsolved, paradigm debates always involve the question: Which problems is it more significant to have solved? Like the issue of competing standards, that question of values can be answered only in terms of criteria that lie outside of normal science altogether, and it is that recourse to external criteria that most obviously makes paradigm debates revolutionary . . . .

Notes

1. Silvanus P. Thompson, *Life of William Thomson Baron Kelvin of Largs* (London, 1910), I, pp. 266–81.
2. See, for example, the remarks by P. P. Wiener in *Philosophy of Science*, XXV (1958), pp. 298.
3. James B. Conant, *Overthrow of the Phlogiston Theory* (Cambridge, 1950), pp. 13–16; and J. R. Partington, *A Short History of Chemistry* (2d ed.; London, 1951), pp. 85–88. The fullest and most sympathetic account of the phlogiston theory's achievements is by H. Metzger, *Newton, Stahl, Boerhaave et al doctrine chimique* (Paris, 1930), Part II.