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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 counter-evidence. 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

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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.