

VIII. The Response to Crisis

Let us then assume that crises are a necessary precondition for the emergence of novel theories and ask next how scientists respond to their existence. Part of the answer, as obvious as it is important, can be discovered by noting first what scientists never do when confronted by even severe and prolonged anomalies. Though they may begin to lose faith and then to consider alternatives, they do not renounce the paradigm that has led them into crisis. They do not, that is, treat anomalies as counterinstances, though in the vocabulary of philosophy of science that is what they are. In part this generalization is simply a statement from historic fact, based upon examples like those given above and, more extensively, below. These hint what our later examination of paradigm rejection will disclose more fully: once it has achieved the status of paradigm, a scientific theory is declared invalid only if an alternate candidate is available to take its place. No process yet disclosed by the historical study of scientific development at all resembles the methodological stereotype of falsification by direct comparison with nature. That remark does not mean that scientists do not reject scientific theories, or that experience and experiment are not essential to the process in which they do so. But it does mean—what will ultimately be a central point—that the act of judgment that leads scientists to reject a previously accepted theory is always based upon more than a comparison of that theory with the world. The decision to reject one paradigm is always simultaneously the decision to accept another, and the judgment leading to that decision involves the comparison of both paradigms with nature *and* with each other.

There is, in addition, a second reason for doubting that scientists reject paradigms because confronted with anomalies or counterinstances. In developing it my argument will itself foreshadow another of this essay's main theses. The reasons for doubt sketched above were purely factual; they were, that is,

themselves counterinstances to a prevalent epistemological theory. As such, if my present point is correct, they can at best help to create a crisis or, more accurately, to reinforce one that is already very much in existence. By themselves they cannot and will not falsify that philosophical theory, for its defenders will do what we have already seen scientists doing when confronted by anomaly. They will devise numerous articulations and *ad hoc* modifications of their theory in order to eliminate any apparent conflict. Many of the relevant modifications and qualifications are, in fact, already in the literature. If, therefore, these epistemological counterinstances are to constitute more than a minor irritant, that will be because they help to permit the emergence of a new and different analysis of science within which they are no longer a source of trouble. Furthermore, if a typical pattern, which we shall later observe in scientific revolutions, is applicable here, these anomalies will then no longer seem to be simply facts. From within a new theory of scientific knowledge, they may instead seem very much like tautologies, statements of situations that could not conceivably have been otherwise.

It has often been observed, for example, that Newton's second law of motion, though it took centuries of difficult factual and theoretical research to achieve, behaves for those committed to Newton's theory very much like a purely logical statement that no amount of observation could refute.¹ In Section X we shall see that the chemical law of fixed proportion, which before Dalton was an occasional experimental finding of very dubious generality, became after Dalton's work an ingredient of a definition of chemical compound that no experimental work could by itself have upset. Something much like that will also happen to the generalization that scientists fail to reject paradigms when faced with anomalies or counterinstances. They could not do so and still remain scientists.

Though history is unlikely to record their names, some men have undoubtedly been driven to desert science because of

¹ See particularly the discussion in N. R. Hanson, *Patterns of Discovery* (Cambridge, 1958), pp. 99-105.

their inability to tolerate crisis. Like artists, creative scientists must occasionally be able to live in a world out of joint—elsewhere I have described that necessity as “the essential tension” implicit in scientific research.² But that rejection of science in favor of another occupation is, I think, the only sort of paradigm rejection to which counterinstances by themselves can lead. Once a first paradigm through which to view nature has been found, there is no such thing as research in the absence of any paradigm. To reject one paradigm without simultaneously substituting another is to reject science itself. That act reflects not on the paradigm but on the man. Inevitably he will be seen by his colleagues as “the carpenter who blames his tools.”

The same point can be made at least equally effectively in reverse: there is no such thing as research without counterinstances. For what is it that differentiates normal science from science in a crisis state? Not, surely, that the former confronts no counterinstances. On the contrary, what we previously called the puzzles that constitute normal science exist only because no paradigm that provides a basis for scientific research ever completely resolves all its problems. The very few that have ever seemed to do so (e.g., geometric optics) have shortly ceased to yield research problems at all and have instead become tools for engineering. Excepting those that are exclusively instrumental, every problem that normal science sees as a puzzle can be seen, from another viewpoint, as a counterinstance and thus as a source of crisis. Copernicus saw as counterinstances what most of Ptolemy's other successors had seen as puzzles in the match between observation and theory. Lavoisier saw as a counterinstance what Priestley had seen as a successfully solved puzzle in the articulation of the phlogiston theory. And Einstein saw as counterinstances what Lorentz, Fitzgerald, and others had seen as puzzles in the articulation of Newton's and Max-

² T. S. Kuhn, “The Essential Tension: Tradition and Innovation in Scientific Research,” in *The Third (1959) University of Utah Research Conference on the Identification of Creative Scientific Talent*, ed. Calvin W. Taylor (Salt Lake City, 1959), pp. 162-77. For the comparable phenomenon among artists, see Frank Barron, “The Psychology of Imagination,” *Scientific American*, CXCIX (September, 1958), 151-66, esp. 160.

well's theories. Furthermore, even the existence of crisis does not by itself transform a puzzle into a counterinstance. There is no such sharp dividing line. Instead, by proliferating versions of the paradigm, crisis loosens the rules of normal puzzle-solving in ways that ultimately permit a new paradigm to emerge. There are, I think, only two alternatives: either no scientific theory ever confronts a counterinstance, or all such theories confront counterinstances at all times.

How can the situation have seemed otherwise? That question necessarily leads to the historical and critical elucidation of philosophy, and those topics are here barred. But we can at least note two reasons why science has seemed to provide so apt an illustration of the generalization that truth and falsity are uniquely and unequivocally determined by the confrontation of statement with fact. Normal science does and must continually strive to bring theory and fact into closer agreement, and that activity can easily be seen as testing or as a search for confirmation or falsification. Instead, its object is to solve a puzzle for whose very existence the validity of the paradigm must be assumed. Failure to achieve a solution discredits only the scientist and not the theory. Here, even more than above, the proverb applies: "It is a poor carpenter who blames his tools." In addition, the manner in which science pedagogy entangles discussion of a theory with remarks on its exemplary applications has helped to reinforce a confirmation-theory drawn predominantly from other sources. Given the slightest reason for doing so, the man who reads a science text can easily take the applications to be the evidence for the theory, the reasons why it ought to be believed. But science students accept theories on the authority of teacher and text, not because of evidence. What alternatives have they, or what competence? The applications given in texts are not there as evidence but because learning them is part of learning the paradigm at the base of current practice. If applications were set forth as evidence, then the very failure of texts to suggest alternative interpretations or to discuss problems for which scientists have failed to produce paradigm solutions

would convict their authors of extreme bias. There is not the slightest reason for such an indictment.

How, then, to return to the initial question, do scientists respond to the awareness of an anomaly in the fit between theory and nature? What has just been said indicates that even a discrepancy unaccountably larger than that experienced in other applications of the theory need not draw any very profound response. There are always some discrepancies. Even the most stubborn ones usually respond at last to normal practice. Very often scientists are willing to wait, particularly if there are many problems available in other parts of the field. We have already noted, for example, that during the sixty years after Newton's original computation, the predicted motion of the moon's perigee remained only half of that observed. As Europe's best mathematical physicists continued to wrestle unsuccessfully with the well-known discrepancy, there were occasional proposals for a modification of Newton's inverse square law. But no one took these proposals very seriously, and in practice this patience with a major anomaly proved justified. Clairaut in 1750 was able to show that only the mathematics of the application had been wrong and that Newtonian theory could stand as before.³ Even in cases where no mere mistake seems quite possible (perhaps because the mathematics involved is simpler or of a familiar and elsewhere successful sort), persistent and recognized anomaly does not always induce crisis. No one seriously questioned Newtonian theory because of the long-recognized discrepancies between predictions from that theory and both the speed of sound and the motion of Mercury. The first discrepancy was ultimately and quite unexpectedly resolved by experiments on heat undertaken for a very different purpose; the second vanished with the general theory of relativity after a crisis that it had had no role in creating.⁴ Apparent-

³ W. Whewell, *History of the Inductive Sciences* (rev. ed.; London, 1847), II, 220-21.

⁴ For the speed of sound, see T. S. Kuhn, "The Caloric Theory of Adiabatic Compression," *Isis*, XLIV (1958), 136-37. For the secular shift in Mercury's perihelion, see E. T. Whittaker, *A History of the Theories of Aether and Electricity*, II (London, 1953), 151, 179.

ly neither had seemed sufficiently fundamental to evoke the malaise that goes with crisis. They could be recognized as counterinstances and still be set aside for later work.

It follows that if an anomaly is to evoke crisis, it must usually be more than just an anomaly. There are always difficulties somewhere in the paradigm-nature fit; most of them are set right sooner or later, often by processes that could not have been foreseen. The scientist who pauses to examine every anomaly he notes will seldom get significant work done. We therefore have to ask what it is that makes an anomaly seem worth concerted scrutiny, and to that question there is probably no fully general answer. The cases we have already examined are characteristic but scarcely prescriptive. Sometimes an anomaly will clearly call into question explicit and fundamental generalizations of the paradigm, as the problem of ether drag did for those who accepted Maxwell's theory. Or, as in the Copernican revolution, an anomaly without apparent fundamental import may evoke crisis if the applications that it inhibits have a particular practical importance, in this case for calendar design and astrology. Or, as in eighteenth-century chemistry, the development of normal science may transform an anomaly that had previously been only a vexation into a source of crisis: the problem of weight relations had a very different status after the evolution of pneumatic-chemical techniques. Presumably there are still other circumstances that can make an anomaly particularly pressing, and ordinarily several of these will combine. We have already noted, for example, that one source of the crisis that confronted Copernicus was the mere length of time during which astronomers had wrestled unsuccessfully with the reduction of the residual discrepancies in Ptolemy's system.

When, for these reasons or others like them, an anomaly comes to seem more than just another puzzle of normal science, the transition to crisis and to extraordinary science has begun. The anomaly itself now comes to be more generally recognized as such by the profession. More and more attention is devoted to it by more and more of the field's most eminent men. If it still continues to resist, as it usually does not, many of them may

come to view its resolution as *the* subject matter of their discipline. For them the field will no longer look quite the same as it had earlier. Part of its different appearance results simply from the new fixation point of scientific scrutiny. An even more important source of change is the divergent nature of the numerous partial solutions that concerted attention to the problem has made available. The early attacks upon the resistant problem will have followed the paradigm rules quite closely. But with continuing resistance, more and more of the attacks upon it will have involved some minor or not so minor articulation of the paradigm, no two of them quite alike, each partially successful, but none sufficiently so to be accepted as paradigm by the group. Through this proliferation of divergent articulations (more and more frequently they will come to be described as *ad hoc* adjustments), the rules of normal science become increasingly blurred. Though there still is a paradigm, few practitioners prove to be entirely agreed about what it is. Even formerly standard solutions of solved problems are called in question.

When acute, this situation is sometimes recognized by the scientists involved. Copernicus complained that in his day astronomers were so "inconsistent in these [astronomical] investigations . . . that they cannot even explain or observe the constant length of the seasonal year." "With them," he continued, "it is as though an artist were to gather the hands, feet, head and other members for his images from diverse models, each part excellently drawn, but not related to a single body, and since they in no way match each other, the result would be monster rather than man."⁵ Einstein, restricted by current usage to less florid language, wrote only, "It was as if the ground had been pulled out from under one, with no firm foundation to be seen anywhere, upon which one could have built."⁶ And Wolfgang Pauli, in the months before Heisenberg's paper on matrix

⁵ Quoted in T. S. Kuhn, *The Copernican Revolution* (Cambridge, Mass., 1957), p. 138.

⁶ Albert Einstein, "Autobiographical Note," in *Albert Einstein: Philosopher-Scientist*, ed. P. A. Schilpp (Evanston, Ill., 1949), p. 45.

mechanics pointed the way to a new quantum theory, wrote to a friend, "At the moment physics is again terribly confused. In any case, it is too difficult for me, and I wish I had been a movie comedian or something of the sort and had never heard of physics." That testimony is particularly impressive if contrasted with Pauli's words less than five months later: "Heisenberg's type of mechanics has again given me hope and joy in life. To be sure it does not supply the solution to the riddle, but I believe it is again possible to march forward."⁷

Such explicit recognitions of breakdown are extremely rare, but the effects of crisis do not entirely depend upon its conscious recognition. What can we say these effects are? Only two of them seem to be universal. All crises begin with the blurring of a paradigm and the consequent loosening of the rules for normal research. In this respect research during crisis very much resembles research during the pre-paradigm period, except that in the former the locus of difference is both smaller and more clearly defined. And all crises close in one of three ways. Sometimes normal science ultimately proves able to handle the crisis-provoking problem despite the despair of those who have seen it as the end of an existing paradigm. On other occasions the problem resists even apparently radical new approaches. Then scientists may conclude that no solution will be forthcoming in the present state of their field. The problem is labelled and set aside for a future generation with more developed tools. Or, finally, the case that will most concern us here, a crisis may end with the emergence of a new candidate for paradigm and with the ensuing battle over its acceptance. This last mode of closure will be considered at length in later sections, but we must anticipate a bit of what will be said there in order to complete these remarks about the evolution and anatomy of the crisis state.

The transition from a paradigm in crisis to a new one from which a new tradition of normal science can emerge is far from a cumulative process, one achieved by an articulation or exten-

⁷ Ralph Kronig, "The Turning Point," in *Theoretical Physics in the Twentieth Century: A Memorial Volume to Wolfgang Pauli*, ed. M. Fierz and V. F. Weisskopf (New York, 1960), pp. 22, 25-26. Much of this article describes the crisis in quantum mechanics in the years immediately before 1925.

sion of the old paradigm. Rather it is a reconstruction of the field from new fundamentals, a reconstruction that changes some of the field's most elementary theoretical generalizations as well as many of its paradigm methods and applications. During the transition period there will be a large but never complete overlap between the problems that can be solved by the old and by the new paradigm. But there will also be a decisive difference in the modes of solution. When the transition is complete, the profession will have changed its view of the field, its methods, and its goals. One perceptive historian, viewing a classic case of a science's reorientation by paradigm change, recently described it as "picking up the other end of the stick," a process that involves "handling the same bundle of data as before, but placing them in a new system of relations with one another by giving them a different framework."⁸ Others who have noted this aspect of scientific advance have emphasized its similarity to a change in visual gestalt: the marks on paper that were first seen as a bird are now seen as an antelope, or vice versa.⁹ That parallel can be misleading. Scientists do not see something *as* something else; instead, they simply see it. We have already examined some of the problems created by saying that Priestley saw oxygen as dephlogisticated air. In addition, the scientist does not preserve the gestalt subject's freedom to switch back and forth between ways of seeing. Nevertheless, the switch of gestalt, particularly because it is today so familiar, is a useful elementary prototype for what occurs in full-scale paradigm shift.

The preceding anticipation may help us recognize crisis as an appropriate prelude to the emergence of new theories, particularly since we have already examined a small-scale version of the same process in discussing the emergence of discoveries. Just because the emergence of a new theory breaks with one tradition of scientific practice and introduces a new one conducted under different rules and within a different universe of

⁸ Herbert Butterfield, *The Origins of Modern Science, 1300-1800* (London, 1949), pp. 1-7.

⁹ Hanson, *op. cit.*, chap. i.

discourse, it is likely to occur only when the first tradition is felt to have gone badly astray. That remark is, however, no more than a prelude to the investigation of the crisis-state, and, unfortunately, the questions to which it leads demand the competence of the psychologist even more than that of the historian. What is extraordinary research like? How is anomaly made law-like? How do scientists proceed when aware only that something has gone fundamentally wrong at a level with which their training has not equipped them to deal? Those questions need far more investigation, and it ought not all be historical. What follows will necessarily be more tentative and less complete than what has gone before.

Often a new paradigm emerges, at least in embryo, before a crisis has developed far or been explicitly recognized. Lavoisier's work provides a case in point. His sealed note was deposited with the French Academy less than a year after the first thorough study of weight relations in the phlogiston theory and before Priestley's publications had revealed the full extent of the crisis in pneumatic chemistry. Or again, Thomas Young's first accounts of the wave theory of light appeared at a very early stage of a developing crisis in optics, one that would be almost unnoticeable except that, with no assistance from Young, it had grown to an international scientific scandal within a decade of the time he first wrote. In cases like these one can say only that a minor breakdown of the paradigm and the very first blurring of its rules for normal science were sufficient to induce in someone a new way of looking at the field. What intervened between the first sense of trouble and the recognition of an available alternate must have been largely unconscious.

In other cases, however—those of Copernicus, Einstein, and contemporary nuclear theory, for example—considerable time elapses between the first consciousness of breakdown and the emergence of a new paradigm. When that occurs, the historian may capture at least a few hints of what extraordinary science is like. Faced with an admittedly fundamental anomaly in theory, the scientist's first effort will often be to isolate it more precisely and to give it structure. Though now aware that they

cannot be quite right, he will push the rules of normal science harder than ever to see, in the area of difficulty, just where and how far they can be made to work. Simultaneously he will seek for ways of magnifying the breakdown, of making it more striking and perhaps also more suggestive than it had been when displayed in experiments the outcome of which was thought to be known in advance. And in the latter effort, more than in any other part of the post-paradigm development of science, he will look almost like our most prevalent image of the scientist. He will, in the first place, often seem a man searching at random, trying experiments just to see what will happen, looking for an effect whose nature he cannot quite guess. Simultaneously, since no experiment can be conceived without some sort of theory, the scientist in crisis will constantly try to generate speculative theories that, if successful, may disclose the road to a new paradigm and, if unsuccessful, can be surrendered with relative ease.

Kepler's account of his prolonged struggle with the motion of Mars and Priestley's description of his response to the proliferation of new gases provide classic examples of the more random sort of research produced by the awareness of anomaly.¹⁰ But probably the best illustrations of all come from contemporary research in field theory and on fundamental particles. In the absence of a crisis that made it necessary to see just how far the rules of normal science could stretch, would the immense effort required to detect the neutrino have seemed justified? Or, if the rules had not obviously broken down at some undisclosed point, would the radical hypothesis of parity non-conservation have been either suggested or tested? Like much other research in physics during the past decade, these experiments were in part attempts to localize and define the source of a still diffuse set of anomalies.

This sort of extraordinary research is often, though by no

¹⁰ For an account of Kepler's work on Mars, see J. L. E. Dreyer, *A History of Astronomy from Thales to Kepler* (2d ed.; New York, 1953), pp. 380-93. Occasional inaccuracies do not prevent Dreyer's précis from providing the material needed here. For Priestley, see his own work, esp. *Experiments and Observations on Different Kinds of Air* (London, 1774-75).

means generally, accompanied by another. It is, I think, particularly in periods of acknowledged crisis that scientists have turned to philosophical analysis as a device for unlocking the riddles of their field. Scientists have not generally needed or wanted to be philosophers. Indeed, normal science usually holds creative philosophy at arm's length, and probably for good reasons. To the extent that normal research work can be conducted by using the paradigm as a model, rules and assumptions need not be made explicit. In Section V we noted that the full set of rules sought by philosophical analysis need not even exist. But that is not to say that the search for assumptions (even for non-existent ones) cannot be an effective way to weaken the grip of a tradition upon the mind and to suggest the basis for a new one. It is no accident that the emergence of Newtonian physics in the seventeenth century and of relativity and quantum mechanics in the twentieth should have been both preceded and accompanied by fundamental philosophical analyses of the contemporary research tradition.¹¹ Nor is it an accident that in both these periods the so-called thought experiment should have played so critical a role in the progress of research. As I have shown elsewhere, the analytical thought experimentation that bulks so large in the writings of Galileo, Einstein, Bohr, and others is perfectly calculated to expose the old paradigm to existing knowledge in ways that isolate the root of crisis with a clarity unattainable in the laboratory.¹²

With the deployment, singly or together, of these extraordinary procedures, one other thing may occur. By concentrating scientific attention upon a narrow area of trouble and by preparing the scientific mind to recognize experimental anomalies for what they are, crisis often proliferates new discoveries. We have already noted how the awareness of crisis distinguishes

¹¹ For the philosophical counterpoint that accompanied seventeenth-century mechanics, see René Dugas, *La mécanique au XVII^e siècle* (Neuchâtel, 1954), particularly chap. xi. For the similar nineteenth-century episode, see the same author's earlier book, *Histoire de la mécanique* (Neuchâtel, 1950), pp. 419-43.

¹² T. S. Kuhn, "A Function for Thought Experiments," in *Mélanges Alexandre Koyré*, ed. R. Taton and I. B. Cohen, to be published by Hermann (Paris) in 1963.

Lavoisier's work on oxygen from Priestley's; and oxygen was not the only new gas that the chemists aware of anomaly were able to discover in Priestley's work. Or again, new optical discoveries accumulated rapidly just before and during the emergence of the wave theory of light. Some, like polarization by reflection, were a result of the accidents that concentrated work in an area of trouble makes likely. (Malus, who made the discovery, was just starting work for the Academy's prize essay on double refraction, a subject widely known to be in an unsatisfactory state.) Others, like the light spot at the center of the shadow of a circular disk, were predictions from the new hypothesis, ones whose success helped to transform it to a paradigm for later work. And still others, like the colors of scratches and of thick plates, were effects that had often been seen and occasionally remarked before, but that, like Priestley's oxygen, had been assimilated to well-known effects in ways that prevented their being seen for what they were.¹³ A similar account could be given of the multiple discoveries that, from about 1895, were a constant concomitant of the emergence of quantum mechanics.

Extraordinary research must have still other manifestations and effects, but in this area we have scarcely begun to discover the questions that need to be asked. Perhaps, however, no more are needed at this point. The preceding remarks should suffice to show how crisis simultaneously loosens the stereotypes and provides the incremental data necessary for a fundamental paradigm shift. Sometimes the shape of the new paradigm is foreshadowed in the structure that extraordinary research has given to the anomaly. Einstein wrote that before he had any substitute for classical mechanics, he could see the interrelation between the known anomalies of black-body radiation, the photoelectric effect, and specific heats.¹⁴ More often no such structure is consciously seen in advance. Instead, the new paradigm, or a sufficient hint to permit later articulation, emerges

¹³ For the new optical discoveries in general, see V. Ronchi, *Histoire de la lumière* (Paris, 1956), chap. vii. For the earlier explanation of one of these effects, see J. Priestley, *The History and Present State of Discoveries Relating to Vision, Light and Colours* (London, 1772), pp. 498-520.

¹⁴ Einstein, *loc. cit.*

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all at once, sometimes in the middle of the night, in the mind of a man deeply immersed in crisis. What the nature of that final stage is—how an individual invents (or finds he has invented) a new way of giving order to data now all assembled—must here remain inscrutable and may be permanently so. Let us here note only one thing about it. Almost always the men who achieve these fundamental inventions of a new paradigm have been either very young or very new to the field whose paradigm they change.¹⁵ And perhaps that point need not have been made explicit, for obviously these are the men who, being little committed by prior practice to the traditional rules of normal science, are particularly likely to see that those rules no longer define a playable game and to conceive another set that can replace them. ¹⁾

The resulting transition to a new paradigm is scientific revolution, a subject that we are at long last prepared to approach directly. Note first, however, one last and apparently elusive respect in which the material of the last three sections has prepared the way. Until Section VI, where the concept of anomaly was first introduced, the terms 'revolution' and 'extraordinary science' may have seemed equivalent. More important, neither term may have seemed to mean more than 'non-normal science,' a circularity that will have bothered at least a few readers. In practice, it need not have done so. We are about to discover that a similar circularity is characteristic of scientific theories. Bothersome or not, however, that circularity is no longer unqualified. This section of the essay and the two preceding have educated numerous criteria of a breakdown in normal scientific activity, criteria that do not at all depend upon whether breakdown is succeeded by revolution. Confronted with anomaly or

¹⁵ This generalization about the role of youth in fundamental scientific research is so common as to be a cliché. Furthermore, a glance at almost any list of fundamental contributions to scientific theory will provide impressionistic confirmation. Nevertheless, the generalization badly needs systematic investigation. Harvey C. Lehman (*Age and Achievement* [Princeton, 1953]) provides many useful data; but his studies make no attempt to single out contributions that involve fundamental reconceptualization. Nor do they inquire about the special circumstances, if any, that may accompany relatively late productivity in the sciences.

with crisis, scientists take a different attitude toward existing paradigms, and the nature of their research changes accordingly. The proliferation of competing articulations, the willingness to try anything, the expression of explicit discontent, the recourse to philosophy and to debate over fundamentals, all these are symptoms of a transition from normal to extraordinary research. It is upon their existence more than upon that of revolutions that the notion of normal science depends.

stability

IX. The Nature and Necessity of Scientific Revolutions

These remarks permit us at last to consider the problems that provide this essay with its title. What are scientific revolutions, and what is their function in scientific development? Much of the answer to these questions has been anticipated in earlier sections. In particular, the preceding discussion has indicated that scientific revolutions are here taken to be those non-cumulative developmental episodes in which an older paradigm is replaced in whole or in part by an incompatible new one. 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, as we noted at the end of Section V, need seem revolutionary only to

those whose paradigms are affected by them. To outsiders they may, like the Balkan revolutions of the 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. That examination is the principal object of this section and the next. 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. In Section X we shall discover how closely the view of science-as-cumulation is entangled with a dominant epistemology that takes knowledge to be a construction placed directly upon raw sense data by the mind. And in Section XI we shall examine the strong support provided to the same historiographic schema by the techniques of effective science pedagogy. 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 law-like. The examples of discovery through paradigm destruction examined in Section VI did not confront us with mere historical accident. There is no other effective way in which discoveries might be generated.

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, as with the three famous anticipations discussed at the end of Section VII, 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 independent-

ly 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 could 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.

¹ Silvanus P. Thompson, *Life of William Thomson Baron Kelvin of Largs* (London, 1910), I, 266-81.

² See, for example, the remarks by P. P. Wiener in *Philosophy of Science*, XXV (1958), 298.

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 prop-

erties. 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

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 la doctrine chimique* (Paris, 1930), Part II.

⁴ Compare the conclusions reached through a very different sort of analysis by R. B. Braithwaite, *Scientific Explanation* (Cambridge, 1953), pp. 50–87, esp. p. 76.

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, \dots, 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 $(v/c)^2 \ll 1$, 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, \dots, 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 repre-

sent 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 word-play, 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 sooth 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

⁵ For corpuscularism in general, see Marie Boas, "The Establishment of the Mechanical Philosophy," *Osiris*, X (1952), 412-541. For the effect of particle-shape on taste, see *ibid.*, p. 483.

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, posi-

⁶ R. Dugas, *La mécanique au XVII^e siècle* (Neuchatel, 1954), pp. 177-85, 284-98, 345-56.

tion, 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 1740's, for example, electricians could speak of the attractive "virtue" of the electric 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. In the next section I shall even suggest a sense in which they transform the world.

⁷ 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), chaps. vi-vii.

⁸ For electricity, see *ibid.*, chaps. viii-ix. For chemistry, see Metzger, *op. cit.*, Part I.

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

⁹ E. Meyerson, *Identity and Reality* (New York, 1930), chap. x.

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 for 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 rever-

¹⁰ E. T. Whittaker, *A History of the Theories of Aether and Electricity*, II (London, 1953), 28-30.

¹¹ For a brilliant and entirely up-to-date attempt to fit scientific development into this Procrustean bed, see C. C. Gillispie, *The Edge of Objectivity: An Essay in the History of Scientific Ideas* (Princeton, 1960).

sal 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. Something even more fundamental than standards and values is, however, also at stake. I have so far argued only that paradigms are constitutive of science. Now I wish to display a sense in which they are constitutive of nature as well.