

Chapter 7

Scientific Revolutions

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A. THE ESSENTIAL TENSION: TRADITION AND INNOVATION IN SCIENTIFIC RESEARCH

I am grateful for the invitation to participate in this important conference, and I interpret it as evidence that students of creativity themselves possess the sensitivity to divergent approaches that they seek to identify in others. But I am not altogether sanguine about the outcome of your experiment with me. As most of you already know, I am no psychologist, but rather an ex-physicist now working in the history of science. Probably my concern is no less with creativity than your own, but my goals, my techniques, and my sources of evidence are so very different from yours that I am far from sure how much we do, or even *should*, have to say to each other. These reservations imply no apology: rather they hint at my central thesis. In the sciences, as I shall suggest below, it is often better to do one's best with the tools at hand than to pause for contemplation of divergent approaches.

If a person of my background and interests has anything relevant to suggest to this conference, it will not be about your central concerns, the creative personality and its early identification. But implicit in the numerous working papers distributed to participants in this conference is an image of the scientific process and of the scientist; that image almost certainly conditions many of the experiments you try as well as the conclusions you draw; and about it the physicist-historian may well have something to say. I shall restrict my attention to one aspect of this image—an aspect epitomized as follows in one of the working papers: The basic scientist "must lack prejudice to a degree where he can look at the most 'self-evident' facts or concepts without necessarily accepting them, and, conversely, allow his imagination to play with the most unlikely possibilities" (Selye, 1959). In the more technical language supplied by other working papers (Getzels and Jackson), this aspect of the image recurs as an emphasis upon "divergent thinking, the freedom to go off in different directions, . . . rejecting the old solution and striking out in some new direction."

I do not at all doubt that this description of "divergent thinking" and the concomitant search for those able to do it are entirely proper. Some divergence characterizes all scientific work, and gigantic divergences lie at the core of the most significant episodes in scientific development. But both my own experience in scientific research and my reading of the history of sciences lead me to wonder whether flexibility and open-mindedness have not been too exclusively emphasized as the characteristics requisite for basic research. I shall therefore suggest below that something like "con-

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vergent thinking" is just as essential to scientific advance as is divergent. Since these two modes of thought are inevitably in conflict, it will follow that the ability to support a tension that can occasionally become almost unbearable is one of the prime requisites for the very best sort of scientific research.

I am elsewhere studying these points more historically, with emphasis on the importance to scientific development of "revolutions."¹ These are episodes—exemplified in their most extreme and readily recognized form by the advent of Copernicanism, Darwinism, or Einsteinianism—in which a scientific community abandons one time-honored way of regarding the world and of pursuing science in favor of some other, usually incompatible, approach to its discipline. I have argued in the draft that the historian constantly encounters many far smaller but structurally similar revolutionary episodes and that they are central to scientific advance. Contrary to a prevalent impression, most new discoveries and theories in the sciences are not merely additions to the existing stockpile of scientific knowledge. To assimilate them the scientist must usually rearrange the intellectual and manipulative equipment he has previously relied upon, discarding some elements of his prior belief and practice while finding new significances in and new relationships between many others. Because the old must be revalued and reordered when assimilating the new, discovery and invention in the sciences are usually intrinsically revolutionary. Therefore, they do demand just that flexibility and open-mindedness that characterize, or indeed define, the divergent thinker. Let us henceforth take for granted the need for these characteristics. Unless many scientists possessed them to a marked degree, there would be no scientific revolutions and very little scientific advance.

Yet flexibility is not enough, and what remains is not obviously compatible with it. Drawing from various fragments of a project still in progress, I must now emphasize that revolutions are but one of two complementary aspects of scientific advance. Almost none of the research undertaken by even the greatest scientists is designed to be revolutionary, and very little of it has any such effect. On the contrary, normal research, even the best of it, is a highly convergent activity based firmly upon a settled consensus acquired from scientific education and reinforced by subsequent life in the profession. Typically, to be sure, this convergent or consensus-bound research ultimately results in revolution. Then, traditional techniques and beliefs are abandoned and replaced by new ones. But revolutionary shifts of a scientific tradition are relatively rare, and extended periods of convergent research are the necessary preliminary to them. As I shall indicate below, only investigations firmly rooted in the contemporary scientific tradition are likely to break that tradition and give rise to a new one. That is why I speak of an 'essential tension' implicit in scientific research. To do his job the scientist must undertake a complex set of intellectual and manipulative commitments. Yet his claim to fame, if he has the talent and good luck to gain one, may finally rest upon his ability to abandon this net of commitments in favor of another of his own invention. Very often the successful scientist must simultaneously display the characteristics of the traditionalist and of the iconoclast.²

The multiple historical examples upon which any full documentation of these points must depend are prohibited by the time limitations of the conference. But another approach will introduce you to at least part of what I have in mind—an examination of the nature of education in the natural sciences. One of the working papers for this conference (Getzels and Jackson) quotes Guilford's very apt description of scientific education as follows: "[It] has emphasized abilities in the areas of convergent thinking and evaluation, often at the expense of development in the area of divergent thinking. We have attempted to teach students how to arrive at 'correct' answers that our

civilization has taught us are correct.... Outside the arts [and I should include most of the social sciences] we have generally discouraged the development of divergent-thinking abilities, unintentionally." That characterization seems to me eminently just, but I wonder whether it is equally just to deplore the product that results. Without defending plain bad teaching, and granting that in this country the trend to convergent thinking in all education may have proceeded entirely too far, we may nevertheless recognize that a rigorous training in convergent thought has been intrinsic to the sciences almost from their origin. I suggest that they could not have achieved their present state or status without it.

Let me try briefly to epitomize the nature of education in the natural sciences, ignoring the many significant yet minor differences between the various sciences and between the approaches of different educational institutions. The single most striking feature of this education is that, to an extent totally unknown in other creative fields, it is conducted entirely through textbooks. Typically, undergraduate *and* graduate students of chemistry, physics, astronomy, geology, or biology acquire the substance of their fields from books written especially for students. Until they are ready, or very nearly ready, to commence work on their own dissertations, they are neither asked to attempt trial research projects nor exposed to the immediate products of research done by others, that is, to the professional communications that scientists write for each other. There are no collections of "readings" in the natural sciences. Nor are science students encouraged to read the historical classics of their fields—works in which they might discover other ways of regarding the problems discussed in their textbooks, but in which they would also meet problems, concepts, and standards of solution that their future professions have long since discarded and replaced.

In contrast, the various textbooks that the student does encounter display different subject matters, rather than, as in many of the social sciences, exemplifying different approaches to a single problem field. Even books that compete for adoption in a single course differ mainly in level and in pedagogic detail, not in substance or conceptual structure. Last, but most important of all, is the characteristic technique of textbook presentation. Except in their occasional introductions, science textbooks do not describe the sorts of problems that the professional may be asked to solve and the variety of techniques available for their solution. Rather, these books exhibit 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 very closely related in both method and substance to those through which the textbook or the accompanying lecture has led him. Nothing could be better calculated to produce "mental sets" or *Einstellungen*. Only in their most elementary courses do other academic fields offer as much as a partial parallel.

Even the most faintly liberal educational theory must view this pedagogic technique as anathema. Students, we would all agree, must begin by learning a good deal of what is already known, but we also insist that education give them vastly more. They must, we say, learn to recognize and evaluate problems to which no unequivocal solution has yet been given; they must be supplied with an arsenal of techniques for approaching these future problems; and they must learn to judge the relevance of these techniques and to evaluate the possibly partial solutions which they can provide. In many respects these attitudes toward education seem to me entirely right, and yet we must recognize two things about them. First, education in the natural sciences seems to have been totally unaffected by their existence. It remains a dogmatic initiation in a pre-established tradition that the student is not equipped to evaluate. Second, at least in the period when it was followed by a term in an apprenticeship relation, this technique of

exclusive exposure to a rigid tradition has been immensely productive of the most consequential sorts of innovations.

I shall shortly inquire about the pattern of scientific practice that grows out of this educational initiation and will then attempt to say why that pattern proves quite so successful. But first, an historical excursion will reinforce what has just been said and prepare the way for what is to follow. I should like to suggest that the various fields of natural science have not always been characterized by rigid education in exclusive paradigms, but that each of them acquired something like that technique at precisely the point when the field began to make rapid and systematic progress. If one asks about the origin of our contemporary knowledge of chemical composition, of earthquakes, of biological reproduction, of motion through space, or of any other subject matter known to the natural sciences, one immediately encounters a characteristic pattern that I shall here illustrate with a single example.

Today, physics textbooks tell us that light exhibits some properties of a wave and some of a particle: both textbook problems and research problems are designed accordingly. But both this view and these textbooks are products of an early twentieth-century revolution. (One characteristic of scientific revolutions is that they call for the rewriting of science textbooks.) For more than half a century before 1900, the books employed in scientific education had been equally unequivocal in stating that light was wave motion. Under those circumstances scientists worked on somewhat different problems and often embraced rather different sorts of solutions to them. The nineteenth-century textbook tradition does not, however, mark the beginning of our subject matter. Throughout the eighteenth century and into the early nineteenth, Newton's *Opticks* and the other books from which men learned science taught almost all students that light was particles, and research guided by this tradition was again different from that which succeeded it. Ignoring a variety of subsidiary changes within these three successive traditions, we may therefore say that our views derive historically from Newton's views by way of two revolutions in optical thought, each of which replaced one tradition of convergent research with another. If we make appropriate allowances for changes in the locus and materials of scientific education, we may say that each of these three traditions was embodied in the sort of education by exposure to unequivocal paradigms that I briefly epitomized above. Since Newton, education and research in physical optics have normally been highly convergent.

The history of theories of light does not, however, begin with Newton. If we ask about knowledge in the field before his time, we encounter a significantly different pattern—a pattern still familiar in the arts and in some social sciences, but one which has largely disappeared in the natural sciences. From remote antiquity until the end of the seventeenth century there was no single set of paradigms for the study of physical optics. Instead, many men advanced a large number of different views about the nature of light. Some of these views found few adherents, but a number of them gave rise to continuing schools of optical thought. Although the historian can note the emergence of new points of view as well as changes in the relative popularity of older ones, there was never anything resembling consensus. As a result, a new man entering the field was inevitably exposed to a variety of conflicting viewpoints; he was forced to examine the evidence for each, and there always was good evidence. The fact that he made a choice and conducted himself accordingly could not entirely prevent his awareness of other possibilities. This earlier mode of education was obviously more suited to produce a scientist without prejudice, alert to novel phenomena, and flexible in his approach to his field. On the other hand, one can scarcely escape the impression

that, during the period characterized by this more liberal educational practice, physical optics made very little progress.³

The preconsensus (we might here call it the divergent) phase in the development of physical optics is, I believe, duplicated in the history of all other scientific specialties, excepting only those that were born by the subdivision and recombination of pre-existing disciplines. In some fields, like mathematics and astronomy, the first firm consensus is prehistoric. In others, like dynamics, geometric optics, and parts of physiology, the paradigms that produced a first consensus date from classical antiquity. Most other natural sciences, though their problems were often discussed in antiquity, did not achieve a first consensus until after the Renaissance. In physical optics, as we have seen, the first firm consensus dates only from the end of the seventeenth century; in electricity, chemistry, and the study of heat, it dates from the eighteenth; while in geology and the nontaxonomic parts of biology no very real consensus developed until after the first third of the nineteenth century. This century appears to be characterized by the emergence of a first consensus in parts of a few of the social sciences.

In all the fields named above, important work was done before the achievement of the maturity produced by consensus. Neither the nature nor the timing of the first consensus in these fields can be understood without a careful examination of both the intellectual and the manipulative techniques developed before the existence of unique paradigms. But the transition to maturity is not less significant because individuals practiced science before it occurred. On the contrary, history strongly suggests that, though one can practice science—as one does philosophy or art or political science—without a firm consensus, this more flexible practice will not produce the pattern of rapid consequential scientific advance to which recent centuries have accustomed us. In that pattern, development occurs from one consensus to another, and alternate approaches are not ordinarily in competition. Except under quite special conditions, the practitioner of a mature science does not pause to examine divergent modes of explanation or experimentation.

I shall shortly ask how this can be so—how a firm orientation toward an apparently unique tradition can be compatible with the practice of the disciplines most noted for the persistent production of novel ideas and techniques. But it will help first to ask what the education that so successfully transmits such a tradition leaves to be done. What can a scientist working within a deeply rooted tradition and little trained in the perception of significant alternatives hope to do in his professional career? Once again limits of time force me to drastic simplification, but the following remarks will at least suggest a position that I am sure can be documented in detail.

In pure or basic science—that somewhat ephemeral category of research undertaken by men whose most immediate goal is to increase understanding rather than control of nature—the characteristic problems are almost always repetitions, with minor modifications, of problems that have been undertaken and partially resolved before. For example, much of the research undertaken within a scientific tradition is an attempt to adjust existing theory or existing observation in order to bring the two into closer and closer agreement. The constant examination of atomic and molecular spectra during the years since the birth of wave mechanics, together with the design of theoretical approximations for the prediction of complex spectra, provides one important instance of this typical sort of work. Another was provided by the remarks about the eighteenth-century development of Newtonian dynamics in the paper on measurement supplied to you in advance of the conference.⁴ The attempt to make existing theory and observation conform more closely is not, of course, the only standard sort of research problem in the basic sciences. The development of chemical thermo-

dynamics or the continuing attempts to unravel organic structure illustrate another type—the extension of existing theory to areas that it is expected to cover but in which it has never before been tried. In addition, to mention a third common sort of research problem, many scientists constantly collect the concrete data (e.g., atomic weights, nuclear moments) required for the application and extension of existing theory.

These are normal research projects in the basic sciences, and they illustrate the sorts of work on which all scientists, even the greatest, spend most of their professional lives and on which many spend all. Clearly their pursuit is neither intended nor likely to produce fundamental discoveries or revolutionary changes in scientific theory. Only if the validity of the contemporary scientific tradition is assumed do these problems make much theoretical or any practical sense. The man who suspected the existence of a totally new type of phenomenon or who had basic doubts about the validity of existing theory would not think problems so closely modeled on textbook paradigms worth undertaking. It follows that the man who does undertake a problem of this sort—and that means all scientists at most times—aims to elucidate the scientific tradition in which he was raised rather than to change it. Furthermore, the fascination of his work lies in the difficulties of elucidation rather than in any surprises that the work is likely to produce. Under normal conditions the research scientist is not an innovator but a solver of puzzles, and the puzzles upon which he concentrates are just those which he believes can be both stated and solved within the existing scientific tradition.

Yet—and this is the point—the ultimate effect of this tradition-bound work has invariably been to change the tradition. Again and again the continuing attempt to elucidate a currently received tradition has at last produced one of those shifts in fundamental theory, in problem field, and in scientific standards to which I previously referred as scientific revolutions. At least for the scientific community as a whole, work within a well-defined and deeply ingrained tradition seems more productive of tradition-shattering novelties than work in which no similarly convergent standards are involved. How can this be so? I think it is because no other sort of work is nearly so well suited to isolate for continuing and concentrated attention those loci of trouble or causes of crisis upon whose recognition the most fundamental advances in basic science depend.

As I have indicated in the first of my working papers, new theories and, to an increasing extent, novel discoveries in the mature sciences are not born *de novo*. On the contrary, they emerge from old theories and within a matrix of old beliefs about the phenomena that the world does *and does not* contain. Ordinarily such novelties are far too esoteric and recondite to be noted by the man without a great deal of scientific training. And even the man with considerable training can seldom afford simply to go out and look for them, let us say by exploring those areas in which existing data and theory have failed to produce understanding. Even in a mature science there are always far too many such areas, areas in which no existing paradigms seem obviously to apply and for whose exploration few tools and standards are available. More likely than not the scientist who ventured into them, relying merely upon his receptivity to new phenomena and his flexibility to new patterns of organization, would get nowhere at all. He would rather return his science to its preconsensus or natural history phase.

Instead, the practitioner of a mature science, from the beginning of his doctoral research, continues to work in the regions for which the paradigms derived from his education and from the research of his contemporaries seem adequate. He tries, that is, to elucidate topographical detail on a map whose main outlines are available in

advance, and he hopes—if he is wise enough to recognize the nature of his field—that he will some day undertake a problem in which the anticipated does *not* occur, a problem that goes wrong in ways suggestive of a fundamental weakness in the paradigm itself. In the mature sciences the prelude to much discovery and to all novel theory is not ignorance, but the recognition that something has gone wrong with existing knowledge and beliefs.

What I have said so far may indicate that it is sufficient for the productive scientist to adopt existing theory as a lightly held tentative hypothesis, employ it *faute de mieux* in order to get a start in his research, and then abandon it as soon as it leads him to a trouble spot, a point at which something has gone wrong. But though the ability to recognize trouble when confronted by it is surely a requisite for scientific advance, trouble must not be too easily recognized. The scientist requires a thoroughgoing commitment to the tradition with which, if he is fully successful, he will break. In part this commitment is demanded by the nature of the problems the scientist normally undertakes. These, as we have seen, are usually esoteric puzzles whose challenge lies less in the information disclosed by their solutions (all but its details are often known in advance) than in the difficulties of technique to be surmounted in providing any solution at all. Problems of this sort are undertaken only by men assured that there is a solution which ingenuity can disclose, and only current theory could possibly provide assurance of that sort. That theory alone gives meaning to most of the problems of normal research. To doubt it is often to doubt that the complex technical puzzles which constitute normal research have any solutions at all. Who, for example, would have developed the elaborate mathematical techniques required for the study of the effects of interplanetary attractions upon basic Keplerian orbits if he had not assumed that Newtonian dynamics, applied to the planets then known, would explain the last details of astronomical observation? But without that assurance, how would Neptune have been discovered and the list of planets changed?

In addition, there are pressing practical reasons for commitment. Every research problem confronts the scientist with anomalies whose sources he cannot quite identify. His theories and observations never quite agree; successive observations never yield quite the same results; his experiments have both theoretical and phenomenological by-products which it would take another research project to unravel. Each of these anomalies or incompletely understood phenomena could conceivably be the clue to a fundamental innovation in scientific theory or technique, but the man who pauses to examine them one by one never completes his first project. Reports of effective research repeatedly imply that all but the most striking and central discrepancies could be taken care of by current theory if only there were time to take them on. The men who make these reports find most discrepancies trivial or uninteresting, an evaluation that they can ordinarily base only upon their faith in current theory. Without that faith their work would be wasteful of time and talent.

Besides, lack of commitment too often results in the scientist's undertaking problems that he has little chance of solving. Pursuit of an anomaly is fruitful only if the anomaly is more than nontrivial. Having discovered it, the scientist's first efforts and those of his profession are to do what nuclear physicists are now doing. They strive to generalize the anomaly, to discover other and more revealing manifestations of the same effect, to give it structure by examining its complex interrelationships with phenomena they still feel they understand. Very few anomalies are susceptible to this sort of treatment. To be so they must be in explicit and unequivocal conflict with some structurally central tenet of current scientific belief. Therefore, their recognition and evaluation once again depend upon a firm commitment to the contemporary scientific tradition.

This central role of an elaborate and often esoteric tradition is what I have principally had in mind when speaking of the essential tension in scientific research. I do not doubt that the scientist must be, at least potentially, an innovator, that he must possess mental flexibility, and that he must be prepared to recognize troubles where they exist. That much of the popular stereotype is surely correct, and it is important accordingly to search for indices of the corresponding personality characteristics. But what is no part of our stereotype and what appears to need careful integration with it is the other face of this same coin. We are, I think, more likely fully to exploit our potential scientific talent if we recognize the extent to which the basic scientist must also be a firm traditionalist, or, if I am using your vocabulary at all correctly, a convergent thinker. Most important of all, we must seek to understand how these two superficially discordant modes of problem solving can be reconciled both within the individual and within the group.

Everything said above needs both elaboration and documentation. Very likely some of it will change in the process. This paper is a report on work in progress. But, though I insist that much of it is tentative and all of it incomplete, I still hope that the paper has indicated why an educational system best described as an initiation into an unequivocal tradition should be thoroughly compatible with successful scientific work. And I hope, in addition, to have made plausible the historical thesis that no part of science has progressed very far or very rapidly before this convergent education and correspondingly convergent normal practice became possible. Finally, though it is beyond my competence to derive personality correlates from this view of scientific development, I hope to have made meaningful the view that the productive scientist must be a traditionalist who enjoys playing intricate games by pre-established rules in order to be a successful innovator who discovers new rules and new pieces with which to play them.

As first planned, my paper was to have ended at this point. But work on it, against the background supplied by the working papers distributed to conference participants, has suggested the need for a postscript. Let me therefore briefly try to eliminate a likely ground of misunderstanding and simultaneously suggest a problem that urgently needs a great deal of investigation.

Everything said above was intended to apply strictly only to basic science, an enterprise whose practitioners have ordinarily been relatively free to choose their own problems. Characteristically, as I have indicated, these problems have been selected in areas where paradigms were clearly applicable but where exciting puzzles remained about how to apply them and how to make nature conform to the results of the application. Clearly the inventor and applied scientist are not generally free to choose puzzles of this sort. The problems among which they may choose are likely to be largely determined by social, economic, or military circumstances external to the sciences. Often the decision to seek a cure for a virulent disease, a new source of household illumination, or an alloy able to withstand the intense heat of rocket engines must be made with little reference to the state of the relevant science. It is, I think, by no means clear that the personality characteristics requisite for pre-eminence in this more immediately practical sort of work are altogether the same as those required for a great achievement in basic science. History indicates that only a few individuals, most of whom worked in readily demarcated areas, have achieved eminence in both.

I am by no means clear where this suggestion leads us. The troublesome distinctions between basic research, applied research, and invention need far more investigation.

Nevertheless, it seems likely, for example, that the applied scientist, to whose problems no scientific paradigm need be fully relevant, may profit by a far broader and less rigid education than that to which the pure scientist has characteristically been exposed. Certainly there are many episodes in the history of technology in which lack of more than the most rudimentary scientific education has proved to be an immense help. This group scarcely needs to be reminded that Edison's electric light was produced in the face of unanimous scientific opinion that the arc light could not be "subdivided," and there are many other episodes of this sort.

This must not suggest, however, that mere differences in education will transform the applied scientist into a basic scientist or vice versa. One could at least argue that Edison's personality, ideal for the inventor and perhaps also for the "oddball" in applied science, barred him from fundamental achievements in the basic sciences. He himself expressed great scorn for scientists and thought of them as wooly-headed people to be hired when needed. But this did not prevent his occasionally arriving at the most sweeping and irresponsible scientific theories of his own. (The pattern recurs in the early history of electrical technology: both Tesla and Gramme advanced absurd cosmic schemes that they thought deserved to replace the current scientific knowledge of their day.) Episodes like this reinforce an impression that the personality requisites of the pure scientist and of the inventor may be quite different, perhaps with those of the applied scientist lying somewhere between.⁵

Is there a further conclusion to be drawn from all this? One speculative thought forces itself upon me. If I read the working papers correctly, they suggest that most of you are really in search of the *inventive* personality, a sort of person who does emphasize divergent thinking but whom the United States has already produced in abundance. In the process you may be ignoring certain of the essential requisites of the basic scientist, a rather different sort of person, to whose ranks America's contributions have as yet been notoriously sparse. Since most of you are, in fact, Americans, this correlation may not be entirely coincidental.

Notes

1. *The Structure of Scientific Revolutions* (Chicago, 1962).
2. Strictly speaking, it is the professional group rather than the individual scientist that must display both these characteristics simultaneously. In a fuller account of the ground covered in this paper that distinction between individual and group characteristics would be basic. Here I can only note that, though recognition of the distinction weakens the conflict or tension referred to above, it does not eliminate it. Within the group some individuals may be more traditionalistic, others more iconoclastic, and their contributions may differ accordingly. Yet education, institutional norms, and the nature of the job to be done will inevitably combine to insure that all group members will, to a greater or lesser extent, be pulled in both directions.
3. The history of physical optics before Newton has recently been well described by Vasco Ronchi in *Histoire de la lumière*, trans. J. Taton (Paris, 1956). His account does justice to the element I elaborate too little above. Many fundamental contributions to physical optics were made in the two millennia before Newton's work. Consensus is not prerequisite to a sort of progress in the natural sciences, any more than it is to progress in the social sciences or the arts. It is, however, prerequisite to the sort of progress that we now generally refer to when distinguishing the natural sciences from the arts and from most social sciences.
4. A revised version appeared in *Isis* 52 (1961): 161–93.
5. For the attitude of scientists toward the technical possibility of the incandescent light see Francis A. Jones, *Thomas Alva Edison* (New York, 1908), pp. 99–100, and Harold C. Passer, *The Electrical Manufacturers, 1875–1900* (Cambridge, Mass., 1953), pp. 82–83. For Edison's attitude toward scientists see Passer, *ibid.*, pp. 180–81. For a sample of Edison's theorizing in realms otherwise subject to scientific treatments see Dagobert D. Runes, ed., *The Diary and Sundry Observations of Thomas Alva Edison* (New York, 1948), pp. 205–44, *passim*.

B. THE NATURE AND NECESSITY OF SCIENTIFIC REVOLUTIONS

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

One aspect of the parallelism must already be apparent. Political revolutions are inaugurated by a growing sense, often restricted to a segment of the political community, that existing institutions have ceased adequately to meet the problems posed by an environment that they have in part created. In much the same way, scientific revolutions are inaugurated by a growing sense, again often restricted to a narrow subdivision of the scientific community, that an existing paradigm has ceased to function adequately in the exploration of an aspect of nature to which that paradigm itself had previously led the way. In both political and scientific development the sense of malfunction that can lead to crisis is prerequisite to revolution. Furthermore, though it admittedly strains the metaphor, that parallelism holds not only for the major paradigm changes, like those attributable to Copernicus and Lavoisier, but also for the far smaller ones associated with the assimilation of a new sort of phenomenon, like oxygen or X-rays. Scientific revolutions ... need seem revolutionary only to those whose paradigms are affected by them. To outsiders they may, like the Balkan revolutions of the 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 extra-institutional 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 . . . 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 nonexistent 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, call 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 . . . 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, . . . the theories that result are seldom accepted, because nature provides no ground for discrimination. A second class of phenomena consists of those whose nature is indicated by existing paradigms but whose details can be understood only through further theory articulation. These are the phenomena to which scientists direct their research much of the time, but that research aims at the articulation of existing paradigms rather than at the invention of new ones. Only when these attempts at articulation fail do scientists encounter the third type of phenomena, the recognized anomalies whose characteristic feature is their stubborn refusal to be assimilated to existing paradigms. This type alone gives rise to new theories. Paradigms provide all phenomena except anomalies with a theory-determined place in the scientist's field of vision.

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

A century ago it would, I think, have been possible to let the case for the necessity of revolutions rest at this point. But today, unfortunately, that cannot be done because the view of the subject developed above cannot be maintained if the most prevalent contemporary interpretation of the nature and function of scientific theory is accepted. That interpretation, closely associated with early logical positivism and not categorically rejected by its successors, would restrict the range and meaning of an accepted theory so that it 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.

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

But, the objection continues, no theory can possibly conflict with one of its special cases. If Einsteinian science seems to make Newtonian dynamics wrong, that is only because some Newtonians were so incautious as to claim that Newtonian theory yielded entirely precise results or that it was valid at very high relative velocities. Since they could not have had any evidence for such claims, they betrayed the standards of science when they made them. In so far as Newtonian theory was ever a truly scientific theory supported by valid evidence, it still is. Only extravagant claims for the theory

—claims that were never properly parts of science—can have been shown by Einstein to be wrong. Purged of these merely human extravagances, Newtonian theory has never been challenged and cannot be.

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

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

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

More important, there is a revealing logical lacuna in the positivist’s argument, one that will reintroduce us immediately to the nature of revolutionary change. Can Newtonian dynamics really be *derived* from relativistic dynamics? What would such a derivation look like? Imagine a set of statements, E_1, E_2, \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 represent Einsteinian space, time, and mass. But the physical referents of these Einsteinian concepts are by no means identical with those of the Newtonian concepts that bear the same name. (Newtonian mass is conserved; Einsteinian is convertible with energy. Only at low relative velocities may the two be measured in the same way, and even then they must not be conceived to be the same.) Unless we change the definitions of the variables in the N_i 's, the statements we have derived are not Newtonian. If we do change them, we cannot properly be said to have *derived* Newton's laws, at least not in any sense of "derive" now generally recognized. Our argument has, of course, explained why Newton's laws ever seemed to work. In doing so it has justified, say, an automobile driver in acting as though he lived in a Newtonian universe. An argument of the same type is used to justify teaching earth-centered astronomy to surveyors. But the argument has still not done what it purported to do. It has not, that is, shown Newton's laws to be a limiting case of Einstein's. For in the passage to the limit it is not only the forms of the laws that have changed. Simultaneously we have had to alter the fundamental structural elements of which the universe to which they apply is composed.

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

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

Let us, therefore, now take it for granted that the differences between successive paradigms are both necessary and irreconcilable. Can we then say more explicitly what sorts of differences these are? The most apparent type has already been illustrated

repeatedly. Successive paradigms tell us different things about the population of the universe and about that population's behavior. They differ, that is, about such questions as the existence of subatomic particles, the materiality of light, and the conservation of heat or of energy. These are the substantive differences between successive paradigms, and they require no further illustration. But paradigms differ in more than substance, for they are directed not only to nature but also back upon the science that produced them. They are the source of the methods, problem-field, and standards of solution accepted by any mature scientific community at any given time. As a result, the reception of a new paradigm often necessitates a redefinition of the corresponding science. Some old problems may be relegated to another science or declared entirely "unscientific." Others that were previously nonexistent 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 solution. Huyghens, Wren, and Wallis carried it still further, partly by experimenting with colliding pendulum bobs, but mostly by applying previously well-known characteristics of motion to the new problem. And Newton embedded their results in his laws of motion. The equal "action" and "reaction" of the third law are the changes in quantity of motion experienced by the two parties to a collision. The same change of motion supplies the definition of dynamical force implicit in the second law. In this case, as in many others during the seventeenth century, the corpuscular paradigm bred both a new problem and a large part of that problem's solution.⁶

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

The resulting change in the standards and problem-field of physical science was once again consequential. By the 1740s, for example, electricians could speak of the attractive "virtue" of the 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 ...

Other examples of these nonsubstantive differences between successive paradigms can be retrieved from the history of any science in almost any period of its development. For the moment let us be content with just two other and far briefer illustrations. Before the chemical revolution, one of the acknowledged tasks of chemistry was to account for the qualities of chemical substances and for the changes these qualities underwent during chemical reactions. With the aid of a small number of elementary "principles"—of which phlogiston was one—the chemist was to explain why some substances are acidic, others metalline, combustible, and so forth. Some success in this direction had been achieved. We have already noted that phlogiston explained why the metals were so much alike, and we could have developed a similar argument for the acids. Lavoisier's reform, however, ultimately did away with chemical "principles,"

and thus ended by depriving chemistry of some actual and much potential explanatory power. To compensate for this loss, a change in standards was required. During much of the nineteenth century failure to explain the qualities of compounds was no indictment of a chemical theory.⁹

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

These characteristic shifts in the scientific community's conception of its legitimate problems and standards would have less significance to this essay's thesis if one could suppose that they always occurred from some methodologically lower to some higher type. In that case their effects, too, would seem cumulative. No wonder that some historians have argued that the history of science records a continuing increase in the maturity and refinement of man's conception of the nature of science.¹¹ Yet the case for cumulative development of science's problems and standards is even harder to make than the case 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 reversal may even be underway in electromagnetic theory. Space, in contemporary physics, is not the inert and homogenous substratum employed in both Newton's and Maxwell's theories; some of its new properties are not unlike those once attributed to the ether; we may someday come to know what an electric displacement is.

By shifting emphasis from the cognitive to the normative functions of paradigms, the preceding examples enlarge our understanding of the ways in which paradigms give form to the scientific life. Previously, we had principally examined the paradigm's role as a vehicle for scientific theory. In that role it functions by telling the scientist about the entities that nature does and does not contain and about the ways in which those entities behave. That information provides a map whose details are elucidated by

mature scientific research. And since nature is too complex and varied to be explored at random, that map is as essential as observation and experiment to science's continuing development. Through the theories they embody, paradigms prove to be constitutive of the research activity. They are also, however, constitutive of science in other respects, and that is now the point. In particular, our most recent examples show that paradigms provide scientists not only with a map but also with some of the directions essential for map-making. In learning a paradigm the scientist acquires theory, methods, and standards together, usually in an inextricable mixture. Therefore, when paradigms change, there are usually significant shifts in the criteria determining the legitimacy both of problems and of proposed solutions.

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

Notes

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.
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.
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.
6. R. Dugas, *La mécanique au XVII^e siècle* (Neuchâtel, 1954), pp. 177–85, 284–98, 345–56.
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.
9. E. Meyerson, *Identity and Reality* (New York, 1930), chap. x.
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).