

I. Introduction: A Role for History

History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed. That image has previously been drawn, even by scientists themselves, mainly from the study of finished scientific achievements as these are recorded in the classics and, more recently, in the textbooks from which each new scientific generation learns to practice its trade. Inevitably, however, the aim of such books is persuasive and pedagogic; a concept of science drawn from them is no more likely to fit the enterprise that produced them than an image of a national culture drawn from a tourist brochure or a language text. This essay attempts to show that we have been misled by them in fundamental ways. Its aim is a sketch of the quite different concept of science that can emerge from the historical record of the research activity itself.

Even from history, however, that new concept will not be forthcoming if historical data continue to be sought and scrutinized mainly to answer questions posed by the unhistorical stereotype drawn from science texts. Those texts have, for example, often seemed to imply that the content of science is uniquely exemplified by the observations, laws, and theories described in their pages. Almost as regularly, the same books have been read as saying that scientific methods are simply the ones illustrated by the manipulative techniques used in gathering textbook data, together with the logical operations employed when relating those data to the textbook's theoretical generalizations. The result has been a concept of science with profound implications about its nature and development.

If science is the constellation of facts, theories, and methods collected in current texts, then scientists are the men who, successfully or not, have striven to contribute one or another element to that particular constellation. Scientific development becomes the piecemeal process by which these items have been

The Structure of Scientific Revolutions

added, singly and in combination, to the ever growing stockpile that constitutes scientific technique and knowledge. And history of science becomes the discipline that chronicles both these successive increments and the obstacles that have inhibited their accumulation. Concerned with scientific development, the historian then appears to have two main tasks. On the one hand, he must determine by what man and at what point in time each contemporary scientific fact, law, and theory was discovered or invented. On the other, he must describe and explain the congeries of error, myth, and superstition that have inhibited the more rapid accumulation of the constituents of the modern science text. Much research has been directed to these ends, and some still is.

In recent years, however, a few historians of science have been finding it more and more difficult to fulfil the functions that the concept of development-by-accumulation assigns to them. As chroniclers of an incremental process, they discover that additional research makes it harder, not easier, to answer questions like: When was oxygen discovered? Who first conceived of energy conservation? Increasingly, a few of them suspect that these are simply the wrong sorts of questions to ask. Perhaps science does not develop by the accumulation of individual discoveries and inventions. Simultaneously, these same historians confront growing difficulties in distinguishing the "scientific" component of past observation and belief from what their predecessors had readily labeled "error" and "superstition." The more carefully they study, say, Aristotelian dynamics, phlogistic chemistry, or caloric thermodynamics, the more certain they feel that those once current views of nature were, as a whole, neither less scientific nor more the product of human idiosyncrasy than those current today. If these out-of-date beliefs are to be called myths, then myths can be produced by the same sorts of methods and held for the same sorts of reasons that now lead to scientific knowledge. If, on the other hand, they are to be called science, then science has included bodies of belief quite incompatible with the ones we hold today. Given these alternatives, the historian must choose the latter. Out-of-

Introduction: A Role for History

date theories are not in principle unscientific because they have been discarded. That choice, however, makes it difficult to see scientific development as a process of accretion. The same historical research that displays the difficulties in isolating individual inventions and discoveries gives ground for profound doubts about the cumulative process through which these individual contributions to science were thought to have been compounded.

The result of all these doubts and difficulties is a historiographic revolution in the study of science, though one that is still in its early stages. Gradually, and often without entirely realizing they are doing so, historians of science have begun to ask new sorts of questions and to trace different, and often less than cumulative, developmental lines for the sciences. Rather than seeking the permanent contributions of an older science to our present vantage, they attempt to display the historical integrity of that science in its own time. They ask, for example, not about the relation of Galileo's views to those of modern science, but rather about the relationship between his views and those of his group, i.e., his teachers, contemporaries, and immediate successors in the sciences. Furthermore, they insist upon studying the opinions of that group and other similar ones from the viewpoint—usually very different from that of modern science—that gives those opinions the maximum internal coherence and the closest possible fit to nature. Seen through the works that result, works perhaps best exemplified in the writings of Alexandre Koyré, science does not seem altogether the same enterprise as the one discussed by writers in the older historiographic tradition. By implication, at least, these historical studies suggest the possibility of a new image of science. This essay aims to delineate that image by making explicit some of the new historiography's implications.

What aspects of science will emerge to prominence in the course of this effort? First, at least in order of presentation, is the insufficiency of methodological directives, by themselves, to dictate a unique substantive conclusion to many sorts of scientific questions. Instructed to examine electrical or chemical phe-

The Structure of Scientific Revolutions

nomena, the man who is ignorant of these fields but who knows what it is to be scientific may legitimately reach any one of a number of incompatible conclusions. Among those legitimate possibilities, the particular conclusions he does arrive at are probably determined by his prior experience in other fields, by the accidents of his investigation, and by his own individual makeup. What beliefs about the stars, for example, does he bring to the study of chemistry or electricity? Which of the many conceivable experiments relevant to the new field does he elect to perform first? And what aspects of the complex phenomenon that then results strike him as particularly relevant to an elucidation of the nature of chemical change or of electrical affinity? For the individual, at least, and sometimes for the scientific community as well, answers to questions like these are often essential determinants of scientific development. We shall note, for example, in Section II that the early developmental stages of most sciences have been characterized by continual competition between a number of distinct views of nature, each partially derived from, and all roughly compatible with, the dictates of scientific observation and method. What differentiated these various schools was not one or another failure of method—they were all “scientific”—but what we shall come to call their incommensurable ways of seeing the world and of practicing science in it. Observation and experience can and must drastically restrict the range of admissible scientific belief, else there would be no science. But they cannot alone determine a particular body of such belief. An apparently arbitrary element, compounded of personal and historical accident, is always a formative ingredient of the beliefs espoused by a given scientific community at a given time.

That element of arbitrariness does not, however, indicate that any scientific group could practice its trade without some set of received beliefs. Nor does it make less consequential the particular constellation to which the group, at a given time, is in fact committed. Effective research scarcely begins before a scientific community thinks it has acquired firm answers to questions like the following: What are the fundamental entities

Introduction: A Role for History

of which the universe is composed? How do these interact with each other and with the senses? What questions may legitimately be asked about such entities and what techniques employed in seeking solutions? At least in the mature sciences, answers (or full substitutes for answers) to questions like these are firmly embedded in the educational initiation that prepares and licenses the student for professional practice. Because that education is both rigorous and rigid, these answers come to exert a deep hold on the scientific mind. That they can do so does much to account both for the peculiar efficiency of the normal research activity and for the direction in which it proceeds at any given time. When examining normal science in Sections III, IV, and V, we shall want finally to describe that research as a strenuous and devoted attempt to force nature into the conceptual boxes supplied by professional education. Simultaneously, we shall wonder whether research could proceed without such boxes, whatever the element of arbitrariness in their historic origins and, occasionally, in their subsequent development.

Yet that element of arbitrariness is present, and it too has an important effect on scientific development, one which will be examined in detail in Sections VI, VII, and VIII. Normal science, the activity in which most scientists inevitably spend almost all their time, is predicated on the assumption that the scientific community knows what the world is like. Much of the success of the enterprise derives from the community's willingness to defend that assumption, if necessary at considerable cost. Normal science, for example, often suppresses fundamental novelties because they are necessarily subversive of its basic commitments. Nevertheless, so long as those commitments retain an element of the arbitrary, the very nature of normal research ensures that novelty shall not be suppressed for very long. Sometimes a normal problem, one that ought to be solvable by known rules and procedures, resists the reiterated onslaught of the ablest members of the group within whose competence it falls. On other occasions a piece of equipment designed and constructed for the purpose of normal research fails

The Structure of Scientific Revolutions

to perform in the anticipated manner, revealing an anomaly that cannot, despite repeated effort, be aligned with professional expectation. In these and other ways besides, normal science repeatedly goes astray. And when it does—when, that is, the profession can no longer evade anomalies that subvert the existing tradition of scientific practice—then begin the extraordinary investigations that lead the profession at last to a new set of commitments, a new basis for the practice of science. The extraordinary episodes in which that shift of professional commitments occurs are the ones known in this essay as scientific revolutions. They are the tradition-shattering complements to the tradition-bound activity of normal science.

The most obvious examples of scientific revolutions are those famous episodes in scientific development that have often been labeled revolutions before. Therefore, in Sections IX and X, where the nature of scientific revolutions is first directly scrutinized, we shall deal repeatedly with the major turning points in scientific development associated with the names of Copernicus, Newton, Lavoisier, and Einstein. More clearly than most other episodes in the history of at least the physical sciences, these display what all scientific revolutions are about. Each of them necessitated the community's rejection of one time-honored scientific theory in favor of another incompatible with it. Each produced a consequent shift in the problems available for scientific scrutiny and in the standards by which the profession determined what should count as an admissible problem or as a legitimate problem-solution. And each transformed the scientific imagination in ways that we shall ultimately need to describe as a transformation of the world within which scientific work was done. Such changes, together with the controversies that almost always accompany them, are the defining characteristics of scientific revolutions.

These characteristics emerge with particular clarity from a study of, say, the Newtonian or the chemical revolution. It is, however, a fundamental thesis of this essay that they can also be retrieved from the study of many other episodes that were not so obviously revolutionary. For the far smaller professional

Introduction: A Role for History

group affected by them, Maxwell's equations were as revolutionary as Einstein's, and they were resisted accordingly. The invention of other new theories regularly, and appropriately, evokes the same response from some of the specialists on whose area of special competence they impinge. For these men the new theory implies a change in the rules governing the prior practice of normal science. Inevitably, therefore, it reflects upon much scientific work they have already successfully completed. That is why a new theory, however special its range of application, is seldom or never just an increment to what is already known. Its assimilation requires the reconstruction of prior theory and the re-evaluation of prior fact, an intrinsically revolutionary process that is seldom completed by a single man and never overnight. No wonder historians have had difficulty in dating precisely this extended process that their vocabulary impels them to view as an isolated event.

Nor are new inventions of theory the only scientific events that have revolutionary impact upon the specialists in whose domain they occur. The commitments that govern normal science specify not only what sorts of entities the universe does contain, but also, by implication, those that it does not. It follows, though the point will require extended discussion, that a discovery like that of oxygen or X-rays does not simply add one more item to the population of the scientist's world. Ultimately it has that effect, but not until the professional community has re-evaluated traditional experimental procedures, altered its conception of entities with which it has long been familiar, and, in the process, shifted the network of theory through which it deals with the world. Scientific fact and theory are not categorically separable, except perhaps within a single tradition of normal-scientific practice. That is why the unexpected discovery is not simply factual in its import and why the scientist's world is qualitatively transformed as well as quantitatively enriched by fundamental novelties of either fact or theory.

This extended conception of the nature of scientific revolutions is the one delineated in the pages that follow. Admittedly the extension strains customary usage. Nevertheless, I shall con-

The Structure of Scientific Revolutions

tinue to speak even of discoveries as revolutionary, because it is just the possibility of relating their structure to that of, say, the Copernican revolution that makes the extended conception seem to me so important. The preceding discussion indicates how the complementary notions of normal science and of scientific revolutions will be developed in the nine sections immediately to follow. The rest of the essay attempts to dispose of three remaining central questions. Section XI, by discussing the textbook tradition, considers why scientific revolutions have previously been so difficult to see. Section XII describes the revolutionary competition between the proponents of the old normal-scientific tradition and the adherents of the new one. It thus considers the process that should somehow, in a theory of scientific inquiry, replace the confirmation or falsification procedures made familiar by our usual image of science. Competition between segments of the scientific community is the only historical process that ever actually results in the rejection of one previously accepted theory or in the adoption of another. Finally, Section XIII will ask how development through revolutions can be compatible with the apparently unique character of scientific progress. For that question, however, this essay will provide no more than the main outlines of an answer, one which depends upon characteristics of the scientific community that require much additional exploration and study.

Undoubtedly, some readers will already have wondered whether historical study can possibly effect the sort of conceptual transformation aimed at here. An entire arsenal of dichotomies is available to suggest that it cannot properly do so. History, we too often say, is a purely descriptive discipline. The theses suggested above are, however, often interpretive and sometimes normative. Again, many of my generalizations are about the sociology or social psychology of scientists; yet at least a few of my conclusions belong traditionally to logic or epistemology. In the preceding paragraph I may even seem to have violated the very influential contemporary distinction between "the context of discovery" and "the context of justifica-



Introduction: A Role for History

tion." Can anything more than profound confusion be indicated by this admixture of diverse fields and concerns?

Having been weaned intellectually on these distinctions and others like them, I could scarcely be more aware of their import and force. For many years I took them to be about the nature of knowledge, and I still suppose that, appropriately recast, they have something important to tell us. Yet my attempts to apply them, even *grosso modo*, to the actual situations in which knowledge is gained, accepted, and assimilated have made them seem extraordinarily problematic. Rather than being elementary logical or methodological distinctions, which would thus be prior to the analysis of scientific knowledge, they now seem integral parts of a traditional set of substantive answers to the very questions upon which they have been deployed. That circularity does not at all invalidate them. But it does make them parts of a theory and, by doing so, subjects them to the same scrutiny regularly applied to theories in other fields. If they are to have more than pure abstraction as their content, then that content must be discovered by observing them in application to the data they are meant to elucidate. How could history of science fail to be a source of phenomena to which theories about knowledge may legitimately be asked to apply?

IV. Normal Science as Puzzle-solving

Perhaps the most striking feature of the normal research problems we have just encountered is how little they aim to produce major novelties, conceptual or phenomenal. Sometimes, as in a wave-length measurement, everything but the most esoteric detail of the result is known in advance, and the typical latitude of expectation is only somewhat wider. Coulomb's measurements need not, perhaps, have fitted an inverse square law; the men who worked on heating by compression were often prepared for any one of several results. Yet even in cases like these the range of anticipated, and thus of assimilable, results is always small compared with the range that imagination can conceive. And the project whose outcome does not fall in that narrower range is usually just a research failure, one which reflects not on nature but on the scientist.

In the eighteenth century, for example, little attention was paid to the experiments that measured electrical attraction with devices like the pan balance. Because they yielded neither consistent nor simple results, they could not be used to articulate the paradigm from which they derived. Therefore, they remained *mere* facts, unrelated and unrelatable to the continuing progress of electrical research. Only in retrospect, possessed of a subsequent paradigm, can we see what characteristics of electrical phenomena they display. Coulomb and his contemporaries, of course, also possessed this later paradigm or one that, when applied to the problem of attraction, yielded the same expectations. That is why Coulomb was able to design apparatus that gave a result assimilable by paradigm articulation. But it is also why that result surprised no one and why several of Coulomb's contemporaries had been able to predict it in advance. Even the project whose goal is paradigm articulation does not aim at the *unexpected* novelty.

But if the aim of normal science is not major substantive novelties—if failure to come near the anticipated result is usually

The Structure of Scientific Revolutions

failure as a scientist—then why are these problems undertaken at all? Part of the answer has already been developed. To scientists, at least, the results gained in normal research are significant because they add to the scope and precision with which the paradigm can be applied. That answer, however, cannot account for the enthusiasm and devotion that scientists display for the problems of normal research. No one devotes years to, say, the development of a better spectrometer or the production of an improved solution to the problem of vibrating strings simply because of the importance of the information that will be obtained. The data to be gained by computing ephemerides or by further measurements with an existing instrument are often just as significant, but those activities are regularly spurned by scientists because they are so largely repetitions of procedures that have been carried through before. That rejection provides a clue to the fascination of the normal research problem. Though its outcome can be anticipated, often in detail so great that what remains to be known is itself uninteresting, the way to achieve that outcome remains very much in doubt. Bringing a normal research problem to a conclusion is achieving the anticipated in a new way, and it requires the solution of all sorts of complex instrumental, conceptual, and mathematical puzzles. The man who succeeds proves himself an expert puzzle-solver, and the challenge of the puzzle is an important part of what usually drives him on.

The terms 'puzzle' and 'puzzle-solver' highlight several of the themes that have become increasingly prominent in the preceding pages. Puzzles are, in the entirely standard meaning here employed, that special category of problems that can serve to test ingenuity or skill in solution. Dictionary illustrations are 'jigsaw puzzle' and 'crossword puzzle,' and it is the characteristics that these share with the problems of normal science that we now need to isolate. One of them has just been mentioned. It is no criterion of goodness in a puzzle that its outcome be intrinsically interesting or important. On the contrary, the really pressing problems, e.g., a cure for cancer or the design of a



Normal Science as Puzzle-solving

lasting peace, are often not puzzles at all, largely because they may not have any solution. Consider the jigsaw puzzle whose pieces are selected at random from each of two different puzzle boxes. Since that problem is likely to defy (though it might not) even the most ingenious of men, it cannot serve as a test of skill in solution. In any usual sense it is not a puzzle at all. Though intrinsic value is no criterion for a puzzle, the assured existence of a solution is.

We have already seen, however, that one of the things a scientific community acquires with a paradigm is a criterion for choosing problems that, while the paradigm is taken for granted, can be assumed to have solutions. To a great extent these are the only problems that the community will admit as scientific or encourage its members to undertake. Other problems, including many that had previously been standard, are rejected as metaphysical, as the concern of another discipline, or sometimes as just too problematic to be worth the time. A paradigm can, for that matter, even insulate the community from those socially important problems that are not reducible to the puzzle form, because they cannot be stated in terms of the conceptual and instrumental tools the paradigm supplies. Such problems can be a distraction, a lesson brilliantly illustrated by several facets of seventeenth-century Baconianism and by some of the contemporary social sciences. One of the reasons why normal science seems to progress so rapidly is that its practitioners concentrate on problems that only their own lack of ingenuity should keep them from solving.

If, however, the problems of normal science are puzzles in this sense, we need no longer ask why scientists attack them with such passion and devotion. A man may be attracted to science for all sorts of reasons. Among them are the desire to be useful, the excitement of exploring new territory, the hope of finding order, and the drive to test established knowledge. These motives and others besides also help to determine the particular problems that will later engage him. Furthermore, though the result is occasional frustration, there is good reason

The Structure of Scientific Revolutions

why motives like these should first attract him and then lead him on.¹ The scientific enterprise as a whole does from time to time prove useful, open up new territory, display order, and test long-accepted belief. Nevertheless, *the individual* engaged on a normal research problem *is almost never doing any one of these things*. Once engaged, his motivation is of a rather different sort. What then challenges him is the conviction that, if only he is skilful enough, he will succeed in solving a puzzle that no one before has solved or solved so well. Many of the greatest scientific minds have devoted all of their professional attention to demanding puzzles of this sort. On most occasions any particular field of specialization offers nothing else to do, a fact that makes it no less fascinating to the proper sort of addict.

Turn now to another, more difficult, and more revealing aspect of the parallelism between puzzles and the problems of normal science. If it is to classify as a puzzle, a problem must be characterized by more than an assured solution. There must also be rules that limit both the nature of acceptable solutions and the steps by which they are to be obtained. To solve a jigsaw puzzle is not, for example, merely "to make a picture." Either a child or a contemporary artist could do that by scattering selected pieces, as abstract shapes, upon some neutral ground. The picture thus produced might be far better, and would certainly be more original, than the one from which the puzzle had been made. Nevertheless, such a picture would not be a solution. To achieve that all the pieces must be used, their plain sides must be turned down, and they must be interlocked without forcing until no holes remain. Those are among the rules that govern jigsaw-puzzle solutions. Similar restrictions upon the admissible solutions of crossword puzzles, riddles, chess problems, and so on, are readily discovered.

If we can accept a considerably broadened use of the term

¹ The frustrations induced by the conflict between the individual's role and the over-all pattern of scientific development can, however, occasionally be quite serious. On this subject, see Lawrence S. Kubie, "Some Unsolved Problems of the Scientific Career," *American Scientist*, XLI (1953), 596-613; and XLII (1954), 104-12.

Normal Science as Puzzle-solving

'rule'—one that will occasionally equate it with 'established viewpoint' or with 'preconception'—then the problems accessible within a given research tradition display something much like this set of puzzle characteristics. The man who builds an instrument to determine optical wave lengths must not be satisfied with a piece of equipment that merely attributes particular numbers to particular spectral lines. He is not just an explorer or measurer. On the contrary, he must show, by analyzing his apparatus in terms of the established body of optical theory, that the numbers his instrument produces are the ones that enter theory as wave lengths. If some residual vagueness in the theory or some unanalyzed component of his apparatus prevents his completing that demonstration, his colleagues may well conclude that he has measured nothing at all. For example, the electron-scattering maxima that were later diagnosed as indices of electron wave length had no apparent significance when first observed and recorded. Before they became measures of anything, they had to be related to a theory that predicted the wave-like behavior of matter in motion. And even after that relation was pointed out, the apparatus had to be redesigned so that the experimental results might be correlated unequivocally with theory.² Until those conditions had been satisfied, no problem had been solved.

Similar sorts of restrictions bound the admissible solutions to theoretical problems. Throughout the eighteenth century those scientists who tried to derive the observed motion of the moon from Newton's laws of motion and gravitation consistently failed to do so. As a result, some of them suggested replacing the inverse square law with a law that deviated from it at small distances. To do that, however, would have been to change the paradigm, to define a new puzzle, and not to solve the old one. In the event, scientists preserved the rules until, in 1750, one of them discovered how they could successfully be applied.³

² For a brief account of the evolution of these experiments, see page 4 of C. J. Davisson's lecture in *Les prix Nobel en 1937* (Stockholm, 1938).

³ W. Whewell, *History of the Inductive Sciences* (rev. ed.; London, 1847), II, 101–5, 220–22.

The Structure of Scientific Revolutions

Only a change in the rules of the game could have provided an alternative.

The study of normal-scientific traditions discloses many additional rules, and these provide much information about the commitments that scientists derive from their paradigms. What can we say are the main categories into which these rules fall?⁴ The most obvious and probably the most binding is exemplified by the sorts of generalizations we have just noted. These are explicit statements of scientific law and about scientific concepts and theories. While they continue to be honored, such statements help to set puzzles and to limit acceptable solutions. Newton's Laws, for example, performed those functions during the eighteenth and nineteenth centuries. As long as they did so, quantity-of-matter was a fundamental ontological category for physical scientists, and the forces that act between bits of matter were a dominant topic for research.⁵ In chemistry the laws of fixed and definite proportions had, for a long time, an exactly similar force—setting the problem of atomic weights, bounding the admissible results of chemical analyses, and informing chemists what atoms and molecules, compounds and mixtures were.⁶ Maxwell's equations and the laws of statistical thermodynamics have the same hold and function today.

Rules like these are, however, neither the only nor even the most interesting variety displayed by historical study. At a level lower or more concrete than that of laws and theories, there is, for example, a multitude of commitments to preferred types of instrumentation and to the ways in which accepted instruments may legitimately be employed. Changing attitudes toward the role of fire in chemical analyses played a vital part in the de-

⁴ I owe this question to W. O. Hagstrom, whose work in the sociology of science sometimes overlaps my own.

⁵ For these aspects of Newtonianism, see I. B. Cohen, *Franklin and Newton: An Inquiry into Speculative Newtonian Experimental Science and Franklin's Work in Electricity as an Example Thereof* (Philadelphia, 1958), chap. vii, esp. pp. 255-57, 275-77.

⁶ This example is discussed at length near the end of Section X.

Normal Science as Puzzle-solving

velopment of chemistry in the seventeenth century.⁷ Helmholtz, in the nineteenth, encountered strong resistance from physiologists to the notion that physical experimentation could illuminate their field.⁸ And in this century the curious history of chemical chromatography again illustrates the endurance of instrumental commitments that, as much as laws and theory, provide scientists with rules of the game.⁹ When we analyze the discovery of X-rays, we shall find reasons for commitments of this sort.

Less local and temporary, though still not unchanging characteristics of science, are the higher level, quasi-metaphysical commitments that historical study so regularly displays. After about 1630, for example, and particularly after the appearance of Descartes's immensely influential scientific writings, most physical scientists assumed that the universe was composed of microscopic corpuscles and that all natural phenomena could be explained in terms of corpuscular shape, size, motion, and interaction. That nest of commitments proved to be both metaphysical and methodological. As metaphysical, it told scientists what sorts of entities the universe did and did not contain: there was only shaped matter in motion. As methodological, it told them what ultimate laws and fundamental explanations must be like: laws must specify corpuscular motion and interaction, and explanation must reduce any given natural phenomenon to corpuscular action under these laws. More important still, the corpuscular conception of the universe told scientists what many of their research problems should be. For example, a chemist who, like Boyle, embraced the new philosophy gave particular attention to reactions that could be viewed as transmutations. More clearly than any others these displayed the process of corpuscular rearrangement that must underlie all

⁷ H. Metzger, *Les doctrines chimiques en France du début du XVII^e siècle à la fin du XVIII^e siècle* (Paris, 1923), pp. 359-61; Marie Boas, *Robert Boyle and Seventeenth-Century Chemistry* (Cambridge, 1958), pp. 112-15.

⁸ Leo Königsberger, *Hermann von Helmholtz*, trans. Francis A. Welby (Oxford, 1908), pp. 65-66.

⁹ James E. Meinhard, "Chromatography: A Perspective," *Science*, CX (1949), 387-92.

The Structure of Scientific Revolutions

chemical change.¹⁰ Similar effects of corpuscularism can be observed in the study of mechanics, optics, and heat.

Finally, at a still higher level, there is another set of commitments without which no man is a scientist. The scientist must, for example, be concerned to understand the world and to extend the precision and scope with which it has been ordered. That commitment must, in turn, lead him to scrutinize, either for himself or through colleagues, some aspect of nature in great empirical detail. And, if that scrutiny displays pockets of apparent disorder, then these must challenge him to a new refinement of his observational techniques or to a further articulation of his theories. Undoubtedly there are still other rules like these, ones which have held for scientists at all times.

The existence of this strong network of commitments—conceptual, theoretical, instrumental, and methodological—is a principal source of the metaphor that relates normal science to puzzle-solving. Because it provides rules that tell the practitioner of a mature specialty what both the world and his science are like, he can concentrate with assurance upon the esoteric problems that these rules and existing knowledge define for him. What then personally challenges him is how to bring the residual puzzle to a solution. In these and other respects a discussion of puzzles and of rules illuminates the nature of normal scientific practice. Yet, in another way, that illumination may be significantly misleading. Though there obviously are rules to which all the practitioners of a scientific specialty adhere at a given time, those rules may not by themselves specify all that the practice of those specialists has in common. Normal science is a highly determined activity, but it need not be entirely determined by rules. That is why, at the start of this essay, I introduced shared paradigms rather than shared rules, assumptions, and points of view as the source of coherence for normal research traditions. Rules, I suggest, derive from paradigms, but paradigms can guide research even in the absence of rules.

¹⁰ For corpuscularism in general, see Marie Boas, "The Establishment of the Mechanical Philosophy," *Osiris*, X (1952), 412–541. For its effects on Boyle's chemistry, see T. S. Kuhn, "Robert Boyle and Structural Chemistry in the Seventeenth Century," *Isis*, XLIII (1952), 12–36.



V. The Priority of Paradigms

To discover the relation between rules, paradigms, and normal science, consider first how the historian isolates the particular loci of commitment that have just been described as accepted rules. Close historical investigation of a given specialty at a given time discloses a set of recurrent and quasi-standard illustrations of various theories in their conceptual, observational, and instrumental applications. These are the community's paradigms, revealed in its textbooks, lectures, and laboratory exercises. By studying them and by practicing with them, the members of the corresponding community learn their trade. The historian, of course, will discover in addition a penumbral area occupied by achievements whose status is still in doubt, but the core of solved problems and techniques will usually be clear. Despite occasional ambiguities, the paradigms of a mature scientific community can be determined with relative ease.

The determination of shared paradigms is not, however, the determination of shared rules. That demands a second step and one of a somewhat different kind. When undertaking it, the historian must compare the community's paradigms with each other and with its current research reports. In doing so, his object is to discover what isolable elements, explicit or implicit, the members of that community may have *abstracted* from their more global paradigms and deployed as rules in their research. Anyone who has attempted to describe or analyze the evolution of a particular scientific tradition will necessarily have sought accepted principles and rules of this sort. Almost certainly, as the preceding section indicates, he will have met with at least partial success. But, if his experience has been at all like my own, he will have found the search for rules both more difficult and less satisfying than the search for paradigms. Some of the generalizations he employs to describe the community's shared beliefs will present no problems. Others, however, in-