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 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 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 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 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.

'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.

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, 1956), chap. vii, esp. pp. 255–57, 275–77.

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

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, 1906), pp. 65–66.

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

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, including some of those used as illustrations above, will seem a shade too strong. Phrased in just that way, or in any other way he can imagine, they would almost certainly have been rejected by some members of the group he studies. Nevertheless, if the coherence of the research tradition is to be understood in terms of rules, some specification of common ground in the corresponding area is needed. As a result, the search for a body of rules competent to constitute a given normal research tradition becomes a source of continual and deep frustration.

Recognizing that frustration, however, makes it possible to diagnose its source. Scientists can agree that a Newton, Lavoisier, Maxwell, or Einstein has produced an apparently permanent solution to a group of outstanding problems and still disagree, sometimes without being aware of it, about the particular abstract characteristics that make those solutions permanent. They can, that is, agree in their *identification* of a paradigm without agreeing on, or even attempting to produce, a full *interpretation* or *rationalization* of it. Lack of a standard interpretation or of an agreed reduction to rules will not prevent a paradigm from guiding research. Normal science can be determined in part by the direct inspection of paradigms, a process that is often aided by but does not depend upon the formulation of rules and assumptions. Indeed, the existence of a paradigm need not even imply that any full set of rules exists.[1]

Inevitably, the first effect of those statements is to raise problems. In the absence of a competent body of rules, what restricts the scientist to a particular normal-scientific tradition? What can the phrase 'direct inspection of paradigms' mean? Partial answers to questions like these were developed by the the late Ludwig Wittgenstein, though in a very different context. Because that context is both more elementary and more familiar, it will help to consider his form of the argument first. What need we know, Wittgenstein asked, in order that we

[1] Michael Polanyi has brilliantly developed a very similar theme, arguing that much of the scientist's success depends upon "tacit knowledge," i.e., upon knowledge that is acquired through practice and that cannot be articulated explicitly. See his *Personal Knowledge* (Chicago, 1958), particularly chaps. v and vi.

apply terms like 'chair,' or 'leaf,' or 'game' unequivocally and without provoking argument?[2]

That question is very old and has generally been answered by saying that we must know, consciously or intuitively, what a chair, or leaf, or game *is*. We must, that is, grasp some set of attributes that all games and that only games have in common. Wittgenstein, however, concluded that, given the way we use language and the sort of world to which we apply it, there need be no such set of characteristics. Though a discussion of *some* of the attributes shared by a *number* of games or chairs or leaves often helps us learn how to employ the corresponding term, there is no set of characteristics that is simultaneously applicable to all members of the class and to them alone. Instead, confronted with a previously unobserved activity, we apply the term 'game' because what we are seeing bears a close "family resemblance" to a number of the activities that we have previously learned to call by that name. For Wittgenstein, in short, games, and chairs, and leaves are natural families, each constituted by a network of overlapping and crisscross resemblances. The existence of such a network sufficiently accounts for our success in identifying the corresponding object or activity. Only if the families we named overlapped and merged gradually into one another—only, that is, if there were no *natural* families—would our success in identifying and naming provide evidence for a set of common characteristics corresponding to each of the class names we employ.

Something of the same sort may very well hold for the various research problems and techniques that arise within a single normal-scientific tradition. What these have in common is not that they satisfy some explicit or even some fully discoverable set of rules and assumptions that gives the tradition its character and its hold upon the scientific mind. Instead, they may relate by resemblance and by modeling to one or another part of the scientific corpus which the community in question al-

[2] Ludwig Wittgenstein, *Philosophical Investigations*, trans. G. E. M. Anscombe (New York, 1953), pp. 31–36. Wittgenstein, however, says almost nothing about the sort of world necessary to support the naming procedure he outlines. Part of the point that follows cannot therefore be attributed to him.

ready recognizes as among its established achievements. Scientists work from models acquired through education and through subsequent exposure to the literature often without quite knowing or needing to know what characteristics have given these models the status of community paradigms. And because they do so, they need no full set of rules. The coherence displayed by the research tradition in which they participate may not imply even the existence of an underlying body of rules and assumptions that additional historical or philosophical investigation might uncover. That scientists do not usually ask or debate what makes a particular problem or solution legitimate tempts us to suppose that, at least intuitively, they know the answer. But it may only indicate that neither the question nor the answer is felt to be relevant to their research. Paradigms may be prior to, more binding, and more complete than any set of rules for research that could be unequivocally abstracted from them.

So far this point has been entirely theoretical: paradigms *could* determine normal science without the intervention of discoverable rules. Let me now try to increase both its clarity and urgency by indicating some of the reasons for believing that paradigms actually do operate in this manner. The first, which has already been discussed quite fully, is the severe difficulty of discovering the rules that have guided particular normal-scientific traditions. That difficulty is very nearly the same as the one the philosopher encounters when he tries to say what all games have in common. The second, to which the first is really a corollary, is rooted in the nature of scientific education. Scientists, it should already be clear, never learn concepts, laws, and theories in the abstract and by themselves. Instead, these intellectual tools are from the start encountered in a historically and pedagogically prior unit that displays them with and through their applications. A new theory is always announced together with applications to some concrete range of natural phenomena; without them it would not be even a candidate for acceptance. After it has been accepted, those same applications or others accompany the theory into the textbooks from which the future practitioner will learn his trade. They are not there merely as embroidery or even as documentation. On the contrary, the process of learning a theory depends upon the study of applications, including practice problem-solving both with a pencil and paper and with instruments in the laboratory. If, for example, the student of Newtonian dynamics ever discovers the meaning of terms like 'force,' 'mass,' 'space,' and 'time,' he does so less from the incomplete though sometimes helpful definitions in his text than by observing and participating in the application of these concepts to problem-solution.

That process of learning by finger exercise or by doing continues throughout the process of professional initiation. As the student proceeds from his freshman course to and through his doctoral dissertation, the problems assigned to him become more complex and less completely precedented. But they continue to be closely modeled on previous achievements as are the problems that normally occupy him during his subsequent independent scientific career. One is at liberty to suppose that somewhere along the way the scientist has intuitively abstracted rules of the game for himself, but there is little reason to believe it. Though many scientists talk easily and well about the particular individual hypotheses that underlie a concrete piece of current research, they are little better than laymen at characterizing the established bases of their field, its legitimate problems and methods. If they have learned such abstractions at all, they show it mainly through their ability to do successful research. That ability can, however, be understood without recourse to hypothetical rules of the game.

These consequences of scientific education have a converse that provides a third reason to suppose that paradigms guide research by direct modeling as well as through abstracted rules. Normal science can proceed without rules only so long as the relevant scientific community accepts without question the particular problem-solutions already achieved. Rules should therefore become important and the characteristic unconcern about them should vanish whenever paradigms or models are felt to be insecure. That is, moreover, exactly what does occur. The pre-paradigm period, in particular, is regularly marked by frequent and deep debates over legitimate methods, problems, and standards of solution, though these serve rather to define schools than to produce agreement. We have already noted a few of these debates in optics and electricity, and they played an even larger role in the development of seventeenth-century chemistry and of early nineteenth-century geology.[3] Furthermore, debates like these do not vanish once and for all with the appearance of a paradigm. Though almost non-existent during periods of normal science, they recur regularly just before and during scientific revolutions, the periods when paradigms are first under attack and then subject to change. The transition from Newtonian to quantum mechanics evoked many debates about both the nature and the standards of physics, some of which still continue.[4] There are people alive today who can remember the similar arguments engendered by Maxwell's electromagnetic theory and by statistical mechanics.[5] And earlier still, the assimilation of Galileo's and Newton's mechanics gave rise to a particularly famous series of debates with Aristotelians, Cartesians, and Leibnizians about the standards legitimate to science.[6] When scientists disagree about whether the fundamental problems of their field have been solved, the search for rules gains a function that it does not ordinarily possess. While

[3] For chemistry, see H. Metzger, *Les doctrines chimiques en France du début du XVII^e à la fin du XVIII^e siècle* (Paris, 1923), pp. 24–27, 146–49; and Marie Boas, *Robert Boyle and Seventeenth-Century Chemistry* (Cambridge, 1958), chap. ii. For geology, see Walter F. Cannon, "The Uniformitarian-Catastrophist Debate," *Isis*, LI (1960), 38–55; and C. C. Gillispie, *Genesis and Geology* (Cambridge, Mass., 1951), chaps. iv–v.

[4] For controversies over quantum mechanics, see Jean Ullmo, *La crise de la physique quantique* (Paris, 1950), chap. ii.

[5] For statistical mechanics, see René Dugas, *La théorie physique au sens de Boltzmann et ses prolongements modernes* (Neuchatel, 1959), pp. 158–84, 206–19. For the reception of Maxwell's work, see Max Planck, "Maxwell's Influence in Germany," in *James Clerk Maxwell: A Commemoration Volume, 1831–1931* (Cambridge, 1931), pp. 45–65, esp. pp. 58–63; and Silvanus P. Thompson, *The Life of William Thomson Baron Kelvin of Largs* (London, 1910), II, 1021–27.

[6] For a sample of the battle with the Aristotelians, see A. Koyré, "A Documentary History of the Problem of Fall from Kepler to Newton," *Transactions of the American Philosophical Society*, XLV (1955), 329–95. For the debates with the Cartesians and Leibnizians, see Pierre Brunet, *L'introduction des théories de Newton en France au XVIII^e siècle* (Paris, 1931); and A. Koyré, *From the Closed World to the Infinite Universe* (Baltimore, 1957), chap. xi.

paradigms remain secure, however, they can function without agreement over rationalization or without any attempted rationalization at all.

A fourth reason for granting paradigms a status prior to that of shared rules and assumptions can conclude this section. The introduction to this essay suggested that there can be small revolutions as well as large ones, that some revolutions affect only the members of a professional subspecialty, and that for such groups even the discovery of a new and unexpected phenomenon may be revolutionary. The next section will introduce selected revolutions of that sort, and it is still far from clear how they can exist. If normal science is so rigid and if scientific communities are so close-knit as the preceding discussion has implied, how can a change of paradigm ever affect only a small subgroup? What has been said so far may have seemed to imply that normal science is a single monolithic and unified enterprise that must stand or fall with any one of its paradigms as well as with all of them together. But science is obviously seldom or never like that. Often, viewing all fields together, it seems instead a rather ramshackle structure with little coherence among its various parts. Nothing said to this point should, however, conflict with that very familiar observation. On the contrary, substituting paradigms for rules should make the diversity of scientific fields and specialties easier to understand. Explicit rules, when they exist, are usually common to a very broad scientific group, but paradigms need not be. The practitioners of widely separated fields, say astronomy and taxonomic botany, are educated by exposure to quite different achievements described in very different books. And even men who, being in the same or in closely related fields, begin by studying many of the same books and achievements may acquire rather different paradigms in the course of professional specialization.

Consider, for a single example, the quite large and diverse community constituted by all physical scientists. Each member of that group today is taught the laws of, say, quantum mechanics, and most of them employ these laws at some point in their research or teaching. But they do not all learn the same applications of these laws, and they are not therefore all affected in the same ways by changes in quantum-mechanical practice. On the road to professional specialization, a few physical scientists encounter only the basic principles of quantum mechanics. Others study in detail the paradigm applications of these principles to chemistry, still others to the physics of the solid state, and so on. What quantum mechanics means to each of them depends upon what courses he has had, what texts he has read, and which journals he studies. It follows that, though a change in quantum-mechanical law will be revolutionary for all of these groups, a change that reflects only on one or another of the paradigm applications of quantum mechanics need be revolutionary only for the members of a particular professional subspecialty. For the rest of the profession and for those who practice other physical sciences, that change need not be revolutionary at all. In short, though quantum mechanics (or Newtonian dynamics, or electromagnetic theory) is a paradigm for many scientific groups, it is not the same paradigm for them all. Therefore, it can simultaneously determine several traditions of normal science that overlap without being coextensive. A revolution produced within one of these traditions will not necessarily extend to the others as well.

One brief illustration of specialization's effect may give this whole series of points additional force. An investigator who hoped to learn something about what scientists took the atomic theory to be asked a distinguished physicist and an eminent chemist whether a single atom of helium was or was not a molecule. Both answered without hesitation, but their answers were not the same. For the chemist the atom of helium was a molecule because it behaved like one with respect to the kinetic theory of gases. For the physicist, on the other hand, the helium atom was not a molecule because it displayed no molecular spectrum.[7] Presumably both men were talking of the same par-

[7] The investigator was James K. Senior, to whom I am indebted for a verbal report. Some related issues are treated in his paper, "The Vernacular of the Laboratory," *Philosophy of Science*, XXV (1958), 163–68.

ticle, but they were viewing it through their own research training and practice. Their experience in problem-solving told them what a molecule must be. Undoubtedly their experiences had had much in common, but they did not, in this case, tell the two specialists the same thing. As we proceed we shall discover how consequential paradigm differences of this sort can occasionally be.