

and by students of motion in the Middle Ages, of physical optics in the late seventeenth century, and of historical geology in the early nineteenth. They had, that is, achieved a paradigm that proved able to guide the whole group's research. Except with the advantage of hindsight, it is hard to find another criterion that so clearly proclaims a field a science.

III. The Nature of Normal Science

What then is the nature of the more professional and esoteric research that a group's reception of a single paradigm permits? If the paradigm represents work that has been done once and for all, what further problems does it leave the united group to resolve? Those questions will seem even more urgent if we now note one respect in which the terms used so far may be misleading. In its established usage, a paradigm is an accepted model or pattern, and that aspect of its meaning has enabled me, lacking a better word, to appropriate 'paradigm' here. But it will shortly be clear that the sense of 'model' and 'pattern' that permits the appropriation is not quite the one usual in defining 'paradigm.' In grammar, for example, '*amo, amas, amat*' is a paradigm because it displays the pattern to be used in conjugating a large number of other Latin verbs, e.g., in producing '*laudo, laudas, laudat.*' In this standard application, the paradigm functions by permitting the replication of examples any one of which could in principle serve to replace it. In a science, on the other hand, a paradigm is rarely an object for replication. Instead, like an accepted judicial decision in the common law, it is an object for further articulation and specification under new or more stringent conditions.

To see how this can be so, we must recognize how very limited in both scope and precision a paradigm can be at the time of its first appearance. Paradigms gain their status because they are more successful than their competitors in solving a few problems that the group of practitioners has come to recognize as acute. To be more successful is not, however, to be either completely successful with a single problem or notably successful with any large number. The success of a paradigm—whether Aristotle's analysis of motion, Ptolemy's computations of planetary position, Lavoisier's application of the balance, or Maxwell's mathematization of the electromagnetic field—is at the start largely a promise of success discoverable in selected and

still incomplete examples. Normal science consists in the actualization of that promise, an actualization achieved by extending the knowledge of those facts that the paradigm displays as particularly revealing, by increasing the extent of the match between those facts and the paradigm's predictions, and by further articulation of the paradigm itself.

Few people who are not actually practitioners of a mature science realize how much mop-up work of this sort a paradigm leaves to be done or quite how fascinating such work can prove in the execution. And these points need to be understood. Mopping-up operations are what engage most scientists throughout their careers. They constitute what I am here calling normal science. Closely examined, whether historically or in the contemporary laboratory, that enterprise seems an attempt to force nature into the preformed and relatively inflexible box that the paradigm supplies. No part of the aim of normal science is to call forth new sorts of phenomena; indeed those that will not fit the box are often not seen at all. Nor do scientists normally aim to invent new theories, and they are often intolerant of those invented by others.¹ Instead, normal-scientific research is directed to the articulation of those phenomena and theories that the paradigm already supplies.

Perhaps these are defects. The areas investigated by normal science are, of course, minuscule; the enterprise now under discussion has drastically restricted vision. But those restrictions, born from confidence in a paradigm, turn out to be essential to the development of science. By focusing attention upon a small range of relatively esoteric problems, the paradigm forces scientists to investigate some part of nature in a detail and depth that would otherwise be unimaginable. And normal science possesses a built-in mechanism that ensures the relaxation of the restrictions that bound research whenever the paradigm from which they derive ceases to function effectively. At that point scientists begin to behave differently, and the nature of their research problems changes. In the interim, however, during the

¹ Bernard Barber, "Resistance by Scientists to Scientific Discovery," *Science*, CXXXIV (1961), 598-602.

period when the paradigm is successful, the profession will have solved problems that its members could scarcely have imagined and would never have undertaken without commitment to the paradigm. And at least part of that achievement always proves to be permanent.

To display more clearly what is meant by normal or paradigm-based research, let me now attempt to classify and illustrate the problems of which normal science principally consists. For convenience I postpone theoretical activity and begin with fact-gathering, that is, with the experiments and observations described in the technical journals through which scientists inform their professional colleagues of the results of their continuing research. On what aspects of nature do scientists ordinarily report? What determines their choice? And, since most scientific observation consumes much time, equipment, and money, what motivates the scientist to pursue that choice to a conclusion?

There are, I think, only three normal foci for factual scientific investigation, and they are neither always nor permanently distinct. First is that class of facts that the paradigm has shown to be particularly revealing of the nature of things. By employing them in solving problems, the paradigm has made them worth determining both with more precision and in a larger variety of situations. At one time or another, these significant factual determinations have included: in astronomy—stellar position and magnitude, the periods of eclipsing binaries and of planets; in physics—the specific gravities and compressibilities of materials, wave lengths and spectral intensities, electrical conductivities and contact potentials; and in chemistry—composition and combining weights, boiling points and acidity of solutions, structural formulas and optical activities. Attempts to increase the accuracy and scope with which facts like these are known occupy a significant fraction of the literature of experimental and observational science. Again and again complex special apparatus has been designed for such purposes, and the invention, construction, and deployment of that apparatus have demanded first-rate talent, much time, and considerable financial

backing. Synchrotrons and radiotelescopes are only the most recent examples of the lengths to which research workers will go if a paradigm assures them that the facts they seek are important. From Tycho Brahe to E. O. Lawrence, some scientists have acquired great reputations, not from any novelty of their discoveries, but from the precision, reliability, and scope of the methods they developed for the redetermination of a previously known sort of fact.

A second usual but smaller class of factual determinations is directed to those facts that, though often without much intrinsic interest, can be compared directly with predictions from the paradigm theory. As we shall see shortly, when I turn from the experimental to the theoretical problems of normal science, there are seldom many areas in which a scientific theory, particularly if it is cast in a predominantly mathematical form, can be directly compared with nature. No more than three such areas are even yet accessible to Einstein's general theory of relativity.² Furthermore, even in those areas where application is possible, it often demands theoretical and instrumental approximations that severely limit the agreement to be expected. Improving that agreement or finding new areas in which agreement can be demonstrated at all presents a constant challenge to the skill and imagination of the experimentalist and observer. Special telescopes to demonstrate the Copernican prediction of annual parallax; Atwood's machine, first invented almost a century after the *Principia*, to give the first unequivocal demonstration of Newton's second law; Foucault's apparatus to show that the speed of light is greater in air than in water; or the gigantic scintillation counter designed to demonstrate the existence of

² The only long-standing check point still generally recognized is the precession of Mercury's perihelion. The red shift in the spectrum of light from distant stars can be derived from considerations more elementary than general relativity, and the same may be possible for the bending of light around the sun, a point now in some dispute. In any case, measurements of the latter phenomenon remain equivocal. One additional check point may have been established very recently: the gravitational shift of Mossbauer radiation. Perhaps there will soon be others in this now active but long dormant field. For an up-to-date capsule account of the problem, see L. I. Schiff, "A Report on the NASA Conference on Experimental Tests of Theories of Relativity," *Physics Today*, XIV (1961), 42-48.

the neutrino—these pieces of special apparatus and many others like them illustrate the immense effort and ingenuity that have been required to bring nature and theory into closer and closer agreement.³ That attempt to demonstrate agreement is a second type of normal experimental work, and it is even more obviously dependent than the first upon a paradigm. The existence of the paradigm sets the problem to be solved; often the paradigm theory is implicated directly in the design of apparatus able to solve the problem. Without the *Principia*, for example, measurements made with the Atwood machine would have meant nothing at all.

A third class of experiments and observations exhausts, I think, the fact-gathering activities of normal science. It consists of empirical work undertaken to articulate the paradigm theory, resolving some of its residual ambiguities and permitting the solution of problems to which it had previously only drawn attention. This class proves to be the most important of all, and its description demands its subdivision. In the more mathematical sciences, some of the experiments aimed at articulation are directed to the determination of physical constants. Newton's work, for example, indicated that the force between two unit masses at unit distance would be the same for all types of matter at all positions in the universe. But his own problems could be solved without even estimating the size of this attraction, the universal gravitational constant; and no one else devised apparatus able to determine it for a century after the *Principia* appeared. Nor was Cavendish's famous determination in the 1790's the last. Because of its central position in physical theory, improved values of the gravitational constant have been the object of repeated efforts ever since by a number of outstanding

³ For two of the parallax telescopes, see Abraham Wolf, *A History of Science, Technology, and Philosophy in the Eighteenth Century* (2d ed.; London, 1952), pp. 103-5. For the Atwood machine, see N. R. Hanson, *Patterns of Discovery* (Cambridge, 1958), pp. 100-102, 207-8. For the last two pieces of special apparatus, see M. L. Foucault, "Méthode générale pour mesurer la vitesse de la lumière dans l'air et les milieux transparents. Vitesses relatives de la lumière dans l'air et dans l'eau . . ." *Comptes rendus . . . de l'Académie des sciences*, XXX (1850), 551-60; and C. L. Cowan, Jr., et al., "Detection of the Free Neutrino: A Confirmation," *Science*, CXXIV (1956), 103-4.

experimentalists.⁴ Other examples of the same sort of continuing work would include determinations of the astronomical unit, Avogadro's number, Joule's coefficient, the electronic charge, and so on. Few of these elaborate efforts would have been conceived and none would have been carried out without a paradigm theory to define the problem and to guarantee the existence of a stable solution.

Efforts to articulate a paradigm are not, however, restricted to the determination of universal constants. They may, for example, also aim at quantitative laws: Boyle's Law relating gas pressure to volume, Coulomb's Law of electrical attraction, and Joule's formula relating heat generated to electrical resistance and current are all in this category. Perhaps it is not apparent that a paradigm is prerequisite to the discovery of laws like these. We often hear that they are found by examining measurements undertaken for their own sake and without theoretical commitment. But history offers no support for so excessively Baconian a method. Boyle's experiments were not conceivable (and if conceived would have received another interpretation or none at all) until air was recognized as an elastic fluid to which all the elaborate concepts of hydrostatics could be applied.⁵ Coulomb's success depended upon his constructing special apparatus to measure the force between point charges. (Those who had previously measured electrical forces using ordinary pan balances, etc., had found no consistent or simple regularity at all.) But that design, in turn, depended upon the previous recognition that every particle of electric fluid acts upon every other at a distance. It was for the force between such particles—the only force which might safely be assumed

⁴ J. H. P[oynting] reviews some two dozen measurements of the gravitational constant between 1741 and 1901 in "Gravitation Constant and Mean Density of the Earth," *Encyclopaedia Britannica* (11th ed.; Cambridge, 1910-11), XII, 385-89.

⁵ For the full transplantation of hydrostatic concepts into pneumatics, see *The Physical Treatises of Pascal*, trans. L. H. B. Spiers and A. C. H. Spiers, with an introduction and notes by F. Barry (New York, 1937). Torricelli's original introduction of the parallelism ("We live submerged at the bottom of an ocean of the element air") occurs on p. 164. Its rapid development is displayed by the two main treatises.

a simple function of distance—that Coulomb was looking.⁶ Joule's experiments could also be used to illustrate how quantitative laws emerge through paradigm articulation. In fact, so general and close is the relation between qualitative paradigm and quantitative law that, since Galileo, such laws have often been correctly guessed with the aid of a paradigm years before apparatus could be designed for their experimental determination.⁷

Finally, there is a third sort of experiment which aims to articulate a paradigm. More than the others this one can resemble exploration, and it is particularly prevalent in those periods and sciences that deal more with the qualitative than with the quantitative aspects of nature's regularity. Often a paradigm developed for one set of phenomena is ambiguous in its application to other closely related ones. Then experiments are necessary to choose among the alternative ways of applying the paradigm to the new area of interest. For example, the paradigm applications of the caloric theory were to heating and cooling by mixtures and by change of state. But heat could be released or absorbed in many other ways—e.g., by chemical combination, by friction, and by compression or absorption of a gas—and to each of these other phenomena the theory could be applied in several ways. If the vacuum had a heat capacity, for example, heating by compression could be explained as the result of mixing gas with void. Or it might be due to a change in the specific heat of gases with changing pressure. And there were several other explanations besides. Many experiments were undertaken to elaborate these various possibilities and to distinguish between them; all these experiments arose from the caloric theory as paradigm, and all exploited it in the design of experiments and in the interpretation of results.⁸ Once the phe-

⁶ Duane Roller and Duane H. D. Roller, *The Development of the Concept of Electric Charge: Electricity from the Greeks to Coulomb* ("Harvard Case Histories in Experimental Science," Case 8; Cambridge, Mass., 1954), pp. 66-80.

⁷ For examples, see T. S. Kuhn, "The Function of Measurement in Modern Physical Science," *Isis*, LII (1961), 161-93.

⁸ T. S. Kuhn, "The Caloric Theory of Adiabatic Compression," *Isis*, XLIX (1958), 132-40.

nomenon of heating by compression had been established, all further experiments in the area were paradigm-dependent in this way. Given the phenomenon, how else could an experiment to elucidate it have been chosen?

Turn now to the theoretical problems of normal science, which fall into very nearly the same classes as the experimental and observational. A part of normal theoretical work, though only a small part, consists simply in the use of existing theory to predict factual information of intrinsic value. The manufacture of astronomical ephemerides, the computation of lens characteristics, and the production of radio propagation curves are examples of problems of this sort. Scientists, however, generally regard them as hack work to be relegated to engineers or technicians. At no time do very many of them appear in significant scientific journals. But these journals do contain a great many theoretical discussions of problems that, to the non-scientist, must seem almost identical. These are the manipulations of theory undertaken, not because the predictions in which they result are intrinsically valuable, but because they can be confronted directly with experiment. Their purpose is to display a new application of the paradigm or to increase the precision of an application that has already been made.

The need for work of this sort arises from the immense difficulties often encountered in developing points of contact between a theory and nature. These difficulties can be briefly illustrated by an examination of the history of dynamics after Newton. By the early eighteenth century those scientists who found a paradigm in the *Principia* took the generality of its conclusions for granted, and they had every reason to do so. No other work known to the history of science has simultaneously permitted so large an increase in both the scope and precision of research. For the heavens Newton had derived Kepler's Laws of planetary motion and also explained certain of the observed respects in which the moon failed to obey them. For the earth he had derived the results of some scattered observations on pendulums and the tides. With the aid of additional but *ad hoc* assumptions, he had also been able to derive Boyle's Law

and an important formula for the speed of sound in air. Given the state of science at the time, the success of the demonstrations was extremely impressive. Yet given the presumptive generality of Newton's Laws, the number of these applications was not great, and Newton developed almost no others. Furthermore, compared with what any graduate student of physics can achieve with those same laws today, Newton's few applications were not even developed with precision. Finally, the *Principia* had been designed for application chiefly to problems of celestial mechanics. How to adapt it for terrestrial applications, particularly for those of motion under constraint, was by no means clear. Terrestrial problems were, in any case, already being attacked with great success by a quite different set of techniques developed originally by Galileo and Huyghens and extended on the Continent during the eighteenth century by the Bernoullis, d'Alembert, and many others. Presumably their techniques and those of the *Principia* could be shown to be special cases of a more general formulation, but for some time no one saw quite how.⁹

Restrict attention for the moment to the problem of precision. We have already illustrated its empirical aspect. Special equipment—like Cavendish's apparatus, the Atwood machine, or improved telescopes—was required in order to provide the special data that the concrete applications of Newton's paradigm demanded. Similar difficulties in obtaining agreement existed on the side of theory. In applying his laws to pendulums, for example, Newton was forced to treat the bob as a mass point in order to provide a unique definition of pendulum length. Most of his theorems, the few exceptions being hypothetical and preliminary, also ignored the effect of air resistance. These were sound physical approximations. Nevertheless, as approximations they restricted the agreement to be expected

⁹ C. Truesdell, "A Program toward Rediscovering the Rational Mechanics of the Age of Reason," *Archive for History of the Exact Sciences*, I (1960), 3-36, and "Reactions of Late Baroque Mechanics to Success, Conjecture, Error, and Failure in Newton's *Principia*," *Texas Quarterly*, X (1967), 281-97. T. L. Hankins, "The Reception of Newton's Second Law of Motion in the Eighteenth Century," *Archives internationales d'histoire des sciences*, XX (1967), 42-65.

between Newton's predictions and actual experiments. The same difficulties appear even more clearly in the application of Newton's theory to the heavens. Simple quantitative telescopic observations indicate that the planets do not quite obey Kepler's Laws, and Newton's theory indicates that they should not. To derive those laws, Newton had been forced to neglect all gravitational attraction except that between individual planets and the sun. Since the planets also attract each other, only approximate agreement between the applied theory and telescopic observation could be expected.¹⁰

The agreement obtained was, of course, more than satisfactory to those who obtained it. Excepting for some terrestrial problems, no other theory could do nearly so well. None of those who questioned the validity of Newton's work did so because of its limited agreement with experiment and observation. Nevertheless, these limitations of agreement left many fascinating theoretical problems for Newton's successors. Theoretical techniques were, for example, required for treating the motions of more than two simultaneously attracting bodies and for investigating the stability of perturbed orbits. Problems like these occupied many of Europe's best mathematicians during the eighteenth and early nineteenth century. Euler, Lagrange, Laplace, and Gauss all did some of their most brilliant work on problems aimed to improve the match between Newton's paradigm and observation of the heavens. Many of these figures worked simultaneously to develop the mathematics required for applications that neither Newton nor the contemporary Continental school of mechanics had even attempted. They produced, for example, an immense literature and some very powerful mathematical techniques for hydrodynamics and for the problem of vibrating strings. These problems of application account for what is probably the most brilliant and consuming scientific work of the eighteenth century. Other examples could be discovered by an examination of the post-paradigm period in the development of thermodynamics, the wave theory of light, electromagnetic the-

¹⁰ Wolf, *op. cit.*, pp. 75-81, 96-101; and William Whewell, *History of the Inductive Sciences* (rev. ed.; London, 1847), II, 213-71.

ory, or any other branch of science whose fundamental laws are fully quantitative. At least in the more mathematical sciences, most theoretical work is of this sort.

But it is not all of this sort. Even in the mathematical sciences there are also theoretical problems of paradigm articulation; and during periods when scientific development is predominantly qualitative, these problems dominate. Some of the problems, in both the more quantitative and more qualitative sciences, aim simply at clarification by reformulation. The *Principia*, for example, did not always prove an easy work to apply, partly because it retained some of the clumsiness inevitable in a first venture and partly because so much of its meaning was only implicit in its applications. For many terrestrial applications, in any case, an apparently unrelated set of Continental techniques seemed vastly more powerful. Therefore, from Euler and Lagrange in the eighteenth century to Hamilton, Jacobi, and Hertz in the nineteenth, many of Europe's most brilliant mathematical physicists repeatedly endeavored to reformulate mechanical theory in an equivalent but logically and aesthetically more satisfying form. They wished, that is, to exhibit the explicit and implicit lessons of the *Principia* and of Continental mechanics in a logically more coherent version, one that would be at once more uniform and less equivocal in its application to the newly elaborated problems of mechanics.¹¹

Similar reformulations of a paradigm have occurred repeatedly in all of the sciences, but most of them have produced more substantial changes in the paradigm than the reformulations of the *Principia* cited above. Such changes result from the empirical work previously described as aimed at paradigm articulation. Indeed, to classify that sort of work as empirical was arbitrary. More than any other sort of normal research, the problems of paradigm articulation are simultaneously theoretical and experimental; the examples given previously will serve equally well here. Before he could construct his equipment and make measurements with it, Coulomb had to employ electrical theory to determine how his equipment should be built. The

¹¹ René Dugas, *Histoire de la mécanique* (Neuchâtel, 1950), Books IV-V.

consequence of his measurements was a refinement in that theory. Or again, the men who designed the experiments that were to distinguish between the various theories of heating by compression were generally the same men who had made up the versions being compared. They were working both with fact and with theory, and their work produced not simply new information but a more precise paradigm, obtained by the elimination of ambiguities that the original from which they worked had retained. In many sciences, most normal work is of this sort.

These three classes of problems—determination of significant fact, matching of facts with theory, and articulation of theory—exhaust, I think, the literature of normal science, both empirical and theoretical. They do not, of course, quite exhaust the entire literature of science. There are also extraordinary problems, and it may well be their resolution that makes the scientific enterprise as a whole so particularly worthwhile. But extraordinary problems are not to be had for the asking. They emerge only on special occasions prepared by the advance of normal research. Inevitably, therefore, the overwhelming majority of the problems undertaken by even the very best scientists usually fall into one of the three categories outlined above. Work under the paradigm can be conducted in no other way, and to desert the paradigm is to cease practicing the science it defines. We shall shortly discover that such desertions do occur. They are the pivots about which scientific revolutions turn. But before beginning the study of such revolutions, we require a more panoramic view of the normal-scientific pursuits that prepare the way.

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