
The Structure of Scientific
REVOLUTIONS

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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

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

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 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 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* fami-

is acquired through practice and that cannot be articulated explicitly. See his *Personal Knowledge* (Chicago, 1958), particularly chaps. v and vi.

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.

lies—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 already 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. Sci-

entists, 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

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* (Neuchâtel, 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.

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 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

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.

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 hesita-

tion, 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 particle, 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.

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.

The Response to Crisis

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

scientists to reject a previously accepted theory is always based upon more than a comparison of that theory with the world. The decision to reject one paradigm is always simultaneously the decision to accept another, and the judgment leading to that decision involves the comparison of both paradigms with nature *and* with each other.

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

It has often been observed, for example, that Newton's second law of motion, though it took centuries of difficult factual and theoretical research to achieve, behaves for those committed to Newton's theory very much like a purely logical statement that no amount of observation could refute.¹ In Section X we shall see that the chemical law of

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

fixed proportion, which before Dalton was an occasional experimental finding of very dubious generality, became after Dalton's work an ingredient of a definition of chemical compound that no experimental work could by itself have upset. Something much like that will also happen to the generalization that scientists fail to reject paradigms when faced with anomalies or counterinstances. They could not do so and still remain scientists.

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

The same point can be made at least equally effectively in reverse: there is no such thing as research without counterinstances. For what is it that differentiates normal science from science in a crisis state? Not, surely, that the former confronts no counterinstances. On the contrary, what we previously called the puzzles that constitute normal science exist only because no paradigm that provides a basis for scientific research ever completely resolves all its problems. The very few that have ever seemed to do so (e.g., geometric optics) have shortly ceased to yield research problems at all and have instead become tools for engineering. Excepting those that are exclusively instrumental,

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

every problem that normal science sees as a puzzle can be seen, from another viewpoint, as a counterinstance and thus as a source of crisis. Copernicus saw as counterinstances what most of Ptolemy's other successors had seen as puzzles in the match between observation and theory. Lavoisier saw as a counterinstance what Priestley had seen as a successfully solved puzzle in the articulation of the phlogiston theory. And Einstein saw as counterinstances what Lorentz, Fitzgerald, and others had seen as puzzles in the articulation of Newton's and Maxwell's theories. Furthermore, even the existence of crisis does not by itself transform a puzzle into a counterinstance. There is no such sharp dividing line. Instead, by proliferating versions of the paradigm, crisis loosens the rules of normal puzzle-solving in ways that ultimately permit a new paradigm to emerge. There are, I think, only two alternatives: either no scientific theory ever confronts a counterinstance, or all such theories confront counterinstances at all times.

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

tives have they, or what competence? The applications given in texts are not there as evidence but because learning them is part of learning the paradigm at the base of current practice. If applications were set forth as evidence, then the very failure of texts to suggest alternative interpretations or to discuss problems for which scientists have failed to produce paradigm solutions would convict their authors of extreme bias. There is not the slightest reason for such an indictment.

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

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

relativity after a crisis that it had had no role in creating.⁴ Apparently neither had seemed sufficiently fundamental to evoke the malaise that goes with crisis. They could be recognized as counterinstances and still be set aside for later work.

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

When, for these reasons or others like them, an anomaly comes to seem more than just another puzzle of normal science, the tran-

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

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

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

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

anywhere, upon which one could have built."⁶ And Wolfgang Pauli, in the months before Heisenberg's paper on matrix mechanics pointed the way to a new quantum theory, wrote to a friend, "At the moment physics is again terribly confused. In any case, it is too difficult for me, and I wish I had been a movie comedian or something of the sort and had never heard of physics." That testimony is particularly impressive if contrasted with Pauli's words less than five months later: "Heisenberg's type of mechanics has again given me hope and joy in life. To be sure it does not supply the solution to the riddle, but I believe it is again possible to march forward."⁷

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

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

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

order to complete these remarks about the evolution and anatomy of the crisis state.

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

The preceding anticipation may help us recognize crisis as an appropriate prelude to the emergence of new theories, particularly since we have already examined a small-scale version of the same process in

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

9. Hanson, *op. cit.*, chap. i.

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

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

In other cases, however—those of Copernicus, Einstein, and contemporary nuclear theory, for example—considerable time elapses between the first consciousness of breakdown and the emergence of a new paradigm. When that occurs, the historian may capture at least a few hints of what extraordinary science is like. Faced with an admit-

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

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

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

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

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

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

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

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

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

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

14. Einstein, *loc. cit.*

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

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

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

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