

VI. Anomaly and the Emergence of Scientific Discoveries

Normal science, the puzzle-solving activity we have just examined, is a highly cumulative enterprise, eminently successful in its aim, the steady extension of the scope and precision of scientific knowledge. In all these respects it fits with great precision the most usual image of scientific work. Yet one standard product of the scientific enterprise is missing. Normal science does not aim at novelties of fact or theory and, when successful, finds none. New and unsuspected phenomena are, however, repeatedly uncovered by scientific research, and radical new theories have again and again been invented by scientists. History even suggests that the scientific enterprise has developed a uniquely powerful technique for producing surprises of this sort. If this characteristic of science is to be reconciled with what has already been said, then research under a paradigm must be a particularly effective way of inducing paradigm change. That is what fundamental novelties of fact and theory do. Produced inadvertently by a game played under one set of rules, their assimilation requires the elaboration of another set. After they have become parts of science, the enterprise, at least of those specialists in whose particular field the novelties lie, is never quite the same again.

We must now ask how changes of this sort can come about, considering first discoveries, or novelties of fact, and then inventions, or novelties of theory. That distinction between discovery and invention or between fact and theory will, however, immediately prove to be exceedingly artificial. Its artificiality is an important clue to several of this essay's main theses. Examining selected discoveries in the rest of this section, we shall quickly find that they are not isolated events but extended episodes with a regularly recurrent structure. Discovery commences with the awareness of anomaly, i.e., with the recognition that nature has somehow violated the paradigm-induced

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expectations that govern normal science. It then continues with a more or less extended exploration of the area of anomaly. And it closes only when the paradigm theory has been adjusted so that the anomalous has become the expected. Assimilating a new sort of fact demands a more than additive adjustment of theory, and until that adjustment is completed—until the scientist has learned to see nature in a different way—the new fact is not quite a scientific fact at all.

To see how closely factual and theoretical novelty are intertwined in scientific discovery examine a particularly famous example, the discovery of oxygen. At least three different men have a legitimate claim to it, and several other chemists must, in the early 1770's, have had enriched air in a laboratory vessel without knowing it.¹ The progress of normal science, in this case of pneumatic chemistry, prepared the way to a breakthrough quite thoroughly. The earliest of the claimants to prepare a relatively pure sample of the gas was the Swedish apothecary, C. W. Scheele. We may, however, ignore his work since it was not published until oxygen's discovery had repeatedly been announced elsewhere and thus had no effect upon the historical pattern that most concerns us here.² The second in time to establish a claim was the British scientist and divine, Joseph Priestley, who collected the gas released by heated red oxide of mercury as one item in a prolonged normal investigation of the "airs" evolved by a large number of solid substances. In 1774 he identified the gas thus produced as nitrous oxide and in 1775, led by further tests, as common air with less than its usual quantity of phlogiston. The third claimant, Lavoisier, started the work that led him to oxygen after Priestley's experiments of 1774 and possibly as the result of a hint from Priestley. Early in

¹ For the still classic discussion of oxygen's discovery, see A. N. Meldrum, *The Eighteenth-Century Revolution in Science—the First Phase* (Calcutta, 1930), chap. v. An indispensable recent review, including an account of the priority controversy, is Maurice Daumas, *Lavoisier, théoricien et expérimentateur* (Paris, 1955), chaps. ii–iii. For a fuller account and bibliography, see also T. S. Kuhn, "The Historical Structure of Scientific Discovery," *Science*, CXXXVI (June 1, 1962), 760–64.

² See, however, Uno Bocklund, "A Lost Letter from Scheele to Lavoisier," *Lychnos*, 1957–58, pp. 39–62, for a different evaluation of Scheele's role.

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1775 Lavoisier reported that the gas obtained by heating the red oxide of mercury was "air itself entire without alteration [except that] . . . it comes out more pure, more respirable."³ By 1777, probably with the assistance of a second hint from Priestley, Lavoisier had concluded that the gas was a distinct species, one of the two main constituents of the atmosphere, a conclusion that Priestley was never able to accept.

This pattern of discovery raises a question that can be asked about every novel phenomenon that has ever entered the consciousness of scientists. Was it Priestley or Lavoisier, if either, who first discovered oxygen? In any case, when was oxygen discovered? In that form the question could be asked even if only one claimant had existed. As a ruling about priority and date, an answer does not at all concern us. Nevertheless, an attempt to produce one will illuminate the nature of discovery, because there is no answer of the kind that is sought. Discovery is not the sort of process about which the question is appropriately asked. The fact that it is asked—the priority for oxygen has repeatedly been contested since the 1780's—is a symptom of something askew in the image of science that gives discovery so fundamental a role. Look once more at our example. Priestley's claim to the discovery of oxygen is based upon his priority in isolating a gas that was later recognized as a distinct species. But Priestley's sample was not pure, and, if holding impure oxygen in one's hands is to discover it, that had been done by everyone who ever bottled atmospheric air. Besides, if Priestley was the discoverer, when was the discovery made? In 1774 he thought he had obtained nitrous oxide, a species he already knew; in 1775 he saw the gas as dephlogisticated air, which is still not oxygen or even, for phlogistic chemists, a quite unexpected sort of gas. Lavoisier's claim may be stronger, but it presents the same problems. If we refuse the palm to Priestley, we cannot award it to Lavoisier for the work of 1775 which led

³ J. B. Conant, *The Overthrow of the Phlogiston Theory: The Chemical Revolution of 1775–1789* ("Harvard Case Histories in Experimental Science," Case 2; Cambridge, Mass., 1950), p. 23. This very useful pamphlet reprints many of the relevant documents.

him to identify the gas as the "air itself entire." Presumably we wait for the work of 1776 and 1777 which led Lavoisier to see not merely the gas but what the gas was. Yet even this award could be questioned, for in 1777 and to the end of his life Lavoisier insisted that oxygen was an atomic "principle of acidity" and that oxygen gas was formed only when that "principle" united with caloric, the matter of heat.⁴ Shall we therefore say that oxygen had not yet been discovered in 1777? Some may be tempted to do so. But the principle of acidity was not banished from chemistry until after 1810, and caloric lingered until the 1860's. Oxygen had become a standard chemical substance before either of those dates.

Clearly we need a new vocabulary and concepts for analyzing events like the discovery of oxygen. Though undoubtedly correct, the sentence, "Oxygen was discovered," misleads by suggesting that discovering something is a single simple act assimilable to our usual (and also questionable) concept of seeing. That is why we so readily assume that discovering, like seeing or touching, should be unequivocally attributable to an individual and to a moment in time. But the latter attribution is always impossible, and the former often is as well. Ignoring Scheele, we can safely say that oxygen had not been discovered before 1774, and we would probably also say that it had been discovered by 1777 or shortly thereafter. But within those limits or others like them, any attempt to date the discovery must inevitably be arbitrary because discovering a new sort of phenomenon is necessarily a complex event, one which involves recognizing both *that* something is and *what* it is. Note, for example, that if oxygen were dephlogisticated air for us, we should insist without hesitation that Priestley had discovered it, though we would still not know quite when. But if both observation and conceptualization, fact and assimilation to theory, are inseparably linked in discovery, then discovery is a process and must take time. Only when all the relevant conceptual categories are prepared in advance, in which case the phenomenon would not

⁴ H. Metzger, *La philosophie de la matière chez Lavoisier* (Paris, 1935); and Daumas, *op. cit.*, chap. vii.

be of a new sort, can discovering *that* and discovering *what* occur effortlessly, together, and in an instant.

Grant now that discovery involves an extended, though not necessarily long, process of conceptual assimilation. Can we also say that it involves a change in paradigm? To that question, no general answer can yet be given, but in this case at least, the answer must be yes. What Lavoisier announced in his papers from 1777 on was not so much the discovery of oxygen as the oxygen theory of combustion. That theory was the keystone for a reformulation of chemistry so vast that it is usually called the chemical revolution. Indeed, if the discovery of oxygen had not been an intimate part of the emergence of a new paradigm for chemistry, the question of priority from which we began would never have seemed so important. In this case as in others, the value placed upon a new phenomenon and thus upon its discoverer varies with our estimate of the extent to which the phenomenon violated paradigm-induced anticipations. Notice, however, since it will be important later, that the discovery of oxygen was not by itself the cause of the change in chemical theory. Long before he played any part in the discovery of the new gas, Lavoisier was convinced both that something was wrong with the phlogiston theory and that burning bodies absorbed some part of the atmosphere. That much he had recorded in a sealed note deposited with the Secretary of the French Academy in 1772.⁵ What the work on oxygen did was to give much additional form and structure to Lavoisier's earlier sense that something was amiss. It told him a thing he was already prepared to discover—the nature of the substance that combustion removes from the atmosphere. That advance awareness of difficulties must be a significant part of what enabled Lavoisier to see in experiments like Priestley's a gas that Priestley had been unable to see there himself. Conversely, the fact that a major paradigm revision was needed to see what Lavoisier saw must be the principal reason why Priestley was, to the end of his long life, unable to see it.

⁵ The most authoritative account of the origin of Lavoisier's discontent is Henry Guerlac, *Lavoisier—the Crucial Year: The Background and Origin of His First Experiments on Combustion in 1772* (Ithaca, N.Y., 1961).

Two other and far briefer examples will reinforce much that has just been said and simultaneously carry us from an elucidation of the nature of discoveries toward an understanding of the circumstances under which they emerge in science. In an effort to represent the main ways in which discoveries can come about, these examples are chosen to be different both from each other and from the discovery of oxygen. The first, X-rays, is a classic case of discovery through accident, a type that occurs more frequently than the impersonal standards of scientific reporting allow us easily to realize. Its story opens on the day that the physicist Roentgen interrupted a normal investigation of cathode rays because he had noticed that a barium platino-cyanide screen at some distance from his shielded apparatus glowed when the discharge was in process. Further investigations—they required seven hectic weeks during which Roentgen rarely left the laboratory—indicated that the cause of the glow came in straight lines from the cathode ray tube, that the radiation cast shadows, could not be deflected by a magnet, and much else besides. Before announcing his discovery, Roentgen had convinced himself that his effect was not due to cathode rays but to an agent with at least some similarity to light.⁶

Even so brief an epitome reveals striking resemblances to the discovery of oxygen: before experimenting with red oxide of mercury, Lavoisier had performed experiments that did not produce the results anticipated under the phlogiston paradigm; Roentgen's discovery commenced with the recognition that his screen glowed when it should not. In both cases the perception of anomaly—of a phenomenon, that is, for which his paradigm had not readied the investigator—played an essential role in preparing the way for perception of novelty. But, again in both cases, the perception that something had gone wrong was only the prelude to discovery. Neither oxygen nor X-rays emerged without a further process of experimentation and assimilation. At what point in Roentgen's investigation, for example, ought we say that X-rays had actually been discovered? Not, in any

⁶ L. W. Taylor, *Physics, the Pioneer Science* (Boston, 1941), pp. 790-94; and T. W. Chalmers, *Historic Researches* (London, 1949), pp. 218-19.

case, at the first instant, when all that had been noted was a glowing screen. At least one other investigator had seen that glow and, to his subsequent chagrin, discovered nothing at all.⁷ Nor, it is almost as clear, can the moment of discovery be pushed forward to a point during the last week of investigation, by which time Roentgen was exploring the properties of the new radiation he had *already* discovered. We can only say that X-rays emerged in Würzburg between November 8 and December 28, 1895.

In a third area, however, the existence of significant parallels between the discoveries of oxygen and of X-rays is far less apparent. Unlike the discovery of oxygen, that of X-rays was not, at least for a decade after the event, implicated in any obvious upheaval in scientific theory. In what sense, then, can the assimilation of that discovery be said to have necessitated paradigm change? The case for denying such a change is very strong. To be sure, the paradigms subscribed to by Roentgen and his contemporaries could not have been used to predict X-rays. (Maxwell's electromagnetic theory had not yet been accepted everywhere, and the particulate theory of cathode rays was only one of several current speculations.) But neither did those paradigms, at least in any obvious sense, prohibit the existence of X-rays as the phlogiston theory had prohibited Lavoisier's interpretation of Priestley's gas. On the contrary, in 1895 accepted scientific theory and practice admitted a number of forms of radiation—visible, infrared, and ultraviolet. Why could not X-rays have been accepted as just one more form of a well-known class of natural phenomena? Why were they not, for example, received in the same way as the discovery of an additional chemical element? New elements to fill empty places in the periodic table were still being sought and found in Roentgen's day. Their pursuit was a standard project for normal science, and success was an occasion only for congratulations, not for surprise.

⁷ E. T. Whittaker, *A History of the Theories of Aether and Electricity*, I (2d ed.; London, 1951), 358, n. 1. Sir George Thomson has informed me of a second near miss. Alerted by unaccountably fogged photographic plates, Sir William Crookes was also on the track of the discovery.

X-rays, however, were greeted not only with surprise but with shock. Lord Kelvin at first pronounced them an elaborate hoax.⁸ Others, though they could not doubt the evidence, were clearly staggered by it. Though X-rays were not prohibited by established theory, they violated deeply entrenched expectations. Those expectations, I suggest, were implicit in the design and interpretation of established laboratory procedures. By the 1890's cathode ray equipment was widely deployed in numerous European laboratories. If Roentgen's apparatus had produced X-rays, then a number of other experimentalists must for some time have been producing those rays without knowing it. Perhaps those rays, which might well have other unacknowledged sources too, were implicated in behavior previously explained without reference to them. At the very least, several sorts of long familiar apparatus would in the future have to be shielded with lead. Previously completed work on normal projects would now have to be done again because earlier scientists had failed to recognize and control a relevant variable. X-rays, to be sure, opened up a new field and thus added to the potential domain of normal science. But they also, and this is now the more important point, changed fields that had already existed. In the process they denied previously paradigmatic types of instrumentation their right to that title.

In short, consciously or not, the decision to employ a particular piece of apparatus and to use it in a particular way carries an assumption that only certain sorts of circumstances will arise. There are instrumental as well as theoretical expectations, and they have often played a decisive role in scientific development. One such expectation is, for example, part of the story of oxygen's belated discovery. Using a standard test for "the goodness of air," both Priestley and Lavoisier mixed two volumes of their gas with one volume of nitric oxide, shook the mixture over water, and measured the volume of the gaseous residue. The previous experience from which this standard procedure had evolved assured them that with atmospheric air the residue

⁸ Silvanus P. Thompson, *The Life of Sir William Thomson Baron Kelvin of Largs* (London, 1910), II, 1125.

would be one volume and that for any other gas (or for polluted air) it would be greater. In the oxygen experiments both found a residue close to one volume and identified the gas accordingly. Only much later and in part through an accident did Priestley renounce the standard procedure and try mixing nitric oxide with his gas in other proportions. He then found that with quadruple the volume of nitric oxide there was almost no residue at all. His commitment to the original test procedure—a procedure sanctioned by much previous experience—had been simultaneously a commitment to the non-existence of gases that could behave as oxygen did.⁹

Illustrations of this sort could be multiplied by reference, for example, to the belated identification of uranium fission. One reason why that nuclear reaction proved especially difficult to recognize was that men who knew what to expect when bombarding uranium chose chemical tests aimed mainly at elements from the upper end of the periodic table.¹⁰ Ought we conclude from the frequency with which such instrumental commitments prove misleading that science should abandon standard tests and standard instruments? That would result in an inconceivable method of research. Paradigm procedures and applications are as necessary to science as paradigm laws and theories, and they have the same effects. Inevitably they restrict the phenomenological field accessible for scientific investigation at any

⁹ Conant, *op. cit.*, pp. 18–20.

¹⁰ K. K. Darrow, "Nuclear Fission," *Bell System Technical Journal*, XIX (1940), 267–89. Krypton, one of the two main fission products, seems not to have been identified by chemical means until after the reaction was well understood. Barium, the other product, was almost identified chemically at a late stage of the investigation because, as it happened, that element had to be added to the radioactive solution to precipitate the heavy element for which nuclear chemists were looking. Failure to separate that added barium from the radioactive product finally led, after the reaction had been repeatedly investigated for almost five years, to the following report: "As chemists we should be led by this research . . . to change all the names in the preceding [reaction] schema and thus write Ba, La, Ce instead of Ra, Ac, Th. But as 'nuclear chemists,' with close affiliations to physics, we cannot bring ourselves to this leap which would contradict all previous experience of nuclear physics. It may be that a series of strange accidents renders our results deceptive" (Otto Hahn and Fritz Strassman, "Über den Nachweis und das Verhalten der bei der Bestrahlung des Urans mittels Neutronen entstehenden Erdalkalimetalle," *Die Naturwissenschaften*, XXVII [1939], 15).

given time. Recognizing that much, we may simultaneously see an essential sense in which a discovery like X-rays necessitates paradigm change—and therefore change in both procedures and expectations—for a special segment of the scientific community. As a result, we may also understand how the discovery of X-rays could seem to open a strange new world to many scientists and could thus participate so effectively in the crisis that led to twentieth-century physics.

Our final example of scientific discovery, that of the Leyden jar, belongs to a class that may be described as theory-induced. Initially, the term may seem paradoxical. Much that has been said so far suggests that discoveries predicted by theory in advance are parts of normal science and result in no *new sort* of fact. I have, for example, previously referred to the discoveries of new chemical elements during the second half of the nineteenth century as proceeding from normal science in that way. But not all theories are paradigm theories. Both during pre-paradigm periods and during the crises that lead to large-scale changes of paradigm, scientists usually develop many speculative and unarticulated theories that can themselves point the way to discovery. Often, however, that discovery is not quite the one anticipated by the speculative and tentative hypothesis. Only as experiment and tentative theory are together articulated to a match does the discovery emerge and the theory become a paradigm.

The discovery of the Leyden jar displays all these features as well as the others we have observed before. When it began, there was no single paradigm for electrical research. Instead, a number of theories, all derived from relatively accessible phenomena, were in competition. None of them succeeded in ordering the whole variety of electrical phenomena very well. That failure is the source of several of the anomalies that provide background for the discovery of the Leyden jar. One of the competing schools of electricians took electricity to be a fluid, and that conception led a number of men to attempt bottling the fluid by holding a water-filled glass vial in their hands and touching the water to a conductor suspended from an active

electrostatic generator. On removing the jar from the machine and touching the water (or a conductor connected to it) with his free hand, each of these investigators experienced a severe shock. Those first experiments did not, however, provide electricians with the Leyden jar. That device emerged more slowly, and it is again impossible to say just when its discovery was completed. The initial attempts to store electrical fluid worked only because investigators held the vial in their hands while standing upon the ground. Electricians had still to learn that the jar required an outer as well as an inner conducting coating and that the fluid is not really stored in the jar at all. Somewhere in the course of the investigations that showed them this, and which introduced them to several other anomalous effects, the device that we call the Leyden jar emerged. Furthermore, the experiments that led to its emergence, many of them performed by Franklin, were also the ones that necessitated the drastic revision of the fluid theory and thus provided the first full paradigm for electricity.¹¹

To a greater or lesser extent (corresponding to the continuum from the shocking to the anticipated result), the characteristics common to the three examples above are characteristic of all discoveries from which new sorts of phenomena emerge. Those characteristics include: the previous awareness of anomaly, the gradual and simultaneous emergence of both observational and conceptual recognition, and the consequent change of paradigm categories and procedures often accompanied by resistance. There is even evidence that these same characteristics are built into the nature of the perceptual process itself. In a psychological experiment that deserves to be far better known outside the trade, Bruner and Postman asked experimental subjects to identify on short and controlled exposure a series of playing cards. Many of the cards were normal, but some were made anoma-

¹¹ For various stages in the Leyden jar's evolution, 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), pp. 385-86, 400-406, 452-67, 506-7. The last stage is described by Whittaker, *op. cit.*, pp. 50-52.

lous, e.g., a red six of spades and a black four of hearts. Each experimental run was constituted by the display of a single card to a single subject in a series of gradually increased exposures. After each exposure the subject was asked what he had seen, and the run was terminated by two successive correct identifications.¹²

Even on the shortest exposures many subjects identified most of the cards, and after a small increase all the subjects identified them all. For the normal cards these identifications were usually correct, but the anomalous cards were almost always identified, without apparent hesitation or puzzlement, as normal. The black four of hearts might, for example, be identified as the four of either spades or hearts. Without any awareness of trouble, it was immediately fitted to one of the conceptual categories prepared by prior experience. One would not even like to say that the subjects had seen something different from what they identified. With a further increase of exposure to the anomalous cards, subjects did begin to hesitate and to display awareness of anomaly. Exposed, for example, to the red six of spades, some would say: That's the six of spades, but there's something wrong with it—the black has a red border. Further increase of exposure resulted in still more hesitation and confusion until finally, and sometimes quite suddenly, most subjects would produce the correct identification without hesitation. Moreover, after doing this with two or three of the anomalous cards, they would have little further difficulty with the others. A few subjects, however, were never able to make the requisite adjustment of their categories. Even at forty times the average exposure required to recognize normal cards for what they were, more than 10 per cent of the anomalous cards were not correctly identified. And the subjects who then failed often experienced acute personal distress. One of them exclaimed: "I can't make the suit out, whatever it is. It didn't even look like a card that time. I don't know what color it is now or whether it's a spade or a heart. I'm

¹² J. S. Bruner and Leo Postman, "On the Perception of Incongruity: A Paradigm," *Journal of Personality*, XVIII (1949), 206-23.

not even sure now what a spade looks like. My God!"¹³ In the next section we shall occasionally see scientists behaving this way too.

Either as a metaphor or because it reflects the nature of the mind, that psychological experiment provides a wonderfully simple and cogent schema for the process of scientific discovery. In science, as in the playing card experiment, novelty emerges only with difficulty, manifested by resistance, against a background provided by expectation. Initially, only the anticipated and usual are experienced even under circumstances where anomaly is later to be observed. Further acquaintance, however, does result in awareness of something wrong or does relate the effect to something that has gone wrong before. That awareness of anomaly opens a period in which conceptual categories are adjusted until the initially anomalous has become the anticipated. At this point the discovery has been completed. I have already urged that that process or one very much like it is involved in the emergence of all fundamental scientific novelties. Let me now point out that, recognizing the process, we can at last begin to see why normal science, a pursuit not directed to novelties and tending at first to suppress them, should nevertheless be so effective in causing them to arise.

In the development of any science, the first received paradigm is usually felt to account quite successfully for most of the observations and experiments easily accessible to that science's practitioners. Further development, therefore, ordinarily calls for the construction of elaborate equipment, the development of an esoteric vocabulary and skills, and a refinement of concepts that increasingly lessens their resemblance to their usual common-sense prototypes. That professionalization leads, on the one hand, to an immense restriction of the scientist's vision and to a considerable resistance to paradigm change. The science has become increasingly rigid. On the other hand, within those areas to which the paradigm directs the attention of the

¹³ *Ibid.*, p. 218. My colleague Postman tells me that, though knowing all about the apparatus and display in advance, he nevertheless found looking at the incongruous cards acutely uncomfortable.

group, normal science leads to a detail of information and to a precision of the observation-theory match that could be achieved in no other way. Furthermore, that detail and precision-of-match have a value that transcends their not always very high intrinsic interest. Without the special apparatus that is constructed mainly for anticipated functions, the results that lead ultimately to novelty could not occur. And even when the apparatus exists, novelty ordinarily emerges only for the man who, knowing *with precision* what he should expect, is able to recognize that something has gone wrong. Anomaly appears only against the background provided by the paradigm. The more precise and far-reaching that paradigm is, the more sensitive an indicator it provides of anomaly and hence of an occasion for paradigm change. In the normal mode of discovery, even resistance to change has a use that will be explored more fully in the next section. By ensuring that the paradigm will not be too easily surrendered, resistance guarantees that scientists will not be lightly distracted and that the anomalies that lead to paradigm change will penetrate existing knowledge to the core. The very fact that a significant scientific novelty so often emerges simultaneously from several laboratories is an index both to the strongly traditional nature of normal science and to the completeness with which that traditional pursuit prepares the way for its own change.