etc., on the one hand, and with the *Sinanthropus* such as the sagittal crest, the torus angularis, the flat orbital roof, the well-developed articular tubercle, the absence of post-glenoid process, etc., on the other" (Woo's own translation which accompanies the Chinese text).
18. W. C. Pei, *Acta Paleontologica Sinica* **4**, No. 4, 477–489 (1956); *Vertebrata Palasiatica* **1**, No. 2, 65–70 (1957).
19. ———, and Y. H. Li, *Vertebrata Palasiatica* **2**, No. 4, 193–197 (1958).
20. ———, *ibid.*, p. 199. (The material I quote is the translation by Pei and Li.)
21. J. K. Woo, *ibid.* **3**, No. 3, 109–110 (1959).
22. ———, *ibid.* pp. 115–116. The following are some of the morphological details of Liu-chiang Man. *Cranium*: 189.3 mm long, 142.2 mm wide, with cephalic index of 75.1, and basion-bregma height of 134.8 mm. Angle between the lines joining nasion and right and left fronto-malare orbitale points is 143.5°, a figure intermediate between the modern Mongoloid (145 to 149°) and the Australo-Negroid (140 to 142°) ranges according to Roginsky and Levin as quoted by Woo. The Simotic index is 28.3, falling within the range for modern Australo-Negroid races (20 to 45). But, as among modern Mongoloids, the nasal bones are flat at the root and broad, with a low ridge and a predominantly concave nasal profile and lacking any depression at nasion. The prenasal fossae are shallow and the malar bones fairly large and protruding. The upper lateral incisors exhibit clear shovel-shaped characters. *Femur*: The right femur has a platymeric index of 73.7 (average for Neanderthals, 75.6; that for modern Northern Chinese, 80.2, as quoted by Woo), and the left femur has an index of robusticity of 36.4 in transverse diameters and one of 38.2 in sagittal diameters (being between the same indices for *Sinanthropus* —32.9 and 37.6—and the Upper Cave average —46.9 and 47.0).
23. Kwangtung Provincial Museum, *Paleovertebrata et Paleoanthropol.* **1**, No. 2, 94 (1959).
24. J. K. Woo and J. T. Peng, *Vertebrata Palasiatica* **3**, No. 4, 175–182 (1959). A few important measurements of the Ma-pa skull are: Thickness of the parietal at bregma is 7 mm; it has a frontal chord/arc index of 86.3, a parietal chord/arc index of 93.9, and an occipital chord/arc index of 79.9. On the cerebral surface of the parietal, imprints of the meningeal vessels show that the posterior branch is, as in the Ordos parietal, more developed than the anterior.
25. H. L. Movius, Jr., *Papers of the Peabody Museum*, Harvard University, 19 (1944).
26. ———, *Trans. Am. Phil. Soc.* **38**, 345–348 (1948).
27. See L. P. Chia, *Vertebrata Palasiatica* **3**, No. 1, 41 (1959).
28. Also found at Nan-kou-ling in Ch'ih-ch'eng, northern Hopei, in association with *Siphneus epitingi*, *Rhinoceros mercki*, and *Equus sanmeniensis*. L. P. Chia and J. C. Chai, *ibid.* **1**, No. 1, 47–51 (1957).
29. L. P. Chia, *ibid.* **3**, No. 1, 41 (1959).
30. H. D. Kahlke and C. K. Hu, *ibid.* **1**, No. 4 (1957).
31. W. P. Huang, *ibid.* **4**, No. 1, 45–46 (1960).
32. B. Kurtén, *Am. Museum Novitates* **No. 1764** (1956); *Quarternaria* **4**, 69–81 (1957); *J. Paleontol.* **31**, 215–227 (1957).
33. ———, *Vertebrata Palasiatica* **3**, No. 4, 173–175 (1959).
34. T. Y. Wang, C. L. Chiu, C. C. Pi, *Paleovertebrata et Paleoanthropologia* **1**, No. 2, 88–91 (1959); L. P. Chia, *K'ao-ku* **1959**, No. 1, 18–20 (1959); C. L. Chiu, *Vertebrata Palasiatica* **2**, No. 4, 281–287 (1958).
35. L. P. Chia and T. Y. Wang, *K'ao-ku-t'ung-hsün* **1957**, No. 5, 12–18 (1957).
36. W. C. Pei *et al.*, *Shan-hsi Hsiang-fen-hsien Ting-ts'un chiu-shih-ch'i shih-tai i-chih fa-chüeh pao-kao* (Peiping, 1958).
37. L. P. Chia, in *Chung-kuo jen-lei hua-shih ti fa-hsien yü yen-chiu* (Peiping 1955).
38. H. L. Movius, Jr., *Quarternaria* **3**, 13 (1956).
39. W. C. Pei *et al.*, *Vertebrata Palasiatica* **2**, No. 4, 213–225 (1958).
40. S. S. Chang, *ibid.* **3**, 47–56 (1959).
41. P. Teilhard de Chardin and C. C. Young, *Bull. Geol. Soc. China* **14**, 161–178 (1935).
42. M. N. Bien, *ibid.* **20**, 179–204 (1940). A Lower Pleistocene dating of this assemblage has been confirmed by new materials collected recently [W. C. Pei, *Vertebrata Palasiatica* **1961**, No. 1, 16–30 (1961)].
43. G. H. R. von Koenigswald, *Anthropol. Papers, Am. Museum Nat. Hist.* **43**, 301–309 (1952). The diagnostic forms of this fauna are: *Pongo* cf. *satyrus*, *Ailuropoda*, *Megatapirus*, *Rhinoceros sinensis*, *Stegodon orientalis*, *Arctonyx*, *Hystrix*, *Rhizomys*, *Paguma*, *Rusa*. Von Koenigswald also includes *Gigantopithecus* and the so-called *Sinanthropus officinalis*.
44. M. C. Chow, *Vertebrata Palasiatica* **1**, No. 1, 57 (1957); W. C. Pei, *ibid.*, p. 16 (1957).
45. W. C. Pei, *ibid.* **4**, No. 1, 41 (1960). A more detailed analysis of this problem is in H. D. Kahlke, *ibid.* **1961**, No. 2, 83–108 (1961).
46. X. Z. Wu, *ibid.* **4**, No. 1, 35–36 (1960); Y. Yen, C. Z. Liu, Y. M. Gu, *ibid.* **4**, No. 2, 103–111 (1960); L. P. Chia and J. K. Woo, *ibid.* **3**, No. 1, 37–39 (1959).
47. J. K. Woo and N. N. Cheboksarov, *Sovet. Etnogr.* **1959**, No. 4, 3–24 (1959); also, J. K. Woo, *Sci. Record (China)* [N.S.] **4**, No. 2, 120–125 (1960). The classification of the Ma-pa skull here is different from Woo's earlier position in his descriptive report on the skull, as mentioned earlier.
48. F. Weidenreich, *Palaeontologia Sinica* [N.S.] Ser. D. **1943**, No. 10, 253–254 (1943). The 12 peculiar traits are: Mandibular torus; shovel-shaped upper incisors; pronounced platymeria and strong deltoid tuberosity; sagittal crest; os Incae; broad nasal bones with small differences between upper and middle breadths; the profile contour of the nasal saddle; the profile angle of the nasal roof; the pronounced frontal orientation displayed by the malar facies and the frontosphenoidal process of the maxilla; the roundness of the infraorbital margin and its evenness with the floor of the orbit; the buccal exostosis of the maxilla; and the exostosis of the external auditory meatus.

Historical Structure of Scientific Discovery

To the historian discovery is seldom a unit event attributable to some particular man, time, and place.

Thomas S. Kuhn

My object in this article is to isolate and illuminate one small part of what I take to be a continuing historiographic revolution in the study of science (*1*). The structure of scientific discovery is my particular topic, and I can best approach it by pointing out that the subject itself may well seem extraordinarily odd. Both scientists and, until quite recently, historians have ordinarily viewed discovery as the sort of event which, though it may have preconditions and surely has consequences, is itself without internal structure. Rather than being seen as a complex development extended both in space and time, discovering something has usually seemed to be a unitary event, one which, like seeing something, happens to an individual at a specifiable time and place.

This view of the nature of discovery has, I suspect, deep roots in the nature of the scientific community. One of the few historical elements recurrent in the textbooks from which the prospective scientist learns his field is the attribution of particular natural phenomena to the historical personages who first discovered them. As a result of this and other aspects of their training, discovery becomes for many scientists an important goal. To make a discovery is to achieve one of the closest approximations to a property right that the scientific career affords. Professional prestige is often closely associated with these acquisitions (*2*). Small wonder, then, that acrimonious disputes about priority and independence in discovery have often marred the normally placid tenor of scientific communication. Even less wonder that many historians of science have seen the individual discovery as an appropriate unit with which to measure scientific progress and have devoted much time and skill

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to determining what man made which discovery at what point in time. If the study of discovery has a surprise to offer, it is only that, despite the immense energy and ingenuity expended upon it, neither polemic nor painstaking scholarship has often succeeded in pinpointing the time and place at which a given discovery could properly be said to have "been made."

Some Discoveries Predictable, Some Not

That failure, both of argument and of research, suggests the thesis that I now wish to develop. Many scientific discoveries, particularly the most interesting and important, are not the sort of event about which the questions "Where?" and, more particularly, "When?" can appropriately be asked. Even if all conceivable data were at hand, those questions would not regularly possess answers. That we are persistently driven to ask them nonetheless is symptomatic of a fundamental inappropriateness in our image of discovery. That inappropriateness is here my main concern, but I approach it by considering first the historical problem presented by the attempt to date and to place a major class of fundamental discoveries.

The troublesome class consists of those discoveries—including oxygen, the electric current, x-rays, and the electron—which could not be predicted from accepted theory in advance and which therefore caught the assembled profession by surprise. That kind of discovery will shortly be my exclusive concern, but it will help first to note that there is another sort and one which presents very few of the same problems. Into this second class of discoveries fall the neutrino, radio waves, and the elements which filled empty places in the periodic table. The existence of all these objects had been predicted from theory before they were discovered, and the men who made the discoveries therefore knew from the start what to look for. That foreknowledge did not make their task less demanding or less interesting, but it did provide criteria which told them when their goal had been reached (*3*). As a result, there have been few priority debates over discoveries of this second sort, and only a paucity of data can prevent the historian from ascribing them to a particular time and place. Those facts help to isolate the difficulties we encounter as we return to the troublesome discoveries of the first class. In the cases that most concern us here there are no benchmarks to inform either the scientist or the historian when the job of discovery has been done.

Oxygen as an Example

As an illustration of this fundamental problem and its consequences, consider first the discovery of oxygen. Because it has repeatedly been studied, often with exemplary care and skill, that discovery is unlikely to offer any purely factual surprises. Therefore it is particularly well suited to clarify points of principle (*4*). At least three scientists—Carl Scheele, Joseph Priestley, and Antoine Lavoisier—have a legitimate claim to this discovery, and polemicists have occasionally entered the same claim for Pierre Bayen (*5*). Scheele's work, though it was almost certainly completed before the relevant researches of Priestley and Lavoisier, was not made public until their work was well known (*6*). Therefore it had no apparent causal role, and I shall simplify my story by omitting it (*7*). Instead, I pick up the main route to the discovery of oxygen with the work of Bayen, who, sometime before March 1774, discovered that red precipitate of mercury (HgO) could, by heating, be made to yield a gas. That aeriform product Bayen identified as fixed air (CO_2), a substance made familiar to most pneumatic chemists by the earlier work of Joseph Black (*8*). A variety of other substances were known to yield the same gas.

At the beginning of August 1774, a few months after Bayen's work had appeared, Joseph Priestley repeated the experiment, though probably independently. Priestley, however, observed that the gaseous product would support combustion and therefore changed the identification. For him the gas obtained on heating red precipitate was nitrous air (N_2O), a substance that he had himself discovered more than two years before (*9*). Later in the same month Priestley made a trip to Paris and there informed Lavoisier of the new reaction. The latter repeated the experiment once more, both in November 1774 and in February 1775. Only, because he used tests somewhat more elaborate than Priestley's, Lavoisier again changed the identification. For him, as of May 1775, the gas released by red precipitate was neither fixed air nor nitrous air. Instead, it was "[atmospheric] air itself entire without alteration . . . even to the point that . . . it comes out more pure" (*10*). Meanwhile, however, Priestley had also been at work, and, before the beginning of March 1775, he too had concluded that the gas must be "common air." To this point all of the men who had produced a gas from red precipitate of mercury had identified it with some previously known species (*11*).

The remainder of this story of discovery is briefly told. During March 1775 Priestley discovered that his gas was in several respects very much "better" than common air, and he therefore re-identified the gas once more, this time calling it "dephlogisticated air," that is, atmospheric air deprived of its normal complement of phlogiston. This conclusion Priestley published in the *Philosophical Transactions*, and it was apparently that publication which led Lavoisier to reexamine his own results (*12*). The reexamination began during February 1776 and within a year had led Lavoisier to the conclusion that the gas was actually a separable component of the atmospheric air which both he and Priestley had previously thought of as homogeneous. With this point reached, with the gas recognized as an irreducibly distinct species, we may conclude that the discovery of oxygen had been completed.

Only, to return to my initial question, when shall we say that oxygen was discovered and what criteria shall we use in answering that question? If discovering oxygen is simply holding an impure sample in one's hands, then the gas had been "discovered" in antiquity by the first man who ever bottled atmospheric air. Undoubtedly, for an experimental criterion, we must at least require a relatively pure sample like that obtained by Priestley in August 1774. But during 1774 Priestley was unaware that he had discovered anything except a new way to produce a relatively familiar species. Throughout that year his "discovery" is scarcely distinguishable from the one made earlier by Bayen, and neither case is quite distinct from that of the Reverend Stephen Hales who had obtained the same gas more than 40 years before (*13*). Apparently to discover something one must also be aware of the discovery and know as well what it is that one has discovered.

But, that being the case, how much must one know? Had Priestley come close enough when he identified the gas as nitrous air? If not, was either he or Lavoisier significantly closer when he changed the identification to common air? And what are we to say about Priestley's next identification, the one made in March 1775? Dephlogisticated air is still not oxygen or even, for the phlogistic chemist, a quite unexpected sort of gas. Rather it is a particularly pure atmospheric air. Presumably, then, we wait for Lavoisier's work in 1776 and 1777, work which led him not merely to isolate the gas but to see what it was. Yet even that decision can 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 (*14*). 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 on until the 1860's. Oxygen had, however, become a standard chemical substance long before either of those dates. Furthermore, what is perhaps the key point, it would probably have gained that status on the basis of Priestley's work alone without benefit of Lavoisier's still partial reinterpretation.

I conclude that we need a new vocabulary and new 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 unequivocally attributable, if only we knew enough, to an individual and an instant in time. When the discovery is unexpected, however, the latter attribution is always impossible and the former often is as well. Ignoring Scheele, we can, for example, safely say that oxygen had not been discovered before 1774; probably we would also insist that it had been discovered by 1777 or shortly thereafter. But within those limits any attempt to date the discovery or to attribute it to an individual must inevitably be arbitrary. Furthermore, it must be arbitrary just because discovering a new sort of phenomenon is necessarily a complex process which involves recognizing both *that* something is and *what* it is. Observation and conceptualization, fact and the assimilation of fact to theory, are inseparably linked in the discovery of scientific novelty. Inevitably, that process extends over time and may often involve a number of people. Only for discoveries in my second category—those whose nature is known in advance—can discovering *that* and discovering *what* occur together and in an instant.

Uranus and X-rays

Two last, simpler, and far briefer examples will simultaneously show how typical the case of oxygen is and also prepare the way for a somewhat more precise conclusion. On the night of 13 March 1781, the astronomer William Herschel made the following entry in his journal: "In the quartile near Zeta Tauri . . . is a curious either nebulous star or perhaps a comet" (*15*). That entry is generally said to record the discovery of the planet Uranus, but it cannot quite have done that. Between 1690 and Herschel's observation in 1781 the same object had been seen and recorded at least 17 times by men who took it to be a star. Herschel differed from them only in supposing that, because in his telescope it appeared especially large, it might actually be a *comet*! Two additional observations on 17 and 19 March confirmed that suspicion by showing that the object he had observed moved among the stars. As a result, astronomers throughout Europe were informed of the discovery, and the mathematicians among them began to compute the new comet's orbit. Only several months later, after all those attempts had repeatedly failed to square with observation, did the astronomer Lexell suggest that the object observed by Herschel might be a planet. And only when additional computations, using a planet's rather than a comet's orbit, proved reconcilable with observation, was that suggestion generally accepted. At what point during 1781 do we want to say that the planet Uranus was discovered? And are we entirely and unequivocally clear that it was Herschel rather than Lexell who discovered it?

Or consider still more briefly the story of the discovery of x-rays, a story which opens on the day in 1895 when the physicist Roentgen interrupted a well-precedented investigation of cathode rays because he noticed that a barium platinocyanide screen far from his shielded apparatus glowed when the discharge was in process (*16*). Additional investigations—they required seven hectic weeks during which Roentgen rarely left the laboratory—indicated that the cause of the glow traveled in straight lines from the cathode ray tube, that the radiation cast shadows, that it 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 themselves but to a new form of radiation with at least some similarity to light. Once again the question suggests itself: When shall we say that x-rays were actually discovered? Not, in any 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. Nor, it is almost as clear, can the moment of discovery be pushed back to a point during the last week of investigation. By that time Roentgen was exploring the properties of the new radiation he had *already* discovered. We may have to settle for the remark that x-rays emerged in Würzburg between 8 November and 28 December 1895.

Awareness of Anomaly

The characteristics shared by these examples are, I think, common to all the episodes by which unanticipated novelties become subjects for scientific attention. I therefore conclude these brief remarks by discussing three such common characteristics, ones which may help to provide a framework for the further study of the extended episodes we customarily call "discoveries."

In the first place, notice that all three of our discoveries—oxygen, Uranus, and x-rays—began with the experimental or observational isolation of an anomaly, that is, with nature's failure to conform entirely to expectation. Notice, further, that the process by which that anomaly was educed displays simultaneously the apparently incompatible characteristics of the inevitable and the accidental. In the case of x-rays, the anomalous glow which provided Roentgen's first clue was clearly the result of an accidental disposition of his apparatus. But by 1895 cathode rays were a normal subject for research all over Europe; that research quite regularly juxtaposed cathode-ray tubes with sensitive screens and films; as a result, Roentgen's accident was almost certain to occur elsewhere, as in fact it had. Those remarks, however, should make Roentgen's case look very much like those of Herschel and Priestley. Herschel first observed his oversized and thus anomalous star in the course of a prolonged survey of the northern heavens. That survey was, except for the magnification provided by Herschel's instruments, precisely of the sort that had repeatedly been carried through before and that had occasionally resulted in prior observations of Uranus. And Priestley, too—when he isolated the gas that behaved almost but not quite like nitrous air and then almost but not quite like common air—was seeing something unintended and wrong in the outcome of a sort of experiment for which there was much European precedent and which had more than once before led to the production of the new gas.

These features suggest the existence of two normal requisites for the beginning of an episode of discovery. The first, which throughout this paper I have largely taken for granted, is the individual skill, wit, or genius to recognize that something has gone wrong in ways that may prove consequential. Not any and every scientist would have noted that no unrecorded star should be so large, that the screen ought not have glowed, that nitrous air should not have supported life. But that requisite presupposes another which is less frequently taken for granted. Whatever the level of genius available to observe them, anomalies do not emerge from the normal course of scientific research until both instruments and concepts have developed sufficiently to make their emergence likely and to make the anomaly which results recognizable as a violation of expectation (*17*). To say that an unexpected discovery begins only when something goes wrong is to say that it begins only when scientists know well both how their instruments and how nature should behave. What distinguished Priestley, who saw an anomaly, from Hales, who did not, is largely the considerable articulation of pneumatic techniques and expectations that had come into being during the four decades which separate their two isolations of oxygen (*18*). The very number of claimants indicates that after 1770 the discovery could not have been postponed for long.

Making the Anomaly Behave

The role of anomaly is the first of the characteristics shared by our three examples. A second can be considered more briefly, for it has provided the main theme for the body of my text. Though awareness of anomaly marks the beginning of a discovery, it marks only the beginning. What necessarily follows, if anything at all is to be discovered, is a more or less extended period during which the individual and often many members of his group struggle to make the anomaly lawlike. Invariably that period demands additional observation or experimentation as well as repeated cogitation. While it continues scientists repeatedly revise their expectations, usually their instrumental standards, and sometimes their most fundamental theories as well. In this sense discoveries have a proper internal history as well as prehistory and a posthistory. Furthermore, within the rather vaguely delimited interval of internal history, there is no single moment or day which the historian, however complete his data, can identify as the point at which the discovery was made. Often, when several individuals are involved, it is even impossible unequivocally to identify any one of them as the discoverer.

Adjustment, Adaptation, and Assimilation

Finally, turning to the third of these selected common characteristics, note briefly what happens as the period of discovery draws to a close. A full discussion of that question would require additional evidence and a separate paper, for I have had little to say about the aftermath of discovery in the body of my text. Nevertheless, the topic must not be entirely neglected, for it is in part a corollary of what has already been said.

Discoveries are often described as mere additions or increments to the growing stockpile of scientific knowledge, and that description has helped make the unit-discovery seem a significant measure of progress. I suggest, however, that it is fully appropriate only to those discoveries which, like the elements that filled missing places in the periodic table, were anticipated and sought in advance and which therefore demanded no adjustment, adaptation, and assimilation from the profession. Though the sorts of discoveries we have here been examining are undoubtedly additions to scientific knowledge, they are also something more. In a sense that I can now develop only in part, they also react back upon what has previously been known, providing a new view of some previously familiar objects and simultaneously changing the way in which even some traditional parts of science are practiced. Those in whose area of special competence the new phenomenon falls often see both the world and their work differently as they emerge from the extended struggle with anomaly which constitutes that phenomenon's discovery.

William Herschel, for example, when he increased by one the time-honored number of planetary bodies, taught astronomers to see new things when they looked at the familiar heavens even with instruments more traditional than his own. That change in the vision of astronomers must be a principal reason why, in the half century after the discovery of Uranus, 20 additional circumsolar bodies were added to the traditional seven (*19*). A similar transformation is even clearer in the aftermath of Roentgen's work. In the first place, established techniques for cathode ray research had to be changed, for scientists found they had failed to control a relevant variable. Those changes included both the redesign of old apparatus and revised ways of asking old questions. In addition, those scientists most concerned experienced the same transformation of vision that we have just noted in the aftermath of the discovery of Uranus. X-rays were the first new sort of radiation discovered since infrared and ultraviolet at the beginning of the century. But within less than a decade after Roentgen's work, four more were disclosed by the new scientific sensitivity (for example, to fogged photographic plates) and by some of the new instrumental techniques that had resulted from Roentgen's work and its assimilation (*20*).

Very often these transformations in the established techniques of scientific practice prove even more important than the incremental knowledge provided by the discovery itself. That could at least be argued in the cases of Uranus and of x-rays; in the case of my third example, oxygen, it is categorically clear. Like the work of Herschel and Roentgen, that of Priestley and Lavoisier taught scientists to view old situations in new ways. Therefore, as we might anticipate, oxygen was not the only new chemical species to be identified in the aftermath of their work. But, in the case of oxygen, the readjustments demanded by assimilation were so profound that they played an integral and essential role—though they were not by themselves the cause—in the gigantic upheaval of chemical theory and practice which has since been known as the "chemical revolution." I do not suggest that every unanticipated discovery has consequences for science so deep and so far-reaching as those which followed the discovery of oxygen. But I do suggest that every such discovery demands, from those most concerned, the sorts of readjustment that, when they are more obvious, we equate with scientific revolution. It is, I believe, just because they demand readjustments like these that the process of discovery is necessarily and inevitably one that shows structure and that therefore extends in time.

References and Notes

1. The larger revolution will be discussed in my forthcoming book, *The Structure of Scientific Revolutions,* to be published in the fall by the University of Chicago Press. The central ideas in this paper have been abstracted from that source, particularly from its third chapter, "Anomaly and the emergence of scientific discoveries."
2. For a brilliant discussion of these points see, R. K. Merton, "Priorities in scientific discovery: a chapter in the sociology of science," *Am. Sociol. Rev.* **22**, 635 (1957). Also very relevant, though it did not appear until this article had been prepared, is F. Reif, "The competitive world of the pure scientist," *Science* **134**, 1957 (1961).
3. Not all discoveries fall so neatly as the preceding into one or the other of my two classes. For example, Anderson's work on the positron was done in complete ignorance of Dirac's electron theory from which the new particle's existence had already been very nearly predicted. On the other hand, the immediately succeeding work by Blackett and Occhialini made full use of Dirac's theory and therefore exploited experiment more fully and constructed a more forceful case for the positron's existence than Anderson had been able to do. On this subject see N. R. Hanson, "Discovering the positron," *Brit. J. Phil. Sci.* **12**, 194 (1961); **12**, 299 (1962). Hanson suggests several of the points developed here. I am much indebted to Professor Hanson for a preprint of this material.
4. I have developed a less familiar example from the same viewpoint in "The caloric theory of adiabatic compression," *Isis* **49**, 132 (1958). A closely similar analysis of the emergence of a new theory is included in the early pages of my essay "Conservation of energy as an example of simultaneous discovery," in *Critical Problems in the History of Science*, M. Clagett, Ed. (Univ. of Wisconsin Press, Madison, 1959), pp. 321–356. Reference to these papers may add depth and detail to the following discussion.
5. The still classic discussion of the discovery of oxygen is A. N. Meldrum, *The Eighteenth Century Revolution in Science—The First Phase* (Calcutta, 1930), chap. 5. A more convenient and generally quite reliable discussion is included in J. B. Conant, *The Overthrow of the Phlogiston Theory: The Chemical Revolution of 1775–1789*, "Harvard Case Histories in Experimental Science, Case 2" (Harvard Univ. Press, Cambridge, 1950). A more recent and indispensable review, which includes an account of the development of the priority controversy, is M. Daumas, *Lavoisier, théoricien et expérimentateur* (Paris, 1955), chaps. 2 and 3. H. Guerlac has added much significant detail to our knowledge of the early relations between Priestley and Lavoisier in his "Joseph Priestley's first papers on gases and their reception in France," *J. Hist. Med.* **12**, 1 (1957), and in his very recent monograph, *Lavoisier—The Crucial Year* (Cornell Univ. Press, Ithaca, 1961). For Scheele see J. R. Partington, *A Short History of Chemistry* (London, ed. 2, 1951), pp. 104–109.
6. For the dating of Scheele's work, see A. E. Nordenskiöld, Carl Wilhelm Scheele, *Nachgelassene Briefe und Aufzeichnungen* (Stockholm, 1892).
7. U. Bocklund ["A lost letter from Scheele to Lavoisier," *Lychnos* (1957–58), pp. 39–62] argues that Scheele communicated his discovery of oxygen to Lavoisier in a letter of 30 Sept. 1774. Certainly the letter is important, and it clearly demonstrates that Scheele was ahead of both Priestley and Lavoisier at the time it was written. But I think the letter is not quite so candid as Bocklund supposes, and I fail to see how Lavoisier could have drawn the discovery of oxygen from it. Scheele describes a procedure for reconstituting common air, not for producing a new gas, and that, as we shall see, is almost the same information that Lavoisier received from Priestley at about the same time. In any case, there is no evidence that Lavoisier performed the sort of experiment that Scheele suggested.
8. P. Bayen, "Essai d'expériences chymiques, faites sur quelques précipités de mercure, dans la vue de découvrir leur nature, Seconde partie," *Observations sur la physique* (1774), vol. 3, pp. 280–295, particularly pp. 289–291.
9. J. B. Conant (see *5*, pp. 34–40).
10. A useful translation of the full text is available in Conant (see *5*). For this description of the gas see p. 23.
11. For simplicity I use the term *red precipitate* throughout. Actually, Bayen used the precipitate; Priestley used both the precipitate and the oxide produced by direct calcination of mercury; and Lavoisier used only the latter. The difference is not without importance, for it was not unequivocally clear to chemists that the two substances were identical.
12. There has been some doubt about Priestley's having influenced Lavoisier's thinking at this point, but, when the latter returned to experimenting with the gas in February 1776, he recorded in his notebooks that he had obtained "l'air dephlogistique de M. Priestley" [M. Daumas (see *5*, p. 36)].
13. J. R. Partington (see *5*, p. 91).
14. For the traditional elements in Lavoisier's interpretations of chemical reactions, see H. Metzger, *La philosophie de la matière chez Lavoisier* (Paris, 1935), and Daumas [see *5* (chap. 7)].
15. P. Doig, *A. Concise History of Astronomy* (Chapman, London, 1950), pp. 115–116.
16. L. W. Taylor, *Physics, the Pioneer Science* (Houghton, Mifflin, Boston, 1941), p. 790.
17. Though the point cannot be argued here, the conditions which make the emergence of anomaly likely and those which make anomaly recognizable are to a very great extent the same. That fact may help us understand the extraordinarily large amount of simultaneous discovery in the sciences.
18. A useful sketch of the development of pneumatic chemistry is included in Partington (*see 5*, chap. 6).
19. R. Wolf, *Geschichte der Astronomie* (Munich, 1877), pp. 513–515, 683–693. The prephotographic discoveries of the asteroids is often seen as an effect of the invention of Bode's law. But that law cannot be the full explanation and may not even have played a large part. Piazzi's discovery of Ceres, in 1801, was made in ignorance of the current speculation about a missing planet in the "hole" between Mars and Jupiter. Instead, like Herschel, Piazzi was engaged on a star survey. More important, Bode's law was old by 1800 (R. Wolf, *ibid.*, p. 683), but only one man before that date seems to have thought it worth while to look for another planet. Finally, Bode's law, by itself, could only suggest the utility of looking for additional planets; it did not tell astronomers where to look. Clearly, however, the drive to look for additional planets dates from Herschel's work on Uranus.
20. For α-, β-, and γ-radiation, discovery of which dates from 1896, see Taylor (*16*, pp. 800–804). For the fourth new form of radiation, N-rays, see D. J. S. Price, *Science Since Babylon* (Yale Univ. Press, New Haven, 1961), pp. 84–89. That N-rays were ultimately the source of a scientific scandal does not make them less revealing of the scientific community's state of mind.