As yet, only a minimum age can be assigned the Pliocene-Pleistocene boundary in these cores. From rates of accumulation of late-Pleistocene sediment determined by radiocarbon dating, and from the known thickness of Pleistocene sediment above the boundary, we estimate the age of the boundary to be not less than 800,000 years. Since the Pleistocene section above the boundary is incomplete because of the removal of some part by slumping, the real age of the boundary must be somewhat greater. To our knowledge, these cores provide the first evidence of the nature of the climatic change which opened the Pleistocene epoch and which, by creating an environment of rigorous selection, started a group of primates upon the evolutionary road that led to the emergence of man (*11*).

References and Notes

1. B. Kullenberg, *Svenska Hydrograf. Biol. Komm. Skrifter Ny Ser. Biol.* **1**, 2 (1947)
2. C. Emiliani, T. Mayeda, R. Selli, *Bull. Geol. Soc. Am.* **72**, 679 (1961).
3. R. F. Flint, *Glacial and Pleistocene Geology* (Wiley, New York, 1957), pp. 1–553.
4. G. Arrhenius, *Swedish Deep-Sea Expedition (1947–1948) Repts.* (1952), vol. 5, pt. 1, p. 199.
5. D. B. Ericson, M. Ewing, G. Wollin, B. C. Heezen, *Bull. Geol. Soc. Am.* **72**, 193 (1961).
6. M. N. Bramlette and W. R. Riedel, *J. Paleontol.* **28**, 385 (1954).
7. D. B. Ericson and G. Wollin, *Micropaleontology* **2**, 257 (1956).
8. O. L. Bandy, *J. Paleontol.* **34**, 671 (1960).
9. D. B. Ericson, *Science* **130**, 219 (1959).
10. D. B. Ericson, *Ann. N.Y. Acad. Sci.* **95**, 537 (1961).

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What Are We Looking For?

Attention to the nature of scientific discovery would produce better information retrieval systems.

Phyllis Allen Richmond

Currently there is a stampede into science information retrieval systems—that is, systems designed to recover factual data from the printed mass—and, to a lesser extent, into document retrieval systems—those designed to disinter a book, article, review, or report, per se, for someone who will then read it to retrieve its information content. There has been too much focusing of attention on ways and means, particularly lucrative ways and means, accompanied by a plethora of silly arguments and downright battles, all obscuring the need to answer the fundamental question: What are we looking for?

The general argument is that we are looking for scientific work that has already been done so that it will not be necessary to do it over again. If we are indeed looking for something like a method of synthesizing a compound, or if we want to know whether such a compound has been made, probably the science information retrieval systems available are as good or bad as we deserve. But suppose we are looking for a new field theory or a scientific approach that will open up new lines of research. Factual retrieval systems will not suffice. Again, what are we looking for?

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Scientific Method

At this point, it would be worth while to consider the nature of scientific method, scientific research, and scientific discovery.

Scientific method may be defined as a means of studying the universe and its contents which is characterized by a critical, systematic process of active investigation and reasoning, leading in general to publicly verifiable conclusions (*1*). The investigational approach may be either observational (direct) or experimental (indirect); it may be based on common-sense procedures or on procedures arising from the acquisition of specialized knowledge. The reasoning from the data produced by either approach may be influenced by hypothesis or theory and may result in generalizations, further theories, predictions, or even quantitative laws (*2*). The requirement for publicly verifiable conclusions eliminates those areas of human knowledge that are characterized by a high reliance on probability, and areas in which "observations" are deduced long after the event.

Scientific method is used in research. Research is defined as "the more or less systematic investigation of phenomena intended to add to the sum total of verifiable knowledge" (*3*). This broad definition excludes experimentation of the trial-and-error type, or pure chance observation. Some degree of planning is involved, certainly mental preparation, although experimentation is not necessarily indicated. Research may be of two main kinds: fundamental and applied. Fundamental research is research motivated by intellectual curiosity rather than by any attempt to solve a specific practical problem. Although fundamental research does undertake to solve particular problems, these are intellectual problems which must be solved in order to bring theoretical structure into line with experimental results, or to suggest new lines of development. Fundamental research ultimately may produce an original theory, or it may elaborate on, prove, or disprove existing theories. Applied research seeks a more limited objective—the production of some well-defined specific scientific occurrence or material. The end product is very often patented; it must be usable, practical, to be eligible for this protection.

The major result of scientific method and scientific research is scientific discovery (*4*); this may come at any time, at any place, and to any man, provided he has the genius to recognize it. A scientific discovery results from a combination of circumstances. The time is ripe—that is to say, a scientific breakthrough is due. The man is available, trained, and intelligent ("Chance favors the prepared mind"; a fool would not recognize opportunity if it bit him). Usually the time is characterized by simultaneous discoveries (examples are the discoveries of anesthesia and the calculus). There is a factor of luck involved, too. The pages of scientific journals are full of works by men who "almost" made a discovery. Robert Koch's teacher, Jakob Henle, foresaw very clearly how to prove the germ theory, but he could not prove it. The luck, of course, was in starting with anthrax; anyone who picked a virus disease failed.

To futher complicate the picture, there are many premature discoveries. Josiah Willard Gibbs's research on thermodynamics is a shining example. Rediscovery brought belated recognition of the real value of his work. Even more common are missed discoveries: the essential discovery was made but not recognized. Pasteur's attempts to get pure cultures, uncontaminated by antibiotics, are an example. He was so intent on establishing the germ theory of disease that he missed the significance of the germ killers. There are even accidental discoveries, such as Roentgen's discovery of the penetrating powers of x-rays. The scientist in such a case then goes to work to see if the "discovery" really is a discovery and so brings all his scientific training to bear on the problem.

A discovery may seem to be a flash of intuition or even of creation, given the favorable circumstances mentioned earlier, but more probably it stems from rigid self-discipline, intensive learning, and prolonged use of the scientific method in fundamental research. It is possible for embryonic field theory to result from applied research, as in the case of Michael Faraday, but this is less usual.

Retrieval

The method, the type of research, the discovery all have a bearing on what is recovered in information and document retrieval. Results of applied research are easier to retrieve than results of fundamental research because, as a rule, applied research is more limited in scope, more definite in procedure, and more clear-cut in exposition. Sometimes trying to describe the reasoning involved in fundamental research is like trying to nail currant jelly to the wall. In the light of subsequent information the ideas seem more clear, but contemporary readers of many expositions of theoretical work must have found them considerably less than lucid. The work on the developing germ theory is a classic case in point (*5*).

Hypotheses and theories are very difficult to classify and index unless they have become quantified laws or principles. It may be almost impossible to convey deductive and inductive reasoning in sufficiently abbreviated form. Yet the reasoning can be more significant than the observational or experimental data. Ultimately, one might say, it always is. It is, for example, rather common for scientists making an experiment to be unable to explain the results obtained. The experiment of Michelson and Morley showed the theory of the ether to be invalid, but Einstein must have been more interested in the explanations which occurred during the next 30 years than in the experiment which made them necessary. Document retrieval, as opposed to information retrieval, is a *sine qua non* for finding this type of information. The scientist must read the article as the author wrote it, with the argument intact.

Discovery is just as hard to pin down as hypothesis and theory. An accepted discovery is no great problem, but almost every accepted discovery has a history of many premature discoveries. The priority quarrels are usually extremely bitter, because there is rarely any doubt that someone other than the man who got the credit first had the idea. Often it is several men. Some, like Mendel, receive full recognition for their discoveries. Others, like Semmelweis and Gibbs, receive credit after they are dead. More often, the man who is able to *establish* the idea, who produces it at the right time, receives the approbation; the others are scientific curiosities, to be mentioned by the historian of science who resurrects them from limbo. There is little reason to believe that any system can perform retrieval of the kind the historian performs. That a premature discovery should be identified as a discovery as soon as possible, in the light of subsequent advances, and that this be publicized, is greatly to be desired, but how are the indexers, classifiers, and abstracters to know when to make such a search and what to look for? More realistically, it would be extremely helpful if, at the very first announcement of a discovery, the premature forerunners could be produced by a retrieval system. This could not be done with an information retrieval system. It is a remote possibility with a document retrieval system. On the other hand, in view of the multiplicity of interested scientists when the times are finally propitious for the acceptance of a discovery or a new idea, is it necessary to worry unduly about a discovery really getting lost? The humanists say that nothing worthwhile is ever lost. Does this not apply to science as well as to the humanities?

An information retrieval system can provide the answer to a request for information of a very specific and limited type, and this is certainly a legitimate and very necessary function in industry and sometimes in government, but an information retrieval system cannot, by its very nature, be a really satisfactory means of keeping up with scientific research. There is no substitute for reading the book, article, review, or report. Every article (including this one) has three meaning patterns: what the author said, what the author thought he said, and what the reader thought the author said. The last is by far the most important pattern of the three. No abstracter or indexer can convey adequately the thought sequence or all of the ideas in any scientific article that is more than a cut-and-dried laboratory report. Not all readers, by any chance, can understand what an author said. In the case of the premature discovery, only a much later rereading, in the light, probably, of events which had not occurred when the article was written, will convey the desired information.

Ends, Then Means

This all comes back to the original question: What are we looking for? We are looking for new scientific knowledge. We are looking for new scientific knowledge of all types—not just for knowledge about how to make something or about what has been produced to date. We are looking for scientific knowledge that does not necessarily fit the present ground rules, and especially for that which contradicts them. We are looking in particular for the kind of new scientific knowledge that will open new paths of research, both fundamental and applied. We are looking for new scientific knowledge that is in advance of its time and that may be obscured among the mass of current publications. We are looking for new ideas in science at all levels of scientific method, not just at the lower levels represented by observation and experimentation.

When a scientist is looking for scientific information of this kind, he must *look*. He should not be satisfied to have anyone else do his looking for him, beyond the point of indicating the source of the sort of information he requires. He may say that he does not have time. If he is involved in fundamental research he must find the time, even if it means working on only one thing at a time. For no one can do his looking for him. No one else has the background, the learning, the attitude of mind necessary for recognizing and grasping the meaning of the information when it comes along. The documentalist and the librarian must design systems to make it easier for the scientist to do his own looking. But they should never interpose themselves between the scientist and the written word. He must read the material himself.

All of the systems, both conventional (that is, library solutions) and nonconventional (documentation solutions), suffer from the weakness that too much attention is paid to means, too little to ends. Nine hundred and ninety-nine separate rules to "clarify" entry still do not make library books easy to find. Hardware belongs in a hardware store until we are intellectually capable of using it—and this has not happened yet. The specific problems to be solved in any kind of retrieval system are still the basic philosophical ones: What is the best way to organize knowledge? How can the system devised accept constant and unlimited changes in this knowledge? How do we show the overlapping, interrelated, multidimensional nature of modern knowledge? Solutions to these problems are vital to successful dissemination of scientific information, particularly of the type necessary for further major advances. In the quest for such solutions, let us, above all, keep in mind what we are looking for—and then make it easier to find.

References and Notes

1. This definition of scientific method is used as a matter of convenience. For an interesting discussion of some of the problems to be faced in defining scientific method, see M. Black, in *Science and Civilization*, R. C. Stauffer, Ed. (Univ. of Wisconsin Press, Madison, 1949), pp. 67–95.
2. M. Black, *Critical Thinking* (Prentice-Hall, New York, 1947), pp. 304–363.
3. R. H. Shryock, *American Medical Research: Past and Present* (Commonwealth Fund, New York, 1947), p. 1.
4. G. Lockemann, *J. Chem. Educ.* **36**, 220 (1959).
5. P. Allen, *Americans and the Germ Theory of Disease* (University Microfilms, Ann Arbor, Mich., 1949).

News and Comment

Test Ban: Prospects for Agreement May Be Dim, But in Cuban Aftermath They Appear Brighter

It has been said that anyone who was not thoroughly frightened by the Cuban missile episode just simply did not know the facts. The facts, of course, are best known to President Kennedy and Premier Khrushchev, and, while they fortunately are among the most steely nerved of the species, it is evident that their "eyeball to eyeball" confrontation has stimulated some serious thoughts about defusing the Cold War. These thoughts, it is clear, were not lacking before, but just as nothing promotes fire safety like a charred hospital, the Cuban affair has provided the incentive for reopening seemingly meaningful talks on what has come to be considered the first step toward arms control—the nuclear test ban.

The foundation for these talks was laid in the Kennedy-Khrushchev correspondence that brought the missile crisis to an end. Khrushchev wrote that it was urgent to think about disarmament issues beyond Cuba, Kennedy responded that he agreed, and shortly thereafter, preparations began for the resumption of the test ban talks, which had recessed in deadlock last fall.

The talks, which got under way 14 January in New York, shifted to Geneva last week, amid reports that the prospects range from dismal to promising. Such reports have been par for the course during the 5-year history of test-ban negotiations, and, since the former appraisal has turned out to be the case, optimists are to be regarded with skepticism. There are, however, substantial indications that things are now moving along, and however dim the prospects may be, it appears that they are a lot brighter than ever before. The evidence lies not only in relaxations of both the Soviet and American positions, but also in Republican rumblings and the beginnings of an administration effort to cultivate public opinion in anticipation of a possible domestic row over the wisdom of a test ban.

Briefly, this is where the negotiations now stand: The Soviet Union, returning to a position that it briefly held and later abandoned in 1958, accepts the principle of on-site inspection and is now willing to permit two to three inspections annually. It would also permit the installation of three unmanned seismic stations, so-called black boxes, on Soviet soil, and it would admit foreign personnel to service the instruments.

The United States, on the basis of what are said to be markedly improved seismic detection techniques, has abandoned its insistence on foreign-manned seismic stations on Soviet soil. And the U.S. insistence on on-site inspection has receded from a demand for 20, in 1960, to 12 to 20, in 1961, and, now, to 8 to 10. The U.S. also wants at least 7 "black boxes" on Soviet soil, but it has made it appear that it has not arrived at a bedrock position on any numbers.

Measured in terms of the history of the test-ban negotiations, the Soviet re-