study goes on to consider some of the factors mediating between size and performance. One of these is the research experience of the research leader: highly experienced scientists profit more from large research groups than do the less experienced (at least in Sweden). Such findings certainly have important implications for science policy. Stankiewicz notes that another potentially mediating variable is the research field in which a group works. In my own work of a few years ago limited to chemistry (and to the United Kingdom) we found that the benefits of large research groups were highly dependent upon the area within chemistry in which the group worked and went on to argue that this had to be explained in terms of the kind of research typical within each. Differences of this kind are likely to be much greater when a wide range of disciplines is included, as here. This aspect of the problem, though noted by Stankiewicz, is not developed. The fact that it is not leads me to my second line of criticism.

Most of the contributions explicitly or implicitly adopt a perspective derived in some way from organizational theory and tend to look at research groups as more or less isolated formal organizations. The sociology of science, also concerned with the production (and validation) of scientific knowledge, some years ago discarded an earlier concentration on the "work group" as its major focus of interest. The rather different ties binding the scientist into the community of his or her peers seemed to be of greater importance. It is noteworthy, then, that Kowalewska, faced with her conclusion that patterns of influence within research units seemed of little importance, ruefully admits, "It may be that the functioning hierarchies that matter for R & D are not primarily defined in terms of roles within a single organization." The point is that in this study the whole notion of scientific community has been sacrificed to the hope of "scientific management" of the process of knowledge production. But work within the sociology of science has already suggested that organizational factors, resources, and division of labor (the parameters with which managers can operate) actually vary in their effects from one specialty to another. The problem then is to make sense of this in terms of the cognitive structures of the sciences. Though this line of sociological study as yet lacks immediate applicability it does suggest that policies and managerial practices to be adopted in furthering research in one specialty will not necessarily be the same as those appropriate to another and that the crucial factor is the set of "objects" and theories with which a specialty is concerned. Admittedly all this relates to the basic sciences. The idea of scientific management certainly has more validity in the case of the applied and technological sciences. If this study had been restricted to those it might have been more successful.

The sociology of science has also, simultaneously, developed along another track. Many studies have shown how social, political, and economic factors within a society influence the working of its scientific institutions. This social context of research has also been lost in this study—a great pity, because there is a great deal to be done in the comparative analysis of scientific communities. It is sincerely to be hoped that sociologists will return to this collection of data with the intention of trying to understand the workings of science rather than, unrealistically, trying to formulate principles of scientific management of universal applicability. The particular countries studied would provide a fascinating comparison from this point of view. I personally would be delighted if some of the contributors to the present volume, who know the data, who know the countries, and to whom my remarks on the sociology of science will be no revelation, would set about producing a second volume. It could be a major contribution to sociology of science.

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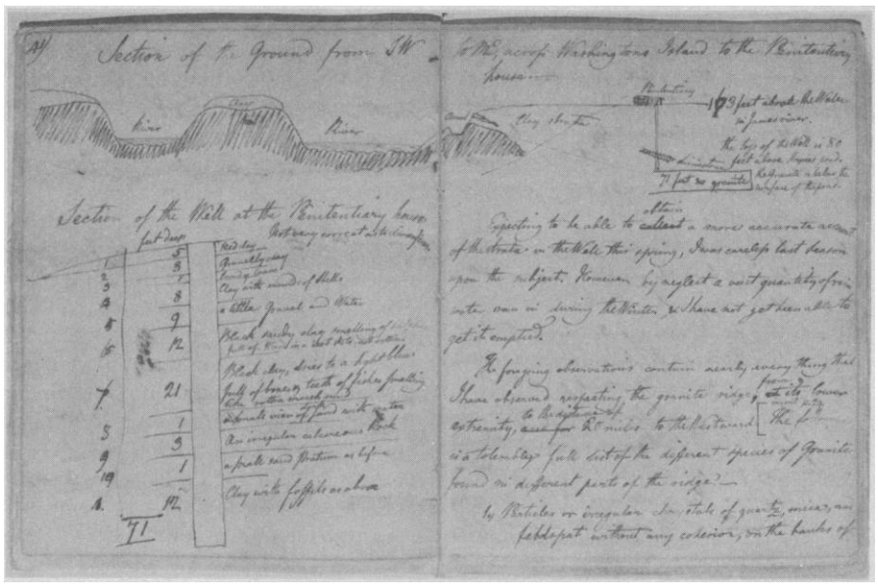
Geology in Retrospect

Two Hundred Years of Geology in America. Proceedings of a conference, Durham, N.H., Oct. 1976. CECIL J. SCHNEER, Ed. Published for the University of New Hampshire by the University Press of New England, Hanover, N.H., 1979. xiv, 386 pp., illus. $20.

You may dimly recall from your high school lessons in Western civilization that the Whigs were an 18th-century political party in England who championed the cause of popular rights and the democratization of government. The tendency of overzealous Whig historians to reconstruct pre-Whig history in ways most flattering to their reform movement has given rise to an expression sometimes used among historians of science today. To be charged with "whiggery" implies that one has made the mistake of evaluating past scientific ideas on the basis of their resemblance to current scientific thought. Could, for example, the 17th- and 18th-century natural philosophers who refined the concept of the Scala Naturae be considered pre-Darwinian evolutionists? Prior to the publication of A. O. Lovejoy's *The Great Chain of Being* (1936) some biologists who dabbled in the history of science believed so. Taken within the context of its own time, however, the Scala Naturae may be interpreted for what it actually was: an elaboration on religious doctrine immune from the question of speciation. Does the actualist approach to historical inquiry demand anything more than a high standard of scholarship? What is unpardonable in seeking out the true roots of a scientific idea? The conflict between the whig and actualist viewpoints is but one theme that threads its way through the present collection of papers.

Two Hundred Years of Geology in America is a welcome addition to the earlier collection edited by Schneer, *Toward a History of Geology* (M.I.T. Press, 1969). The authors of the 27 papers that make up the book are a mixed group of 32 geologists and historians who met at the University of New Hampshire in 1976 to celebrate both the nation's bicentennial and the long profession of geology in America. Unlike the proceedings of most symposia, the papers were not read at the meetings but were circulated among the conferees ahead of time in order to provide additional time for structured discussion. It is not clear to what extent, if any, the various authors subsequently revised their contributions, but the excitement of dialogue does manage to come across in a surprising number of papers.

In his paper "Geology in 1776," historian Kenneth L. Taylor forces the whig-versus-actualist issue by demonstrating the incipient nature of the science at the time of the American Revolution (the invention of the word "geology" by a European was still two years away). Paleontologist Stephen J. Gould presents an intriguing twist on whiggery in his study "Agassiz' later, private thoughts on evolution," based on the marginalia found in Louis Agassiz's own copy of Haeckel's *Natürliche Schöpfungsgeschichte* (1868). The detailed commentary scattered through the entire 568 pages of Haeckel's book suggests that Agassiz did not retreat blindly from "rational science" in his later years, as is often charged by 20th-century historians. Three significant papers in the collection relate to the topics of continental drift and plate tectonics, and



Two pages, dated 4 May 1798, from the journals of Benjamin Henry Latrobe. Latrobe "directed the construction of the United States Capitol and the White House, built the first comprehensive steam-powered water system at Philadelphia, and participated in numerous other architectural and engineering projects. He was in addition a keen scientific observer, and one of the things that excited his interest in his new country was its geology. . . . In many instances his geological knowledge was vital to the successful execution of his work." These pages show (top) "a section from south (left) to north (right) of Richmond, Virginia, and the adjacent James River. The Penitentiary (of Latrobe's design) and its well are on the right, and the strata of the well are sketched on the lower left-hand page." [Reproduced in *Two Hundred Years of Geology in America* from the papers of Benjamin Henry Latrobe, Maryland Historical Society, Baltimore. Courtesy of the Papers of Benjamin Henry Latrobe]

bear traces of the whig-versus-actualist theme. These include "Very like a spear" by oceanographer (and director of the U.S. Geological Survey) H. William Menard, historian Henry Frankel's "Why drift theory was accepted with the confirmation of Harry Hess's concept of sea floor spreading," and geologist Robert H. Dott, Jr.'s "The geosyncline—first major geological concept 'made in America.'"

Dott's excellent paper is concerned primarily with the historical development of the geosynclinal theory by two giants in American geology: James Hall and James D. Dana. After tracing this history through the latter half of the 19th century, Dott finishes by showing why American geologists were so slow to pick up the concept of continental drift early in this century. Thus the question for a scholarly whig geologist is not whether Sir Francis Bacon envisioned plate tectonics in his *Novum Organum* (1620) but how the independence of geosynclinal theory in America retarded the introduction of continental drift theory. As an aside, how many geologists know that F. B. Taylor wrote an article on continental drift as a mountain-building mechanism that appeared in the bulletin of the Geological Society of America five years before Alfred Wegener published his *Die Entstehung der Kontinente und Ozeane* (1915)? Menard's graceful contribution is truly an "oral history" regarding the birth of the plate tectonics model during the late 1950's and early 1960's. It is an example of the pure actualist viewpoint, an eyewitness account of history in the making. Both the papers by Dott and Menard should be required reading for serious geology students.

The education of young geologists is not complete without some exposure to the history of geology. A traditional approach involves the use of readings taken straight from their original sources, such as *A Source Book in Geology* compiled by Mather and Mason 40 years ago, or Cloud's more recent *Adventures in Earth History* (1970). Well-planned symposium volumes such as *Two Hundred Years of Geology in America* move beyond this tradition to provide a source of stimulation for students, professional geologists, and historians alike. Some failings are inherent to the genre, and Schneer's new volume is not exempt. A few of the papers included clearly do not belong within the 1776–1976 time frame, and coverage of the "heroic" age of American geology is incomplete (no examination of the roles of Amos Eaton and William Maclure). What Rudwick concluded in *The Meaning of Fossils* (1972) about the history of paleontology applies equally well to its parent discipline of geology: "That it should not become a-historical in outlook is important, not for nostalgic antiquarian reasons, but because the loss of historical perspective would lead to conceptual impoverishment." *Two Hundred Years of Geology in America* offers much to all its readers.

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Metallurgy in Antiquity

The Search for Ancient Tin. Papers from a seminar, Washington, D.C., March 1977. ALAN D. FRANKLIN, JACQUELINE S. OLIN, and THEODORE A. WERTIME, Eds. Smithsonian Institution, Washington, D.C., 1978 (available from the Superintendent of Documents, Washington, D.C.). viii, 64 pp., illus. Paper, $5.

There is little doubt that the sources of tin used in antiquity is an intriguing topic. In his introduction to this volume of nine symposium papers on the subject, Theodore A. Wertime speaks of each of the symposiasts as detectives involved in solving a mystery. Though the detective story told in this volume is one that has no swift denouement, tin is not quite as elusive as it used to be. Since the publication of J. D. Muhly's comprehensive *Copper and Tin* (1973) with its supplement (1976), new clues have been revealed. It is in the summary presentation of this newest information that the significance of this volume lies.

The search for geological loci of tin, in stream beds, granite masses, or the gossan caps of ore bodies, has been narrowed somewhat by recent research. J. A. Charles and George Rapp provide valuable descriptions of the main geological environments, primarily granite-associated, in which tin is to be found. These descriptions substantiate Rapp's point that the answers to many questions pertaining to ancient tin are to be found in geological and archeometallurgical field investigations.

Prentiss de Jesus's paper provides maps of the tin deposits situated in the major tin belts of the world. Most of the sources discussed by the contributors lie within the boundaries of these tin belts, in particular those in the Near East and South Asia. Perhaps the most significant new find is in the eastern desert of Egypt, as reported by Wertime. In sever-