BOOK REVIEWS

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CRACKING ROCKS AND DEFENDING DEMOCRACY—KIRTLEY FLETCHER MATHER: SCIENTIST, TEACHER, SOCIAL ACTIVIST, 1888–1978. *Kennard Baker Bork. 1994. Pacific Division AAAS, San Francisco. vi + 336 pp. Hardcover, $31.95.*

When one ponders works as diverse as, *The fauna of the Morrow Group of Arkansas and Oklahoma, Christian Fundamentals in Light of Modern Science, Science in Search of God,* and *The Earth Beneath Us,* one might get the impression that they represent the writings of different people involved in different occupations. But they represent a fraction of the diversity and complexity of Kirtley Fletcher Mather, Professor of Geology at Harvard, 1924–1954; University of Arkansas, 1911–1914; and Dennison University 1921–1924 (Class of '09). In addition, his career included graduate studies under Salisbury and Chamberlin at the University of Chicago; serving as a defense witness at the 1925 Scopes Trial in Tennessee; and an interview of Marshall Tito in 1949, which provided some fuel for the "fires" of Senator McCarthy and the "House Committee on Un-American Activities"—better known as the witch hunts of the 1950's. These are but some of the highlights of the long and illustrious life of K. F. Mather which Dr. Bork has presented with marvelous insight and care. Working with the permission and assistance of Mather's surviving family, Bork has woven a tale that seems more fiction than fact. Fiction that included such diversity would not seem believable.

The bibliography is essentially chronological in its arrangement, but there are overlaps between chapters which keeps it from being too linear. The first portion (Chapters 1–7) develops Mather's early education and his career as a teacher and a geologist. Next is a section (Chapters 8–10) which concentrates on his work as a writer and his role in public education, while chapters 11–14 describe Mather's conflicts in the political arena. The following three chapters (15–17) cover the pinnacle of his professional achievement—presiding over the AAAS and the American Academy of Arts and Sciences; his attempts to integrate science and religion and society, and finally his retirement years. There is a small section of photographs between pages 116 and 117, and I wish there were more of these. To have so few photographs to support such a vibrant life seems a little cruel to the reader. The text is relatively free of typographical errors, but for page headers of chapter 13 and an extra letter in Alaskan in a photograph caption; none of which detract from the quality of the book.

Not only did Mather team up with Clarence Darrow in defense of young John Scopes (although the judge ruled Mather could only give a deposition and he was not allowed to testify in court), but also two years later Mather was again championing the evolution cause in a debate with the Rev. Dr. John Roach Straton (N.Y. Calvary Baptist Church) in May of 1927. There are two bits of irony in this. One lies in the fact that Mather was a very devout Baptist himself, and secondly that Straton's son, Douglas, who was 11 at the time of the debates, was so impressed with Mather during these debates that when he attended Harvard 6 or so years later, he specifically requested his financial aid work be done in the office of Dr. Mather. In the 1960's, the then-Dr. Douglas Straton, Professor of Religion and Dean of the Chapel, extended an invitation to Mather to speak at Colorado College. Such long-term friendships with people was one of Mather's strengths. And before leaving the subject of the Scopes trial, did you ever wonder why little was heard of the affair in the legal sense after Scopes was fined $100? The answer is on page 73; I shall not spoil it for the reader.

There are few people in the field of geology, or other fields for that matter, who have never heard of Kirtley Mather's work—a published output of a dozen books, 250 articles, and over 1000 book reviews. But until this biography, few knew much of Kirtley Mather the person. Bork has filled this void with his well prepared work. Mather was a person who had a greater influence on our world through his students and through his attempts at public education than through his geological work. He, obviously, was a powerful member of the academy who, long before many of his scientific colleagues, felt that as a scientist he had both a right and a duty to speak out on issues. The final sentence of the epilogue provides a succinct summary of the man, "Whether or not we agree with his political views, his belief in the mutual relevance of science and religion, or his commitment to non-specialized knowledge, the integrated life of Kirtley Fletcher Mather epitomizes the spirit of the socially concerned scientist." (p. 282)

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SCIENCE AND NATURE: ESSAYS IN THE HISTORY OF THE ENVIRONMENTAL SCIENCES. *Michael Shortland, ed. 1993. Alden Press, Oxford. [ISBN 0-906450-08-x, viii +] 291 pp. Softcover, £10.00 [US $19] (Available postfree from The British Society for the History of Science, 31 High Street, Stanford in the Vale, Farringdon, Oxon UK SN7 8LH)*

In recent years the study of the environmental sciences has become an important dimension of the history of science. Previously ignored because of traditional emphases on great theoretical revolutions, the field has taken on new significance in light of changing perspectives in the history of science and increasing concern over the impact of science and technology on the environment. The essays in this timely, well-conceived volume offer insightful analyses of specific historical developments in the environmental sciences and highlight important historiographical features of contemporary history of science.

Ecology constitutes an important component of the environmental sciences, and several essays examine the conceptual and institutional factors that affected the history of that field. The emergence of systems ecology, according to Chunglin Kwa, is best understood as a social movement. The discipline developed in the 1950's through programs in radiation ecology sponsored by the Atomic Energy Commission. Kwa focuses on Eugene Odum and Stanley Auerbach, ecologists at the University of Georgia and the Oak Ridge National Laboratories, whose distinct approaches and styles led to programs of fundamental research in an applied context. Odum and Auerbach, however, did more than secure a niche for systems ecology. Building on postwar faith in science and technology and employing information systems metaphors, those men and their associates placed the management of natural resources on a rational foundation, and by the 1960's systems ecology was flourishing.

Other efforts were not equally fruitful. Sharon Kingsland's essay describes the difficulties encountered at the Carnegie Institution's Desert Laboratory. For twenty years the laboratory's founder, Daniel T. MacDougal, sustained a program that emphasized basic biological research, but by the 1930's personal, intellectual, and especially utilitarian factors had resulted in the institution's demise. For Kingsland the story of the desert laboratory illustrates a larger problem: the fact that ecology is an eclectic field that includes fundamental research as well as practical programs. Ecologists have advanced conflicting strategies for identifying and legitimizing their activities, and the failure of the Desert Laboratory reflects the problems the field has had maintaining a competitive posture in science. Another endeavor that did not succeed was human ecology, the study of human groups and their environments that attracted scholars from geography, economics, and sociology. As Eugene Cittadino indicates, the diversity of interests, approaches, and methods, coupled with the skepticism of plant and animal ecologists, prevented the development of a unified discipline of human ecology. These essays, besides indicating the complex factors that shaped the development of ecology in different times and places, illustrate that ecology did not derive legitimacy from a preservationist ethos or applied practical interests.

Other essays address the relationship between ecology and environmentalism. John Sheail's study of pollution and fisheries in Britain in the 1920's indicates that comprehensive regulatory efforts began only when several different constituencies, including scientists and fisheries interest groups in addition to civil servants, took an interest in the problem. Sheail's paper concentrates on the series of changes that eventually led politicians to recognize the need for communities of expertise. Other essays offer penetrating analyses of the conceptual, economic, and politic factors that affected particular environmental endeavors. Stephen Bocking points out that the establishment of the Nature Conservancy derived from more than just political or conceptual developments. While A. G. Tansley and Charles Elton identified new approaches in ecology that called for new techniques, long term studies, and freedom from practical applications, it was only in the 1940's, when a nature reserve movement, government interest in land use planning, and increased respect for science converged, that an independent environmental organization was established. Libby Robin's examination of the Little Desert dispute in Australia defines a convergence of economists, agriculturalists, and public interests that thwarted a government development program. But conservation meant different things to different people, and while the Little Desert issue launched the Australian environmental movement, it was also a temporary alliance of varying constituencies that later broke apart over conflicting views and agendas. Pallo Palladino's examination of competing pest control programs in the United States in the 1960's and 70's likewise defines environmentalism as a complex of political, economic and scientific objectives. Palladino distinguishes the Department of Agriculture, which emphasized pest eradication, from state agriculturalists, an impoverished group that joined with environmental interests to promote a program of "integrated control." Blocked by powerful government interests in the 1960's, that effort eventually captured the attention of a Nixon administration eager to meet the nation's environmental concerns. The confluence of ideological, economic, and political factors thus influenced the development of a particular scientific research program. Similarly, as Timothy Boon's essay illustrates, the film, "The Smoke Menace," was a product of complex social relations among scientists, the gas industry, and documentary filmmakers, each with their own agendas. These essays do not define environmentalism as a strictly political movement on the one hand, or a direct outgrowth of ecology on the other. Rather each historical development constitutes a contingent artifact of social, political, and scientific events in specific times and places.

The papers in this volume are noteworthy in several respects. In his introductory essay, Peter Bowler describes the value of research in the history of environmental science, particularly given its previous marginalization, but cautions historians and environmentalists against commitment to political agendas that can yield stereotypical views and oversimplified interpretations. These essays do much more than heed Bowler's warning; they go far toward setting a standard for future research. Through careful analyses of particular events, texts, and programs, they illustrate the range of nuanced, complex, and interacting factors that have shaped the history of ecology as well as the relationship between ecology and the environmental movement. The volume focuses on issues pertaining to the agricultural and biological aspects of the environmental sciences; no papers address the problems in the earth sciences which, as Bowler notes, also fall within that framework. Nevertheless, the scope, scholarship, and attention to historiographical issues make the essays in this volume an important contribution to the history of science.

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RICHARD OWEN COMMEMORATION. THREE STUDIES. *Jacob W. Gruber and John C. Thackray. 1992. Natural History Museum Publications, London. [8] + 181 pp. £29.95.*

Richard Owen, anatomist, vertebrate palaeontologist and astute politician, was a towering figure in Victorian times; yet, from historians of science, his name evokes equivocal responses.

Beyond doubt, Owen was an outstanding scientist. The sesquicentennial of his naming of the dinosaurs has recently been celebrated with due pomp—albeit, as Hugh Torrens has striven so hard and so unavailingly to drum into wilfully deaf ears, one year prematurely. Certainly it marked an epoch in our understanding of these vanished creatures and, even if the reconstructions made by Waterhouse Hawkins under Owen's supervision are now known to be absurdly wrong, at least they gave their awed Victorian viewers a concept of the size of these long-gone monsters. Owen deserves great credit also as prime mover in the creation of the British Museum (Natural History), an institution long to be renowned in research (though now sadly shortened in name, as fitting mark of its sad decline into a children's educational entertainment centre). It was Owen who secured for that Museum the first good *Archaeopteryx,* describing it fully and accurately—acts greatly to the credit of so staunch an opponent of Darwinian evolution, for which theory this linking form between reptiles and birds has proved so solid a plank. Owen's other attainments were numerous; the list of his publications, furnished by his grandson, the Rev. Richard Owen, in a properly pious two-volume *Life* (1857–1858), runs to almost thirty closely-printed pages and spans very many themes, scientific, political, and religious.

Why, then, does posterity view Owen with such tempered enthusiasm? Is it simply because he was a leading and sometimes unreasonable opponent of a theory now endorsed (in essence, at least) by most scientists? Is it because anyone so much revered in his own time is necessarily a target for the iconoclasts of later times?

No, those are not the answers. The truth is that there is too much about Owen that is unappealing. In his time he was a scientific dictator, with an authority which, in his own fields, almost rivalled that of Sir Roderick Murchison in wider scientific domains—and was just as likely to be employed unmercifully. The respect offered Owen and his contemporaries was always uneasy. Owen's praise could exalt their achievements, but it was rarely given. In contrast, his cold criticisms were often expressed and could be devastating—and woe betide anyone who, in any least way, seemed to Owen a possible rival! Beyond doubt, he was extremely egocentric and arrogant. He delighted to be called "the English Cuvier," revelling in the honours offered him by scientific organizations at home and abroad—his respectful biographer's list of them runs to four pages of fine print—and, in particular, in the Royal approval that led to the granting of a knighthood and a house in Richmond Park.

As consequence of this equivocal viewing by posterity of Owen, the centenary of his death has not been so widely celebrated as might be expected of a scientist of such magnitude. The volume here reviewed is the Museum's tribute to him—and it is rather a guarded tribute. It features a catalogue of the Museum's Owen Collection, comprising correspondence, manuscripts and drawings by William Clift, Sir Everard Home and Owen himself; this was prepared by John Thackray. To this is prefaced a brief introductory essay and a more lengthy analysis of Owen's relations with his correspondents, both written by Jacob W. Gruber. Though striving in general to be sympathetic, Dr. Gruber tells us clearly why Owen's achievements are nowadays viewed so ambivalently. For example, let us read p. 66:

> Although Owen was very sensitive to possible infringements on his own rights of priority, he was not always so careful with those of others. Whether the product of a busy professional life or, as some of his colleagues charged, a conscious decision not to give credit where credit was due, he was charged with ignoring the work of others, or, what was much more serious, [with] outright plagiarism. At the lesser end of the scale was his occasional omission of appropriate credit for work which others had done.

Having been furnished with good instances, let us move on to p. 67:

Owen found himself party to such conflicts more often, it seems, than his colleagues. Moreover, his behaviour in such matters, even when he was at the peak of his reputation, is difficult to understand. He was quick to take umbrage at what he perceived as an intrusion of his "scientific" space and even, on occasion, at what he perceived as the theft of his intellectual property. His behaviour on such occasions was in sharp contrast to the aid that many of his colleagues, whether professional or amateur, received from him. His conflict with Mantell during the last years of Mantell's life was an embarrassment to those who were friends of both. It cost him the presidency of the Geological Society which his palaeontological work had earned for him. Of lesser consequence was a mutual charge of plagiarism arising out of what may well have been the simultaneous discovery of a late stage in the development of the tooth by both Owen and Alexander Nasmyth, a young dental anatomist. . . . Owen's friends were at a loss either to make a judgment as to the merits of the case or to understand Owen's virulent reaction to any attempt to resolve the difficulty quietly, with as much dignity and as little embarrassment as possible.

No, Sir Richard Owen is not an appealing figure, at least when viewed through contemporary spectacles; yet of his immense importance to the history of science, there can be no question. For John Thackray's meticulous documentation of an important scientific archive and Jacob Gruber's erudite assessment of Owen's entertainments, scientific historians own them a considerable debt. Whether one loved him or hated him, in his time and afterward, Owen was beyond doubt a great man.

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REEVALUATION OF PALLAS' THEORY OF THE EARTH (1778). *Albert V. Carozzi and Marguerite Carozzi. 1994. Reprint of Archives des Sciences, vol. 44. Genève: Société de Physique et d'Histoire naturelle, [1991].*

Pierre Simon Pallas (1747–1811) traveled extensively through Russia in the early 1770's, publishing a five-volume travel account (1771–1776) that was widely translated from the original German. In 1777 he presented a theory of the earth to the Academy of Sciences at St. Petersburg, with the King of Sweden in attendance. This theory was published by the Academy as "Observations sur la formation des montagnes et les changemens arrivés au Globe, particulièrement à l'égard de l'Empire Russe." An alternate draft, written in German, was published at Frankfurt and Leipzig the following year.

The German version, slightly longer than the French, is translated for the first time by the Carozzis in the present volume, accompanied by translated excerpts from H. B. Saussure's unpublished notes on Pallas' *Travels* and theory of the earth. Regrettably, the German text of Pallas' theory is not reprinted. The Carozzis include translations of footnotes to the German text (many of which differ significantly from the French version), and supply copious annotations of their own, including a diagrammatic representation of the successive stages envisioned by Pallas' theory. Companion essays discuss eighteenth-century terminology, the modern geology of the Ural Mountains, and previous historiographical assessments of Pallas' work.

Pallas' theory of the earth illustrates the inadequacy of the once-common Neptunist/Plutonist classification of late eighteenth-century theories of the earth. He insisted that one should combine diluvial, volcanic, and other causes to account for the earth's history, and "not refer only to a single one" (p. 30). Such a "big picture" provided a framework for geological theorizing in which field observations, especially from underexplored areas such as Russia, were critical. Indeed, Pallas began with a relatively lengthy description of the mountains and steppes of Asia, interspersed with accounts of their inferred changes over time. More recently, Pallas suggested, great underground fires and eruptions beneath the Indian Ocean had produced a northward-rushing flood that sculpted various features of the Asian continent while depositing heaps of animal remains (such as elephant ivory or a frozen rhinoceros carcass) in Siberia.

Pallas' theory of the earth is typically cited for its schematized description of the structure of mountains, according to which major mountain ranges consist of (1) a central granite core, surrounded on either side by zones of (2) primitive schistose rocks, then (3) secondary limestones, and finally (4) tertiary shales and sandstones.

On a map of the Urals published as part of his *Travels* in 1773, Pallas marked various rock types and mineral sites, based on his field observations, which special punctual symbols. The Carozzis transpose these mineralogical and structural symbols into band markings, thus facilitating a comparison of Pallas' work with a modern geological map. The result is a demonstration that, in his descriptive work represented by the *Travels,* Pallas "achieved a remarkable understanding of the geology of the area" (p. 95).

The Carozzis go further, however, and argue that the observations of the *Travels* appear inconsistent with the schema Pallas proposed in his theory of the earth. According to Pallas' observations:

(1) Feldspathic quartzites, not granite, comprise the axial zone (in addition, the axial zone is discontinuous).
(2) Primitive schistose zones on either side of the central core are not equivalent. The western bands are non-metamorphic shales; the eastern bands are highly metamorphic and mineralized schists.
(3) Limestones on the west are extensive, including horizontal and highly inclined zones, but the eastern limestones are poorly developed.

The Carozzis therefore conclude that "contrary to his statement in his theory of mountain chains in general, the various bands of rocks in the Urals are asymmetrical and granite does not exist at the center of the chain" (p. 95).

How should we explain this discrepancy between Pallas' descriptive work and his theory of the earth? The Carozzis suggest that Pallas' contribution lies in his precise observations, not his theory of the earth. The latter was prepared in haste, and because of the presence of the King of Sweden "he sacrificed his personal accurate observations for political reasons to please his benefactor, the Empress of Russia" (p. 3).

A crucial historiographical problem arises with the translation of eighteenth-century terminology, which the Carozzis explore in a separate chapter. As corroboration for their political explanation, the Carozzis point to Pallas' adoption of the Swedish mineralogist Wallerius' terminology for granitic rocks. When writing of the Urals, Pallas described the central rocks as highly inclined beds of *Vitrescirendes Gebürge und Quartz* (vitreous rocks and quartz), distinguishing them from the granite comprising the tops of other mountain ranges. However, in *Systema Mineralogicum* (1772), Wallerius included feldspathic quartzite as a species of granite, and in his theory of the earth Pallas employed granite in this wider sense.

In a similar case, the Carozzis note that Pallas referred to both shales and schists as *Schiefer,* although he recognized the differences between them, as indicated above, on either side of the axial core. The Carozzis chose to translate *Schiefer* as either shales or schists "according to the modern geological context of the Urals" (p. 15). While a certain amount of confusion is thereby avoided for the modern reader intent on visualizing the "actual" geology of the Urals, something of the historical sense of *Schiefer* as a unifying term, encompassing both shales and schists, is lost. Seen in this light, the inconsistencies urged by the Carozzis between Pallas' observations and his theory of the earth are diminished, arising at least in part as an artifact of translation. While political expediency may well have played a role in Pallas' formulation of his theory of the earth, we should be careful not to obscure the eighteenth-century search for regularities embodied in theories of the earth and expressed in terminology that later became more precise and restricted in scope (although even Saussure at times found Pallas' nonspecific use of *Schiefer* confusing).

In the hands of careful science historians such as the Carozzis, the translation of archaic terminology into more precise modern "equivalents," like the transformation of Pallas' map into a modern format, manifests certain characteristics (such as the reliability of Pallas' fieldwork) that otherwise might be overlooked by historians of science with less training in the geosciences. In a complementary fashion, historians of science with a more humanistic orientation may elucidate interesting and valuable contributions of an historical actor or genre that are of less interest to geologist-historians.

Theories of the earth provide a case in point, being marginal if regarded as proto-geology but nevertheless comprising a genre which certainly was not insignificant to the development of geology. It is consequently inappropriate to evaluate a particular theory of the earth in terms of its individual longevity or resemblance to lasting geological knowledge. Discarded theories were not entirely unsuccessful if they provided a systematic framework for posing particular research problems by which their own deficiencies were exposed. Pallas conceded such an eventful fate for his own system, confessing that his hypotheses could "never be presented as proofs," and were not "entirely free of difficulties," but attained only a relative "degree of perfection." His influence on theorists of the earth after him, such as Saussure and De Luc, was substantial. The open question with regard to Pallas' theory of the earth and others of the same periods is the degree to which they situated field observation, such as his account of the Urals, as the primary evidence for the reconstruction of the earth's past.

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BASIC QUESTIONS IN PALEONTOLOGY. GEOLOGIC TIME, ORGANIC EVOLUTION, AND BIOLOGICAL SYSTEMATICS. *Otto H. Schindewolf. 1993. Translated by Judith Schaefer; edited, and with an afterword, by Wolf-Ernst Reif; foreword by Stephen Jay Gould. University of Chicago Press, Chicago and London. xviii + 467 p. 32 pl. Hardcover, $75.00; softcover, $29.95.*

In Germany, it was for very long the custom that academic positions—and, indeed, many other scientific appointments—should not be advertized in journals or newspapers and open to all contenders, but should be filled through private contact between the eminent scientist doing the hiring and colleagues whom he esteemed. (No, almost never 'she': women were rarely to be encountered in such circles). The consequence was that, yes, it was important that a student should work hard—indeed, he must—but he must be always respectful of his supervisor and his supervisor's cronies. In particular he must not challenge, or express even mild disagreement with, the particular theories that they cherished, while to point out errors in their work or their thinking would be to commit professional suicide.

Some of the effects of this on the earth sciences are well known to historians. When Albert Oppel adopted the stratigraphical divisions formulated by Alcide d'Orbigny, he knew this to be in contradiction to the strong opinions of his mentor, Friedrich August von Quenstedt of the University of Tübingen; consequently, he felt it needful to conceal his adoption of d'Orbigny's units beneath a smokescreen of altered terminology. In more recent years, my good friend Hans

Gocht early accepted W. R. Evitt's demonstration that the typical hystrichospheres were not an independent group, but cysts of dinoflagellates; however, his great respect for his senior colleague, Albert Eisenack, caused Gocht to conceal that opinion until Eisenack himself had changed his mind. There must have been many other occasions, less familiar or altogether unknown to historians, when a junior researcher suppressed original thoughts or even evidence in deference to a superior—and, no doubt, the progress of science has suffered in consequence.

Such academic authoritarianism is by no means unknown on the western shores of the Atlantic, yet it is rarer and very often associated with an immigrant scientist from Europe. Louis Agassiz is an example; his opposition to evolutionary concepts and his eccentric concepts of biological classification were long claimed—and accepted—as dogma. Aleš Hrdlicka's belief that man was a relatively recent immigrant to the New World, and his suppression of all evidence to the contrary, held back progress in North American prehistory for many decades. Nowadays there remain senior professors, certainly in Europe and even on these western shores, who view disagreement as insubordination and challenge as sacrilege. Mercifully, though, they have become few.

Otto Heinrich Schindewolf (1896–1971) was a successor of Quenstedt at Tübingen and wielded a comparable authority. This was strengthened in the post-war years by the chance that, almost alone among German universities, Tübingen had emerged almost unscathed from the devastations of bombing and battle. Indeed, it became a particular academic refuge, numbering several Nobel laureates among an academic staff of high distinction and immense influence.

Unfortunately for German science—and, in particular, for German palaeontology—Schindewolf held and proclaimed very special views. These views were expressed most fully in his book *Grundfrage der Paläontologie*, now made available for the first time in full translation. Schindewolf accepted the concept of evolution, yes; but his as an anti-Darwinian evolutionary theory, denying the influences of random variation and selection as consequence of environmental factors. Instead, he believed firmly that morphological change followed orthogenetic pathways, involving saltatory advances and influenced only to a small degree by external circumstances.

His concepts were not merely presented as a theory, but accepted as an orthodoxy. As Stephen Jay Gould writes in his introduction (p. ix):

> [Shindewolf's] position as a professor of the most prestigious chair at Tübingen gave him virtual hegemony over evolutionary palaeontology in post-war Germany. . . . I met Schindewolf only once during a meeting in 1970. . . . [He] was kind, even courtly to me and the few other foreign visitors, but I well remember the hushed awe that attended his few interventions, and I became decidedly uncomfortable when not a single German palaeontologist dared to question anything he said during the public forum.

That aura of awe has diminished by now. In an extended, and historically very valuable, afterword, Wolf Reif expounds and examines Schindewolf's concepts lucidly and detachedly. However, his analysis makes clear the inhibiting effect that their acceptance, not as theory but as dogma, had for so long on German evolutionary thinking. Reif's summation (p. 451) merits full quotation:

> Many German paleontologists avoided any discussion of evolutionary theory or explicit opinions on typostrophism versus the Modern Synthesis either in writing or at scientific meetings. Zoologists were reluctant to discuss the origin of higher taxa with paleontologists. Furthermore, no attempts were made to give adaptive explanations for seeming overspecializations. Orthogenesis in a strict sense ("rectilinear evolution") was taken as a reliable model and as a measure for evolutionary progress. This went so far that a certain "evolutionary height" of a taxon was used as a stratigraphic marker. Finally, as late as the 1970s, young authors risked censure by their superiors if they discussed typostrophism critically. Under the influence of Schindewolf's authority, evolution was no topic for the would-be paleontologist.

To understand the history of science—and, in particular, to comprehend properly why the forward-rolling of the wheel of knowledge has been so often stalled—it is important that such works as this should be made readily available to scholars. This work deserves to be on the shelves of all persons seriously interested in the development of evolutionary concepts, within and beyond palaeontology.

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THE OIL FINDERS. *Allen G. Hatley, ed. 1992. American Association of Petroleum Geologists, Tulsa, Oklahoma. 134 p. Softcover, $18.00.*

The Oil Finders comprises an important record of how capable, strong-minded petroleum explorationists successfully thought, analyzed, led, planned, organized, communicated, and persisted, often for protracted periods of time, to bring about oil and gas discoveries in various frontier and more mature areas of the world in recent decades. It is factual documentation written by earth scientists who were successful leaders and managers of the programs they describe.

The authors, alone or with others, could conceive and/or recognize potentially productive geologic models for previously unproductive rock sequences. But, of critical importance, is the fact that they could also gain in and maintain the needed support and backing of others, regarding technical, financial, organizational, political, and often cultural matters, sufficient to make their exploration programs succeed. They often had to do much of this "on their own," with minimal and difficult contact with home offices or supporting partners. They tell of positive achievement by independents, medium-size companies, and some "majors."

In order, Kingston tells of the value Exxon found in looking firsthand at local field geology and data in a wide range of global settings, in order to understand the total of world oil and gas distribution and the potential attractiveness of basins. Dunn recounts Phillips' extensive review of European onshore geology, contemplating geologic trends extending offshore under the North Sea, and working with host governments, all of which lead to discovery of the Ekofisk complex of fields in offshore Norway in 1969. Todd describes sensing in the early sixties that onshore producing trends in Indonesia might extend offshore, spending extensive time in working with backers and partners and in earning respect of and cooperation from local government regimes—all leading to significant offshore discoveries of the northwest coast of Java, starting in 1969.

Hatley tells how sensing carbonate deposits on existing seismic data led to a reef discovery in offshore Philippines in '76. Kinard recounts how a small field was discovered in the countryside of southern England in 1980, resulting from the study of local data and considering that deeper geology would not mirror that of strata exposed at the surface. Martini and Payne recount a Chevron geologist suggesting that a buried aborted rift basin might exist under alluvium in southern Sudan in 1973, followed by the gaining of a concession, the carrying out of a "textbook" complete exploration program in a virgin area, and the ultimate discovery in 1980 of significant oil.

Young explains how postulating the possible attractiveness of local structural trends with Middle East-like stratigraphy lead to multiple discoveries in southern Pakistan, starting in 1981. Masters tells of sensing in 1973 that "tight sands" of western Alberta might contain prolific economic gas if produced properly, carrying out an extensive regional analysis of electric logs, leasing aggressively, and drilling a succession of discoveries that ultimately documented the giant size of the today's Elmworth gas area. Fairchild informs us that contemplating a potentially attractive Middle East setting for Yemen, combined with favorable impressions from field examination, lead to significant oil and gas discovery there, starting in 1984.

One can learn what such people, and their associates, had to personally do to keep exploration progressing, from inception to discovery. Such efforts, often taking years, are rarely found described in textbooks or journals. Through good times and bad, they had to be sure that they were thinking straight when strengthening geologic ideas, obtaining intellectual and financial backing with within or without their organization, negotiating aggressively but fairly with host governments, and planning geological, geophysical, or drilling programs. Often, planned action had to be modified because of combinations of the drilling results and changes in geologic understanding, political situations, economic condition, or management and partner philosophy. Dry holes, and/or unanticipated discoveries were instrumental in requiring explorers to remain flexible, intellectually and operationally. Their families had to be supportive, resilient, and adventuresome. They contributed!

The chapters of the book are highly variable. They can be studied individually. The book emphasizes that individual explorationists can "move mountains"; that searching the literature and what other know, using the right geological, geophysical, or drilling activity in the correct sequence, and working with local governments and inhabitants are all important.

The lessons to be learned from *The Oil Finders* are timely, and not just history of the past—many are applicable in today's changing world and will be in the future. This book documents what goes on in the global exploration scene—for the benefit of lay people, young and old earth scientists, or teachers.

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THE MAKING OF A MINING DISTRICT: KEWEENAW NATIVE COPPER 1500-1870. *David J. Krause. 1992. Wayne State University Press, Great Lakes Books Series, Detroit, Michigan 48202, USA. 297 p. Hardcover, $39.95, paperback, $19.95.*

I was happy for a chance to reread this book and to offer the perspective of an historical archaeologist in a review to appear in ESH. I first became aware of Krause's work as a dissertation in Geological Sciences in 1986, when a colleague brought it to my attention. I read it with great interest then and was very pleased to see it published in the Great Lakes Books Series by Wayne State University Press in 1992, aware that the press would do it justice in both production and promotion.

This book tackles several interrelated issues in a novel and effective way. It is essentially a revisionist history of the development of a poorly understood but important mining district, an analysis of the maturation of scientific worldview, the role of political and economic forces in shaping events, and an evaluation of the roles of influential individuals in the intersection of these powerful themes. It is a complex book, extremely well-researched and the arguments convincingly presented. Krause provides a wealth of information in a format that is accessible and interesting, making it easy to understand the complex natural events and social interests that melded to form the Lake Superior mineral district.

Krause poses his basic arguments clearly up front. He suggests that the Keweenaw Peninsula of Upper Michigan deserves more attention from those interested in the development of mining and geology. As the major North American source of copper for most of the nineteenth century, source of over 10 billion pounds of refined mineral, its economic role cannot be over-estimated but is relatively unappreciated in print. Its uniqueness as a mining district based on copper in its native or metallic form makes it additionally significant. And it is also important because the region's discovery and development coincided with an important phase in the early nineteenth century development of geology as a science. The interplay of these themes makes a fascinating weave, at the same time illuminating the careers of three prominent individuals who held prominent roles within the region and in early American science: Henry Rowe Schoolcraft, Douglass Houghton and Charles T. Jackson. Krause offers a fresh and critical view of the contributions of all three, within the context of the opening and maturation of the Keweenaw.

Krause devotes only a few lines to the Pre-Columbian use of native copper. For the interested reader of this review, native people mined this metal for several thousand years, trading and/or transporting it over vast distances, and virtually every important early mine in the district was founded on aboriginal workings. Though this is an intensely interesting topic, it is understandably beyond the scope of this book.

European attention was focused on the copper from the time that the earliest explorers penetrated the interior of the continent. Especially with the relations of the Jesuits, recognition of the potential mineral wealth of the Lake Superior area was consistently mentioned. Serious proposals to exploit the copper date at least as early as the 1720's but failed to materialize. With the rise of British hegemony after the Treaty of Paris, a partnership organized by Alexander Henry attempted to mine on the Ontonagon River, near a famed large mass of copper known as the Ontonagon Boulder. The application of European methods and perspectives failed in this attempt; no successful mining was accomplished until the 1840's.

In the context of a discussion of the amateur and professional traditions in geology, Krause offers a clear description of the occurrence of native copper in three basic forms: amygdaloid, conglomerate, and fissure lodes. While native copper was known from other contexts, and was known in the Keweenaw from early times, its significance was not known. This distinction between the existence of native copper as opposed to its meaning in this setting was a key issue, not satisfactorily solved until middle of the nineteenth century. The likelihood of understanding this novel situation was, according to Krause, somewhat stifled by scientific orthodoxy. Despite the new emphasis on empiricism and observation in science, the doctrinaire views held by early geologists influenced by previous experience in other settings prevented immediate understanding of the geological and economic significance of the metallic copper in the Keweenaw.

The treatment of Henry Schoolcraft's role in the saga is well done, historically rich and convincing. Schoolcraft was an astute observer, noting the abundance and volcanic origin of the metallic copper, and the scarcity of chemically combined copper ores. His work in the 1820's and 30's heightened government awareness of the mining potential of the region, and he employed Douglass Houghton as a mineralogist, introducing him to the Keweenaw.

Houghton is the focal figure in the historical treatment of the Keweenaw district. His treatment "verges on hagiography", according to Krause. From my perspective, I think that he is right; Houghton has been lionized for his contributions to the establishment of the district. As a young scientist, an energetic observer and popular public figure, Houghton was definitely a central factor in the recognition of the potential of the district. He traveled widely, employing his formal training from Rensselaer to explore the region in the early 1830's. With a medical degree and broad natural history training, he collected plants and minerals and wrote an official report of the Schoolcraft expedition on the existence of copper in the region, a report that was published and widely circulated. During the middle 1830's, he returned to Detroit, started a medical practice, dabbled in real estate and took up a faculty post at the University of Michigan. With some stimulation from the New England states, Michigan established a State Geological Survey in 1837, within a month of achieving statehood. Douglass Houghton was a popular choice as State Geologist.

In his role as State Geologist, Houghton pursued the survey of the copper district with enthusiasm and contributed to the fledgling professionalism of geology through the Association of American Geologists and Naturalists. The field season of 1840 generated data for an influential annual report that laid out the basics of the Keweenaw geology, but had a more profound impact by alerting the country to the mining potential of the region. The Treaty of La Pointe in 1842 extinguished Chippewa Indian title to the land south of Lake Superior, opening the way for America's first great mineral rush, stimulated largely by Houghton's reports. In 1844 he conceived of a combined geological and linear survey to subdivide the lands, and contracted with the Federal government to carry out the survey. This approach met with great approval and anticipation for the completion of the popular geologist's research. Such was not to be however, for Houghton drowned in a Fall storm off the Keweenaw coast in October 1845, never completing his final report.

Houghton's tragic demise may have contributed to his elevation in the eyes of subsequent writers, what Krause terms the "Houghton Tradition". Whatever the source, he has been regularly praised for insight, creativity, integrity, and near godly qualities, particularly in regards to his recognition of the significance of the native copper in Keweenaw and his freedom from scientific preconceptions. In fact, the first issue of ESH contained a biographical article by Merk that perpetuates some of the mythology attendant to Houghton's memory. Krause examines Houghton's work critically, concluding that his contribution was not so significant, that he did not truly understand the geology of the region but was heavily influenced by the orthodox geological views of his day. He points out several instances, for example, where Houghton emphasized the importance of the several ores of copper to the future of the district, while they ultimately played no serious role at all, and other instances where Houghton's thinking was clearly conventional.

Rather than Houghton, Krause suggests that Charles T. Jackson, the first federal geologist to study the region, had the most accurate view of the nature of the deposits and their economic potential. This revelation is something of a surprise, for Jackson was notorious for generating scientific controversy by claiming priority of ideas and inventions. A truly disturbed individual, he effectively destroyed the government-sponsored survey, was forced to resign and subsequently vilified his assistants Josiah Whitney and John Foster. Despite his bad reputation, Krause finds historical support in his writings that Jackson, not Houghton, had an understanding of the native copper deposits of the Keweenaw that stood up to the test of time. The subsequent success of the district's development stands as testimony.

The ultimate irony is that the particular form of native copper studied by Schoolcraft, Houghton and Jackson alike, the large masses, up to 500 tons in size, found in the fissure lodes, proved not be the foundation of the mining district after all. Rather, the finely disseminated copper in amygdaloidal and conglomerate deposits, running to as little as 2% of the rock mined, proved to be the base upon which fortunes were made and the district flourished for a century.

Krause does a masterful job of placing scientific controversy within a social context. His extensive coverage of relevant documents is excellent, the framework of presentation is both engaging and convincing. I highly recommend this book to those interested in the foundations of geology and the intersections of science and industry in nineteenth century America.

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A HISTORY OF ANTARCTIC SCIENCE. *G. E. Fogg. Introduction by Rt. Hon. Margaret Thatcher. 1992. Cambridge University Press, Cambridge, England. xxi + 483 pp. $130.*

Almost eight years ago Stephen J. Pyne, in his book *The Ice* (1986), furnished readers with an extraordinary evocation of the Antarctic environment, its discoverers and the literary figures who had drawn upon this source for their plots or imagery. His text was not only lucid, but quite often almost poetic:

> To enter Greater Antarctica is to be drawn into a slow maelstrom of ice. Ice is the beginning of Antarctica and ice is its end. As one moves from perimeter to interior, the proportion of ice relentlessly increases. Ice creates more ice, and ice defines ice. Everything else is suppressed. This is a world derived from a single substance, water, in a single crystalline state, snow, transformed into a lithosphere composed of a single mineral, ice. This is earthscape transfigured into icescape. Here is a world informed by ice: ice that welds together a continent: ice on such a scale that it shapes and defines itself: ice that is both substance and style: ice that is both landscape and allegory. (Pyne, 1986, p. 2)

Though it included difficult passages and was at times somewhat repetitious, Pyne's book transcended scientific writing to become literature. Yet it was necessarily light on detail, the history of Antarctic science being only one among several concerns.

Very different is the work presently under review. The author has amassed a vast volume of facts, in consequence of what must have been prolonged searchings of a great diversity of sources. In his "Preface" (p. xvii) he sets forth his objective fairly, while recognizing his own limitations.

> One person cannot have the expertise to assess properly developments in all the diverse disciplines which are concerned with the Antarctic. The difficulty of tracing their history might be to some extent avoided by stopping at the point at which things become really complicated—some 30 years ago—but this would be to leave the tree without its fruits and, much as a professional historian of science may disapprove, the attempt has been made to bring this account as nearly up to date as possible. In spite of the problems, I am convinced that the history of science in Antarctica is worth writing about and that it should be attempted by a single author. . . . Multi-author treatments are ill-suited to do this; an inspired collaboration between two or three specialists would best but failing this a synthesis is only to be achieved by the individual mind. The result may be only a rough sketch of what is desirable but one hopes it may provide a foundation on which others may build. (p. xvii)

In similar fashion, the proper consideration of a text with so broad a compass would require its study by a team of reviewers; like the book, this review can be only a rough sketch. I suspect the author will consider it one coarsely drawn; for, though admiring the immense industry requisite to such a compilation, I did not find this to be a very satisfactory product of such a prolonged labour.

Though much on geology is included in earlier chapters, it is convenient to focus attention for this review on Chapter 8, "The earth sciences" (pp. 247–288)—firstly, because it will attract the most immediate attention of the readers of this journal and secondly, because the problems with this chapter epitomize those with the book as a whole.

First of all, there are errors and omissions. William T. Blanford becomes "Blandford" (twice on p. 248) and is omitted from the index. "Andersson" is mentioned on p. 250 without introduction—surely Johann Gunnar Andersson (1874–1960), but no initials are given and, since the index omits him also, he cannot readily be traced on other pages. The fossil genus *Archaeocyatha* is mentioned on p. 251, in a context suggesting it might be a plant; only on p. 261 does the reader learn that it was a "limestone reef forming organism." In citing the title of Wegener's classic book, the German word 'Entstehung" is misspelled (p. 257), while on p. 270, the English word "principle" is misused (line 24). It is possible that Dr. Fogg has precedents for employing the spelling "ventefact" for stones shaped by the wind's abrasive action (p. 281–182), but I have not found them: "ventifact" is both the original and the customary spelling (see Challinor, *A Dictionary of Geology* 1973, p. 312).

The inconsistencies are also disturbing. Why, on p. 265, is the French name "Expéditions Polaires Françaises" retained whereas, a few lines later, a Russian name becomes "the First Continental Expedition of the U.S.S.R."? Why, on p. 261, is the concept of geosynclines said to be one "now fallen from favour" yet then stolidly applied, in the passage that follows, without further comment? Whyever does a section on "Meteorites on the ice-sheet" (p. 279) begin

> Ice-cores contain micrometeorites and presumably there is a remote chance that one day a corer will hit a large meteorite.

but then proceed:

> However, the first meteorite to be found on the Antarctic continent was discovered on the surface in December 1912. . . .

These are all points of detail. A much greater problem for the reader is the difficult writing style. Is there any need for paragraphs one-and-a-half pages long (e.g., pp. 260–261), especially when so packed with quite diverse facts? *Must* commas be so firmly eschewed, when they would make sentences so much more readily comprehensible? (Those same two pages afford many examples). Why need there be so many sentences of convoluted meaning? An example is found at the beginning of the section "Physical limnology" (pp. 284–285):

> The ponds and lakes of the McMurdo Sound area chiefly interested the biologists . . . of the heroic age of exploration and little was done on their physical limnology except that Mawson reported on the formation of vertical prismatic ice and crystallization of salts and Murray reported on seasonal changes in temperature at various depths in lake ice. . . .

The answer to these questions is, of course, that the text has not been edited as thoroughly and carefully as so important a work merited. In view of the broad spread of Dr. Fogg's researches, it should have been sent out for critical reading to a series of referees, each expert in a particular field of science. Did that happen? If so, the earth sciences referee cannot be complimented on the job he did!

In particular, however, it was the task of the editorial staff at Cambridge University Press to suggest to the author the changes in format and punctuation that could have so greatly eased the task of the reader and the improvements to the Index which would have facilitated its use. Such advice, it appears, was not given or, if proffered, was not taken.

Recognizing as I do the immense scholarship that went into the production of this work, I regret having to write so caustically. It is too often a problem nowadays, that referees and editors allow works of stature to pass unmodified through their hands, when some careful shaping would have been very beneficial. With a book so expensive, greater care should have been taken to produce a product more nearly worth the very high price.

However, let me conclude more positively by saying that this book is truly a mine of information about the history of Antarctic science. If the working of it be effortful and some careful refining necessary to eliminate dross, nevertheless there is much profit to be gained thereby.

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A HISTORY OF THE EARTH. *John J. W. Rogers. 1993. Cambridge University Press, Cambridge and New York. 312 p. Hardcover, $89.95; softcover, $39.95.*

A book with a title like *A History of the Earth* leads one to envision a great tome or series of volumes. Not so. This book is relatively slim (300-plus pages) and designed to be, according to the Preface, ". . . largely an exercise in scale." John J. W. Rogers of University of North Carolina-Chapel Hill attempts "to discover, in each of the many aspects of geologic history, some level of information that is important to all geologists." The book is intended primarily for college seniors and first-year graduate students in geology, but the completeness and insight clearly mark a useful general reference. The price is certainly reasonable for the amount of information it contains. To be able to synthesize great amounts of information is an art not commonly appreciated, and the author succeeds admirably here.

The book is divided into nine parts: 1) Geologic time; 2) Principal controls on Earth history; 3) The Archean; 4) Processes in a rigid lithosphere; 5) The Proterozoic; 6) The Paleozoic—Part I. Life, Climates and Oceans; 7) The Paleozoic—Part II. Tectonics; 8) The Mesozoic and Cenozoic—Part I. Oceans, Atmosphere, Climates and Life; and 9) The Mesozoic and Cenozoic—Part II. Consumption, Collision and the Development of small ocean basins. References are listed at the end of each chapter, and briefly annotated reference notes follow the end of each chapter section. There are also author and subject indices. Photographs and illustrations are uniformly sharp.

The present volume is clearly a modern version of a historical geology text and does not discuss any history of ideas. From a historical perspective it is, however, the first text of which I am aware that calls the "Milankovitch cycles" the "Croll-Milankovitch cycles" (p. 19). Rogers introduces this designation in tribute to the contribution of Scottish glaciologist James Croll (1821–1890; not "A. Croll" as on p. 19) to theories of climatic change, which included Earth's orbital perturbations and changes in solar insolation (I acknowledge Stephen Boss of the University of North Carolina as the source of this information). Whether others adopt the terminology "Croll-Milankovitch cycles" remains to be seen.

Extensive coverage of the history of the geosciences was clearly beyond the scope of Dr. Roger's book. Because the widespread appreciation of the history of ideas is a goal of Earth science historians, a comparable text for the history of geological sciences, patterned after this book, might be a worthy project.

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TALES OF THE EARTH. PAROXYSMS AND PERTURBATIONS OF THE BLUE PLANET. *Charles Officer and Jake Page. 1993. Oxford University Press, New York and Oxford. 226 p. Hardcover, $24.00.*

Once again a working scientist and a science popularizer team up to give us a beautifully written book. Charles Officer is a research professor, whose expertise lies in geology and geophysics, affiliated with the Department of Earth Sciences and the Thayer School of Engineering at Dartmouth College (New Hampshire). Jake Page writes both fiction and nonfiction, including a regular column ("Jake's Page") for "Destination Discovery." With the writing experience that combined to produce this book, no wonder it is such fun to read.

The book is divided into 3 parts: Part One, "Nature's Effect on Man" (4 chapters); Part Two, "Changes in Climate and Life on Earth" (2 chapters); and Part Three, "Man's Effect on Nature" (3 chapters). Chapter titles are both catchy and detailed. For example, Chapter 3 is titled "There Have Been Frequent Flooding and Sea-Level Change Events on Earth," and Chapter 4 reads ". . . And Occasional Visitors from Outer Space." The first two parts concentrate on historic events such as earthquakes, volcanic eruptions, meteoritic impacts, sea-level changes, and plagues. The third part shows convincingly why human-induced changes on the planet need to be taken seriously, even in view of the more devastating powers of natural forces.

This book focuses more on eye-witness accounts or the commentary of prominent figures—rather than writings of working scientists—regarding catastrophic events, but this sets the thoughts of scientists of the day in their historical context. The phrase "gripping tale" may be a cliché, but the eye-witness accounts quoted in this book can be adequately described in no fewer words.

Photographs and drawn illustrations are of good quality throughout. The jacket illustration is a reproduction of the painting "Kilauea, Night Scene" by Titian Ramsay Peale (1842), on display at the Bishop Museum in Honolulu. The vivid reds and oranges of this scene are a stark contrast to the "blue planet" reference in the book's subtitle.

This book is obviously written for the educated lay public. It discusses both science and its historical context. As popular Earth science, *Tales of the Earth* will not be a primary source for Earth science historians, but it should be consulted for the integration of sciences and humanities that it presents. Budding scientists will find this book a winner; I also recommend it to budding science historians.

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INTERESTING PUBLICATIONS

Since the start of this journal, Founding Editor Gerald M. Friedman has prepared this column. Contributors wishing to list recent books and papers of interest to our members are requested to send them to Gerald M. Friedman, Brooklyn College and Graduate School of the City University of New York, c/o Northeastern Science Foundation, Inc., Rensselaer Center of Applied Geology, P.O. Box 746, Troy, NY 12181–0746 U.S.A.

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