BOOK REVIEWS

Gretchen Luepke, Book Review Editor

THE EARTH, THE HEAVENS, AND THE CARNEGIE INSTITUTION OF WASHINGTON. *Gregory Good (Editor). 1994. American Geophysical Union, Washington, DC. 252 p. Hardcover, $29.40 for AGU members, $42 for nonmembers.*

In December 1901 Andrew Carnegie proposed to donate $10,000,000 (in U.S. Steel 50-year bonds) to found an institution for scientific research. This became the basis of the Carnegie Institution of Washington (CIW), which over the following years played a major role in many areas of science. The essays in this book (volume 5 of the AGU series History of Geophysics), which are derived from presentations at a meeting in July 1992, examine aspects of the history of CIW's work in two areas: the earth sciences, as based (primarily) at the Geophysical Laboratory and the Department of Terrestrial Magnetism (DTM) in Washington, D.C.; and astronomy, at the Mount Wilson Observatory in Pasadena.

As the opening paper, by Ellis Yocheson, makes clear, one of the problems which the trustees of the CIW had to face initially was in what ways the endowment could best be used. There was considerable infighting over this, and even Carnegie became somewhat involved; a famous letter from him (reproduced in this volume) stated his belief that "exceptional men should be encouraged to do their exceptional work in their own environment" since there was "nothing so deadening as gathering together a staff in an institution". Ironically, this was addressed to the man who Yochelson shows was indeed exceptional in his ability to create and manage an organization, C. D. Walcott, then the director of the U.S. Geological Survey. Yochelson's description of Walcott's role should be read by all who know Walcott only through Steven J. Gould's portrayal of him as an overworked (and overly conventional) administrator who lacked the time and the insight to correctly interpret the fossils of the Burgess Shale.

One of Walcott's influences was to drive the CIW towards building several large laboratories, rather than (in today's terms) running a small-grants program; Ralph Jewell describes one program that was in the small-grants category, namely Vilhelm Bjerknes' work in meteorology. As the papers by H. S. Yoder and Gregory Good show, others besides Walcott favored large facilities, sometimes (as in the case of George F. Becker) beyond even the CIW's resources. An example of the "big-science" approach made possible by the CIW's endowment was the establishment of the Department of Terrestrial Magnetism (DTM) by L. A. Bauer, in order to perform a comprehensive survey of the Earth's magnetic field. In pursuit of this, the DTM constructed the nonmagnetic ship *Carnegie*-as Good points out, by far the most expensive scientific instrument created to that time for geomagnetism. In addition, the DTM sent out numerous land expeditions and also set up magnetic observatories in areas that would otherwise be without them, in a move quite correctly termed "scientific imperialism". One pair of papers (by R. W. Home and W. D. Parkinson) deals with one such observatory, at Watheroo, Western Australia. This pairing nicely shows the complementarity of a professional historian's archival work and the reminiscences of a participant: for example, Home notes that the buildings were constructed to a standard plan, devised in Washington, which included a roof to withstand the (nonexistent) snow loads-no doubt the explanation of Parkinson's story that the carpenter who built the house said it used enough wood for three houses.

As several papers make clear, the CIW sponsored other kinds of expeditions as well. Naomi Oreskes examines one such, the 1928 gravity expedition of the submarine S-21, in detail. She explains its scientific motivation in terms of American interest in the theory of isostasy, and makes a case for its having been one of the founding efforts in American marine geophysics; but perhaps the most interesting part of this paper is how it illustrates the interactions between CIW, the U.S. Coast and Geodetic Survey, and the U.S. Navy, all of whom made major contributions to the expedition. Within the earth sciences, and especially in geophysics, the longstanding work of government bureaus had made Washington, D.C. one of the scientific centers of the country, and the CIW was able to take full advantage of this. C. Stewart Gillmor and Bruce Hevly make the same point, the latter even speaking of a "Washington network", in describing the CIW's research in the 1920's on the ionosphere, where the Navy (again) and the Bureau of Standards provided both cooperation and competition.

The ionospheric research at DTM was begun by Gregory Breit and Merle Tuve; as several papers in this volume make clear, the latter was indeed another "exceptional man": like Walcott, perhaps more as an administrator and research director than as a scientist.

H. E. LeGrand describes how Tuve fostered rock magnetism and isotope dating studies (at the expense of the magnetic-field program), but then, in the face of accumulating evidence, decided to drop the rock magnetism studies. Viewed from the 1960's or later, abandoning paleomagnetism on the eve of the discovery of magnetic reversals looks like a terrible mistake, but LeGrand's examination of the record shows that it was soundly based on what DTM's researchers thought they were finding. Thomas D. Cornell gives an exhaustive description of Tuve's own program in explosion seismology, based on extensive use of the archives. One gem that this has turned up is Tuve's characterization, in 1946, of Roger Revelle, generally regarded as one of the architects of enlightened postwar science policy, as "an engineer, [whose] concept clearly is that scientific men can and should be persuaded to take direction in their work from those in charge of (Navy) funds."

Somewhat separate from all this earth-science work in Washington, D.C. was "the heavens", namely the CIW work at Mount Wilson. This owed much to a third "exceptional man", George Ellery Hale. Since the history of astronomy has been much more cultivated than the history of the earth sciences, the broad outlines of this story are better known, but several papers here provide closer looks. R. S. Brashear examines how Hale and the Smithsonian worked (sometimes together, sometimes not) on solar research at Mt. Wilson. Norris Hetherington, in a good example of the benefits of studying failed research programs, describes how Hale's attempt to develop a joint research program with T. C. Chamberlain foundered on the sheer difficulty of the problem chosen. David H. DeVorkin presents H. N. Russell's work at Mt. Wilson, and makes a good case that, in line with Hale's goals, he helped to reoriented much of the research there into a more physics-based mould. John Lankford examines the role of women in the work of the observatory, in an paper nicely counterpointed by Owen Gingerich's well-stated plea for a more thoroughly contextual approach. Gingerich points out that the papers here say little about the wider national or international world of astronomical research; while much the same could be said of many of the papers discussing earth-science topics, it is probably unfair to expect this in such a relatively unexplored part of the history of science.

Finally, this volume closes with several reviews of the available sources for the history of the CIW, and one summary, by Deborah Day, of the state of archival sources for the history of geophysics. All scientists interested in history should read this paper, if no other here, for it makes clear how little of the vital source material for historical studies is being saved.

All in all, this is a fine collection of essays that advances our knowledge of the history of geophysics. The editor is to be congratulated not only on organizing this conference, but also on getting the results out in a very respectable time. We can certainly hope that the AGU will sponsor more such activities.

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RAPHAEL PUMPELLY: GENTLEMAN GEOLOGIST OF THE GILDED AGE. *Peggy Champlin. 1994. The University of Alabama Press, Tuscaloosa. xiii + 273 p. Hardcover, $49.95.*

Raphael Pumpelly (1837–1923) was a man who began his career in the heroic age of American geology. But as he lived well into the twentieth century, he may also be described as a transitional figure in the history of geology.

Pumpelly came from a wealthy New York State family, and was thus able to pursue his geological interests to some extent in the style of a nineteenth-century gentleman-amateur. But he was also a fully trained mining geologist, who received what was perhaps the best available training in practical geology in the 1850s—at the Freiberg Mining School. And to all intents and purposes he was a professional geologist, though one who, by reason of his financial position, could choose the jobs that interested him, rather than having to scrabble for posts.

Pumpelly was an extremely well-travelled man. Indeed, he went round the world in his twenties, doing important reconnaissance work in Japan and China. In his old age, he visited Turkestan, carrying out important investigations at the borderland between geology, archaeology, and physiography. In his middle years, he worked for the Michigan, Missouri, and U.S. Federal Surveys, and also for the Northern Transcontinental Survey (associated with construction of the Northern Pacific Railroad). In addition to his survey work, Pumpelly made studies of copper ores, and the processes of metasomatism. He also attended to such controverted matters as the determination of the Cambrian–Precambrian boundary.

A good deal is already known about Pumpelly from his autobiographical *Across America and Asia* (1870) and his two-volume *Reminiscences* (1918). But Peggy Champlin (like Pumpelly an independent scholar) is the first to have written a detailed biography. This she has done in fine style, in a thoroughly researched and clearly written study that will be full of interest to readers of *Earth Sciences History.* There are rich archival resources for a life of Pumpelly held at the Huntington Library at San Marino, California, the National Archives in Washington, the Smithsonian, the Carnegie Institution, and elsewhere; and it is apparent that such resources have been used with care and to good effect.

It is not always the case that a reviewer has a detailed knowledge of even one section of a book under review. But in the present case I have, with a Chinese colleague

Yang Jing-Yi, made a close study of Pumpelly's work in China, for a paper forthcoming in *Annals of Science*. On the basis of this knowledge, I am able to testify to the quality and accuracy of Dr Champlin's historical work; and I feel confident in extrapolating from the detailed knowledge I have of one part of her topic to the accuracy of the whole. Needless to say, I am delighted that I have now had the opportunity to gain a much clearer view of the totality of Pumpelly's life and work.

Pumpelly was an intrepid traveller—an adventurer indeed. He went up the Yangtze Gorge at a time when Westerners were often unwelcome in China. He journeyed through Mongolia and Siberia in the middle of winter. As his first paid job, he worked a silver mine in Arizona under threat from the local Apaches. And as mentioned, he returned to Asia late in his career, the purpose of this last major investigation being to try to determine the recent geological history of Turkestan and to relate it to the early history of human life in that region, with special reference to the 'Aryan Question'. All this, and much more besides, is admirably treated by Champlin.

In my limited examination of the work of Pumpelly, I have been interested in the question of *how* geological reconnaissance work is done. How does one know what to do in a land where one is the first geological explorer? There must be such a 'bloomin', buzzin', confusion' of information available! How does one know what is useful or relevant, or which things should be examined first?

The study of Pumpelly's early fieldwork throws some light on such questions. From his field notes and his printed accounts of his work, we find that he had an excellent memory, for there are many published details that do *not* seem to be in the notes. (These notes were, incidentally, pretty rough, with crude sketch-sections and all sorts of items of miscellaneous information jumbled together.) Pumpelly sought as much information as possible from local informants and from existing Chinese geographical knowledge. He hired guides and sought the assistance of Westerners who had settled in China, were interested in a bit of travel, and could act as interpreters. On his return to the States, Pumpelly proved himself able to draw meaningful generalizations from his rough field notes, even though the 'inductions' were premature, being based on very limited personal experience. He looked sympathetically at the cultures of the peoples that he visited as well as the rocks. But for Pumpelly, rocks had to do with money. As befitted his early training, he was always interested in the economic aspects of geology, though he was also willing to enter into more general theoretical issues such as the notorious 'Taconic Question'.

Using the historiographical 'category' suggested by S.F. Cannon, Champlin represents Pumpelly as a 'Humboldtian' scientist. This may well seem an apt term, especially when one considers that Pumpelly and von Humboldt both studied at the Freiberg Academy. But it is not entirely obvious that what Pumpelly did (in his early explorations at least) was Humboldtian, rather than (say) Baconian. The notion of 'Humboldtian science' is something that is, I think, in need of clarification, though I have no particular objection to Champlin deploying the term as she does, given the present state of development of our understanding of nineteenth-century science or natural history.

Raphael Pumpelly offers a fine interweaving of the story of Pumpelly's private, scientific, and public life. Unpretentiously, and without becoming enmeshed in technical detail, it offers one of the best biographies of early American geologists that I have encountered. Pumpelly had, I believe, outstanding qualities as a man of energy and determination. Judging by his autobiographical works, and considering his strong early connection with German culture, it seems appropriate to represent him as a Romantic, even though his scientific ideas were not distinctively Romantic in character, and despite(?) the fact that he was so interested in economic geology. Anyway, Pumpelly's must have been a marvellously interesting career—of constant impression and adventure.

Besides being an accomplished pioneer geologist, Pumpelly came to hold important positions in the hierarchy of American geology, rising, for example, to the presidency of the Geological Society of America. Without being associated with any fundamentally new theoretical positions, he was unquestionably a major force to be reckoned with in the community of American geologists.

Champlin brings her subject's career to life in a way that is entirely congenial to me, and I have every reason to believe that other readers will find her work no less interesting and informative. It is an excellent thing that Pumpelly has now found the biographer he deserves. The only pity is that the author did not use (or was not allowed?) many more illustrations. A work such as *Raphael Pumpelly* calls out for illustrations from field notebooks, copies of maps, pictures of the places where Pumpelly worked, and so on. The history of geology benefits from 'visual imagery' no less than does geology itself.

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THE MASS-EXTINCTION DEBATES: HOW SCIENCE WORKS IN A CRISIS. *William Glen, ed. 1994. Stanford University Press, California. 370 p. Hardcover, $49.50, Softcover, $17.95.*

The hypothesis that a meteor collided with the Earth 65 my ago and killed off the dinosaurs is the stuff of lurid science fiction and tabloid journalism, not the subject of mainstream conservative scientific thought.

Nevertheless both the geologic and paleontologic communities have been in turmoil ever since 1980, when Luis and Walter Alvarez and their geochemical coworkers, Frank Asaro and Helen V. Michel, published this idea in *Science*. The scientific community then split into three major groups that continue trench warfare to this day. These groups include: 1) true believers—those who accepted at once the powerful argument that the presence of iridium at the Cretaceous/Tertiary (K/T) boundary, where dramatic changes in life forms have long been recognized, is an indicator that an extra-terrestrial body had struck the earth and was in some way responsible for mass extinction; 2) the volcanic apologists—those who rejected the extra-terrestrial source as the killing method, but argued instead that massive basaltic volcanism poisoned the environment, spewed out iridium-bearing ash, and led to the demise of many plants and animals at the end of the Cretaceous Period; and 3) the doubters—those who rejected the idea that a sudden mass-extinction had ever occurred, but if it had, neither asteroid impact nor volcanism had anything to do with it!

Lines were drawn in the sand and 15 years later, only the volcanic apologists appear to have lost ground. The recent discovery of an enormous buried structure on the Yucatan Peninsula that is interpreted as an impact crater formed 65 my ago has provide the smoking gun predicted by the original hypothesis. This interpretation is fortified by the discovery of tsunami deposits containing voluminous quantities of melted droplets in terminal Cretaceous deposits rimming the Gulf of Mexico and also occurring in Haiti. Few now doubt that an asteroid impact occurred in the Caribbean at the time of the K/T boundary; the effects of this event on the biosphere remain contentious, as does the identity of the killing mechanism.

Bill Glen's book attempts to take us backstage and to plumb the minds of some of the protagonists and antagonists. Strangely, no one from the Alvarez team is included, nor is a representative from the volcanic school given a voice. The paleontologists are well represented, with David Raup, John Sepkoski, Jr., and Stephen Jay Gould providing interesting insights into the impact of the new Alvarez hypothesis on their ideas concerning patterns of variation in biologic diversity through time and the significance of punctuated, rather than steady-state, evolution. These three authors were charter members of the true-believer club. Bill Clemens, a charter member of the doubters, carries the spear for those paleontologists who see in their data that stepwise demise of many fossil groups had begun long before the K/T boundary. Clemens and his students have provided important data bearing on the killing mechanism, first postulated to be freezing due to blockage of sunlight by dust thrown into the atmosphere. Their discovery of Late Cretaceous dinosaurs on the North Slope of Alaska cast doubt on this interpretation, as it appears that dinosaurs could adapt to long periods of cold and darkness.

Inclusion of some contributions in this book remain a mystery as they seem to contribute little to the subject debate. For example, Herb Shaw's essay on "The Liturgy of Science: Chaos, Number, and the Meaning of Evolution," appears to by a cryptic extract from an unpublished book dealing with non-linear dynamics, and would have been better left to another forum.

Much remains to be learned concerning the role of catastrophic, extra-terrestrial events in shaping the course of biologic evolution. Are all major pre-Pleistocene declines (mass-extinctions) due to impact? Can major impacts occur without significant biogenic effects? Is a big one out there headed our way??? Stay tuned. There's more to come in this fascinating story, and Bill Glen's book is a good way to acquire an insider's point of view.

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STRANGE GENIUS: THE LIFE OF FERDINAND VANDEVEER HAYDEN. *Mike Foster. 1994. Roberts Rinehart Publishers, Niwot, Colorado. 352 p. Hardcover, $29.95.*

Though not all those interested in the history of geology will immediately appreciate this book, Mike Foster's biography of F. V. Hayden is an important addition to the field, representing as it does the first serious full-scale study of a man who was without a doubt the best-known American geologist of the mid-nineteenth century. In recent decades historians of science have begun to do what Foster has given us: an effort at a "true biography," one which, rather than merely celebrates the accomplishments of the heroes of 19th-century American science, "begins and ends with the man, the whole man, and recognizes that man for what he was, and was not, rather than making him over to suit the predilections of a biographer." (p. 292) Foster's *Strange Genius* should further this development, since it ratifies this new standard and model for scientific biography, and contents call into doubt much that has been previously taken as settled about the development of American science in the 19th century.

It is well past time that historians of geology gave Hayden the credit he deserves for his accomplishments—credit that most of his contemporaries had little difficulty appreciating, much as they might have disapproved of him on other grounds. But as Foster clearly shows, all too frequently in Hayden's case, the views of a small coterie of his political and scientific opponents have prevailed, views which took advantage of real weaknesses and frequently exaggerated them far out of proportion and context. Only recently have some scholars and students begun to look more closely, questioning these received opinions, and only now do we have a new view of the whole man—and a very interesting man he proves to have been.

Foster writes that as a biographer, his interest was

in coming to know Hayden as a man, and he has probably succeeded in knowing Hayden better than even many of those who knew and lived and worked with him during his lifetime. For as Foster quickly makes clear, Hayden was not an easy man to know, and even went out of his way to obscure and re-write his life's story. Even now, Hayden remains something of an enigma–in part because many of his personal papers have been destroyed, and in part because in those that have remained Hayden was little given to conscious self-reflection. He was too busy—and too anxious—for that. Thus, most of what can be known of Hayden the man has to be abstracted from others' perceptions of him as well as those reflections of the man that Hayden inevitably, but unconsciously, put in writing. Certainly no one has put more time or effort into tracking down the myths and clearing away the debris that have substituted for the history of F. V. Hayden than Mike Foster has, and the results are richly rewarding.

In many ways, this Hayden is familiar. He remains a publicist of the west and geology, an enthusiast for his survey and its work and for the regions he explores, and a popularizer of science and the "Great West." He remains, as well, an amazingly effective scientific lobbyist who could be both obsequiously generous and ruthlessly competitive, and exhibit surprising and unpredictable mood swings. And he remains, naturally, the head of the largest and most impressive of the Great Surveys of the 1870s. But Foster shows us other sides to this familiar figure—sides which reveal that the figure was, perhaps, not all that familiar after all—or at the very least were only very partial views and impressions.

Besides the familiar Hayden, Foster also introduces a man who was consumed by demons, a man shaped by what sociologists today would call a classically dysfunctional family, who spent an entire career advancing from one father-figure to another in his patrons, and was in a constant pursuit of approval. But this is not the only forgotten Hayden that we meet, since Foster also re-introduces us to Hayden the scientist, one of the last classical field natural historians of the older mode, a man in love with the whole of nature, and a scientist who generally resisted theorizing but instead consciously (or perhaps unconsciously) adopted a travelogue style as not only the most appropriate mode to draw people into an interest in natural history and geology, but also to express the way he viewed nature: as wholly inter-connected, and only able to be appreciated one bit at a time, with the whole finally arising out of the parts. But even here, we meet Hayden the geologist, who could be a gifted theorizer, quickly drawing connections upon the most cursory of observations, with an eye for the important details, and a man who spent nearly three decades studying what became one of the central geological questions of the American West: the Great Lignite and the Cretaceous-Tertiary border. As a result of Foster's integrative study, we are enabled to have a much better appreciation of the man Hayden was, with his weaknesses and his strengths: a man of impressive talents, and some equally impressive weaknesses.

Withal, Foster is generally successful in avoiding the temptation to make Hayden into a hero. Admittedly, in an honest portrayal of Hayden, this is not always much of a temptation. But Foster does present us with a Hayden with whom one can have a certain sympathy, or at least an understanding, and this is especially so since Foster succeeds very well in placing Hayden within his context, both personally and institutionally.

Making a career in geology and natural history was no simple matter in 19th-century America—particularly outside of academia—and those who succeeded in doing so have come down to us as giants. Hayden was one of the most successful of these giants, and Foster reveals that the extent of his success was all the more remarkable given both his own obscure beginnings and the competition he encountered in what was a very proprietary environment, as men scrambled—frequently ruthlessly—for every advantage in their efforts to create scientific careers and, if possible, fame and fortune. This examination of Hayden thus provides us with more than a biography of an isolated individual. On the contrary, it provides an entree into a much wider scene–and a double-edged one at that, since we can follow Hayden both on the way up and on the way down.

I have only three criticisms with Foster's book, none of them major. The first is that serious consideration should be given to including maps in any reprinting or paperback edition. During the course of his career, Hayden and/or his survey covered the better portion of at least a half-dozen states, and Foster seeks to follow him throughout much of his wanderings, in a worthy effort to watch him in action. However, for readers unfamiliar with many of the regions Hayden so energetically explored, well-considered maps would make the job much easier. My second criticism is in Foster's method of citation. Unfortunately, in my opinion, he does not use footnotes, but rather page references, and these seem to me rather sparingly used—a system that will make the task of those who wish to follow him somewhat more difficult. My final criticism is perhaps misnamed, since I think that if anything, in the end Foster gives Hayden less credit than the evidence presented may merit. Foster clearly shows that even in his final "defeat," Hayden landed on his feet, and if the verdict of the competition for the directorship of the USGS was as fore-ordained as Foster indicates (and that Hayden himself knew from President Hayes that he was not to receive the position), then perhaps Hayden succeeded better than his opponents intended in maintaining his own credibility and securing a virtual sinecure from which he could oversee the completion of the work of his own survey. But perhaps this is not a criticism, so much as the recognition of a biographer who is careful not to go too far beyond his evidence.

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VESTIGES OF THE NATURAL HISTORY OF CREATION AND OTHER EVOLUTIONARY WRITINGS—ROBERT CHAMBERS. *James A. Secord (Editor). 1994. The University of Chicago Press, Chicago and London. 254 p. Softcover. $19.95.*

Most historians of science know of *Vestiges,* written by the Edinburgh publisher Robert Chambers and first published anonymously in 1844. It is commonly thought of as a kind of fuse for the explosion that was produced by the publication of *The Origin of Species* in 1859.

It is not difficult to pick up a second-hand copy of *Vestiges* somewhere. My own was/is cheap (but nasty). Now, thanks to the Chicago Press and the scholarly endeavours of Jim Secord, one may purchase an inexpensive (and nice) facsimile copy of the first edition, along with a facsimile of the rather rare 188-page *Explanations: A Sequel to "Vestiges of the Natural History of Creation",* which Chambers published (again anonymously) in 1845 in response to the numerous criticisms that the first work provoked. Also, the reader of this new edition has the bonus of some of Chambers' minor writings on evolution, an annotated list of the first twelve editions of the *Vestiges* and of the American, Dutch, and German editions, a list of the book-length responses to the work, a list of all the periodical reviews (with their authors revealed where possible), an index, a useful bibliography of the secondary literature that has grown up around Chambers' work, and Secord's excellent introductory essay. The whole is evidently a kind of prolegomenon to the full-scale biography of Chambers that Secord is currently writing.

Since *Vestiges* itself is quite a well-known work, I shall not say much about it here, beyond reminding readers that the evolutionary theory that it espoused was *not* based on the natural selection model. Rather, as Chambers put it, it involved the idea of a 'universal gestation of nature'. Chambers was interested in the whole history of the cosmos, so he began his discussion with an account of Laplace's version of the nebular hypothesis (as expounded by 'Compte'). Chambers was also interested in the work being carried out on embryology in his day, and he saw the evolution of living organisms as a process analogous to embryonic development. Thus the most telling part of his book was the table that showed parallels between the stages in the foetal development of the human brain, the system of classification that had been developed for animals (from "Zoophyta" to humans), and the various kinds of fossils found in the different systems of the stratigraphic column. For Chambers, it was as if the earth were gradually giving birth to living organisms of ever higher type.

While the idea of a universal gestation of nature might seem far-fetched, the parallels between the results of taxonomy, stratigraphy, and embryology—as set down by Chambers in his table—surely called out for explanation and were hardly compatible with a six-day creation of the world (though if it were all done by an omnipotent God, then, I suppose, just about anything might be possible, no matter how peculiar).

With the various evolutionary works of Chambers gathered together, and with the help of Secord's introduction, the reader can now see just what an interesting figure Chambers was, and what a significant role he played in his day. It is hard today to see why anyone might have thought him impious, even if his ideas were intellectually provoking and out of step with those of the majority of his contemporaries. The *Explanations* reveal Chambers as a most devout man (though Secord tells us that he had a brush with the Scottish Presbyterian Church at one stage). Secord refers to the youthful Chambers not as a political radical, but (on the basis of a reading of an early essay of 1822—"Vindication of the World and of Providence"—reprinted by Chicago) as an exponent of 'cosmic Toryism'.

However, quite apart from the issue of Chambers' particular religious and political leanings, there are the broader and perhaps more interesting questions relating to the character of the scientific milieu in Britain in the first half of the nineteenth century, and the manner in which scientific publication was undertaken at that time, within that social context. There is also the question of the way that Chambers' work would have been read at the time of its publication, given that it broke the conventions of scientific publication by appearing anonymously. Chambers apparently had no interest in 'intellectual property' so far as seeing his name on the title page of *Vestiges* was concerned. But this gave a piquant flavour to his work, and evidently stimulated interest in the book.

Secord deals with such matters both succinctly and effectively. As he says: 'anonymity created a vertiginous sense of dislocation and uncertainty' (p. xl). Reviewers didn't know *who* their target was, even if they knew *what* it was. It might prove exceedingly awkward if the author were later to emerge from 'behind the veil' to reveal himself as a high-ranking figure such as Lyell, Babbage, Owen, or even Prince Albert! Again quoting Secord: 'one could never be sure who . . . [one was] attacking, what their politics, religion, class, gender, or [and this was important] their level of expertise might be' (p. xliii).

So in thinking about the "problem of anonymity", which is in itself an interesting historiographic issue, it may be more profitable to contemplate its effect on the reader, rather than worrying about the motives of an author who, for some reason, chose to be anonymous. In this way, we can perhaps get closer to understanding the impact of *Vestiges* on Chambers' contemporaries, than by considering the steps that led to its composition and publication (though Secord also tells us what we may wish to know about such matters).

Readers of *Earth Sciences History* will, of course, be particularly interested in the geological information in

Vestiges and more particularly in *Explanations,* since this book is much less well known. *Vestiges* gives one a conspectus of the stratigraphic column as it was known in the 1840s. But *Explanations* takes the reader further in this direction, for in endeavouring to defend himself against his critics Chambers entered into a fair amount of detail about the geological debates that were taking place in Britain at that time. For example, we get a convenient view, from a bystander's position, of the points that separated Lyell and Murchison concerning the first appearance of vertebrates (fish) in the stratigraphic column. Indeed, we get a useful view of several of the technical controversies that were exercising the minds of paleontologists in Chambers' day. However, since Chambers did not give references for his sources, *Explanations* would hardly be a convenient starting point for research on the history of paleontology in the 1840s.

Moreover, one should recognise that Chambers' ideas were often idiosyncratic. (Mixing metaphors,) he liked to put his own eccentric spin on the empirical evidence, viewing it through the lens of his "embryological" theory of evolution. For example, he opined that whales and semals were the "immediate basis" of pachyderms and carnivores. Thus whales were "vestiges" of the ancestral elephants!

This brings to our attention the significance that the concept of "vestiges" had in Chambers' general theory. The well-known title of his book may trip off the tongue rather lightly, so that its theoretical significance may too easily be forgotten. Secord brings this point to our attention in his discussion of Chambers' ideas about the fauna of the Galapagos islands. Following the information provided by Darwin, Chambers regarded them as volcanic islands that were geologically recent. But their terrestrial fauna seemed to be archaic. The explanation, for Chambers, was that the iguanas and other strange reptiles of the Galapagos archipelago had not had sufficient time to 'gestate' to more modern types.

Such notions seem bizarre to us, and also appeared unacceptable to Chambers' contemporaries, but it should be remarked that they meshed well with his ideas on the development of the different human races. He regarded these as having proceeded different distances along essentially the same evolutionary (or "gestatory") road. So far as the history of humans was concerned, there was nothing idiosyncratic about such a doctrine in the mid-nineteenth century. It is to be found underpinning views such as those espoused by E.B. Tylor in the 1860s, with his 'comparative method' in anthropology and his concept of the "psychic unity" of mankind. Indeed, they appeared repeatedly in anthropological treatises in the 1870s (e.g., Lewis Henry Morgan), and eventually found their way into Marxist theories of the history of mankind, which have been of overwhelming influence in history in the last hundred years or more. It would be interesting, I think, to see how (if at all) Chambers' views linked up with these better known and more influential theories, and I hope that Secord will give the matter his attention in his forthcoming biography of Chambers.

Meanwhile, we have a thoroughly useful scholarly edition of Chambers' evolutionary writings, which students of the histories of nineteenth-century biology and geology will surely welcome most warmly, as does this reviewer.

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PATRONAGE, PRACTICE, AND THE CULTURE OF AMERICAN SCIENCE: ALEXANDER DALLAS BACHE AND THE U. S. COAST SURVEY. *Hugh Richard Slotten. 1994. Cambridge University Press. 228 p. Hardcover. $44.95.*

Many of the institutions central to the promotion of science in the United States, such as the American Association for the Advancement of Science, the National Academy of Sciences, and the Smithsonian Institution, were founded in the mid-nineteenth century, leading historian Robert V. Bruce to talk of "the launching of modern American science" during the period 1846–1876. As Bruce, Nathan Reingold, Sally G. Kohlstedt, and A. Hunter Dupree have established, Alexander Dallas Bache, head of the U. S. Coast Survey from 1843 until his death in 1867, played a pivotal role in this increasing professionalization of American science. There still does not exist a satisfactory full-scale biography of Bache, but Slotten in his recent book, based on a dissertation written at the University of Wisconsin, fleshes out our knowledge of Bache by examining in detail the cultural and institutional dimensions of his activities at the Coast Survey.

In the first two chapters of his book, Slotten aims to place Bache's professionalizing zeal within the context of contemporary Whiggish culture. Leaning heavily on the work of cultural historian Daniel Walker Howe, Slotten defines Whiggish culture as a set "of three cultural themes: an emphasis on planning and control, absolute moral standards, and the need for order and balance" (p. 16). While his attempt to relate Bache's ideology of science to this larger cultural trend is very admirable, Slotten tends to argue more by putting forward sweeping characterizations of Bache's general worldview rather than analyzing in detail selected expressions of Bache's ideology; thus, for those not already inclined to accept such a cultural reading of science, this account may be more suggestive than convincing.

In his next three chapters, Slotten traces in detail the political strategies used by Bache to consolidate his control over the Coast Survey and transform the agency, founded in 1807 to map the nation's coastlines, into the country's premier bureau of science. Bache's

predecessor, the Swiss-born geodetician Ferdinand Hassler, had managed to alienate many political supporters through his unabashed elitism and arrogance. By contrast, the politically skilled Bache, nephew of a U. S. vice president, mobilized considerable support by tempering his elitist desires for scientific autonomy while paying close attention to the requirements of Congressional politics. Slotten does a particularly good job in showing how Bache secured the Coast Survey's position by arguing, depending on the audience, that geodetic surveying was a highly technical and abstruse science requiring trained professionals rather than amateurs (such as the naval officers who threatened to take over the survey) for its pursuit, and that geodetic surveying was not at all an abstruse, ivory-tower science, but a highly practical activity which produced immediate benefits for navigation and commerce. Thus, Slotten reinforces the point already made by Reingold that pure and applied science were differentiated much less rigidly in the nineteenth century than in the twentieth.

The most interesting and important chapter of Slotten's book, in my opinion, is Chapter six, which traces in detail the patronage network centered on Bache's Coast Survey. Basing himself on meticulously examined archival records, Slotten inventories the many ways through which the Coast Survey supported work in astronomy, terrestrial magnetism, geodesy, oceanography, and other geophysical fields outside the survey by, for example, paying summer salaries, loaning instruments, and providing general advice and encouragement to university professors and independent researchers. As Slotten demonstrates, the Coast Survey was the most important provider of government largesse to science at mid-century. He also argues convincingly that this patronage had a significant effect on the character of American science, as it favored those sciences which "sought to measure the geographical distribution of interrelated [physical and natural] phenomena in order to discover laws and quantitative relationships that might be presented graphically" (pp. 113–114). Coast Survey patronage thus goes a long way toward explaining the preference for this "Humboldtian science" (to use the term coined by Susan F. Cannon) over laboratory science in nineteenth-century America.

In his final, somewhat disjointed chapter, Slotten examines several of the cultural attributes of the Coast Survey, which Bache transformed into a highly regimented and hierarchically organized body in which the paternalistic director insured both the technical and the moral discipline of his underlings. While Slotten argues that the Coast Survey thus came to embody deeply held cultural and moral values, Bache's style may have been due as much to his experiences as a student at the U. S. Military Academy as to some amorphous Whiggish culture. Slotten also does not pay much attention to alternate cultural programs within the Coast Survey; while noting that some Coast Survey employees resented Bache's paternalism, he does not, despite having worked in the George Davidson Papers at the Bancroft Library, follow up on the contention put forth by Michael L. Smith in *Pacific Visions* (1987) that Davidson, one of Bache's early proteges in the Coast Survey, welcomed assignment to California in order to construct an alternative style of surveying practice consonant with more democractic and communitarian values.

All in all, Slotten has significantly extended our understanding of Bache as an institution-builder who played an important role in shaping nineteenth-century American science. Slotten also deserves praise for beginning the examination of the Coast Survey's cultural milieu. It is to be hoped that other historians of the earth sciences will follow Slotten in examining the political and cultural dimensions not only of the Coast Survey, but also of the many state and federal geological surveys which characterized American science at this time.

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THE SCIENTIFIC LIBRARY OF HORACE-BÉNÉDICT DE SAUSSURE (1797): ANNOTATED CATALOG OF AN 18TH-CENTURY BIBLIOGRAPHIC AND HISTORIC TREASURE. *Albert V. Carozzi and Gerda Bouvier. 1994. Mémoires de la Société de Physique et d'Histoire Naturelle de Genève, volume 46. 201 pp. Softcover. SFr 56; $40.00. (Postage included; order prepaid from SPHN, Mus. d'Hist. Nat., C.P. 6434, CH-1211 Geneva 6, Switzerland.)*

The purposes of this tall (29.6 cm) and attractive volume are well expressed by the authors in the following statement: "More than a catalog, this work is a piece of research on the history of science in the 18th century. It documents the contents of the private library of a wealthy and famous enlightened naturalist and reveals how that library was selectively built, personally organized, and used for the scientific and philosophical endeavors of the owner" (p. 59). The result gives us access to valuable information about how Saussure dealt with the problem of assembling the textual sources he thought necessary for his work. It is also instructive as a piece of historical detective work.

Carozzi and Bouvier have aimed at "a reconstitution, according to modern bibliographical standards, of the scientific holdings of Saussure's library" (p. 1). Reconstituting was necessary, because much of the library was dispersed, apparently sold at the end of Saussure's life. Of the 1202 entries in this catalog, around 350 (or 30%) represent titles retained in the private libraries of three of Saussure's descendants. The other 70% of the catalog entries are derived from several sorts of evidence.

About 1788 Saussure prepared an incomplete manuscript catalog of his mineralogical and chemical books.

A second manuscript catalog, also incomplete, was drawn up about 1797, perhaps in preparation for offering the books for sale. There also exist lists of books Saussure considered especially valuable—most of these included sets of hand-colored plates—when, late in his life (ca. 1795–1796) he began to sell parts of his library because of financial need. These documents, which Carozzi and Bouvier describe (with photographic reproduction of several pages), clearly provide highly reliable information on books Saussure owned. Another kind of manuscript information source, with somewhat less secure implications for Saussure's ownership of books (as the authors carefully explain), is found in Saussure's *Livres à demander*—book wishlists—and also in lists of books ordered from booksellers at various junctures between 1769 and 1792. In the absence of complete documentation on book orders and payments or other correspondence with the booksellers—although parts of these do survive—this category of evidence, while quite valuable for reconstituting Saussure's library, is less decisive than lists of books owned; for example, it is evidently not always possible to be certain that a bookseller actually furnished a book that was ordered. However, Saussure's book orders, which are to a large extent transcribed here (items not considered scientific being omitted), tell us much about the buyer regardless.

A final type of evidence used in drawing up the entries comes from Saussure's own notes and digests of his reading, and in his quotations and references from various works in his writings. The authors draw particularly on Saussure's references and discussions in his two main works (*Essais sur l'hygrométrie* and *Voyages dans les Alpes*) and in letters he wrote to Albrecht von Haller between 1760 and 1777. I cannot help but wonder whether the authors place too much confidence in inference of Saussure's personal possession of all the works used in this fashion. Absent specific allusions by Saussure to his ownership, we may ask why Saussure could not have relied to some extent on books borrowed from others. In the small and tight-knit community of active naturalists in Geneva, might not Saussure and some of his compatriots have shared use of some scientific books with one another? What about the personal libraries of Charles Bonnet (Saussure's uncle) or Jean Senebier (who was for some time the librarian for the Geneva Republic)? I don't know the answers to these questions, but they are not raised in this volume.

Saussure's outright ownership of every book in this list is of course not the only issue (and the authors do clearly indicate which titles are included by inference from his notes, quotations, and discussions). For most purposes, no doubt the most important thing to know is what works Saussure considered *worth* both possessing and reading; to this the catalog responds admirably. Besides providing full identifications of the works, Carozzi and Bouvier give emphasis to what can be shown about the highly variable rate at which Saussure put together his library (a large fraction of it was assembled between 1769 and 1772), and the different countries where the books were published. One is struck by the impressively large number of German publications in which Saussure took an interest. Whatever else this may mean, it is testimony to the vitality of natural science in the German states during Saussure's lifetime. There is also tantalizing information on Saussure's ideas about how to organize and categorize his scientific books, with implications for how he viewed the configuration of the sciences and the relationships between them.

This book makes a valuable contribution to our understanding of Saussure's life and work, drawing attention to this versatile scientist's substantial dedication of energy and financial resources to his library. It is altogether fitting, in a time when historians of science are being drawn increasingly to investigate past scientific practices, to recognize the importance figures like Saussure attached to the problem of identifying and laying hold of the printed resources necessary to engage successfully in science. Carozzi and Bouvier have shown how central a component library-building was in Saussure's scientific activities. This book should be examined by anyone interested in the scientific culture of the 18th century.

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SCIENCE ON THE RUN: INFORMATION MANAGEMENT AND INDUSTRIAL GEOPHYSICS AT SCHLUMBERGER, 1920-1940. *Geoffrey C. Bowker. 1994. The MIT Press. 191 p. Hardcover. $27.50.*

The central focus of Bowker's book is on the Paris-based Schlumberger company, which during the interwar period became the leading international provider of electrical well-logging services to oil companies and in the process significantly advanced the use of geophysical methods for petroleum exploration. Bowker, however, does not provide a straightforward narrative history; for this, the reader is referred to a work by Schlumberger engineers Louis Allaud and Maurice Martin, which appeared in English as *Schlumberger: The History of a Technique* (1977). Instead, Bowker in his extended essay (which covers less than 150 pages of text) seeks to uncover the reasons for Schlumberger's spectacular success. In this endeavor he leans heavily on the work of Bruno Latour, the influential but rather idiosyncratic French theoretician of science.

Bowker wishes to counter the notion that Schlumberger's success was based on the unproblematic application of universally valid scientific law. Instead, he argues, Schlumberger strategically mobilized various bodies of locally valid knowledge and techniques. The

origin and nature of this local knowledge is not made very clear by Bowker, nor is the process by which these various bodies were integrated with each other. Indeed, Allaud and Martin's straightforward and technically informed history, despite some tendencies toward hagiography, gives a much better picture of the iterative process by which Schlumberger continually tested and refined its prospecting tools. Moreover, Allaud and Martin's emphasis on the empirical nature of Schlumberger's research belies Bowker's assertion that Schlumberger's science is commonly portrayed as moving from theory to application.

Bowker also wishes to argue that the organizational innovations of modern industry preceded rather than followed the development of industrial science and thereby "rendered industrial science possible" (p. 13). Bowker particularly focuses on infrastructural work "which operated by locally controlling social and natural time and space" (ibid.). Just as oil companies in Venezuela carved out heavily defended spaces from the chaotic pre-industrial jungle and imposed the orderly rhythms of industrial time in them, so Schlumberger, according to Bowker, imposed industrial notions of space and time on the mud circulating through boreholes. This portrayal of the organizational and infrastructural work necessary to maintain modern industry, while certainly quite flashy, is nevertheless too restricted. There is little in Bowker's account, for example, of the intense political and economic negotiations so vividly portrayed in David Yergin's *The Prize: The Epic Quest for Oil, Money & Power* (1991). As a result, Bowker cannot really deliver on his promise to uncover Schlumberger's business strategy.

In his final substantive chapter, Bowker uses a protracted patent trial to investigate the rhetorical practices by which Schlumberger submerged the local and infrastructural aspects of its science. While Bowker presents much interesting material here (the trial is dismissed in a single sentence by Allaud and Martin), his account is too disjointed for the reader to discern what was actually going on at that trial.

Bowker certainly provides some very useful insights. His analysis of the reasons why Schlumberger failed in the United States but succeeded in the Soviet Union in the late 1920s, for example, greatly adds to Allaud and Martin's historical account, and the program he outlines in his Introduction for studying industrial science should certainly stimulate future historians of geophysics. Nevertheless, Bowker's work suffers from many of the flaws characteristic of Latour's writings: excessive jargon (even such innocuous words as black box, laboratory, and parasite are used in highly specific and theory-laden ways), convoluted and often murky arguments, a tendency toward insidious puns, and a penchant for occasionally maddening overstatements based on questionably used historical evidence. Moreover, the bibliography is incomplete and poorly proofread; an important work by Galambos repeatedly referred to in the Introduction, for example, is not identified. As a result, this book is likely to be difficult reading for anyone not already completely habituated to Latourian analysis.

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THE G.D. HARRIS 1890 MANUSCRIPT ON THE STRATIGRAPHY OF MIOCENE AND PLIOCENE BEDS AT YORKTOWN, VIRGINIA. *Lauck W. Ward. 1993. Paleontological Research Institution Special Publication 20. Ithaca, N.Y. 118 p. Softcover. $20.00. (Available from the institution at 1259 Trumansburg Road, Ithaca, NY 14850.)*

This is the first-time printing of a manuscript that will help to elucidate the stratigraphy of the Yorktown Formation in Virginia and the stratigraphic and geographic location of the fossil localities of G.D. Harris.

The format Ward has used for this book is to be commended. The original, beautifully handwritten manuscript is exactly reproduced on the odd-numbered pages along with the original sketches of sections, and Ward's typeset identical version is on the even-numbered pages. Corrections and additions by Ward are handled in footnotes on the typeset pages. A correlation chart that shows the Yorktown Formation and its currently used members is included in the introduction.

Gilbert Dennison Harris lived from 1864 to 1952. He was founder of the Paleontological Research Institution in Ithaca, N.Y. To fill a need for publishing paleontological papers of his and other authors, he established the *Bulletins of American Paleontology* and *Palaeontographica Americana*. A memorial to him was written by Katherine Van Winkle Palmer in the *Bulletins of American Paleontology* (v. 35, no. 146, 1953) and Ward reproduces it at the end of his book.

Gilbert Harris joined the Arkansas Geological Survey in 1888 after receiving a PhB degree from Cornell University in 1886, followed by a year of graduate study there in geology. In 1889 he joined the U.S. Geological Survey in the Paleozoic Division and later transferred to the Cenozoic Division, where he was an assistant to William H. Dall in the Smithsonian Institution. During this time Harris mapped the areal distribution of the Tertiary in Arkansas and distinguished its subdivisions. While working with Dall, Harris became familiar with eastern and southern North American Cenozoic stratigraphy. In 1892, owing to a downsizing of the U.S. Geological Survey, Harris became Tertiary paleontologist to the Geological Survey of Texas. In 1894 Harris was hired as Assistant Professor of paleontology and stratigraphy at Cornell University, was promoted to full professor in 1909, and taught there until he retired in 1934 at the age of 70.

Over 100 years have passed since Harris wrote the original manuscript, and the fossil and stratigraphic localities described along the York River are no longer

accessible, because almost all the riverbanks have been covered by riprap. Knowledge of the stratigraphic localities along the York River would be lost but for the meticulous descriptions of them by Harris as reproduced by Ward. Our stratigraphic knowledge of Tertiary rocks in Virginia has increased, as well it should, and Ward brings the descriptions of Harris up to date nomenclaturally by the means of footnotes on the even pages.

The fossil collections made by Harris along the York River in 1890 are housed in the U.S. National Museum of Natural History. The entries that Harris made of his fossil-collecting localities into the U.S. Geological Survey Cenozoic Locality Book are reproduced in his hand with a typeset version added on the opposing page. Lists of the taxa in each collection occur with them, which apparently were prepared by Ralph Arnold of the U.S. Geological Survey in the early 1900's. The lists have all been taxonomically revised by Ward. Some lists Ward believes contain fossils from three different stratigraphic units, and these are differentiated by him by numbers following the taxa. Seven modern photographs of the York River are included at the back of the book.

This book is recommended to serious students of stratigraphic paleontology of the eastern United States.

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AMERICAN CHEMISTS AND CHEMICAL ENGINEERS, V.2. *Wyndham D. Miles and Robert F. Gould, Editors. 1994. Gould Books, Guilford, Conn. 365 p. Hardcover, $20 plus $3 postage and handling.*

Of the 269 people identified as chemists and engineers in this book, about 10% are known primarily as geologists or have contributed to the advance of geology in some way. Volume One, published by the American Chemical Society, included biographies of 517 people among whom a number of prominent American geologists were included. The criteria for selection for both volumes were rather broad and did not require research per se in chemistry. Those included might have been "any American alchemist;" someone who was involved with chemistry before 1800 or spent a large part of his/her career in chemistry before 1870; or a person who was president of a chemical society, founded a chemical company, had a laboratory named in his/her honor, attended the Priestley Centennial in 1874, who supported the chemical profession in various ways, or who was an original member of the American Chemical Society or other similar group. Slightly over 4% of the subjects were women, of whom none could be characterized as geologists.

The definition of a "geologist" is also difficult. While it is common knowledge that in general the farther we got back in the history of science the less specialized the practitioners were, still in these succinct biographies the sheer range of activity over the professional life of a man or woman was sometimes purely amazing. The people who have real accomplishments in geology might not only have been active in chemistry as well, but also in medicine, business, academic administration, the military, pharmacy, or as journal founders/editors or ministers. I didn't develop hard and fast criteria in order to identify these chemists as geologists. Employment by a state geologic survey might seem fairly objective. However, a few people worked for state surveys in the capacity of chemists, and analyzed mainly soil, fruit and vegetable samples. For some, making a contribution to geological theory would have been a good choice, but since this is a book about chemists this information is not always obvious from the contents of these biographies. One of my favorites, Isaac Tyson, Jr., certainly employed geology in his search for chrome and other metallic ore deposits, but his interests were focussed on exploitation, not on scientific advances. Some of the names are well known and the men are recognized as geologists. There are also those intriguing others, not well known to those who haven't studied their local areas, who may bear further inspection as contributors to American geology. I will discuss both of these classes briefly.

Probably the men best known as geologists, or as working in closely related disciplines such as mineralogy, are Archibald Bruce, George Jarvis Brush, Jules Timoleon Ducatel, Ebenezer Emmons, and Persifor Frazer. The surprise in some of these cases may be that their names appear in a book about chemists. In a familiar scenario for early geologists, whether American or not, Bruce (1777–1818) earned an M.D. degree, his in Edinburgh, and began collecting minerals while still in Europe. He taught medicine and mineralogy on his return, but is perhaps best known as the person who published *The American Mineralogical Journal* from 1810 until 1814, which journal included articles on general geology. Brush (1831–1912) did not work in geology per se, although part of his study was at the School of Mines in Freiberg, and his major work, *Manual of Determinative Mineralogy, with an Introduction on Blow-Pipe Analysis,* which was published in many editions over a period of fifty years, was of inestimable service to geologists. Ducatel (1796–1849) is perhaps best known in Maryland, having served as State Geologist in 1833–41, during which time he helped produce a geological map of the state. For a time he was a professor at both the University of Maryland and at St. John's College, Annapolis, but resigned to have more time for geology. Emmons (1799–1863) is known for his work in the New York and North Carolina geological surveys, and for his connection with Rensselaer Polytechnic Institute. Because he is best known for his geology, the author of this biography found it necessary to remind readers of Emmons' many contributions to chemistry. Frazer (1844–1909) began by studying chemistry, but finished at the School of Mines in Freiberg. He was mineralogist and metallurgist with

Hayden's U.S. Geological Survey partly and wrote part of the third annual report. After teaching chemistry at the University of Pennsylvania, Frazer resigned to work for eight years with the second geological survey of Pennsylvania.

Geologists who may not be so well known include Frederic Miller Endlich, George Augustus Koenig, Oscar Montgomery Lieber, Nathaniel Alpheus Pratt, and Edgar Theodore Wherry. I'm sure some readers will quarrel with the "not well known" designation of one or more of these men if they happen to be familiar with the situation in which they worked, such as a state survey or a particular agency. Endlich (1851–1899) was educated in Europe after he was 15. He studied a broad spectrum in the sciences, and the schools he attended included the School of Mines at Freiberg. After his return he worked at the Smithsonian Institution, then was a member of Hayden's survey groups in the west in 1873 and 1879. He later consulted on geology and mines in the west, and published a book on blowpipe analysis. After emigrating to the U.S. from his native Germany, Koenig (1844–1913) taught mineralogy and geology at the University of Pennsylvania. He had studied at several universities in Europe, but finished with a year at Freiberg. He is credited with discovering diamonds in meteoric iron, as well as with the discovery of "more than a dozen" new minerals. Lieber (1830–1862) also studied in Germany, including Freiberg, although he did not take degrees there. Upon his return he was assistant geologist at the University of Mississippi, assisted with the Alabama survey, and led the South Carolina survey. Pratt (1834–1906) studied mainly in the U.S. He became involved in minerals and mining in the Civil War, and later became State Chemist of Georgia, followed by an appointment from 1885–1889 as State Geologist of Georgia in their Department of Agriculture. Wherry's (1885–1982) degrees from the University of Pennsylvania were in chemistry and mineralogy. He became an assistant curator in the Division of Mineralogy and Petrology at the U.S. National Museum, and an expert in identifying minerals by crystallographic methods. He noted the influence of acidity in soils on plant growth, and became a self-taught expert in botany, ecology, and soil science.

In a multi-authored book such as this there is bound to be some uneveness in the writing and information offered. Apparently each author has searched sources to the extent thought sufficient, with the source list given at the end of each sketch. In a few cases one might wish that there had been a more extensive search, but in general this volume is most useful. It serves not only for information on chemists and geologists, but is also an initial reference on the scientific lives of women in the sciences and their career paths, which in a significant number of cases led away from their education in the physical sciences and toward medicine or the founding of home economics (domestic science) departments. The source lists provide a way into literature of interest for a time, place, or topic.

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A GEOLOGIST STRIKES OUT, 1954-1993 (RECOLLECTIONS). *Reg Sprigg. 1993. Arkaroola Pty. Ltd., Adelaide, South Australia. 359 p. Hardcover, Aust$39.95. (Available from the author at Arkaroola Pty. Ltd, 50 Pirie Street, Adelaide, South Australia 5000. Total costs, including economy air mail, are AUST$69, US$50)*

The sequel to Reg Sprigg's *Geology is Fun* (reviewed in ESH, v. 10, n,. 2, 1991) has arrived: *A Geologist Strikes Out* offers a light-hearted look at this famous Australian geologist's long and illustrious career from age 35 to 74. His thirty-fifth birthday, March 1, 1954, actually turned out to be geologically momentous: on that day Adelaide, in South Australia, experienced its worst earthquake on record!

The book's foreword is written by one of many "friends in high places," namely Sir Mark Oliphant, the nuclear physicist and former governor of South Australia. The cartoons of Australian cartoonist Stephen "Lafferty" Stanley grace this book as they did Sprigg's previous one; I was glad to see some information in the book's end papers about this obviously talented artist. After an "Acknowledgement and Atonements" section, the book contains 25 chapters of varying length.

Sprigg begins where he left off in *Geology is Fun*—leaving the security of government service to found Geosurveys (Australia) Ltd.; its studies eventually spanned both the Australian continent and the continental shelf. His work also took him to New Zealand (Chapter 19) and Turkey (Chapters 20 and 21). The phrase "strike out" has no application to the familiar baseball metaphor, for though there were the inevitable failures there were many more successes.

Only a few highlights can be noted in this review. Sprigg founded the Australian Petroleum Exploration Association (AEPA; Chapter 11), one of the first professional organizations to tackle conservation issues. The most detailed—and hair-raising—chapter in the book (Chapter 15: M.V. *Saori* and *Menfish*) describes oceanographic and SCUBA adventures; Sprigg was a pioneer in both pursuits in Australia. In these early days of SCUBA, brushes with disaster were almost routine. His motorized crossing of "the dreaded Simpson" (Chapter 16) with his wife and two young children marked the first time a non-Aboriginal woman or any child had ever ventured into this formidable desert. The final chapter reads exactly like a promotional pamphlet for Sprigg's Arkaroola Wildlife and Conservation Tourist Reserve. Sounds like a great place to visit.

This book contains many photographs and maps, including 24 color plates grouped in the middle. The black and white photographs are of uniform high qual-

ity; a few of the color photographs appear lighter than desirable, but most of them are of good quality. It would have been nice if the color pictures had been numbered for easier reference in the text.

The book's style reads somewhat like a transcript of a tape recording. Therefore repetition of earlier material among the chapters is common. Commas in many parts of the text sometimes appear to have been placed almost at random. Noticeable errors occur where references are not to Australia: The Adirondack Mountains are located in New York, not West Virginia (p. 2), but "Berkeley University" and "Santa Barbara University", while not technically correct, will not seriously mislead anyone familiar with the University of California system. The mineral name "spodumene" is misspelled (p. 11), and reference to Stephen Hawkins (p. 309) certainly should be to Stephen Hawking. The spelling of Raphael Pumpelly's name as "Pumpelli" is the older Italian spelling (information courtesy of Raphael Pumpelly III of Pacific Palisades, California.—ed)

Personal memoirs are most helpful to historians. Sprigg's writing style makes this a fun book. Geologists and historians alike will enjoy the adventures and misadventures (many clearly involving brushes with death) of a man born under a mighty lucky star.

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EFFECTS OF SCALE ON ARCHAEOLOGICAL AND GEOLOGICAL PERSPECTIVES. *Julie K. Stein and Angela R. Linse (Editors). 1993. Geological Society of America, Boulder, CO, Special Paper 283. 91pp. $32.00 (softcover).*

This book is a collection of seven papers that define scales from various perspectives and address concerns about how spatial and temporal scales are used differently by geologists, archaeologists, and geoarchaeologists. It helps clarify some interdisciplinary translation problems that can arise when specialists with different training communicate. Importantly, most of the authors call for coordination and timing of collaborative associations from the beginning to the end. Stein, in particular, notes that scale is considered twice in geologic and archaeologic studies: once during data acquisition (the observation and measurement scales), and again at the interpretation phase (spatial and temporal scales of reconstructions).

Site typologies and subsequent interpretations can be influenced by geologic processes, and typically landscape-scale geoarchaeologic studies have underemphasized the effects of potentially significant roles of small-scale geologic process on archaeologic interpretation. Linse gives good examples of the interpretative potential for small-scale geoarchaeological analyses from the Jornada Mogollon territory in New Mexico and Texas. She cautions that when justifications for geoarchaeologic analysis are developed after the research-design and data-acquisition stage, then there is a risk that the geologic data may never be fully integrated into the archaeologic interpretation.

An excellent paper by Richard E. Hughes and Robert L. Smith deals with obsidian provenance studies, in which they describe the formation of obsidian, the importance of geochemical assays, the implications of flow homogeneity, and the ramifications of multiple sources contributing to alluvial deposits. They refer to 1860's authors Wilson and Lyell, who discussed the influence that geology has had on the development of the science of archaeology, and note the profound change in archaeology over the past three decades with the widespread adoption of the multidisciplinary approach and refinements in archaeologic provenance interpretations. However, the need for geologists and geochemical specialists to understand the scale at which archaeologists use the data is critical to the successful deciphering of human prehistory.

Jeffrey S. Dean's paper gives a well-presented discussion of chronologic considerations in geoarchaeology. He points out that unifying archaeological and geologic perspectives has been impeded by disparities in the temporal scales of analysis. Because geologic and human-behavorial processes operate at different rates, the chronologic information inherent in them cannot be resolved at different scales. I enjoyed his discussion of the shortest period necessary to understand the dynamics of a natural or cultural system and how it varies directly with the age of the units being studied. Independent dating techniques need to be applied to both archaeologic and geologic phenomena to reconcile the differing time perspectives. His illustrative example from Northeastern Arizona employed radiocarbon and tree-ring dating, stratigraphic relations, ceramic placement, and direct association between geologic, archaeologic, and chronometric samples.

Archaeochronology and scale are discussed by Bonnie A. Blackwell and Henry P. Schwartz from a perspective that calls for greater understanding of the accuracy, precision, and limitations of the various dating techniques and how they effect archaeological interpretations. For instance, they give a brief discussion of the changes in the values for half life used in ^{14}C dating since 1949 and warn of a 50% discrepancy in calculations for the ^{32}Si half life that prevents its use as a dating tool. They note that no matter which technique is applied or the age of the sample, the errors typically are from 1% to 10%. Therefore errors in calendar years are much larger for older samples. On the positive side they show the relative precision of most techniques and give good examples of how changes in dating techniques and precision have changed our ideas about the evolution of hominids, both biologically and culturally. Historical geologists may appreciate the references cited where the transitions in geologic interpretations have led to evolving archaeologic ideas about the antiquity of our species.

Archaeogeophysical research and scale are addressed by Rinita A. Dalan, who laments that the application of geophysical methods in archaeology has been quite limited in comparison with other research areas such as engineering, ground water, oil and gas, etc. Owing part to the focus on detail of archaeologic features that are generally smaller in scale than features of interest in traditional exploration geophysics, she suggests that archaeologists should broaden their scale to encompass larger phenomena. Illustrating geophysical archaeology applied on both micro- and macro-levels at Cahokia Mounds State Historic Site in Illinois, she shows that there are more advantages to geophysics than just telling the archaeologist where to dig.

Vance T. Holliday, C. Reid Ferring, and Paul Goldberg discuss the close alliance between the temporal and spatial scales used in soil science (particularly pedology) and archaeology that are apparent in both regional and site-specific studies. In the Earth sciences, pedology is the closest in research scale to archaeology, with Quaternary stratigraphy and geomorphology next. They refer to historic collaborative efforts, such as Haynes' work in 1968, Leighton's in 1937, and Bryan and Albritton's in 1943, and argue that pedologists should be included in all phases of archaeologic research. "At the regional survey level, soil-geomorphic and soil-stratigraphic principles can aid in determining the age of landscapes, the age of deposits, and the likelihood for deposits to contain cultural remains."

This book is important not only to those who call themselves geoarchaeologists, but also to the larger family of fields of geology, archaeology, and their related subdisciplines. It should be read by most professionals and students because it gives a perspective that helps individuals broaden their outlooks and perceive questions from a wider range of viewpoints.

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SEISMOSAURUS, THE EARTH SHAKER. *David D. Gillette. Illustrated by Mark Hallett. Columbia University Press, New York. 1993. 205 pp. 205 pp. $39.95.*

Prior to, and in the wake of, the screening of *Jurassic Park,* a plethora of large and handsomely illustrated books on dinosaurs have been published—so many, indeed, and of such variable scientific quality that each must be seriously undercutting the sales of the others. Of the recent crop, only one is overtly historical in treatment—David Spalding's excellent *Dinosaur Hunters* (Key Porter Books, Toronto, 1993)—and most treat this theme skimpily, with tiresome inexactitudes and too many false assumptions. The very size and eye-catching cover designs of most of those books must serve, not to attract the scientific historian, but to deter him from even scrutinizing their contents.

At first sight, the title and presentation of the book here reviewed might seem to place it firmly into that class of books for dinosaurophiliacs. Not so; here is a work destined to be of great historical importance in the field of palaeontology. There are several reasons for this. Firstly, there have been no comparably detailed exegeses on the history of a single excavation; all stages, from first discovery to eventual completion, are clearly recounted and excellently illustrated. Secondly, the dinosaur discovered has proved to be the largest land animal yet known. (When the caudal vertebrae were first found, I teased David Gillette by telling him that it might merely have had a thick tail and might be more fittingly named "*Pachycaudosaurus*", but his subsequent work has fully justified his own faith in that sauropod's great size). Thirdly, the difficulties of the excavation, and in particular Dr. Gillette's urgent desire to discover more of the bones, caused him to have recourse to 'black-box technology' in hope of expediting the quest. Never hitherto have techniques from geophysics—ground-penetrating radar, proton free-precession magnetometry, the use of a portable scintillation counter to measure bone radiation and of acoustic diffraction tomography to interpret their differential transmission of sound waves—been so extensively employed in palaentological research, albeit—as one must reluctantly confess—with small success. Fourthly, it's rare for any author to set forth, so lucidly and reasonably, the sequential development of his ideas concerning the environment, the cause of death, and the circumstances of fossilization of the fossil creature he is studying.

This, then, is a highly unusual book which is destined to be a classic of vertebrate palaeontology. Since the publishers have indeed been seeking to attract the eyes of casual purchasers, the illustrations are lavish and well executed, the quality of the photographs being high and the reconstructions and diagrams clear and coherent. Since Dr. Gillette has ensured both their scientific accuracy and their relevance to the story he is telling, such visual materials are truly a benefit to the telling of his story. Altogether, this is a well-produced, attractive and important work, fully worth its purchase price.

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JAMES HUTTON AND THE HISTORY OF GEOLOGY. *Dennis R. Dean. 1992. Cornell University Press, Ithaca. 303 p. Hardcover, $39.95.*

Philosopher of science Thomas Kuhn, in his enormously influential book *The Structure of Scientific Revolutions* (2nd ed., 1970, University of Chicago Press), argues that, contrary to popular belief, science does not make major advances by the incremental accumulation of small steps but rather by revolutions—para-

digmatic shifts from one model that is more-or-less universally accepted by a community of scientists, to a different model. In geology the standard example has become the revolutionary transition from a fixist model of mountain building to a mobilist plate tectonic model. But what if no such widely embraced paradigm has ever developed in the first place? What if the scholars concerned with a certain topic cannot agree on the appropriate methods of research or even on the nature of the problem? Kuhn refers to such times as "pre-paradigmatic periods," characterized by debates over research methods and problems, and by the evolution of competing schools. The late 18th century is a good example of such a pre-paradigmatic period in geology, when a wide range of ideas about earth history were being advanced, schools were beginning to emerge, and consensus was unimaginable. There was no common vocabulary for geologic materials and structures, there was no agreement on the appropriate methods of investigating geologic processes and earth history, and there was bitter disagreement over whether theorizing about earth history was even a legitimate activity. It is the troublesome, pre-paradigmatic period that Dennis Dean explores in the book under review.

This book was published five years early. It should have come out in 1997 to commemorate the 200th anniversary of the death of James Hutton (1726–1797) and the birth of Charles Lyell (1797–1875). Although Dean refers to his book as a ". . . brief survey of Hutton and his influence" (p. 264), it is, in fact, a penetrating, comprehensive examination of Hutton and the intellectual landscape that helped to shape his views of earth history. It is intended to be an introduction and guide to Hutton's publications; it is certainly that and more. There are separate chapters devoted to John Playfair and Charles Lyell, and useful discussions of many other late 18th and early 19th century naturalists. For example, Dean discusses in some detail a series of ten geological lectures presented in 1805, eight years after Hutton's death, by Humphrey Davy, "the most prestigious British chemist of his generation." The book is not light reading and will be slow going for the reader who is not already familiar with the main issues and participants. For the serious student of the history of geology, Dennis Dean has provided a wonderfully insightful analysis of the work of one of the most innovative and influential thinkers in the history of geology.

Of the twelve chapters, the first three are specifically devoted to Hutton's life and writings, and the writings of his critics (especially Jean André Deluc). In these chapters Dean chronologically discusses each of Hutton's publications, followed by a discussion of published reviews and criticisms of these works. The result is a very methodical and thorough examination of Hutton's work. Key illustrations from Hutton's publications are included. These chapters contain many references to Hutton's Edinburgh colleagues such as David Hume, Adam Smith, the chemist Joseph Black, and to the geologic writings that may have influenced the development of Hutton's ideas, such as those of Horace Benedict de Saussure and John Whitehurst. Dean summarizes Hutton's well known lack of talent in expressing himself in writing with the statement that ". . . he was almost entirely innocent of rhetorical accomplishments" (p. 18). He points out, however, that in attempting to describe geological features Hutton was hampered by the fact that a geological vocabulary had not yet developed.

One of the most intriguing aspects of Hutton's work is the role that religious beliefs played in the development of his "theory." Hutton believed in a benign and omnipotent God who designed the earth for human habitation, a belief that is reflected in his concept that cycles of mountain building and erosion serve God's purpose of ensuring a supply of soil for human agriculture. But Hutton was a Deist, embracing the popular 18th century religious and philosophical view that, after God created the world, He didn't keep fiddling with it. Dean coins the phrase "Agricultural Deism" (p. 264) to capture Hutton's unique brand of agrarian theology. Hutton rejected a literal interpretation of the Creation narrative in Genesis, and he considered the Noachian flood to be completely inadequate to explain fossils. These views were abhorent to orthodox Christians and stimulated fierce challenges to Hutton's publications, most notably by Richard Kirwan, "Hutton's most vociferous opponent" (p. 99). Dean explores such topics with great dexterity, and he includes a revealing unpublished preface that Hutton had planned to include with his first published *Theory of the Earth* (1788) in an attempt to persuade biblical literalists that his theory did not contradict the Old Testament.

I discuss Hutton and A.G. Werner each year in my Historical Geology course, and this book helped me develop a much greater awareness of the pastiche of conflicting viewpoints that coexisted in the late 18th century. In helping students understand the essence of the controversies during this dynamic developmental period it is useful to focus on contrasting interpretations of the two rock types that were especially important to Hutton and his contemporaries—basalt and granite. Basalt occupied much of Hutton's attention between 1768 and 1785, when he first presented his ideas to the Royal Society of Edinburgh. There were two main camps of geological theorists in Europe at the time—Vulcanists and Neptunists. Vulcanists attributed great importance to ancient and present-day volcanoes and considered basalt to be volcanic. Neptunists, such as Werner, minimized the effects of volcanoes and postulated a universal ocean in which basalt had been precipitated. Hutton hobnobbed with the Vulcanists, but he adopted a non-Vulcanist distinction between lava and basalt; lava, according to Hutton's definition, contained empty vesicles, while basalt had vesicles filled with various types of minerals. He considered both to be of igneous origin, but he believed that of the two rock types only lava had actually been erupted from a volcano; basalt was thought to have a

subterraneous origin after which it could be exposed by erosion.

In 1785 Hutton turned his attention to the granite problem. The Neptunists were arguing that granite was a "primitive" rock type that had precipitated from the sea during the Creation or shortly thereafter. The concept of an "original" or "primitive" rock or landscape was completely contrary to Hutton's concept of a constantly self-destructing and regenerating earth in which rocks that had formed in one cycle were eroded away and reconstituted in the next. So it was important for Hutton to discredit the idea that granite is a "primitive" rock type. He undertook various excursions in Scotland to make observations on granite that would bolster his position. Dean chronologically presents the results of these excursions and the evolution of Hutton's arguments during this period.

A key element in Hutton's "theory" was the postulated presence of a ubiquitous, somewhat mysterious, force that he called subterranean fire or heat that uplifted mountains and consolidated loose sediments into solid rock. An igneous origin of granite, together with its formation at different times in earth history, was crucial to Hutton's "theory of the earth," whose supporters came to be known as the Plutonic school. (The first known use of the word *plutonic* was in 1793 by Richard Kirwan in a lengthy criticism of Hutton's 1788 "Theory of the Earth" article. Hutton's more famous two-volume book with the same title was published in 1795.)

Dean devotes one chapter to three contemporaries of Hutton: Sir James Hall (the "father of experimental geology"), Abraham Werner, and Robert Jameson. I especially enjoyed Dean's discussion of James Hall's experiments on the effects of heat and compression on the texture of basalt and other rocks. Hutton himself had no confidence in this sort of experimental approach by which one tries to "judge of the great operations of the mineral kingdom from having kindled a fire, and looked into the bottom of a little crucible" (p. 88). Hall and Hutton enjoyed a close friendship in which they frequently engaged in good-natured debate about the merits of experimentation in geology.

If Hutton is indeed the "founder of modern geology," as inscribed on his gravestone in Greyfriars Churchyard in Edinburgh, it was not obvious to unbiased observers during his lifetime. In the following passage Dean neatly captures the position that Hutton found himself relative to contemporary theorists (p. 92–93):

> In proposing and elaborating his own geological theory, Hutton had tacitly accepted the opposition of almost every other school claiming to deal with the same subject. As a Plutonist emphasizing subterranean but unerupted rocks, he clashed with those extreme Vulcanists who championed the efficacy of more purely volcanic processes. As a fluvialist emphasizing the erosive powers of running water over long periods of time, he conflicted with all those who still supported geologically effective sudden floods, whether one or more. As a uniformitarian who relied solely on observable present-day forces (except in the bowels of the earth, where we cannot see them), he similarly opposed widespread catastrophes of any kind while postulating an earth much older than traditional estimates allowed. It is not surprising, then, that almost everyone who attempted geological theorizing at all found himself seriously at odds with Hutton in one or more respects.

A separate chapter is devoted to the work of John Playfair, Hutton's close friend and biographer who played a key role in explaining and popularizing Hutton's ideas (and to whose memory Dean dedicates his book). Most 19th century participants in the evolution of ideas about earth history and geological processes, including Charles Lyell, knew of Hutton's "theory of the earth" only through Playfair's *Illustrations of the Huttonian Theory* (1802). So it is interesting to compare Playfair's presentation of Huttonian ideas with the way they were presented by Hutton himself. Playfair, for example, abandoned Hutton's emphasis on divine purpose, and Playfair specified heat as the subterranean agent causing uplift of mountains, whereas Hutton described a more vague subterranean force that he often called fire.

In geology textbooks James Hutton is often credited with originating the application of uniformitarianism to earth history. Dean discusses this in his final chapter, titled "Toward Modernity." He points out that earlier scholars, particularly Buffon, had applied uniformitarian assumptions to specific geologic problems, but that Hutton was the first philosopher to base a comprehensive theory of the earth *entirely* on gradualistic naturalism, completely rejecting any preceding cosmological myth.

Like Alfred Wegener, the father of the plate tectonic "theory of the earth," James Hutton's biggest problem was the lack of a credible mechanism. Just as Wegener had no plausible way for continents to drift across seas of basalt, Hutton had no plausible source of perpetual central heat within the earth to drive his cycle of rock formation and continental uplift. As presented by Hutton, his "subterranean fire" apparently operated only beneath ocean basins; once a new continent emerged from the sea it would stop rising, at which time the erosional process that was so important to Hutton's theory would take over. In the words of one of Hutton's many critics (p. 127): "It is difficult to conceive anything more fantastic and improbable. . ." In addition to the problem of a plausible mechanism, Hutton and Wegener share one other disappointment—neither lived to see the vindication of his paradigm.

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CHALLENGER AT SEA: A SHIP THAT REVOLUTIONIZED EARTH SCIENCE. *Kenneth J. Hsü. 1992. Princeton University Press, Princeton, New Jersey. 417 p. Hard cover, $39.50.*

In this book the author discusses some of the science, politics, and history of deep sea drilling (and the ship)

that revolutionized the understanding of ocean basins and continental margins. It includes personal experiences of a scientist who not only analyzed the data, but also matured in his understanding of that scientific revolution. Hsü's book is a somewhat biased and incomplete history of the deep sea drilling programs from 1963 to 1983 that used the drill ship *GLOMAR CHALLENGER* as a platform. Since 1983, the drill ship *RESOLUTION* has continued probing the sediment and rocks beneath the sea floor.

The Deep Sea Drilling Project (DSDP) and its successors, the International Phase of Ocean Drilling (IPOD) and the Ocean Drilling Program (ODP), have revolutionized the earth sciences. These programs have been a giant impetus for understanding the history of the earth and have served as a scientific stimulus for hundreds of scientists and students. There has been no comparable program in the earth science community. The thousands of cores stored in repositories are an international treasure. Data from these cores will guide interpretations of earth history and sea-floor processes for centuries.

The reader will take a ride down the river of this earth science revolution. Mentally bouncing to and fro through still waters and rapids, the reader at times is exhilarated by scientific discovery and at other times is knocked against rocks of political frustration and the author's ego. I know the project very well and was fortunate to participate on four cruises of the *GLOMAR CHALLENGER*. The book brought back many personal memories as it will for all who sailed on that famous ship.

The book is in part a translation from two earlier books by Hsü, one published in German (1982) and the other in Chinese (1984). The author organized the book into four major parts: 1) events leading up to the first voyage of the *GLOMAR CHALLENGER* (1963–1968); 2) the verification of predictions by studying the recovered cores (1968–1973); 3) some major discoveries (1973–1975); and 4) mopping up actions during the International Phase of Ocean Drilling (1975–1983).

Before reading the book, look carefully at the Preface. Hsü outlines the major objectives on page xviii. He states that the book is 1) mainly for geologists, students of geology, and historians of geology; 2) not easy reading for general readers, although it should be understandable to anyone with a college education; 3) not arranged in an orderly fashion, and is interspersed with sidetracks; 4) not a scholarly treatise on the history of geology, and the references are not footnoted; and 5) the story of a participant, and the partisanship is undisguised. I believe, however, that the author was too hard on himself when these objectives were written. I found it easy to sort out his partisanship and biases. I greatly appreciate that the author was not afraid to give opinions and to tell the reader about his personal experiences and professional frustrations.

The book has 20 chapters, an epilogue, two appendices, and an index. Appendix A includes a list of the 96 legs or cruises of the ship with dates, chief scientists, region drilled, and site numbers. Appendix B contains bibliographical notes, with some references, and acknowledgments for each chapter. Most figures in the text are reproductions of published diagrams. There are 15 black and white photographs; one is the frontispiece and the other 14 are all placed at the end of Chapter 16. Additional photographs would enhance the book, particularly some showing the ship laboratories, coring and logging activities, and the drilling platform.

Without any doubt, the reader will gain a great deal from the book. Not only does it present histories of several cruises, but it also gives a perspective on what it was like to be an earth scientist during the scientific revolution that involved the acceptance of sea-floor spreading and the theory of plate tectonics. Hsü does an incredible job of taking readers on a personal journey of discovery, from his own early rejection of sea-floor spreading and plate tectonic theory to his later acceptance of them. Earth scientists who took part in this extraordinary scientific revolution will recall similar experiences.

I recommend this book to historians, students of the Earth, and all earth scientists who have been influenced by the drilling results. I particularly recommend it to shipboard participants and to members of committees who influenced drilling objectives during the voyages of the *GLOMAR CHALLENGER*. There are some excellent reviews of regional geology that should be interesting to many readers. In particular, I liked the parts that discussed the drying up of the Mediterranean Sea and the geology of the Black Sea.

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UNIVERSAL ICE. Science and Ideology in the Nazi State. *Robert Bowen. 1992. Belhaven, London (distributed in the U.S. by Wiley, New York), 189 p. Hardcover $59 or £39.50.*

Robert Bowen, a geologist, whose book *Isotopes and Climates* (1991) created widespread interest, is the author of this interesting history of the earth sciences. Although this book stirred in me many unpleasant memories of Germany and the Nazis, its exciting rendering of how the Nazis promoted pseudoscientific cults engrossed me. Even the *Führer,* Hitler himself, became involved. The hero of this book is Hanns Hörbiger (1860–1931) whose son Paul (1894–?) I remember from the movies. Hörbiger formulated a theory that is now termed *World Ice Theory* and co-authored with Philipp Fauth (1867–1941) a book titled *Glacial Cosmogeny.* Millions of copies were sold, hence in terms of sales this book is the most successful earth-science book yet written.

Hörbiger was an Austrian machine engineer and in-

ventor. He patented the frictionless steel-plate valve in 1897 and founded successful industrial ventures. A company bearing Hörbiger's name is still in business today. The International Astronomical Union honored Hörbiger in 1935 by naming a large area of the moon after him. In 1985 the Austrian post office issued a special stamp on his 125th birthday.

Borrowed from the geological contention of the struggle between plutonists and neptunists, the theory propounded the universal conflict between heat and ice, which the Nazis expanded as a struggle that would allow Hitler to clean up politics. Hörbiger's book presented a general theory of astronomy and the earth sciences which claimed that the universe was for the most part composed of water ice and that ice infall profoundly affected the Earth and its climate. Further, the Milky Way was largely a ring of ice crystals surrounding the solar system. Icy moons had impacted the earth, creating catastrophies comparable to those now interpreted for the K/T boundary. The theory reached its most successful climax under the Nazis in Germany, when the Brownshirts, known as SA or Sturmabteilung, promoted it in line with ancient Nordic life and myth held dear in Nazi ideology.

Relevant headings in Bowen's book include meteorology, geology, ice in space, development of the solar system, plutonism of the sun, neptunism of the sun, the inner planets and planetoids, the outer planets and late Tertiary life. Hörbiger and Fauth explained the influence of cosmic ice on the planets. They probed the age of the earth, its past and future, in cataclysmic terms, and interpreted the ice ages in line with pre-Moon satellites and, in time to come, of the Moon too. They claimed a glacio-cosmogenic origin for coal, oil, salt and limestone. According to Hörbiger and Fauth, the origin of Man traces back to Amphibia.

As a history of science study this book has been one of the most exciting I have read in recent years. Yet a book review states "Robert Bowen's *Universal Ice* is the first historical study of this obscure, fascinating, and potentially illuminating topic. Unfortunately, that is the only good thing that can be said about the book" (M.J. Neufeld, 1993, Hörbigerism: Science, v. 262, p. 2069–2070). I totally disagree. The reviewer Neufeld stated "Bowen, a geologist apparently researched the book in isolation from the disciplines of history and history of science". As examples, Neufeld claimed that the Brownshirts known as "SA (Sturmabteilung) was politically impotent after the bloody purge of 1934, and the Third Reich was a heterogeneous collection of competing Nazi organizations". Nothing can be further from the truth. I was in the midst of the action during the Nazi era. On June 30, 1934 my father and I met in the streets of Berlin my home-room teacher (ein alter Kampfer, "a Nazi fighter since 1923") who was returning from sessions at the Government complex of Wilhelm Strasse. He had worked all night on the "lists" (of those to be murdered), and ordered us to the radio for that evening. June 30, 1934 became known as the "night of long knives" and among those murdered were indeed the head of the SA (Roehm) and his second-in-command, Obergruppenführer Ernst. However these men were replaced and contrary to Neufeld the SA became even more powerful in its own sphere of operating. In fact, for one who grew up in those days after 1934 the SA was more intimidating than ever. Almost every German family had one of its members in the SA. In retrospect, I wished that Neufeld's words were right that the "SA was politically impotent" after June 1934. Likewise to think of the Third Reich as a "heterogeneous collection of competing Nazi organizations" suggests incompetence and is far off the mark. Despite some overlap, the Nazi organizations were efficient, focused, and ruthless. They wanted to conquer the world and almost did. When the railroads transported men and equipment to fronts in Europe, Asia, and Africa and "cleansed" the conquered world behind the fronts, they still managed to round up millions and shipped them efficiently to Auschwitz. The German physicists and engineers were an integral part of the Nazi team. Many of the German universities were "brown" even before the 1933 Nazi triumph. In 1944 my home in England was destroyed by a V 1. V 1's and V 2's were the work of Nazi physicists and engineers, a key component of whom later immigrated to the United States to do science here. Despite honorable exceptions, like the rest of the German population, the German scientific community was in cahoots with the Nazis. In my opinion Neufeld is well off the mark and Bowen's view is more compatible.

Bowen presents an excellent case of how a set of baseless concepts gained scientific prominence and advocacy for politically correct Nazis under the Hitler regime. He demonstrates how Hörbiger twisted geological theories and how they were incorporated into Nazi ideology.

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THE CREATIONISTS. *Ronald L. Numbers. 1992. Alfred A. Knopf, New York. 458 p. Hardcover. $27.50.*

Long before Charles Darwin published his *On the Origin of Species* in 1859, students of science were wrestling with problems of how to reconcile biblical statements with conflicting scientific observations. In particular, early nineteenth century geologists encountered difficulty in harmonizing the Mosaic account of the age of the earth with stratigraphic and fossil evidence. Unwilling to forego the validity of the Scriptures, they found it necessary to liberalize their interpretation of the early verses of the Book of Genesis,

either by allowing for a "gap" of enormous time between the creation of the earth and the creation of the first humans or by extending each day of the original creation from twenty-four hours to a vast period of time. In general, their reinterpretations precipitated no major protest, for the advocates of expanded time not only remained faithful to the notion of special creation by God but also argued that their view enhanced, rather than diminished, the majesty of the Creator.

Similar arguments developed in connection with the theory of evolution during the 1870's and 1880's, and the proponents of evolution encountered relatively minor opposition to their liberal interpretation of the account in Genesis. In fact, before the 1920's, accommodation of evolutionism was more common than rejection, at least among American intellectuals, including a significant number of theologians. The form generally accepted, however, was neo-Lamarckian, for it could be more readily harmonized with the religious views of liberal Protestants. The theory even found its way into high-school textbooks.

By the 1890's, however, evolutionism was encountering more criticism, and by the second decade of the twentieth century, as the number of high-school students swelled and as religious fundamentalism began to flourish, it was the focus of concerted resistance. Initially, the fundamentalists expressed far more concern over the movement for higher criticism of the Bible than over evolution, for the former threatened to undermine their source of spiritual authority more directly than evolutionism did. Eventually, however, they also came to view the theory of evolution as the archenemy of the Mosaic account of creation, and from the ensuing conflict arose the creationist movement.

For quite some time we have needed a critical study of that movement, and now, thanks to the efforts of Ronald Numbers, we have it in *The Creationists*. Over a period of two decades, Numbers plumbed the archival sources and interviewed leading creationists in order to write a comprehensive history of anti-evolutionism in North America and Britain. Remarkably even-handed in his treatment of the subject, Numbers neither offers an apology for evolutionism nor condemns creationism. His goal is to inform his readers about the history and nature of the controversy, the background and views of the major anti-evolutionists, the arguments between and among the differing schools of creationist thought, and the essentially religious character of the movement. He has achieved his goal with splendid success.

As Numbers shows, the pioneering creationist George McCready Price labored virtually alone for many years to offer a "scientific" alternative to the evolutionary view of the origins of the earth and its inhabitants. He began with the assumption that the Bible is the inerrant source of truth about the origin of the earth, but he endeavored to draw evidence from geology to support his belief in special creation by God. From his reading of the first few verses of Genesis and from his procrustean study of geology, he concluded that the Noachian flood was a world-wide deluge that accounts for all present landforms, strata, and fossils. Several creationists later advocated other interpretations of Genesis, some following a "day-age" interpretation, and others, a "gap" view. Either interpretation allowed for the great antiquity of the earth and of life, usually excepting humans, and either could accommodate some form of evolution as part of the Divine Plan. But Price's "flood geology" ultimately prevailed in the movement and continues to be the dominant creationist view today.

For many years the creationist movement had to depend upon men (women were generally excluded) who acquired their knowledge of science informally, and only a few of them held an advanced degree. Even fewer were formally trained in biology, or, especially, geology or paleontology. In later years the movement began to attract proponents who possessed the "requisite scientific credentials," and it ultimately included several members who held a Ph.D. in a field of biology or chemistry. Only quite recently, however, could it claim geologists holding the Ph.D degree. Leaders of the movement remained acutely aware that their cause was hurt by the paucity of bona fide scientists, and they struggled against the charge that religion, not science, determined their anti-evolutionist views.

Eventually, creationist leaders came to believe that they could in fact develop a "creation science" that was as legitimate as, or, indeed, better than, a science that allowed itself to be guided by the theory of evolution. During the 1970's and 1980's, their belief in creation science emboldened them to make a claim for at least parity of the creationist view with evolutionist theory in the schools. Statutes to that effect were enacted but later struck down by the U.S. Supreme Court. The creationists did not retreat from their belief, however, that, when properly done, science would in fact support the idea of the original act of creation—and to many of them it would support flood geology. Toward that end they bolstered their support of institutes of creation research, though, as Numbers amply illustrates, varying schools of thought about how to interpret Genesis prevented a unified effort.

Numbers notes that many creationists rely upon a "dictionary definition" of science as factual knowledge and follow the Baconian precept of inductive inquiry. Such restricted perceptions, along with the basic assumption of the inerrancy of the Mosaic account of creation, tend not only to separate the creationists from the mainstream of scientific thought but also to cause them to denigrate the role of theory in science. Although he says much by implication about the latter problem, the author never directly addresses what theory means to scientists. In common parlance "theory" is synonymous with opinion or speculation, and this view leads to misunderstanding of the role and place of theory in science. In general, creationists also misunderstand the nature of scientific theory; indeed, much of their argument is built upon their view that scientific theory is mere speculation. Contrary to the creationist

view, however, a valid scientific theory, such as that of evolution, is neither a mere guess nor a procrustean model; indeed, it can be, and is constantly being, tested, and it must accommodate new facts, observations, and experiments. After all, the goal of science is to seek the truth about the phenomena it studies; nothing is sacrosanct. Scientific theory is neither a belief nor a dogma, but it may indeed challenge an established belief, as, without question, the theory of evolution has done.

Since the creationists are unlikely to forfeit or even to modify their basic assumption about the Bible as the inerrant source of truth about the origin and nature of the earth and of life, they must continue to depreciate the theory of evolution. Hence, we need to be fully informed about the roots and development of their movement in order to understand the nature of their position. In *The Creationists,* Ronald Numbers has given us a superb account of their movement. Indeed, his work is a major contribution to the history of science, the history of religion, and the history of ideas.

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DARWIN IN ITALY. SCIENCE ACROSS CULTURAL FRONTIERS. *Guiliano Pancaldi. 1991. Transl. by Ruey Brodine Morelli. Indiana University Press, Bloomington and Indianapolis. 222 p. Hardcover. US $35.00.*

It was in Italy—or rather, in the various states into which the Italian peninsula was then divided—that the first clear formulations of most major geological concepts took place between the fifteenth and seventeenth centuries. Some of these were noticed in other countries, but derided; the brilliant interpretations of Nicholas Steno—the Dane Nils Stensen, then residing in Tuscany—suffered that fate. Others passed virtually unnoticed; the percipient works of Fabio Colonna and Agostino Scilla, for example, received much less regard than they merited, while the geological writings of Lazzaro Spallanzani were lost in the shadows of his medical and biological researches. Leonardo da Vinci, perhaps the most gifted human being ever born, deliberately concealed his own scientific speculations, so that they were not discovered until long after his brilliantly original deductions had been made anew by others. It is arguable, indeed, that none of this early Italian work had any lasting influence upon the development of the earth sciences, serving rather as a fascinating prelude to their burgeoning in the early nineteenth century.

However, as the author notes (p. xii), the Swiss naturalist Alphonse de Candolle concluded that, during the nineteenth century, Italy had dropped from third to sixth place in the European intellectual community. Candolle was writing at the time when the recent incorporation of Rome had completed the unification of Italy and, beyond doubt, the Wars of Independence (1848–1861) had set back Italy's intellectual life; yet, as is also noted (p. 153), the proportion of university students in Italy was among the highest in Europe—higher than in France or Germany—and an even larger relative proportion of Italians were receiving secondary education.

The cause of this scientific recession is unclear, especially since new and important scientific works were being rapidly translated into Italian and circulated. Whatever the reason, it was during a period of intellectual decline that the Darwinian concepts arrived and were received. Perhaps that is the reason why, as this work makes evident, those concepts were so imperfectly understood. There was little initial hostility and, indeed, none of Darwin's books was included in the Catholic church's Index of prohibited books. Instead an intellectual mish-mash developed, in which the pre-Darwinian concepts of Lamarck and the ideas of Brocchi were very much intermingled with those of Ernst Haeckel and Herbert Spencer—concepts far removed from Darwin's own, even if often confused with them. The self-proclaimed Italian Darwinians were sometimes well aware that their writings departed widely from anything expressed in the *Origin of Species,* but often wholly unaware. Attempts to apply Darwinian theory in social or political contexts were yet more woolly-minded.

It is intriguing to note how certain marginal notes, written by the Italian botanist Federico Delpino into his copy of the *Origin,* pre-echo some of the ideas expounded by one of Darwin's most vociferous present-day advocates, Stephen Jay Gould:

> Nature proceeds gradually everywhere, Darwin states repeatedly in the *Origin*: and Delpino commented in the margin "Here is a principle I consider false" (p. 113).

Thus Delpino presaged Gould's advocacy of punctuated evolution and, moreover, anticipated Gould by believing in the importance of the rare geological event, such as catastrophic extinctions caused by changes upon or within the Earth. Though many of us differ on these matters from both the 19th-century Italian and the 20th-century American, Gould himself should be delighted!

This ground-breaking study, meticulously translated, does not treat very much with geology beyond its earliest chapters. However, it merits reading as a case-study in the reception of perhaps the greatest of the scientific concepts underlying present-day science.

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THE INCOMPLETE GUIDE TO THE ART OF DISCOVERY. *Jack E. Oliver. 1991. Columbia University Press, New York. 208 p. $17.95*

There is a genre of books suggesting how to do science that ranges from the autobiographical to the philosophically prescriptive; say, from Hardy's *A Mathematician's Apology* or Darwin's *Autobiography* to Medawar's *Advice to a Young Scientist* or part II of J.F.W. Herschel's *A Preliminary Discourse on the Study of Natural Philosophy.* Of these four, the book under review is closer to a more direct version of Medawar's, but as stated by the author, it was written without particular knowledge of the tradition to which it contributes.

Oliver is a leading US geophysicist who contributed significantly to the development of plate tectonic ideas and more recently has been a major proponent in the push to explore the buried continental crust. The intended audience of his book includes students and young scientists, but it is written in a clear, plain-vanilla style that communicates to nonscientists as well. A representative cartoon from the many that illustrate the text shows a roadside billboard advertizing a lottery for scientists only. The grand prize is a new paradigm, with lesser awards down to a mere viable hypothesis as fifth prize.

The book represents Oliver's advice on how to improve your chances for making a major scientific discovery. Science is based on empirical observation untainted by theory. Ideally, one enters early into a previously inaccessible field with the benefit of a new instrument or procedure adapted from another field of science. You then make a reconnaissance study of that field, skim the cream of the results, avoid getting bogged down in trivia, and leave the field as the curve of discovery flattens out. "Discoverers" are a different breed from the "Do-It-Righters" who follow them into the field to tidy up the details.

Scientists should beware of jargon, classification schemes, sophistication, and indoctrination; good advice certainly, but obtaining such an outside perspective will be as difficult for the scientist immersed in the work as it was for the singer from the 1930's big-band era whom the author quotes: "if I'd known it was going to be an era, I would have paid more attention to it."

Earth science is repeatedly likened to geography. The lesson is that earth scientists of today, like explorers of the seventeenth century, face a shrinking store of unknowns, suggesting that the rate of significant discoveries in earth science will decline, sooner rather than later.

To historians of earth science, this book may have value as a source recording the views of a major contributor to the plate tectonic revolution. Maurice Ewing, under whom the author began his scientific career, appears in at least ten places, usually in heroic terms. The author soberly advises scientists to avoid the mistake that Sir Harold Jeffreys made by opposing plate tectonics, identifying where Jeffreys went astray, his fatal error, his downfall.

Oliver invokes a reverse uniformitarianism by using discoveries from post-World War II geophysical exploration of the oceans to anticipate the discoveries that await exploration of the earth's buried crust. An analogous uniformitarianism may use the late twentieth-century historical view of nineteenth-century geology to support expectation that future historians will look back differently on twentieth-century earth science. This will be an interesting book for such historians.

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JOURNALS AND MEMOIRS OF THOMAS RUSSELL, 1791-5. *Christopher J. Woods, ed. Foreword by Marianne Elliott. 1991. Irish Academic Press, in association with The Linen Hall Library, Belfast. 199 p. Hardcover, US $29.95.*

For Irish historians, the name of Thomas Russell will be very familiar as that of an Irish patriot and close friend of Theobald Wolfe Tone who, after participating in the Irish uprising of 1803, was convicted of high treason and hanged. However, he has been familiar to historians mostly from the reports of his contemporaries. Though certain of his memoirs and journals were known to survive in Trinity College, Dublin, Russell's difficult handwriting had hitherto defied transcription. Now at last, as consequence of the careful work of editor Christopher Woods, we are permitted to hear Russell's own voice and learn of his musings, political, romantic (for he was an eager lover), and scientific.

Russell was not merely an Irish patriot, but also an antiquarian, naturalist, and geologist. His journals report extensively upon two of his geological excursions in Ulster—one in the Mourne Mountains (pp. 109–121) and the other a traverse of the terrain close to Enniskillen (pp. 124–133, 135, 159). In addition, geological and mineralogical notes are to be found among writings primarily upon other themes (e.g., p. 30, 86).

This young man—he was scarcely 36 when his life was ended—was, for his time, a percipient observer, the loss of whose potential contributions to the development of Irish geology can only be lamented. These transcriptions of his observations furnish the basis for a full reassessment, to discover which later researches he might have anticipated.

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ROCKS FROM SPACE. METEORITES AND METEOR HUNTERS. *O. Richard Norton. 1994. Illustrated by Dorothy S. Norton. Mountain Press Publishing Co., Missoula, Montana. 449 p. Softcover $20.*

What has been the role of meteorites in the history of this Earth of ours? That has been a question exercising the minds of geologists ever since the time our science came into being and arousing speculation long before that time. Meteorites have been objects for veneration; one was in the temple of Apollo at Delphi in ancient Greece, another is contained today in the Kaaba, holiest of Moslem shrines, in Mecca, Saudi Arabia. Some scientists believe, as discussed below, that meteorite impacts have distorted the course of evolution by causing massive extinctions; others have even claimed that life was brought to this planet by carbonaceous meteorites containing amino acids. However, meteorites receive only small attention, if any at all, in academic geology courses today—and, when discussed, tend to gain mention only in association with theory rather than in terms of their direct contribution to the earth's crustal rocks and geochemistry.

Yet that contribution is considerable and deserves better recognition. It is estimated that, at present, between 35,000 and 100,000 tons of meteoritic dust come to rest on Earth annually (p. 13, 45). That figure may not sound large, in view of the Earth's volume: but by multiplying it by the earth's age—let's say 4,500,000 years—and by bearing in mind that, during our planet's earlier history, the rate of annual accretion of meteoritic debris was almost certainly vastly higher, one begins to perceive how large the contribution truly has been.

This work serves as an admirable textbook to the science of meteoritics. The history of the study of meteors by astronomers, of meteorites by mineralogists and of impact phenomena by physical geologists is told lucidly; the different types of meteorites, meteoritic dust and tektites are described and well illustrated with good photographs and clear drawings; and the processes of their genesis are discussed.

My only personal regret is that the author has so wholly accepted the highly dubious concept of a link between meteorite or comet impact and the supposed "mass extinctions" of past times. In particular, that "terminal Cretaceous event" is stressed—the curiously selective event that allegedly extinguished the dinosaurs and the ammonites (groups already in deep decline), yet permitted most other groups—the lizards and snakes, the mammals and birds, the crocodiles and champsosaurs, the bony fishes and the sharks—to flourish and multiply unscathed. Maybe there *was* a meteorite impact at that time; maybe the iridium layer *does* mark that happening; but why, if so, does that layer never rest upon the remains of the beasts slain in that supposed celestial holocaust? To explain the extinctions by that event is quite as unreasonable as accounting for the extinction of the mastodons and mammoths by the explosion of atom bombs!

However, I must not end churlishly; for this is an excellent work, admirably presented. It deserves to be on the shelves of any geologist brave enough to poke his or her nose out from their particular professional groove to sniff, from time to time, the fresher airs of scientific thought.

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DIRECTORY OF BRITISH GEOLOGICAL MUSEUMS. *John R. Nudds, (ed.). 1994. The Geological Society of London, Miscellaneous Publication No. 18. 141 p. Softcover. £14.95. (Sold in the United States by AAPG Bookstore, P.O. Box 979, Tulsa, Oklahoma 74101-0979. $26.90 including postage.)*

This book has obvious value for tourists, but its greatest worth may be as a research tool for sedentary historians of geology. Museums are clustered by region within the British Isles, including all of Ireland, the Isle of Wight, and the Channel Islands. Stylized descriptions characterize the geological holdings of 86 sizable museums with notes on eight small institutions. Most descriptions cover exhibits, research collections, and archives. Many institutions also provide bibliographies of publications on their collections. These lead the reader to literature such as *Transactions of the Devonshire Association* and *Caradoc and Severn Valley Field Club, Occasional Papers.* With luck, they are available by interlibrary loan.

Museums that are little-known to foreigners can have real treasures. Most historians of geology know of Adam Sedgwick and Archibald Geike. The Ulster Museum (p. 10) is one of several institutions with Sedgwick correspondence. The Haslemere Educational Museum (p. 92) has archival holdings on Geike. In mineralogy, paleontology, and other fields, British geological museums conserve the collections and papers of legions of pioneers, including some who were not British. A person interested in early studies of pyramidellacean gastropods, for example, can identify collections by A. Bell, P. P. Carpenter, A. E. M. Cossmann, G. P. Deshayes, and a half dozen others. With conventional credentials, a scholar may be able to borrow specimens by mail.

Some collections listed in this book date from the 1600's; some are modern. Quite a few moved from institution to institution. Each museum in this directory submitted a brief history. Many trace the movement of well-known collections and identify older museums that merged into existing organizations. This is potentially useful information since some researchers may wonder what happened to specimens once owned

by groups such as the Shropshire and North Wales Natural History Society (p. 77).

For the preserved property of dozens of defunct museums, as well as the collections of scholars, this small book is a good place to begin looking. It is also a possible starting point for those concerned with rocks, minerals, or fossils described from any region within the British Isles. An especially valuable feature is the listing of some collections that are now lost. For example, all early collections in a half dozen institutions fell victim to bomber raids in World War II. Descriptions of these institutions identify lost material.

As in any compilation, entries in this text vary in clarity and in level of detail. Large institutions, such as the Yorkshire Museum (p. 58–59), may have hundreds of named specimen collections or immense archives. In a text of reasonable size, a full listing is impossible. They generally list a small portion of their holdings. Presumably these are the things of greatest interest to scholars. Perhaps a few institutions simply gave up trying to make selective lists. The Irish National Museum (p. 22), for example, described its holdings in a single page. Yet, its geological collection totals 100,000 specimens.

Lack of an index and brevity are undesirable features of this book. Still, for a historian of geology or a museum buff it is a work of great interest and value.

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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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SECRETARY'S REPORT FOR 1993

In the year 1993 the Society witnessed many significant developments. Among the most notable was the change in editorship of the journal, EARTH SCIENCES HISTORY. For eleven years Gerald M. Friedman, played a vital role in launching and sustaining the journal as the leading forum for work in the history of the earth sciences. His interest, enthusiasm, and the hard work that he devoted to developing and maintaining the journal has been crucial for the society and all interested in the history of the earth sciences. His work laid the foundation on which others will now build. The new editor is Mott T. Greene of the University of Puget Sound. A historian of science best known for his books *Geology in the Nineteenth Century* (1982) and *Natural Knowledge in Preclassical Antiquity* (1992), Greene will expand the journal's scope by including work in the history of meteorology and oceanography as well as the emphasis on the history of geology that has traditionally characterized the journal.

Several important meetings of interest to historians of the earth sciences took place during the year. Although not officially sponsored by the Society, many members participated in the Penrose Conference, "From the Inside and From the Outside: Interdisciplinary Perspectives on the History of the Earth Sciences." This five-day conference held in March 1994 in San Diego and organized by Society members Léo Laporte, Naomi Oreskes, and Kenneth Taylor, brought together some eighty-five scientists, historians, philosophers, sociologists, archivists, and librarians to examine and discuss differing perspectives on and approaches to the study of the history of the earth sciences. In contrast to most meetings, the conference emphasized methodological issues pertaining to the history of the earth sciences, and sessions focused on such topics as the different strengths that historians and scientists bring to bear on the subject, the role of biography in the history of the earth sciences, and the relationship of the history of the earth sciences to the history of science. Organized around a unique format that included not only invited keynote addresses and commentaries but also opportunities for participants to discuss their own work, the conference stimulated lively discussions that lasted far into the evening. While the intense and productive nature of the conference afforded participants few opportunities to enjoy the nearby beach and ocean, a cruise and evening dinner was the social highlight of the conference. Participants never did reach consensus on who were "insiders" and who were "outsiders," but the enthusiasm and interest stemming from the symposium led to the creation of an informal organization, "Friends of GeoClio," that plans to sponsor future meetings dealing with a single topic or theme on a triennial cycle.

In June 1993 Gerald M. Friedman organized the second annual History of Earth Sciences Society Meeting in Missoula, Montana. The meeting, held in conjunction with the annual meeting of the Pacific Coast Division of the American Association for the Advancement of Science, included a full day of presentations on the history of the earth sciences. Papers focused primarily on the history of the earth sciences in the Rocky Mountain region, and several Society members participated. The following day Tom Dutro organized and led a field trip focusing on historic and geologic sites from Missoula to Glacier National Park. A third annual meeting is scheduled for Troy, New York in July 1994. For detailed information on that meeting contact Dr. Gerald M. Friedman, Northeastern Science Foundation, Inc., Rensselaer Center for Applied Geology, P.O. Box 746, Troy, NY 12181.

A third, related meeting was the Fifth International Congress for the History of Oceanography held in July 1993 at the Scripps Institution of Oceanography in La Jolla, California. The meeting was organized by Philip F. Rehbock and Keith R. Benson, and Deborah Day, archivist at Scripps, served as local arrangements chair. One hundred and twenty-one people attended the meeting, and over 100 papers were delivered. Robert Marc Friedman delivered the keynote address: "Contexts for Constructing an Ocean Science: The Career of Harald Ulrik Sverdrup." Among the several plenary sessions one on "Continents v. Oceans in the Earth Sciences Revolution," included papers by HESS members Alan Allwardt, Homer LeGrand, and Naomi Oreskes. Rehbock and Benson are editing papers from the meeting for publication. A related series of papers on the history of oceanography was published in EARTH SCIENCES HISTORY 12, no. 1 (1993).

The May 1993 elections produced a strong set of officers. Hatten Yoder, the prominent petrologist and Director Emeritus of the Geophysical Laboratory of the Carnegie Institution of Washington, is President-Elect. He will take over from Léo Laporte in 1994. Ronald Rainger, Department of History, Texas Tech University, is the new secretary. The new Councilor for 1994–1995 is Beryl Hamilton of the Department of Geography, Liverpool Institute of Higher Education. Throughout the year Tom Pickett continued his excellent work as Treasurer.

The most notable event in the rotation of officers concerned the culmination of Ken Bork's tenure as Society secretary. For six years Ken played a pivotal role reporting on all Society happenings, staying in close touch with all members, and in general serving as the force that kept the Society running smoothly. And he did so with integrity and an infectious enthusiasm for the Society and the study of the history of the earth sciences. As the culmination of his active and highly successful years of service, Ken completed a new Membership Directory in 1993. The fact that there are now almost 450 members listed in the Directory, as

well as five new members that have subsequently joined the Society, are in no small part due to Ken's efforts. I cannot hope to equal Ken's accomplishments, but only hope that I can benefit by his example.

Respectfully submitted,
Ronald Rainger
Secretary, H. E. S. S.

SECRETARY'S REPORT FOR 1994

The principal news for 1994 was the election of a new slate of officers and the passage of a new amendment. The annual election resulted in overwhelming victories for all of the following officers:

President Elect—Kenneth L. Taylor
Treasurer—Dorothy Sack
Program Officer—Bruce Hevly
Councilor—David Oldroyd

The members also approved by a margin of 120 members in favor, 2 opposed, and 6 abstentions, a new amendment that provides a two-year term for the president. As a result the current President, Hatten S. Yoder, Jr., will serve for two years beginning in 1995, and the President Elect, Kenneth L. Taylor, will serve two years beginning in 1997.

The society continues to maintain a sizeable membership. At the end of the year the membership totalled 390. During the year 15 new members joined, but 44 relinquished their membership. Both the Penrose Conference in March 1994, and the GSA meeting in October 1994 generated many new subscriptions and active interest in the organization.

The change in officers brings to a close Tom Pickett's six-year tenure as treasurer of the society. During that time Tom wisely and effectively managed the society's finances. For 5 years he and Ken Bork, the former secretary, played a central role in expanding the society's membership rolls, and Tom particularly played an important role in sustaining the society through some difficult financial situations. Although I had the opportunity to work with Tom for only one year, he proved a helpful mentor, one who was always willing to provide guidance and good advice to a newcomer. HESS owes a special debt of gratitude to Tom for maintaining the society's stable financial status and providing a solid foundation and clear set of guidelines for his successor, Dorothy Sack.

Respectfully submitted,
Ronald Rainger
HESS Secretary