BOOK REVIEWS

Gretchen Luepke, Book Review Editor

***T. REX* AND THE CRATER OF DOOM.** *Walter Alvarez. 1997. Princeton University Press. 185 p. Hardcover, $24.95.*

This is an exciting intellectual adventure story of discovery. It is also a detective story, even though it took place 65 million years ago, and we know that the butler didn't do it. *T. rex* is an account of determination to find answers to important questions, of collaborators both planned and fortuitous who walked along for the joy of the journey and with an equal desire to find the truth, and an honest account of how the scientific method really works in practice. Walter Alvarez leads us gently along a bumpy intellectual road, complete with misleading detours, to the concept that a giant impact killed off the dinosaurs and other animals and plants, and finally to the discovery of the crater itself at Chicxulub, Yucatán.

The history of the Earth is divided up into periods of time based on sudden changes in the fossils preserved. Paleontologists since the early 1800s believed that evolution was a slow and gradual process. Therefore, it followed that these breaks in the fossil record, which could be recognized around the world, likely represented large and unrecorded chunks of time. One of these breaks is between the Cretaceous and Tertiary Periods, about 65 million years ago. This break, called the K/T boundary, first caught the attention of Alvarez when he was shown at Gubbio, Italy, that microscopic organisms called forams were large and abundant below the boundary in the Cretaceous, but only small ones survived into the Tertiary above the boundary, which also was marked by a thin, continuous, unfossiliferous clay layer. Thus the stage was set for the discoveries that followed.

Walter Alvarez talked with his father, Luis W. Alvarez, an experimental physicist, about the extinction at the K/T boundary and about the clay layer at Gubbio. Was the clay deposited rapidly, suggesting a sudden cause for extinction, or slowly suggesting a gradual cause? Did the clay bed represent a few years or a long interval of unrecorded time? Luis Alvarez decided that the best approach would be to measure the abundance of the element iridium, because brief deposition ought to be free of iridium, whereas a long time inteval should contain measurable quantities from the Earth's normal input of meteor dust. Frank Asaro, at the Lawrence Berkeley Laboratory, was asked to perform the analysis. This is complicated and painstaking work, and Walter Alvarez credits Asaro's ability to make the necessary measurements as a critcal reason that we know what caused the extinction of the dinosaurs. The final tests showed an extraordinary amount of the usually rare iridium. Now an answer needed to be found to explain its abundance and also to find out if this is a worldwide anomaly or restricted to the strata at Gubbio, Italy. If it were a worldwide feature, this would indicate that it was a critical clue to global mass extinction. Eventually two other continuous sections were studied, and testing showed the iridium anomaly at the K/T boundary also in these places. So, how could a presumed global iridium anomaly be explained? One theory put forth in the '60s and '70s for the extinction of the dinosaurs was a nearby supernova explosion. But to geologists trained in the theory of uniformitarianism and gradual, not catastrophic, change, this idea was unacceptable. More tests were needed.

Luis Alvarez suggested that plutonium-244 as well as iridium would be deposited by a supernova, so more painstaking tests were made. Initial results showed that plutonium-244 was present in the day at the K/T boundary, and that a supernova indeed seemed to have killed off the dinosaurs. But the scientific method requires testing, and testing again. Further analyses showed no plutonium-244, and the supernova theory had to be junked. What remained was the iridium anomaly.

So what else happened at the K/T boundary? About half of the animals, plants, and single-celled organisms had died out. How and why? What other event could have been responsible? How about an impact by a giant meteor or comet? After all, the craters on the Moon are usually assumed to be caused by impacts. But an impact would not itself cause a worldwide killing of animals and plants. Back again to the scientific-method drawing board.

Luis Alvarez suggested that a giant impact might create so much dust in the air that the world would become dark, and the end result could be mass extinction. Walter Alvarez was not so sure and decided to present only his data on the iridium anomaly (which had already appeared in the press) at an upcoming 1979 meeting of specialists in Copenhagen. At this meeting Walter Alvarez met Jan Smit from Amsterdam, who told him of an additional stratigraphic section of the K/T boundary he had found at Caravaca, Spain, that contains anomalous iridium. The importance of this confirmation led Walter Alvarez to think of Jan Smit as a codiscoverer of this class of evidence for impact. In 1980 a now-

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classic paper was published in *Science* by Luis Alvarez, Walter Alvarez, and others, presenting the iridium anomaly along with the impact hypothesis. Thus began a controversy that raged hotly at meetings and in scientific literature over the subsequent years. For a time there were more skeptics than believers.

Scientists search for the truth and want answers to questions that can be proved correct. The impact hypothesis attracted the attention of thousands of scientists from many scientific disciplines, each with its own specialized language. To solve the problem of interdisciplinary communication, a meeting was set up in 1981 at Snowbird, Utah, where specialists in different scientific fields learned from one another how their particular field related to the revolutionary impact theory. Thus began a unique scientific culture where scientists could question one another on the most basic principles of a discipline previously remote to them. The removal of these barriers made possible the progress toward our present understanding of the K/T extinction.

Attention next was focused on finding the crater that must have been excavated if the impact hypothesis is correct. Alvarez takes us through the various steps in chronological order, including all of the tricks of nature and stumbling blocks, up to the discovery of the impact crater at Chicxulub on the Yucatán Peninsula. He describes the mistakes and wrong inferences along the way as "a salutary lesson in humility."

T. rex is a very well written account of how long and tiring the road to the truth can be, how hard it can be to find answers, and how useful ones colleagues, family, and friends can be in finding the answers. This book should be read by all young scientists just starting out and also by the curious nonscientists. It describes the scientific method in an interesting and exciting way and demonstrates beyond doubt that many right (and wrong) questions must be asked on the road to discovery.

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NORTH FROM THE HOOK: 150 YEARS OF THE GEOLOGICAL SURVEY OF IRELAND.

Gordon L. Herries Davies. 1995. Geological Survey of Ireland. 342 p. Hardcover, £34.95, Stg£34.95, US$57.00. (Postage & Packing within Ireland is IR£4; IR£5 (Stg.£5, US$8.50) for surface mail delivery elsewhere.)

The Irish Geological Survey is almost as old as that of New York State; the former dates back to 1845 and that of New York to 1836. Davies, a distinguished earth scientist and historian of geology, tells the story of the survey over its life of 150 years. This book is well written and provides fascinating details; it clearly expresses the history of the Geological Survey which reflected the troubled history of the Republic of Ireland and its relationship with Britain.

Richard Kirwan (1733–1812), Ireland's most famous scientist, author of *Elements of Mineralogy* (1784) and *Geological Essays* (1799) had a plan for the *Management of Mines in the Kingdom of Ireland* which would have been a forerunner of the Geological Survey of Ireland, but it did not materialize. As early as 1808, Sir Richard Griffith (1784–1878) had produced the first geological map of Ireland. His map lacked a suitable base which, however, he obtained in the mid-1830's. A six-sheet geologic map followed, but this excellent map fell into oblivion, ignored and passed over.

The first local director of the official Geological Survey of Ireland was Sir Henry James (1803–1877) whose name is not listed in W.A.S. Sarjeant's Geologists and History of Geology. The Director was Sir Henry De La Beche (1796–1855), first director of the British Geological Survey. Henry James served for only one year (1845–1846) when he ran into problems with De La Beche. The latter was a difficult personality, whom empire builder and later Director of the British Geological Survey Sir Rodney Murchison (1792–1871) called "a dirty dog".

Author Davies recalls the constant struggle against lack of funds and staff, general neglect, and after independence, the economic plight of Ireland until just forty years ago. The years since 1959 have seemed more promising: drilling for coal (1959, 1960), publication of the 1:750,000 geological map of Ireland (1962), publication of the first number of the Geological Survey of Ireland *Bulletin* (1970), first Irish nationwide vertical aerial photographic survey (1973–1977), construction of Survey headquarters (1974), aeromagnetic survey (1979–1981), release of previously confidential exploration company reports (1983), survey moves into its new headquarters (1984), and publication of the first nine sheets of 1:25,000 Bedrock Geological Map of Ireland (1988).

In summary, this book makes fascinating reading. It is a tale of people who want to do good geology, but incidents of poor organization, human weakness, and even scandals in a famine-stricken land create hazards.

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MARY ANNING OF LYME REGIS.

Crispin Tickell. 1995. Lyme Regis Philpot Museum, Lyme Regis, Dorset, England. 31 p. Softcover, £2.99.

On the walls of the for-long-exclusively masculine environment of the Geological Society's rooms in Bur-

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lington House (Picadilly, London), formerly there hung only one portrait of a woman. It was an oil painting of Mary Anning, bonneted and crinoline-clad, but with a background of seacoast and strata. In her hand was shown the geological hammer with which, venturing forth from her home in Lyme Regis, she sought for Jurassic fossils along the coast of Dorset—a coast eroding so actively that new discoveries were always being made. By her side was her dog, whom she set to guard especially spectacular finds while going to seek aid in extracting them.

Well, like most other once-masculine establishments, the Geological Society's stronghold has long since been infiltrated by what has been called by Simone de Beauvoir "the second sex," its conquest being signified when Janet Watson became president in 1982. However, the fact that Mary Anning's portrait so long hung alone is a measure of the respect with which she was regarded as perhaps the finest and most fortunate fossil collector of the early nineteenth century.

Inevitably, she has attracted a series of studies, notably the series of papers by W. D. Land, published in the *Proceedinqs of the Dorset Natural History and Archaeological Society* between 1935 and 1963; a pamphlet by Richard Curle (1963); children's books by Helen Bush (*Mary Anning's Treasure,* 1967) and Ruth Van Ness Blair (*Mary's Monster,* 1975); and, most recently, a Presidential Address to the British Society for the History of Science (1995) which, it appears, serves as a prelude to a lengthier biography (see W. A. S. Sarjeant's *Geologists and the History of Geology* series for an extensive list of Mary Anning references). Moreover, though being neither scientist nor active in the cause of women's rights, her name is to be found in several recent studies of women in science (e.g., M. Alic, *Hypatia's Heritage,* 1986; P. Phillips, *The Scientific Lady,* 1990). All in all, she is one of Dorset's most renowned citizens of past times.

This new booklet on Mary Anning has an elegant introduction by John Fowles and a text of matching elegance by Sir Crispin Tickell, both of whom are patrons of the Philpot Museum in Lyme Regis. It sets right a long-standing misapprehension by stressing that her mother "Molly" Anning (née Mary Moore) carried out the fossil-collecting business after father Richard's death, Mary herself taking charge only around 1820. However, it was Mary who made the major finds, earning so great a posthumous fame that her mother's contributions were forgotten till Hugh Torrens (1995, *Brit. J. Hist. Sci.,* v. 28, p. 258–284) recently brought them back to memory.

All in all, this is a charming and refreshing account of Mary Anning, enhanced by its citation of comments on her by her contemporaries. She is indeed a fitting heroine for her own, or any other, age. As Tickell writes:

> If Mary Anning has become an Icon, she fully deserves it. But Icons need definition. She was no dainty heroine from children's tales, no controversial creature of fantasy, no mere local prodigy, no defender of women's rights, no prettified hand maiden of science. Instead, she was a tough, practical, complex, generous, sometimes prickly, independent-minded person of great intelligence, who surmounted the obstacles of her sex and circumstance to help lay the foundation of a new science of the earth.

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STRATA SMITH AND HIS STRATIGRAPHIC CROSS SECTIONS, 1819: A REVIEW OF FACTS WORTH KNOWING ABOUT THE ORIGIN OF STRATIGRAPHIC GEOLOGY IN THE MIND OF WILLIAM SMITH (1769–1839), AN ENGLISH COUNTRY SURVEYOR AND CIVIL ENGINEER. *J.G.C.M. Fuller. 1995. The American Association of Petroleum Geologists and The Geological Society of London. Poster 30 × 48 inches and explanatory pamphlet, 9 pages. $19.00.*

This unusual publication presents a facsimile of seven colored cross sections prepared by William Smith and published in 1819. The sections are presented at approximately 80% of their original size and are very faithfully reproduced using a dozen different colors. Whereas Smith's geological maps are well known, although not easily available, his cross sections are virtually unknown, at least outside Britain. Thus Fuller and the publishers are to be thanked for reproducing these beautiful sections—and at a surprisingly reasonable price. The poster was originally displayed, together with other Smith memorabilia, by Fuller (retired petroleum geologist) and Hugh S. Torrens (the University of Keele) at the annual meeting of the American Association of Petroleum geologists in Denver in 1994. The poster is an outstanding teaching aid and a fine piece of art to decorate offices, libraries, and museums.

In the accompanying pamphlet, Fuller has provided a very informative, yet concise, explanation of the cross sections. He indicates the origin of stratigraphic cross sections with English landowner John Strachey in 1719. It was Strachey who first drew scaled cross sections through strata near Bath, England, "much as an architect makes sections through a building." Strachey's sections, which were published by the Royal Society of London in 1719 and 1725, displayed coal-bearing strata near his home in southwestern England by using information from mines a few miles apart. He extrapolated between the data points to show the positions of the strata between mines. [In the center of Strachey's 1725 cross section, one can discern a clear fault offsetting the tilted coal measures "where they trap down or trap up from their regular course"; an angular unconformity with overlying flat strata is also clearly shown].

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Fuller makes a convincing argument that William Smith, when he was engaged 73 years later to survey the estate of a descendant of Strachey's, learned of the pioneering cross sections. Fuller first developed this argument in a paper in the *AAPG Bulletin* (53: 2256–2273). This experience must have inspired Smith around 1793 to make his own measured cross sections of strata found in the coal mines. At the same time, he was engaged by a group of mine owners to survey a projected canal intended for moving coal from several collieries in Strachey country south of Bath. In the course of this work, Smith became more and more aware of "that wonderful order & regularity with which Nature has disposed of these singular productions [fossils] and assigned to each Class its peculiar Stratum." In 1796, he wrote down on a scrap of paper that wonderful order of strata, which he had discovered. This back-of-an-envelope memorandum was never published, but in 1799 he dictated 23 names for divisions of strata with their thicknesses, characteristic fossils, and identifying lithological characteristics. By this time he also had begun plotting with colors on a base map where his different strata appear across the surface of the land. Fuller calls this a "stratigraphic map" and stresses that it was neither a soils map nor a mineral map. To me it was the first true geologic map, and I have long deemed it of the utmost historical importance to recognize that stratigraphy developed as a handmaiden of early geologic mapping and the making of cross sections. This was a fine product of expert surveying and cartography in that Smith's "delineations of strata" carefully displayed the sinuous traces of the "basal planes" across irregular topography. By 1801 he had extended his delineations clear across England on a small-scale map and had begun a manuscript to be titled *Natural Order of Strata,* which was never completed. Mapping was much easier for Smith than writing, and earning a livelihood left little time and only modest funds for his geological researches. Nonetheless, he used his maps and fossil collection to help cultivate clients for his civil engineering services for coal prospecting and water drainage control. At last in 1815 he published his hand-colored map at a scale of 5 miles per inch in 16 sheets and titled *A Delineation of the Strata of England and Wales, with part of Scotland.* In 1817 he published a *Table of British Organized Fossils* and in 1819 he published the *Geological Sections* that were reproduced in the 1995 poster.

Smith's original cross sections were presented on five separate, hand-colored sheets. He supervised the coloring very carefully because consistency was essential for easy discrimination of the strata and he wanted the colors on the sections to match those on his geologic maps; he also wanted them to match as closely as possible the actual color of the different strata. For further clarity, Smith numbered the strata starting with the topmost. His 1799 list had 23 named divisions, but by 1817 the list had grown to 33. The surface topography on the sections is portrayed as a three-dimensional drawing; that is, there is an impression of depth extending into the sheet as if one were looking beyond the plane of the cross section. The effect is very realistic. Another innovation was to show extensions of the strata below sea level in slightly darker shades than above. Surprisingly, there is no indication of scale, but Fuller determined that vertical exaggeration is about 25 times. Figure 1 of the pamphlet is an index map to the locations of each of the cross sections across southern England from Norfolk southwest and west to Somersetshire. In sequence from top to bottom on the poster, their approximate locations are as follows: *Section 1 upper* (57 miles long), *Section 1 lower* (56 miles) and *Section 3 upper* (56 miles) are all nearly east-west and lie north of the Thames estuary, mainly across Norfolk, Suffolk, and Essex respectively; the Chalk and adjacent strata are most prominent in these. *Section 3 lower* and *Section 4* extend north-south and almost join at London; *Section 3 lower* extends from London north to Cambridge, and is dominated by the Chalk and London Clay, whereas number 4 extends south to the English Channel at Brighton across the great Weald anticline; curiously, the anticline is shown as if its crest were as angular as a roof ridgeline, whereas the London basin syncline is broadly curved. *Section 2* extends southeast from Bath to the Channel at Southampton (52 miles) and shows Coal Measures (beneath Bath), Lias, Oolite, Chalk and associated strata. *Section 5* extends from Taunton in Somerset southeastward to the Channel at Christchurch and shows stratum no. 29 (Permian) and Lias, Oolite, and Chalk. Several of the sections show an "unconformableness" with missing strata. Soil characteristics appear in a few places, for example "stonebrash soils" on dry calcareous uplands such as the chalk, which are good for raising grains. A few fossils are also noted for certain layers.

On page 8 of the pamphlet, Fuller presents an interesting discussion of the implications of Smith's stratigraphic insights, which differ significantly from the conventional wisdom presented to most students when they learn about "Strata Smith" [to me a deplorable nickname, because it seems to trivialize the man]. Fuller makes two important points. First, strata had no relative age significance for Smith because he believed until 1817 that the entire surface of the earth had been formed by a single, instantaneous creative event, a last effect of which was the sculpting of the present landscape by the subsiding Deluge. Second, Smith believed that whatever had governed the natural order of strata, had also governed the occurrences of fossils within them. Fuller argues that whereas "particular fossils found in a stratum could be used to give individual identity to that stratum: *age and time are not involved in establishing identity.*" (emphasis added). Fuller is adamant that Smith's conception of the order of fossils and their containing strata had nothing to say about relative age or time, and to claim otherwise "misrepresents history."

How are we to take Fuller's assessment of William

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Smith's contributions to stratigraphy? In Smith's natural order, each named and numbered stratum occupied a relative position and, in preparing his maps and sections, Smith used the contained fossils as well as other lithologic attributes to identify each stratum. Does Fuller want us to speak only of relative position rather than age? Apparently so, but Fuller himself tells us that, by 1817, Smith had modified his view about the origin of strata so that each of his 33 named strata and its contained fossils ". . . must be considered as a separate creation; or how could the earth be formed *stratum super stratum,* and each abundantly stored with a different race of animals and plants" (Fuller, p. 8; from Smith, 1817, p. vii). Whether Smith was aware of the fact or not, this important modification was consistent with the contemporary and widely publicized ideas of Cuvier about the origin of strata and fossils.

Surely 33 separate creations producing *stratum super stratum* records a chronological succession of events! Whether Smith stated it or not, relative age of such a natural order of strata is implied. Further, if fossils differ from one stratum to another, as Smith so thoroughly demonstrated with his "imperishable achievement" (as Fuller has so aptly characterized it), then, because their containing strata lie *stratum super stratum,* they, too, lie *fossil super fossil* and also imply relative age. Is not a natural order also a succession reflecting relative age of deposition of strata and fossils? Whether he realized it or not, Smith clearly had discovered a criterion of relative age!

What of the old question of priority? Fuller notes the snobbish "barefaced piracy" of Smith's geologic mapping effort by the elitist, upper class barons of the Geological Society of London, who published the Greenough map in 1816 and bankrupted working class Smith in the process. That the geologic map's time had come is underscored by the fact that none other than Smith's nephew, William Phillips, pubished in 1818 a geologic map and cross sections with his *Outline of the Geology of England and Wales,* presumably with his uncle's approval. But what of the Brongniart-Cuvier geologic map of the Paris Basin published in 1811, which long appeared to represent an outstanding example of independent discovery? Rudwick (*ESH,* 1996, *15:*31) has recently argued that Brongniart, during an 1802 visit to England, probably saw a preliminary copy of Smith's map, thus likely gained not only the insights for making such maps but also the value of fossils for determining relative stratigraphic position. The English mineral surveyor, John Farey, promptly defended Smith's priority in the *Philosophical Magazine.*

Regardless of what the man himself thought that the "wonderful order and regularity" of strata meant, William Smith's imperishable achievement is now more accessible and more secure thanks to the republication of Smith's cross sections by John Fuller, The American Association of Petroleum Geologists, and the London Geological Society.

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LINNAEUS—THE MAN AND HIS WORK. *(revised edition). Tore Frängsmyr, ed. 1994. Watson Publishing International, Box 493, Canton, MA 02021. 232 p. Softcover, $19.95*

William Thompson (1827–1907), also known as Lord Kelvin, was certainly not a geologist, yet he was of concern to physical geologists during his lifetime and since his death has been a boon for historians of science. To step back more than a century, Carl Linné (1707–1778), a.k.a. Carolus Linnaeus, has also been a boon to historians of science, though he did not have the impact on geology in his era that Kelvin had in his time. In part, this may be because Linnaeus was a much broader scientist, or perhaps more accurately a natural philospher, and in part because the science of the earth was only just beginning to become a science during the Linnean era.

Nevertheless, there is material in Linnaeus worth studying, or, perhaps more accurately, studying those who have studied Linnaeus. Professor Frängsmyr has written one of the four chapters in this book, 46 pages on Linnaeus as a geologist, or, again perhaps more accurately, the concerns of natural philosophers at the times when Linnaeus was about and what this teacher added to earth science.

One area that Linnaeus contributed writings to was the geography of Paradise. The Garden of Eden obviously had to have water to flourish. If rivers are to flow and not be swamps, mountains are needed. If these rivers and mountains were located on this globe, one has to have an estimate of the number of rivers and mountains to match them to present-day locations. I confess that this is a field of geology I had never considered before, and it provides a bit more insight into what concerned many people at the time when James Hutton was getting his thoughts organized.

Another point of concern was diminution of the waters, and I confess that this is another item new to me. There is strong evidence in Scandinavia of former water levels far inland and higher than the present surface of the Baltic. Sea shells occur great distances from the sea; on the Island of Gotland, Linnaeus noted eleven lines of beach shingle above the present strand line. Perhaps the water in the oceans was condensing through time. No doubt this is a ridiculous idea, but hardly any sillier than the early 20th century idea that continents might move.

Linnaeus also commented on the sea stacks around Gotland and the various boulders of exotic origin sitting around the Swedish landscape. He was an astute observer and a clear writer describing what he saw. He also had some novel ideas on how rocks are deposited and stratified; in this instance, bizarre might be

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the better operative word. Still, this chapter provides insight into the mind of one of the important early naturalists. Certainly the ideas propounded here were not the same as those that influenced Hutton, but then again Hutton was not faced with the problem of diminution of the seas; isostatic rebound after ice melt could not come into consideration until after continental glaciation was seriously considered, half a century after Linnaeus ceased to write.

In addition to the editor's preface and his chapter, three other chapters cover different aspects of the career of this towering figure of a scientist. The first 63 pages consider Linnaeus as a public figure and as a private person. He was outgoing and much beloved by his students, yet he wrote multiple autobiographies to be certain that the importance of his contributions were recognized by posterity. Perhaps great men have not changed so much in two centuries.

The main contribution of Linnaeus was in the field of nomenclature and in his attempts to systematize the plant and animal world. Linnaean nomenclature may be the finest information retrieval system ever invented and it is worthwhile exploring its origin. These 47 pages also show what a keen observer he was of living organisms as well as the landscape, and they give a hint of some of his contributions to ecology and plant geography. The chapter also documents what a prodigious writer he was and shows the source of some of the ideas he expounded. It is a fine example of impressive scholarship.

The last chapter of 37 pages is concerned with the position Linnaeus gave to man in his classification. Incidentally, there is no type specimen of *Homo sapiens* Linnaeus; perhaps the late Professor Linnaeus, safely kept in the floor of the cathedral at Uppsala, Sweden, would be an appropriate choice. Where man should fit in—if he did not stand completely apart—as the various monkeys and apes were gradually discovered was a hot topic in the 18th Century. Linnaeus did not follow the politically correct course, but chose to classify man as an animal, despite his strong religious upbringing and his defence of the bible. He was an objective scientist and we should all be in his debt for taking a dramatic original step in classifying us.

The first edition of this publication came out in 1983 and this one has few changes. There may not be quite enough history of geology included to justify owning this book, but certainly the library at one's institution should have it. If one's colleagues in classical biology are not familiar with this work, they should be; tell them. It also would not hurt even to call this to the attention of the molecular geneticists to let them know from whence they came. If such rigorous investigators question my arithmetic, there are two short indexes (indices?), a list of contributors, and a dozen pages of illustrations.

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FROM COASTAL WILDERNESS TO FRUITED PLAIN: A HISTORY OF ENVIRONMENTAL CHANGE IN TEMPERATE NORTH AMERICA FROM 1500 TO THE PRESENT. *Gordon G. Whitney. 1994. Cambridge University Press. 451p. Hardcover, $69.95; Softcover, $32.95.*

An important aspect of the field of environmental history is the examination of the nature and results of Earth-human interactions and of the relationships between "natural" landscapes and cultural landscapes through time. Research in this growing field is necessarily interdisciplinary, calling on knowledge and methods of history (including history of science), ecology, geography/earth sciences, and other fields.

The environmental history of North America is a rich area of study that continues to draw scholars from diverse fields. In *From Coastal Wilderness to Fruited Plain,* Gordon Whitney evaluates the ecological history of what is now the northeastern and central United States from the period just prior to European settlement to the present day. Rather than presenting an environmental interpretation of human history, Whitney, whose research is in historical ecology and forest ecology, attempts to examine "history from the viewpoint of nature" by assessing scientifically and quantitatively the type and extent of ecologic changes in mid-latitude forests and grasslands of eastern and central North America. His goal is to focus on biotic elements (plant and animal communities) and abiotic elements of these landscapes to document both human-induced and other environmental change, as well as "man's" role as a geomorphologic agent. Whitney also explores to a lesser extent the role of the environment in the development of Euro-American attitudes towards, and uses of, these landscapes.

European explorers and colonists, who traveled through and settled the eastern half of North America, encountered vast woodlands and open grasslands inhabited and modified by Native Americans. The processes of settling, living on, and developing the forested regions and prairies resulted in extensive, dramatic, and in many cases rapid environmental transformation. For example, the clearing of forests, which took centuries in western Europe, took but decades in eastern America.

The ecological ramifications of the European (and Euro-American) encounter with and settlement of eastern North America have been the subject of recent studies in environmental history. One frequently cited example is historian William Cronon's *Changes in the Land* (Hill and Wang, New York, 1983), an ecological history of colonial New England that considers the ecological and cultural consequences of Europeans and Indians interacting in and with that landscape. *From Coastal Wilderness to Fruited Plain* is a significant contribution in that it not only extends the temporal and geographic coverage beyond Cronon's work, but

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also critically assesses a broad range of ecologic evidence to document the causes and extent of environmental change through time.

Whitney relies on extensive documentary and field evidence to evaluate environmental (primarily floral and faunal) changes. By compiling and examining accounts of early travelers and settlers, emigrant guides, scientific writings of the day, legal documents, local histories, and maps and land-survey records, as well as field evidence from archeological and palynological studies and analyses of old-growth forests, Whitney reconstructs the nature of forests and grasslands prior to European colonization as well as through time.

The book is organized into sixteen chapters. Following a photographic essay illustrating the nature of and changes in presettlement vegetation, the first chapter outlines the context of the book and provides a brief overview of how different disciplines have approached the study of "landscapes" and human-environment relationships. The second chapter ("Reconstructing the past") is valuable in itself as a primer on methods of and approaches to doing ecological history. The chapter describes and critiques the available source materials and discusses how they were used as evidence in this study. The third chapter ("Nature imposes") presents a generalized and simplified overview of the geologic and climatic setting of the eastern half of the continent. Chapter four ("The forest primeval") estimates the composition and structure of "baseline" forests and prairie lands prior to European settlement, and presents important species distribution maps.

The following chapters evaluate land use and associated ecological changes through time, particularly in the 19th century. They consider a wide range of topics on the environmental impact of human settlement and land use on a variety of scales, including the use of fire by Indians, European farming systems, logging practices and fuelwood cutting, links between deforestation and climate, causes and impact of soil erosion and changes in soil fertility, and the development of conservation ideas. In addition, the voluminous (106-page) bibliography is an excellent, comprehensive resource to those interested in the human and natural history of the region as well as the broad field of environmental history.

Although the general geology of the eastern half of the continent and the impact of agricultural practices on soil fertility and erosion are presented, *From Coastal Wilderness to Fruited Plain* does not really examine the role of physiographic or geologic setting in American land-use history (e.g., considering the geologic and topographic setting of, or influences on, settlements). The book focuses on the ecological history of forest and grassland areas, and it should be of great interest to students and researchers in a variety of fields concerned with the impact of human activities on temperate ecosystems or on landscapes in general.

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THE GREAT GEOLOGICAL DISCOVERIES. ESSAYS ON THE HISTORY OF GEOLOGICAL KNOWLEDGE, *v. 30. S. I. Romanovsky. 1995. VSEGEI Press, St. Petersburg, Russia. 215 p. Softcover (no price available)*

S. I. Romanovsky, a well-known Russian sedimentologist, historian of geology, and author of several scientific biographies of famous geologists, has written a new, important, and interesting book, *The Great Geological Discoveries.* By his own confession (p. 9), the title is a paraphrase of the famous book, *Great Geological Controversies* by Anthony Hallam (1983). Because Romanovsky's book is published in Russian (only its title page and content are in English), it is appropriate to briefly summarize its contents.

The book contains seven un-numbered chapters. The first two, "About discoveries, and a bit about psychology and history" and "Let's Look Back," are introductory. The five following discuss the five greatest geological discoveries–in my opinion, correctly chosen.

The chapter entitled "Going upstairs to the past" presents in detail a history of the knowledge of ancient geological processes. The author writes that it was Georges Louis Leclerc de Buffon (1778), who first advanced an idea of a directional development of the Earth. Then James Hutton (1785) pointed to an extremely long time for the operation of geological processes (Hutton's principle, as Vladimir Ivanovich Vernadsky had formulated). The next character of this plot was Charles Lyell (1830), who further advanced knowledge of ancient geological processes.

The chapter entitled "What? When? How?" deals with a discovery of the possibility to arrange geological events in chronological order—in other words, a history of stratigraphy. This chapter tells about the first general geological times scales (as we call them now) which were made by Johann Gottlob Lehmann (1750), Giovanni Arduino (1760), and Georg Christian Füchsel (1762), and also about the innovative works on biostratigraphy that were made at the turn of the 18th and 19th centuries by William Smith in England, and Alexandre Brongniart and Georges Cuvier in France. The chapter ends with a clear presentation of three main stratigraphic principles: a principle of transition layers, by Alexander Petrovich Karpinsky; a principle of discontinuities in local stratigraphic scales by N. A. Golovkinsky; and a principle of chronological interchanging of stratigraphic indicators, by S. V. Meyen. It is rather strange, however, that radiometric dating of rocks—which, in my opinion, has a direct relationship to the problem under discussion—isn't mentioned in this chapter.

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The title of the next chapter, "Where it thins, a curve occurs" is connected to a Russian proverb, "Where it thins, a break occurs." This chapter is dedicated to a history of the geosyncline-and-platform concept. The author notes that an origin of the geosyncline idea is connected with the names of James Hall (1859) and James Dwight Dana (1873); moreover, "if J. Hall found a big brait, J.D. Dana polished it and turned the find into a unique diamond" (p. 121). Then Eduard Suess (1875), "an unsurpassed master of global generalization" (p. 130), proposed a division of the Earth's crust of continents into geosynclines and platforms (using other terms, however), and Hans Stille (1940) presented the first systematics of geosynclines. As for platforms, it was A. P. Karpinsky who was a pioneer of their investigations. The author quotes some surprising lines from a private letter by V. I. Vernadsky, dating possibly around 1935: "Concerning geosynclines, I don't have a good feeling about them (i.e., I'm not sure I fully believe the geosynclinal concept). Where will they be after 10 years?" (p. 131). Indeed, after some decades the geosyncline-and-platform concept was replaced by the concept of plate tectonics, which is discussed in the next chapter.

This chapter is entitled "And nevertheless, they are moving." The author shares the judgment of Hallam (1983) that the plate tectonics concept is in fact the new geological paradigm. S. I. Romanovsky proposes two stages of development of the paradigm. The first stage (its prehistory, in fact) is connected with Suess's hypothesis (1883) of the existence of a single paleocontinent of Gondwanaland and a paleoocean of Tethys, and also with the great Alfred Wegener's continental drift hypothesis (1915). The second stage started in the middle of the 1950's. It is ironic that this great geological discovery (possibly the greatest in history) was made not by geologists, but by geophysicists and physicists. In a very short time, in 1967–1968, the main ideas of plate tectonics were propounded by Dan P. McKenzie, R. L. Parker, William Jason Morgan, Xavier Le Pichon, Bryan L. Isaacs, Jack Oliver, and Lynn R. Sykes.

The final chapter is entitled "Lessons of the biosphere." Vernadsky's innovative concept of the biosphere (1926) and its paramount importance for the development of geology is recounted here in an orderly fashion and with reasonable criticism. Vernadsky's holistic approach to the biosphere is stressed (but neither Vernadsky nor Romanovsky uses this term). The author's idea that the biosphere isn't merely an envelope which wraps the Earth, but is that common, which joins the atmosphere, hydrosphere, and lithosphere into a single whole, is very felicitous. At the end of this chapter, numerous distresses of modern civilization (in other words, those stages of biospheric development that V. I. Vernadsky, being an incorrigible optimist, has named the "noosphere") are enumerated.

Thus the whole history of the basic ideas of modern geology are ably summarized by the author in this rather small book. Portraits of the "main characters"—Nicolaus Steno, Mikhail Vasilyevich Lomonosov, James Hutton, Charles Lyell, William Smith, N. A. Golovkinsky, James Hall, James Dwight Dana, Eduard Suess, Alexander Petrovich Karpinsky, Alfred Wegener, and Vladimir Ivanovich Vernadsky—enhance the text. Unfortunately, as is typical for Russian printed matter, the book has no index.

The book is clearly aimed at geologists, geology students, and anyone interested in Earth sciences and who can read Russian. A wide range of discussed problems and their clear and colorful description are the main advantages of the book. Other advantages include the author's adequate evaluations of Russia's contribution to the problem under discussion. Being a patriot of his (and my) homeland, the author, nevertheless, shuns the exaggeration that was so typical of official science of the former USSR ("Russia is a birthplace of elephants," as Soviet dissidents used to joke back then). Disadvantages of the book are the somewhat non-obligatory character of the first introductory chapter and several repetitions which take place here and there. Another disadvantage is the lack of complete references (they are given as footnotes).

A brief explanation is in order for a non-periodic series "Essay on the History of Geological Knowledge" (which includes the book under review as v. 30), because it seems to be largely unknown in the West. This series was founded in 1953 and published entirely in Russian without English summaries. The contents have been published in English since v. 8 (1959) and title pages since v. 11 (1963). It should be noted that the English title of the series varies in different volumes: There are "contributions" instead of "essays," and "sciences" instead of "knowledge" in some cases. Several of the first volumes (volumes 1 through 8, and 10) didn't concern a particular subject. Special volumes were published later; these were dedicated both to outstanding Russian scientists [beginning with M. V. Lomonosov (1711–1765) and ending with N. S. Shatsky (1895–1960)] and to various geological institutions of Russia. Monographs and special issue concerning the history of development of different geological concepts and lines of investigation have also been published: *Development of the Idea of Geosynclines* by G. P. Chomizuri (v. 18, 1976); a collection of articles on *The History of the Geological Map* (v. 21, 1982); *Tectonic Correlations: History of Ideas* by I. G. Malakhova (v. 26, 1989); and *Gravitation and Geological Processes* by I. M. Sukhov (v. 29, 1994). Two volumes of this series are worthy of special note: a monograph *Geology the Academy of Sciences (from Lomonsov to Karpinsky)* by the late V. V. Tikhomirov, a founder of the series and its responsible editor until 1990 (v. 20, 1979); and a collection of brilliant historical geological "stories" *On the Silurian Plateau* by the late R. Th. Heckler (v. 24, 1987).

A famous Russian poet and essayist V. Khodasevich once wrote "Original poetical works live forever, because they have the ability to change human percep-

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tion." It is also true for classic scientific works. In this context, S. I. Romanovsky's *Great Geological Discoveries*—a fresh, contemporary view of the most valuable possession of modern geology—is very useful.

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THE CONNECTICUT VALLEY IN THE AGE OF DINOSAURS: A GUIDE TO THE GEOLOGIC LITERATURE, 1681–1995. *Nicholas G. McDonald. 1996. Bulletin 116, State Geological and Natural History Survey of Connecticut, Hartford. 242 p., Hardcover, $49.95, Softcover, $34.95. To order, call 860-424-3555.*

Although this volume is primarily what the title suggests, namely a bibliography of the geologic literature related to a portion of the Connecticut River Valley, the author's introduction provides a nice, succinct summary of the geology and geological nomenclature of that region. McDonald restricts his coverage to those parts of Connecticut and Massachusetts which are underlain by either Triassic or Jurassic rocks; extending from Long Island Sound at New Haven, northward through the Deerfield Basin and stopping at the Northfield Basin just south of the Vermont/New Hampshire state lines. On the west side he includes the isolated areas of the Pomperaug and Cherry Brook Basins.

Interest in the geological aspects of the region date back to the Dutch explorers as early as 1614. However, it was the discovery of economically viable precious minerals in North America that provided the impetus for extensive exploration of the Connecticut Valley during the late 1600's and throughout the 1700's. Copper, starting in 1709, and lead, by 1765, were successfully mined, while many ventures seeking other metals and coal were largely unsuccessful. But it was potential for economic exploitation that fueled the geological activity in the Valley, and McDonald's introduction makes this point quite well. He gives a special emphasis to the Portland Brownstone, which was shipped as far away as San Francisco via Cape Horn. In the part of the Introduction devoted to mining and quarry operations, he describes one of the first comprehensive geologic reports published about the Valley, the "Sketch of the Mineralogy of the Town of New Haven", by a young professor of chemistry at Yale, Benjamin Silliman.

McDonald gives the reader a brief history of the development of geological ideas by such well known people as Silliman, Hitchcock, Dana, Percival, and Davis, and he has a nice description of Silliman's "public" conversion to the "Plutonists" view of basalt and granite. There is a nice chronology of the important geological discoveries in the Valley starting with the 1614 Dutch explorations and stopping in 1983 with C. M. Kaye's discovery of the Middleton Basin north of Boston. And no description of the Connecticut Valley would be complete without the paleontological aspects and footprints, and McDonald, who did his graduate work on Jurassic fishes of the area, has a brief section on fossils and footprints.

The book is divided into three major sections, I: Introduction (pages 1–46; described above); II: Bibliography (pages 47–191); and III: Subject Index (pages 201–239), which refers users back to the Bibliographic section to the author(s) entry for the complete reference. The bibliographic entries follow the standard format with author/year/title/etc., and he has added to each entry the key words used in the Subject Index section. The entries are primarily published material (70% in serial publications and popular magazines—a list is found on pages 193–199) and the remainder are graduate and undergraduate theses, non-serial books, monographs, and reports, but McDonald did not include undergraduate reports, company reports, or newspaper articles. However, some unpublished manuscripts, if readily accessible, are included in the bibliography making a total of more than 4000 entries. Items are easy to find in the Bibliographic section, as they are listed alphabetically by author (last name), with the author's name (or names) in bold type and the rest of the reference indented so the name(s) are easy to read and locate.

McDonald's compilation provides a well prepared, single source in which to locate literature references for the geology of the Mesozoic rock portion of the Connecticut River Valley. In addition, he has a brief introduction to the areal and economic geology, and to the history of geologic ideas for the region. Anyone working in that part of New England or planning to take students there for field trips will find this a very useful volume to have.

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SOLAR SYSTEM ASTRONOMY IN AMERICA: COMMUNITIES, PATRONAGE, AND INTERDISCIPLINARY RESEARCH, 1920–1960. *Ronald E. Doel. 1996. Cambridge University Press, New York. 280 p. Hardcover, $59.95.*

In the years since the launching of Sputnik in 1957 and the establishment of the National Aeronautics and Space Administration soon thereafter, lunar and planetary science has become a well-recognized and vigorous discipline closely related to earth science. The development of lunar and planetary science in the past four decades has been well treated by historians, for example Joseph Tatarewicz in his *Space Technology & Planetary Astronomy* (1990). Many of these historians have repeated the assertions of lunar and plane-

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tary scientists that the solar system was generally neglected by astronomers and other scientists before 1957. By looking more closely at the history of solar system astronomy (a more inclusive term for the study of asteroids, comets, and meteors as well as planets and their satellites that was favored before the 1960s) in America, Doel strongly challenges this claim. As Doel makes abundantly clear, though, the nature of research on the solar system changed significantly during the 1950s. Whereas present-day lunar and planetary science (considered either as a discipline in its own right or as a subdivision of astronomy) is a well-defined field with its own societies, journals, meetings, and graduate programs, the earlier solar system astronomy was fundamentally an *interdisciplinary* field pursued by scientists who also conducted research in and received recognition from other disciplines.

Doel first examines a number of research programs in solar system studies pursued in the years 1920–1940 by astronomers who also had an interest in stellar and galactic astronomy. These programs include the vigorous investigations of celestial mechanics conducted at Berkeley, the studies of meteor orbits undertaken at Harvard, and the efforts of Henry Norris Russell at the Yerkes Observatory to understand the origin of the solar system. As Doel shows, these programs were supported at small yet stable levels by the philanthropic foundations that were the main source of scientific patronage before World War II.

As the federal government, and especially the military, replaced foundations as the main patrons of science after the war, scientists significantly changed their research interests in solar system studies. Doel first investigates this change in the area of planetary atmospheres, a topic of particular interest to aeronautical and missile researchers in the military. In the decade after 1945, a number of astronomers collaborated with meteorologists in order to understand the composition, physical characteristics, and general circulation of planetary atmospheres. These research programs proved to be relatively short-lived, as the instruments deployed in these studies were generally not capable of providing the information desired by military patrons.

Doel next tums to the collaboration of astronomers with geochemists in efforts to understand the chemical characteristics of planetary bodies and their implications for the origin and evolution of the solar system. He traces in considerable detail the work in planetary geochemistry conducted by Harold C. Urey and Harrison Brown at Chicago and subsequently by Brown and Claire Patterson at Caltech. As Doel convincingly demonstrates, their research was significantly shaped by their experiences with nuclear weapons work during the war. Doel then discusses the contemporary efforts of such astronomers as Fred Whipple and Gerard P. Kuiper to understand the origin of comets and of the solar system in general. At first, these astronomers collaborated enthusiastically with the geochemists; by the early 1950s, though, their relations soured over debates on whether astronomical or chemical methods of proof and explanation should receive precedence. These tensions climaxed in a notorious public confrontation between Urey and Kuiper, which was driven as much by personal disappointments as by scientific disagreements over the thermal history of the moon.

Doel next considers the collaboration between astronomers and geologists investigating the origin (volcanic or impact) of lunar craters and analogous terrestrial features. In the 1920s and 30s, this question was the subject of an interdisciplinary committee organized by the Carnegie Institution of Washington and headed by geologist Frederick E. Wright, which was unable to reach any firm conclusions. Immediately after World War II, an impact origin for these craters was strongly championed by astronomer Ralph Baldwin, who had gained considerable familiarity with explosives and ballistics research during the war. Subsequently, astronomer Carlyle Beals at the Dominion Observatory of Canada and geologist Eugene Shoemaker at the U.S. Geological Survey undertook concerted (and quite successful, in the long run) efforts to identify terrestrial meteorite impact structures. Shoemaker's research in particular was strongly affected by his close contact with Cold War patrons; assigned by the USGS to work with the Atomic Energy Commission, Shoemaker was able to make considerable use of classified data on craters created by nuclear test explosions which sharpened his understanding of the mechanics of cratering. Unfortunately, Doel does not give much sustained attention to the geologists who opposed impact interpretations for lunar and terrestrial craters. Moreover, given the time constraints of his study, Doel does not carry his story forward past 1960 to show how the work of Beals and Shoemaker may have laid the groundwork for present-day research on meteorite impacts and mass extinctions.

In his final substantive chapter, Doel discusses the transformation of solar system studies during the 1950s. In the decades before 1957, astronomers investigating the solar system had been part of a remarkably stable disciplinary community that had remained small in scale, both in terms of number of investigators and in terms of funding. In the 1950s, however, astronomy shattered as a coherent discipline as it exploded in size. The growth of astronomy was driven at first by the ambitions of the newly-established National Science Foundation and subsequently by the rise of NASA. As a result of these profound changes in patronage, so Doel argues, astronomy split into distinct subfields, and lunar and planetary science emerged as a distinct discipline in its own right. Moreover, with the field awash in money, work in this area now acquired the characteristics of big science: the discipline became dominated by large, hierarchically organized, and permanent research institutions run by managers concerned with keeping fickle patrons happy. As Doel points out, this transition to big science was quite traumatic for a number of established solar system astronomers, who had been quite comfortable with the in-

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formal and transient research collaborations funded in a consistent and predictable manner before the 1950s.

Doel's book highlights a number of important issues that need to be considered in any history of modern American science: the role of patronage (particularly the shifts from foundations to the federal government as the major source of funding after World War II); the rise of big science and its social and intellectual effects; the importance of transient institutions and interdisciplinary collaborations; the difficulties posed to interdisciplinary work by differing disciplinary standards and methods of explanation (which explains why so many of the collaborations were transient); and the often subtle role played by instruments and their capabilities in shaping the direction taken by research. In treating these issues, Doel has relied not only on wide reading in published primary and secondary sources, but also on extensive archival research (54 manuscript collections are cited) and numerous oral history interviews. This book is a model historical study that needs to be seriously studied by anyone interested in the profound changes that affected the earth as well as the planetary sciences in the post-World War II era.

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LIFE'S SPLENDID DRAMA: EVOLUTIONARY BIOLOGY AND THE RECONSTRUCTION OF LIFE'S ANCESTRY 1860–1940. *Peter J. Bowler. 1996. University of Chicago Press, Chicago and London. 525 p. Hardcover, $37.95.*

The study of the history of evolution is alive and well. One of the most prodigious workers in Darwin's vineyard has written another book, though it is not one to keep on the nightstand to read a few pages before nodding off. Professor Bowler stomps a mean grape in the vineyard and wrings out every last drop of juice. One is forced to move slowly through the underbrush of scholarship, but the trees are never obscured.

A few minor points ought to be noted. First, for anyone interested in the history of geology, a detailed study of history of evolution is ancillary. This is like being interested in radioactive dating and as a result reading a biography of Madame Curie. It never hurts to broaden one's background, but do not expect to understand the nuances of conodont zonation or other biostratigraphic principles after going over the text. Second, the subtitle is a little misleading. A brief history of evolutionary theory is: a lot of noise and some light during Darwin's day; then a reaction against his ideas; next came "neo-Darwinism"; a time of the doldrums; and in the 1930s, THE NEW SYNTHESIS, which is an amalgamation of genetic, ecology, classical Darwinism, and a few other elements. Prof. Bowler wanted to get to the start of the synthesis, but he probably should have stopped at that great watershed of the first world war, for the few comments and references to work in the 1920s and 1930s should have been explored in the same depth given to the 1860–1910 interval.

There are nine chapters; the introductory one is a little short, but the others are nearly equal in length. Their titles give a flavor of the approach: The first evolutionary biology; The tree of life; Are the Arthropoda a natural group; Vertebrate origins; From fish to amphibian; The origin of birds and mammals; Patterns in the past; The geography of life; and The metaphor of evolution. One particularly nice feature is a 15-page appendix in which those mentioned in the book are given thumbnail biographies.

For geologists such as seismologists or tectonicists, not directly concerned with fossils and perhaps not particularly interested in them, the chapter on geography might provide some insight. For more than half a century after Darwin's "Origin," biologists built and sank land bridges to help explain various attributes of the distribution of living organisms. It is appropriate today to smirk at such ignorance, but many of the proposals did have a solid basis of facts that needed explaining. Smirkers should recall that a Permian freshwater reptile was one reason for surmising that Africa and South America might have been united.

The chapters are arranged in a systematic and presumably phylogenetic manner. Taxonomy, with concepts of phylogeny, embryology, form and function woven in, leads to the chapter on invertebrates. The invertebrates never receive the attention that they deserve in histories of biology, but Peter Bowler has at least included a significant piece on views as to what were thought to be the roots of this largest invertebrate phyla. He touches on the eurypterids and the trilobites, the larval stages of Crustacea, and other important developments in trying to work out the history of this group. One need only read a little about the Burgess Shale fossils to realize that issues have not all been resolved.

The remaining series of chapters discuss in detail the various ideas which have influenced studies of "higher organisms." After Darwin, the earlier notions of the great chain of being, or the ladder of life, gave way to the model of the tree of life, which became increasingly bushy (in my view, phylogeny now is closer to the model of a field of grass with large numbers of independent stalks). There is much to be learned here about the rise and fall of idea in biology, and the general patterns of change may well be applicable to other fields of history of science. There is simply too much to summarize, except to note that a number of complex and disparate ideas are woven into a coherent style.

Regardless of how Darwin changed the world view, and man's place in nature, any mention of "higher organisms" brings human beings to mind. They do not appear in this book, for Professor Bowler has an earlier work dealing with us—in the historical biological sense. He has included in this book an enormous cast of characters. One might think that the book would be devoted

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primarily to British scientists, and in fact it is, but there is no chauvinism. Those from other countries and continents are skillfully woven into his tale of how the search for the history of organisms changed biology.

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THE LIFE AND LEGACY OF G. I. TAYLOR. *George Batchelor. 1996. Cambridge University Press, New York. 285 p. Hardcover, $75.*

The common geophysical fluids are air and water. In the nineteenth century, the study of moving fluids was mostly hydrodynamics. Lamb's classic book by that name is still in print. In the twentieth century, the study of moving fluids is mostly fluid mechanics. *The Journal of Fluid Mechanics*, Founded by Batchelor, remains the prestige publication in this field.

Fluid mechanics differs from hydrodynamics in its treatment of viscosity, the fluid property that leads to shear, mixing, turbulence, particle transport, water wave generation, and many other geophysical phenomena. Viscosity is an essential fluid property in fluid mechanics, but viscosity does not exist in hydrodynamics.

In the twentieth century, there have been three giants in fluid mechanics: Ludwig Prandtl (1875–1953), Theodore von Kármán (1881–1963), and Geoffrey Ingram Taylor (1886–1975). *The Life and Legacy of G.I. Taylor* is the biography of the last of these three, by Taylor's student, colleague, and admirer, G. K. Batchelor.

The 'Life' part of this book (chapters 1–8, 10, 13, 17) contains lengthy excerpts, enlightened by Batchelor's personal knowledge, from Taylor's retrospective writings, some previously unpublished. These chapters, along with the Preface and the four appendixes, should be accessible to the historians and earth scientists who read this journal. The 'Legacy' part of this book on fluid mechanics (chapters 9, 12, 14–16) contains Batchelor's compact paraphrase of Taylor's technical results. The reader not already familiar with fluid mechanics will benefit less from these chapters. The last chapter on 'The Scientific Legacy of G. I. Taylor' is text for both classes of readers to appreciate. (About one-fifth of Taylor's published work concerns the mechanics of solids, particularly plasticity and dislocations, on which he was a pioneer investigator. Brief discussions of this work by Rodney Hill, Neville Mott, and Taylor himself constitute Chapter 11).

On his mother's side, G. I. Taylor was the grandson of George Boole, the self-taught mathematician whose logic has influenced the development of modern computers, and Mary Everest, the niece of the man for whom the mountain is named. The father of G. I. Taylor was a painter, Edward I. Taylor, of London who designed stained glass and decorated the interior of large public rooms, particularly those on passenger liners. Taylor's paternal ancestors were not well known prior to this book which includes new information discovered by Batchelor's wife.

In 1905, Taylor began at Trinity College, Cambridge University, as a student of mathematics, and he was 22nd Wrangler in Part I of the 1907 Mathematical Tripos. He then transferred to Part II of the Natural Sciences Tripos and studied physics in the Cavendish Laboratory. He obtained first class honors in the final examination of 1908 and was awarded a major scholarship at Trinity College that started him on a lifelong career at Cambridge University.

Particularly in his early years, Taylor gained unusual field experience to guide his physical intuition. From boyhood to old age, Taylor was an avid and an honored sailor, a pursuit that put him at the air-water interface that figures prominently in his research. The sinking of the Titanic led to Taylor serving as the meteorologist on a six-month expedition to the north Atlantic aboard the Scotia, a wooden sailing ship. Taylor's best known contributions to fluid mechanics are those involving turbulence, and this trip gave him field experience and perhaps time to think. (A 1961 published compilation of 12 classical papers on turbulence from the first half of this century has papers numbered 1, 3, and 5 by Taylor, and paper number 2 is a comment by Richardson on Taylor's work). But the voyage on the Scotia was not the equivalent of the naturalists' voyages in the nineteenth century. The introduction of the airplane into World War I led to Taylor learning to fly the primitive planes of the day in order to gain insight into his research on problems of military aviation.

Productive formal research began with his return to Cambridge after the war, "it was as if a coiled spring had been released." The three problems he addressed first are important geophysical phenomena: tidal friction, rotating fluid systems, and the stability of steady flow between rotating concentric cylinders. Results from his work on tidal dissipation in the Irish Sea (1919, 1920) entered into Harold Jeffreys' explanation for the observed gradual increase in the length of the lunar day.

In four papers (1921–1923), he worked on rotating fluid systems, a field much investigated in the literature of geophysical fluid dynamics in recent decades. Batchelor says of this work by Taylor that it "possibly has been given more praise than, strictly speaking it deserves, for in fact these papers do not do all that appears to be claimed . . ." The third subject in this series was "G. I.'s grand and definitive investigation of the stability of steady flow between concentric circular cylinders in relative motion." (p. 86).

Because turbulence is Batchelor's field, the reader with a background in fluid mechanics will benefit especially from a close review of Chapter 12 on Taylor's turbulence research. After what would constitute a remarkable research career for the normal biographee, Taylor 'retired' in 1951 at age 65, and in the next 21 years, "he wrote 48 scientific papers, most of which are substantial and some of which are pure scientific delight." (described in Chapter 16).

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His technical work is well-documented, but what sort of a man was he personally? In his science, Taylor always looked for physical evidence to confirm his theory. This book presents at least superficial physical evidence about Taylor, the man, in the form of excellent photographs and revealing letters. From the photos, Taylor was a handsome man in his prime, and he aged gracefully. Evidence from the letters is mixed. A seven-page letter reproduced here in manuscript (p. 100–101) was written as a young boy while on holiday. The letter shows the boy to be a keen observer, an enthusiastic sailor, and aware of his father's feelings. A two-page note (p. 256–257) was written at age 74 after participating in a lecture series with Robertson, Fowler, and Bardeen. In it Taylor vows with enthusiasm to learn some more physics. In between, there are a pair of letters (p. 93–94) written at age 38–39 to the woman he would marry. They seem pretentious and indirect, entirely different from the style of his science. Batchelor says of one letter, with understatement, "the logic of this letter may not stand up to analysis."

Taylor approached an experiment after understanding the empirical facts, making appropriate approximations, and deriving a testable prediction from theory. The point of the experiment was to verify the result of which he was already reasonably sure. He also derived considerable aesthetic pleasure out of the confirmation of theory with experiment. Batchelor observes that Taylor's experiments sometimes agreed better with theory than they had a right to, given the limitations on both theory and experiment. When Batchelor pointed this out to Taylor, "he did not like it at all." (p. 62).

As a person, "He was not reflective, and moral or philosophical issues did not often engage his mind" (p. 210). "He had a razor sharp mind but it was not much used on political issues, social affairs, teaching or serious literature . . . . Such men do not normally make interesting companions, and he was certainly not a good conversationalist" (p. 254). The implication is that G. I. Taylor was the better scientist because of, not in spite of, these characteristics.

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NATURAL HISTORY AND DEVELOPMENT OF THE TRANSMISSISSIPPI REGION. *Transactions of the Kansas Academy of Science. D.F. Merriam, ed. v. 99, no. 3/4, December 1996, p. 66–68, available at $20 (including shipping and handling) from Dr. Pieter Berendsen, Kansas Geological Survey, University of Kansas, Lawrence, KS 66047*

This issue of the *Transactions of the Kansas Academy of Science* is composed of two parts, (1) a symposium on the natural history and development of the Trans-Mississippi Region, and (2) the publication of technical papers. This review concerns only the symposium.

This symposium, held in 1993, consists of six papers, most of which focus on geology; others look beyond Kansas and/or are non-geologic. P.D. Thomas relates his efforts to Thomas Jefferson (1743–1826) who served as catalyst for initiating early Kansas exploration, especially the Meriwether Lewis and Lewis and Clark expeditions. D.J. Blakeslee discusses the role of Native Americans, D.F. Merriam is concerned with 19th century geologic maps of Kansas, and M.E. Nelson with the geologic data of the Kansas Academy of Science for 1868–1879. R.J. Zakrzewski traces geologic studies in western Kansas in the 19th century, and L.E. Page describes the life story and accomplishments of George Clinton Swallow (1817–1899), state geologist of Kansas.

Since we celebrate this year the 200th anniversary of the birth of Sir Charles Lyell, I checked in D.F. Merriam's paper on 19th-century maps of Kansas to see what Lyell's contributions included. Lyell's 1845 map was compiled as a result of his travels in North America. His map includes Kansas as part of the western extension of the Ozarks and shows granite and gneiss overlain by Upper Silurian (of the time) Niagara, Clinton, Onondaga, and Helderberg equivalents. Obviously the sources from the Kansas area were meager, but Lyell's map is the first to show Kansas geology with a classification scheme of names that are currently in use.

R.C. Buchanan in his introduction points out that geologists who are concerned with one of the most historically oriented branches of the sciences have little knowledge of the history of their own discipline, thus "they tend to know little about their predecessors on the plains." These transactions provide background on the history of geology of Kansas, so that "later generations of scientists will know something about the shoulders on which they stand."

These papers add to the published literature on the geology of Kansas and complement and supplement special issues on this subject in *Earth Sciences History.*

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Correction: The price of the book: *Horace-Bénédict de Saussure: Forerunner in Glaciology*, by A.V Carozzi and J.K. Newman, is 65.00 Swiss Francs. It may be ordered prepaid from Éditions Passé Present, Case Postale 483, CH-1211, Geneva 13 (Switzerland). The prepaid price includes surface postage and handling. The price was inadvertently omitted from the review that appeared in ESH Vol. 16, #1, 1997, pp.52–53.

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INTERESTING PUBLICATIONS

Gerald M. Friedman, EDITOR

Since the start of this journal, Editor Gerald M. Friedman has prepared this column. Contributors wishing to list recent books and papers of interest to our membership are requested to send them to the Founding Editor Gerald M. Friedman, Department of Geology, Brooklyn College and Graduate School of the City University of New York, c/o Northeastern Science Foundation affiliated with Brooklyn College of the City University of New York, Rensselaer Center of Applied Geology, 15 Third Street, P.O. Box 746, Troy, NY 12181-0746

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HESS SECRETARY'S REPORT FOR 1997

The principal news for 1997 was the election of a new slate of officers and the passage of a new amendment. The election resulted in overwhelming victories for all of the following officers:

President Elect	Kennard B. Bork
Secretary	Ronald Rainger
Program Officer	Bruce Hevly
Editor Earth Sciences History	Mott T. Greene
Councilor	Silvia F de M. Figueiroa

The membership also approved by a margin of 105 in favor, opposed, and 1 abstention, an amendment to the By-Laws that will increase the number of councilors from 2 to 4. That change, which will begin with the 1998 election ballot, will implement a system of annual staggered elections for a 4 member council. It is hoped that the increase in the number of councilors will more effectively promote the society.

The change in officers brings to a close Hatten Yoder's term as president. During his two years Hat not only played a central role in coordinating and overseeing the society's development, but took an active and personal interest in all its activities. Hat took the lead in advancing a number of new initiatives, including the proposal for increasing the number of councilors. For me it was a great pleasure to work with someone who expressed so much interest, enthusiasm and dedication to HESS and its objectives. Hat continues to remain very much involved with HESS and its endeavors, and the society owes him its gratitude for his two years as president.

The society continues to maintain a sizeable membership. We currently have 360 members, and continue to add new members on a regular basis.

During the year the secretary, with considerable help from the treasurer, Dorothy Sack, completed a new HESS Membership Directory. In the effort to keep up with new developments in technology, the new directory includes email addresses and fax numbers. If any member did not receive a copy of the directory, please get in touch with the secretary. In addition the secretary and the treasurer arranged for the exchange of brochures and fliers between HESS and the Society for the History of Natural History. Again any member who is interested in the SHNH and did not receive a copy of its brochure, should get in touch with the secretary.

Respectfully submitted,
Ronald Rainger
HESS Secretary

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