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Book Reviews / Critique

Book Reviews

Notions de Géologie

By Bruno Landry and Michel Mercier
Modulo Editeur, Mont-Royal, Québec
Third Edition, 565 p.

Reviewed by Gerard V. Middleton
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This is the third edition of an introductory text in geology for students in Quebec. (The first edition, published in 1983, was also reviewed in *Geoscience Canada.*) The work is the only modern introduction to geology written specifically for Canadians, albeit for francophones only. It strikes me as a very polished and balanced work; it provides treatment of most of the traditional topics included in freshman texts. What is particularly interesting is the extensive, indeed almost exclusive, use of well-documented and illustrated Canadian examples, with the great majority drawn from the province of Quebec.

Is it too much to hope that anglophone instructors will at least draw this book to the attention of their students? If there are any universities that still cling to doctoral language requirements, reading through this text might also serve graduate students double-duty as a preparation for both qualifying and language exams.

Oil Sands Scientist. The Letters of Karl A. Clark 1920-1949

Edited and with an introduction by
Mary Clark Sheppard
Foreword by Robert S. Blair
University of Alberta Press, Edmonton
xvi + 498 p., 1989, $30.00

Reviewed by William A.S. Sarjeant
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Ever since their discovery by a Geological Survey of Canada (GSC) field party in 1875, the Athabasca oil sands have been a focus for scientific and economic speculation. The story of the attempts at their exploitation is one of the encountering of successive frustrating problems, the solving of each of which merely served to generate another. The amount of money sunk into what seemed a most promising commercial endeavour, only to be lost in a morass of difficulties as sticky as the sands themselves, was to be enormous. This whole story is both a depressing and a heartening one — heartening, because of the persons who refused to be defeated and tried again and again to overcome these difficulties.

Foremost among the scientists striving to extract petroleum of good grade from the sands was Karl Adolph Clark (1848–1966). Clark was born in Ontario, owing his names not to any German descent, but to the fact that his father, a professor of languages at McMaster University, specialized in that language. Clark himself qualified as a chemist at McMaster and thereafter at the University of Illinois. His involvement with geology was a consequence of an appointment during the First World War with the GSC (his eyesight disqualified him from military service). It accorded well, however, with a deep love of the outdoors. In ensuing years, opportunities for field work were always relished.

Clark's work for the Survey included the consideration of how outcroppings of clays caused problems with roads in Manitoba. He recognized that wet bentonite particles were a prime problem and that waterproofing would be a solution. Since oil was a natural water repellant, could the Athabasca tar sands provide a suitable source for the waterproofing substance? So it was that Clark began the investigations that were to occupy most of his research life, at first in the laboratories of the Survey in Ottawa and afterward in the Research Department (later also in the Chemistry Department) of the University of Alberta. This work was done in association with assistants and colleagues within or without the University, and partly in profitable co-operation, partly in competition, with the successive commercial concerns that turned hopeful — but usually, soon to be disillusioned — eyes upon the Athabasca sands. Although Clark was never quite to develop a technique that would enable exploitation of the oil sands to yield the sizable profits so long anticipated by proponents of oil sands development, he did contrive to render it marginally profitable. Moreover, his researches during almost 30 years contributed massively to the understanding of the nature of the sands and laid the foundations for the eventual breakthrough.

The story is a fascinating one, well told, with an elegantly written introduction by Clark's daughter Mary, and well bolstered by a carefully assembled documentation, supplemented by a rich array of historic photographs — some 120 of them — and three clear maps. The book is well produced and moderately priced. It deserves a place on the shelves of all persons interested in the history of Canada's petroleum industry and its technology.

Great Geological Controversies, Second Edition

By Anthony Hallam
Oxford University Press
xii + 244 p., 1989, $31.95

Reviewed by W.A.S. Sarjeant
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The first edition of this work was reviewed by me eight years ago (*Geoscience Canada*, v. 11, p. 213-214). In that original work, five controversies were considered — the confrontation between "Neptunists, vulcanists and plutonists"; the question of "Catastrophists and uniformitarianism"; the emergence of concepts of "The Ice Age" and of "The age of the Earth"; and the battleground of "Continental drift". This second edition reviews two further controversial themes, "The emergence of stratigraphy" and "Mass extinctions". Minor additions and amendments are made to earlier chapters, but, alas, none of the specific criticisms I made in 1984 have been properly answered; even the misquotation of Archbishop Usher's findings, at the beginning of what is now Chapter 5, stands uncorrected.

In this review, I shall concentrate on the two new chapters. Hallam's account of the emergence of stratigraphy arouses mixed emotions. It contains an excellent précis of the quite complex sequence of events surrounding the emergence of a nomenclature for the Early and Middle Paleozoic systems (Cambrian to Devonian). Moreover, his characterization of the protagonists is careful and, in the case of that egotistical puller of all possible strings, Sir Roderick Murchison, perhaps even unduly gentle.

On the other hand, I regretted discovering that Hallam had allowed himself to be recruited to the "Let's-all-bash-William-Smith" school, apparently through swallowing whole the too-highly-spiced broth of Rachel Laudan's comments (1987) without also reading the responses they have evoked. Hallam appears unaware that (a) Smith completed his first geological map of England and Wales as early as 1801; there is a copy in existence; (b) the published map was being continuously revised, so that the various copies surviving from the 400 or so that were printed show repeated updatings, as Smith's knowledge grew; (c) in dealing with areas he had not visited — and, after all, he was a professional surveyor, undertaking the geological mapping as a self-imposed task in his limited spare time — Smith had to rely upon reports from other geologists whose work, very often, was less careful than his own; and (d) Greenough's map of 1820, vaunted by Hallam as "showing much more accuracy in most areas" than William Smith's, drew very heavily upon information from Smith, indeed utilizing data that Smith had not published. Moreover, the claim that "most British geologists learned their mapping from the continental tradition" (p. 67) can scarcely be maintained when most of those geologists commenced their work after 1815, more than a dozen years after Smith's concepts had come to be widely known in Britain (see Woodward, 1902; Sheppard, 1917; Cox, 1948; Eyles, 1969). Hallam should ponder, in particular, Joan Eyles' response (1979) to Rachel Laudan's strictures — the judgement of a lifetime of scholarship.

I am puzzled also that, though Hallam sets out so unpatriotically to claim for Continental Europe an entire priority in early stratigraphical discovery, he says nothing of the pioneer work of Giovanni Arduino of Italy and does not mention the anticipation of Smith's recognition of the value of fossils as stratigraphic indices by the French geologist J.L. Girard Soulavie. However, Hallam is scarcely to be blamed for his inadequate assessment of the attainments of Alcide d'Orbigny; though Michel Rioult's careful study of d'Orbigny's concepts has been published in both English and French (1969, 1971), it has attracted far too little attention from geological historians.

For Hallam's other new chapter on "Mass extinctions", I have high praise. This is detailed and judicial. The arguments in favour of an extraterrestrial event at the Cretaceous-Tertiary boundary are properly assessed and compared with the contrary arguments for volcanic causes of such features as the iridium anomaly. Hallam properly points out the lack of supporting evidence from fossils for any cataclysm of short duration. The questions presented by Hallam (p. 208) deserve to be considered and answered, so that a final *finis* can be written to this particular attention-consuming, and ever more absurd, controversy. However, he does not pose one of the questions that should be asked of enthusiasts for that idea: Why is it that *nowhere in the world* has the iridium layer been found resting upon the bones of the vertebrates, or the shells of the invertebrates, that the impact or the supernova (or whatever) is supposed to have wiped out?

Whatever one's personal view of Hallam's conclusions, his writing stimulates even when it irritates. This is desirable reading then, for historians of geology and, indeed, for geologists at large.

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Petrology of Lamproites

Roger H. Mitchell and Steven C. Bergman
Plenum Press, 447 pages, U.S. $75.00

Reviewed by Tony Peterson
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If there is a "last frontier" to be explored in igneous petrology, where ascent paths are unmapped and hypotheses run wild, it lies in the realm of alkalic rocks of deep upper-mantle origin. Detailed physical and thermodynamic models exist for more common magmas, from basalts to granites, and it is now possible to calculate with reasonable accuracy the life history of a batch of basaltic magma from its birth in the mantle to its death on the surface. In contrast, petrologists still have a difficult time agreeing even on the *names* of certain alkalic rocks, let alone which ones are primary and which derivative, which are mixed and which unmixed, and so on.

Of the two branches of alkalic rocks — the sodic and the potassic — the potassic branch has been by far the most vexatious, and the one most burdened with misconceptions and taxonomic inconsistencies. The legacy of Joplin's 1969 paper on the "shosonitic association", into which was lumped virtually every potassic rock known on Earth, is still with us. Geologists persist in recognizing this association in their field areas, despite the fact that petrologists have gradually stripped other potassic rocks away from it until nothing remains but .... shosonites. Not uncommonly, one reads of rocks termed "shosonitic lamprophyres", a clear contradiction in terms, since shoshonites contain phenocrysts of feldspar and lamprophyres, by definition, cannot.

Not surprisingly, a situation like this breeds petrologists determined to set things right. One such is Roger Mitchell, who has made it his business to make the study of potassic alkalic rocks a — let us not be afraid to say it — more scientific one. Whether he has always succeeded is a matter of opinion (and opinions differ), but there is no doubt that *Petrology of Lamproites* is his *magnus opus* in this regard. Steven Bergman, the second author, has made a significant contribution (writing chapters 3, 4, 5 and 9 in this 10 chapter book, as well as numerous papers in journals), but the tome bears Mitchell's mark throughout.

Lamproites are ultrapotassic rocks (taking the average of all authors' data, this means $K_2O/Na_2O \geq 3$ by weight, with high K_2O and MgO) strongly enriched in incompatible elements and depleted in CaO and Al_2O_3. They are usually rich in one or more of the minerals phlogopite, leucite, sanidine, clinopyroxene, olivine and high-K, high-Ti richterite, and are often glassy. Groundmass phlogopite tends to be very rich in tetraferriphlogopite (an effect of high K/Al, not high fO_2), and minor K-Ba-Ti-Zr phases such as priderite and wadeite are common. Lamproites occur in virtually all intrusive and extrusive forms, and are found exclusively on continents. They are so rare that every known occurrence is readily catalogued (there are about 25 of them, allowing for some ambiguous cases), but they have an economic and scientific importance out of all proportion to their volume. The diamond deposit with the highest known grade is lamproite hosted (Argyle, in Australia; ≥700 ct/100 tonnes). It is clear from the provenance and isotopic composition of lamproites that they are generated in very old subcontinental mantle with a complex history. Alkaline rock petrologists are fond of stating that their rocks are windows into the subcontinental mantle's past; in many cases the claim is dubious, but it cannot be doubted for lamproites.

The book's purpose is to summarize the state of current knowledge of lamproites, and to set the record straight on things potassic. Chapter 1 reviews the recognition and classification of lamproites, which have been incremental since 1870. Much of the confusion originates from the petrographic variability of potassic rocks (the crystallization of leucite, sanidine, phlogopite and olivine are strongly dependent on total pressure and volatile content) and the wide range in certain geochemical parameters that lamproites exhibit. It is important to note that lamproites, as Mitchell and Bergman define them, are a "clan"(as opposed to a rock type) which includes differentiated as well as closely related, but distinctive, primary magmas. The relationship of lamproites to other potassic rocks, such as kimberlites, Roman Province leucitites and shosonites, is detailed in Chapter 2; geologists who encounter potassic rocks in their studies would do well to read it. This material is greatly enhanced by 22 colour photographs. This chapter closes with formal geochemical and mineralogical criteria for placing a rock within the lamproite clan.

The geology of all known lamproite occurrences is summarized in Chapter 3, with inferences about the tectonic environment of lamproite magmatism made in Chapter 4. What is clear from the distribution of lamproites in space and time is that major tectonic events, such as rifting or subduction, have no necessary connection with active lamproite volcanism. In this, lamproites contrast with Roman Province potassic rocks and shoshonites (clearly subduction-controlled), and kamafugites and the great majority of sodic alkalic rocks (rift-related). Triggers for lamproite volcanism are apparently highly variable, since lamproites can erupt within such strongly contrasting environments as the periphery of hotspots or other mantle upwellings (western United States, Antarctica), and the hinterlands of collision zones (Spain, Corsica). But it is well established that lamproites are derived from subcontinental mantle affected by old enrichment events — typically, subduction-related metasomatism — and that this mantle was previously depleted by even older extraction of basaltic melts. This makes lamproites unique and important probes of the subcontinental upper mantle.

Chapter 5 describes the observed facies of lamproite volcanism; perhaps it is overlong or even unnecessary, since these facies (flows, lava lakes, dykes, pyroclastic flows) are common to basaltic rocks and familiar to most geologists (an exception might be breccia pipes). Chapter 6 is a detailed and exhaustive catalogue of the mineralogy of lamproites, and logically forms the centerpiece of the book, since it comprises the bulk of Mitchell's contributions to the field. The distinctive geochemistry of lamproites is presented in Chapter 7, and experimental data bearing on the origin of lamproites, in Chapter 8. Chapter 9 discusses diamonds and xenoliths in lamproites, and Chapter 10 summarizes the authors' views on the petrogenesis of lamproites.

This book is nearly the best conceivable that could be written on lamproites at this time. Should anyone care? After all, lamproites are extremely rare rocks, which few geologists will ever encounter, except on dedicated field trips. In my opinion, the answer is an emphatic *yes*; this book belongs on the shelf of any igneous petrologist concerned with continental magmatism (not to mention anyone with an interest in diamond-bearing rocks). The issues regarding lamproite generation touch upon most aspects of the evolution of the subcontinental upper mantle, and that alone is enough. But this is the first book — or even review paper — which succeeds in rationalizing the wide variety of highly potassic, mafic igneous rocks since the broad pattern of that variety became evident when kimberlites were subdivided into three completely different rock types, nearly ten years ago.

That is not to say that I find no faults in the book. Some portions (notably Chapters 1, 2 and 8) are written with an ungenerous attitude to previous workers that is decidedly uncomfortable to read. The index is less than five pages long, which is merely adequate. Due (in my interpretation) to a long-standing, often rancorous disagreement with Nicholas Rock, who preferred to place lamproites into a lamprophyre basket with a host of unrelated rocks, the authors accord minettes hardly any status in the book (except, here and there, to note that many minettes are puzzlingly similar to lamproites). For example, on page 405 the authors state, "We thus regard the statement of Rock (1989) that lamproites grade globally on the one hand into minettes...as totally misleading and petrologically unfounded". Nevertheless, it seems likely that minettes are somehow a bridge between lamproites and rocks of less extreme composition, as many authors have suggested (including, elsewhere, Steve Bergman). Thus, as the authors report, the Tertiary Murcia-Almeria lamproites of Spain are quite atypical among lamproites for having unusually sodic feldspars and amphiboles, and unusually aluminous micas, with only a minor tetraferriphlogopite component; for lacking priderite and wadeite; for having relatively low TiO_2 and P_2O_5; and for displaying several other characteristics that are very similar to some post-orogenic minettes. They are also the only lamproites known to contain phenocrystic orthopyroxene. Mitchell and Bergman may feel constrained to include the Murcia-Almeria rocks within the lamproite clan because they were used at an early date by Niggli as the type location of lamproite. Which pigeonhole transitional rocks are placed in is unimportant; what *is* important is to recognize that they are transitional. The attitude expressed in this book is that lamproites are a world apart from other igneous rocks, and I, for one, dispute that.

The best test of any book which aims to be authoritative is in its actual use, and this test it easily passes. I have had occasion to refer to *Petrology of Lamproites* many times during the past eight months in the course of preparation of manuscripts on related rocks, and have found it comprehensive and invaluable. The large volume of well-organized comparative mineralogical and geochemical data and more than 1000 literature citations which it contains make it a valuable and unique reference work. Its price is more than competitive, it is downright reasonable, especially considering the four pages of colour photographs. Don't miss this useful and important book.

A Dictionary of Scientific Quotations

By Alan L. Mackay
Adam Hilger, Bristol
297 p., 1991

The Oxford Dictionary for Scientific Writers and Editors

Edited by Alan Isaacs, John Daintith and Elizabeth Martin
Oxford University Press, 389 p., 1991

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The world of science is not well served by reference manuals. Certainly, there are innumerable multi-volume encyclopediae, and a few invaluable data-laden reference books, like the venerable *Handbook of Chemistry and Physics* (The Chemical Rubber Co., Cleveland, Ohio), and there are a few scientific and mathematical dictionaries, but where are the equivalents of Bartlett's *Quotations*, or Fowler's *Modern English Usage*, or Roget's *Thesaurus*, all books written for the educated reader or scholar?

A year or so ago, I was reading a book in which the quotation, "there is physics, and the rest is stamp collecting", was attributed to Lord Kelvin. I felt sure that something like that was said by Ernest Rutherford, and my recollection was confirmed by several colleagues (consulted over lunch in that source of all wisdom, the Faculty Club), but a determined search in the library failed to turn up any Dictionary of Scientific Quotations, and the standard biographies of Rutherford were not particularly helpful. And anyway, when exactly did Kelvin and Rutherford live? That information can be obtained from the *Dictionary of Scientific Biography*, but it is not the kind of reference work that any scientist can expect to have on his own bookshelf.

So every scientist should be grateful for the appearance of not just one, but two, small volumes that go far to close the gap between science and the rest of the scholarly world. Yes, it was Rutherford who made the remark about stamp collecting, although Mackay is able to give us only a secondary source (Bernal, *The Social Function of Science*, cited without publisher or page number, which makes it a little difficult for a sceptic to check). Both volumes tell us that Rutherford's dates were 1871 to 1937, and Kelvin's were 1824 to 1907 (although in Mackay, you have to know that Kelvin was otherwise known as William Thomson).

Mackay's book is the more idiosyncratic of the two, and also the most fun for browsing. Sir Charles Lyell said, "A scientific hypothesis is elegant and exciting in so far as it contradicts common sense", although that is a paraphrase from Steven Jay Gould (what did he really say, I wonder?). Theodore von Karman said, "The scientist describes what is: the engineer creates what never was." I cannot see how any literate scientist can bear to be without this book, now that it is available (and in a reasonably priced paperback edition) from the API. But what, you might ask, is the API? Well, the Oxford Dictionary isn't much help on that one (or on AGU either), although it will tell you that HOMO is the acronym for "highest occupied molecular orbit".

Mackay's book is weak on geology. Playfair's Law is not to be found, and there is nothing quoted from Hutton. My favourite poets of science, Hugh Macdiarmid and John Updike, are both represented, although Macdiarmid by only a single quotation from one of his dialect poems (one would have liked to see something from his great poem "On a Raised Beach"). And there are altogether too many quotations from non-scientific sources (there are ten from Mao Tse-Tung). Why should a scientist care that Edward Gibbon said, "The wind and waves are always on the side of the ablest of navigators"?

The Oxford Dictionary is more balanced, although often maddeningly uninformative and subject to some strange lapses. Hutton, Werner, von Buch, Sedgwick and Murchison, and even Vine, Mathews and our own Tuzo Wilson rate an entry. So does Stokes, but not Navier. H.H. Read is listed, but not N.L. Bowen (now there's an anomaly for you!). Reynolds and Froude are recognized: the Reynolds number is defined, but the Froude number is only mentioned, and no help is given on the difficult question of whether the name is pronounced "Frood" or "Frowd". The standard units are given their approved abbreviations, and defined, but for exotica like a Bubnoff unit you will still have to depend on the AGI Glossary. The Oxford book is designed, in part, as a style manual for scientists writing for Oxford University Press (OUP) publications. I looked for fundamental divergences from Cambridge, but found only one: OUP believes citations to more than two authors should always be listed as Smith *et al.* As far as I know Cambridge has not put out a comparable manual, but the Cambridge University Press books on my shelf allow three authors (Smith, Jones and Thomas) and do not italicize "et al.". Now if only the younger generation of scientist could learn that "et" is not an abbreviation ...

Both books deserve a place on your personal shelf.

An Agenda for Antiquity. Henry Fairfield Osborn and Vertebrate Paleontology at the American Museum of Natural History, 1890–1935

By Ronald Rainger
University of Alabama Press
Tuscaloosa and London
xiii + 360 p., 1991, US $37.95 (cloth)

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The story of the confrontation between Edward Drinker Cope and Othniel C. Marsh, the two very rich men (initially, at least!) whose scientific ambition and personal hostility both stimulated and hampered the development of vertebrate paleontology in North America, has become one of the familiar "legends" of geology. It is perhaps because, though equally able and ambitious, he was involved in no such personal rivalry that the name of a third very rich man, involved in that scientific discipline only a little later, is so much less familiar nowadays. It is arguable, however, that his influence has been more lasting, for better or for worse.

Another factor is that, though good biographies have been written of Cope and Marsh, no biography of comparable quality has yet been written of Henry Fairfield Osborn. Yes, Osborn himself wrote an autobiography, but it is short and, as Rainger comments (p. 121), a "didactic, egotistical" work of slight value. Nor does the work here reviewed fill this gap for, as its author states at the outset, it is:

> ... not a biography of Osborn. It does not pretend to cover his entire career, nor does it examine all aspects of his work at the American Museum. Instead it is an analyzing of how and why Osborn developed the leading center for vertebrate paleontology in the United States and what kind of program he created. (p. 1)

Though there had been earlier discoveries, the study of vertebrate paleontology in the United States was effectively begun by Joseph Leidy and reported to the Academy of Natural Sciences in Philadelphia, then arguably the leading scientific centre of the western hemisphere. Leidy, however, was progressively edged out of the field. As he remarked bitterly to Archibald Geikie:

> Formerly, every fossil one found in the states came to me, for nobody else cared about such things: but now Professors Marsh and Cope, with long purses, offer money for what used to come to me for nothing, and in that respect I cannot compete with them. (p. 16)

The Academy itself was to fall victim to one of those wealthy gentlemen. Cope was initially a member and kept his collections at his Philadelphia home. His attempts to convert the Academy into a research institute, however, provoked a bitter quarrel and caused the Academy to become, by the 1890s, merely "a paleontological backwater" (p. 15).

The ensuing battle for supremacy by Cope and Marsh resulted in massive enlargements of their collections and a plethora of publications, many of which (especially by Cope) were hasty and careless. Cope's vengeful campaign against his rival — aided in part by Osborn (p. 37) — succeeded in destroying Marsh's links with the United States Geological Survey, but his fortune was forfeited in the struggle, and his collection was ultimately sold to the American Museum of Natural History. Marsh likewise dissipated his wealth, improperly drawing more and more upon the donation that his uncle had intended for the maintenance of the Peabody Museum and coming ultimately to rely upon a salary from Yale College (p. 17-18). Indeed, as Rainger remarks, "As aggressive entrepreneurs, Cope and Marsh had changed the character of vertebrate paleontology" (p. 18).

The problem was that they had been too dominant. Marsh was difficult, suspicious and so reluctant to give credit to his assistants that he attracted no disciples. Cope was a man of more intense passion, but greater charm; yet even he was too prickly to be easy to work with. His followers and pupils tended to be persons who knew him casually, not persons who worked with him closely; and, unlike Marsh, he had no museum under his personal charge. As the two men neared the end of their lives, moreover, scientific circumstances in the United States were changing:

> ... vertebrate paleontology ... was an expensive, non-utilitarian study that had trouble maintaining a niche in the changing structure of late nineteenth-century American science. Federal sponsorship of science and technology emphasized practical, utilitarian considerations. (p. 18)

To add to the discipline's difficulties, the scientific value of vertebrate fossils was coming into question as experimental biology became ever more fashionable. Rainger notes that:

> Problems pertaining to taxonomy and evolution continued to interest a number of scientists. But biologists coming to the fore in the late nineteenth century increasingly emphasized new approaches. In newly emerging university departments and new disciplinary societies, morphology, including paleontology, was pushed to the periphery. (p. 20)

This, then, was the scientific environment into which Osborn emerged. It was one in which the currents were flowing strongly against the interests that were specifically his, for Osborn very definitely was one of the followers of Cope; indeed, he and his classmate William Berryman Scott were Cope's leading disciples. Unlike Scott, however, Osborn could draw upon the enormous financial resources of a family shipping and railroad business (p. 24-25). Moreover, throughout his youth he had mixed with New York's financial élite, in that city or during social gatherings at the Osborn family mansion at Garrison-on-the-Hudson. He understood and shared the viewpoints of the rich New Yorkers; moreover, he was gracious in manner and knew how to charm and persuade. As the years went by and Osborn's scientific and personal ambitions expanded, he was able to wheedle substantial contributions from persons like John Pierpoint Morgan, whom Scott — or indeed, most contemporary naturalists — would not have been even able to approach (p. 45, 47, 60, 63).

Osborn's intellectual capacity and his research abilities were alike considerable. Before beginning his studies of fossil vertebrates, he undertook important studies in embryology, recognizing homologies and using these to determine phyletic relationships (p. 33). His paleontological researches were wide ranging, embracing dinosaurs, but concentrating especially on fossil mammals. In later years, when most of the preparations, drawings and even observations were being made for him by his assistants, Osborn's acute mind enabled him to elucidate details and draw conclusions that contributed substantially to the resultant publications.

Such a man, with such charm, such abilities and such connections, was sure to rise rapidly in the academic world. First of all, he and Scott gained positions at Princeton University, their *alma mater*. In 1894, however, Osborn moved on to become the Da Costa Professor of Biology at Columbia College in New York: he was to retain his ties with that College, formally or informally, throughout his life and helped it to become Columbia University. Simultaneous with that appointment came another, as curator of fossil vertebrates at the American Museum of Natural History. By 1908, he had been appointed President of the Museum.

In many ways, Osborn was the ideal appointee. Already he had demonstrated his administrative abilities and vision by developing a first-order technical team and producing excellent displays. He was henceforward to demonstrate three other abilities: as promoter of the Museum to the public at large, as raiser of funds from his rich friends and as organizer of ambitious fossil-collecting expeditions, not merely in the United States, but also in many other countries of Europe, Asia, Africa and South America. Although he publicized vigorously the adventures undertaken and the scientific results achieved by the members of those expeditions, and proclaimed the view that "having lost contact with nature, man kind was degenerating" (p. 119), Osborn participated in such expeditions rarely, briefly and only when good publicity was guaranteed (p. 35-69).

Osborn was very conscious of social distinctions, functioning always

> ... as an aristocrat ... To all except a few old friends like Scott or social equals like the Morgans, the Fricks and the Dodges, he was always Professor Osborn. Such distinctions extended to departmental activities. Trips by Osborn's secretaries and editorial assistants to his palatial home at Castle Rock had the appearance of a retinue of scientific servants. Carrying mountains of material by train from New York to Garrison, they worked and ate in the outer buildings on the estate, rarely in the main house with Osborn and his family. Departmental social gatherings were stratified affairs; scientific assistants dined with Osborn and his family, while secretaries, preparators and others ate in a separate room. (p. 73)

As Osborn's influence grew, so did his self-esteem. While as a young man he had been "cognizant of his limitations" (p. 74),

> Over the years ... he lost all critical perspective on the character of his work. Although Osborn asked for and received trenchant criticism from his colleagues, especially [W.D.] Matthew, he never wavered from the belief that his interpretations were absolutely correct. He spoke of his own work in the same breath as the researches of Darwin, Huxley or other towering figures in biology ... [Indeed] Osborn came to view himself as the inheritor of their scientific mantle. He considered his own life and work exemplary, a model for making important scientific discoveries and achieving social and scientific prominence ... In his own mind, Osborn was a great man, and he did not hesitate to make that opinion known or to employ it to his advantage. (p. 74).

His increasing arrogance caused him to flaunt his status. He would take care to let it be known in advance when he would be arriving at the museum, the zoo or scientific meetings and, when he did so, he would exact proper respect from his minions or from lesser scientific lights. His employees were required immediately to drop their tasks and assist him, whenever he required it. He would offer

> ... criticisms on the personal lives of his assistants, suggesting that Matthew abandon smoking and coffee and expounding on the rewards of a daily walk or horseback ride. (p. 73-74)

Yet, at the same time, Osborn was "solicitous of the interests and needs of his staff", not only emphasizing their attainments and contributions in his annual reports and publications but also lobbying to obtain for them higher salaries and better working conditions (p. 78). If an autocrat, Osborn was on the whole a benign one, always provided, of course, that his own status was properly acknowledged and respected!

His attainments in advancing museum display techniques were considerable. He drew into his service first-rate preparators and artists: the work of Charles R. Knight is especially remembered, but he was only the most prominent among a distinguished group. The American Museum exhibits were among the first that displayed the skeletons of extinct creatures in life-like posture and were associated with restorations, in the form of paintings or models (p. 243-244). The new approaches developed by Osborn were to prove influential in enhancing the display techniques of many other museums in New York and elsewhere. The exhibit he developed on the evolution of the horse was not only innovative and effective, but also of particular appeal to the museum's trustees, many of whom both owned and rode horses (p. 91). It has served as a model for many similar exhibits elsewhere, even today in Saskatoon!

In part through his contacts with President Theodore Roosevelt and other members of the Boone and Crockett Club,

> ... a group of wealthy New Yorkers who, while devoted to 'manly sport with the rifle', dedicated themselves to the study of animal habits and natural history and the preservation of wildlife (p. 115),

Osborn became an active and dedicated conservationist. His faith in evolution caused him to be especially irritated by William Jennings Bryan's fundamentalist attacks, to the point that he wrote a short book presenting the evolutionary viewpoint and entitled it *The Earth Speaks to Bryan* (1925). His many other writings on the evolution of man, even though marred by a polyphyletic theory of human evolution that was grounded in his own elitist and racial prejudices, were highly influential in their time.

Osborn's life, then, was one of many attainments. For more than 20 years, he wielded an immense influence in the scientific life of the United States of his time and, on the whole, employed his power benignly. By the 1930s, however, that power had begun to wane. Osborn had drawn heavily upon the support of wealthy associates who shared his interests and, by then, that generation was fading. The flow of personal donations was drying up, and Osborn had not been able to persuade his associates to establish charitable foundations that would serve as renewing financial springs. It did not help that Osborn's scientific views, so firmly held and so strongly expressed, were being increasingly challenged:

> By the 1930s his outright rejection of genetics and experimental biology was no longer viable, and many scientists had distanced themselves from the hereditarian eugenics of the 1920s. (p. 246)

The exponents of the theories Osborn had rejected took revenge by denying that vertebrate paleontology had any great value in the understanding of evolution. One of them, T.H. Morgan, went so far as to discourage his students even from studying anatomy! Not until the work of George Gaylord Simpson in the 1940s was this distortion of the perspective of biologists to be remedied.

In his concluding paragraph, Rainger gives so excellent an assessment of Osborn's attainments that I shall quote it in full:

> Osborn's legacy is ambiguous. In certain respects it is easy to dismiss him as a pompous and rather ridiculous figure whose interpretations had little or no influence. His status and authority derived largely from his connections to wealthy and powerful New Yorkers. His scientific interpretations failed to incorporate the leading conceptual and methodological developments of the day and were influenced by social and political values. Yet he played an important role in developing early twentieth-century American vertebrate paleontology. Osborn, particularly in his later years, was a bloated, egotistical figure whose views required reinterpretation; nevertheless he established the institutional foundations and promoted the scientific research that would effect that reinterpretation. (p. 247-248)

This, then, is an important and meticulously researched study, destined to be a source work for future studies of the history of vertebrate paleontology and museology. It is not always easy reading and there are perhaps too many infelicitous phrases (*e.g.*, "a somewhat notorious reputation", p. 79). With so much that is worthwhile to be discovered, however, the diligent reader will find ample reward in these pages.

Living Earth: A Short History of Life and its Home

By E.G Nisbet
Routledge, New York
237 p., 1991, $29.95

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This book is written to introduce "... all people, students and general readers alike, [to] the history of life on Earth ... and to ... realize that geology is not just a collection of rocks, minerals and fossils. Instead it is the history of our home, how it was built, and how our ancestors lived in it and shaped it ..." Nisbet presents his selection of fact, knowledge and opinion in a lively way, and the content is a great frame over which to build a course in geology for an interested lay person. He has a knack for keeping the reader interested, often by insertion of interesting aspects in otherwise rather dry, factual recitals. One of my favourites is:

> The probable link between dinosaurs and birds is in the most precious of all fossils, Archeopteryx, which is the oldest clearly feathered fossil bird known. It lived in the late Jurassic, about 150 million years ago. Of all the missing links, this is the best. It is a mixture of reptile and bird, with feathers and wishbone, and also a long bony tail, teeth and three fingers on its front claws. It is a dinosaur, but lies on the evolutionary path between its archosaur ancestors and modern birds, which have, over their evolution (except the young of the modern South American hoatzin bird), lost their teeth and front claws.
>
> Birds keep their legs free for walking or running, or — one of nature's most dramatic sights — for an eagle's fall, killing prey. They seem to have had advantages over the pterosaurs, and so the second reptiles to fly displaced the first. From them we have our modern birds: the dinosaurs are still with us, as chicks; warm, cuddly, fluffy little things, executed horribly, then roasted and fried by us. (p. 174-175).

The book covers selected episodes from the history of the Earth and discusses them in contexts which could be considered "holistic". Consideration has been given to the interactions between life and the outer shells of the Earth. Much attention is devoted to the role of the unicellular inhabitants in these feedback systems. References for further reading mention books by Margulis and Lovelock, indicating that groundwork has been laid for more research in the directions these authours have explored.

In this book, life does not culminate in *Homo sapiens* at the top of the evolutionary tree. Below is a quote from a chapter called Humanity:

> There seems to be a pattern. The rats show it; so do humans, if the Pacific legends are true. A greatly advantaged species arrives on an island, devours the abundant food supply, explodes in population, devastates its environment and collapses. Eventually an impoverished stability is restored, which may perhaps include a few members of the species that caused the disaster ... (p. 222)

and another from chapter 10:

> Will there be any other signs of our civilization? Probably not. The larger cities will be washed to the sea ... our last relic may be a Foster's can ... (p. 183)

I read the book in a few sittings and found much within its pages to think about, so I recommend it to practising geologists as well as to its intended audience. Many topics are covered. A brief but representative look at the table of contents reveals this eclectic segment: Hawaii, heart origin, heavy water, hedgehogs, Hell Creek Formation, heterotroph, Himalayas, hippos, hoatzin. The breadth of topics succeeds in transmitting the societal consequences and implications of geological work to interested readers.

The old Historical Geology texts spent an obligatory few pages on the Precambrian, and then got on with the fossil record. This book, on the other hand, allots the first 122 pages to the Precambrian record, and covers the Phanerozoic in 80 pages, including a brief look at "the dance of the continents". The last 25 pages are spent in showing us where we fit in. The book is built around a theme which underlies much of the text:

> Overall, there is no equilibrium, but rather a fluctuation about an ever-altering average state. There is constant competition, but there is also total interdependence. This is co-evolution. Occasionally, the system crashes ... (p. 74)

The book shows some signs of hurried assembly. The maps in the book are inconsistent. For example, Figure 3.1 (p. 48) shows very little Archean outcrop in Canada's northeast Arctic, whereas Figure 6.1 (p. 99) shows more. I agree with the latter. The wonderful book by Gould on the Burgess Shale is not included in the references, but clearly would be a good one to include in further reading on the chapter devoted to those shales. The subdivision of life into many kingdoms (Figure 5.7; p. 84) is at odds with some current practise in biology to limit the number of kingdoms to five. This presumably means that the book will, undeservedly, find less enthusiasm from the lecturers of beginning biological courses. On the other hand, the presentation of various scenarios of life beginnings is very interesting, although I was surprised not to see Towe's ideas discussed. These are minor quibbles in a fine book.

In a next revision (I'm sure the book will be a popular text), I suggest that the author include more reference to the end of the Permian: that extinction, after all, was more efficient than the Cretaceous one. As well, some interesting lessons can be drawn from it, as outlined in Kauffmann. Another point, which is implicit throughout the book but could be developed much more explicitly, is the role of feedbacks, buffers and nonlinear reactions. So many people believe that quantity of result is a direct proportion to resource allocated. This is surely one of the things that the history of life and science shows us to be blatantly false.

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