BOOK REVIEWS

Edited by Vic Baker

WORLDS BEFORE ADAM: THE RECONSTRUCTION OF GEOHISTORY IN THE AGE OF REFORM. *Martin J. S. Rudwick, 2008. Chicago and London: University of Chicago Press. 614 pp. Hardcover, US$49.00.*

Martin Rudwick has written a massive study of developments in geology from about 1822 to 1845—a period when geology was growing and changing rapidly. Despite the size of the work, Rudwick disclaims (p. 555) that he intended to describe the history of the Earth sciences as a whole. Nor did he. The omissions from his discussion are significant and bear directly on his various historical arguments. But let us first consider what he has done and in many instances has done superbly.

Rudwick begins with an account of Cuvier's continued work on fossil bones after the first edition of his *Recherches sur les ossemens fossiles de quadrupèdes* (1812). In a second edition of this work, published in five volumes from 1821 to 1824, Cuvier included a revised version of his 'Preliminary Discourse'. Rudwick is extravagant in his praise of Cuvier, stating that: "Cuvier presented a forceful and eloquent case for treating geology as a *historical* science" (p. 22). That is not true. In using the phrase 'bursting the limits of time', Cuvier assumed that the former state of the Earth, represented by the fossil mammals from the gypsum quarries of Montmartre, was separated from the present natural order by a barrier that needed to be breached. Cuvier argued that the alternation of marine with freshwater formations among the Tertiary strata of the Paris Basin had resulted from incursions of the sea that had destroyed whole populations of animals. Such a view of the geological past is not historical because, for Cuvier, the events of later periods do not grow out of those of earlier ones. They are separated from them by successive barriers.

Rudwick gives a full account of the discovery of large fossil reptiles in English Secondary strata. He describes the development of stratigraphy and geological mapping in England as a result of the influence of William Smith, who established the practice of the identification of rock strata by characteristic fossils. Smith's recognition of the value of fossils for the recognition of strata exerted a far-reaching influence on geology. Rudwick considers Smith's influence to have been exaggerated by "later and historically ignorant myth making" (p. 35). But Smith's contemporaries recognized the importance of his work, albeit belatedly. In 1830 the Geological Society of London awarded him its first Wollaston Medal. Rudwick notes the importance of the growing knowledge of fossil life, marked by Anselme-Gaetan Desmarest's work on fossil crustaceans, Alexandre Brongniart's on trilobites in 'Transition' strata, and John Miller's work on fossil echinoderms and belemnites. Similarly important were Adolphe Brongniart's studies of fossil plants, particularly those in coal. The cumulative effect of such accounts of fossils led Henri de Blainville to coin the term *palaeontology* for the study of fossil life.

Rudwick provides a detailed account of William Buckland's discovery of *Megalosaurus* in the Stonesfield slate of Oxfordshire and Gideon Mantell's of *Iguanodon* in the Tilgate Forest beds of Sussex—both very large land reptiles. Iguanodon was especially remarkable for being herbivorous. Rudwick does not discuss the significance of Mantell's demonstration that the Tilgate Forest beds were a freshwater formation, representing an ancient delta. They lay beneath the Chalk, a marine formation. Consequently the Tilgate Forest strata showed that the land surface had sunk beneath the sea to be buried beneath hundreds of feet of marine strata. Later, the Tilgate Forest formation was re-elevated and the

overlying strata eroded away, providing evidence of large-scale land movements over an immense period of time.

Rudwick's description of the conventional view of Earth history in the 1820s fails to emphasize the innovative work then being done in geology. He presents (p. 122) a translated version of Alexandre Brongniart's table of the sequence and disposition (*i.e*., arrangement) of rocks, published in 1829. It is an epitome of conventional geological opinion. At the base of his sequence, Brongniart depicted a mass of granite forming the core of a mountain. Beside it were smaller masses of syenite and diorite. Overlying these igneous rocks were layers of gneiss, schist, and greywacke followed by the Secondary formations from the Coal through the Chalk. At the top of the sequence were the Tertiary formations, capped by Diluvium and Alluvium. Apart from the Tertiaries, the general features of Brongniart's sequence were in accord with eighteenth-century concepts of the Earth's crust and of Earth history such as were held, for instance, by Horace Bénedict de Saussure and Abraham Werner. To them, the masses of granite at the centres of mountains represented the original surface of the Earth. Surrounding the mountains were the waters of a universal ocean from which the layers of crystalline gneiss and schist were precipitated. As the level of the ocean fell, the various Secondary strata settled from it in succession. By the 1820s geologists such as Brongniart may not have thought in terms of a former universal ocean, but they did assume a single conformable sequence of formations. They agreed, says Rudwick, "that the huge pile of formations represented a correspondingly vast span of time", much longer than that of the historical period. Yet that span of time, while vague, was delimited by the rocks at the lower end of Brongniart's sequence. It was not indefinitely vast. The succession of formations, continues Rudwick, "suggested a history of life marked by directional and even progressive change" (p. 133). What the direction of the directional change was, Rudwick does not specify though he links the course of change to the theory of a cooling Earth advocated by Louis Cordier in 1827 on the basis of the increase of temperature with depth observable in deep mines. Volcanic activity also demonstrated that the Earth's interior was hot.

Rudwick devotes nine chapters to the growing importance of Tertiary geology in the years 1824 to 1830. In a further nine chapters, he takes up the work of Charles Lyell, beginning with an account of Lyell's tour through central France with Roderick Murchison in 1828, and by Lyell's subsequent travels alone through Italy and Sicily. He gives extensive summaries of the three volumes of Lyell's *Principles of Geology*, followed by a discussion of contemporary reviews and criticisms of the *Principles*. Rudwick acknowledges Lyell's great contribution to Tertiary geology in establishing four successive subdivisions, the Eocene, Miocene, and older and newer Pliocene (later termed the Pliocene and Pleistocene), and demonstrating the gradual change of fossil shell species through the course of the Tertiary.

Although Rudwick acknowledges the historical importance of Lyell's work, especially by the space he devotes to it, he denies that Lyell's work was revolutionary. But in contrast to his contemporaries, Lyell took a very different approach to Earth history. Was that not revolutionary? Lyell's insistence on the uniformity of the geological past with the present, on the continuity of geological history, and on the dynamic stability of the Earth stood in direct opposition to contemporary geological opinion. The prevailing view of the geological past was that it had been progressive and violent, marked by the abrupt elevation and folding of strata. Particularly unacceptable was the concept of a boundless vista of past time, as envisioned by both Hutton and Lyell. The fact that most geologists of the time disagreed with Hutton and Lyell does not detract from the validity of their views. Hutton's and Lyell's vision of the indefinite vastness of geological time and of the continuity and uniformity of Earth history would ultimately prevail. It represented an accurate picture of the dynamic stability of the Earth through time.

Hutton's vision of the geological past derived from the simple hypothesis that stratified rocks were sediments that were originally deposited on the sea floor in horizontal layers. If stratified rocks were sediments they must have been formed by the wearing down of pre-existing land. Hutton's concept of the vastness of geological time was influenced also by his observations of unconformities, where horizontal rock strata rested on the edges of older, vertical or steeply inclined strata. Playfair vividly described Hutton's explanation of the meaning of the unconformity at Siccar Point on the Scottish coast. The older strata accumulated in the sea in horizontal layers. Later they were elevated to form land, and raised from a horizontal to a vertical position. After their erosion the vertical strata then sank beneath the sea again, to be buried under a second series of horizontal sediments. Finally the whole mass of vertical and overlying sediments was raised above the sea to become the present land of Scotland. Yet the older vertical strata had come from the wearing down of still older land. Thus, said Playfair: "Revolutions still more remote appeared in the distance of this extraordinary perspective".[1]

Hutton was also influenced by his observation that granite had apparently been intruded among sedimentary strata in a molten state, altering them by its heat. Granite intrusions elevated mountains. The granite, therefore, was younger than the sedimentary strata that it upheaved or intruded. It did not represent the original surface of the Earth.

Lyell's personal vision of the vastness of geological time emerged during his tour through France and Italy in 1828—Rudwick devotes two chapters to this tour. He describes Lyell and Murchison's observations on the formation of valleys but does not mention that Lyell was deeply impressed by the time required for the accumulation of the laminated freshwater marls of central France and their similarity in appearance to much older Secondary marine strata in England. In Sicily, Lyell repeatedly saw evidence of the vast amount of time needed to deposit the strata he was observing. The Tertiary limestone of the Val di Noto plateau overlay marl containing living Mediterranean species; the great cone of Etna also rested upon strata containing shells of living species. He was compelled to conclude that, in human terms, the Mediterranean fauna was very old—much older than Etna—yet it was only the last in a succession of faunas that had changed slowly and gradually through the course of the Tertiary.

Rudwick refers to Hutton's and Lyell's concept of a vast, unlimited geological past as 'eternalism'. He describes the rejection of Lyell's view of a steady-state Earth by critics, with the seeming implication that Hutton and Lyell were mistaken. At the same time he describes Élie de Beaumont's hypothesis of the sudden elevation of mountain chains quite uncritically. Indeed, he describes it as "impeccably natural" (p. 471), despite its preposterous character. It is only fair to Lyell to say that his vision of a dynamically stable Earth, of immense age, was correct. We know now that the Earth is 4,650 million years old. If that is not eternity, it would have seemed so to Lyell's critics. Furthermore, quite recently geologists have identified sedimentary rocks in Australia that are 4,400 to 4,500 million years old.[2] The sedimentary character of the rocks means that they were laid down in water and represented sediments washed down from pre-existing land. They prove the presence of oceans at a very early period of Earth's history. The discovery illustrates E. G. Nisbet's remark in 1987 that "it is one of the most remarkable aspects of life on Earth that the surface temperature has remained within the stability field of liquid water for perhaps 4×10^9 years—uniformity indeed!".[3] As Lord Kelvin recognized long ago, solid rock is a very poor conductor of heat. Once a crust formed over the Earth's surface, it acted as an insulating blanket to retain the heat within. Through its long history, the interior of the Earth has been hot, but its surface has remained equable, much of it covered by oceans in which life has evolved.

In sum, Rudwick's book is tendentious. His history is, I suggest, distorted throughout by a veiled agenda, intended to demonstrate that geologists opposed to Hutton and later

those opposed to Lyell accepted a vast timescale for the history of the Earth. They did not. In 1797, the Irish mineralogist Richard Kirwan justified his objections to Hutton's theory "by observing how fatal the suspicion of the high antiquity of the globe had been to the credit of the Mosaic history and consequently to religion and morality".[4] Kirwan was not alone. Devout Christians, including geologists such as William Buckland and William Conybeare, tried to reconcile their geology with the Bible, but their efforts failed.

Rudwick also sees past glaciation in Europe as a serious challenge to Lyell's faith in geological uniformity. It might have been if Lyell had been unwilling to study the effects of glaciation. But he did study those effects. Before Agassiz proclaimed an Ice Age, Lyell was comparing Arctic species of fossil shells from Sweden with Arctic fossil shells from Canada. Among Tertiary fossil shells, Lyell detected evidence for a gradual cooling of climate beginning in the Miocene. Moreover, glaciation was not as extensive as Louis Agassiz asserted: and England south of the Thames Estuary was never glaciated. Furthermore, the Pleistocene was marked by periods of warmer climate alternating with glacial periods. Geologists had to account for the presence of the hippopotamus and elephant in Britain as well as the musk ox. In 1867, in the tenth edition of the *Principles of Geology*, Lyell discussed the possible factors influencing fluctuations in world climate, including James Croll's theory of the role of the eccentricity of the Earth's orbit in causing glacial periods. Today, geologists accept a modified form of Croll's theory as the probable cause of glacial periods.

Parts of Rudwick's book are excellent. The numerous illustrations are well selected and often beautiful. His account of the early development of the glacial theory is clear and informative. But taken as a whole, the book misleads as much as it informs.

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1. Playfair, J. 1805. Biographical account of the late Dr. James Hutton, F. R. J. Edin. *Transactions of the Royal Society of Edinburgh* 5 Part 3: 39–99 (at p. 73).
2. Harrison, T. M. *et al*. 2005. Heterogenerous Hadean hafnium: evidence of continental crust at 4.4 to 4.5 Ga. *Science* 310: 1947–1950.
3. Nisbet, E. G. 1987. *The Young Earth: An Introduction to Archaean Geology*. Boston: Allen & Unwin, pp. 128–130.
4. Kirwan, R. 1797. On the primitive state of the globe and its subsequent catastrophe. *Transactions of the Royal Irish Academy* 6: 233–317 (at p. 307).

A PASSION FOR NATURE: THE LIFE OF JOHN MUIR. Donald Worster, 2008. New York: Oxford University Press. 535 pp. Hardcover, $34.95.

> *A story of glaciers pouring into Yosemite Valley "by every one of its canons," cascading over the edge like a never ending-frozen water waterfall, filling the entire valley . . . the ice marked its track with striations etched on granite.*
>
> John Muir (1871)

During September1871, John Muir, then an obscure figure, sent off an essay titled 'Yosemite Glaciers', to Horace Greeley's New York *Daily Tribune*. It became his first published work. He received $200 for the article, a large sum for such a piece. Probably an essay of its length and topic would not be printed by most newspapers today.

The great Yosemite Valley, Muir wrote, "was brought forth and fashioned by a grand combination of glaciers". That same year Clarence King reported in the *American Journal*

of Science and Arts on a Mt Shasta glacier. He is generally given credit for the first discovery of them in the United States.

Josiah D. Whitney, director of the California State Survey, and the State's most prominent scientist, sneered at Muir's explanation for Yosemite: "the absurd notion", he said, "of a mere sheep herder". Whitney favored a cataclysmic origin for the valley: "the result of an underlying fault in the bedrock that abruptly opened one day, letting the floor collapse". In his *magnus opus*, *Systematic Geology* (1878), King agreed with Whitney, writing that Muir's "vagaries will not deceive geologists who are personally acquainted with California". He urged Muir to seek a different channel for his research, "if there is one, in which his attainments would save him from hopeless floundering". But early in the twentieth century most geologists began to recognize the soundness of Muir's observations about Yosemite. Francois Matthes of the U.S. Geological Survey said in 1950 that Muir "was more nearly right . . . than any professional geologist of his time".

Born in Dunbar, a seaport on Scotland's east coast, Muir was a Lowland Scot, and for all of his long life (1838–1914), he remained very much one. At the mouth of the Firth of Forth east of Edinburgh, the town was an ancient settlement site, dating back to the second century AD. As a boy, he was fond "of everything that was wild", and all his life he grew fonder "of wild places and wild creatures". He was also molded by his parents and by the society of Dunbar, gaining almost all of his formal schooling there before university. Scottish oatmeal porridge remained his lifelong choice for breakfast.

In 1849, the Muirs migrated to a frontier community in central Wisconsin, there to raise wheat. The simple life attracted Muir's father, who sought "a more primitive self-reliant Christianity". He was rebelling against organized religion and finally decided to become his own clergyman. When he came to Dunbar, he "refused to join the established Church of Scotland", instead joining the Secession Church. Calvinism in Scotland had "put people in a tough place", with little hope of eventual salvation, no matter how faithfully they trudged the golden road.

Whatever his eight children wanted to do for fun Muir's father concluded that it was a bad thing. He was stern and, according to Muir, reached quickly for the rod[1] though in their later lives not all of the children recalled their father's cruelty in the same way that Muir did.

Though Muir's father was a successful grain merchant in Scotland, and the family arrived in the United States with substantial financial resources, Wisconsin was a challenge they could not overcome. Their land was forested and had to be cleared. It was covered with glacial ground moraine; gravel and boulders were ubiquitous. For a good part of the year, Wisconsin was cold and snow-covered. Through farming, however, Muir acquired the knowledge and experience of husbandry that would be the basis of his later success raising grapes in California.

At the age of fifteen or sixteen Muir was drawn to renew his studies. On his own he took up successively algebra, geometry, and trigonometry. He also turned to poetry, especially Shakespeare, Milton, and the bible. He set up a workshop in the cellar of his family's home in Wisconsin and began to invent a variety of useful things: a self-setting sawmill, a thermometer, waterwheels, door locks and latches, clocks, and a barometer.

On the basis of his mechanical aptitude, Muir's sister hoped and thought that he could become a great inventor. His mother believed he might become a minister. Early on, Muir longed to be a physician. But what he finally became was a great conservationist, the founder of the Sierra Club.

Late in the summer of 1860, Muir, twenty-two years old, left home, took the train from Pardeeville to Madison, about forty miles, and began college at the University of Wisconsin. He carried with him some of his inventions and he sporadically continued such work in Madison. At university, Professor Ezra Carr sparked an interest in chemistry and

natural history. There were also a few lectures in geology. Carr's wife, Jeanne, became Muir's close friend and a mentor, eventually choosing and introducing him in California to Louisa Strentzel, who became his wife.

In the summer of 1863, with tear-stained eyes, Muir left the university for good, never to attain a degree. He had been in attendance for about three years, not the four he later claimed. His dramatic pronouncement on leaving was that of an avowed transferee to 'The University of the Wilderness'.

Early in 1864, fearful of being drafted and pulled into the terrible civil war that violated "the most basic Christian ideals", Muir joined his younger brother Dan in Canada. Among many others, he had become a draft dodger; but in the spring of 1866 he returned to the United States. Lee surrendered to Grant on 9 April 1866 at Appomatox Courthouse, Virginia.

Soon after turning from studying plants and flowers in the US and Canada, Muir took a factory job in Indianapolis. There a file slipped from his hand, "as he was setting up a new circular saw". It flew into his right eye, "piercing the edge of the cornea", partially blinding him. Stoically, he walked home and climbed into bed. In a few hours, the left eye also went dark.

In about a month, Muir regained most of his sight, but the accident was a turning point, perhaps the most important one in his life. He threw down his tools, abandoning forever further work in industry or in pursing a career as an inventor. On 2 September 1867, he took off on an ambitious thousand-mile walk to the Gulf of Mexico. It was the kind of adventure—though more enterprising than most—that many want to do when they are young.

Muir frequently slept out without blankets and went without supper or breakfast, nearly starving to death. He found most of the towns dreary, shabby, and filthy. Many of the people were 'backward' and some were 'dangerous'. He trudged south from Indianapolis across Kentucky, Tennessee, North Carolina, Georgia, and Florida to the Gulf Coast, making the trek in less than two months. Collapsing with malaria in Ceder Kay, Florida, he was terribly sick for two months.

Muir had broken away from his previous life and was through with university and formal studies. Down deep he resolved to use his remaining days on Earth to live in and study wilderness. Soon that led him to the Grand Canyon, Yosemite, and the glaciers and Indian villages of Alaska.

A Thousand-Mile Walk to the Gulf, one of three Muir books assembled by his literary executor William Frederic Badč, first appeared about two years after Muir's death. Two others, *The Cruise of the Corwin* (1917) and *Steep Trails* (1918), soon followed. *Travels in Alaska* (1915), brought together by Marion Parsons, came out shortly after Muir's passing. There was a great rush to make his notes and journals available. However, none of these or any other of Muir's books are equal to his first book and classic *The Mountains of California*, written when he was fifty-six. Somewhat of a collage, *Mountains* contains sixteen chapters, with two essays that were new and fourteen from back issues of various magazine and newspapers. Muir worked very hard to become an interesting writer in the pieces he wrote for publication, which for many years constituted his chief source of income.

Muir found a paradise in California. Arriving in San Francisco in 1868, and always the walker, he headed on foot with a Cockney immigrant he had met aboard ship for the Yosemite Valley. He carried a few books, a journal, and soon, from necessity, he became a sheep herder, shepherding a flock of twenty-five hundred. But he detested sheep, calling them 'hoofed locusts'. Otherwise, the golden California days were idyllic for Muir: "We are now in the mountains and they are in us", he wrote. That period of his life appeared in the delightful book *My First Summer in the Sierra*, published more than four decades later

(1911). Pretty much a self-taught botanist, *Summer* contains countless wonderful passages by Muir on the varied and lovely Sierra flora. Significantly, he saw his God everywhere.

Donald Worster is also the author of *A River Running West*, a biography of John Wesley Powell. What he has written here is the most complete treatment of the illustrious conservationist John Muir. It is a long book but one that reads quickly. Worster especially explores Muir's marriage and family, and his relationship with an abusive father, with whom, however, he finally became reconciled. Particularly interesting are Muir's friendships with John Burroughs, Theodore Roosevelt, Ralph Waldo Emerson, G. K. Gilbert and a multitude of others, many of them less well-known. The writing is markedly compelling about glaciers and the weeks spent in Alaska.

Worster's book is knowing and original, his paragraphs written with an ease and intelligence that invites readers in. Starting slowly, it builds to a powerful evocation of John Muir, our most prolific writer on conservationism. Needless to say, I have read and own—mostly in paperback—all his books. As he might have appreciated, I took some of them with me into petroleum basins of Wyoming, into the Uinta and Paradox basins in Utah, to Nevada, on Italian journeys, indeed to wherever I was in the field in the last six decades. Muir was a wonder.

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1. Muir explored his early life in a series of essays called *The Story of My Boyhood and Youth* published in 1914, the year of his death. The book roughly covers Muir's three early worlds: the first eleven years in Dunbar, where he began school before he was three; from 1849–1860 in America in the Wisconsin wilderness; and his years at the University of Wisconsin. For the eleven years in the Wisconsin wilderness, he was tied to a few square miles around a farm. The nearest city, Portage, was a dozen miles away. His father had chosen a poor piece of land. Today, much of it is again wilderness or planted in trees.

***TYRANNOSAURUS REX*: THE TYRANT KING**, edited by Peter Larson and Kenneth Carpenter. 2008. Bloomington and Indianapolis: Indiana University Press. xvii + 435pp. and with a CD-ROM. Hardcover. $49.95.

Tyrannosaurus rex, says this volume, is "arguably the best known and most popular of dinosaurs among laymen". The published output in book form suggests this may well be true for dinosaur palaeontologists too. Among the few books devoted to a single genus/species of dinosaur, *T. rex* takes more room on my bookshelf than all the others put together—several (including one recently reviewed in these pages) focus on the discovery and interpretation of a single specimen. This substantial volume, supplemented by a CD-ROM, is an important addition to the collection. It contains enough historical information to justify attention here, but since it is not primarily historical this review will only be brief and will focus on the material that is of historical interest.

The editors Larson and Carpenter are both prolific authors on dinosaurs. Robert Larson is Founder and President of the Black Hills Institute of Geological Research, and a principal figure in the still controversial events surrounding the discovery, seizure and sale of 'Sue'. Kenneth Carpenter is dinosaur palaeontologist at the Denver Museum of Nature and Science. Most of the thirty contributors are from the US; only one is from Canada. Together they present twenty-one papers reflecting the current state of knowledge of and about *T. rex*. These summarize existing information, describe new specimens, and discuss aspects of chronology, lifestyle, morphology, movements, musculature, pathology, predator-

prey relations, sexual dimorphism, taphonomy—and the puzzling tiny forelimbs.

Tyrannosaurus rex was named in 1905 by Henry Fairfield Osborn, of the American Museum of Natural History in New York. His description was based on a skeleton collected by Barnum Brown, then at the outset of his career, but later one of the most famous of dinosaur collectors. Four partial skeletons were the only material available until 1967; *The Complete T. rex* (Horner and Lessem 1993) refers to eleven specimens. The new volume opens with a fifty-five-page chapter summarizing information on forty-five whole or partial skeletons now known. Many individual specimens are known to aficionados by nicknames, of which 'Sue' has had by far the biggest press. For each, we are offered a historical and geological context, told where the specimen is displayed and described, and given an estimate of the completeness of the specimen. Useful tables summarize the information historically, and (for the twenty best) the order of completeness. This shows that only the recently discovered skeletons 'Sue' and 'Stan' are more than 50% complete, reminding us that the apparently complete skeletons in museums are a tribute to the ingenuity of technicians producing mirror image casts or reasonable facsimiles (or approximations) of missing bones.

One matter not directly discussed, but of interest to the scientist and historian, is the sometimes uneasy combination of science, entrepreneurship and government in US palaeontology. The ease with which the amateur can enter into the collection and study of dinosaurs has yielded some specimens that would doubtless have not been discovered, and amateurs have developed considerable expertise outside the academic community and led to the creation of new (if sometimes underfunded?) institutions. The checkered career of 'Sue' is well known; but we learn here that 'Peck's Rex' was likewise seized by the Government and that the ownership of 'Tinker' has been under litigation for several years. Half a dozen other specimens do not have a public repository, and one has been sold piecemeal to various collectors.

Among other curiosities, we learn that DMNH 2827 was discovered by Charlie Fickle and his dog ('both amateurs' adds the text drily), and 'Bucky' "could be considered the first discovery of T. rex by a horse"!

A short paper on Wyoming's related *Dynamosaurus* discusses early finds of fragments of *Tyrannosaurus* and its relatives, back to Leidy's *Deinodon* (1856). It also talks about Osborn's odd museum practices, selling the type of *T. rex* to Pittsburgh and of *Dynamosaurus* to London. An explanation of *T. rex*'s puny arms involves an interesting discussion of the development of morphodynamics, going back to Goethe in 1797.

Perhaps the best-known dinosaur from the laboratory to the nursery, *T. rex* is the only species of animal usually referred to by an abbreviated (and often eccentrically spelled) version of its scientific name. In the final chapter, 'A Century of Celebrity', Donald Glut discusses the interplay of science, visual art and the mass media in the popularization and presentation of this remarkable species to an international public.

"Although *Tyrannosaurus rex* itself is extinct, ideas regarding its life and times continue to evolve" we are told. And indeed, the rest of the volume explores many old and new ideas about the biology of this remarkable animal. Particularly striking is Chapter 11 "Rex, Sit: Digital Modeling of *Tyrannosaurus rex* at rest". Here the accompanying CD-ROM breaks new publishing ground (as far as my experience goes), presenting movie demonstrations of the animal sitting and standing.

Amply—even lavishly—illustrated, *Tyrannosaurus Rex: The Tyrant King* is worthy of attention by historians of earth sciences as well as the palaeontologists who will form its primary audience.

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PASSING STRANGE: A GILDED AGE TALE OF LOVE AND DECEPTION ACROSS THE COLOR LINE. Martha A. Sandweiss, 2009. The Penguin Press. New York. 370 pp. Hardcover, $27.95.

"Civilization's going to pieces," broke out Tom violently. "I've gotten to be a terrible pessimist about things. Have you read 'The Rise of the Colored Empires' by this man Goddard?"
"Why no," I answered, rather surprised by his tone.
"Well, it's a fine book and everybody ought to read it. The idea is if we don't look out the white race will be—will be utterly submerged. It's all scientific stuff; it's been proved."
F. Scott Fitzgerald, *The Great Gatsby* (1925, p. 16)

Interest in Clarence King is booming, at least to the extent attention is paid to any geologist. Since 2006 there have been three new biographies: Robert Wilson's *The Explorer King: Adventure, Science, and the Great Diamond Hoax—Clarence King in the Old West*; James Gregory Moore's *King of the 40th Parallel: Discovery in The American West*; and this book by Martha Sandweiss. Thurman Wilkins' superior work, *Clarence King*, appeared in 1958. Thus, about fifty years have passed since the Wilkins book. It has been one hundred thirty-one years since King's last substantial geological publication, his *Systematic Geology*.

Time has not enhanced King's reputation. Among nineteenth-century explorers of the West, Powell, Gilbert, and probably Dutton are all better known and esteemed by geologists and earth science historians. Likely these new studies of King will generate further consideration of the man and his work.

During his last thirteen years, Clarence King (1842–1901) lived a secret life in New York City, passing as a black man—the husband of Ada Copeland Todd (1860–1964), a former slave born in Georgia. He called himself James Todd, initially claiming to be a Pullman porter, a position then held only by black men. Out of necessity, King and Copeland lived a hidden life. To do otherwise would have been difficult for Copeland and disastrous for King, irrevocably damaging his personal reputation, his relationships with family and friends, and his geological career. In particular King's mother, with whom he was close, would have been devastated. Like others of that time, he dreaded scandal, placing "decency above courage", as Sandweiss has quoted from Edith Wharton's novel of manners, *The Age of Innocence*.

In the late nineteenth century, King, was one of the great explorers of the American West—a hero along with Powell, Hayden, Wheeler, Dutton, Gilbert, Brewer and Whitney. A graduate in 1862 of Yale University (PhB) "with honor", he soon secured support from Congress for a field study, and in 1867 he became head of the US Geological Exploration of the Fortieth Parallel. His sponsors kept him out of Washington DC so that members of Congress would not see how young he was.

Climbing Mount Shasta in 1870, King discovered an active glacier, the first documented one in the continental United States. Two years later he exposed a diamond hoax in northwest Colorado (Browns Park), saving some actual and many erstwhile investors from losses to swindlers who had 'salted' the ground with gems. He climbed the highest peak in the Sierras and the three highest peaks in the Uinta Mountains of Utah.

William Brewer, an early colleague on the California State Geological Survey during King's first real field experience, described him as, "wonderfully tough . . . [with] the greatest endurance I have ever seen . . . [and] very muscular." He was also an innovative and gifted writer. A collection of adventures, his 1872 book *Mountaineering in the Sierra*

Nevada, is still in print. Reviewers found the prose comparable to writing by "Bret Harte and Mark Twain, John Burroughs and Francis Parkman, crediting King with a place as founder of a California school of literature". Though he was prone to exaggeration and invention, *Mountaineering* is comparable to other excellent nature memoirs.

The monumental (815-page) *Systematic Geology* appeared in the fall of 1878. That study brought together much of western geology, and included mapping by King and others of the Fortieth Parallel Survey. In preference to F. V. Hayden, the initial favorite for the post, King was appointed in 1879 the first Director of the United States Geological Survey. John Hay,[1] the former Secretary of State and King's friend, called him "the best and brightest man of his generation".

King's service as Director of the Survey lasted only twenty-two months, from April 1879 to March 1881. Eager to get rich through mining ventures and consulting, he never intended to serve for long. Somewhere in 1887 or early 1888, he met Ada Copeland, a black nursemaid in lower Manhattan. No documents survive and no one knows the circumstances of their meeting and of their subsequent courtship.

Copeland had migrated from Georgia to New York in the 1880s. There her dark skin "circumscribed where she could work, how she could travel, how safe she could feel". In earlier conversations and in his writing, King frequently revealed an attraction to "dark-complected" women and was especially struck, he said, by those he saw in Tahiti and Hawaii.

When Copeland and King met, he was a celebrity. Whether she was aware of it is doubtful, but she must have been impressed by his apparent affluence compared with her marginal circumstances. Without obtaining a civil license, they soon married, settled in Brooklyn, and later moved to Flushing, Queens.

Five children were born from their common-law marriage but one child died while still a toddler. Each birth came quickly, the first two a year apart. Of the surviving children, the oldest was six years older than the youngest. Nothing indicates that King was present when any of them were born.

What King was mostly doing was consulting and managing mining ventures in the American West, in Mexico, and elsewhere, none of which were successful enough for him to prosper for long. It was tough arduous work and dangerous. To succeed in mining, you have to 'make a mine', which King, despite all his many friends and fame and contacts was unable to accomplish. To maintain a comfortable life for himself and his family, he borrowed large sums from John Hay and owed him the equivalent today of a million dollars when, aged fifty-nine, he died from tuberculosis in Arizona, his hope of a desert cure gone.

Of his friends and family, only Clarence P. Townsley, his brother-in-law, was with him at the end, which came on Christmas Eve at 2:00 a.m. Pallbearers included the brothers Arnold and James Hague, James Gardiner, Frank Emmons, and George Becker—friends from the western survey days—Henry Adams (author of *Democracy* and *The Education of Henry Adams*), and the superb painter, Albert Bierstadt, himself just weeks away from his last day. King was buried near where his mother would eventually lie. Ada King was not allowed by King's friends, who knew about her by then, to attend the funeral. She outlived him by sixty-two years.

Shortly before his death, King wrote Ada a long letter telling her who he really was and saying that there was a trust fund for her and the children. But it never materialized. For some thirty years, she remained in their home in New York City, sparingly supported by her son Wallace and receiving some funds from a secret benefactor (John Hay). Later on, she received a small monthly stipend from the Legal Aid Society.

In the early 1930s, Ada King began a long struggle in the courts to try and recover the trust fund she thought King had left for her, "money and property totaling around $80,000", she believed. After many lawyers had litigated on her behalf, the court finally

ruled that she had not established the existence of such a trust. She was left with title to the house on Kalmia Avenue, paid for by secret benefactors but received no more monthly stipends. Said the *Amsterdam News*: "she fought the group of wealthy white folk to regain the trust fund"; and she lost.

Contrary to the opinion of a recent reviewer in the *New York Times*, I believe the evidence indicates that King and Ada Copeland were very much in love throughout their life together. Given the circumstances and the time (late nineteenth century), they both did the best they could. Martha Sandweiss has done a remarkable job of research and writing. Using what is available, she has reconstructed a stirring history of King and Ada's life together. Though Thurman Wilkins and Robert Wilson mention the relationship between King and Ada briefly in their books they chose to virtually ignore it. I always wondered about Ada King and the children. Now we know about all we ever will.

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1. John Hay has been overlooked and warrants a new full-scale biography. Three years older than King, a writer and public servant, he went to the White House in 1861 as an assistant secretary to Abraham Lincoln and was beside him when the President died. King's expose of the Browns Park diamond hoax spared Hay and other investors

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BRETZ'S FLOOD: THE REMARKABLE STORY OF A REBEL GEOLOGIST AND THE WORLD'S GREATEST FLOOD. John Soennichsen, 2008. Seattle: Sasquatch Books. 289 pp. Hardcover, $23.95.

As we all know, geology counts among its practitioners a great many 'characters'. And J (there is no period after the 'J') Harlen Bretz (1882–1981) was a character among characters. Bretz, of course, is familiar to many geologists because of his role in the 'Spokane Flood Controversy', relating to the Channeled Scabland region of east-central Washington state. The bizarre landscape of the 'scablands' had puzzled many famous geologists, and the eccentric Professor Bretz shocked most of them by ascribing its origin to cataclysmic flooding.

While the debates over the origin of the Channeled Scabland are well known, there has not been a full biography of Bretz until now. John Soennichsen, an accomplished freelance writer who lives in the Channeled Scabland, has done solid job of searching through Bretz's papers, correspondence, and reminiscences. Some the most interesting anecdotes in this entertaining book derive from interviews with Bretz's family, former students, and surviving colleagues. Soennichsen touches lightly on the science issues, but his main focus is on the man at the center of a controversy, and how that man related to the landscape that he so brilliantly came to explain.

Harley Bretz (the name change came later) was born in a farming area of Michigan, but his childhood passion was astronomy. As an undergraduate at Albion College, he discovered that he was poor at mathematics, which necessitated a change to a major in biology. But Bretz also had an interest in geology. He later attributed his mathematical weakness to hitting a part of his head in his youth—the part that does mathematics, of course. His 1904 paper in the Albion College *Pleiad* concerned a glacial river channel. Another indication of Bretz's developing character was the new name that he chose for himself, and under which he submitted that paper.

Soennichsen quotes Bretz's daughter Rhoda on the name change: "He invented the Harlen thing, just as he had invented the J in front of his name—made the whole thing up. Harley Bretz was his given name but it just didn't ring a bell for him; maybe he didn't think it sounded professional enough".

Although he began his professional life as a biology teacher, J Harlen Bretz became more and more interested in geology. After moving to Seattle, Washington, where he taught from 1908–1911 at Franklin High School, Bretz began to spend his off-time exploring the glacial geology of the Puget Sound region. In 1912 he enrolled in graduate school at The University of Chicago, where he studied for his PhD with Rollin D. Salisbury. In reviewing the submitted PhD dissertation, Salisbury asked Bretz to spell out his first name on the author line. Bretz responded that the J was his entire first name. Salisbury replied: "Then never put or allow a typist or printer to use a period after that J". This was but one of many controversies in which Bretz subsequently came to be embroiled. "Throughout all my life", he recalled, "I have fought typists and printers to leave off that damned period and haven't always won".

In 1915 Bretz began his long career on the faculty of The University of Chicago. At Chicago he became a renowned teacher, and it was in the course of teaching a field course for advanced students that he came to grips with the Channeled Scabland problem. In the Columbia Plateau area of eastern Washington he and his students found an amazing assemblage of landforms that included coulees, immense dry cataracts, rock basins, anastomosing channel ways, and gravel bars. Field relations among these features, most notably the multiple levels of divide crossings, led Bretz to propose that an immense cataclysmic flood had swept across the Columbia Plateau in late Pleistocene time.

I think that Soennichsen, not being an academic, missed the significance of how Bretz got his first paper published on this outrageous hypothesis. Although he had published an earlier, mostly descriptive paper in 1923 (*Geological Society of America, Bulletin* 34: 573–608), it was Bretz's second that year (*Journal of Geology* 31: 617–619) that concluded: "It was a debacle which swept the Columbia Plateau". Bretz named this debacle the 'Spokane Flood'; and with these words he initiated the famous controversy. Of course, Soennichsen provides a lively description of that controversy, including interesting observations from family members as to how Bretz felt personally about the intense criticism that he received for his ideas. However, this key point gets missed: how was Bretz able to have his paper published in the first place? In today's world of 'publish or perish', outrageous ideas get soundly squelched within the secrecy of peer review. There is no need for a public meeting to denounce an upstart's published theories, as happened to Bretz with the infamous January 1927 meeting of the Geological Society of Washington.

But two things happened just prior to publication of the 1923 *Journal of Geology* paper. First, Bretz was named to the editorial board of that journal, which meant that he was a maker rather than a recipient of the journal's editorial decisions. Second, he was awarded tenure at Chicago, with promotion to the rank of Associate Professor.

There is much, much more to say about the Spokane Flood and its surrounding controversy, and Soennichsen tells the story well. Resolution of the controversy took many years, and it involved some of the most prominent geologists of the day. As Max Planck once said of physics: "A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it". Bretz always most wanted his critics to study the field evidence free from the blinders of preconceived notions. Some critics, like James Gilluly, did look at the evidence and found themselves converted to what they had believed to be an affront to geology's sacred uniformitarian principle. Others could never shake what we have now come to see as a flawed conception of that principle.

Shortly after receiving the Penrose Medal of The Geological Society of America, J Harlen Bretz reached the age of ninety-eight. He had outlived his critics, and his ideas on the role of cataclysmic flooding in the origin of landscapes were being used to explain the newly discovered ancient flood channels on Mars. As he told me during interviews in the late 1970s, when I was preparing his nomination for the award, he had only one regret. He had outlived all his critics, but he was not able to gloat over them when receiving the long overdue recognition for his work.

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NOTES ON CONTRIBUTORS

Lois B. Arnold, EdD, retired from Rutgers, the State University of New Jersey, in 2002. At Rutgers she provided the science leadership for an Initiative funded by the National Science Foundation and the New Jersey Legislature designed to improve science, mathematics and technology education statewide. Her longtime interest in the history of American women in science has previously led to published treatments of Maria Martin Bachman, Almira Hart Lincoln Phelps, Louisa Allen Gregory, Eleanora Bliss Knopf, and several essays on aspects of the life of Florence Bascom.

M. Dane Picard, Professor Emeritus of Geology and Geophysics at the University of Utah, Salt Lake City, taught sedimentary petrology for forty-five years at several colleges and universities in the U. S. His principal research has been on lacustrine rocks, fine-grained clastics, ephemeral streams, physical stratigraphy, sedimentary structures, Mars, and Italy. He was President of the Rocky Mountain Section of the American Association of Petroleum Geologists (1972–1973), the Society of Economic Paleontologists and Mineralogists (1984–1985), and the National Association of Geology Teachers (1988–1989). In 2002, he received the Francis J. Pettijohn Medal for excellence in sedimentary geology. He is an Honorary Member of the American Association of Petroleum Geologists, the Society of Economic Paleontologists and Mineralogists, the Utah Geological Association, and the Wyoming Geological Association. In all, Dane has written or edited ten books, most recently *A Late Roundup: Poetry and Essays* (2007).

Christopher Cleal is Head of the Vegetation History Section at the National Museum Wales, Cardiff, UK. He has been investigating Palaeozoic palaeobotany for over thirty-five years, with a publication record of over a hundred papers and seventeen books. He has also been researching various aspects of the history of the subject, with published papers on the role of illustrations in early nineteenth-century palaeobotany, early twentieth-century women palaeobotanists, and the work of the Welsh biostratigraphical palaeobotanist Emily Dix.

Geoffrey Dannell is an archaeologist who has undertaken work in the Lower Nene Valley (in East Anglia), where Edmund Tyrrell Artis operated in the nineteenth century. In researching Artis's work, he came to compile a dossier of information not only about Artis's archaeological activities but also much of his private life. Currently he is an Assistant Honorary Visiting Fellow of the University of Nottingham, and is engaged with an international team in the realisation of a database relating to *Terra Sigillata*—the red 'slip' used as a finish for certain types of Roman pottery (Samian ware).

Helen Fraser has a background in historical studies. Since joining the Botany Department of the National Museum of Wales (now part of the Department of Biodiversity and Systematic Biology) she has developed an interest in the history of palaeobotany, in particular early British palaeobotanists and the role of women in the subject. She is also an editor of the biennial *Bibliography of European Palaeobotany and Palynology*.

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Maureen Lazarus, FLS, has been the Collections Manager of the Botanical Illustrations Collections at the National Museum Wales for twelve years. During this time, she has organised exhibitions in collaboration with Dr Heather Pardoe and published a book and catalogue based on the collection. She has also been investigating the history of illustrations in palaeobotanical publications.

Wolf Mayer is a graduate of the University of Auckland, New Zealand, and the University of New England, Australia. He taught geology at both the University of Canberra and the Australian National University and is currently a Visiting Fellow at the latter institution. In recent years his main interests have been in the history of geology, particularly in relation to the early geological exploration of Australia.

Barbara Mohr received her university education at Bonn, completing her PhD in 1982 on Tertiary pollen floras from the Cologne area, Germany. As a post-doc she worked in West Berlin as a palynologist (1982–1985) in close cooperation with vertebrate palaeontologists. She spent ten years as assistant professor at the ETH of Zurich, Switzerland, mostly studying Mesozoic and Tertiary pollen floras from Antarctica in order to understand southern high-latitude past vegetation and climate change. Since 1996 she has held a position as curator of the Mesophytic plant collections of the Museum of Natural History, Berlin. Her primary research area is now the early evolution of angiosperms and she is especially interested in the history of geosciences and palaeontological collections.

Davis Young is a Professor of Geology, Emeritus, Calvin College, Grand Rapids, MI, USA. His most recent books are *Mind over Magma: The Story of Igneous Petrology*, and *John Calvin and the Natural World*. A forthcoming book co-authored with paleontologist Ralph Stearley, *The Bible, Rocks and Time*, is addressed primarily to Christian readers. This work presents much of the geological evidence for Earth's antiquity and includes chapters that summarize the history of ideas about geologic time.

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