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Dr. Hilde Grebe working as a palynologist at the
Geological Survey of Nordrhein-Westfalen at Krefeld

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EDITOR: Gregory A. Good
History Department, Woodburn Hall, Room 202A
West Virginia University
Morgantown, WV 26506-6303 USA
Tel: 304-293-2421 Ext. 5247 Fax: 304-293-3616
E-mail: ggood@wvu.edu

JOURNAL/SOCIETY WEB SITE: www.historyearthscience.org

BOOK REVIEW EDITOR: Gretchen Luepke Bynum
650 Alamo Court #6, Mountain View, CA 94043 USA

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EDITORIAL

NEW ASSOCIATE EDITORS

In the last issue, I expressed my sincere appreciation of the many years of service rendered by the former Editorial Board of **Earth Sciences History.** Now I take equal pleasure in welcoming a new group of Associate Editors to the journal. The duties of the Associate Editors include advising the Editor, especially in areas beyond my expertise. The group includes individuals knowledgeable in histories of paleontology, geology in different periods and places, geophysics, geography, space science, meteorology, climatology, mineralogy, oceanography, and more. Several Associate Editors have linguistic abilities and backgrounds that will also contribute greatly. At the moment, eight countries are represented by the twelve Associate Editors. The new board will certainly grow somewhat in the months ahead. The term of office of Associate Editors coincides with my own: 2002, 2003, and 2004. At that point, a new Editor will take the helm and may well wish to name a new board.

The duties of the Associate Editors are somewhat broader than those of the previous board. Associate Editors may solicit or accept manuscripts for review. They may also "manage" the referee and revision processes for prospective authors. An Associate Editor and I will confer concerning acceptance or rejection of submissions. Hence, prospective authors may submit articles to me as in the past, or they may submit directly to the Associate Editor who seems most appropriate for the subject of the manuscript. Contact information for Associate Editors appears inside the front cover of the journal.

The Associate Editors will increase the efficiency of the journal. I have often felt swamped with all of the tasks of correspondence, finding referees, goading referees, guiding authors through revisions, copy editing (myself), bringing articles into line with the journal style, and reviewing of proofs. Although I will continue to do each of these tasks to some extent, the assistance of the Associate Editors will allow concentrating my efforts more productively. I look forward to bringing the journal back on schedule with their help, but even more, I anticipate that a more scholarly and more interesting journal will result.

This issue includes an article on women in paleobotany in Germany and one on the beginnings of geology in France. Both of these articles started out in languages other than English. Thanks to careful translation by the authors and others (including Michael Mackert, engaged by HESS and West Virginia University), these articles can now reach a broader audience. It is possible that in the future some articles might be printed in languages other than English. Readers should note that in Kenneth L. Taylor's article on Nicolas Desmarest, although all quotations have been rendered in English, the original French is printed in the footnotes so that readers may judge fine points for themselves. Professor Taylor has retained contemporary spelling and punctuation, as well as Desmarest's "idiosyncrasies or errors," rather than obscure what is original to Desmarest and what the historian superimposes.

Both articles merit some comments. Barbara A. R. Mohr and Annette Vogt have undertaken an exhaustive investigation of the actual careers of women in one field of geoscience in the twentieth century, in one country. They based their empirical investigation on printed and archival sources, and also on oral history interviews. The investigation raises a number of interesting issues, including reasons that some fields have been more receptive to women than others and the porosity of disciplinary boundaries. But whatever one might make of their inter-

pretations, their careful exposition of the data firmly establishes a baseline that others can draw on.

Kenneth L. Taylor terms both Desmarest and himself "empiricists in principle," an apt description which this article systematically embodies. Throughout the essay, Professor Taylor carefully distinguishes his interpretations from his sources. He notes the limitations of the manuscript sources and the effect of this on his interpretation. Desmarest and Taylor provide models for historians of science and scientist-historians. Scholarship demands a cautious empiricism, in which the values of claims and interpretations are judged by how well the investigator supports them with evidence, whether from nature or from texts. Taylor's meticulous essay, appropriately, is dedicated to François Ellenberger, the accomplished French historian of geology who died in 2000. This essay appeared originally in 1997 in a volume dedicated to M. Ellenberger on his 80th birthday. I extend personal thanks and the appreciation of HESS to Gabriel Gohau and the Comité des Travaux Historiques et Scientifiques for permission to print this translation.

Lastly, I am now preparing a series of indexes to the first twenty volumes of **Earth Sciences History:** an author index, an index of key words in titles and abstracts, and an index of books reviewed in the journal. These will appear either in one of the two regular issues of volume 21 or as a special issue.

Correction: An error in a book review in vol. 19, no. 2, must be noted. The price of H. S. Yoder, Jr., *Planned Invasion of Japan 1945: Siberian Weather Advantage* was mistakenly listed as $35. It is $25. Hatten Yoder reports that there are still 160 copies in stock.

LETTERS TO THE EDITOR

Dear Editor,

I do not normally reply to reviewers, but there are matters in W. A. S. Sarjeant's inaccurate discussion [EARTH SCIENCES HISTORY, 19:142–145] of my *Gideon Mantell and the Discovery of Dinosaurs* (1999) that only the misrepresented author can clarify.

Mantell did not wait until 1838 to reject James Parkinson's equation of biblical days with geological periods. He never argued for that concept in any of his publications. His mistaken ideas concerning *Cetiosaurus* (see all references below) and Siberian mammoths derived from Richard Owen and Roderick Murchison, respectively. The more important relationships were with Cuvier and Lyell. It is untrue that Walter Mantell discovered whole moa eggs in New Zealand; he found shell fragments only (Dean, *Gideon Algernon Mantell,* 1998, pp. 107, 208). I did not say that the Pentland letters are "often misdated." My word was "sometimes," and I happen to know from my own research that more than one date is wrong. I *commended* Torrens's paper on Mary Anning. To say erroneously that I "dismissed" it is an egregious misreading, as if intended to stir up trouble between two mutually respecting colleagues. I cite Torrens and Cooper on Richardson in both my 1998 and 1999 books; their paper is flawed because my resources were more comprehensive than theirs. On the other hand, they knew some things that I didn't. I ignored Norman on the Maidstone Iguanodon because he argued mistakenly about Mantell's attempted restoration of 1832 (Dean, *Gideon Algernon Mantell,* 1998, #96), which I discuss in both my books. Norman was of course unaware of "Reptiles restored," an important discovery of mine that Sarjeant failed to mention—and could hardly have missed because it was featured on the book jacket. "The Weald is that part of South-East England bounded by the North and South Downs" (W. Gibbons, *The Weald,* 1981, p. vii) and may therefore be described as a valley. Having researched the topic himself, Sarjeant is surely aware that miscellaneous bone finds do not constitute discoveries (of dinosaurs) until they are reasonably identified. Like birds, pterosaurs could glide as well as fly.

The unannotated, undated sketch by Mantell of *Iguanodon*'s osteology (Dean, *Gideon Algernon Mantell,* 1998, #96; Dean, *Gideon Mantell and the Discovery of Dinosaurs,* 1999, p. 121) is of great interest, being the first attempt ever to reconstruct the skeleton of a dinosaur. Norman argued that Mantell drew his sketch in 1834, on the basis of the Maidstone Iguanodon. I think the correct date is December 1832 and that the sketch was constructed from loose bones rather than a matrix specimen (which he did not then have). The famous "horn" (since reidentified as a spiky thumb bone) in Mantell's sketch was not present in the Maidstone Iguanodon. One of the front limb bones, moreover (as Norman himself pointed out to me), does not belong to *Iguanodon* and was likewise absent from the Maidstone specimen. In 1833 Mantell used his sketch to suggest a lifelike reconstruction of *Iguanodon* (also the first for any dinosaur) based on its resemblance to the iguana. This, together with other such restorations, became the unpublished (till 1999) "Reptiles restored," a finished work of art explicitly dated 1833. Created more than one year before the discovery of the Maidstone Iguanodon, it constituted evidence that Norman, expert on *Iguanodon,* had not seen.

Probably in 1834, however, after the Maidstone Iguanodon had come into his hands, Mantell emended his sketch, retracing some of its bones in a different ink, as if to coordinate his earlier conjectures with the later discovery. Norman was therefore correct in associating the sketch with the Maidstone Iguanodon but had been misled as to its fuller history.

At several points, Sarjeant attempted to fault my work by citing facts that have only recently come to light. He failed to remember that my book describes what knowledge was like during Mantell's lifetime, not what it has since become. In summarizing more current knowledge, I do not claim that the extinction of the dinosaurs was either "abrupt" or "sudden." I say instead "At the close of the Cretaceous period, sixty-five million years ago, dinosaurs of all sizes rapidly died out—just how suddenly or why, no one is sure" (2). On p. 107 I am discussing nineteenth-century concepts, as I do throughout. Nor do I say, on p. 2, that the extinction of other Cretaceous genera was "sudden," though Sarjeant attributes the word to me twice. Belemnites may have hung on a little longer, but they are still regarded as having become extinct about that time (Charig in Sarjeant, ed., *Vertebrate Fossils and the Evolution of Scientific Concepts,* 1995, p. 313). On p. 272 I say "At the end of the Mesozoic Era, between the Cretaceous and Tertiary periods, a great wave of extinctions occurred. As a result, the dinosaurs, plesiosaurs, icthyosaurs, mosasaurs, and pterosaurs all disappeared, as did the ammonites and (a little later) the belemnites."

Regarding the "minor errors" (some of them valid), there were two nineteenth-century spellings of "Ornithic(h)nites." The H-less one, which Sarjeant faults me for, was earlier and appeared in the *Proceedings* of the Geological Society of London; the other spelling appeared in the *American Journal of Science.* In my 1998 bibliography of Mantell, I preserve each spelling in the proper place (#154, 158). The word "chelonite" appears in Owen (1842), p. 173; it means a piece of fossil turtle (rather than the living animal). I used it as Owen did. As to various forms of "iguanodon," I wasn't using the last two of Sarjeant's four at all until Sarjeant himself, as editor of the Halstead volume, insisted that they be preferred to my consistent nineteenth-century usages. If Sarjeant regards "outrightly" (attested from 1642) and "snidely" (from 1953) as "innovative vocabulary" (i.e., nonce words not in common use), he needs to buy a better dictionary. I am astonished to find that the sentences he next quotes from me "defy comprehension" as they have been clear to everyone else. The indented quotation from my page 225 is a series of three independent clauses (with individual subject and verb), each linked to the next by a stated or implied coordinate conjunction, and the whole arranged to form an elementary compound sentence. It is not a difficult construction for anyone who knows basic grammar.

Sarjeant also faults me for challenging Owen as to the supposed unity of the suborder Dinosauria. He is welcome to his own opinion about that, but, as Alan Charig wrote in 1979, "the 'Dinosauria' have not yet been shown to form a natural group" (p. 15). Since Charig's expertise was in dinosaurs (as opposed to dinoflagellates), I prefer his authority to Sarjeant's. All of Owen's defining characteristics of the group Dinosauria have been shown to be false. Therefore, any evidence cited in support of saurian unity must be based on discoveries that Gideon never lived to see.

Finally, Sarjeant complains that I do not say enough about Mantell and the dinoflagellates, a topic on which he has written well himself. I discuss Mantell and microscopics, citing Sarjeant; beyond that, I would refer him and others to the title of my book. It is worth pointing out that I have also published two other relevant books, both of which Sarjeant ignores. In the short time since my work

on Mantell has appeared, two copycat books on dinosaur discoverers have also appeared, each of them largely sans apparatus. Both were written in less than a year whereas my research, based largely on manuscript sources, required more than twenty.

Dennis R. Dean

834 Washington Street, Apt. 3W, Evanston, Illinois 60202

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Dear Editor,

When a historian of science, in the early pages of a book (Dean, *Gideon Mantell and the Discovery of Dinosaurs,* 1999), censures earlier historians and biographers for their "slipshod scholarship" (p. 6), he is effectively inviting a critical examination of the quality of his own scholarship. Dr. Dean's book contains much that is new and interesting concerning the life of Gideon Algernon Mantell—as indeed it should, when Dr. Dean has had access to manuscript resources unavailable to earlier historians. However, this reviewer discovered—and, in view of that early, lordly pronouncement, felt obliged to comment extendedly upon—much that is slipshod in his own work.

Dr. Dean succeeds also in arousing one's hostility by his critical comments on contemporary geological historians. On what page did he commend Hugh Torrens's paper on Mary Anning? Not on any of the five pages cited in his index. Instead, Hugh Torrens found his extensive researches dismissed on p. 58 as a mere "recent updating" of W. D. Lang's earlier writings and his joint study of George Richardson, when mentioned on p. 156, disparagingly annotated as cited "despite some flaws". I can assure Dr. Dean that my friend Hugh was as deeply offended by these carpings as were Justin Delair and I, when informed condescendingly that the letters by J. B. Pentland we had published were "misdated"—letters, be it said, that Dr. Dean had seen only in our published version, since he has not visited the University of Nottingham library where the originals are kept.

I wonder how David Norman will feel, on having his paper on the Maidstone *Iguanodon* (1993) first of all ignored and now dismissed because "he argued mistakenly about Mantell's attempted restoration of 1832"? That paper contained much other original scholarship, as did John Cooper's paper on George Bax Holmes (1993)—also ignored, without explanation of why, even now.

Dr. Dean chides me for my lack of knowledge of vertebrate palaeontology—a subject upon which, in fact, I teach two courses. Yet he can envision ichthyosaurs and plesiosaurs eating bivalves (p. 1) and, on the same page, can imply incorrectly that Jurassic pterosaurs were bigger than their avian contemporaries. Not so; most pterosaurs were small—*Pterodactylus* was the size of a sparrow—and the truly huge ones were not "gliding" over Mesozoic seas till the Late Cretaceous.

Concerning the Dinosauria, surely Dr. Dean must be aware of the many discoveries that have been made since "Alan Charig wrote in 1979"? Among subsequent publications is the most substantial work yet published on dinosaurs. Three leading contemporary specialists edited that work and at least a dozen others were contributors, yet they were content to title it *The Dinosauria* (1990). Moreover, its editors wrote as follows (p. 4):—

> While it once seemed an easy matter to classify animals as saurischian or ornithischian, recent reconsiderations of dinosaur relationships have forced the classic bipartite division to be abandoned. . . . There is a monophyletic assemblage that includes all animals we choose to call dinosaurs.

Perhaps Dr. Dean should extend his paleontological readings beyond the works of my late friend Alan Charig?

Dr. Dean claims that certain passages, which defied my comprehension as reviewer, were "clear to everyone else"—a remarkably sweeping assertion. He states also that "anyone who knows basic grammar" can construe the turgid text on p. 225. Does he truly believe that the meaning of such phrases as "a section on geysers added New Zealand to Iceland" are anything other than bewildering to readers?

So, for Dr. Dean, any lower land between higher ridges, like the Weald, constitutes a valley? Yet the Weald does not accord with any of the three definitions given in *Chambers' Twentieth-Century Dictionary* (1962 ed., p. 1221), since it is not "an elongated hollow between hills"; neither is it "a stretch of country watered by a river" nor "a trough between ridges." It is perhaps because Dr. Dean so often utilises his own concepts, without explaining them, that so many paragraphs furnish such heavy work for the reader.

When Dr. Dean uses terms like "chelonite" (p. 184), an explanation would have been useful: how, otherwise, might readers know that it was "a piece of fossil turtle" and neither a mineral nor a mis-spelling?

In his response, Dr. Dean denies that he claimed the end-of-Cretaceous extinction of the dinosaurs to be "abrupt" or "sudden": yet he speaks of their "abrupt extinction"—and *not* in a contemporary citation—on p. 107 and, on p. 2, claims that they "rapidly died out." Of course, as we now know, they didn't die out, since they had long since grown feathers and given rise to the birds.

The term "ornithichnites" did not originate in a paper in the *Proceedings* of the Geological Society of London: it was formulated by Edward Hitchcock (1836) for what he believed to be bird footprints in the red sandstones of the Connecticut valley and was merely borrowed by English writers. It seems Dr. Dean had his reasons for using a misspelling, but they should have been explained. S. H. Beckles's discovery of fossil footprints "during the 1850s" (p. 189) was *not* the first in southeast England: they had been found earlier by the Reverend Edward Tagart in Sussex (1846) and by S. M. Saxby (1846) on the Isle of Wight. [Mantell's

discovery of a Wealden footprint on the Isle of Wight, cited by Beckles (1851), may well have preceded Saxby's find but cannot be dated.]

I did not state that Mantell ever accepted Parkinson's ideas, noting only that he rejected them explicitly in 1838. No claim was made that he had ever utilized them.

Dr. Dean's dismissal of the significance of "miscellaneous bone finds" is surprising when he claims priority for Mantell over Buckland in the discovery of *Megalosaurus,* on the basis of Mantell's finding of "some fragments of a cylindrical bone" in the Wealden of Tilgate Forest, Sussex (p. 70). Yes, those might have been fragments of the skeleton of a carnosaur; but they were not bones of the creature "soon to be known as *Megalosaurus,*" since that dinosaur has been recorded only from Middle Jurassic strata.

All in all, after reading Dr. Dean's critique of my review, I am in no way persuaded that I misrepresented him. I concluded that review by observing that, despite his book's demerits, "it is destined to remain of long-term importance." This remains my opinion. I trust also that Dr. Dean may persuade the Alexander Turnbull Library to publish the Mantell Family Papers (p. ix) in full, for they are very evidently a mine of information for researchers on the history of paleontology.

William A. S. Sarjeant

Department of Geological Sciences, University of Saskatchewan, 114 Science Place, Saskatoon, Saskatchewan, S7N 5E2, Canada

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