ESSAY REVIEWS

Vic Baker, Book Review Editor

THE PRACTICE OF BRITISH GEOLOGY, 1750–1850. *Hugh Torrens. 2002. Ashgate Variorum Publishing, Burlington, Vermont and Aldershot, Hampshire, U.K. 356 p. Hardcover, $111.95*

In this new collection of papers by Hugh Torrens, the author admits in his introduction that his historiographic practices are entirely stratigraphic. I must say I "raised my eyebrows" at this, rather than at his suggestion that I would do so because he lacks qualifications in history. Scientists don't write histories; if they decide to write history then they become historians, and if they don't recognize this fact, then they risk becoming bad historians. Torrens long ago made the conversion to a historian, and this intentionally disarming remark rather underplays the rigor and sophistication of his historical methods. But yet his analogy does ring true. Rarely—perhaps never—does Torrens refer to any historiographic theory or methodology, and rarely too will you find his very empirical work situated in the wider context of the rich secondary literature on the history of geology or science. He is very much working with his nose up against a stratigraphic section of archival sources, contemporary literature, and sites. One only needs to read the paper in this volume on John Williams (1732–1795) to see the dense stratigraphy of his history writing. It reminds me of Martin Simpson (1800–1892)—the curator of Whitby Literary and Philosophical Society, best remembered for his work on ammonites—who constructed stratigraphic logs to the finest resolution. Every bedding plane was counted and annotated. In Simpson's era it was not the kind of stratigraphy the science demanded, and it never gave him wealth, fame, or reputation. In contrast, Torrens's dense stratigraphic historiography has at least brought him the latter.

Simpson died in poverty and obscurity. It was a typical Victorian life—if we are to believe Charles Dickens (and Dickens could have made much of Simpson). With its sad ironies and downtrodden subject, I can also see that Simpson's story would appeal to Torrens, whose subjects tend towards the lowly, the unappreciated, and the disenfranchised. Though I doubt that anyone who knows him would think it, I can't help thinking that there is something of the romantic in his choice of subject; there is certainly something political (with a small "p") in it.

Rather notoriously, Torrens has published in out-of-the-way places. Perhaps this has been a guerrilla tactic against the much-disliked Research Assessment Exercise, which all British academics have to undergo, and which straitjackets creativity and innovation. Although his pattern of publishing has a tendency to make some of his work difficult to obtain, it also reflects an egalitarian quality that matches his view of the history of science as tending towards a distorted and rather elitist view of the past. To have some of his best papers brought together in a single volume, then, is something to be celebrated. The papers have also been carefully chosen so as to give an overlapping and, therefore, fairly integrated view of the practical man and the world of mineral (particularly coal) exploration.

The dense empiricism of these papers gives them added value, as the ideas expressed here have not dated much at all. And, looking at the subject matter, one can almost imagine how the author's interests have evolved and diversified:

William Smith and Bath link to John Farey, who also links to Williams, while all three take us into the world of failed coal prospecting. Ryan is mentioned by Farey and Farey wrote in *Rees's Cyclopaedia,* one of Torrens's favorite works, and so on. In searching for the obscure, with a rigorous perseverance, he has turned up a wealth of other data, which in his magpie-like way has been added to his collection of people-facts. Again there are parallels in the history of geology: Richard Ripley (1788–1857), a contemporary of Simpson, produced a rich collection of rare ammonites by distilling them from the mass of material which laborers bought into his fossil dealership. Torrens has created a mental and physical repository from reading vast amounts of primary material. It is an unparalleled collection in this field, and particularly so for being almost totally concerned with those actors who are missing, or appear only as names, in the histories of the great and the good. Like the early stratigrapher, Torrens is working with partial sequences of collected facts and joining them up in the equivalent of his museum (in this case it is a museum of paper—his archive). He does this with a caution for which William Smith was noted in later life. Smith had learned, as the years passed, to be patient and cautious, to gather and ponder, and to triangulate a solution from a plethora of little facts.

These days, when all that the successful (in the popular sense) writer in the history of science needs is the speed, accuracy, and the methods of a journalist, it is perversely refreshing to delve into Torrens's dense prose. While these popular writers hang a lot of fiction on a mile of clothesline suspended between their two facts, Torrens puts his clothesline posts so close together there is hardly any room for the washing! While many popular writers give us histories with the consistency of soup (tasty but with little substance), our cutting-edge historians of geology make histories which have that degree of plasticity and elasticity demanded by modern historiography. In contrast, Torrens is, for the most part, pouring concrete—solid documentation of events built on hard won evidence, conducted with a belief that by this means one gains the truth. Of course, the fact of the matter is that there is as much interpretation here as there is in those thematic monographs that grace our field.

The Williams paper typifies the challenge Torrens sets himself. To deal with the obscure using archives is extremely hard (far easier to write biographies of great men and published works). To deal with those that travel and, as he says, "do rather than write" is next to impossible. Then to set oneself the highest levels of empiricism and select subjects who have very common British surnames is utterly crazy. It explains why Torrens can be working on a subject for more than twenty years before he has material that will satisfy him, and why he constantly runs the risk of having his work pirated by some charming journalist.

The papers here all reveal his particular interest in biography. He has long been known as an authority on William Smith and Mary Anning; Smith appears in this volume but Anning does not. He is also the biographer of hundreds of other less well known individuals, and has made a massive contribution to the *New Dictionary of National Biography.* However, for me, Torrens gets most interesting when he enters the wider social context, looks at relationships and juxtaposition, gets to the contemporary context of scientific discovery, reveals networks and sites of activity, and gives a sense of another world of geological practice. Each of these papers was like a minor earth tremor shifting the ground which underpins the traditional history of geology, but unfortunately we are not given an introduction here that really sets them in the context of a changing historiography of the history of geology. And it is here, in the very short introduction, that I find my only fault. There have been major shifts in thinking since Roy Porter published his groundbreaking review of the development of the science in 1977. Books by Gillispie, Rudwick, Secord, and even my own work, have been about practice, and it is these—amongst many others—which should give context to the work presented here. I do not find Torrens's criticisms of Oldroyd, Porter, and Rudwick relevant to this purpose, or indeed correct. Over the last thirty years (and as can be seen in the papers in this volume) Torrens has produced a considerable weight of evidence for another view of the science. His work shows the profound and subtle influence of Smith in particular, but sets Smith in a wonderfully interconnected world of practical men and women. This needed to be overtly stated rather than left to the reader to discern, but again parallels with Smith come to mind. Despite creating the most beautiful, rigorous and accurate maps and sections, Smith remained an empiricist supposedly with no theory to sell. He left it to others to take his ideas and slot them into the bigger picture. Of course, Smith did, and Torrens does, have their own precious theories to sell, but both have preferred to expound them verbally and be rather more circumspect when it comes to committing them to paper! Both Smith and Torrens have been used and abused by those who sought a little fame and reputation.

The thirteen papers in this volume include, in addition to a biography of Williams, some information on James Ryan (c1770–1847) and his inventions for boring, and a rather different interpretation of boring in the context of geological activity in the Bath area. Here, Torrens tells us it was E. M. da Costa (1717–1791) that was boring Josiah Wedgwood. The richness of the Bath scene in the eighteenth century is particularly interesting, though we are slightly led astray by Torrens calling this 'geological' communication when the communicators would have no conception of the word. However, such historiographic niceties were not prevalent when the paper was first published (1979, not the 1997 of the second edition given here). Certainly the paper provides an utterly remarkable picture of a localized eighteenth-century interest in fossils and rocks (it is quite an amazing context for Smith's development), which, although not peculiar to Bath, does seem to be extraordinary and extraordinarily influential. Here we get the first of a series of contributions on the history of William Smith's idea, which is further developed in an essay on the Brewham trials for coal. (It is unfortunate that this latter paper is still in its original French as it is particularly important and rather less accessible in this form). John Farey then enters the story in a discussion of Joseph Banks's patronage, and Farey and Smith's relationship. Farey is then given his own paper. All these papers work wonderfully well together and, given that data-rich Torrensian approach which lays each sequence next to each other, the reader can reach for correlations and construct that bigger view. Here we are also given a Farey bibliography of 258 works. It is indicative of Torrens's rigor but what is remarkable is that he does not get lost in this detail (he is no mere bibliographer), he always has something of significance to tell us, even if the history of science has cast these as minor players. Before Torrens paid attention to Farey, Farey was almost completely unknown—a footnote rather than a key participant. That has now changed, and in his own way Farey was as significant to those early years as the likes of William Fitton were to be later on; indeed, he may have been more important. Farey also appears in a paper on the Bexhill attempts for coal in the first decade of the nineteenth century. We then return to a more detailed study of Ryan before the examination of Arthur Aikin's (1773–1854) key contribution to Shropshire geology and Murchison's claims of entering an intellectual wilderness to make his own discoveries (also tackled in a separate paper). Here Torrens gives a measured reading of the world of provincial geology making its transition to fossil-centered stratigraphy, and in this regard Aikin, as a practitioner in the early part of the century, proves particularly useful. With typical rigor, Torrens, here, even corrects Fitton about a key date—even though Fitton was a contemporary of the event! Torrens demonstrates that Aikin, like Smith, was a financial failure not an intellectual one, though history has sometimes judged them otherwise.

And, as if to parallel the Farey bibliography, here we are given a remarkably detailed breakdown of the subscribers to Aikin's Shropshire survey. The last three papers continue this theme of mineral surveying with: Joseph Harrison Fryer (1777–1855)—who traveled to Bolivia; William Logan (1798–1875)—who was recently nominated as the most important scientist in Canada's history (rather a surprise to Torrens who must have then wondered why he was studying him—surely the antithesis of his usual subject!); and finally James Buckman's (1814–1884) consulting work in the mid-nineteenth-century coal fields of Virginia.

Many might be surprised to learn that this is Hugh Torrens's first book—he has produced the equivalent of many books over the years but has the stratigrapher's preference for papers. The paper is, however, the perfect vehicle for his peculiar kind of high resolution, biographically driven research and surely only by this means has it been possible for him to produce these extremely important insights. He has also been able to underpin, enrich, and evangelize the development of the history of geology—most of these papers were presented in some form at a conference or meeting. In other words, he too, like many of his subjects, has spent a good deal of his time 'doing' though we are fortunate that he has also published and carried through his research to the most exacting standards. At 356 pages this is a solid piece of work, but with a factual density of encyclopedic proportions it seems even bigger. The papers demonstrate admirably how he has forced historians of geology to re-examine presumptions about the contributions of great men and the surveyor. This is a particularly valuable addition to the Variorum series, and a book to sit on the shelves of anyone serious about the history of geology.

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PLATE TECTONICS: AN INSIDER'S HISTORY OF THE MODERN THEORY OF THE EARTH. *Naomi Oreskes, ed., with Homer Le Grand. 2001. Westview Press, Boulder and Oxford, 424 p. Hardcover, $35.00*

This is a fascinating book. Historians Naomi Oreskes and Homer Le Grand (whose participation was intermittent) did a major service by inviting a number of contributors (to the initial concepts of, or to bases for concepts of, plate tectonics) to write personal accounts of their backgrounds, how and why they became involved, and what they accomplished. Most of the sixteen—twelve geophysicists, two geologists, and two hybrids—whose essays appear here, responded with commendable candor. An additional chapter, by Dave Sandwell, discusses the sequence and relative importance of the different types of geophysical data that went into, or could have gone into, development of plate concepts.

Ron Mason leads off the scientists' essays with an account of the technology and surveys that led to the late-1950s recognition in the northeast Pacific of seafloor magnetic stripes and their apparent great strike-slip offsets (later explained by others, with his prompt acceptance, as transform faults and fracture zones). Fred Vine follows with an account of his schooling and research that led to the Vine and Matthews tape-recorder explanation for magnetic-anomaly bands. Next, Lawrence Morley recounts his simultaneous and independent recognition of the tape-recorder concept, and is rightly resentful that reviewers for *Nature* and for *Journal of Geophysical Research* blocked publication of views contrary to their own assumptions that neither magnetic-field reversals nor seafloor spreading could occur. Oreskes's comments on this make apparent her unawareness of the obstructive role played far too often, now as then, by peer-reviewers of manuscripts, grants, and appointments who block dissemination of views with which they disagree.

Lamont marine geophysicists rejected not only continental drift but also seafloor spreading when first proposed. However, thanks to Maurice Ewing and Manik Talwani, only Lamont had easily recoverable global magnetic and seismicity data, so its scientists were uniquely positioned to test the new concepts, and did so quickly when motivated to examine them several years later. Walter Pitman here describes his 1965 cruise as a graduate student, beginning as a disbeliever in both paleomagnetic evidence for continental drift and magnetic-stripe evidence for seafloor spreading and ending with a remarkably symmetrical long magnetic profile across a ridge that demanded the spreading explanation. Work with other Lamont records quickly defined the global geometry of spreading. Neil Opdyke, the only early Lamont "drifter" other than isolated geologist Bruce Heezen, describes his work with paleomagnetism and the magnetic-reversal chronology of young marine sediments.

The essay by the late Gordon MacDonald is bizarre. In papers published in 1960–1964, before the rise of plate concepts, MacDonald embellished bad assumptions with impressive mathematics to "prove" Earth to be so rigid that continents and oceans are permanently fixed in both relative and absolute positions. The pleased stabilists who then utterly dominated American geoscience showered MacDonald with awards, including membership in the National Academy of Sciences, while he was in his late 20s and early 30s. I had a long discussion with him in 1964, with complete philosophical disconnect. I argued that the geologic and paleomagnetic evidence for continental drift, and for major strike-slip faulting, was overwhelming, although I did not know the mechanism. (I did not see plate tectonics coming.) He argued that such motions are impossible, so all purported evidence for them is either false or misinterpreted. His present essay maintains this attitude: only the lack of mathematical competence by mobilists permitted development of nonsensical plate tectonics. He claims, petulantly and falsely, to have been one of a small minority of right-thinking scientists who published stabilist papers in the early 1960s, whereas in fact he epitomized, and benefited hugely from, the ruling stabilist dogma of those late pre-plate years. As Peter Molnar emphasizes in his own essay in the book, much current computer modeling is [like MacDonald's mathematics] mere elaboration of bad assumptions.

John Sclater in his chapter describes the slow integration of oceanic heat-flow measurements into mobilism and plate tectonics. He notes that Richard Von Herzen proposed in his 1960 thesis that mantle convection beneath oceanic crust caused the moderately elevated heat flow at ridges. The heat-flow community was late accepting seafloor spreading because measured ridge heat flow is much lower than they anticipated for that process. After they did accept spreading, Sclater and associates rationalized, as they still do, the deficit to be due to seawater circulating in hot, young oceanic crust, which indeed must be a factor; but as Anne Hofmeister has pointed out, their calculations assume oceanic lithosphere to have constant conductivity, whereas the actual dramatic decrease in conductivity with increasing temperature requires most of the observed discrepancy and invalidates most published thermal modeling of such lithosphere and also most calculations of Earth's heat loss.

Bruce Bolt shows that plate-tectonic comprehension required, and followed quickly upon, seismological advances in the 1960s—deployment of the world-wide standard seismograph network, computerizing (in which he played a major role) of earthquake locations, and development of methodology for first-motion solutions for fault slips. Jack Oliver reviews the contributions of seismicity and seismology to the rapid development of plate concepts. Earthquake locations and first motions by Lynn Sykes confirmed Wilson's proposal for transform faulting of spreading ridges, and Oliver recognized the subducting Tonga slab by its low attenuation.

Dan McKenzie discusses his role in developing the geometric concepts of plate tectonics. He emphasizes the simultaneous recognition of those concepts by several people working independently with analogous data in the late 1960s. He describes his own progress away from the assumption, still widespread, that hot rising convection currents directly drive seafloor spreading to recognition that ridges spread passively. He argues that plate tectonics is a proved and completed narrow explanation of surficial relative motions and does not incorporate dynamic theory, and here we part company. Surficial relative motions of the internally more or less rigid parts of plates indeed are proved, but once we consider (though few investigators do so beyond the level of invalid assumptions) even the surficial kinematics of rifting and subduction, there is no way that a host of dynamic problems can be ignored. McKenzie also separates internal deformation of plates, and other complexities such as back-arc spreading, aggregation, and rifting, from plate tectonics, whereas I agree with Bill Dickinson and Peter Molnar, in their essays, that these processes are products of plate interactions and are properly encompassed in the field.

Mathematician Bob Parker wrote the computer-mapping program that made possible geometric manipulations of plates and magnetic anomalies. Here, he describes also his work with McKenzie on plate kinematics.

Xavier Le Pichon explains that he wrote anti-drift papers at Lamont through 1966 because his teachers had misled him and because he saw undeformed trench-floor sediments in reflection profiles. (Lamont single-channel reflection profiles crossed many accretionary wedges, the concept of which did not then exist, but resolved almost no structure within or beneath them.) He denigrates Bruce Heezen's dead-ended expanding-Earth speculation without acknowledging that Heezen early recognized that ridges must be spreading but was persuaded by Lamont geophysicists that there was no convergence at trenches. Le Pichon jumped to the advancing bandwagon in 1967, and was briefly an important contributor to geometric two-plate concepts.

John Dewey narrates his coming to plate tectonics via regional geology of Britain and Appalachian Canada. He early recognized that post-Paleozoic opening of the North Atlantic was required by the geology on opposite sides and was arguing this publicly by 1965. A 1967–1968 sabbatical at Lamont gave him the new concepts of plate tectonics, and he was off and running with broad syntheses of continental orogens in plate-convergence terms. Tanya Atwater describes, with delightful enthusiasm, the interactions with people and data that led to her spectacularly successful Scripps-PhD plate-kinematic explanation of North American Cenozoic geology in terms of migrating triple junctions whereby the continental margin has evolved from mostly subduction to mostly strike slip.

Bill Dickinson tells, with the most polished prose in the volume, of his 1967–1968 evolution from a "casual stabilist" to a plate-tectonic mobilist. He worked early with island arcs and arc magmatism, and subsequently with sedimentary basins and sedimentology as related to convergent systems. He sketches the development of convergent-plate concepts, and describes the influential Asilomar conference he convened in 1969, which was attended by most of the people who would make major contributions to convergent-plate tectonics in the next decade.

Peter Molnar was a seismology graduate student at Lamont in the late 1960s. He presents an introspective account of his contributions there to data and concepts of subduction mechanics, and continues with descriptions of his subsequent work with, particularly, the dynamics of internally deforming continental plates.

(No author in the book mentions the need for study of strain in oceanic plates, which cannot have the ideal rigidity commonly assumed for them.)

These authors were in the right places during the critical few years, and they used their opportunities well. Early plate concepts were developed primarily at Cambridge, Lamont, and Scripps, whose denizens moved back and forth, and into which gifted outsiders like Tuzo Wilson and Jason Morgan came intermittently. Most of the authors were young professors or graduate students, and most were working with new types of geomagnetic and seismic data, and with new processing systems, at the leading edge of technology. Conceptual advances rapidly followed the technology. Some of the authors stayed with global tectonics throughout their careers, whereas for others it was a one-shot affair.

The subsequent rapid development of plate-interaction geologic concepts occurred at many more centers. The trend-setting papers were published in 1969 and 1970, and here also early associations mattered. Thus, as an outsider, I had advanced little past the beginning concept of two-plate convergence before taking a visiting professorship at Scripps, in the fall of 1968, where students, including Tanya Atwater and Dan Karig, brought me up to speed in triple junctions, back-arc spreading, and more. Further, my subsequent integration of onshore and offshore tectonics of the Indonesian region would have been impossible without the 1970 release to me by Maurice Ewing of the myriad Lamont reflection profiles, previously uninterpreted, in that region.

Useful additions to this volume would have been accounts by a developer of the late Neogene magnetic-reversal chronology, by a pioneer in applying Euler plate geometry (in addition to latecomers McKenzie and Le Pichon), and by a pre-plates "drifter" geologist. Also desirable would have been an account by an early paleomagneticist; Ted Irving contributed an invited paper, but withdrew it when editors insisted it be rewritten, away from objective history, with personal notes, to more wholly personal narrative.

Recognition in science tends to go to people who pushed concepts on the eve of broad acceptance, and not to earlier originators. The key to plate kinematics is Euler spherical geometry, and late-1960s formalizers McKenzie, Morgan, and Le Pichon are customarily given primary credit for it because their timing was right. Nevertheless (as McKenzie here acknowledges but Le Pichon does not), paleomagneticists were using Euler geometry to manipulate paleomagnetic poles, latitudes, and continental positions defined by them in the 1950s. S. W. Carey was implicitly using plates and Euler geometry for continental reconstructions in the 1950s. Jim Everett (unmentioned in this book, though he would have made a good additional author) recognized the need for Euler geometry, and he programmed and made the rigorous Euler fits of continents in the "Bullard et al. 1965" paper, which was based primarily on geometric work by him and geologic work by Alan Smith, and not on work or concepts by Bullard. Wilson recognized in 1965 the general pattern of plate tectonics, including three-plate interactions (but not their specific Euler nature). Princetonian Harry Hess is here repeatedly credited with a major pre-plate breakthrough because of his direct influence on participants, yet his concepts were vastly inferior to those expressed by outsider Bob Dietz at the same time, and little better than those proposed three decades earlier by Arthur Holmes.

Most of the writers (Sclater, Oliver, McKenzie, Dickinson) who here ponder how science really works see major advances, as do I, as coming primarily from new syntheses incorporating diverse key observations, building from details to new comprehension, in contrast to the deductive style, dominant in geoscience, of forcing new observations to fit old models. Several authors comment on the stifling nature of research-funding procedures. Molnar observes that developments are mature or senile by the time funding agencies focus on them, and urges more support for individual scientists seeking to create new and exciting topics. Dewey laments the distortion of georesearch as funding has become increasingly constrained by narrow thematic requirements, and as myopic specialists have lost contact with rocks and geology.

Oreskes and most of her authors accept plate-tectonic concepts as having quickly achieved ultimate truth, and yet those early developments merely defined surficial relative motions and are but a step in comprehension of how the Earth works. Few authors mention problems or suggest routes to further progress. There is, for example, no mention here of what I regard as self-evident: subduction, itself a byproduct of chilling of oceanic asthenosphere from above, provides the fundamental drive for plate motions, and thermal convection is a product, not a cause, of this drive.

Opdyke was a proponent of continental drift from 1956 onward. Of the other authors whose careers began before the late 1960s, geomagneticists Mason, Morley, and Vine regarded drift as established by paleomagnetic evidence, and Dewey recognized drift in the geologic evidence. More evaluation of why others were early stabilists would have been appropriate. Oreskes parrots the self-justifying assertion by many uninformed geophysicists that geologic data are so "unverifiable" as to have proved nothing about drift before geophysics was applied. Those who assert such ambiguity flaunt ignorance of the many independent but mutually consistent datasets. Geophysicists tend to work with one or two sets of quantifiable data, whereas geologists are more likely to integrate many types of qualitative information—to practice pattern recognition (a term that John Sclater here emphasizes in this context, with attribution to Jerry Winterer). The approaches are complementary and both are essential. Particularly compelling for me at the time were the accordant paleoclimatic, paleomagnetic, and marine and terrestrial paleontologic and paleobiogeographic indicators of late Paleozoic paleolatitudes, connections, and separations. Paleotropical indicators of all types are scattered from near the present North Pole (e.g., equatorial ringless wood in Spitzbergen) across the equator, whereas cold-climate indicators are strewn from Antarctica across the equator (e.g., continental ice sheets, bearing granite erratics, flowed on to South Africa from the east, where now lies the subtropical Indian Ocean). No repositioning of the spin axis through the present globe improves these pole-to-equator arrays. Provinces of distinctive tectonic, stratigraphic, and paleontologic assemblages are separated by younger oceans, whereas sharply disjunct provinces are juxtaposed across or within sutures (which were first recognized in the 1920s). Pre-plate evidence for the reality of drift is often denigrated because proponents did not perceive a viable mechanism—but in my view the current dominant assumption that hot, rising convection currents drive plates is false, yet this casts no doubt on relative plate motions. Powerful geologic evidence for reconstructions is often now disregarded in plate reassemblies; as, when the east side of India is placed against East Antarctica instead of western Australia.

Oreskes attributes (as in her 1999 book, *The Rejection of Continental Drift*) the pre-plate-tectonic dismissal of continental-drift to uniformitarianism, to a commitment to multiple working hypotheses, to anti-authoritarianism, and to a belief in Pratt isostasy. None of these were significant parts of the resistance I met everywhere as a publishing and lecturing pre-plates 1960s 'drifter'. (For my view of history, see *American Geophysical Union Geodynamics Series,* 2002, 30:359–410.) "Multiple working hypotheses" is pious cant for freshman classes: drift was rejected without consideration of evidence. To me, then and now, "uniformitarianism" requires, not refutes, drift. Isostasy?—pah. The resistance was due to *pro*-authoritarianism, to mindless acceptance of the dogma of permanence of continents and oceans intoned by uninformed gurus. The present broad acceptance of the easily disproved conjecture that fixed "plumes" rise from the deep mantle to the surface is a modern analog for pre-1970 stabilism.

Oreskes adds overview chapters and many endnotes (and also inserts references to her prior book in many of the narratives). These commonly are informative but are marred by many errors in science and some in history. For example, her discussion of gravity anomalies associated with forearcs (not "trenches") is garbled as to locations, assumptions, and significance of the anomalies at issue, and she wrongly asserts that Vening Meinesz and Hess interpreted the gravity correctly as requiring great dynamic downpulling of trenches, whereas Ewing and associates were foolish to not agree. (Indeed, Vening Meinesz himself castigated them in 1954 for disagreeing with him.) Gravity anomalies are merely differences calculated between observations and sets of arbitrary assumptions, and different types convey quite different signals. In this case, Vening Meinesz assumed that constant-thickness crust of density 2.67 g cm^{-3} everywhere lies directly beneath the sea floor. His calculated near-trench "isostatic" anomalies reflect primarily the huge difference between this bad assumption and the reality that arcward-thickening accretionary wedges of offscraped low-density sediments commonly underlie fore-arc slopes (not trenches themselves) and structurally overlie oceanic crust, which everywhere is much denser than 2.67. These wedges result in very large negative gravity anomalies, with the Vening Meinesz reduction, centered at the thickest parts of wedges, commonly at the fore-arc ridge, often 100 km or more from the trench, where the wedge can be 20 km thick. Ewing et al. did not then know about such wedges, but they argued, correctly, that low-density material, not dynamic imbalance, had to be the explanation of the large negative anomalies.

Such caveats aside, this book presents an invaluable account of the way science at its best really operates, and it deserves broad readership.

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BOOK REVIEWS

Vic Baker, Book Review Editor

THE EARTH INSIDE OUT: SOME MAJOR CONTRIBUTIONS TO GEOLOGY IN THE TWENTIETH CENTURY. *David R. Oldroyd, ed. 2002. Geological Society of London Special Publication 192, 369p. Hardcover, $142.00*

Diverse broad to narrow topics of geoscience history are treated, in part by historians and in part by participants, in this international assembly of papers. Both historical and scientific investigators will find items of much interest.

Editor Oldroyd begins the book with a summary of what follows. Davis Young reviews the career of Norman L. Bowen and the experimental and theoretical work that led him to regard magmatic fractionation as the dominant mechanism producing variety in igneous rocks. (Knowledge of phase petrology is vanishing; nowadays, rocks are represented mostly by chemists' powders and spreadsheets.) Jacques Touret and Timo Nijland expertly review two centuries of development of metamorphic petrology from a descriptive field to one integrating analytical and experimental methods and branching off into thermobarometry, chronology, and structural petrology. They lament the expulsion of the discipline from many universities as trendier fields get the financing. Bernhard Fritscher discusses the contrasted approaches, from about 1890 to 1930, to metamorphic petrology. One line led to metamorphic zonation, established by observation and description, and the other to metamorphic facies, derived from mineral-equilibrium rationales.

Cherry Lewis continues her careful exposition of the contributions of the remarkable Arthur Holmes. Ursula Marvin gives a wandering account of what has been learned about the Moon, and to a lesser extent impacts, meteorites, and other planets, as a result primarily of the space program, and variably integrates it with historical development of concepts and with narratives regarding prominent individuals. Richard Howarth presents an excellent and meticulously illustrated review of the evolution of graphical, statistical, and computer methodologies in geoscience.

Victor Khain and Anatoly Ryabukhin discuss the slow progress toward mobilism in Russia, where broad acceptance of plate tectonics came fifteen years or so after that in the West. Russia lacks the potential reality check available (although too widely ignored) in the West through the work of marine geoscientists working with actual plate boundaries. The result, although Khain and Ryabukhin are unaware of it, is that current Russian plate-tectonic literature is even more *ad hoc* than is most of the dry-land work published in the West.

Homer Le Grand writes on "Plate tectonics, terranes, and continental geology"—on the development of the notion that tracts formed elsewhere are accreted to continents by conveyor-belt plate motions. He mistakes the contribution of the several people who proposed the term "terrane" for such exotica to be that of development of a new field rather than of elaboration of an existing one. I am a biased observer: in the decade before the terraners got in the act, I published many papers identifying, and developing the criteria for so doing, many of the tracts at issue in North America, Eurasia, and Indonesia and surrounding regions.

Cathy Barton adds a welcome biography of neglected Marie Tharp, whose physiographic diagrams of the oceans, with Bruce Heezen, were for more than a decade the best maps available, and which strongly influenced the development by others of plate-tectonic concepts. Barton should have documented the falling out of Heezen and Lamont director Maurice Ewing, from which Tharp, as well as Heezen, suffered greatly, but the rift is barely mentioned. The account ends abruptly, and unsatisfactorily, in 1977, when Heezen died and Tharp's last ties to Lamont were suddenly cut.

Gregory Good briefly discusses the development, especially from 1940 to 1970, of concepts of geomagnetism and of the divergence of its topics into isolated specialties. Eugen and Ilse Seibold describe, also briefly, a century of increasing complexity and specialization in defining the character, deposition, and setting of sediments. Hugh Torrens discusses the uses and limitations of many methods for temporal correlation of strata, and laments the severe decline of quality work in stratigraphy, biostratigraphy, and paleontology. The late William Sargeant presented an exhaustive annotated bibliography of papers (including about sixty of his own) that he considered important in the branches of palynology in which he has worked.

The final paper in the book is for me the most important: the account by Simon Knell of the downsizing, dumbing down, and hyping up of government-funded geoscience activities as a result of pressure by conservative politicians in Britain and the United States. He emphasizes museums and paleontology but ranges widely across other research. He could have added, among other places in similar straits, Canada, New Zealand, Australia, and South Africa.

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BONE WARS: THE EXCAVATION AND CELEBRITY OF ANDREW CARNEGIE'S DINOSAUR. *Tom Rea. 2001. University of Pittsburgh Press, Pittsburgh, Pennsylvania. 276 p. Hardcover, $25.00*

Bone Wars is largely a historical narrative that focuses on the discovery, excavation, scientific study, and fame of the sauropod dinosaur *Diplodocus carnegii.* This dinosaur occupies a special place in the history of paleontology, and especially that of dinosaurs, because of the involvement of Andrew Carnegie, the American industrialist and philanthropist. Carnegie funded not only the excavation of the specimen but also the production of plaster casts of the skeleton that were mounted in numerous museums in the Americas and Europe, making *Diplodocus carnegii* the best known dinosaur in the world during the early part of the twentieth century.

The presence of mounted skeletons of this dinosaur in many of the world's great museums that resulted from Andrew Carnegie's philanthropy was a major contributing factor to the establishment of the public fascination with dinosaurs, a fascination that is stronger than ever today. Because of the role of this particular skeleton in the development of the public's interest in dinosaurs, *Diplodocus carnegii* is an appropriate subject for a book-length historical analysis. The story is an interesting mix of science, politics, and personal ambitions. The book is written for a general audience, but the detailed coverage of the subject matter will make it of interest to professional paleontologists and historians as well.

The dinosaur in question was discovered in 1899 in Wyoming. Most of Rea's book focuses on the period that brackets the discovery, from the 1870s, when O. C. Marsh and E. D. Cope were competing for dinosaurs and other fossil discoveries in the American West, to the early 1930s, when the last of the casts of the specimen paid for by Carnegie or his widow was given to Munich. The book also touches on a number of other subjects, most of which have close links to the main story, such as the railway business in the expanding American West, Antarctic exploration, and the early days of the Carnegie Museum of Natural History.

The book has a large cast of characters, but five play the most prominent roles. Andrew Carnegie was the driving force because of his desire for an exhibit-quality dinosaur skeleton for his new museum. Bill Reed was the dinosaur collector whose field work in Wyoming caught Carnegie's attention and led to the discovery. William Holland was the first director of the Carnegie Museum and orchestrated the field expeditions and the casting and mounting of the replicas of the skeleton. Jacob Wortman was the paleontologist who led the field expedition that collected the skeleton. John Hatcher was the brilliant young paleontologist who published the scientific description of *Diplodocus carnegii*.

Rea also considers some of the scientific debates of the time. Did the large sauropods such as *Diplodocus* have a sprawled posture similar to that of modern lizards or did they have an upright stance more like that of an elephant? Did the huge bulk of the sauropods confine them to aquatic environments where the water would have provided buoyancy? At the end of this chapter he considers more recent research such as that functionally comparing the tail of *Diplodocus* to a whip. The longstanding, but only fairly recently resolved, issue of the correct head for the closely related sauropod *Apatosaurus* is also discussed.

While they are interesting in their own right, I found some of the secondary story lines in the book a bit distracting; I found myself hurrying past them so that I could get back to the main subject matter of the book. Aside from the obvious links in time or place, the connections to the main story are not always evident. The amount of space allotted to the description of the Fossil Fields Expedition of 1899 organized by Wilbur C. Knight, Professor of Geology at the University of Wyoming, is especially puzzling. While some reference to Hatcher's fieldwork in Patagonia helps to explain the character of the man, again, the amount of space allotted to this topic seems unnecessary. I would have preferred that this space be devoted to a more in-depth analysis of the role *Diplodocus carnegii* played in the establishment of the public love affair with dinosaurs.

The book is clearly very well researched, and there are 41 pages of notes that follow the final chapter and a good bibliography. Unfortunately there are no references to the notes in the text so the reader has no way of knowing about which items one can find more information in this section. The text is very readable, and it is fairly well illustrated. I noticed only a few typographic errors.

I recommend this book to anyone with an interest in dinosaurs or any of the characters that function prominently in the story. Given the role of "Dippy" in the development of the widespread fascination with dinosaurs and all things dinosaurian, a book on this particular dinosaur is long overdue.

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EARTHSHAKING SCIENCE: WHAT WE KNOW (AND DON'T KNOW) ABOUT EARTHQUAKES. *Susan Elizabeth Hough. 2002. Princeton University Press, Princeton, New Jersey and Oxford, U. K. 238 p. Hardcover, $24.95; £17.95*

Research geophysicist Susan Hough of the U.S. Geological Survey has written a jargon-free book that not only describes but also gives a brief history of the science of earthquakes. *Earthshaking Science* is an outstanding addition to the body of work by earth scientists who write for the general public about their profession. I particularly liked the book's subtitle, because the author makes it abundantly clear how much there is that is not known about earthquakes.

After the Preface and Acknowledgments, the book is divided into eight chapters. Chapter One, "the Plate Tectonics Revolution," is a nicely encapsulated summary of an event that was earthshaking in its own way. This chapter will be of most interest to science historians. Chapters Two ("Sizing Up Earthquakes"), Three ("Earthquake Interactions"), and Four ("Ground Motions") have self-explanatory titles. Chapter Three contains an excellent description of the New Madrid earthquakes of 1811–1812 in the central United States. Chapter Five ("The Holy Grail of Earthquake Prediction") includes an account of the Parkfield [California] Prediction Experiment and why it turned out to be overly optimistic. Chapters Six ("Mapping Seismic Hazard"), Seven ("A Journey Back in Time"), and Eight ("Bringing the Science Home") complete this information-packed book.

Hough uses a device I have not seen previously: "sidebars" in the text with numbers analogous to figure numbers. This allows the author to add information that would otherwise break the flow of the narrative if it were incorporated directly into the text. More authors would do well to adopt this practice, instead of relegating all supplementary material into the Notes section.

The Notes in this book are in fact all references. And noteworthy in the "Notes" is the juxtaposition of Darwin's classic *Origin of the Species* (1859) with a year-2001 reference to an Internet website. (Note to future historians: what will be the longevity of an average website?) Terms are defined directly in the text, not in a glossary; this moves the reader along apace. Each chapter begins with an interesting quote and ends with an annotated, suggested reading list. An index is also included.

The only spelling error I saw was on p. 43: in the given context, "lightening" should be "lightning." Both are of course legitimate words, which only proves the quip,"Spellcheckers are knot infallible." As to the ever-present problem with people's names, the geophysical community will know that Allan Linde (p. 75) and Allan Lindh (pp. 117, 124) are different people. The lay reader should be assured that this is not a spelling error (I point this out as one who has had her name misspelled frequently).

This book definitely belongs in school and public libraries, and not just in known earthquake–prone areas like California. While this is not primarily a book on the history of a science, science historians could profitably consult it to see how one working geophysicist views the history of her science. For anyone interested in earthquakes, I highly recommend this book.

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JAMES HUTTON: THE FOUNDER OF MODERN GEOLOGY. *Donald B. McIntyre and Alan McKirdy. 2001 (revised and amended from 1997 edition). NMS Publishing Ltd., National Museums of Scotland, Edinburgh. 51 p. Softcover, £8.99/$14.95 (To order, contact Arthur Schwartz & Co., Inc., 15 Meads Mountain Road, Woodstock, New York 12498 USA; aschwartz@aschwartz.com)*

This small, attractive booklet provides an excellent introduction to James Hutton's *Theory of the Earth.* It has many excellent colored field photographs, three portraits, and more than a dozen mostly drawn from field trips by Hutton's friend, John Clerk of Eldin, to whom the book is dedicated. The book is printed on high-quality paper. I was surprised to find nine photos from North America, most subjects of which probably could have been illustrated from Scotland.

Illustrations occupy about half of the booklet, and the text is very succinct, but the combination is sufficient to accomplish the authors' purpose "to give Hutton's work the public attention it deserves, using his own words where possible and emphasizing that his knowledge was based on extensive observations of natural features and especially of rock outcrops." Apparently the booklet was stimulated by the recent completion of the "Our Dynamic Earth" museum, located in eastern Edinburgh only a half a kilometer from the site of James Hutton's Edinburgh home, where a memorial plaque was placed in 1997 on the 200th anniversary of Hutton's death.

The book is organized as follows: Introduction; Chapter 1. Discovery of Deep Time; 2. Field Observation; 3. Publication of the *Theory of the Earth*; 4. Interpreting the Testimony of the Rocks; 5. Confirmation by Field Evidence; and 6. Hutton's Legacy. This title of the first chapter set the stage for one the authors' principal messages, deep time, which complements the theme of Our Dynamic Earth. This is not the only important theme, however, for the authors also stress the importance of refutation and prediction to the testing of theories. These procedures were honed during the Scottish Enlightenment by such men as philosopher David Hume, economist Adam Smith, mathematician John Playfair, chemist Joseph Black, and Hutton himself. This emphasis is especially appropriate in light of doubts raised about Hutton's importance by revisionist rhetoric in recent years. For example S. J. Gould characterized him as already old fashioned in his reasoning and scolded him for formulating his theory first and then seeking empirical field evidence to support it. Others have argued that aspects of the theory had been anticipated by others, and that Hutton was in reality an obscure investigator on the fringe of the European intelligentsia. Be that as it may, Hutton still stands as the dominant exponent of a profound revolution in thinking, which saw the solid earth as dynamic rather than as a static platform upon which only the neptunian sea changed. We should remember that Darwin had Wallace; Agassiz stood on the shoulders of Charpentier, Venetz, and Alpine peasants; and Wegener was anticipated by Americans Taylor and Baker. Did this diminish their importance?

Gould's criticism is particularly surprising, for prediction has long been touted as an important element of the scientific process. The philosopher Karl Popper stressed the crucial roles of conjecture and refutation, which McIntyre and McKirdy show were the methods of Hutton. Within the framework of his theory he formulated specific conjectures (predictions), such as intrusive granites and the existence of buried ancient landscapes (the unconformity). He then proceeded to refute (test) each conjecture by seeking the critical field relationships. The authors illustrate this process of analysis by Hutton in a forceful manner. Before the announcement of this theory, Hutton had developed an understanding of soil formation, erosion, slowness of most geological changes (thus deep time), how to read relative ages from rocks, intrusive relations of basalt around Edinburgh, and had inferred an igneous origin for the graphic granite of Portsoy. After publishing a sketch of the theory in 1785, Hutton sought and within three years found the field evidence that tested his predictions by verification of intrusive granite dikes, the angular unconformity, and fossils in presumed Primitive or Primary schistus. Thus was the theory supported from "observations made on purpose to elucidate the subject."

In the largest sense, Hutton should also be remembered for resolving an eighteenth-century paradox by conceiving a unified, dynamic theory of cyclic earthly renovation. He thus explained how a hospitable habitat for life could be indefinitely sustained in the face of obvious decay of landscapes. The *Theory of the Earth* secured Hutton a just claim to be, if not THE founder, then at least pre-eminent among a few founders of modern geology. McIntyre's and McKirdy's claim for Hutton as anticipator also of Darwin's evolution by natural selection seems less secure, but there can be no doubt of importance in laying important groundwork even for that later revolution in human thought.

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READING THE SKIES: A CULTURAL HISTORY OF ENGLISH WEATHER, 1650–1820. *Vladimir Jankovic. 2000. University of Chicago Press. 272 p. Hardcover, $55.00; Softcover, $20.00*

From the time of Aristotle until the early nineteenth century, the term "meteoritics" covered any and all spectacular phenomena in the sky, such as shooting stars, hailstorms, comets, cloud formations, and eclipses. And so "meteorology" came to mean the study of these phenomena. But although "peculiar" weather—meteorology—has always been a feature of the British climate, even in the late eighteenth century, little was known about the laws that governed it. Paradoxically, it seems, even those who kept a record of the weather—and would therefore appear to at least believe in the possibility of atmospheric laws—lamented that their records were of little help in understanding meteorology. To them, the weather was such an "irritable mixture" of airs that any attempt to understand it would prove pointless.

In *Reading the Skies,* Vladimir Jankovic attempts to understand this pessimism about achievements in meteorology, which seems so contrary to the optimism that surrounded contemporary astronomy, natural history, and chemistry. This was also a time which saw a rapid rise in the popularity of the barometer and an increasing number of people engaged in recording their observations. To find answers to this conundrum, Jankovic looks at the social, theoretical, and literary characteristics of British meteorology during the eighteenth century, by considering early weather observations as "narrative news." He explores the extent to which such reports were not just about the weather, but that they also reflected the state of national commerce, politics, and religion, since the weather could inflict bodily pain and financial loss, and perhaps even be the source of national characteristics. By stressing the qualitative and narrative framing of "important" weather in Britain, between 1650 and 1820, Jankovic questions the view that instrumental recording represented the cornerstone of modern meteorology, and instead argues for a cultural and epistemological continuity during this period.

Being English I am, of course, interested in the weather, and so there is much in this book on the history of meteorology to interest me. Being interested in history as well, I was fascinated by the accounts of weather-related phenomena, by the descriptions of the journals kept by the clergy who were paragons of the "meteoritic tradition," and, in particular, by the illustrations, one of which reconstructs the path taken by lightning through a house, and another which shows the damage "Concerning a Spout of Water that Happened at Topsham." Then, as now, tremendous interest was taken in spectacular weather. However, while this is a very scholarly work, or perhaps because of that, I found a ridge of low pressure came over me every time I picked it up to read a bit more. It is hard going. Although a short book, the type is unnecessarily small, the text is highly detailed, and I am afraid that I did not find Jankovic's account "engaging," as twice promised on the back cover.

The author explains how and why these early investigations of local weather differed so radically from the scientific analyses with which we are familiar today, and he interweaves classical traditions, folklore, and practices with the increase of quantitative approaches. It seems that things have not changed much since that time. Our interest in "spectacular" weather has remained undiminished over the centuries, and our understanding of climatic variation, now interpreted as "global warming," is still a mix of folklore, quantitative analyses, pessimism, and misunderstanding about what the data are telling us. Some comment on that would have been interesting.

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THE ESTABLISHMENT OF SCIENCE IN AMERICA: 150 YEARS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE. *Sally Gregory Kohlstedt, Michael M. Sokal, and Bruce V. Lewenstein. Foreword by Stephen Jay Gould. 1999. Rutgers University Press, New Brunswick, New Jersey. 272 p. Hardcover, $35.00*

This book had every opportunity to be a bad book, and failed miserably. It is, in fact, a wonderful book. An institutional history written to observe a significant chronological milestone, the 150th anniversary of the founding of the American Association for the Advancement of Science (AAAS), it might either have been laudatory prose or a compilation of anecdotes and undigested bits of information, free of analysis, evaluative reflection, or context. And while the well established skills of the authors of this particular volume virtually assured that the book would avoid all those pitfalls, the very fact that it was the work of multiple authors might have created an anthology rather than a unified narrative.

This volume has many virtues, not least of which is its unity. Multi-author volumes can vary tremendously in tone, quality, and focus. This volume is unified in several ways. The writing is uniformly clear and engaging. The organization is generally, but not slavishly, chronological. The approach is explicitly narrative, starting with Keith Benson and Jane Maienschein's "Introduction: AAAS Narrative History" (pp. 1–6). If you want the six-page version of the book, there it is, but like any good appetizer, it's more likely to whet your appetite than to satisfy. *The Establishment of Science in America* is, in many ways, a biography of the AAAS: its life story told in the context of the people and events that shaped its development and were impacted by its actions and decisions.

The second most striking thing about the volume is its readability. Stephen Jay Gould observes in his "Foreword" that he "had no intention of reading the book beyond a skim and a scan for some illustrative material . . . So I dunked my toes into this text, as planned, and then a funny thing happened. I read the whole book, every word of it, in one sitting, because the account was so informative, interesting, and honorably written (p. vii)." I did not manage to read the volume in a single sitting, but that was a reflection of my schedule, not the volume itself. I was always more than ready to pick it up again, and came to consider Gould's pithy "informative, interesting, and honorably written" the best one-sentence book review I'd ever read.

The book is rich in both information and anecdote, but these three skilled historians have used their raw materials wisely and well. Names, dates, facts, and figures are woven together in context so that their meaning and importance are always clear. Anecdotes are used sparingly and always to illustrate and clarify. Each of these authors surely has a file of "good stories" from the history of the AAAS that they would have loved to include, but which they omitted in the interest of making the "big picture" clear and understandable.

Kohlstedt's chapter, "Creating a Forum for Science: AAAS in the Nineteenth Century," traces the structural evolution of the AAAS from its founding in 1848 to the end of the century. (Kohlstedt's 1976 *Formation of the American Scientific Community* covers only the period to 1860.) A central theme in nineteenth-century history of the AAAS, and a challenge still very real in the twenty-first century, was the struggle to remain "democratic" or inclusive, while giving recognition, and the leadership of the institution, to "professional" scientists. Another familiar issue weaves its way through this period as the leaders of the AAAS try to find the proper relationship between "science" and "the press." The evolving relationship between the AAAS and *Science* both illustrates and is driven by this challenge.

Sokal's chapter, "Promoting Science in the New Century: The Middle Years of the AAAS," begins as Kohlstedt's ends, with the rise of disciplinary specialization and of specialized societies. The threats to the AAAS were two-fold: the obvious dangers of fragmentation in a general science society, but also a new kind of "democracy" problem. With numerous new disciplinary groups emerging, and boundaries between groups frequently shifting, the AAAS struggled to maintain broad participation within the Association and broad representation of science to external constituencies. Of the three periods of AAAS history, the central one is the most internally focused, the one in which presenting science to the "public" is least emphasized.

Lewenstein's chapter, "Shifting Science from People to Programs: AAAS in the Postwar Years," brings the story down to the late 1990s. It is largely the story of the creation and implementation of the "Arden House Statement." In 1951, the executive committee of the AAAS and a number of consultants met at the Columbia University's Hudson River retreat, Arden House, to discuss what the AAAS was, what it should be, and how to successfully traverse the intervening distance. That meeting, Lewenstein argues, "defined AAAS as we know it at the close of the twentieth century (p. 109)." Central to the Arden House Statement is an emphasis on fostering "public understanding and appreciation of the importance and promise of the methods of science in human progress." The phrase comes from the AAAS constitution, and Arden House was largely about translating that abstract statement of purpose into real action.

There are Big Themes here: the challenges of democracy in science and science in a democracy; the related problem of science in the press and getting press for science; the unity and fragmentation of "science"; the ways in which American science has been shaped by, and has tried to shape, its social and political context. All three authors also clearly illustrate the impact personality and individual leadership style can have on the life and health of an institution. I would make this volume recommended reading for every member of any scientific society in America, and required reading for anyone who holds a leadership position.

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EVOLUTIONARY CATASTROPHES: THE SCIENCE OF MASS EXTINCTIONS. *Vincent Courtillot; Joe McClinton, translator. 2002. Cambridge University Press, U. K. 173 p. Softcover, $16.00*

I was misled by the book's ambitious subtitle, "The Science of Mass Extinction." This is *not* a comprehensive review of the phenomenon of mass extinction, a synthesis of all relevant data, and a proposal of a general hypothesis to explain mass extinction. A more accurate (although unwieldy) title would be, "Meteorite impact or volcanic eruption as a cause of mass extinction at the end of the Cretaceous Period: One paleomagnetician's view." The book is in fact a highly personal synopsis of (1) the events that led to the 1980 announcement by the physicist/geologist, father/son team of Luis/Walter Alvarez that a meteorite impact had precipitated the extinctions that killed the dinosaurs; (2) the ensuing controversy; and (3) the primary alternative hypothesis to the impact extinction theory, that massive basalt flows could have accomplished the same effect. Courtillot is an expert in paleomagnetism, a geologist who measures the remanent magnetic polarity of igneous rocks, particularly basalt. "Paleomag" data can be used to determine the relative age of the rocks and aid in correlation of strata. Courtillot is no impartial observer of this controversy, as his own research concerns volcanic events at the Cretaceous-Tertiary (K-T) boundary. To his credit Courtillot is fair in his treatment of the impact scenario; he admits his prejudices, and gives a generous description of the supporting research. He is not impassive, however, and his first-person anecdotes and editorial asides infuse the narrative with quirky charm that makes the book more of a personal memoir than a scientific treatise. As one example, he laments the loss of civility among scientists from the two different (and often antagonistic) camps, and the lopsided press coverage (impact scenarios garnered far more magazine covers and feature-length films than basalt flows did) that unfortunately characterize research on the K-T boundary.

Despite his equitable treatment of the facts on both sides, there is no surprise which K-T extinction scenario Courtillot favors, although he acknowledges the inability of volcanism to explain the presence in K-T boundary sediments of shocked quartz (quartz crystals shot through with microscopic defects, a texture known to form from the force of impact, and not yet conclusively linked to terrestrial volcanism). Courtillot rationally concludes that the physical evidence points to both phenomena occurring at the K/T boundary (although the volcanism started earlier and was longer in duration than the impact) and he allows that both mechanisms must have had a hand in the extinctions. This conclusion, seemingly so reasonable to those of us with no personal stake in the matter, would not be accepted by more partisan researchers whose work is cited in this book.

After dispensing with the K-T controversy, Courtillot turns his attention to other episodes of mass extinction from the last 300 million years and finds a striking correlation with (surprise!) episodes of basaltic volcanism. Courtillot is keeping score: his final tally is volcanism, 7 (the number of mass extinctions correlated with, and thus plausibly explained by, this mechanism), impacts, 1 (the K-T extinction). This scorecard must have justified, at least in an editor's mind, the book's far-reaching title.

I am a paleontologist. I have suffered along with my disciplinary colleagues the ignominy of having a physicist (and subsequently, a host of other physicists, astronomers, geophysicists, geochemists, and even my own distant relations) expound on the definitive reason for the extinction of the dinosaurs. To his credit, Courtillot acknowledges our pain, but that does not prevent him from adding to it. His mass extinction/basalt flow correlation goes back 300 million years and includes the terminal Permian extinctions, but one wonders why he stopped there. Significant extinctions earlier in the history of the Earth, including stromatolites and the Ediacaran metazoans at the end of the Proterozoic, archaeocyathids after the Middle Cambrian, trilobites repeatedly during the Cambrian, the bizarre Burgess Shale and Chenjiang faunas of the Early and Middle Cambrian Period, and terminal Cambrian, Ordovician, and Late Devonian major faunal turnovers are not addressed. In a book with the title, "The Science of Mass Extinctions," the reader is left to wonder at the omissions; are there no basalt flows correlated with these extinctions? Or perhaps these other extinctions are not big enough? (No definition of "mass extinction" is offered.) Courtillot's scorecard, presumably tallied for the book's first edition (1995) must be revised in light of the ever-growing "impacts" literature. At least two extinctions not credited by Courtillot to impacts (end-Permian and end-Triassic) are now claimed by the other camp (updated score: volcanism 7, impacts 3). We are left wishing that Paleozoic extinctions and basalt flows were included in Courtillot's narrative.

By the end of the book Courtillot posits that the physical Earth, including mantle processes, volcanism, and climate "may be subtly linked" with the biosphere. This might be a new insight to a paleomagnetician, but it is a central tenant of evolutionary biology: organisms evolve in part by responding to their physical environment. Thus, under the influence of Earth's gravitational field, upright *Homo sapiens* developed valves in blood-vessels to prevent wrong-way blood flow; under the influence of the Earth's magnetic field some birds use biomineralized magnetite in migratory navigation, and countless other plants and animals demonstrate exquisite adaptation to their physical environment. There is in fact not much subtlety in the geosphere/biosphere link (although biologists have yet to consider a biologic link specifically with mantle convection). Yes, a big extraterrestrial impact or volcanic eruption would undoubtedly affect the biosphere, but the question of *how* the biosphere might be affected by extreme physical events is not, understandably, adequately addressed by those who measure Gausses or examine quartz with a petrographic microscope. It is the domain of the life-scientist. I beseech our brethren who are schooled in physical phenomena to subcontract hypotheses of the possible biotic effects of physical disasters to those experienced in the unique complexity of biological systems. In return, we promise not to dabble in modeling mantle convection.

Courtillot may not have delivered the over-arching paradigm for mass extinction that was promised in the title, but there are better reasons to read this book. It is a highly readable account by a reasonably objective participant from the less-glamorous (and less-publicized) side of the K-T extinction controversy. Courtillot's explanation of the techniques of paleomagnetism and how paleomag works is accessible to the novice (with the exception of one confusing bit about north and south poles). North American readers will find Courtillot's Francophile perspective broadening, but the absence of a bibliography (apparently a French trait for certain trade books) is maddening, and the narrative is interrupted by numerous explanatory (and citation-free) footnotes.

This would be a terrific book for a seminar on the method—or perhaps more vividly—the "personality" of science. Courtillot comments on ego, funding, peer review, the importance of timing in the success or failure of scientific discovery, and the vagaries of getting published in *Science* and *Nature,* among other things.

As a paleontologist, I am chagrined that our data have not yet resolved the controversy. I confess that I grimaced at Courtillot's account of the "double-blind" test based on fossil foraminifera that was supposed to resolve the nature of K-T extinctions once and for all. Despite the work of many talented people (so many data, so few conclusions!) ambiguity still clings to the controversy. This ambiguity is wonderfully represented by the artwork on the book's paperback cover, which portrays a dinosaur skull about to be enveloped in bright orange-yellow flame. Is the flame the tail of a fiery meteor, or the glow from red-hot lava?

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DARWIN'S MENTOR: JOHN STEVENS HENSLOW, 1796–1861. *S. M. Walters and E. A. Snow. 2001. Cambridge University Press, Cambridge, U.K. 338 p. Hardcover, $59.95; £40.00*

This book can be roughly divided into three sections, each corresponding to three periods of Henslow's life. The first section, chapters 1 to 4, addresses his early years, namely, his childhood and his university education. Even though he eventually would become Cambridge's Professor of Botany in 1824, these initial chapters treat several aspects of his career that are relevant to the history of the earth sciences—particularly chapter 3's investigation of his early days at Cambridge. We learn that Adam Sedgwick and E. D. Clark notably influenced Henslow, and that his first published paper (1821) appeared in the *Transactions of the Geological Society of London.* The second section, chapters 5 to 9, outlines Henslow's teaching career at Cambridge. It is principally in these chapters where the authors discuss issues relevant to Henslow's role as "Darwin's Mentor." Even though his methodological and botanical teachings are briefly explained, the strong emphasis upon Darwin often shifts the focus off Henslow and on to his soon-to-be famous student. Additionally, aside from a few paragraphs on Henslow's excursions to the Gog-Magog hills, Devil's Dyke, and Bottisham Fen, most of the geological topics focus on sources (Lyell, for example) that influenced Darwin and not Henslow. The third section, chapters 10 to 12, investigates Henslow's later years as a priest in Hitcham, Suffolk. Because his parish was not a convenient distance from Cambridge, Henslow moved away from the university and ended up devoting most of his time to his parishioners. Walters and Stow address these years by focusing on Henslow's involvement in local politics and by detailing his role in educational initiatives.

The most pressing problem with the book is that it does not offer an original analysis of Henslow's influence upon Darwin. Under normal circumstances such an omission would not necessarily be a problem for a book that is interested in addressing the career of an early nineteenth-century scientist—especially since viewing Georgian natural philosophy through Darwinian spectacles has become increasingly *passé* in recent years (as the authors themselves mention). However, since the very title of the book makes explicit reference to the relationship between Darwin and Henslow, one would expect it to offer something more enlightening than has already been addressed in the books written by Janet Brown, Jim Moore, and Peter Bowler.

Alas, this is not the case. Instead of offering a fresh perspective, the book reinforces the traditional surety that "Henslow facilitated Darwin's enquiries, and pointed him in a useful direction" (p. 167). This particularly can be seen in chapter 6, entitled "Educating Charles Darwin," where the authors pose the following rhetorical question: "Without Henslow, what would the young Darwin have made of his career?" This is answered by the following assessment: "Of course, we cannot answer this question." This sort of ambiguity is present in most of the sections that address the Henslow-Darwin relationship. The most that the reader is offered on this matter is a long list of subjective clauses that suggest possibilities. In fact, about half-way through the book the authors admit, ". . . there is little evidence that Henslow's opinion or advice was of much importance to him [Darwin], whether on matters of religious belief or scientific questions" (p. 169). Statements like this are not few, and (based on the evidence offered by the authors) they make one wonder if the word "Mentor" in the title was appropriately chosen.

In addition to avoiding the focus implied in the title, there are a few other problematic aspects of the book. For instance, its intended audience is hard to discern. The first two chapters seem to suggest the book is targeting an unsophisticated readership, that is, university undergraduates or people who are unfamiliar with the history of science. The authors continually apologize for Henslow's inability to foresee discoveries that would eventually be made in twentieth-century botany, and they often give trite explanations for Cambridge's early nineteenth-century scientific context. Further evidence for this simplified approach can be found in Chapter 9's brief treatment given to how Henslow's religion may have potentially guided his scientific endeavors. Granted, the epistemological and teleological issues that might have trickled into Darwin via Henslow are not the stuff of an introductory text. However, as the book progresses into the chapters that treat Henslow's Hitcham years, the analysis becomes quite entangled in the nuances of his life as a parish priest and proceeds far beyond the general issues that would captivate a neophyte audience.

The book's construction is another point that many readers might find rather curious. First, although it is clear that the authors have consulted a number of primary sources, parts of the book (especially those in the first half) depend heavily upon Leonard Jenyns's *Memoir of the Reverend John Stevens Henslow* (1862) and Janet Brown's *Charles Darwin, Voyaging* (1995). Second, in addition to citing these and a few other works *ad nausea,* the book is mired with so many tortuously long secondary-source quotations that it is sometimes hard to follow the stated thrust of a given chapter. This makes several of its sections look as if the ideas and quotations of other authors had been cobbled together to form one main text. Such an undigested appearance also leads the reader to wonder whether or not several of the primary source quotations have been included to support the theses of authors like Jenyns and Brown, or if they are being cited because they actually present a novel portrayal of Henslow. Third, despite the semi-chronological arrangement of the book's chapters, the authors insist upon flip-flopping between decades so that the reader is forced to frequently consult the timeline that is contained in the appendix. Relatedly, the text tends to jump from one point to another without giving the reader a clear transitional sentence. In some cases this problem could have been eliminated by the inclusion of a simple sub-heading.

Aside from the above drawbacks, the book is lavishly illustrated, and it cites some delightfully obscure sources—the Hitcham vestry book, for example. Despite the book's title, the most original bits occur in the areas where Darwin is not even under discussion. A foreshadowing of this occurs in chapter 7's brief account (five pages!) of Henslow's involvement in Cambridge politics. It details his committee responsibilities and explains why his enemies labeled him a "Common Informer." This is followed by chapter 8's informative treatment of the university's decision to move the botanic garden during the 1830s and 1840s. The book's final chapters (10 to 12) about Henslow's time in Hitcham competently address previously obscured aspects of his career as a parish priest. The authors show how Henslow's post-Cambridge years saw him combine his views on science, theology, and social education to produce such interesting pedagogical artifacts as botanical diagrams and bioscopes—the latter being ". . . [a] 'Life-Dial' on which one's progress through life could be recorded at each birthday." (p.

220). Henslow's contributions to local naturalism are also treated, especially his involvement in planning horticultural shows, collecting botanical and mineralogical specimens, teaching in the parish school, founding the Ipswich Museum, and taking his parishioners on trips out into the country. The authors suggest that it was the latter capacity that allowed Henslow to most freely express his continuing interest in both botany and geology. Darwin is briefly mentioned in these chapters, but only as a correspondent who viewed Henslow more as a friend than as an academic mentor.

As a whole, this book inhabits that uncomfortable middle ground which exists between introductory texts and monographs. Its treatment of the Darwin-Henslow relationship is often derivative, and its chapters on Henslow's Hitcham years need to be teased out into a more comprehensive analysis. This situation is a rather disappointing one because an in-depth biography of Henslow's life is sorely needed. In this sense, *Darwin's Mentor* is a good case of a missed opportunity. As the book currently stands, the authors would have done well to eliminate their rehashing of the Darwin-Henslow relationship and to expand the sections that treat Henslow's publications, public interests and parish responsibilities. Had they focused on these seemingly non-Darwinian aspects of Henslow's career, they might have produced a more original picture of Henslow's life and, consequently, his role as "Darwin's Mentor."

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GOLD IN HISTORY, GEOLOGY, AND CULTURE: COLLECTED ESSAYS. *Richard F. Knapp and Robert M. Topkins, eds. 2001. Division of Archives and History, North Carolina Department of Cultural Resources, Raleigh, xxi + 379 p. Soft-cover, $20.00*

This collection of twenty-two essays is the direct result of Hurricane Fran that struck the eastern portions of North Carolina on September 16, 1999. The symposium, "Gold in Carolina and America: A Bicentennial Perspective," had been scheduled at the University of North Carolina at Charlotte from September 16 to 18. However, it had to be cancelled because of the threat of Hurricane Fran. The subsequent flooding and collateral damage in eastern and central North Carolina precluded rescheduling this symposium. Only through the hard work of many interested individuals and groups was funding obtained so that this volume could be compiled and published.

This work can be split into three topic areas: gold and its lure around the world from prehistory to today; the discovery, exploration, and exploitation of gold in North Carolina from 1799 to the present; and post-1848 gold mining in the American West and the Klondike.

The first four essays and the last essay of this volume help set the stage in evaluating the lure of gold and the reasons for its rarity. Geoffrey Feiss's essay (pp. 33–42) examines gold as to its chemical nature, presence in the Earth, and the tectonic regimes in which it is found, while Byron Berger (pp. 43–62) continues this study with an examination of the world-wide distribution of gold formations. I. S. Parrish (pp. 63–82) goes into detail about Pre-Columbian gold exploration, and R. L. Smith (pp. 1–32) looks at the later historical gold rushes throughout the New World, Africa, Australia, and Asia. Lastly, the closing essay by William Bischoff (pp. 347–368) details how art and gold are intertwined from ornaments to coins to manuscripts.

The next ten essays address the discovery of gold in America and, in particular, North Carolina. Peter Blakewell (pp. 83–94) gives an overview of the search for gold in America prior to 1799. Richard Knapp (pp. 95–112) and Brent Glass (pp. 113–130) look at the evolution of gold mining in North Carolina from 1799 to 1915. However, the history of gold mining in North Carolina is also a history of immigration and cultural changes. Elizabeth Hines (pp. 131–146) considers the Cornish miner who brought the skills and muscle to develop deep shaft mining in the Piedmont. Robert Moye (pp. 187–206) and Jeffrey Forret (pp. 207–222) investigate how slavery and the use of slaves affected the mining of gold in this state. What the miners did with the gold once they wrestled it from the ground is the focus of Rodney Barfield (pp. 147–162) and Richard Doty (pp. 163–178) essays on the early private minting of gold by the Bechtler family and the development of the federal mint in Charlotte. Charlotte, the city, played an important part in the gold mining history of this region, which is examined by David Goldfield (pp. 179–186). Lastly, Dennis LaPoint (pp. 223–240) reviews post-1915 gold mining in North Carolina and discusses the potential for future exploration and mining.

The remainder of the volume covers a wide range of gold mining regions throughout the American West and Alaska. Malcolm Rohrbough (pp. 241–256) starts with an overview of the 1849 California gold rush, which slowed but did not stop gold mining in the southeastern United States. Ed Hunter (pp. 257–266) and David Vardiman (pp. 267–280) examine Colorado's gold rush at Cripple Creek. J. Alan Coope (pp. 281–302) gives an overview of Nevada's gold mining history and the discovery of the Carlin Deposit, while Ken Coates (pp. 319–334) and Joan Antonson (pp. 335–346) finish up the continent by examining the gold of Alaska and the Klondike gold rush. Tucked in between these historical and geological studies is the essay by Peter Maciulaitis (pp. 303–318) that examines how the California to Alaska gold rushes were financed, a worthy topic that is often overlooked.

Although written for a broad audience, this volume of essays helps to wear away at the common misconception that gold mining in America started with the 1848 discovery on the American River in California. The gold mining boom in North Carolina and throughout the southeastern United States in the early 1800s provided a testing ground for the modification and development of mining techniques and equipment. The techniques and machinery, as well as the skilled miners who learned their trade in the southeastern United States, were transported to the rich California and western United States gold fields. However, even with the richness of the western gold fields, gold mining still continued in the American South and was rejuvenated, although at a reduced level, by the refined techniques and skills brought from the West. The other reason for the importance of this period in the history of the expanding United States was that it was a preview of how the search for gold could affect the social, cultural, and political landscape of not only the specific mining region, but the nation as a whole.

Two companion volumes for this topic area are David Williams's *The Georgia Gold Rush* (Columbia: University of South Carolina Press, 1993) and Richard Knapp's and Brent Glass's *Gold Mining in North Carolina: A Bicentennial History* (Raleigh: North Carolina Division of Archives and History, 1999). Together these three volumes make a core set of readings for the gold mining history of southeastern United States. Buy all three for your library (or convince your library representative that they should be purchased).

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NATURE'S MUSEUMS: VICTORIAN SCIENCE AND THE ARCHITECTURE OF DISPLAY. *Carla Yanni. 1999. The Johns Hopkins University Press, Baltimore, Maryland. 199 p. Hardcover, $54.95*

This most interesting book concerns Victorian buildings and museum collections housed in them. The various reviewers say, "... the Victorians are celebrated for their science and their spectacular public buildings. *Nature's Museums* unites these concerns in an exciting and original analysis. It is a major contribution to our understanding of the history of public architecture, scientific practice, and the cultural life of the Victorian era" (James Secord). "Piled high with bones and stuffed animals, natural history museums were the primary places of interaction between natural science and its diverse publics. Studies of the natural world (what we now think of as biology and geology) were changing and conflicted disciplines, and thus no single vision of nature emerged in the Victorian period. *Nature's Museums* analyzes how the architecture of selected natural history museums in Britain contributed to the legitimization of knowledge" (Author Carla Yanni). "Focusing on the Oxford University Museum, the Edinburgh Museum of Science and Art, and the Natural History Museum of London, [Author Carla] Yanni explores how such institutions reflected varying, often contradictory concepts of nature—from the handiwork of God to a resource to be exploited. She explains how the rise of museums accompanied and influenced the transformation of science from a 'gentleman's hobby' to a paying profession." Please note the emphasis on the word "gentleman" in the last sentence.

This book reminds me of my own history. In 1938 I served as a farm laborer in England. That designation made me a member of the working class, and hence *I was no gentleman.* My boss was a gentleman farmer, and, as this title implies, *he was a gentleman.* Today I am a professor, and, by Victorian definition and standards, I am still no gentleman. Why? A gentleman in Victorian times had a private income from his farm operation, whereas a modern professor receives a salary, which excludes him from being a gentleman. Projecting back one hundred years, and reflecting the time of Victorian museums, which are the objects of this book, the class of gentlemen consisted of two kinds of people, (1) gentleman farmers and (2) vicars of the Church of England. I started my career at the University of Cambridge, England, and the admission requirements consisted of passing examinations in Latin, Greek, and Hebrew, because a "gentleman" must be familiar with the bible written in Hebrew (Old Testament), Greek (New Testament), and the language of the learned gentry was Latin. As Adrian Desmond is quoted in this book, "Oxford and Cambridge were finishing schools for prosperous Anglicans." Note that Simon Winchester's new book (2001) *The Map That Changed the World: William Smith and the Birth of Modern Geology* shows that William Smith (1776–1842) was no gentleman. He received pay for doing geology. If you were not a gentleman, it was easy in England to end up in prison, like William Smith, or, in the United States, like Amos Eaton (1769–1839), who likewise was no gentleman, and spent about eight years in prison before he as released and became the father of American geology.

For the most part, this book concerns early nineteenth-century museums, their architecture and displays, and the gentlemen responsible for their exhibits. Please note there are no ladies and, to be honest, few Victorian-defined gentlemen. After all, gentlemen farmers were too busy, and clergy felt needs elsewhere. However there is an exception: the "Museum of Creation," described in the "Epilogue" of this book:

Its exhibitions are arranged by each of the days of Creation, followed by galleries with information on Noah's Ark, the Biblical Flood, and the Ice Age. The Ice Age gallery is small, blue, and purposely refrigerated. After the Ice Age, visitors encounter a room on the civilization of Babel, which includes wall-cases on dinosaurs and Neanderthals. The Neanderthal bones are not presented as proof of evolution. The Neanderthal skull is explained in the following way: some people have pronounced brows, old people have protruding lower jaws because their teeth are missing, and Eskimos have flat noses; therefore, since folks in the Bible are described as living to hundreds of years, the Neanderthal skulls do not represent a separate species at all but just the remains of really old Eskimos. The next three rooms, after Babel and the old Eskimos, focus on written history: the Greeks are signaled out for special derision (they had all those extra gods); a pause in the display celebrates the life of Christ, and there are wall displays on the Dark Ages and the Renaissance. The next sequence, like the entire museum, is carefully choreographed. Visitors are funneled through a narrow room representing the nineteenth century, with creationist worthies on the right, and evolutionist un-worthies on the left. Charles Darwin is the key villain on the evolutionist wall. Darwin's theory of evolution is presented as the precursor for many twentieth-century evils, including Nazism. In the next room, after the comparison of creationists and evolutionists, we face a key concept stated in a wall label above a photograph of Hitler. It reads: "Racism: The Fruit of Evolution." It is the secondary title of Darwin's magnum opus which is emphasized, not *On the Origin of Species by Nature Selection,* but *The Preservation of Favored Races in the Struggle for Life.* Since Darwin assumed that animals struggled continuously, and that evolution occurred concurrently, by extension, humans also evolved. Therefore, Darwin must have believed that races within the human species were vying for superiority. The logic goes further: Creationists teach that all human beings are equal—the Bible does not mention races—while evolutionists teach racism, and therefore evolutionary theory ultimately brought about the Holocaust.

Typical chapter headings are "displays of natural knowledge in the 1830s and 1840s" which includes many Victorian museums. An interesting or maybe embarrassing chapter heading is "Nature in Conflict" which the architect Robert Kerr (1864) expresses as follows: "A vast Bazaar, like the Crystal Palace or the International Exhibition Building, however suitable for other purposes, is not adapted for those of a Natural History Collection: specimens lose scale and importance, the casual visitor is bewildered, the student is interrupted, and the display sinks from the character of science to that of show."

Yanni states, ". . . the architectural history of the Natural History Museum in London culminates many of the themes of this book: scientists and architects debated the scope of the displays, the suitability of exhibition-style architecture, and the appropriateness of ecclesiastical imagery for a museum."

I have visited several of the museums in England, especially those in London, Cambridge, and Oxford, and enjoyed the displays. My interest in architecture is minimal—the samples on display are my delight. I enjoyed the book, but, as a native German and survivor of the 1930s, I felt shocked for Yanni to resurrect Nazis, Hitler, and the Holocaust. Words can adequately explain the contents of the book without bringing in horror.

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TIDES: A SCIENTIFIC HISTORY. *David Edgar Cartwright. 1999. Cambridge University Press. Cambridge. 292 p. Hardcover, $69.95, Softcover, $30.00*

The study of tides began at ports where tides have significant range, and on rivers where tidal bores provide drama. Records from this study go back to the eleventh and twelfth centuries in China and around the British Isles. For the last four hundred years there have been two tracks on the path of tidal studies: a theoretical track and an empirical track. Newton provided the theory to explain tides in an idealized ocean acted on by the Moon and the Sun. Laplace refined that theory. But Newton and Laplace did not eliminate the requirement to measure tides at each port, which measurements, up until recently, have been the motivation for developments described in Cartwright's interesting history. Tide tables for the world's ports derive from site-specific measurements. Had Newton and Enlightenment France never arrived, predicted times and heights of the tide would still be needed today to enter a port.

This book, *Tides: A Scientific History,* is an internal history of the science of tides, as it developed from Newton's time to the present, by a scientist who has a place in that history. At the beginning of the book (pp. 2 and 3), Cartwright outlines the main points of this history in the following paragraphs, which summarize it better than paraphrase by a reviewer could:

> The tides have been an 'old subject' for a long time.
>
> From time to time a new idea has arisen to cast fresh light on the subject. While such events have spurred some to follow up the new ideas and their implications, they have also had a negative effect by appearing superficially to solve all the outstanding problems. Newton's gravitational theory of tides (Chapter 5) explained so many previously misunderstood phenomena that British scientists in the 18th century saw little point in pursuing the subject further. The initiative passed to the French Academie Royale, culminating in the work of Laplace (Chapter 7) who took over where Newton stopped, at the dynamic response of the ocean to Newton's correctly defined force field. Similarly, William Thomson's idea of harmonic analysis, which stemmed from Laplaces's theory, was so successful (after development by George Darwin) in providing for accurate predictions at any site where the tide had been measured for a long enough period of time, that one of the mainstays of research, namely from the commercial and naval producers of tide-tables, was transferred to routine computing activity. It was left to altruistic bodies like the British Association for the Advancement of Science (who had in fact promoted the development of the 'harmonic method' of prediction) to encourage further research into the *spatial* and *global* properties of tides and their currents.

When the new subject of *geophysics* began to develop towards the end of the nineteenth century, its investigators soon found that many of its problems involved the large-scale properties of the tides of ocean, atmosphere, and the elastic Earth. Increasingly, these problems were seen to be global in character and solutions to them few or lacking. Of central and lasting interest was the total rate of dissipation of energy by the oceanic tides, and its implications for the apparent acceleration of the moon's longitude and (later) the rate of increase in the length of the day. Progress was now made, not by the tide-table experts, the naval hydrographers and academic mathematicians, who had hitherto kept tidal lore to themselves, but by geophysicists and by certain oceanographers inclined to mathematical physics.

The central problem of the twentieth century, essential to the understanding of global energy dissipation as well as to a host of other geophysical problems, has remained that of determining the behavior of tides in the deep ocean. This is essentially the same problem as had bothered Whewell in the previous century, but at an altogether more refined level of precision than Whewell ever imagined. Persistently this problem defied formal mathematical analysis and measurement technique, and final or nearly final solutions have had to await modern technology.

Research on oceanic tides in the modern sense, then, has spanned at least four centuries. It has involved scientists from disciplines ranging from astronomy and satellite geodesy to ocean instrument technology, and activity from mathematical analysis and computing to sea-going expeditions. Relatively few people have been involved at any one time, but the subject seems to have had a peculiar fascination for 'lone workers'. As one worker has 'shot his bolt' or retired, another has taken up the challenge from different viewpoint or discipline. Schools of expertise in different countries have led certain aspects of the field at different times, chiefly in Britain, France, Germany, Russia, and USA.

The book has fifteen chapters, six brief technical Appendixes (including two glossaries), and two brief indexes. Each chapter ends with a list of 'Notes and References'. Theory and measurement are usually covered in separate chapters. Because turning points in theory and in practice rarely coincide, and because the topic of each chapter has different roots in the past, chapters on the two tracks cover overlapping time intervals. The science of tides has depended on technology from earliest days to the present, and this book makes clear how the needs of theory and the possibilities of contemporary technology combined to advance the subject. By the first half of the twentieth century, available tide data were more accurate than the soundings under the keels of ships entering port. Non-tidal fluctuations in water level due to weather removed further benefit from refinement in tidal predictions. Objectives of tidal studies shifted as new technology appeared. Automatic computers, instrument technology, and satellite technology, each with a separate chapter describing events in the last half of the twentieth century, address problems that had not been imagined a century earlier.

There are ten portraits and fifty-seven figures. Engravings of Newton and Laplace constitute the frontispiece, as befits their primacy in the subject. Two sheets (pp. 128, 153) of portrait photos, four to a sheet, present William Whewell (1794–1866), William Thomson (1824–1907), George H. Darwin (1845–1912), Rollin A. Harris (1863–1918), Albert Defant (1884–1974), Joseph Proudman (1888–1975), Harald Sverdrup (1888–1957), and Arthur Doodson (1890–1968). These eight include five who were British, plus an American, an Austrian, and a Norwegian.

The fifty-seven figures include at least thirty-five that reproduce text or illustrations from important documents dating between 1546 and 1995. Of the first seven in this class, the second is in Latin (*Principia*) and the other six are in French, graphically illustrating the author's point, in the quotation above, that the British abandoned the subject to the French during the eighteenth century because they saw little opportunity to improve on Newton's contribution.

Even the new illustrations have an historical pedigree: for example, Figure 5.1 is Cartwright's clarification of Proudman's explanation of Newton's diagrams in Book I, Proposition 66. Discussions of modern developments use illustrations from the history of the subject: Chapter 13, 'The Impact of Instrument Technology, 1960–1990', has five figures, including a two-page reproduction of a 1665 paper by Sir Robert Moray on the extraordinary tides in the West Isles of Scotland, photographs of internal waves by Nils Zeilon published in 1912, and the author's own photograph of Munk and Snodgrass recovering a tide capsule at sea in 1967.

All is not smooth sailing for the illustrations, however. Despite their appropriateness and abundance, there are no lists of illustrations or of portraits, and the sources of the reproductions are often difficult to date from the figure itself or its caption. Notes and nearby text associated with a figure may ambiguously identify or date its source (check, for example, Figures 5.4, 6.1, 8.2, and others).

Tides are a peculiarly British subject, providing the British scientist-historian with a bibliographic advantage. More papers on tides have appeared in the *Philosophical Transactions* of the Royal Society than in any other scientific journal in the world, and the Royal Society is the source of most photos of original documents in the book.

The study of tides over most of its history has been driven by economics of sea-borne trade, but the physical geography of Britain—its coast long compared to its mean radius, in waters with high tidal range—suggest a basis in physical geography as well. As the length of the coast or the range of the tides increases, so too would the importance of tidal problems in the country's science. As the area of the country increased, the relative importance of coastal affairs would decrease. This line of reasoning suggests that Chile might be a future significant contributor to the study of tides.

The Preface implies that the book addresses "the general scientific reader who understands mathematical language," and Cartwright's book bears this out, in general. More specifically, he hopes that the book will be useful "to the modern science historian" and "to those specialists in oceanography, hydrography, geophysics, geodesy, astronomy, and navigation whose subjects involve tides." That list does not specifically include me, but Cartwright's book is among the more useful that I have studied in many years. For about four decades, I have worked professionally with tide data, while having only a general idea of the underlying physics and its historical evolution. This book addresses both topics very well. It would be a useful resource to those who lecture on any subject involving the sea; to historians of science and technology interested in their mutual interaction; to students of naval and economic history; and of course to coastal engineers.

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INTERESTING PUBLICATIONS

Gerald M. Friedman, CONTRIBUTING EDITOR

Since the start of this journal, Founding Editor Gerald M. Friedman has prepared this column. Contributors wishing to list recent books and papers of interest to our membership are requested to send them to Professor Gerald M. Friedman, Brooklyn College and Graduate Center of the City University of New York ℅ Northeastern Science Foundation, Rensselaer Center of Applied Geology, P.O. Box 746, Troy, NY 12181-0746 U.S.A.; FAX: 518-273-3249; *gmfriedman@juno.com*

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