ESSAY REVIEW: A RANKEAN VIEW OF HISTORICAL GEOLOGY AND ITS DEVELOPMENT—WITH AUTHOR'S RESPONSE

Rudwick, Martin J. S. 2005. *Bursting the Limits of Time: The Reconstruction of Geohistory in the Age of Revolution*, 2005. Chicago & London: Chicago University Press. xxiv + 708 pp. ISBN 0-226-73111-1, Hardcover, US$ 45.00.

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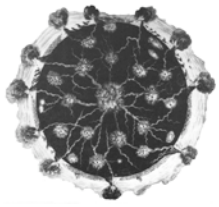
To the memory of Wilhelm von Humboldt



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ABSTRACT

Bursting the Limits of Time, subtitled *The Reconstruction of Geohistory in the Age of Revolution*, by Martin Rudwick embodies an attempt to show how historical geology emerged at the end of the eighteenth century by imitating the Judeo–Christian historiographical tradition. This tradition, Rudwick argues, attempts to reconstruct a unique history by the relicts and testimonia without recourse to general laws or models. Rudwick thinks that a similar project was initiated in geology by such men as de Luc and Cuvier, one relying on physical chronometers provided by geological processes and the other on the succession of life through the ages, using comparative anatomy of fossil animals as a basis. I think this interpretation of how historical geology emerged fundamentally flawed for two reasons: (1) no historical reconstruction is possible without recourse to preconceived models because of the incompleteness of the record; (2) because decoding the past of the Earth had already become possible by understanding the processes that governed it. That understanding had grown mainly through the seventeenth and the eighteenth centuries when rules of sedimentation and the nature of igneous bodies, making a comprehension of cross-cutting relationships possible, became clear and religion stopped being a menace to a proper appreciation of natural history. When religion lost its authority, it became permissible to assume that past events had not been necessarily different from present events, which made model building feasible. Contrary to Rudwick's assessment, de Luc contributed nothing of lasting value to this development, mainly because his avowed goal in doing geology was to vindicate the veracity of *Genesis*. By contrast, Cuvier made long distance correlation of sedimentary sequences possible by essentially inventing biostratigraphy and cleared the path for a testable, detailed global Earth history. The laudation accorded to him by Rudwick for this feat (but *not* for 'bursting the limits of time', for that had already been accomplished by others like Hutton) is fully deserved. Rudwick's attempts to argue that religion was not a threat, but, rather, a help for the development of geology in the late eighteenth century is not borne out by the evidence, which he reviews inadequately.

1. INTRODUCTION

Bursting the Limits of Time, subtitled *The Reconstruction of Geohistory in the Age of Revolution*, by the distinguished historian of geology Martin Rudwick, is a massive book

with essentially two parts.[1] The first chiefly discusses what Rudwick calls a synchronous history of the birth of what eventually became historical geology, i.e., the science of the Earth's past, in Western Europe, between the times of Horace Bénédict de Saussure's (1740–1799) epoch-making ascent of Mont Blanc (4,808 m) in 1787 and the investigation of the Kirkdale hyena cave in northern Yorkshire, England, by William Buckland (1784–1856) in the winter of 1821–1822. The second part is a diachronous history of how this emergence of historical geology actually happened.

Rudwick's massive tome is, then, about the birth of geology as an historical science, in the light of what the author considers history (both natural and human) to be about. If I understand him correctly, Rudwick makes a distinction between "philosophical or conjectural history" (of the type that Dugald Stewart [1753–1828] said Adam Smith [1723–1790] had practised (Stewart 1793 [1980], p. 293) and which Wilhelm von Humboldt [1767–1835] later recommended) and "erudite history . . . based on detailed critical study of massive documentary evidence" (pp. 181–182), made respectable by the great Prussian historian Leopold von Ranke (1795–1886). Thus, Rudwick seems to suggest that people who interpret the past on the basis of some pre-conceived model and those who attempt to reconstruct history only from the fragments of evidence that survive are engaged in two fundamentally different undertakings; and that geology began to adopt the latter practice towards the end of the eighteenth century with the emergence of biostratigraphy and the interpretation of certain physical geological phenomena as 'clocks' to attempt to establish a geochronology. This 'erudite' sort of geological practice, so Rudwick maintains, was in part learnt from the Judeo–Christian tradition. For one of the primary concerns of that tradition is the supposedly inspired narration of the unique history of our world as created and governed by God and as recorded in the Old and the New Testaments (p. 237). As for the book itself, it may appear to the reader as itself being Rankean in character, for it is undoubtedly erudite and based "on a detailed critical study of massive historical evidence". And, I think, the author would claim that he approached the archives and texts without preconceptions, and that the picture of the history of the emergence of geoscience that he paints emerged on the evidence that the texts themselves 'proclaim'. The present review, however, questions this claim, and offers a rebuttal of the idea of geology growing from the Judeo–Christian tradition *per se*, even if many of the early geologists were nurtured in that tradition. I also identify a number of errors of omission and commission that I find in his book, which significantly undermine its thesis. To be sure, previous reviewers have sung the praises of this book, but they have perhaps been overawed by its scale and detail, and its problematic features have not yet been realised. I therefore take the liberty of examining and also questioning some its major features, without trying to follow up every detail of such a massive book—which would obviously be an impossible task here.

Rudwick is undoubtedly an important figure in the historiography of geology, with numerous valuable and often cited contributions to the subject and his views therefore command close attention and respect. It is out of that respect that I endeavour to scrutinise what I think are the weak points of his book and its background of ideas, because, I think, many of the book's reviews, which emphasise its significance and excellent qualities, have, in my view, fallen short of a satisfactory critical assessment of its central thesis.

[1] There is in addition a recent large-scale sequel (Rudwick 2008), but this is not discussed here. (An earlier and much shorter version of Professor Şengör's present review appeared in *Episodes*, Vol. 31, 2008, 31: 363–366 [ed.].)

2. TWO SORTS OF HISTORY

First, let us look at the roots of the idea of the claimed two sorts of history and try to discover whether a distinction between them is possible or warranted. The distinction arose during the eighteenth century and was expressed by such writers as Dugald Stuart, mentioned above. One of the best accounts of the difference between the two kinds of historical research found its expression in Baeumler's (1926) comparison of Wilhelm von Humboldt's method of reconstructing the past and that of Leopold von Ranke. Baeumler pointed out that von Humboldt's views were most succinctly outlined in his lecture *Über die Aufgaben des Geschichtsschreibers* (On the Tasks of the Historiographer) to the Berlin Academy on 12 April, 1821.[2] Baeumler described von Humboldt's method in detail, with 'totality' as its basis. Later in Baeumler's essay, this 'totality' appeared as "knowledge of mankind in general", which knowledge served as the model in the framework of which the fragmentary record of history is interpreted. Baeumler then contrasted von Humboldt's method with that of Leopold von Ranke, for whom:

> mankind . . . [was], explicable or inexplicable: the life of the individual, of generations, of peoples, at times with God's hand over them. . . . For Ranke there [was] no ideal mankind, no totality, no plan to be recognised. There [were] only individuals, which then naturally [came] into contact. . . . There [were] only presences that stem[med] directly from God and God [was] the context, the sense, not the totality. The trust in God [was] Ranke's basis for trusting the individual (Baeumler 1926, p. clxviv).

Baeumler emphasised the difference between von Humboldt's eighteenth-century 'classicism' and von Ranke's nineteenth-century 'romanticism'.

I too see a direct descendent of the Enlightenment in von Humboldt, but he saw the need for a model for interpreting history and was aware that he could only work on models that he (or other people) could build and hope to improve by evidence-based criticism, because the record of the past is so incomplete:

> The job of the historiographer is the description of what happened. . . . But what happened is available to our senses only in part. The rest must be felt, deduced, guessed. What does appear of it is dispersed, torn up, isolated. What joins up these pieces, what illuminates the individual piece in the right light, gives the whole a shape, is hidden from direct observation. . . . That is why, nothing is so rare as an absolutely true narrative . . . (von Humboldt 1821 [1959], p. 153).

Von Ranke, by contrast, naïvely supposed that he was just describing, without any assumptions or models and said in his first book that he aimed only to show how it—the past—really had been.[3] However, in practice he, like von Humboldt, also needed a model to pass over Hume's barrier between limited experience and general inference. Ranke found it

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2 For the text, see von Humboldt 1821 [1959].

3 "*wie es eigentlich gewesen*": Ranke (1824, p. vi). Ranke (pp. v and vi) wrote this against those who considered judging the past and illuminating the future as among the tasks of history. Michael Grant (1995, p. 94) has pointed out that Ranke's statement was probably inspired by the parallel statement 'to say exactly how things happened' by the 'Syrian' satirist and novelist Lucian of Samosata (*ca* 120–180 AD). For Ranke's *naïvété*, see Geyl (1952, esp. p. 7). However, Ranke's 'sin' was worse than *naïvété*: he was aware that the historical data were necessarily incomplete, yet he believed he could achieve complete reconstructions (possibly, with the help of his 'God model': Oncken [1922, pp. 10–11]), writing: "Naturally it is regrettable that our history is only a fragment—often dark, often entirely unknown—. Despite [this], we know much; others, can be reconstructed. The whole can perhaps still be known" (Ranke to his brother Heinrich in 1827).

in religion, but without realising that it, like other models, was just a complex set of hypotheses, with the difference that, in the case of religious suppositions, they could not be tested. Modern historians have long recognised the impossibility of doing historiography according to von Ranke's recipe (e.g. Carr 1987).

There is an analogous dichotomy among geologists: there are those who think that the past must be reconstructed from the preserved evidence, without any *à priori* model in mind; and those who think that this ideal is unrealistic because of the fragmentary nature of the evidence, in consequence of which the past must be recreated in the light of pre-conceived models, which are then tested and modified according to evidence. I have discussed these two schools of thinking in detail elsewhere,[1] although it may not be out of place to cite here Eugen Wegmann's (1896–1982) rebuke of the claims of the first group, in order to underline the intensity of the disagreement:

> Again and again one hears geologists and petrographers claim in a convinced tone, when they talk about themselves, that they only would report "what they have really observed, only that what Nature, the great teacher, taught them without an intermediary". The undertone generally implies that this should be taken as a virtue. When such claims are used only to ornament a speech perhaps, one need not take them too seriously. But if they are meant to be believed, and if [their words] are uttered by those whose job it is to train future generations, then one should perhaps be permitted to consider and clarify the question from a different angle (Wegmann 1967, p. 1).

Wegmann then proceeded to demolish the claim that geology can be done without recourse to *à priori* models.

What I understand from Rudwick's book is that he thinks that geology turned in the late eighteenth century from basing its conclusions about the Earth's past on pre-conceived models (analogous to the 'conjectures' of "philosophical or conjectural history") to reconstructions based on data collected from rocks and fossils (analogous, I take it, to "massive documentary evidence") without an intermediary of pre-conceived models, He implies that handling the Earth's past on the basis of pre-conceived models is no way to do historical geology (i.e., attempting to learn about our planet's past). In a way, he seems to think about historical geology as von Ranke did about history, and in opposition to Wegmann. That is, Rudwick seemingly believes that geology can tell, on the basis of the evidence that it collects, 'how it really was' in the past.

But I dispute two things: first, as a practising geologist engaged in the study of historical geology, I do not think that our science works as Rudwick suggests. Second, as an historian of geology, I see no reason to believe that it has done so at any time in its history, contrary to what Rudwick apparently supposes. No history, natural or human, can ever work without conjectures, because of the incompleteness of the documentation. In human history, even when the historian is trying to put himself into the mind of its subjects as Collingwood recommended (1946, p. 228)[2] he is in fact conjecturing, i.e., building a model that he hopes to make consistent with whatever evidence he or she can find. Those who think they work without conjectures, yet accumulating evidence leading them to the truth, are simply unaware that they too have conjectures in their minds in order to make sense of the evidence. But their conviction makes them unconscious of their unrecognised

[1] See Şengör (1982a, 1982b, 1991, 1992, 1993, 1996, 2001, 2005).

[2] Collingwood was wrong, however, in thinking that the historian cannot penetrate the thoughts of its subjects using a scientific method. He in fact contradicts himself only three pages later when he writes that psychology and physiology study human thought processes.

conjectures and, because of that, they cannot criticise them. This has often led to dogmatism in geology.

So we really do have two kinds of historians: those able to criticise their conjectures and those who cannot, because they are unaware that they are making them or leave them unstated. They assume that 'facts' carry their own explicit 'messages' and require no interpretation or evaluation. The main point of Rudwick's book is that it is the second kind of naturalists/savants who created historical geology; and this is the point I contest most strongly.

3. THE SEPARATE SCIENCES OF THE EARTH ACCORDING TO RUDWICK

Parallel with what appears to be his Rankean view of history[3] is Rudwick's four-fold classification of the Earth sciences in the time period his book covers (pp. 59–115): (1) mineralogy, the science of the description and classification of the constituent materials of the Earth including, in much of the time interval under consideration, also what we today comprehend under fossils; (2) physical geography, the science of the description and classification of the larger surface features of the globe; (3) geognosy, the science of the description and classification of the architectural elements of the crust as far as could be established from surface and subterranean (in mine galleries) outcrops;[4] and (4) what Rudwick calls 'Earth physics'—or, at a higher level of generality pertaining to the planet as a whole, 'geotheory' (p. 136)—which aimed to find a comprehensive theory to explain the observations gathered by the three other Earth sciences.[5] Rudwick repeats his interpretation with emphasis, when he presents the range of each of the three allegedly purely descriptive sciences, that they were in no way concerned with the explanation of what they described. But I find this difficult to bring into agreement with what I know of the writings and reputations of the people who supposedly confined their work to the 'descriptive' disciplines that Rudwick defines (1, 2, 3 above). Indeed, as Rudwick's exposition progresses, he too introduces his readers to the geotheories and/or more limited 'Earth physics' theories put forward by the practitioners of his supposedly purely descriptive sciences. In fact, none of these practitioners seem to have respected the sharp distinctions that Rudwick proposes, so the reader may be left wondering what the purpose of the insistence on these strict categories, which are said to be proposed "in order to clarify the analysis" (p. 229), may be. They seem more to confuse than to clarify.

However, by this time, the reader may be surprised by what seems the unaccountable behaviour of some of the 'heroes' of the narrative, allegedly taking care to publish their descriptions in one place, but their interpretations in another. Rudwick gives

[3] But Rudwick says that 'Rankean' ideas appeared in studies of the Earth in the eighteenth century, well before the time of von Ranke himself.

[4] In making geognosy a purely descriptive science (which some of its practitioners such as Werner advertised it to be, contrary to their actual practice) Rudwick follows Ellenberger (1993, p. 323). One might think that such a view could be justified by Alexander von Humboldt's statement in his *Essai géognostique sur le gisement des roches dans les deux Hémisphères* (1823a), where, on p. 135, he said "When the geognost has examined the relations of position and composition, he has fulfilled his task" (von Humboldt 1823b, p. 173). However, like his teacher Werner, von Humboldt also discussed, in his *geognostic* book, both oryctognostic (i.e., mineralogical) and geohistorical questions. As any reader of that work knows, position and composition *had temporal connotations* in a Wernerian world and it took a long time to shed that bias.

[5] Rudwick's claim (pp. 137–138) that geotheory also had to be related to human nature and human society, and seemingly even to morals, is difficult to generalize to the genre: there is not a trace of such relations in the works of Johann Gottlob Lehmann (1719–1767) or in Buffon (1707–1788) or in Peter Simon Pallas (1741–1811), for example.

one such example in the case of Peter Simon Pallas (1741–1811),[6] but omits many other examples, where such separation of descriptive and theoretical publications is not present. The hero, for example, with whom he opens his book and who is made to stand at the beginning of the period that it covers, namely Horace-Bénédict de Saussure, is presented as the supreme physical geographer of the period in question. Yet Rudwick tells us—albeit not with the thoroughness and emphasis that de Saussure's interpretations might warrant in such a context (see particularly p. 136)—that the latter repeatedly toyed with ideas to explain his observations (see esp. pp. 342–343: "Comments scattered throughout his *Travels* . . ."). That is, he was *generating theories* (though according to Rudwick he belonged to the genre of 'physical geographer'). For instance we owe the first interpretation of such, then esoteric, features as the *Alpine Nagelfluh* and the *Klippen* to de Saussure (Şengör 2003). He also promised to include a general theory of the Earth in the last volume of his *Travels*: p. 343). We know how much de Saussure was eventually disappointed in this, on account of his failure to find an all-encompassing theory of the Alps.

Nicolas Desmarest, another of Rudwick's physical geographers, not only had a theory of the origin of basalt, but he also generated a geological history of the Auvergne. This is normally judged to be an *historical* accomplishment (which Rudwick acknowledges: pp. 214 ff., 564). Or again, it seems odd to suppose that while Pallas was looking at fossils, he was conscious of doing mineralogy (in Rudwick's sense); while studying conglomerates, geognosy; and when considering the mountains and the volcanoes of Asia, physical geography; and that he did not think about their origin all the time. I have little doubt that Rudwick himself does not imagine the activity of savants like Pallas to have been so compartmentalised as his historiography might suggest, but his book conveyed such a message to me. I find it difficult to imagine what it might convey to readers who are not geologists.

Rudwick' book also makes some unexpected omissions that, had he included them, would have revealed further the implausibility of his fourfold division of writers about the Earth. While talking about the recent recession of the sea in the Baltic (p. 178), for example, he doesn't mention Urban Hiärne (1641–1724), the man who first drew the scientific world's attention to that important phenomenon with careful collection of data and with an explanation in an important and much-cited, but now little-known, two-volume work.[7] Rudwick's book also makes no mention of the work of Torbern Olof Bergman (1735–1784),[8] one of the principal influences on Werner (Hedberg 1969a, 1969b), or that of the Russian polymath Mikhail Vasilievich Lomonosov (1711–1765), which covered mineralogy, physical geology, and geotheory (as characterised by Rudwick).[9]

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6 Even in Pallas' case Rudwick's claim hardly applies, as Pallas's discussion of the physical geography and geognosy of the Urals in the preface to his third volume documents. See Pallas (1776, *Vorrede* [unnumbered pages]).

7 See esp. pp. 282–291 in Hiärne's second volume (Hiärne 1706) for his geohistorical discussions.

8 For Bergman's most important work on geology, see Bergman (1780). Its title should not mislead the reader into thinking that it is only a 'physical geography' book. It has much mineralogy (in Rudwick's sense, i.e., including fossils: Volume 1, Chapter 6, pp. 230–264), geognosy (e.g. Volume 1, Chapter 5, pp. 174–229) and geohistory (e.g., Volume 2, Chapters 3 and 4, pp. 211–326) in Rudwick's sense. Bergman's ideas stand on the double pillars of von Linné's 'Neptunist' Earth theory—see Linné (1744), Nathorst (1908, 1909), Sjögren (1909), von Engelhardt (1980), Frängsmyr (1994), and Şengör (2003b)—and the Swedish mining tradition, on which see, for example, Tunberg (1939). Bergman was the chief founder of the 'humid' method for the chemical analysis of minerals, by fusion in alkali, and successive precipitations by the addition of suitable reagents. For the total scientific output of Bergman, see Möström (1957). The scientific correspondence of Bergman began to be published in 1965 (Carlid and Nordström 1965), but, as far as I can ascertain, the project did not progress beyond Volume 1.

9 It is extraordinary how Lomonosov has been largely ignored by historians of geology outside the Russian sphere of influence, despite the fact that his writings have long been available in translation in Western

It is hard indeed to see the reason for the omission of Bergman, for he was a figure of critical importance in the development of the Neptunist version of historical geology (as I would call it). In fact, without knowledge of the Swedish mining traditions and von Linné's school of natural history it is difficult to comprehend the history of Neptunism in Europe in the second half of the eighteenth century. Lomonosov might perhaps have been omitted. He died before de Saussure climbed Mont Blanc (but that has not stopped Rudwick from mentioning Lehmann, for example), but the more likely reason for the omission is that Rudwick apparently considers the Russian Empire to have been a "distant outlier" (p. 289; but see p. 305, for a concession that Russia did belong in the European 'Republic of Letters'). Yet it was the only country that had anything resembling a geological survey even at the beginning of the eighteenth century[10] and also had a number of distinguished 'Earth science savants' (including Lehmann!) in the period of Rudwick's book (see Tikhomirov 1969, p. 369).

So it remains unclear why Rudwick makes such a 'big deal' of the distinctions among his four Earth sciences, which in reality never stood in isolation and always existed in different combinations in the practices of particular individuals. Could Rudwick's emphasis on the compartmentalisation be because of his emphasis in his book that it is the *accumulation of data* that eventually allowed individuals to attempt to create an Earth history, because that history consisted of a concatenation of contingent events that cannot be modelled as can, for example, a physical phenomenon such as gravitational attraction?

It may be in this light, perhaps, that we understand better why we learn nothing from Rudwick's book of the theoretical work concerning the structure of the planet by such people as Pierre Bouguer (1698–1758), one of the 'fathers' of gravity measurements, Henry Cavendish (1731–1810), the man who first determined the Earth's density, Leonhard Euler (1707–1783), who first estimated the size of a terrestrial core on the basis of the moment of inertia of the planet, or Pierre-Simon Marquis de Laplace (1749–1827), who pointed out the increase of pressure with depth. They, and others like them, were the people who did the outstanding pioneering work on "mathematical analysis and causal explanation" that Rudwick mentions on p. 289 (see Deparis and Legros [2002]). I really do not understand why they are not mentioned among the Earth physicists, although Rudwick clearly needed to know how much was known about the Earth's interior (p. 349) to be able to judge the various geotheories in existence during the time-span of his book. Some of the great mathematicians of the late eighteenth century in Western Europe, who made notable contributions to what might today be termed geophysics, were friends of the geological savants, such as Cuvier, who was a friend of Laplace, to whom Cuvier was to dedicate his *Recherches sur les ossemens fossiles* in 1812. Such friendships and mutual esteem at times appear to have influenced the choice of geotheories by the geological savants, as exemplified by Cuvier's leaning towards a 'cometary by-pass' explanation of his purported catastrophes (Coleman 1964, p. 135; Rupke 1983a, p. 77 and Figure 7; Rupke 1983b, p.

countries (but see Dott [1969]). For a German translation of Lomonosov's major work on geology, *On the Layers of the Earth* (1742, published posthumously in 1763), see Lomonossow (1961, pp. 441–549). We know that the book, *Pervii Osnovaniya Metallurgii* (*First Fundamentals of Metallurgy*), of which the *Layers* text was a long appendix, became popular not only in Russia, but also among Western intellectuals, with Diderot being among those who purchased a copy (see Pavlova and Fedorov [1984], p. 228). Lomonosov's standard biography, available in the West, has long been Morosow (1954). A shorter English biography is also available (Menshutkin 1952). For his geological studies, see particularly Tikhomirov (1969) and Rowland (2002).

[10] For a summary of the history of the *Geologicheskaya Sluzhba Rosii* (Russian Geological Survey) see Orlov (1995). Also see the excellent history of geological mapping in the Russian Empire in the last three centuries: Burde *et al*. (2000).

40).[11] The geophysical deductions in the late eighteenth century had also already shown that some of the 'geohistorical ideas' that were being bandied about (such as de Luc's collapses of the crust into vast internal cavities) were simply nonsense, according to the ideas of the 'Earth physicists' of the day. It would have been interesting to review some of the opinions regarding such conflicts at that time and it is a pity that Rudwick chose to omit them from his narrative, especially in view of the later influence of the crustal collapse idea. These had their roots in Descartes' *Principia* (1644) and Johann Jacob Scheuchzer's (1716, p. 115) Earth and mountain models, which were greatly improved by Georg Christian Füchsel (1722–1773) (Füchsel 1773). Füchsel eliminated the cavities implicit in the earlier models and replaced them by volumes of lower density. Such ideas influenced future researchers like Constant Prévost (1787–1856) and Eduard Suess (1831–1914).

One of the main points of Rudwick's book is his repeated claim that the temporal evolution of individual regions that his various mineralogists (including what we would today call palaeontologists) physical geographers, geognosts, or geotheorists reconstructed on the basis of local data *cannot or should not be considered geohistory*. This is presumably because, Rudwick seems to think, these people did not 'reconstruct', but 'invented', histories. One can understand that a 'historiography' that is purely a narrative of 'what ought to have been'—i.e., a deduction from first principles without reference to the specific actors in what actually happened and their specific relations to the contemporary condition(s) of 'ought,' i.e., to the first principles used—cannot be regarded as history (it would be physics). But Desmarest's reconstructions in his physical geographical work, for example, were based on *local* observations of thoroughly *contingent* events (though interpeted in the framework of the then available Earth-behaviour models).

Moreover, what Desmarest was doing was hardly a novelty, contrary to what Rudwick seems to believe (p. 215). Lehmann gave both a local and a universal history of the Earth on the basis of his own observations of strata and fossils (Lehmann 1756).[12] Füchsel, too, emphatically said that he was going to use his local observations in Thuringia in a *historical way* so as to establish the *history of the land and the sea*![13] A century earlier, Nicolaus Steno's (1638–1686) narrative of six episodes in the history of Etruria (present Tuscany, i.e. a local place) had been based on *local observations*, interpreted in terms of an overarching model (Stenonis 1669, pp. 67–76 and the six lowermost figures on this work's folded plate) (in this case the Genesis myth), as is any historical geological interpretation—from Steno to our own days, including those of Cuvier.[14] Lehmann's history of the Harz Mountains was as much a geohistory as was Steno's; and so was Füchsel's. Füchsel in particular, emphasised that the documents with which human history is studied are not conducive to study the history of the Earth, because they do not reach far enough back in time. And he then proceeded to present *first a physical geography* and *then a geognosy* (in Rudwick's sense) and, *finally, a geological history* of Europe. The explorer Pallas also presented what could be called a geohistory of the whole of Asia, using its rocks (extensive conglomerates, former lake deposits), fossils (mammoth and woolly rhinoceros finds), and physical geographical features (abandoned lake shores, the Himalayas, and the volcanic chains of southeast Asia). I cannot see why Rudwick would deny the historicity of Pallas's synthetic narrative history of what had happened in Asia. Of course, Pallas had

[11] In his *Exposition du système du monde*, Laplace had pointed out that should a comet indeed come close to the Earth, it could have created "waves that could easily top mountains; that could explain many things in natural history" (Laplace, 1835 [1984], p. 367).

[12] Also see Tikhomirov (1969, p. 369) for Lehmann's general geohistorical outlook.

[13] Füchsel (1773, p. 23) expressly wrote "*Geschichte des Landes und des Meeres*" in Thuringia.

[14] I do not accept Gould's (1987) interpretation of Steno's linear history as cyclic on the basis of how he organized the figures representing the six episodes.

neither Cuvier's comparative anatomy, nor his biostratigraphy. However, what is of interest to the historian of geology is not whether the histories proposed are now thought to be right or wrong, or which did or did not utilise appropriate methodologies: it is sufficient to know that their authors put their interpretations forward as geohistories based on what was then considered sound reasoning and geological evidence, be it local or global.

Even in Johann Friedrich Wilhelm Charpentier's (1728–1805) *Mineralogical Geography of Saxony* we find not only 'genetic' discussions (i.e. terrestrial physics in the eighteenth-century sense), but also those pertaining to the historical geology of parts of this region, as, for example, in his discussion of the relative ages and origin of the granites, gneisses and marbles in Saxony, as compared with those in such places as Peru and Italy (Charpentier 1778, pp. 391–407). In another mineralogical geography by Hans Michael Renovantz (1744–1798) (1788), about the Altay Mountains, we again see a detailed geohistorical narrative of the Russian Altay and the plains to their immediate west, made on the basis of local observations. Not only were the conditions of former seas explained, but also the existence of what we now know to be geologically recent lakes were deduced from the seas' former coast-lines and from the salt plains that are found in various stages of plant colonisation (Renovantz 1788)![15]

The foregoing examples suffice, I believe, to show that doing geohistory, both local and global, was no 'marginal genre' in the latter half of the eighteenth century, as Rudwick would have us believe (p. 291) and neither was there a neat separation of mineralogy from physical geography; or of those from geohistory and Earth physics (in the restricted sense that Rudwick uses the term) in the writings of the savants whose works I have just cited. In his 'Coda', admittedly, Rudwick does say that: "although the causal science of Earth physics was intrinsically ahistorical, in practice some of the geotheories that were built on its foundations did incorporate some degree of historicity" (p. 643). But the examples given above indicate, I think, that eighteenth-century 'Earth physics' was *not* intrinsically ahistorical and included more than "only some degree of historicity."

4. 'GEOGNOSY' *VERSUS* HISTORICAL GEOLOGY

Rudwick's point that "Geognostic practice was primarily concerned with structural order, not temporal sequence, let alone geohistorical reconstruction" (p. 97) is, then, difficult to accept, as the works of the people just cited show. A few pages earlier (p. 94) he concedes that geognostic classification was "incidentally temporal". (But it was temporal nevertheless and, as I have illustrated, not merely 'incidentally')! Heinrich Bingel's (1934) detailed analysis of Abraham Gottlob Werner's (1749–1817) geognosy (which Rudwick does not cite and which is often overlooked by Anglophones, but to which Alexander Ospovat's later work (1960, 1971) was much indebted), clearly documents its *geohistorical* character; and so also do the books published by Werner's students. Bingel's portrayal of Werner's work shows that Werner had developed a *geotheory* of his own. Otfried Wagenbreth (1967, p. 83) has pointed out more recently that although Werner was not the creator of Neptunism, he indeed perfected it. When, for example, Christian Gottlob Voigt

[15] For the evolution of salt lakes, see pp. 65–73. For an example of the narrative of the older geological history, see pp. 75–78. I do not know the extent to which Renovantz was aware of Robert de Paul de Lamanon's reconstruction of the Parisian lake only six years earlier (discussed by Rudwick, pp. 212–213). What we do know is that Renovantz was almost constantly travelling in Siberia from 1780, because he was in contact with other Siberian workers concerning his observations (see Patrin [1783], esp. p. 32, where he mentioned his conversation with Renovantz). In any case, the ancient west-Siberian lakes were well known. Pallas described them at length in his expedition narrative and Patrin (1742–1815) also mentioned them. So Renovantz had no need of inspiration from de Lamanon's work.

(1743–1819) (one of Werner's more famous students, and future opponent) was employed by Johann Wolfgang von Goethe as a geognost in Ilmenau in 1780 (1749–1832), among the written instructions he received was to establish the stratal order in the area, the height at which certain fossils (especially shark teeth) appeared, and how such heights correlated with one another. Presumably this was required with a view to establishing the supposed former heights of the depositing sea-level (von Goethe, 1970, pp. 1–2).[16] Although it is not known what Werner taught in terms of the stratal order and Earth history before 1782 (we know that although he announced a lecture series under the title *Gebirgslehre* [teaching of rock masses] in 1779, he only started giving the lectures in 1782: see Wagenbreth [1967, p. 85]), it is known that Voigt went to Weimar with at least an outline of Werner's version of the Neptunist theory, because it seems that Goethe learnt it from him (Kühn and von Engelhardt 1989, pp. 541–542; von Engelhardt 2003, p. 67) and enthusiastically wrote an essay on "epochs of rock-building" (Goethe 1970, pp. 15–17).[17]

I would also remark that Rudwick appears to downplay any temporal significance to the terms 'Primary' and 'Secondary', after having pointed out that the latter rocks were in places, even then, understood to be the erosion products of the former, and after having himself said that "relative age was indeed one of the criteria by which these two great categories of rocks were to be distinguished" (p. 91).[18] Had not Lehmann expressly explained the temporal connotations of these terms, which he had introduced in a St Petersburg Academy lecture in 1762 (Tikhomirov 1969, p. 369)? Rudwick downplays the age connotation because it "was inferred from structural position", i.e. from superposition. But, in those days, superposition was *the* main criterion by which relative ages of rock

[16] For commentary on this matter, see Kühn and von Engelhardt (1989, pp. 577–578); also von Engelhardt (2003, pp. 67–70).

[17] For commentary on this fragment, see Kühn and von Engelhardt (1989, pp. 587–588).

[18] In this discussion, Rudwick mistranslates *uranfänglich*—which is a point worth some extended consideration. The word means 'pertaining to the very first beginning', not 'apparently primitive' as he states. He is also incorrect when he implies (p. 91, note 42) that the German prefix *ur-* has connotations other than just temporal. In modern High German the prefix has, and has had, *only* a temporal or an idiomatic amplificatory connotation, hinting at the origins of things. In Middle High German, we do see such other connotations as 'out', and this meaning gave rise to the prefix *er-* in Modern High German. When used as an independent word with a slightly different vowel tone, *ûr* meant 'aurochs' (*Bos primigenius*), but it has never had the meaning of 'fundamental' or 'lowest' without the connotation of 'original' or 'earliest' that Rudwick thinks it has (Henschel and Kienast 1930). From the Grimm dictionary (Grimm and Grimm 1936) we learn that the original prefix stems from the German *us-*, which can be traced back to the Indo-European *uds-* and had connotations of indicating an outward or upward movement. As such, it came to designate in Modern High German—which Werner and all his contemporaries used—temporally older and oldest and also used to amplify existing meanings. Goethe's *Urpflanze*, for example, which Rudwick mentions as a counter-example of an allegedly non-temporal usage of the prefix, sits, as *the* ideal and *original* plant, at the head of imagined Platonic *temporal* sequences of derivation, as Goethe himself explained in the history of his botanical studies, referring to the variations in the plant world that he had observed: "[a]s they now permit themselves to be collected under one concept, it became slowly clearer and clearer to me that the view could be enlivened in a yet higher way: a desire that appeared to me then in the sensual form of a transcendental *Urpflanze* (= original plant). I examined all the forms that I came across in their transformations and finally, at the last destination of my trip, in Sicily, the original identity (*ursprüngliche Identität*) of all plant parts fully dawned on me and from then on I sought these out everywhere to recognize them again" (Goethe 1964, p. 334). In fact, the Grimm dictionary gives Goethe's usage as an example of a temporal plus amplificatory usage: " . . . the *Urpflanze* as original, simplest imaginable model". The same is indicated in Grimm to be true of the *Urphänomen* in the same source (Grimm and Grimm 1936, col. 2358). It would seem, then, that Rudwick is not a meticulous German scholar (his misunderstanding of Leopold von Buch's erratics paper I am afraid corroborates this inference: see below). He rightly criticizes those who try to write the history of geology of the times without a command of French (p. 4). But the same criticism applies to those who try to do it without a command of German and Italian.

strata could be established.[19] Did not de Luc say so in his—according to Rudwick—exemplary geohistorical *Lettres à La Métherie*? (p. 309). Why is it that superposition was no guide to history when geognosts used it, but it became so when de Luc used it? Did not Steno make it clear, and did not people such as Füchsel and Lehmann and many others up to de Luc use it successfully—so much so that it did not still need stating clearly? (p. 309). It is a criterion that lithostratigraphy (and other derivative stratigraphies such as sequence stratigraphy) still uses and we remain grateful to Steno for having enunciated it in the seventeenth century. It remains as valid a temporal criterion today as it was when first put forward for sedimentary sequences that had not been subsequently overturned or overthrust. Rudwick's apparent dismissal of it as a temporal criterion, widely used since Steno, will surely come as an unexpected surprise to most practising geologists.

5. GEOLOGICAL HISTORY *VERSUS* SACRED HISTORY

Rudwick is particularly keen to convince his readers that the historical Earth sciences emerged by imitating or emulating historians of the humanity's past and especially the chronologers and antiquaries. Indeed, *he asserts that this is the central thesis of his book* (p. 181). But Rudwick's assertion that geology, as a historical science, learnt methodology from the "radically historical Judeo–Christian religion" (p. 237) is a statement I find difficult to accept. Indeed the reverse was sometimes true, as exemplified by Scheuchzer's sumptuously illustrated *Kupfer-Bibel* or *Physica-sacra*, in which *the methods and knowledge of the natural sciences were used to interpret the Bible*. On p. 290, Rudwick does qualify his statement by saying that only *some* savants had been so inspired. But, I suggest, in such cases, the religious component in their thinking did not consist of some special *methodology*, but of *hypotheses* (Creation, the Flood, origin of man, etc.) and, in the time period that Rudwick's book covers, they proved more a hindrance than a help. One remembers, for example, how people in the Middle Ages had ideas much the same as those of de Luc, even including his emphasis on the *causes actuelles* to interpret God's works—as for instance in the writings of Giles of Rome (= Egidius Romanus) (1247?–1316) or Jean Buridan (1300–1358). De Luc (whom Rudwick seeks to represent as a significant figure in the history of geology) was thoroughly unoriginal in his theoretical views in geology, and his religiosity did not allow him to go one step farther in his fundamental interpretations than his Mediaeval predecessors, not even when he was supposedly defending 'causes still in operation'. Although he divided earth history into pre-Diluvian and post-Diluvian stages in what Rudwick (2001) has dubbed a 'binary history', and considered the second to be governed by presently acting operations of nature, his understanding of the latter was so primitive that it does not allow him to be among the harbingers of modern historical geology. He denied the erosional origin of valleys, the erosive power of the sea (especially the origin of sea cliffs by wave action, which he could have learnt by consulting Lavoisier's 1789 paper) and the existence of alluvial plains created by rivers (even Herodotus, II. 10, knew as much). At the other end of the scale, one may remember how the genius Leibniz felt compelled to hide the evolutionary ideas of Anaximander of Miletus (fl. ca 560 BC) between his lines in his posthumously published

[19] See the excellent paper by Wagenbreth (1967) on the temporal implications of the geognostic rock sequences on the example of Werner's historical geology. Ellenberger (1993, p. 323) points out (at the price of some inconsistency of his own account) that turning the structural order into a temporal succession was the great vision of the Neptunists who practiced geognosy.

Protogaea.[20] The proposition of a 'binary history' allows Rudwick to present de Luc as a progressive actualistic geologist, and sweep under the carpet his traditional yet speculative ideas about the Earth's early history and his reactionary views (as seen from the viewpoint of the progressive leaders of geology of his own time) concerning processes in the alleged post-Diluvial history.

Rudwick also downplays the clashes that 'geological' savants had to endure not only with the Church and the religious intelligentsia,[21] but also with their own religious colleagues (for clashes on these issues among savants, see, for example, p. 317) within the period that falls under his study. For instance, although he mentions Buffon's troubles with the Faculty of Theology of the Sorbonne, Rudwick does not cite the University's publications in which Buffon was censured twice.[22] Moreover, that censure was not confined to those publications. The Jansenists (including Louis Racine, for example) attacked him for deviating from the mythology of the Genesis.[23] And it was not only 'professional' theologians who assailed the great naturalist. His own religiously-minded colleagues, such as René-Antoine Ferchault de Réaumur (1683–1757) (who was also a personal enemy) did likewise (Roger 1989, pp. 255–258).

The contents of these attacks further show that Rudwick's statement that "the magnitude of the timescale, however intriguing scientifically, was simply irrelevant to the imaginative and religious impact of the Creation story" (p. 117) is at odds with the historical record. The Faculty of Theology of the Sorbonne was furious at the liberty with which Buffon had increased the age of the Earth. To his remark that he thought some ten thousand years must have passed since the separation of America from Europe, the Theological Faculty responded by saying (in the second of the publications—which Rudwick does not cite) that the chronology of the Sacred Historian opposed the view that ten thousand years had passed since the foundering of the Atlantis (Anonymous 1780, pp. 15–16). And when Buffon tried to stretch the six days of the Creation with the 'excuse' that the Lord had adjusted his revelation to the intellectual level of His audience, the Faculty pointed out that what Buffon had written to reconcile his *Époques* with the days of Moses did not make his system any more probable. It further reminded Buffon that, had the Creator wished, it would have been "easy for the Sacred Historian to express, in Hebrew, years, centuries, even thousands of centuries". But, the Faculty pointed out, Moses had specifically measured the duration of the Creation in days after the creation of the Sun, and

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[20] "Some go so far in their arbitrary suppositions as to believe that at times, when the ocean covered everything, animals now living on land had lived in the water and then, with the withdrawal of the waters they became amphibious and their offspring were estranged from their original homes. But this contradicts the Holy Writers and it is a sin to deviate from them" (Leibniz 1749, pp. 10–11). Leibniz had a habit of not publishing what he thought might be displeasing to the members of the society in which he lived and the Court where he was employed. As Bertrand Russell (1937) showed, he refrained from publishing the most original and profound parts of his philosophy for the same reason. The second edition of Russell's book is more important than the first, because of Louis Couturat's corroborative work of 1903.

[21] Did not the Jansenist Louis Racine (1692–1763), who turned religious after losing his son to the Lisbon Earthquake, exclaim against Buffon:

> "Submit, mortals: that your faith should destroy
> These worlds that to him your reason constructed,
> And blush not in abandoning Descartes and Newton for Moses"?

(quoted from Rousseau 1959, p. 377).

[22] The first one ('Lettre de MM. les Députés et Syndic de la Faculté de Théologie à M. de Buffon; réponse de Buffon; Seconde lettre de MM. les Députés et Syndic de la Faculté de Théologie à M. de Buffon') was printed in Buffon (1753, pp. v–xvii). The second, independent, publication, is cited here as Anonymous (1780).

[23] In their semi-clandestine journal *Nouvelles ecclésiastiques*, 6 and 13 February. See Piveteau (1952, p. 126); also Roger (1989, pp. 250–251).

thus there was little doubt as to what had been meant in the Genesis by the word 'day' (Anonymous 1780, pp. 7–8).

In his discussion of the problem of the age of the Earth from the viewpoint of the Church, Rudwick omits to mention that Archbishop James Ussher's (1581–1656) chronology and those of his followers continued a time-honoured Christian tradition since Theophilus of Antioch (AD 115–180) of attacking the pagan view that the Earth could be millions of years old. Rudwick seems to think that the Church was only combating eternalism. But this is untrue, as both Theophilus' and Ushher's words document.

In his three books addressed to his pagan friend Autolycus, Theophilus—one of the earliest apologists for the Christian faith—reviewed the origin of the world in some detail (Hosmann 1729; see also Gross 1895). When Theophilus came to dating the origin of the world in Book 3 of *Ad Autolycum*, Chapter 17, he wrote:

> With God's help I will now present a chronology, so that you will perceive that our teaching is neither new, nor fabulous, but older and more credible than all the poets and historians writing of dark times. Those who hold the world as uncreated, go too far. Others, however, have assured us of the opposite, but maintained that it has existed for fifteen million three thousand and seventy-five years. So writes Apollonius the Egyptian (Hosmann 1729, p. 156).

After having reviewed a number of both pagan and Christian chronologies, Theophilus found, by a remarkable feat of biblical scholarship, that 5,981 years had elapsed since the Creation up to his time (Hosmann 1729, p. 238). But Theophilus's chronology was *not* conceived as a simple calendar. It was, rather, a polemical device against the heathens to convince his friend Autolycus that the Christian chronology was better founded.

Fifteen centuries later[24] Archbishop Ussher presented his own chronology with the same express purpose:

> *Why is the order of the years of the world so carefully set down in the Scripture?*
> To convince all Heathen that either thought that the world was without a beginning, or that it began Millions of years before it did (Ussher 1645, s. 96, italics and capitals his).

Thus, Christian chronology explicitly had *not* the same purpose of simply establishing a time series of contingent events, as the pagan chronologies had had: it had a polemical agenda against the long timescales (i.e. *not* only against Aristotelian eternalism as Rudwick implies: pp. 117–118) used by some of the pagans. The Church[25] was explicitly interested in upholding its mythology as truth (literal or allegorical, depending on which sect was doing the defending) against the increasingly alarming findings of geology, as the reaction of the Theological Faculty of the Sorbonne to Buffon clearly documents. So what Rudwick says—"religious authorities were, quite properly, more concerned with theology and its practical implications than with literalism . . ." (p. 117)—is not apposite in every case. Geology was beginning to show that biblical chronology was based on legends or myths—and internally inconsistent ones at that—and that no degree of appeal to allegory

[24] For a summary of some other Christian chronologers, see Wyse Jackson (2006, Chapter 2, pp. 13–31).

[25] By 'the Church' I mean here the Christian Churches that quarrelled with geology. The Orthodox Church, for example, never had such a problem, because it was always under State control (Boris A. Natal'in, personal communication, 2007). Rudwick pays little attention to the different relations of the Catholic and the Protestant Churches with the Earth sciences between the Renaissance and the nineteenth century, as have been documented so usefully by Manfred Büttner and his colleagues in Bochum (e.g., Büttner [ed.] 1979).

would be able to save it in the long run. The Church, and religious savants such as de Luc, were doing their best to bend the biblical narrative to bring it into parallelism with the mounting adverse geological evidence—with a view to 'saving the appearances' of the Bible!

Others, less overtly 'apologetic' than de Luc, took up the defence of the short time scale. For example, in one of the most detailed historical attempts at establishing the age of the Earth in the late eighteenth century, a natural science (*Naturkunde*) teacher in Lemberg, Franz Süßmann, explicitly wrote that he was writing against those who "attempt to impose on us myriads of years", not against those who claimed an uncreated, eternal world (Süßmann 1782, p. 13).

The budding Earth sciences were thus not up against biblical literalists only. Any degree of religious involvement in geology was bound to clash with the scientific inferences sooner or later and in fact it began sooner than later. But this, too, was nothing really new. Steno had felt that he had to accommodate his inferences within the limits of the Judeo–Christian mythology in the previous century, and so had Leibniz (though, as mentioned, he showed his hand in his later letters). Rudwick calls claims that the Church retarded the progress of science a 'stereotype' and says that the situation was more complicated (p. 116). In this view, he follows John Hedley Brooke (1991), but Brooke concedes that in the clash of religion and science it was always the suggestion that a God seemed unnecessary to explain nature that had bothered the religious camp. The Edinburgh professor and natural philosopher John Robison (1739–1805), for example, was perturbed by Laplace, who showed that the Solar system seemed stable enough not to require God's interference, because he had not drawn the theological consequence that God's creations were perfect (Brooke 1991, p. 240)! The 'complexities' in Church–science relationships did indeed arise because some savants (including the clergyman Soulavie—who later abandoned the priesthood) disregarded the Church's panic, the 'sensitivities' of their religious colleagues, and that of the public. But this was not because the Church's thinking had become more accommodating, but because it had lost some of its repressive power by the later part of the eighteenth century.[26]

However, the fact remains that the Church did *try* to keep science in line with the Judeo–Christian mythology and the biblical time scale. As the clerical attack on Buffon documents, religious pressure did succeed in halting science, because, in this case, Buffon not only recanted, but also did not publish his initial time scale (Roger 1962, see esp. pp. lxiii–lxvii and particularly the comparative table on p. lxv) Rudwick's claim that the short time scale was not challenged by science, because it had already been challenged by Aristotelian eternalism (p. 117) is thus incorrect. Rather, the short time scale was challenged *both* by the pagan long time scales *and* by science. But only the time scale of science was ultimately testable.

I should, however, acknowledge the factor in the relations of the Church with geology that Rudwick emphasises: some religious concepts did indeed provide geology with initial models, hypotheses and even assurances of success, such as the flood myth that had been instrumental in the birth of stratigraphy in the seventeenth century and even more so in the fact that it gave geologists confidence that global correlation of strata could be possible. But this does not negate the obstructive and misleading rôle that religion played during the time period here considered. And Rudwick's general statement that "Church–

[26] I quote Pierre Rousseau's (1959, p. 378) conclusion: "It was the Church that surrendered"—as the Catholic Church recently also surrendered to Darwin (see Holden 1996). As this review was being revised, I read that the Anglican communion followed suit on 15 September 2008.

geology relations were more complicated than is generally appreciated" is too vague to be useful.

Rudwick would have readers believe that the clashes between the religious readers of the geological record and the others were between two rival 'religions', namely theists and deists (pp. 57 and 317). I read the record differently and for the following reasons. First let me state my reading. The clashes were between those who thought that science had its own ends and those who wanted to make science subservient to mythology. The clashes were a part of what has appropriately been called a 'war' between religion and science (though some historians no longer seem to like the term). Let us take Hutton and de Luc as examples to see clearly how what I here propose to call, for the sake of the present argument, 'scientifics' and 'religionists' approached scientific issues in geology. I deal with Hutton as an example of the former, and with de Luc as a representative of the latter group.

Hutton appears to me as an example of a proper scientist, despite his deistic statements (Şengör 2001). However, even if he really had been a deist (and not a closet atheist, as I have endeavoured to argue), his attitude and methodology would remain scientific. He was modern both in his methodology (with his proposal of a hypothesis only to explain the available observations and its test by further observations[27]) and, it so happens, in his conclusions. Although by no means a measure of the degree of scientificity, all his geological inferences, with the singular exception of the sedimentary part of the heat-controlled induration hypothesis, have stood the test of time to our own days. Therefore, his modernity is not 'spurious' as strangely Rudwick claims

His modernity as a scientific thinker would be untouched, even if his support of a very high antiquity for the Earth as the work of a deity that created a perfect system were really a product of his deism as Rudwick, following many other Hutton scholars, claims. This is because Hutton originally had been led to consider the high antiquity of the Earth, when, as a farmer, he noted that the high rates of erosion seen during rains, for example, did not seem to strip the land of its soil cover in a short time. If he really had been a deist he would presumably have thought: "what a wise thing to ensure that what feeds this world is not washed out in a short time. Why doesn't that happen? What mechanism did the wise and benevolent god put into place to prevent the soil from wasting?" Note here that the hypothesis of a high antiquity of the Earth as an explanation of the longevity of the soil cover comes first and only after that is it attached to a deity; and then it is questioned how the deity ensured the operation of that fine balance of erosion and new soil formation.

If, on the other hand, Hutton was a closet atheist (as I think and a number of his contemporaries thought he was), he would likely have constructed the chain of reasoning as follows: "If the soil is not being wasted away something must replenish it. As erosion leads to denudation, this replenishment must take the form of uplift. Such a sequence of uplift–denudation–renewed uplift implies an Earth that is much older than anybody now imagines. [If I were a god and wished to ensure the prosperity of my creatures for a long time, this is the sort of thing I would have done. Thus, what I have discovered in the behaviour of the planet can be presented as the work of some sort of a benevolent deity, even if not the work of the Judeo–Christian God]". In this case, all one would have to do to reconstruct Hutton's real thought would be to take the deity out of the chain of reasoning suggested above (i.e. delete the part in square brackets) and the operation of the reasoning would not only remain intact, but also scientific.

[27] I discuss Hutton's scientific methodology at some length in Şengör (2001). See also McIntyre and McKirdy (1997). To see Hutton's position fully, one must consider also his epistemological work as recorded in his *Principles of Knowledge* (Hutton 1794, reprinted 1999).

When Hutton showed how the Earth displayed two sets of opposing processes fed by two different energy sources, this made all finalistic scenarios with short time scales suspect.[28] Possibly under de Luc's (1809, p. 24) or Lyell's (1830, p. 86) influence, who wrote that Huttonians asserted an "uniformity [of nature] without any limitation as to time", Rudwick endeavours to make Hutton an eternalist (pp. 277, 285, 432). But Hutton *never* uttered a single word to that effect and even protested against Kirwan that he was not an eternalist, a protest Rudwick unaccountably dismisses as 'hardly convincing' (p. 171, note 63)—which, I suppose, is why he calls Hutton an "implicit eternalist" (pp. 277 and 228). Rudwick concedes that Hutton implied that the world seemed eternal only to human apprehension; and he agrees that Hutton had meant—in reference to the history of the human species—"almost eternal in relation to the few millennia of recorded history" (p. 317). In his 'Coda', Rudwick repeats that Hutton's geotheory was "tacitly eternalistic" (p. 642). Yet in the final page of Hutton's famous 1788 paper (the last paragraph of which Rudwick reproduces on his p. 171) he simply says that he finds no evidence of a beginning and he does not see how the presently functioning Earth machine might stop in the future. But did not Cuvier—justifiably Rudwick's great hero of historical geology—say in his report to Napoléon in 1808 (published in 1810), that the state of affairs in geology led "us to despair of ever discerning its origin" (p. 462)? Would that make him an eternalist? Hutton's machine analogy for the Earth (p. 162) may indeed refer to the Newcomen steam engine as Rudwick believes (pp. 162–163). But if Rudwick is right in his suspicion that Hutton was a closet eternalist, he must have had a Newcomen engine working as a perpetual motion machine!

Rudwick cites Hutton as not giving "anything like de Luc's natural measures of time" (p. 169). But neither did Burtin (pp. 202, 297), nor did Desmarest (p. 211) and nor indeed did Soulavie (p. 221), all of whom were proto-geohistorians according to Rudwick and not eternalists. Indeed he writes: "[d]espite . . . a widespread qualitative sense of the likely immensity of time, savants rarely offered quantitative figures for the ages of specific features, let alone for the Earth as a whole" (pp. 127, 641). However, Rudwick interprets the silence of everybody except Hutton, concerning the length of time as a manifestation of scrupulousness, not eternalism. He also thinks of the narratives of Desmarest, Burtin and Soulavie as sorts of (but not quite) geohistory—but omits to mention, for example, Hutton's interpretation of the progressive erosion of the European continent as shown by Ailsa Craig, the little isle of Pladda off the coast of Arran in the Hebrides and the Continent itself (Hutton 1899 [1997], pp. 260–262). The basis of Rudwick's exclusion of Hutton seems to be that Hutton's general history was generated on the basis of few pieces of data interpreted in the light of his theory, whereas the other three based their narratives purely on local evidence. He does not seem to appreciate that the local evidence was read by Burtin, Desmarest, and Soulavie (and everybody else who has made the attempt) on the basis of a plethora of previous hypotheses. This persistent separation of geohistories that had models in mind and those that were without them is, I contend, Rudwick's principal error in his book.

In the geohistorical part of Hutton's work, we have, fundamentally, nothing but the application of Steno's stratigraphic principles, plus a heavy emphasis on cross-cutting relationships for intrusive bodies. What Steno had shown to have happened in Tuscany in his six 'episodes', the products of which are separated by unconformities, Hutton showed to

[28] And in envisaging a thoroughly dynamic Earth, with uplifts, subsidences, and erosion, Hutton was not "almost without precedent", as Rudwick says (p. 172). He had such predecessors in Empedocles, Aristotle and Theophrastus in the ancient world, and Jean Buridan and Albert of Saxony in the thirteenth century, who thought that the Earth was millions of years old.

have happened many more times in Earth history. By using Steno's stratigraphy (including the unconformities) and the cross-cutting relationships, Hutton made his models (however they may have been conceived) testable, as opposed to the "unashamedly ad hoc" (and therefore untestable and thus unscientific) explanations (p. 155, also see p. 158) of the self-styled "Christian philosopher" de Luc (pp. 151, 235, 314). That is why, Hutton's was *not* "yet another "system"" as Rudwick represents it (p. 158), but a proper scientific theory that could be 'tried' in detail, as Hutton himself said many times (and not 'verified,' or 'confirmed' as Rudwick makes Hutton say: pp. 164–166, p. 642).

When Rudwick relates that Hutton had conceived his theory without making observations first (a perfectly admissible and common scientific procedure) he ignores his days as a farmer, when he surely observed the erosion of the soil and contemplated why the Earth did not lose its soil cover in a short time, developing testable hypotheses to understand it, as discussed above. And that is why it is misleading to suggest, with Rudwick, that "any model was about as plausible as any other" (p. 179). This also explains why de Luc, as a geological savant, rapidly sank into oblivion (p. 158, esp. 496) and why Gillispie treated him in the way he did (p. 151, note 26). By contrast, Hutton's image has grown progressively as geology advanced as a science, so that, for example, the historian of geomorphology Gordon Harries Davies (1969) accorded him a place but little removed from Newton's in the pantheon of science. Rudwick simply calls de Luc's religious cosmology 'different', rather than identifying its difference as residing in its mythological basis (e.g., p. 173).

Finally, Rudwick is also wrong when he says Geikie established Hutton's reputation as a founder of modern geology. Even if we pass over what Lyell wrote of his influence (without actually reading Hutton's own writings!), did not Friedrich Hoffmann (1797–1836) write in the 1830s, in connection with Hutton, "we may therefore consider Scotland as the cradle of the new geology" (Hoffmann 1838, p. 210)? And was he not followed in the histories written by Keferstein (1840) and Vogelsang (1867), well before Geikie?

Rudwick's treatment of Hutton is one of the most curious and unsatisfactory features of his book and is hard to account for. But I suspect that his distaste for Hutton's metaphysical position may have something to do with the matter.

De Luc, in sharp contrast to Hutton, appears as one who clung to mythology with his attempt "to establish the certainty of the revelation to Moses" and because of his unsound methodology (e.g. measuring the age of a continental surface by the ages of the lakes nested on it), he was clearly left behind the times. Looking at rate of deposition of sediments in deltas was a well-known means of estimating time since Herodotus's attempt (II. 10) and de Luc applied this procedure to estimate the age of Lake Geneva. There was nothing original in that. What was original was his claim that by doing that one could estimate the age of the continental surface and therefore the time of the retreat of the waters from the present-day continents. One could reach these conclusions if one assumed *à priori* that the basin of Lake Geneva had formed simultaneously with the last recession of the seas. But de Luc could show no evidence for that. He must have known that by that time most geologists with any experience of lakes knew that lakes come and go. Could he not have known, among others, Robert de Paul de Lamanon's or Pallas's or Patrin's or Ronevantz's work illustrating the *ephemeral* nature of lakes on continents? Therefore de Luc's argument, the age of the Rhône Delta in Lake Geneva being a proxy for the age of the continental surfaces in general, was known to be nonsense even as he wrote it. And in 1809, he was still trying to uphold a "relatively literal interpretation of the mythological Flood legend of the Bible" (p. 564). But de Luc's illustrations did not stand the test of time—not even the lifetime of the next generation, as Rudwick himself documents (p. 328).

It is unimportant in judging de Luc's stature as a scientist that he was wrong, but it is important to remark that he was selecting and interpreting observations to support the mythology of the Bible. That is not a scientific attitude. His reasoning started with a belief in the veracity of the Bible and an express wish to demonstrate it. In fact he stated that it would be calamitous for mankind should geology disprove the story of *Genesis*![29] So let us now try to do the same thing to de Luc's reasoning that we did to Hutton's. Let us follow his reasoning and look at the chronology that the infilling of Lake Geneva yielded him.

For de Luc, the lake basin could not have been the result of river erosion, but had to be fault-controlled. He never bothered to check this statement. He then made an assessment of the age of the lake on the basis of the delta growth agreeable to his assumption that it had to be accommodated within the biblical chronology. (It would appear, in fact, that he simply assumed an age for the delta, because his age and the age as now estimated differ by a factor of two. See Houbolt and Jonker [1968].) He then assumed that the lake's basin had been generated during the last retreat of the waters from the continents. Now, in this chain of reasoning, if one takes the unmentioned Biblical time constraint out, not much remains except what Herodotus already had known. I would not call this advancing the methodology of historical geology significantly.

Moreover, the "essential distinction [that] was to be made among the various phenomena which the surface of the Earth exhibits, with respect to their causes; determining of each of them whether the causes which have produced it are still in action, or have at some epoch, ceased to act" (de Luc 1809, p. 36) was made on grounds that would have been considered thoroughly unscientific, even when de Luc was writing. For example, to prove the originally 'precipitated nature' of granite (a process that was, according to de Luc, no longer in operation) he was driven to deny the compound nature of water and the nature of heat (which de Luc oddly derived from light and concluded that before light existed no granite could have formed). Many of his deductions of agents no longer operative, would have driven his contemporary physicists and chemists to smile. But it is clear that they were conceived to salvage the *Genesis* account (e.g. his insistence on the rôle of light and on "times before the creation of light"). It is therefore surprising that Rudwick considers de Luc's "binary geo-theory" (also referred to by Lyell [1830, p. 76], without actually naming it as such) to have been "backed by a mass of new field evidence" (p. 240). Not a scrap of real field evidence was adduced by de Luc in any of his works to support his geohistorical reconstructions, or the belief that geohistory was in fact 'binary'. Neither did he discuss the real and relevant field evidence that Hutton had published.

So, de Luc's methodology of doing geohistory was, even in his own time, demonstrably fallacious and was, I submit, employed only to save Judaeo–Christian mythology (which, *pace* Rudwick, was his primary object). We know that many of his contemporaries saw through his reasoning and abandoned it, to "free . . . geology once and forever from the clutches of Moses" as Lyell's friend George Poulett-Scrope later picturesquely put it (letter to Lyell, 11 June 1830, quoted in Wilson 1972, p. 275).

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[29] De Luc was alarmed by claims that geology was uncovering facts contrary to what is stated in *Genesis*. He introduced his geological treatise that summed up his life's work by expressing this fear: "In proportion, therefore, to the influence which geological systems will necessarily have, must be the caution observed by the friends of mankind in their inquiries respecting this great object [i.e. the history of the Earth]. No general inference, indeed, drawn from the physical sciences, could be more important to men than that in which Genesis was involved; for to consider that book fabulous was to plunge them into a final uncertainty, with regard to what is most concerned them to know, viz. their origin, their duties, and their destination: it was sapping the very foundation on which the great edifice of society has always rested: it was, in short, abandoning men to themselves, and those must have been little acquainted with them, who did not foresee the fatal consequences which would inevitably ensue" (de Luc 1809, pp. 4–5). Rudwick largely skates over this side of de Luc's thinking.

I underline that Rudwick is wrong in thinking that de Luc's history of the Earth contained no more 'ad hocness' than any other theory of the time. None of Hutton's arguments were *ad hoc*, whereas de Luc could show no sensible and testable cause for the deluge or for its regression (p. 565), except by retreating into an Empedoclean Earth, which, by that time, had long been shown to be nonsense by Euler and Laplace. Neither could de Luc point to any reason why the Earth so drastically changed behaviour after the Deluge so as to justify his binary history. Of course, one *could* indeed claim that de Luc was as hypothetico-deductive as Hutton, but his 'theory' was the primitive Middle Eastern mythology of the Bible, which would make de Luc a fable-maker rather than a scientist. I therefore do not think that de Luc can be considered a 'historian' (of the Earth, of course) in Rudwick's sense.

6. THE EMERGENCE OF HISTORICAL GEOLOGY OR JUST THE EMERGENCE OF EARTH HISTORY BASED ON BIOSTRATIGRAPHY?

The second part of Rudwick's book is a straightforward narrative history of the development of stratigraphy in Western Europe. Yet there are still some curious features. He almost equates stratigraphy with geognosy and insists that the latter had neither an historical nor an explanatory component (p. 536). This is a claim I think neither a geognost nor a stratigrapher can accept. Even William Smith, whom no one would accuse of being a savant, had ambitions toward writing a geohistory and tried to do so in his later years.

In the second part of his book, Rudwick takes up the history from two years after de Saussure's mountaineering feat and concludes with Buckland's speleological discovery in Kirkdale Cavern. Rudwick rightly views Cuvier as the central figure of the development of what we today call biostratigraphy and the historical geology that can be deduced from it. The reader learns much about Cuvier's predecessors, not only in studying fossil animals but also fossil plants, such as Johann Friedrich Blumenbach (1752–1840) and Ernst Friedrich Freiherr von Schlotheim (1764–1832). (But strangely there is no mention of Carl Friedrich Kielmeyer (1765–1844), Cuvier's first teacher and close friend.) Rudwick also praises, and rightly too, Cuvier's great scholarship, not only in the natural sciences but also in history, emphasising his review of the evidence across many cultures for the reality of a major deluge early in the history of mankind. And he underlines the fact that Cuvier viewed the Judaic narrative of that event as but one among many.

However, the reader learns little of Cuvier's sources, except that he was a consultant for the French edition of the *Asiatick Researches* (p. 394). But it may be noted, for example, that his remark that the Altaic people and the Caucasians must have been derived from two different shores of the diluvial sea (p. 595) was straight out of de Guignes' *Histoire générale des Huns, des Mongoles, des Turcs et des autres Tartares occidentaux* (1756–1758).[30]

Rudwick gives much credit to Alexandre Brongniart and to William Smith for developing the geological implications of the strata-bound character of fossils and he correctly judges Smith's accomplishment as one primarily with petrographic-structural aims ('geognostic' as Rudwick somewhat idiosyncratically calls it). In this context, it is probably an oversight that Rudwick does not mention that in his original table of 1799 Smith included entirely inorganic objects, such as pyrite concretions and ochre, among his 'characteristic fossils', which supports Rudwick's view of him as one in the geognostic tradition. Another important point Rudwick makes is that both Smith and Brongniart were

[30] Joseph de Guignes (1721–1800) was Professor of Syriac in the *Collège de France*, and at the same time a great sinologist and the first serious historian of the Altaic peoples.

mapping in places where there are few natural outcrops and that they were tracing the courses of what could not be seen (pp. 436 and 474), which showed the power of the methods they were using.[31]

However, I dissent from Rudwick's view of early Werner, the geognost (i.e., from at least 1782, but not the oryctognost), as one who had little interest in the temporal sequence of the *Gebirge*, i.e., the rock masses, as Rudwick correctly translates the term. Already in Werner's book on veins, we see an interest in temporal sequences expressed not only by Steno's superposition principle, which Werner had been using in his lectures for at least the previous decade (as shown by Wagenbreth and von Engelhardt), but also by cross-cutting relationships. In any case, if Cuvier regarded geognosy as geology (p. 462), he must have regarded geognosy as being historical, which was his exclusive concern in the Earth sciences. And although Werner had a global theory, he was in fact ignorant of world geology (even of what little was then known), and as Greenough rightly complained when he met him, he did not know even the geology of German lands (p. 603).[32] Rudwick says (p. 175, note 67) that I am almost the only one who has recently presented a highly critical view of Werner. In that case, I find myself in the excellent company of the many distinguished contemporary critics of Werner (eventually including even his staunch defender Goethe).

What I find most valuable in Rudwick's history of stratigraphy is his combination of the studies in France, Italy, and Austria, all inspired by the work of Cuvier and Brongniart and in part made possible by Jean-Baptiste-Pierre-Antoine de Monnet de Lamarck's (1744–1829) studies of fossil shells. This work eventually led to a complete denial by Louis-Constant Prévost (1787–1856) of the main geohistorical theses of his teachers Brongniart and Cuvier and led, in the second half of the nineteenth century, to the most comprehensive tectonic synthesis of the globe the world had yet seen in the hands of Eduard Suess (1831–1914).

Another point that Rudwick makes forcefully, and I think correctly, is the influence of the erratic blocks strewn in the Alpine foreland and in Central Europe in providing support for the diluvial nature of the 'last catastrophe' that allegedly wiped out the ante-diluvial terrestrial fauna. I disagree, however, with his opinion that Christian Leopold von Buch (Baron of Gelmersdorf) (1773–1853) had *not* thought that the blocks had been hurled across the Swiss Plain by a giant cascade as a result of the catastrophic uplifting of the Alps. To document that von Buch really did have such a view, we have not only his illustrated publication (see Figure 1) showing how a great, muddy flood had hurled the blocks from the tops of the Alps to the tops of the Jura, passing over the intervening ground, but also Roderick Murchison's testimony (p. 576). Rudwick's opinion is evidently a result of his misunderstanding of von Buch's German text.

I also disagree with the suggestion that von Buch was a new type of geologist, disposed to change his mind concerning things on which he had once formed an opinion (p. 577). In fact, he was notorious as a dogmatic geologist and Suess gave an example of his ruining Carl Friedrich Schimper's (1803–1867) geological career because of his unbending

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[31] While it is true that Smith (at least initially) identified some rock units by means of their lithologies, it is a mistake to downplay his use of fossils in the identification and correlation of strata. For example, he used fossils to distinguish between the Upper and Lower Oolite in his Bath district. Yet one may agree that his interests were primarily structural and economic rather than historical, and when he attempted a geohistory of southern Britain in his old age (unpublished) it was in the style of a 'theory of the Earth'. On the other hand, Smith produced a successful biostratigraphi-cal/lithological map, which others could use to help them write a geological history of Britain. So to call the biostratigrapher Smith a geognost is unusual, but acceptable.

[32] See further Torrens (1998).

views (Suess 1913, pp. 13–14). He was able to criticise others' views (including his teacher Werner's), but seemingly not his own. Like von Buch, Cuvier was also disposed to forming causal opinions, as Coleman (1964, p. 135) has nicely demonstrated and he too could be dogmatic.

An important thing about Cuvier's interpretation of faunal change that Rudwick does not discuss in any depth is Cuvier's view that, in any one area, the 'newcomers' after any terminal catastrophe had probably migrated from elsewhere. Rudwick says this would have led to an infinite regress. But it was worse, as Cuvier's student constant Prévost recognised: it implies that Cuvier really had a position analogous to Hutton's! For, if, after every catastrophe, there was a supply of different animals to repopulate the devastated places the planet must have always had them. Thus, ultimately, there could have been either no change or only an impoverishment in the terrestrial fauna—unless new species were in constant production (as Lamarck supposed). This position was, then, as eternalist as Hutton's (or as 'morbid' as Buffon's) because, unless there was a production of new species, even Cuvier could not tell when (or how) the beginning had been (and let us remember that he did say as much, as Rudwick reminds us on his p. 462).

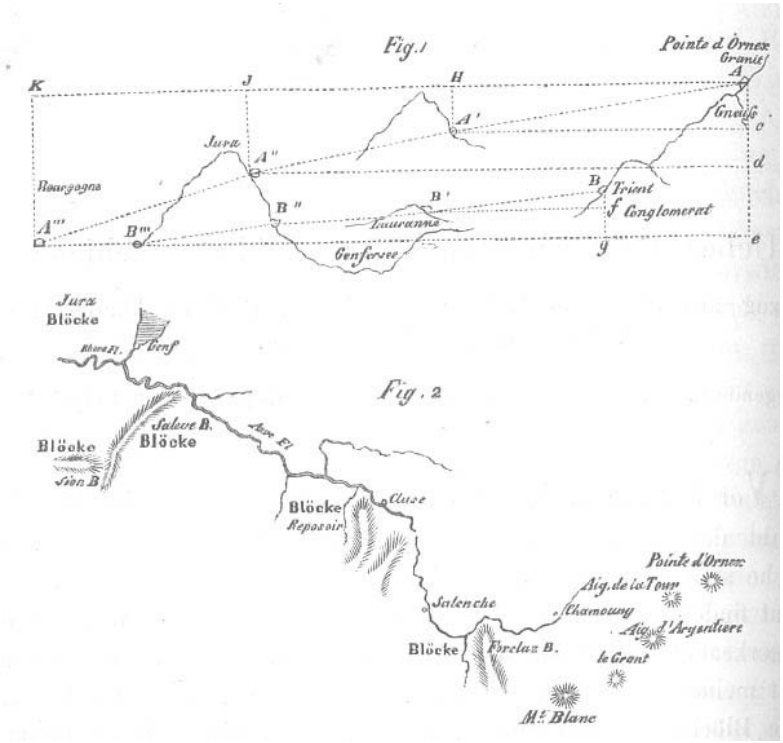

Figure 1. Von Buch's figure representing the hurling of boulders across the Swiss Plain to the Jura Mountains from the tops of the Alpine heights. From von Buch (1827 [1877]). Lines A, A′, A′′, A′′′ and B, B′, B′′ are the supposed trajectories of erratic blocks carried across intervening areas (shown on the map marked Fig. 2) including Lake Geneva: Genfersee *in the figure) by muddy floods. c, d, and e are points to which the blocks would descend in the flood during transport to the distances H, J, and K respectively.*

So in principle Cuvier, too, could not do without a production of species or some sort of transformism (unless he was ready to resort to the *ad hoc* model of creation, which, unfortunately, one of his most brilliant pupils, Alcide Desalines d'Orbigny [1802–1857] later adopted, no doubt because he carried Cuvier's 'no evolution' model to its logical extreme). However, Cuvier did not live long enough to see the logical consequences of the history he had written with such devotion and conviction. He died, universally admired and

celebrated, yet perhaps heartbroken, after his beloved daughter Clémentine, the only remaining one of his children, passed away, leaving him alone with his aging wife in this impersonal world that he had described with so much enthusiasm and genius. Two years before Cuvier died, the light of Huttonism had reappeared in the writings of the young Scottish gentleman, Charles Lyell, who had earlier been his guest. The great Cuvier had become a victim of his own strict positivism, and perhaps of his conviction that history could be done without hypotheses, without models, by trusting *veracitas naturae*. Although, in sharp contrast to de Luc, he never allowed it to interfere with his science, he did believe that there was some divine order in nature, even a *veracitas dei*. In the end, however, his life became a standing and glorious monument to the impossibility of a Rankean history—to which his most recent chronicler and one time fellow palaeontologist Rudwick seems committed.

7. CONCLUSIONS

After reading *Bursting the Limits of Time*, I have been unable to fight off the impression that Rudwick's historical interpretations seem more than a little coloured by his own religious commitment, as suggested by his attempts to show the Church less culpable than it really was in retarding science and to present piously Christian savants in a light more favourable than their writings and accomplishments justify. Despite that, Rudwick's richly documented, copiously illustrated, and elegantly written book is a most welcome addition to the literature of the history of geology owing to its rich harvest of erudition. But from my perspective it seems to me that its goal of interpreting 'what actually happened' in geology at the end of the eighteenth and at the beginning of the nineteenth centuries is by no means 'verisimilitudinous'.

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AUTHOR'S RESPONSE TO ŞENGÖR'S REVIEW OF *BURSTING THE LIMITS OF TIME*

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Most authors with any sense make it a rule not to respond publicly to hostile reviews of their work. This does at least avoid the unedifying spectacle of an exchange of brickbats that has to be terminated by an editorial decision that 'this correspondence is now closed'. So it is with great reluctance that I respond to Celâl Şengör's lengthy critique of my *Bursting the Limits of Time*, and I do so only at the Editor's request and with the Review Editor's assurance that the exchange will end here. Of course I am flattered that a geologist as distinguished as Şengör has chosen to give my book such close, though uneven, attention. But I would really prefer that readers of *ESH* should also read *BLT* (as I've long called it in my notes and computer files) for themselves, and then make up their own minds about its value, or lack of value. I shall therefore make this response as brief as possible, and without attempting to cover all the points that Şengör makes.

Şengör claims at the start that his critique is needed because other reviewers of my book "have perhaps been overawed by its scale and detail" and have therefore overlooked its defects and its downright errors. Perhaps we in turn should overlook the condescension implicit in this assertion. It would have been helpful if Şengör had cited, with as much diligence as he cites his sources in the rest of his essay, just which reviews he had in mind. This would at least have revealed to readers of *ESH* the calibre of those whose judgement he has so lightly dismissed, and would have enabled them, on looking at the reviews, to see for themselves that most of my reviewers have not been naively overawed by my work or flatteringly uncritical of it. He might have mentioned, for example, the notable 'review symposium' in *Metascience* (Vol. 16, pp. 359–395, 2007), which includes substantial contributions by Gabriel Gohau, Simon Schaffer, and James Secord—hardly a flock of featherweights among historians of the sciences—and also my response. This exchange had a courteous tone, wholly compatible with some trenchant criticism, which I find lacking in Şengör's essay.

Şengor's attention to my book has been uneven, because although he seems to have read much of it—which of course I applaud as a heroic achievement—and even criticises some of the footnotes, he seems not to have understood the large-scale structure of the work. He is apparently unfamiliar with historians' (and social scientists') routine use of the terms 'synchronic' and 'diachronic'; for right at the start of his essay he seems to describe the first, synchronic, part of the book as being a 'history' of historical geology (or geohistory) from 1787 to 1822 (though his syntax is unclear, and I may have misinterpreted him), and the second, diachronic, part as a "history of how [it] actually happened". In fact, Part 1 is explicitly a survey of the relevant earth sciences on a specific time-plane—I adopt a geologist's metaphor by treating Saussure's ascent of Mont Blanc in 1787 as a convenient and not wholly arbitrary 'golden spike'—while Part 2 is indeed a narrative of events from that time forwards as far as the early 1820s (*Worlds Before Adam* [Chicago, 2008], the sequel to *BLT*, continues the narrative almost to mid-century). I explain clearly that I start

with a synchronic survey in order to avoid having to extend the scene-setting in an indefinite regress into the more distant past.

BLT is explicitly *not* intended to be a full history of the earth sciences, even within my chosen period; instead, it offers an analysis and narrative of a decisive phase in the development of a geohistorical perspective within the practice of these sciences. So Şengör's complaint that I fail to mention a whole string of important figures from the late seventeenth century and earlier eighteenth—such as Hiärne, Bergman, Lomonosov and Bouguer—is beside the point. To avoid making the book even longer than it is, I chose to mention, in my synchronic survey, only a selection of earlier figures (such as Lehmann) whose work was still relevant to what was going on in the 1780s; and, in my diachronic narrative, only a selection of figures whose work in later years was either innovative or exemplary. My survey in Part 1 is explicitly a survey of the 'state of the art', or what was taken for granted among the relevant savants, around the time that Saussure climbed Mont Blanc and shortly before the start of the Revolution in France; it is not intended to be an account of the earth sciences throughout the earlier eighteenth century, let alone back in the seventeenth. Perhaps we can hope that Şengör will in future utilise his ample knowledge of some of the sources to improve our understanding of that earlier period.

Şengör also seems unfamiliar with the analytical tools routinely used by historians (and social scientists) when we distinguish 'actors' categories' from those of 'analysts'—or the terms and concepts used by historical figures from those that we now apply in order to clarify their meanings and intentions. He seems to be unaware that in the present case it was some of the historical actors themselves who drew a distinction between 'conjectural' and 'philosophical' histories on the one hand and 'erudite' or archive-based histories on the other. (I would have been more impressed with his critique on this point if he had cited research on the history of historiography more recent than that of, for example, Baeumler; and I doubt if I will be the only reader to find a dedication to the great Wilhelm von Humboldt a trifle pretentious when attached to what is, after all, just an essay-review.)

Şengör characterises my approach as naively 'Rankean', although I think I only mentioned von Ranke once, and then only to help label a later period in the history of historiography. I was primarily concerned to portray the opinion of many of the relevant historical actors—those who during this period came to call themselves 'geologists'—that reconstructions of geohistory had been decisively facilitated by their rejection of the genre that I call 'geotheory'—the geological analogue of 'conjectural' or 'philosophical' human history—and their pursuit, instead, of the analogue of archive-based 'erudite' history. I contrast these as, respectively, 'top-down' and 'bottom-up' strategies of (geo)historical reconstruction; but I make it clear that both employed models and conjectures, though in quite different contexts. Those who pioneered the 'bottom-up' or 'erudite' approach were not so naive—and nor am I—as to claim, or assume, that geohistory could be reconstructed from raw 'facts', without any use of *a priori* models and testable conjectures to make sense of fragmentary evidence. For example, I point out how such reconstructions were consistently based on the actualistic model, namely that—expressed epigrammatically—'the present is the key to the past' (and thus that the effective use of this interpretative model did not have to wait for the coming of Lyell). To repeat the point: those who practised this kind of newly historicised geology were not so naive—and nor am I—as to follow (or, strictly speaking, anticipate) von Ranke's famous aphorism about discovering what really happened (*wie es eigentlich gewesen*) without the use of historical models and low-level conjectures.

Şengör misunderstands what I mean by 'geotheory'. Being my coinage, it is an analyst's term, but in fact it denotes an actors' category, namely the genre that they

consistently labelled 'theory of the earth'. My neologism was introduced simply to avoid having to remind readers every time that the actors were using 'theory' in this specific sense. It denoted a *genre* of scientific work that aspired to explain all the major features and processes on earth under a unified causal umbrella of some kind, with the use of characteristically high-level conjectures. Hutton's *Theory of the Earth* was only one of many publications that used that phrase in their titles, and many more showed by their contents that they belonged to the same genre (the use of the term 'system' is a reliable telltale marker). When I introduced the term 'geotheory' I explained that my description of the wide scope of such 'theories of the earth' was a Weberian 'ideal type'. It is therefore beside the point for Şengör to claim (correctly) that not all geotheories displayed all the characteristics typical of the genre, though I do think that the geotheorists' aspiration to link the physical with the human, for example, was more pervasive than he allows (I am very surprised he does not see such links in Buffon).

Şengör is strangely antipathetic to my typology of the sciences of the Earth as they were being practised around the time of my historical golden spike. I do not claim that these practices were always sharply separated, or that specific figures pursued only one (or only one at a time). Saussure for example clearly regarded himself primarily as an outdoor 'physical geographer'; but that did not inhibit him from doing indoor 'mineralogical' work on specimens brought back from the field, or from having 'geognostic' insight into the three-dimensional character of a spectacular Alpine fold structure, or from conjecturing in an 'earth-physics' manner about its possible causation, or even from aspiring in due course to offer his very own global 'geotheory'. I do claim, however, that actors such as Saussure were well aware of which kind of practice they were adopting at any one time, or for any one publication; and it is clearly the case, as is shown by the titles they chose, that they generally distinguished the different genres within and between their publications. In some of his comments about the geohistorical element in such works, Şengör does not show adequate vigilance about the notorious ambiguity surrounding the word 'history' and its cognates (*histoire*, *Geschichte*, etc.) at this period: they could indeed denote a history in the modern temporal sense, but more often they denoted a purely descriptive sense of '*natural history*'.

Underlying much of Şengör's criticism, here and elsewhere, is his conceptual confusion between temporality and historicity. I make the rather obvious point that any causal explanation is bound to entail temporality, as for example in the analysis of the course of a chemical reaction or a planetary orbit. But such temporal processes, and their geological analogues such as erosion and sedimentation, vulcanicity and seismicity, do not necessarily entail any truly historical dimension; indeed the goal of causal explanations is that they should be independent of (geo)historical location and be valid always and everywhere. This conceptual confusion vitiates, for example, Şengör's critique of my analysis of geognosy as a primarily structural practice, rather than a geohistorical one. Of course the pile of *Gebirge* or formations was recognised by geognosts as also being (usually, though not invariably) a pile that had accumulated in sequence; that is, it had temporality. But I regard it as a significant telltale sign of the geognosts' *structural* objectives that they commonly described their formations from top downwards, in the very opposite of any temporal order, let alone of any geohistorical reconstruction. Of course, they did sometimes translate a structural order into a geohistorical sequence of events, but their practice remained primarily structural and only derivatively geohistorical. For example, they used the language of 'above' and 'below' far more commonly than that of 'before' and 'after', and so did many of their successors who came to be called stratigraphers.

I do not think, therefore, that it should be shocking to geologists that I treat Smith as a geognost in all but name. I call his practice—which he himself named 'stratigraphy'—an 'enriched' geognosy, because of course I recognise the heuristic power of his new emphasis on fossils as 'characteristic' of specific 'Strata' (as he called them). Şengör rightly points out that Smith initially included inorganic objects among his marker 'fossils'; but this only heightens still further his affinities with earlier geognosts. However, Smith's kind of practice soon generated in turn what I call a 'doubly enriched' geognosy, when Cuvier and Brongniart first used the fossils in the sequence of formations around Paris not only to help identify the rocks across country—they rightly called their map a '*carte géognostique*'—but also to reconstruct a contingent local geohistory of complex environmental changes.

The real core of Şengör's antipathy to *BLT* only emerges, however, with his critique of my suggestion that *one* important and positive input to the emergent practice of geohistory (though explicitly not the only one) came from the putative historicity that was considered by the actors—religious and anti-religious alike—to underlie the Judaeo-Christian scriptures and the religious traditions that treated them as foundational. Most of us who work on the history of the sciences had hoped that the time was past when any serious scholar could refer, as Şengör does, to a point at which "religion stopped being a menace" or to "the Church [as] culpable . . . in retarding science". The telltale use of the homogenised and unhistorical category of 'Religion' (in the singular) parallels the equally unenlightening use of the category of 'Science' (in the singular). Both effectively conceal the huge diversity, past and present, not only within each of these falsely hypostatised categories, but also in the relations between specific instantiations of each. In sharp contrast to this kind of essentialist historiography—or, put more simply, childish tales of goodies and baddies—historians nowadays agree that any serious analysis of these issues must define clearly the where, the when, and the who in any specific case, and above all which social groups were involved. Şengör is evidently unaware of this scholarly consensus about how these important issues should be tackled.

For example, with respect to the timescales imputed to the past history of the earth, it is of course true, as Şengör points out at some length, that many eighteenth-century ecclesiastics continued to insist on what modern American fundamentalists would call a 'young earth'; but a growing number of savants who worked on the sciences of the earth—some of whom regarded themselves as religious believers—inferred a vastly longer (though unquantifiable) geohistory, yet were not persecuted for their pains, even if they were criticised. Şengör's comments on the case of Buffon, for example, would carry more weight if he had studied more carefully the authoritative evaluation of the relevant episodes by the late Jacques Roger, the finest Buffon scholar of modern times, and if in particular he had consulted the best modern analysis (by Jean Stengers) of the archival evidence on the naturalist's brushes with the ecclesiastics at the Sorbonne.

Şengör's treatment of the cases of Hutton and de Luc (or Deluc) illustrates even more clearly the severe limitations of the outworn historiography of 'conflict' or 'warfare' between 'Religion' and 'Science', of heroic struggle between Manichean forces of darkness and light. Şengör, of course, presents Hutton as pure light in a dark world, and he does his best to make Hutton out to be an atheist like himself. Hutton may possibly have been an atheist, in his private heart of hearts, for all that any of us can now tell. But what remains accessible to historians is how he chose to present himself to his fellow savants, and that was as a typical eighteenth-century deist: expounding, in his case, pervasive machine-like intelligent design, a cyclic 'system' ensuring stability and habitability from and to eternity, and so on. Şengör acknowledges that his own opinion—that Hutton's deism was no more

than a prudent cloak for his 'closet atheism'—runs counter to the judgement of most modern Hutton scholars; and he does not seem to have followed his own prescription that one should read the *Principles of Knowledge* as well as the *Theory of the Earth*, which might have given him pause. My own opinion—and it can be no more than that—is that it would have required a feat of psychological contortionism for Hutton to devise (for public display) such a brilliantly coherent 'system' or geotheory of pervasive designfulness, if his true conviction (inside the closet) was that nothing of the kind was really the case. Whatever we may think of Hutton's brand of theology, it seems perverse to deny that he had one; his quarrel with de Luc was a case of deism *versus* theism (or—if we must use the term—Religion *versus* Religion), with *some* good scientific arguments on *both* sides.

Şengör, however, will have none of this. For him, de Luc embodies darkness without the smallest glimmer of scientific light: a reactionary obscurantist, by definition, because he sought to demonstrate the historical reliability of the book of Genesis. But this exemplifies a not uncommon conceptual confusion between motives and arguments. De Luc was indeed consistently open about his motives in terms of Christian apologetics: he hoped to persuade the sceptics or 'cultured despisers of religion'—not a brave embattled minority but the dominant party among the intellectuals of the Enlightenment—that Christianity was still an intellectually tenable option. But his motives in apologetics do not invalidate his scientific arguments, any more than the arguments and evidence invoked by Hutton in the service of his deism. It is no accident that it was de Luc, not Hutton, who coined the term 'actual causes' (*causes actuelles*) to denote the observable physical processes currently in operation in the present world ('actual' in a sense almost obsolete in English, though not in other languages). Some of his examples may indeed have been observed too sketchily and analysed too loosely, but his underlying actualistic method of inferring the past from the present was certainly sound (and of course it was adopted later by Lyell, though he obscured de Luc's intellectual paternity by renaming the processes 'modern causes').

De Luc inferred that 'actual causes' had only begun their work on the present continents in the aftermath of some kind of exceptional disruptive event in the (geohistorically) recent past. He claimed that they therefore constituted natural 'chronometers' of elapsed time, and that their cumulative effects indicated a timing for this natural event (only a few millennia ago) that was roughly compatible with the various dates computed by traditional textual chronologers for Noah's Flood or Deluge; though de Luc interpreted that story in a far from literal manner, recruiting for example Bryant's work on comparative mythology. Conversely, Hutton's principled denial that there could have been any such disruption would have to be counted equally 'unscientific', in the light of the Pleistocene glacial episodes that are now held responsible for many of the putative 'diluvial' features, though I doubt if Şengör would be so unhistorical as to judge it so. De Luc deserves to be taken as seriously by us as he was by his contemporaries, whether they agreed with him or not; he called himself a *philosophe Chrétien*, and both words should be respected. (The symposium volume on de Luc, currently in preparation under the editorship of René Sigrist and John Heilbron, will help to show what a versatile Enlightenment *philosophe* he was, far beyond his work on geohistory and Genesis.)

After all this, I was slightly surprised, but relieved, to see that in the final section of his essay Şengör did have a few positive words to say about *BLT*, and particularly for what I write about the importance of erratic blocks in the pan-European debates about geohistory. But even here, on a closer reading, the concession turns out to be somewhat grudging and the praise decidedly faint. Şengör has misunderstood what I say about von Buch's interpretation of erratics (it is somewhat patronising of him to attribute this to my failure to understand the German texts). In von Buch's earlier work on erratics he remarked that their strange distribution was as if they had been shot from a cannon, from the high

Alps all the way to the Jura, but he later said that this was only a loose metaphor for a mysterious event that had left him utterly perplexed. But after the notorious natural dam-burst and consequent catastrophic mud-flow in the Val de Bagnes he (and others) adopted this actual cause as a persuasive analogue of what might in fact have happened on a far larger scale in the distant past. It was this kind of mega-tsunami or sub-aerial turbidity current that he depicted in the much later paper to which Şengör refers. One positive by-product of his generally negative essay is that it makes available a fine reproduction of von Buch's map and diagram showing the effects of the putative mega-tsunamis down the Arve and Rhône valleys. (I considered using this instructive picture to illustrate the relevant passage in *Worlds Before Adam*, but felt at the time that I needed to restrain my boundless appetite for visual images!)

In the introduction to *BLT*, I summarised in advance how it would suggest *two* distinct sources for the growing sense of historicity in the sciences of the earth during the period covered by the book. The first, which I judge extremely significant but about which Şengör has little to say, was the 'antiquarian' or archaeological work exemplified by the then recent excavations at Pompeii and Herculaneum, which was closely related to the newly critical human historiography represented by the self-styled 'erudite' tradition. It was these studies that jointly yielded the immensely fertile stock of powerful analogies—of *Nature*'s documents, monuments, coins, inscriptions, archives, and so on—that Soulavie and others put to such effective use in pioneering the reconstruction of local or regional geohistories.

This heightened sense of human history, accurately knowable from both material and textual evidence, had links to the science (*Wissenschaft*) of 'chronology', which likewise aspired to make rigorous use of detailed textual sources. Chronology has acquired an undeservedly bad reputation, because it was of course generally focused on the 'sacred history' epitomised by the traditional dating system of years BC and AD. But this does not detract from its historical significance as a scholarly attempt to construct an accurate time-line for universal human history, using secular as much as scriptural sources. It expressed concretely the sense of contingent historicity that pervaded the Christian world-view of the time, the sense of an overall directional movement in human history from primal Creation through pivotal Incarnation towards ultimate Parousia. My suggestion in *BLT* is that those savants (such as de Luc, but also many others) who shared this highly historical outlook were pre-adapted, as it were, to find it easy and congenial to extend it back beyond human history, as soon as the empirical evidence began to suggest that there had indeed been an unimaginably lengthy and diversely eventful pre-human geohistory. This, then, is the second of the two major sources that I suggest for the decisive historicising of the sciences of the earth during a few decades before and after 1800 (how the two were related to each other is an issue that I did not resolve satisfactorily in *BLT*, and that needs further work, though probably not by me).

It is this second claim that Şengör cannot stomach, because it entails conceding that his singular 'Science' received a positive input from the realm of darkness represented (for him) by an equally undifferentiated 'Religion'. However, I think my inference is no less plausible than the notion, which has long been accepted among historians, that the development of our modern natural sciences may also have been facilitated (during the 'early modern' period, well before the period my volumes cover) by magic, astrology, alchemy, Hermeticism, numerology and so on. That such practices still have some adherents in our modern world is irrelevant to our task as historians, which is primarily to understand empathetically how they made sense and seemed plausible in their own time; a historian of seventeenth-century natural magic, for example, is probably not suspected by his or her academic colleagues of dabbling in magic, black or white, at the weekends. So

my suggestion—which is hardly original—about there having been a positive input from the Christian religion into what became the science of geology does not require or entail any religious commitment on the part of the historian.

However, in view of the anti-religious prejudices that are currently so widespread in academic culture, it is worth pointing out that such a commitment may not be altogether a disadvantage, let alone a disqualification. To clarify this point, I cannot do better than repeat what I wrote in the introduction to *BLT* (in retrospect, I should perhaps have promoted this footnote to the main text). "The conclusion summarized in this paragraph [here, the one before last] came to seem compelling only at a late stage in the writing of this book, as a result of my detailed research; it was not a guiding feature of my interpretation from the start. My own personal beliefs may have made me more open to the evidence in its favor than I might otherwise have been—I did not approach the sources with the usual knee-jerk hostility to all things religious—but I did not expect this conclusion, still less strive to "demonstrate it". It should go without saying that this does not mean that I approached the sources with a Rankean *tabula rasa*, but that I applied testable conjectures to them and that I think they stood up to that test.

So, finally, Şengör concludes his review by stating that "Rudwick's historical interpretations seem more than a little coloured by his own religious commitment". I could respond, quoting the apt English proverb, that "what's sauce for the goose is sauce for the gander": that is, I could retort that Şengör's historical interpretations seem more than a little coloured by his own atheistic commitment. But this kind of mud-slinging just muddies the scholarly waters, and I will take it no further. I have the greatest sympathy with those of our colleagues who currently feel embattled and threatened by the recent rise to political prominence of various forms of fundamentalism, whether they have to live with a supposedly Christian creationism in power, at least locally and potentially, in the United States, or with the (moderately?) Islamist AK party now actually in power in Şengör's native country. But it is surely extremely unwise to confront the challenge of religious fundamentalisms by doing anything to encourage the growth—which is currently all too apparent worldwide—of an atheistic fundamentalism that is equally dogmatic, narrow-minded and intolerant of criticism. We must hope that Şengör has no intention of doing so, but his indiscriminate hostility to an undifferentiated and unhistorical category of 'Religion' greatly detracts from the value of his critique of my *Bursting the Limits of Time*.

Vic Baker, Book Reviews Editor

A HISTORY OF PALEONTOLOGY ILLUSTRATION. *Jane P. Davidson, 2008. Indiana University Press, Bloomington, Indiana, 217 pp. Hardcover, $39.95*

"Dinosaur images are not art: they are scientific illustration, or kitsch". So said W. J. T. Mitchell in his cultural survey of dinosaur imagery, *The Last Dinosaur Book* (1998). By contrast, the author of this new study says unequivocally: "paleontology illustration is art, and it has its own place in art history". This thought-provoking survey places the development of paleontological illustration in both artistic and scientific contexts.

Davidson teaches the history of both art and science at the University of Nevada, and is well qualified to write this book. Several degrees in art history are complemented by a passion for paleontology: she grew up "devouring every word" of the Fentons' *Life Long Ago* (1937), and is known to historians of vertebrate paleontology for her 1997 biography of Edward Drinker Cope, *The Bone Sharp*. Now she has provided a detailed history of paleontological illustration.

The visual record of paleontology is not an entirely neglected subject. Donald Glut gathered a wealth of images from popular media in *The Dinosaur Scrapbook* (1980), while Martin Rudwick's *Scenes from Deep Time* (1992) provided the first critical study of the art of tfossil reconstructions and landscapes. The experience of the sculptor was called on by Alan and Diane Debus in their *Paleoimagery* (2002), and Mark Berry has surveyed movies in *The Dinosaur Filmography* (2005). A pioneering monograph on paleontology artist Charles Knight (1982) has been followed by a recent volume on Waterhouse Hawkins (2008).

"A History . . . " suggests a more all-embracing study than we actually get. Davidson starts in the fifteenth century with a Flemish painting of a goldsmith's shop on the wall of which a couple of sharks' teeth (which may or may not be fossilized) can be seen. Half the text covers the succeeding centuries to 1900, then twentieth century coverage is detailed to 1937; developments of the last seventy years are sketched in a few pages. Thirty pages are given to notes, bibliography, and index. The focus is mainly on published illustrations of fossils, of paleontologists (in a chapter in which 'The Paleontologist Poses With Fossils'), and on restorations of extinct forms in their natural environment. (Other subjects—the work of paleontology in the field and laboratory, and representations in sculpture, film and television, folk art, caricatures and cartoons—receive only passing mention). The fossils presented are almost entirely animal; some 73% of the illustrations feature vertebrates, and 23% invertebrates, while plant fossils are important only in 4%. When Waterhouse Hawkins moved his studio from Britain to the United States in 1869, the author's focus follows, and largely remains there.

A book on art stands or falls on the quantity and quality of its illustrations, and Davidson has been well served by her publisher. Eight color plates and around ninety monochrome, many of them at half or full page size, are generally well reproduced, and provide a splendid sample of the illustrations under discussion. Some are familiar; others have not been extensively reproduced elsewhere; many I suspect appear for the first time since their original publication.

The text follows the evolution of paleontological illustration, letting history provide the flow unless an interesting story develops justifying a departure from strict chronology. A series of technical and popular illustrated works on paleontology appear presented more or less sequentially, each with a discussion of the technical and artistic quality of the published illustrations. Some information about authors, artists and (where appropriate) lithographer or engraver is presented. New techniques used in book illustration are

introduced as they developed, and there is much incidental information, as for instance about the mysterious footprints of *Chirotherium*, and Henry Ward's business in catalogue sales of fossils, casts, and models.

Davidson thus provides the first detailed account of technical illustration of fossils, and she provides much useful information on the evolution of techniques for illustration and publication as applied to the extinct world. The chapter on paleontologists and their fossils is the first study I am aware of on this interesting sub-genre. Her work on restored landscapes of the past overlaps to some extent that of Rudwick up to his end point in the 1860s, but includes some later examples into the twentiethth century. There are many fascinating details—the first fossil photographs were daguerreotypes as early as 1839; Roy Chapman Andrews staged his famous photographs of Mongolian dinosaur nests; while authors frustrated at slow publication in the modern era may reflect on the illustrations of fossils in the Papal cabinet prepared before 1600 but which were not published until 1717.

There are a few obvious errors. The role of founder of the Geological Survey of Great Britain has been mysteriously transferred from Henry De La Beche to William Hellier Baily (who was a draughtsman and geologist with the Survey). William Buckland is credited with authorship of plural Bridgewater Treatises, but only wrote one of the eight, while C. H. Sternberg wrote two autobiographical books, not just the one cited here as "his autobiography". And the mysterious Joseph 'Prestwick' invited to Hawkins' celebrated 'dinner in a dinosaur' must surely be Joseph Prestwich, Professor of Geology at Oxford (who had taken lessons in oil painting and lithography from Hawkins, and later discovered a new species of *Iguanodon*). A few names are misspelled—W. H. Bail(e)y, Richard Brook(e)s, Philip Cur(r)ie, and Waterhouse Hopkins (Hawkins) may puzzle readers. I also noticed a few errors with fossils: Figure 2.4 shows a goniatite, not an ammonite, and 'Oredon' should presumably be *Oreodon*.

There are places where a little further information would have been helpful. While the author is careful to cite original illustration numbers in the text, these are not given in the captions, so it is not always clear if she is discussing pictures reproduced in her book. And while art may be international, artists are not; the U.S. would not seem to dominate the story quite so much if national affiliations of artists from other countries were acknowledged, and if the author told us, for example, that Zdenek Burian was Czech, that Brian Cooley is Canadian, and Luis Rey is a Spanish-Mexican artist living in London.

Nor will all of Davidson's interpretations meet with universal approval. It is surprising to be told, for instance that "Hooke was not a geologist", when as early as 1935 he was acknowledged as "the first English geologist". Not is it made clear that the recent change of dinosaur images from dull greens and browns to bright colors arises from the changing taxonomic view of dinosaurs and their relationships—"one gets tired of brightly colored fauna" says the author, as if modern painters were guilty of a lapse in taste rather than reflecting changing understanding of science. While Rudwick draws the connection between illustrators of fossils and the established tradition of nature art, Davidson ignores this background, mentioning for instance "J. Sowerby, known by his monogram, JCDS" without any further elucidation of his life. Surely it is pertinent that James de Carle Sowerby, and the rest of his four-generation family played a major role as naturalists and illustrators in Victorian Britain. Also puzzling is Davidson's surprise in finding "how many paleontologists were themselves artists"—drawing specimens has long been an important technique to facilitate close observation, and not all scientists are without skill in this activity.

Choices of what to include and exclude are ultimately individual, but the book would have been greatly strengthened by dropping a few routine invertebrates or the token treatment of the last eight decades, to include a few art works that are of great historical importance to paleontology. Mayor, for instance, has shown that paleontological illustration began at least two millennia before Davidson's first example, with a fossil-

bearing Greek vase dated *ca* 550 B.C. The first reconstructions of vertebrates (by Mary Anning, Gideon Mantell, Cope and others) have not been not published until modern times, but their drawings are surely of great historical importance. More plant examples and pioneer illustrations of microfossils would have broadened and enhanced the scope of the work. On the other hand, the inadequate treatment of the last eighty years could just as well have been left out, leaving the rich and vibrant paleontology art work created since the dinosaur renaissance (by fine artists from around the world) to be the subject of the book-length treatment it deserves.

These criticisms should not obscure Davidson's overall achievement. The historian of the earth sciences will find much in *A History of Paleontology Illustration* that is not readily available elsewhere, notably a splendid series of illustrations with a supportive, though by no means comprehensive, text. It will stay on my shelf as a useful work of reference.

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THE ROLE OF WOMEN IN THE HISTORY OF GEOLOGY. *Edited by C. V. Burek and B. Higgs, 2007. Geological Society Special Publication 281, 342 pp. Hardcover £85.00.*

This volume is an extension of a conference held at the Geological Society of London in November, 2005. At its best it is a collection of papers about interesting human beings doing significant geological work in specific historical, social, cultural, and personal circum-stances. The fact that those individuals are women may have been the impetus for the volume, but it is certainly not the only, nor necessarily even the primary, reason a reader would find the volume valuable.

Written by a mix of historians and geologists, the papers reflect the methodological and stylistic differences of those disciplines. And from a historian's perspective, many lack much foundation at all in the existing secondary literature. But overall the papers are well written and firmly rooted in the primary documents. There is much here to enrich the understanding and reward the time spent by any reader.

Much, but not all, of the focus of the volume is on Great Britain. Most, but not all, of the focus of the volume is on the twentieth century. A few of the papers read as catalogs of information, inviting contextualization and analysis in a broader framework. Others provide rich contextual frameworks that both provide biographical detail of individual lives and create much broader understanding of the practice and content of geological science. And among the individuals represented here are examples of human beings of great social consciousness; intellectual strength, curiosity, and passion; and individual sense of self.

There are twenty-two papers in the volume, and every one of them is worth reading. In the interest of editorial space and the reader's time, let brief accounts of three entice you to peruse the whole.

M. R. S. Crease's "Fossil hunters, a cave explorer, and a rock analyst: notes on some early women contributors to geology" looks at a total of nine women, from Scotland, the United States, Sweden, and Russia. The biographical sketches are brief, but the paper is rich in information about their scientific work and its social and scientific context. It also provides some comparison of these four national contexts with regard to women's participation in geology in the late nineteenth and early twentieth centuries.

H. E. Fraser and C. J. Cleal's "The contribution of British women to Carboniferous palaeobotany during the first half of the 20th century" also works from a specific case study to more general conclusions, but uses longer biographical sketches with greater detail about the content of the individual's scientific work within a specific national context, and concludes with 'Others' and 'Discussion' sections that provide broader context and pattern analysis.

M. Kölbl-Ebert's "The role of British and German women in early 19th-century geology: a comparative assessment" develops an intriguing and important argument. Kölbl-Ebert builds the case that the different timelines in these two nations for industrialization and the professionalization in geology created a window from approximately 1770 to 1840 for women's participation in geology in Britain that never existed in Germany.

The editors of the volume are explicit in their preface that they hope this volume will be the beginning of much additional work. The material included here provides both a wealth of starting points for further study and numerous examples of how significant that work will be to expanding our understanding not of 'women's geology', and not just of women's contributions to geology, but of the way geological science is encouraged, supported, conducted, and disseminated by *all* its supporters and practitioners in a variety of national and societal contexts across a range of chronological periods.

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AWFUL SPLENDOR: A FIRE HISTORY OF CANADA. *Stephen J. Pyne. 2007. UBC Press, Vancouver, British Columbia. 549 pp. Hardcover, $93.95. Softcover, $34.95 (CAN).*

Fire is crucial to most of Canada even today, be it a log on a largely ornamental fireplace, a giant coal-fired generating station producing smokeless heat in electric heaters, or atomic fires driving turbines to protect Canadians from their climate. Over most of Canadian history, fire has also been a deadly danger: native long-houses swept away in winter fires that doomed their inhabitants to freezing and starvation, settlers' cabins consumed with their surrounding woods and frontier shack-towns erased in an hour's inferno. So vast a topic has given rise to a large literature, but until now, it has been largely a literature fragmented across time and space. Stephen Pyne's book addresses the need for a modern, over-reaching narrative of fire in Canada.

This book is the latest in the series that Pyne has published on the topic of fire around the world and through the ages. There is a large literature treating various aspects of the fire experience in Canada, but this one is unique in its scope. Pyne tells us frankly in his introduction that he found the Canadian experience of fire difficult to deal with because the country is divided by its geography and climate into discreet environments of fire. Coupled with the geographic disunion of the environment is, of course, the political division of the country into political satrapies at loggerheads with one another in a way that is perfectly familiar to Canadians and startling to new students of the Canadian scene.

Pyne examines the human experience of fire in Canada under three principal headings. The first 'Torch' deals with Canadian pre-history and early history. The second section, entitled 'Axe', deals with the coming of Western agriculture and commercial forestry to the Canadian woods; and the last section looks at modern efforts to deal with fire in Canada under the rubric 'Engine'. Book 1, 'Torch', is interesting because of its scope in space and time. The author discusses the role of fire in pre-Columbian society on the basis

of a broad spectrum of sources, many of them very recent, with the result that he often gets into areas that are little known. As a result, his conclusions often go beyond the standard interpretations of the topic. He shows, for example, that human-caused fires in pre-European Canada were more numerous and smaller than those produced by lightening, just the opposite of the situation when the Europeans became established.

Book 2, 'Axe', treats of territory more familiar to the student of Canadian history as the author examines the role of fire in the settler's world. Much of this is familiar territory; vast fires thundered through miles of prime forest or swept over the parched plains; terrified inhabitants fled or perished miserably. Pioneers were resigned to the fact that every period of drought brought peril to their farms and towns. In a world lit and heated by open flame, fires were frequent. The threat of fire was part of pioneer life in much of North America and nowhere more so than in the Canadian boreal forest. Here Pyne's most significant contribution is his introduction of fire experiences far afield and deep in time. The unique fire history of Newfoundland is a good example of his range and perception. Why has Canada's coldest and wettest province suffered from such destructive fires? Pyne finds the answer in geography and government. He also shows us how nineteenth-century Canadians applied urban attitudes toward uncontrolled fire in town, where it was always bad, to fire in the wilderness, where periodic fire is often necessary for the health of a forest. This leads him to Book 3, 'Engine', and the Canadian war to stamp out forest fires.

A casual reader might expect that a book about Canadian forest fires would concentrate on the century starting with the Miramichi catastrophe of 1825 and ending perhaps at the Ontario Claybelt holocausts in the 1920s. That was the era of great fires that were ruinous to settlers and deadly to human life. Instead, Pyne devotes more than half the book to the aftermath of that period, to governmental efforts to quell the flames and assure the safety of the population and its timber-berths. This is at first glance unpromising territory, but Pyne has been there before in his earlier works and he does a masterful job. The story is laid out in a narrative form that makes clear the growth of fire policies across the country. Pyne is an American, but with a good grasp of the intricacies of Canadian Confederation and the particularism that, as far as fire is concerned, has led lower levels of government to put their own people at risk rather than share one iota of their powers. The author traces the struggle between the Federal Government and the lower levels clearly and with admirable impartiality. The rule of the game is that the provinces and territories have jurisdiction over forests and the reality is that big fires don't care about jurisdictions. Settlement pushes deeper into the bush and onto the plains. Fires become more common and more destructive. Big logging companies use their financial push to drive their agenda at the provincial level, while provincial and territorial governments defend their turf against all the evidence of common sense and common danger. The reluctant provinces are periodically forced to call on the Federal team for help. The deadly danger of fire keeps dragging the politicos back to the reality that individual provinces and territories, even the largest, can't afford the resources to fight the biggest forest fires. In the end, fire wins, and the provinces have to agree to a Federally co-ordinated fire-fighting agency that now directs fire-fighting with a pool of Federal equipment, especially aircraft.

Underneath all the political wrangling is an unexpected substrate: the unsung, almost unrecognized work of Canada's fire scientists. Pyne discusses their work, chiefly done at the Pettawawa Fire Science Station near Pembroke, Ontario, and concludes that the Canadians produced a more useful body of work than their American and European counter-parts—work that has worldwide recognition today. The author's respect and even affection for these scientists is striking. He is particularly impressed by the long tenures many of the Canadian researchers held at the top of their profession, in a way that reflected what he sees as their very Canadian love of doing something valuable rather than pursuing

promotion and prestige elsewhere. Contemplating the spectacle of these scientists doing world-class work against a background of endless political quarreling and grandstanding, Pyne concludes that Canadians are great workers but poor leaders. Nothing in the history of Canada's fire services would seem to contradict him.

This is an admirable book except for one surprising flaw: the book's language is over-blown to a shocking degree, especially in the first section. The tangled grammatical structures and constant use of exotic words make the book hard to read and it frequently leads to oddities that are irritating or even hilarious. For example, in talking about bicycle patrols, Pyne invariably uses the antique term 'velocipede'. In a description of native duck-hunting he describes the hunters' canoes "drifting unsuspectingly into the flocks", and instead of saying something like "the deadwood piled up" he says "The deading timber amassed belts of combustibles". There are very many similar missteps that detract from an otherwise important addition to Canadian historical studies. Beyond the linguistic stumbles, however, this is a valuable book for anyone who wants to understand the role of fire in Canadian history and government.

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LANGUAGE OF THE EARTH. *2nd edn. Edited by Frank H. T. Rhodes, Richard O. Stone, and Bruce D. Malamud. 2008. Blackwell Publishing, Malden, MA, 328 pp. Hardcover, $34.95.*

We are formed of the dust of the Earth, just as the ancient scripture affirms. But that dust is star dust (F. H. T. Rhodes and F. D. Malamud, Preface to *Language of the Earth*, 2008, p. ix).

That sounds like poetry (Roy—a character in the 1941 Humphrey Bogart movie High Sierra).

The first edition of this book, now out of print for a decade, appeared in 1981. That volume, co-edited by Frank Rhodes and Richard Stone, who passed on before the book was finished, was superb. Based on a survey of readers of the *Journal of Geological Education* (1993, v. 41, p. 26), it gained a well-deserved place as one of the 'Great Books of Geology'. Still it drifted out of print, though from time to time someone at a convention of geologists would say it was returning. And so it has, some twenty-seven years after the first edition. I feel fortunate to be here to read it.

Twenty of the authors from the first edition have been dropped; in their place, fifty-five new pieces have been added. (Disclosure: I am one of those, tiptoeing in with a page-and-a-half on the Bingham Canyon copper pit in Utah.) In all, this second edition contains 122 items, most of them extracts of longer articles. Were that there had been room for more.

The book is arranged in four parts: 'The Earth Experienced', 'Interpreting the Earth', 'Language of the Earth', and 'The Crowded Planet'. It is further divided into fourteen chapters. In the 'Crowded Planet', for example, they are called human history, resources, and the benevolent planet. Some pieces could be placed within more than one of the four major parts or in different chapters. Frank Rhodes, who contributed two articles to the book, rewrote and expanded all the introductions from the first edition. These are

illuminating and a valuable addition to each section. Since the first edition appeared, the Earth's population has grown from 4.5 billion to 6.5 billion and continues on its stressful increase, writes Rhodes.

The range in time of the writing is from around twenty-five hundred years ago (The Book of Job) to 2001. Almost four-fifths of all the pieces are from the twentieth century, of those, about four-fifths of them appeared in the second half. Fifteen selections come from the eighteenth century and four from the eighteenth (Voltaire's *Candide* and his 'Lisbon Earthquake'; James Hutton writing on the system of the Earth, its duration and stability; and Jon Thorlakson on the eruption of the Icelandic volcano Oraefajökull in 1727).

Though some of the writers are scientists, and there is a moderate amount of geology among the pieces, this is not a book of science. In range, however, the book is broad; it is also eclectic. Probably there is as much 'philosophy' here as science. Such a varied collection will appeal to general readers and would be a great addition to a beginning class. It is also a book to savor through the year—a keeper. I know of no other anthology about the Earth that surpasses this one.

Geologists who have lived as long as I have will be struck, I believe, by how many of the writers they actually know or have at least met, an artifact of many conventions and field trips and not least through their reading. Of those whose work I have long admired, and wish I had met in my eight decades, there are four women: Isak Dinesen (1885–1962), Rachel L. Carson (1907–1964), Jacquetta Hawkes (1910–1996), and Diane Ackerman (1948–).

With its emphasis on Earth and its many moods, *Language of the Earth*, features poetry, "whether narrative, lyric, or dramatic". The editors include fifteen examples, particularly work by William Wordsworth, A. E. Housman, Kenneth Rexroth, A. R. Ammons, and Charles Simic, recently our Poet Laureate and along with Wordsworth my favorite of the group. As a senior in high school, in the U.S. Navy, I have carried him for the rest of my life. Frequently, the writing in many of the other extracts in the book is also poetic—words that apparently came to the authors out of the air during contemplation of broken silver bentonite in Wyoming, or while watching a downy woodpecker working away on a maple beside a dormant willow.

One of the longer chapters is called 'Prose'. The writers represented there are, in order: Isak Dinesen, T. E. Lawrence, Ernest Hemingway, Antoine de Saint-Exupéry, John Fowles, John Muir, Mark Twain, Thomas Fairchild Sherman, John McPhee, John Darnton, Kim Stanley Robinson, and Sir Arthur Conan Doyle. These are all men, except for Dinesen, who Hemingway—a writer not inclined to lavish praise on other writers—once said deserved the Nobel Prize for Literature. All of the extracts are short passages from books. Through the years I have read all of them, except Robinson's *Antarctica*, a strange omission on my part since I have a great interest in Antarctica.

In my peripatetic days as a book reviewer, I have reviewed Fowles, Muir, McPhee, and Twain (our first great writer). There you have it. This section is my favorite part of the book. A large chunk of my life has been spent reading, starting as a kid and reading late at night under the covers with a flashlight to finish *The Adventures of Huckleberry Finn* before the world did. My guess is that this chapter may be the one favored by the majority of readers.

Disclosure two. Last spring I heard again that this second edition was about to appear. Those words, perhaps rumors as they had been in the past, prompted me to 'Google'. (This is an enterprise one of the candidates for president of the United States, the losing one, would embark on later in the year, according to journalists on CNN.) What I found was a surprise. Google said the book would be forthcoming on the 18th or 20th of April 2008. It would also, said the publisher, represent "the human experience over the

centuries, covering a span of 2,500 years". "Further, it reflects the planet's extraordinary physical diversity".

Well and good. But what most caught my attention was the assertion that the book would "contain writings by scientists, artists, aviators, poets, philosophers, novelists, historians, and sociologists including Charles Darwin, Dane Picard, Rachel Carson, John Muir, Mark Twain and Archibald Geikie". How I landed in that mix of long-gone giants I do not know. Luck and geography are everything. For a brief moment—the seconds it took me to print it out—I was on a high. Though the inclusion was thoroughly undeserved, I still felt much as I did the time in 1945 when I scored the winning basket against Basin in overtime, a victory for the Worland Warriors.

Such a review as this may not prepare a reader for the richness and variety of *Language of the Earth*. A relatively short book, it feels long because of the vast territory it covers. Though not exhaustive (an impossibility), Rhodes, Stone and Malamud have put together what will be the standard anthology for a new generation of geologists and other Earth lovers. The clarity of writing, its excellence, and the warm feelings for the Earth expressed in these pieces should capture most any reader. I loved it.

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WALLACE STEGNER AND THE AMERICAN WEST. Philip L. Fradkin. 2008. Alfred A. Knopf. New York, 369 pp. Hardcover, $27.50.

What we call man's power over nature turns out to be a power exercised by some men over other men with nature as its instrument (C. S. Lewis, *The Abolition of Man*, p. 35).

Aspens all around us on the overlook are shedding ice with a scolding sort of clatter; they sound like fussy women sweeping their porches clean of litter after a storm. Where they have not shed, they glitter in sheaths of white ice. The air pure, without haze; the sun warming up. The snow soft, the trail thawing in the ruts and against north banks. A mule deer buck with a rack like an elk takes off into the aspens just before the Forest Service Lookout. Further west, a beaver dam and a lot of felled aspens lying crisscrossed in the woods (Wallace Stegner, in Fradkin's book, p. 210)

Born in Iowa in 1909, Wallace Stegner died in 1993 at the age of 84, a few days after pulling in front of another vehicle near Santa Fe, New Mexico. A major writer of fiction and non-fiction and one of the century's most important conservationists, he spanned more than eight decades. In thirteen novels, nine non-fiction books (one with his son, Page), 242 non-fiction articles, and fifty-seven short stories, he chronicled with feeling and great skill lives and landscapes of the West, dancing nimbly—Fred Astaire-like—with both history and fiction.

Stegner earned his Bachelors degree at the University of Utah, later claiming those were "the happiest years I ever knew or will know". He came to regard Salt Lake City as the closest to a hometown that he ever experienced. Influenced at Utah by the teacher and (later) novelist, Vardis Fisher, he began to believe that he could be a writer. Bolstered by some publishing success while an undergraduate, he went on writing, never wavering for long through his whole life. For three years (1934–1937) following his graduation, he taught English at the university.

In this, the best of three biographies of Stegner's life, Fradkin calls him our "premier chronicler of the twentieth-century western American experience". Though his first priority was always writing, he was also a superb teacher, who influenced Edward Abbey, Wendell Berry, Larry McMurtry, Scott Turow, and many others during his tenure at Stanford University when he headed its creative writing program. His contribution to younger writers throughout his career was especially notable.

For earth scientists, Stegner's best-known work, appearing in 1954, is *Beyond the Hundredth Meridian*, subtitled *John Wesley Powell and the Second Opening of the West*. Captain Clarence E. Dutton (1841–1912), Powell's close colleague and the brilliant author of the *Tertiary History of the Grand Canyon District*, was the subject of Stegner's doctoral dissertation at the University of Iowa. Later on, it was published by the University of Utah Press (1935).

Fradkin's 1968 book, *A River No More*, was the most comprehensive account ever written on the Colorado River and its tributaries. Stegner noticed its publication, and in the *New Republic*, said: "*A River No More* makes a statement of the utmost importance and gravity". For those unfamiliar with Fradkin's writing, I also recommend *The Great Earthquake and Firestorms of 1916* or "How San Francisco nearly destroyed itself", published in 2005.

Stegner mostly lived and worked on the Canadian frontier on the border between Montana and Saskatchewan, and in Utah, California, and Vermont. Often a very 'autobiographical' writer, depending on his past or the past of others for material, he wrote important books set in all of these regions. *Wolf Willow*, one of his finest efforts, was *A History, a Story, and a Memory of the Last Plains Frontier*, where Stegner spent his boyhood. Having lived similar days on Badwater Creek in central Wyoming and been in similar storms in winter to those depicted in *Wolf Willow*, it is one of my favorite Stegners. He got it right—the winter weather, the furious storms, and the frantic and frequently futile efforts to save the cows.

The Big Rock Candy Mountain, Stegner's first important and still one of his most popular novels, appeared in 1943. Much of it set in central Utah, the book explored Stegner's own dysfunctional family and their wanderings. Almost three decades later (1971) his finest novel, *Angle of Repose*, won the Pulitzer Prize. Stegner's primary aim in *Angle* was to connect the present to the American past of the century before he wrote. As he had done for the Canadian prairie province in *Wolf Willow*, he strove to do (and succeeded) in *Angle*. In 1977, for *The Spectator Bird* he won another Pulitzer and a National Book Award. The autobiographical novel *Recapitulation* (1979), set in Salt Lake City, is a companion book to *The Big Rock Candy Mountain*. More or less it was written to deal with the anger and humiliation that Stegner suffered from an abusive father, who killed a 'girlfriend' in Salt Lake City, then turned the gun on himself, "shooting himself to death in a flea-bag Salt Lake hotel".

In the 1950s and 1960s, conservation meant "national parks, national monuments, dams and wilderness areas", writes Fradkin. Later on (late 1960s and early 1970s), ascendant environmentalists mostly were less willing than the conservationists to compromise, which sometimes led to the extreme positions enthusiastically embraced in books such as Edward Abbey's hilarious novel *The Monkey Wrench Gang*. Though Stegner held both points of view, largely minus the extremes, he was most comfortable among conservationists.

For a short time in 1961, Stegner worked in the Department of the Interior in Washington, D.C., an organization man. For many years he also was on an important advisory panel that reported to Stewart L. Udall, Secretary of the Interior. He served on the boards of the Sierra Club, the Wilderness Society, and local committees concerned about

the environment. “His words and active presence”, writes Fradkin, “had a discernible and immediate impact on government policy, regulations, and legislation”.

Stegner tried mightily to save the West—that area of the United States west of the 98th meridian, ending at the Pacific Ocean. As he did during most of his career, he wrote ceaselessly: journalism, magazine essays, books. David Brower of the Sierra Club may have been as important a voice or an even greater spokesman on conservation issues through his speaking and writing, but Brower lacked the lyricism of Stegner. Presciently, Stegner warned Brower of the folly of giving away Glen Canyon on the Colorado River in return for no dams on the Green River. The outcome was a dam named after Glen Canyon and a reservoir named after John Wesley Powell.

Greatly influenced by Powell’s research and writing, particularly his 1878 *Report on the Lands of the Arid Region*, and by his own life in Saskatchewan, Salt Lake City, California, and at the family cabin on Fish Lake in southern Utah near the Grand Canyon, Stegner came to conservationism very early, and to writing and speaking about nature, the environment, and the West. The West is dry, less than twenty inches of rain a year, much less than that in very large parts of the region. However, for Stegner and for me it is the most varied and “most splendid” part of our country—the beautiful and loved land we grew up in. It is also the most fragile. The true West, outside the cities, differs from the East in an overwhelming and majestic way: space. Lack of water, the aridity, has led to that space and the enormous distances between settlements in the West, as Stegner understood very early and wrote about his whole life. The great space and distances dwarf but illuminate the smaller features—a herd of antelope, sandy dry ephemeral streams, a pair of eagles on a telephone line beside a road.

Much of what Philip Fradkin writes is as lovely as ripples on red siltstone or mud cracks on Triassic layers. He has written a vivid account of Stegner.

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NOTES ON CONTRIBUTORS

Gregory B. Estes has a degree in biology from the University of London. He has worked as a naturalist in Galápagos since 1982, leading island trips, lecturing on Darwin, and conducting research. His wife, **K. Thalia Grant**, is a behavioural ecologist by training and has conducted ecological and historical research on the Galápagos Islands since 1973 and has lived there since 1995. In 1996, Greg and Thalia retraced Charles Darwin's footsteps through the archipelago using Darwin's original manuscripts and Captain Fitz-Roy's log of the *Beagle*. They published the results of their expedition as a scientific paper in 2000. They have also written a book on Darwin and Galápagos, which is being published by Princeton University Press and will appear at the end of 2009. They organize educational trips to the Galapagos Islands and work as consultants for visiting scientists and film groups.

Dennis Geist is a volcanologist who has been studying the volcanoes of the Galápagos Archipelago for over twenty-five years. Recently, he has focused on the links between the palaeogeography of the islands and their biodiversity. He is on the Board of the Charles Darwin Foundation, an organization devoted to scientific research in support of conservation in the Galápagos.

Sally Gibson is a Senior Lecturer in the Department of Earth Sciences at the University of Cambridge. Her research is focused on the petrology and geochemistry of volcanic rocks. She was the principal scientist on the July 2007 expedition to northwest Santiago and has subsequently embarked on a detailed study of volcanic rocks from across the island.

Sandra Herbert has recently retired from teaching the history of science at the University of Maryland Baltimore County, where she was Professor of History and Director of the undergraduate certificate program in the Human Context of Science and Technology. Much of her scholarly career has been devoted to Darwin studies and she is author of a major study of Darwin's geological work: *Charles Darwin, Geologist* (2005).

Warren Huff is Professor of Geology at the University of Cincinnati. He joined the faculty in 1963 and has worked extensively on bentonites, particularly developing chemical fingerprints to aid in their long-distance correlation.

Bruno Soffientino obtained a PhD in Biological Oceanography at The University of Rhode Island in 2003. He carried out postdoctoral work on deep sediment microbiology at URI, and is currently Visiting Professor of Biology at Siena College.

Michael E. Q. Pilson is Professor Emeritus of Oceanography at The University of Rhode Island. He is currently preparing a second edition of his textbook, *An Introduction to the Chemistry of the Sea*.

Paul E. Potter is Professor Emeritus of Geology at the University of Cincinnati, where he taught from 1971 to 1992. Before that he was with the University of Indiana and the Illinois Geological Survey. He is the recipient of the Pettijohn Medal of the Society for Sedimentary Geology and the John T. Galey Memorial Award of the Eastern Section of the American Association of Petroleum Geologists.

Barry Maynard is Professor of Geology at the University of Cincinnati where he has taught since 1972. He is the co-author with Paul Potter of two books on mudstones. His current research focuses on environmental issues related to mining and to water distribution systems.

The 2007 Galápagos expedition formed part of **Andrew Miles**'s Earth Sciences MSci project at the University of Cambridge. It was aimed at applying modern knowledge of the Earth's geology to further the initial findings of Charles Darwin and to retrace his journey around the island of Santiago. He is currently reading for a PhD at the University of Edinburgh and participated in fieldwork in the Chilean Andes in 2008.

David Norman is Director of the Sedgwick Museum of Earth Sciences, Cambridge, and Odell Fellow in Natural Sciences at Christ's College, Cambridge. He is a palaeobiologist and geologist, with subsidiary interest in the history of science and ideas.

Martin Rudwick is a past President of the History of Earth Sciences Society. His most recent book, *Worlds Before Adam* (Chicago, 2008), is the sequel to the volume discussed in this issue of *ESH*. In 2007 he received the Sarton Medal of the History of Science Society, the first time its highest award has been given to a historian of the earth sciences. In 2008, he was elected a Fellow of the British Academy and also received the *Prix Eugène Wegmann* of the *Société Géologique de France*.

A. M. Celâl Şengör is Professor of Geology in the İstanbul Technical University and a former chairman of the History of Geology Division of the Geological Society of America. He is the author, editor, co-author or co-editor of fourteen books and some two hundred research papers on diverse aspects of geology, a number of them being on the history of the earth sciences. In 2004–2005 academic year, he occupied the *Chaire International* in the *Collège de France*, where he gave a course of lectures on the history of tectonics. Professor Şengör is a member of the Turkish Academy of Sciences and the *Academia Europaea* and also a foreign associate of the U. S. National Academy of Sciences and the Russian Academy of Sciences. He has received numerous awards for his geological work, including the Bigsby Medal of the Geological Society of London and the Lutaud Award of the Academy of Sciences in Paris.

Leonard G. Wilson is primarily an historian of biology and medicine, although he has had a long-standing interest in the history of geology. He has edited *Sir Charles Lyell's Scientific Journals on the Species Question* (1970), a series of seven notebooks written between 1855 and 1861 in which Lyell recorded his growing doubts about the fixity of species. He has also published two volumes of a projected three-volume biography of Lyell: *Charles Lyell, the Years to 1841* (1972) and *Lyell in America* (1998).

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Bullen, K. E. and Bolt, Bruce A. 1985. *Introduction to the Theory of Seismology*. 4th edn. Cambridge: Cambridge University Press.

Good, Gregory A. (ed.). 1998. S*ciences of the Earth: An Encyclopedia of Events, People, and Phenomena*. 2 vols. New York and London: Garland Publishing Inc.

Suess, Eduard, 1904–1924. *The Face of the Earth (Das Antliz der Erde)*, translated by Hertha B. C. Sollas. 5 vols. Oxford: Clarendon Press.

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Jago, J. B., Pharaoh, M. D. and Wilson-Roberts, C. L. 2005. Douglas Mawson's first major geological expedition: the New Hebrides, 1903. *Earth Sciences History* 24: 93–111.

Articles or chapters in books

Branagan, D. F. 1998. Geological periodization. In: S*ciences of the Earth: An Encyclopedia of Events, People, and Phenomena*, edited by Gregory A. Good, Vol. 2, 306–314. New York and London: Garland Publishing Inc.

Unpublished thesis or dissertation

Wolter, John A. 1975. The Emerging Discipline of Cartography. PhD dissertation, University of Minnesota.

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Errata

We regret that there were several errors in the table in the published in Dr Torben Wolff's paper on the *Ingolf* Expedition (*Earth Sciences History*, 2008, No. 2, pp 182–183). The correct data are:

Number of pages in the Ingolf *Report, Volumes 1–15, 1898–1953*

	In English	Plates	In Danish
Reports of the voyage	21	1	20
Hydrographical work, drift bottles	143	35	17
Deposits of the sea bottom	89	7	8
Fish	226	20	217
Ascideacea (tunicates)	640	–	–
Echinodermata (echinoderms)	732	58	183
Mollusca (molluscs)	168	9	46
Crustacea (crustaceans)	1,390	50	–
Pycnogonida (sea spiders)	71	5	65
Polychaetes, etc. (worms, etc.)	287	38	–
Cnidaria (polyps, etc.)	1,318	60	91
Porifera (sponges)	469	50	105
Totals	**5,554**	**333**	**950**
43 papers by 19 Danish authors	3,998	303	
14 papers by 6 foreign authors	1,556	30	

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