BOOK REVIEWS

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WONDERFUL LIFE. THE BURGESS SHALE AND THE NATURE OF HISTORY. *Stephen Jay Gould, 1989. W.W. Norton, New York 347 p. Softcover, $19.95.*

Gould's Wonderful Life is a wonderful book, and he should have had a wonderful time writing it. The book includes all the Gouldian trademarks. First he uses his great storytelling talent to narrate a ripping good yarn of how three British Scientists, named Simon, Derek, and Harry, unknowingly embarked on a voyage of discovery that has revolutionized our ideas of how life on Earth has evolved over the past 600 million years. Then he has indulged one of his passions, iconography, to illustrate our unconscious bias towards "progress", and another historical milieu, to explain why Walcott, the discoverer of the 530-million-year-old Burgess Shale, had to "shoehorn" the Burgess Shale fossils into living animal groups. Finally, in the best part of the book, Gould presents an eloquent exposition on Contingency, and The Nature of History, which sets the significance of the Burgess Shale in a human perspective. By providing a maximum set of animal blueprints from 530 millions years ago, for comparison with the reduced number alive today, the Burgess Shale demonstrates how highly improbable it was that humans would evolve. This is an unsettling perception, and the source of the book's title, Wonderful Life based on the Frank Capra movie "It's a Wonderful Life." Gould could have stopped here but instead he explores "Possible Worlds," the consequences if contingencies at critical times in the remote past had produced a different result, and ends with an Epilogue--a description of *Pikaia,* the wormlike animal nearest to the human ancestor, from the Burgess Shale.

Wonderful Life is a wonderful book for the reader. Gould has taken the many strands of a difficult subject largely unknown to the public and woven a richly-textured, colorful, and sharply detailed tapestry. Wonderful Life is certain to become a popular science classic, and a seminal "must have" book for paleontologists, biologists, and their students for years to come.

Wonderful Life is also a wonderful book to criticize. This is not surprising, considering Gould's self-indulgence, his desire to tell a good story, and his occasional overblown rhetoric. All contribute to Gould's entertaining writing style, but sometimes they carry him too far.

Perhaps the best example of Gould's self-indulgence is his attempt to "disperse" the legend of Walcott's discovery of the Burgess Shale. Walcott himself said that he found the first loose slab in 1909 and, accompanied by sons Sidney and Stuart, located the source in 1910. In 1971, Sidney Walcott said that it took a week in 1910 to locate the source, which confirms this part of the "canonical" tale of the Burgess Shale discovery told by Charles Schuchert in a 1928 Walcott obituary. Despite these accounts by the participants, Gould "strongly suspects" that Walcott found the source in 1901, and if not in 1909, then on the first or second day (August 1, 2) of the 1910 season. Gould bases his suspicions on his reading of Walcott's diaries for 1909 and 1910. However, he neglects Walcott's entry for August 9, 1910: "Collecting all day from "Lace Crab beds" which Stuart and I located in the morning," which confirms that the search took a week, as Schuchert and Sidney had said. Moreover, Gould apparently misread part of the 1909 diary, because he says that Walcott's party remained on Burgess ridge until September 7th. Walcott's diary entry for September 4, 1909 reads "Camp outfit moved to Field & up on Mt. Stephen 1600 feet above hotel," making it evident that the "fossil beds" Walcott collected from on September 7th were on Mt. Stephen rather than on Burgess ridge. So, with Sidney Walcott's 1971 emendation that Walcott broke up the discovery slab in 1909 to prevent their horses from slipping on the trail, which Sidney must have been told by the members of the 1909 party because he wasn't part of it, the "canonical tale" of the Burgess Shale discovery seems to be confirmed, more or less, by Walcott's diaries.

Another self-indulgence is Gould's "obsession" to discover why Walcott "shoehorned" Burgess fossils into living animal groups. In a revealing passage, Gould says he searched the Walcott archives at the Smithsonian, seeking evidence that Walcott's reasons were rooted in his allegiance to traditional attitudes and values. Naturally, he found the clues he sought, and they all indicated that "Walcott had been driven to the shoehorn from the core of his being and beliefs." This allows Gould to indulge his passion for historical milieu to describe the one in which Walcott lived. The real reason Walcott put the fossils into living groups probably has more to do with the facts that he lacked formal education, especially in biology, and that the training he did have was as an excellent fossil collector and biostratigrapher. Indeed, Gould's observation that Walcott began his career "while driving on a local farm, he collected trilobites" gives the wrong impression. Walcott worked on the Rust farm part-time to make a living, so that he could devote the rest of his time to collecting fossils on the farm and nearby. From his papers, it is evident that Walcott classified his Burgess fossils by reference to textbooks and by examining modern examples provided by his scientific colleagues. When in doubt, he deferred to academically trained experts, such as Dr. Austin H. Clark, for instance, who

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suggested that *Eldonia* was a holothuria (sea-cucumber).

Gould also describes Walcott as ignoring less than perfect specimens and working with parts only, even trading counterparts. These are the practices of a fossil collector, not those of an academically trained paleontologist. However, when Walcott was challenged in the area of his expertise, the appendages of trilobites, he wrote two full accounts, in 1918 and 1921. These two publications undermine Gould's (and Walcott's) assertion that he was too busy to redescribe the Burgess Shale fossils. Where Walcott had the expertise, he used it. It seems more likely that Walcott did not have the skills to make a detailed description of many of the Burgess fossils, and the older he got, the less able he became to learn the required skills. The only detailed descriptions he attempted were of four trilobite-like forms published posthumously in 1931. Summarizing, Gould is no doubt correct that Walcott's classification of Burgess fossils in existing phyla reflects the biological views of the time. However, this was probably not due to his convictions as an "archtraditionalist, but more likely because his lack of education in biology left him no alternative but to follow the biological views of the time."

Gould's enthusiasms for iconography has produced some strange observations in Wonderful Life. Perhaps the most curious is his description of the vertical line on the left-hand side of Walcott's chart of the phylogeny of the Burgess arthropods (Fig. 4.8) . Gould observes that this line was "added to represent a philosophy of life" when it looks like it was there to separate the names of the epochs from the names of the arthropods. Again, it is not at all apparent that the "conventional view of mammalian phylogeny" on page 42 is different from the lower half of a "hypothetical evolutionary tree reflecting a view of life's history suggesting a reinterpretation of the Burgess fauna" on page 216, as Gould indicates. Both figures represent the early stages of evolution of the groups portrayed, and they look alike.

Lastly, Gould's desire to tell a good story sometimes leads to unnecessary distortions. A significant one is his account of "the second of three major transitions" in his narrative: Simon Conway Morris' 1975 field season in the cabinets of the Smithsonian. Simon's five papers on five oddballs may have changed "the whole atmosphere" of the research, but Harry Whittington's mumbling about "people running before they learned to walk" has been borne out. Of the five oddballs, *Nectocaris* is based upon one small, incomplete specimen and probably should not have been described; *Odontogripus* is not a conodont animal; and *Hallucigenia* is probably part of a larger animal. Only *Amiskwia* and *Dinomischus* are still more or less as Simon described them. Ironically, the one animal described in this period by Simon that he classified in existing phyla, *Laggania*, turned out to be the oddest oddball of them all, *Anomalocaris*. But Gould leaves out *Laggania* in this part of his narrative.

A less significant distortion is Gould's footnote on William Howard Taft, who introduced the Walcott memorial meeting at the Smithsonian on January 24, 1928. Gould describes Taft as then ex-president and acting chief justice of the United States, implying the great repute in which Walcott was held. This may have been true, but Taft was officially there doing his duty as the Chancellor of the Smithsonian Institution. Another footnote describes A.M. Burgess, after whom the Shale is ultimately named, as a 19th century governor-general of Canada. Actually, Burgess was Deputy Minister of the Interior.

However, all of these indulgences are peripheral to Gould's main thesis of Disparity and Contingency. Does the substance of his concept withstand close scrutiny?

Gould essentially builds this thesis around Harry's conversion from presuming that the animals he studies belonged to extant phyla, to believing that some belonged to hitherto unknown phyla. However, if one ignores Gould's hyperbole, such as describing Harry's reconstruction of Opabinia as one of the great documents in the history of human knowledge, what did Harry actually do? Of the 15 genera he described, he ultimately considered only two, Opabinia and Anomalocaris, to belong to an unknown phylum. Simon has a higher number, 6 of 18 genera described, but it is evident that he was a convert from the start, and was actively seeking new phyla in his field work at the Smithsonian. Except for his work with Harry on Anomalocaris, Derek Briggs has no genera in unknown phyla. In total, of the 42 genera described or redescribed, only 8 cannot be placed in an extant phylum. Gould suggest that the 8 genera represent 8 extinct phyla. He further suggests that some other Burgess "weird wonders" still to be described or redescribed may represent other extinct phyla. If so, then the extinct phyla equal or exceed the number of living phyla described in the Burgess Shale, and bear grim witness to the effects of Contingency.

However, are the 8 genera distinct, extinct phyla? The five specimens of Amiskwia have so far defied classification, and could be from a distinct phylum; Anomalocaris and Opabinia have features in common and may belong to the same phylum or even, as several active researchers think, to an extinct class of arthropods. Nectocaris is based on one incomplete specimen, Odontogriphus on one specimen, and Hallucigenia on two good, but possibly incomplete specimens, making their classification in any phylum difficult. With the new information from the larger Chinese species, Dinomischus may yet prove to represent a distinct phylum; whereas the last, Wiwaxia, has been related to both polychaetes and molluscs and may still be classified in one of these phyla. Overall, Disparity at the phylum level in the Burgess Shale may not be as great as Gould suspects.

Gould also uses the number of Burgess arthropod genera classifiable to class to indicate the degree of Disparity. Of the arthropod genera recently described, 5 or 6 are classified in known classes and 15 are not, indicating a Disparity even greater than that for phyla. However, Briggs and Fortey have recently questioned Gould's assertion that the 15 unclassified genera represent 15 distinct arthropod classes. The problem is trying to fit these early arthropods into a classification derived from the advanced arthropods living today. Even the 5 or 6 classifiable Burgess arthropods have primitive morphology not known elsewhere in their classes. Rather than continue to attempt to shoehorn Burgess arthropods into today's classification shoes, it seems more sensible to use larger shoe sizes that accommodate the similarities that do exist between Cambrian and present arthropods. Briggs and Collins did this for Sanctacaris by enlarging the definition of chelicerates to include forms without chelicerae.

In review, Gould can be seen to have accepted Simon's approach of seeking out "weird wonders." He certainly seems prepared to accept more new phyla and arthropod classes than Harry has. Indeed, even though Gould rests his case upon the narrative of Harry's conversion, that conversion was away from the old rather than towards the new. In most cases, Harry commits himself to saying what the animal is not, rather than what it is. Gould is not so constrained and accepts the maximum Disparity in the Burgess animals. This, of course, also maximizes the apparent effects of Contingency.

Gould's thesis can therefore be seen to be one end of a spectrum of Disparity and Contingency, with Walcott's interpretation at the other end. Gould thus has provided both a major contribution, and a major challenge, to Evolutionary Biology. It is now up to paleontologists and biologists to look closely at the Burgess animals and attempt to classify them. Upon the broad acceptance of these classifications will depend the accurate assessment of Cambrian Disparity. Upon this, in turn, will depend our estimates of the past effects of Contingency, and the probability of human evolution.

If Gould is right, Contingency must be added to Mutation and Natural Selection as a major factor in the process of Evolution. If he is wrong, then the presently unclassifiable arthropods will probably be included in a modified classification of existing classes, and the "weird wonders" recognized as early forms or bizarre offshoots of existing phyla. Either way, just like Jimmy Stewart in the movie, "It's a Wonderful Life," in the book, Wonderful Life, Stephen Jay Gould has made a crucial contribution, and our understanding of the evolution of life on Earth has been changed forever.

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THE HIGHLANDS CONTROVERSY--CONSTRUCTING GEOLOGICAL KNOWLEDGE THROUGH FIELDWORK IN THE NINETEENTH-CENTURY BRITAIN. *David R. Oldroyd. 1990. University of Chicago Press, Chicago and London. 438 p. Hardcover, $65.00; Softcover, $29.95.*

Author of an excellent previous survey of recent philosophy of science, David Oldroyd has written a compelling and important account of a key developmental episode in 19th century British geology. Covering the period between 1821 and 1907, the present work is in some ways an independent continuation of Rudwick's The Great Devonian Controversy (1985) and Secord's The Cambrian Silurian Dispute (1986). One of the many common links of this "English trilogy" is the work and influence on the geosciences in England of Sir Roderick Murchison.

The Highlands Controversy in particular focuses on the third major intellectual battle of Murchison's geological career: his debates with Scottish geologist James Nicol about interpretations of a particularly complex metamorphic sequence in the northwest highlands of Scotland. The debate's main point was whether this sequence could provide, as Murchison believed, a solid foundation for interpreting the entire stratigraphic column of England, or was a complex but more localized feature, as in Nicol's view.

While the sequence first proposed by Macculloch in 1819 was essentially that still maintained today, the total context of structural interpretation and evidence thereto was the main focus of the extended and at times intemperate controversies. Some of the specific issues of contention included whether one or two layers of rock variously cited as gneiss or schist might conformably overlie Macculloch's "quartz rock" series (in flat or curved? layers), and yet elsewhere unconformably underlie sandstones. Apart from a small number of dissertations, Oldroyd's is probably one of the very best accounts capturing in detail the transition of much of geological mapping and interpretation from simple and idealistic sketches to complex cross-sections.

Technically, The Highlands Controversy examines the status, methods, and results of field reconnaissance and full-scale mapping surveys as a central example of how diverse forms of geoscientific information were acquired, processed, interpreted, and integrated into the general body of geological understanding. As this study shows, much of the troubles in the Highlands debates arose from attempts to sustain or reconcile explanations arrived at via older reconnaissance work (by professional surveyors) in the face of more detailed and partially-conflicting information from more detailed and systematic surveys (by a combination of professional and "amateurs"). Included in Oldroyd's narrative are accounts of the changing and at times problematic relationships between "amateur" and "professional" geologists, as well as between the import and varieties of lithological and paleontological data and their respective (stratigraphic) classificational systems.

During this period, geological maps were simple, general, and rare, usually without scale or compass direction. Most argument and interpretation was based on what seems to be lumped or averaged cross-sections. Some of the further complicating issues in trying to resolve the Highlands controversies include inexact rock identifications in the field, fuzzy overlapping rock classifications, and the unfortunately frequent mis-identification of a local unrepresentative fragment as typical of a whole geological taxon or area. Particularly problematic were identification of stratal inversions, thrust planes vs planes of unconformity, and visual differentiation of horizontal laminations due to bedding, foliation, and cleavage. The facts of the matter are that the particular areas of contention in the northwest Highlands are complex overfolding and thrusting mix of original and altered structures. A major source of conflict in The Highland Controversy was that the warring parties had not generally been looking at the same rocks, sequence, or even locality.

Tracing the origins, methods, personalities, and institutions involved in this debates leads Oldroyd into a historicocritical investigation of the key emerging ideas, technologies, projects, and forms of professionalization in the British Geological Survey and the larger earth science community circa the second half of the 19th century. Although this is carefully documented, well-written, and peripatetically-retraced study probably holds few factual surprises to geoscientist familiar with the topic, Oldroyd's examination of many more primary and archival source materials than previous studies displays in much greater detail the personalities, motivations, and interactions of the diverse participants in this controversy. As is repeatedly noted, practically the entire Geological Survey in Britain and much of academia were participants in or influenced by the Highlands controversy.

Methodologically, Oldroyd is somewhat between contemporary Anglo-American philosophy of science and traditional topical history in his sociological orientation. The Highlands Controversy is a successful micro-study of the socio-conceptual history of a nationally specific subset of the geosciences, in welcome contrast to most studies of science-in-general. In agreement with many "neo-realist" interpreters of science, as well as many geoscientists, Oldroyd holds that the growth of (geo)scientific knowledge over time is not illusory or conventional but real, becoming qualitatively and quantitatively more verisimilitudinous as well as empirically and theoretically more coherent. However, although the raw data of geology may be pregiven facts, for Oldroyd (as for Latour, Woolger, Merton, and other sociologists of the natural sciences), the nomenclatures, methods, theories, and organization of geoscientific knowledge is also a definite social product, resulting from what he underscores as the "agonistic" individual and group competitions over such issues as professional accreditation and hegemony, patronage and funding, and publication, as well as from pure and theory laden observation and reasoning. While always maintaining a cool, scholarly remove and objectivity, some aspects of Oldroyd's stratigraphic controversy reminds us that "value" or "subjectivity", free science has rarely been more than a temporary and rather rare condition. As this author shows, such intellectual detective work need not be indiscreet or truculent while being true to its subjects.

Although not extensively examined as such (as in Kitt's The Structure of Geology, Laudan's From Mineralogy to Geology, and von Engelhardt and Zimmermann's Theory of Earth Science) many developments of The Highlands Controversy offer meta-geological questions not irrelevant to contemporary geoscience. Several problems (developing consistent and congruent regions and terranes and their boundaries using different features and measures) already apparent in the Murchison-Geikie vs Nicol debate are still urgent today. The discussions of the recurrent problems of geophysical vs geological zonation in the Basin and Range, discussed in GSA's recent volume on Geophysics of the Continental US, is but one example of what is a general problem not dispelled through geostatistics or nomenclatural fiat. As repeated throughout The Highlands Controversy, the problems of defining regions and boundaries motivates asking precisely how objective and useful (and impermanent) are classifications using one or more types of morphologic, structural, stratigraphic, petrologic, paleontologic, and geophysical data? Aside from personal and group judgment of experienced geoscientists, and the thresholds of numerical indices from remote sensing and statistical image processing, in what sense are there "really" clear (fuzzy?) edges to different regions and terranes? To what extent are there characteristic problems and solutions to taxonomic and regionalization issues in one or more (all?) of the particular earth science subdisciplines, that are really different from seemingly similar "solutions" in image segmentation, demographic segmentation, and cartographic boundary assignment? As remains a contentious point in the ongoing debates between advocates of fractals (Mandelbrot) and of kriging (Matheron) in zoning different topographic and mineralized regions, are there theoretical and/or empirical bases for explaining the occurrence and structure of geological Patterns in terms of (multiple) geological Processes?

Geoscience, academic, and general readers will find that the style, level, and abundance of illustrations (including high-quality color facsimiles) of The Highlands Controversy make this as accessible and readable a book as its subject is fascinating. There is a nicely conceived geologic glossary for nonspecialists, and the author' s historiographic, epistemologic, and sociologic standpoints are well set out in the first and final chapters. This book will very probably be a model for future detailed studies in the history and structure of the natural sciences. Masterfully produced and packaged, it deserves to be a popular as well as scholarly success. Historically-inclined upper level students will enjoy and profit from Oldroyd's study. Conceivably it could even be used to supplement historical geology for nonmajors, and as a thinking-man's guide to the geology of the Scottish Highlands. A mandatory acquisition for larger public and most university libraries, was well as departmental and institutional collections in geoscience, and the history, philosophy, sociology, and policy of the science.

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THE POLITICS OF EVOLUTION: MORPHOLOGY, MEDICINE, AND REFORM IN RADICAL LONDON. *Adrian Desmond. 1989. University of Chicago Press. 503 pp. Hardcover. $34.95.*

The key to this book lies in the connection between title and subtitle. To explain the course of the acceptance of the theory of evolution in England, Desmond has studied the actions and views of London's radical medical practitioners during the turbulent decade of the 1830s. Rarely has a book in the history of science been written with so sharp a sense of time and place. During the 1830s much in English life was at issue: expansion of the franchise, and the rights of such established institutions as the church, the ancient universities, and the licensing bodies for physicians and surgeons. Among the most discontent with the established order were the London "GPs" or general practitioners, and their opinions -- and those of their advocates and teachers -- are center stage.

At this point the reader of this journal may wonder what relevance Desmond's story has to the history of geology, but here the answer is straightforward. The most prominent subject in British medical education of the period was anatomy, which included comparative anatomy. Comparative anatomy, in turn, lay at the heart of paleontology, an expanding subfield of geology in the late 1830s and a logical home for any discussion of evolution. Thus the fortunes of medicine, geology, and evolution were for the moment intertwined.

The "politics" referred to in Desmond's title operated on both practical and intellectual levels. Practically, GPs were shut out of the corporate worlds represented by the Royal College of Physicians and the Royal College of Surgeons. In seeking recognition of their position the GPs were championed by such men as Thomas Wakley (1795-1862), radical M.P. and founder of the Lancet, and Robert Edmond Grant (1793-1874), holder of the chair in comparative anatomy at the newly formed and secular London University. Further, it was this medical underworld -- Desmond's "secular anatomy schools and radical Nonconformist colleges" (p. 3) -- that supported the notion of evolution.

Grant was the primary bearer of evolutionary tidings to this radical underworld, and, as Desmond shows, the course of his career is instructive. First, Grant's connections to the French evolutionists Lamarck and Geoffroy could hardly be more concrete. Grant was off to France to study as early as 1815, the very year of Napoleon's defeat. Second, Grant always connected the notion of evolution with that of progress, which explains the compatibility of his radical science and his radical politics "in the medical underworld, where Lamarck's and Geoffroy's doctrines mingled with anti-Church-and-state propaganda" (p. 21). Third, while the idea of evolution (though much modified) was ultimately successful, Grant himself was a worldly failure. As Desmond puts it, "Grant's rise in the angry thirties and fall in the hungry forties was symptomatic of radical fortunes generally. As much as anything, this book is a history of these fortunes and the way they told in Grant's career" (p. 21).

The political reasons for Grant's failure pertain to the successful accommodation to reform by London's medical establishment. Grant could never accept this accommodation, and refused to apply to the Royal College of Physicians for a license. This left him with his integrity intact but without adequate income to support a research career. (One of Desmond's telling details is of an impoverished Grant traveling with packed sandwiches). Lord John Russell was astonished at Grant's predicament: here was "'one of the most distinguished naturalists of the present day,' licensed in Edinburgh, yet unable to 'prescribe in London for a single patient'" (p. 385).

Grant also suffered defeats as an intellectual. Desmond provides a careful exposition of the series of episodes in which the conservative comparative anatomist Richard Owen (1804-1892) "devoted himself to abolishing the central Lamarckian tenet--the serial continuity of life" (p. 279). One of these episodes was played out as a direct challenge to Grant in the arena provided by the Geological Society of London.

It was in this episode in 1838-39 that we first see Owen and [William] Buckland working on a joint strategy to outmaneuver Grant, who was refusing to accept the tiny jaws from the (Jurassic) Stonesfield slate as mammalian (p. 308).

With his serialist criterion, Grant believed the jaws came too early in the fossil record to be mammalian, but Owen's interpretation of the fossil as an opposumlike marsupial carried the day (pp. 22, 306-320). Thereafter Grant's stock in the Geological Society fell, and later it declined to publish his 200-page monograph on the mastodon, which remained unpublished. By the 1840s Grant's career was in decline; by the 1850s he had become a mere "shadow of a reputation" (p. 397). In contrast, Owen went from success to success.

Various portions of this story have been told before, particularly as it relates to Owen. But Desmond is the first to bring to life the world of those in London who supported evolution in the 1830s. What is of greatest interest in this story is that the logical gathering places for discussion of evolution -- the learned societies devoted to zoology, botany and geology -- were inhospitable to the subject, at least in part for ideological reasons. Only by combing through obscure medical journals has Desmond been able to locate a sector of English scientific society that was open to Lamarck, Geoffroy, and French notions of progress. He has thereby considerably altered our understanding of the reception of evolutionary ideas in England, and his book is a significant achievement.

In a long book with a strong thesis, most reviewers will hesitate to follow the author through every turn. Questions occurred to this reviewer at the following junctures:

(1) Were Grant and Owen as equal in ability as Desmond often implies? Was not Owen's criticism of Grants' serialism just?

(2) Were there not commonalities as well as differences between Grant and Owen? Scientifically, a belief in species extinction? Politically, an acceptance, even appreciation, of British colonialism?

(3) Is it historically accurate to use such current terms as "evolution" (rather than the drab but usual contemporary term "transmutation"), "Creationist," (pp. 311, 380) and "punctuated" (p. 327)?

(4) Should not the work of naturalists (frequently Anglican clergymen towards whom Desmond is unsympathetic) on "species" figure as largely as the work of comparative anatomists on "form" in a discussion of the movement of evolutionary opinion in the 1830s?

(5) Does not Lyell's well-reasoned critique of Lamarck in volume 2 of the Principles warrant a more prominent place in the discussion? is it possible to know what Grant thought of the work?

(6) Did not Charles Darwin (whom Desmond treats briefly) owe as much to Owen, who described the Beagle's fossil mammals, as to Grant, who taught him at Edinburgh? Did not Darwin (unlike Grant) succeed partly because he owed as much to the nativist biological and political radicalism of his own grandfather, Erasmus, as to the imported and hence suspect ideas of the French?

Whatever the answers to these questions, one hopes that Desmond will continue to explore the social situation of evolutionary ideas. Perhaps he will carry his story forward in time, or perhaps, as has been done in part for Darwin, he will publish some of Grant's or Owen's manuscripts, thus allowing scholars better access to the inner lives and reasonings of men who, despite severe disagreements, both contributed to the eventual acceptance of the theory of evolution.

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SCIENTIST OF EMPIRE. SIR RODERICK MURCHISON, SCIENTIFIC EXPLORATION, AND VICTORIAN IMPERIALISM. *Robert A. Stafford. Cambridge University Press, New York. 293 p. Hardcover, $49.50*

Figure 1 of this book by Robert A. Stafford is a map of the British empire, circa 1870, locating 23 mountains, capes, islands, rivers, falls, and the like, all named Murchison. These 23 do not separately count the tributaries, the Roderick and the Impey, to the Australian Murchison River, or the Murchison Glacier feeding the New Zealand Murchison River, or the Murchison Glacier on the Canadian Mount Murchison. These place names were gifts of other explorers honoring their patron, Murchison, or seeking future favors from this man of influence. On the basis of this book, it is difficult to imagine a late 20th century man in Washington with influence over natural science comparable to that held by Murchison in mid-19th century London. Today, a comparable person would probably be simultaneously director of NASA and NSF and president of the National Geographic Society.

As the Murchison place names suggest, the book is aptly titled, and its text bears out the accuracy of its subtitle. There are eight chapters, starting with a concise but comprehensive biography, followed by six geographically arranged discussions covering Australia and New Zealand, the Americas, the Middle East, India and Central Asia, the Far East, and Africa, and ending with an overview.

Roderick Impey Murchison was a Scot, the older legitimate son of a man who made his fortune as a surgeon in the service of the East India Company, from a family with military traditions. As a young man, Murchison fought in the Peninsula War, married the only child of a wealthy general, resigned his commission when it became apparent that peace was in prospect, and spent time touring and fox hunting. He had a good eye for topography, outstanding endurance, little facility for mathematics, a good income, and ambition to make a name for himself. In 1825, he became a fellow of the Geological Society of London.

In field work over the next two decades, he established stratigraphic evidence for the Silurian, Devonian, and Permian periods and indirectly determined limits to the remaining Paleozoic periods. He conducted his research in military fashion, much of it in Wales and western Russia, and reported on it in straightforward language. In Murchison's mind, his major contribution to science and posterity was the definition and worldwide extension of the Silurian system based on its characteristic fossils. This work he summarized in Siluria. The History of the Oldest Known Rocks Containing Organic Remains (1854). The flavor of Murchison's self-promotion comes across in the text as well as title. For Murchison (in the 1830's), the Silurian encompassed all sub-Carboniferous rocks with organic remains, but the advance of geologic knowledge chipped away at his claims so that the final 1872 edition of his book is entitled: Siluria. A History of the Oldest Rocks in the British Isles and other Countries. The definite article became indefinite, the general geographic claim was restricted, and the claim for the oldest life was abandoned. Posthumous compromises have resulted in a Silurian which is now (1990), by radiometric dating, shorter than any other period in the Precambrian, Paleozoic, and Mesozoic eras. Murchison would be mortified.

While geology advanced under other hammers, Murchison established himself as principal administrative scientist in Victorian England. He was for sixteen years the elected President of the Royal Geographical Society and for sixteen years the second Director-General of the Geological Survey of the United Kingdom. The description of what he did from these and other positions of power makes up the body of the book.

Murchison was an imperialist of the British empire and of his own empire of Siluria, and his advancement of both was backed by most of his geologic contemporaries in the British Isles. Siluria depended on a set of interlocking assumptions and hypotheses whose relevance is indicated in the text, including stands against evolution and the erosive power of glaciers, and the use of the upper Silurian boundary as a lower limit to the occurrence of coal and an upper limit to extensive occurrences of gold. Stafford describes how Murchison usually favored the man on his side of these issues. It didn't hurt to be a Scotsman as well.

Despite his penchant for like-minded appointees and for self-promotion, Murchison appears to have been basically a fair man when he took time to investigate a question. His long reign at the top in Victorian natural science is one indication of this, but Stafford gives a number of examples where Murchison arranged positions or honors for men with independent views. He declined invitations to involve himself in stock promotion at a time when he had a high reputation as a gold finder. He even played down favorable inferences of his own (apparently successful) theory of gold occurrence when that seemed in the interest of colonial New Zealand.

As far as this reviewer can tell, Stafford thoroughly and fairly presents his defined subject. My few criticisms come mainly from presenting the story too much in the context of its own times. The nongeological reader will come away from this book with an inflated idea of the importance of Silurian time in geologic history. One occasionally grows weary of the intricate details, but no doubt students of Australian or Indian colonial history will benefit. Some general conclusions on the final pages seem evident without the author's scholarship in the earlier chapters. Being of the old school, I am curious about Murchison's last days as a childless widower, but the casual reader of this book finds few clues to the fact that he did die, other than the date (1871) on the frontispiece portrait and Figure 1.

Most readers of this journal, to judge from the HESS membership directory, are geologists first and historians of science only incidentally. It is outside Stafford's objective to describe Murchison's personal geologic work. For that, it is necessary to consult, among recent books, Rudwick (1985) on the Silurian-Devonian question, Secord (1986) on the Cambrian-Silurian question, and Wilson (1972) for the Murchisons' field operations, as observed by Lyell; or among Murchison's contemporaries, Geikie's (1875) two-volume work on Murchison's life, Sedgwick's Life and Letters (Clark and Hughes, 1890), and Lyell's Life, Letters and Journals (K. Lyell, 1881). But to better understand Murchison's place in history, this book must be read.

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DARWIN AND THE GENERAL READER. *The reception of Darwin's Theory of Evolution in the British periodical press, 1859-1872. Alvar Ellegård. 1990. University of Chicago Press. 394 p. Softcover, $17.95.*

Darwin and the General Reader was originally published in a very obscure place in 1958, and has been long out of print. A re-issue of this important work is therefore most welcome.

Ellegård, a Swedish professor of English literature, meticulously surveyed a representative samples of British periodicals. From this he obtained a picture of the public image of Darwinism in the early years of its reception, covering the period from the publication of The Origin of the Species to that of The Descent of Man, supported with statistical data and tables. The publications examined included a broad spectrum of authors and readers with respect to education, religious background, and political sentiment.

In some ways the results are what one might have expected. The popular mind seems to have been little prepared to understand scientific the scientific issues, or even to know what these were. The basic concern was human ancestry, its religious implications, and possible threats to political order. Natural selection was of interest only insofar as it provided a naturalistic explanation where previously none had been taken seriously by the intellectual leadership.

Ellegård provides a scale with respect to how people accommodated evolutionary thinking: 1) absolute creation, 2) progressive creation, 3) derivation, 4) directed selection, and 5) natural selection. Absolute creationism maintained the naive fundamentalist position. Progressive creation allowed for change through time by a series of successive creations, but no ancestor-descendant relationship between taxonomic groups. Derivation allowed for such ancestry, but included a supernatural element guiding the change. Directed selection admitted that natural selection might play a limited role in evolution, but retained some kind of teleological element for the more important changes. Natural selection treated factors other than natural selection as of minor importance and admitted no teleology or suspension of the laws of nature.

Ellegård found a good correlation between anti-evolutionism, however strong, and idealistic metaphysics. Selectionists leaned toward empiricism. How tight the connection is might be contested, but it makes a lot of sense. Louis Agassiz, perhaps the most eminent advocate of progressive creation, maintained that species are "categories of thought" -- God's thought. Richard Owen, who advocated derivation, was an openly professed Platonist, who looked upon organic change as analogous to an ontogeny guided by the "archetypal light" (i.e., by God). Directed selection as advocated by Asa Gray included a strong dose of what John Dewey called "design on the installment plan." Selection could work hand-in-hand with Providence.

One striking result of reading the book is the realization of how little has changed in over a century. The old anti-evolutionists made as little concession to evolution and natural selection as they thought they could get away with, in order to salvage their religious and political views. It matters little if the modern ones have somewhat different positions to defend: they are still anti-evolutionists. And if it is somewhat harder to be an absolute creationist, or even a progressive one, a move toward derivation or directed selection is still the same basic strategic maneuver, with the same rationale.

Just because one admits that in some sense evolution has occurred does not mean one is something other than an anti-evolutionist. One can still claim that, like everything that took place in the past, evolution is not something of which we have reliable knowledge. One can say that evolutionary biology is not useful, that it tends to corrupt the youth, that it should be excluded from the curriculum, and that paleobiologists should not be provided with faculty positions or grant support. Much the same may be said for natural selection and the other mechanisms that have come to be understood and appreciated since 1859.

Viewed in this admittedly rather depressing light, anti-evolutionism is still a major force in modern life, and not just among the uneducated, and not just outside the circle of professional biologists and earth scientists. The so-called "transformed cladism" that denies any evolutionary significance to phylogenetic systematics makes a lot more sense when we realize that its proponents conceive of themselves as returning to the idealistic morphology of pre-Darwinian times. Idealism has obviously underlain such notions as orthogenesis and macromutation both old and new. The influence of idealism on contemporary structuralism, with its efforts to replace history with laws of nature as yet unknown to us, is perhaps only a little less obvious. And if the emphasis has shifted from religious dogma to political ideology, the discourse is really about values, not facts. Where Harvard professors once tried and failed to reconcile Darwin and the fossil record with Christianity, these days they try and fail to reconcile Darwin and the fossil record with academic Marxism. And how about such morphological slogans as "organic design" and "Bauplan"? Any "Darwinian" who uses them is probably a wolf in sheep's clothing.

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HISTORY OF MINERAL EXPLORATION IN HUNGARY UNTIL 1945. *G. Csiky and Gy. Vitalis, eds. 1989. Annals of the History of Hungarian Geology, Special Issue No. 2, Hungarian Geological Institute. Hungarian Geological Society, Budapest. 109 p. No price given.*

Though among the small European countries, Hungary has long-standing geological and mining traditions. On specific occasions a group of historians active in the Hungarian Geological Society publishes on various aspects of the history of the geosciences in Hungary. A special issue on the history of applied geological disciplines in Hungary was occasioned by the XIVth Symposium of the International Commission on the History of Geological Sciences (INHIGEO) in Washington, D.C. (USA, 1989). This present volume is based on a colloquium on the "History of Mineral Resources Exploration in Hungary from the Beginning till 1945" held in Budapest in 1987. It contains the abridged conference papers as completed and revised by the authors.

The history of geological research and the exploration for non-ferrous ores, hard and soft coal, lignite, petroleum and natural gas, groundwater, bauxite, building stones, ceramics and cementing raw materials is covered in 14 papers. In a concise and easily accessible way, these papers offer relevant information on applied geology in Hungary, which had some impact on the mining history of Europe.

The authors make a point of closely following the history of mining from its earliest beginnings to the middle of the 20th century. The detail increases as more written sources become available in the archives, especially since the 16th century. So, for example., we learn from L. Zsamboki that the total annual gold production of lower Hungary could be put at 400-600 kilograms between 1680 and 1760. At the end of the 17th century, the annual output of silver was 26,000 to 30,000 kilograms. These production figures for precious metals are indicative of the significance of mining in Hungary for the whole of Europe. The situation changed abruptly and dramatically when Hungary lost 98 percent of her mining areas, containing deposits of precious metals, copper, lead, zinc, antimony, and quicksilver, at the end of the First World War. Between the wars, prospecting focused on hydrocarbon, coal, groundwater, and bauxite.

G. Csiky describes the history of crude oil and natural gas exploration in Hungary up to 1918. He emphasizes the scientific works of L. Loczy, K. Papp, and F. Pavai-Vajna. In the early 20th century H. Böckh is considered the "father" of Hungarian petroleum exploration by the Hungarians. His success came with the introduction of geophysical prospecting methods in Hungary, which had a global impact:

> "The first geophysical instrument employed to this end was the torsion-balance developed by Hungarian physicist R. Eötvös and utilized for practical purposes by H. Böckh in the Transylvanian Basin in 1912 to prospect for salt-plugs; the method was tested for petroleum prospecting in the Egbell (Gbely) oil field discovered in 1914 by drilling in the vicinity of natural gas seepages" (p. 66).

After the war these methods lead to rapid discovery of oilfields in the USA, Mexico, and the USSR.

The present volume offers a variety of interesting details about the history of geology from the perspective of an individual. Because of its stimulation, one wishes for many more publications on this or similar topics. Historical research of this kind would bring us closer to an international overview of the history of the geological sciences.

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A CHRONOLOGY OF GEOLOGICAL THINKING FROM ANTIQUITY TO 1899. *Susan J. Thompson. 1988. The Scarecrow Press, Metuchen, N.J. and London. 320 p.*

This chronology began to take form during the author's undergraduate geology studies. She found that in each course, the instructor presented a brief historical overview of the subject matter. Thompson found these loose "bits and pieces" of history to be frustrating, and began to work of them into a coherent whole.

This task was full of problems: references were scattered, incomplete, elusive, or difficult to identify. Thompson recognized a need for a bibliography tied to chronology and events, as well as to the cited geologists. This work was planned to be small enough so that students could use it, yet large enough to serve as a reference work.

Thompson's efforts have resulted in a bibliography which she describes as "intentionally unorthodox." Entries are by year of publication, not by author or topic. She maintains that this approach reflects "the ebb and flow of discovery (and rediscovery)."

The first ten to eleven pages cite references dating back three to four thousand years. Dates are quoted as B.C., the abbreviation for "Before Christ". Such a chronologic reference to a religious event (the birth of Christ) is at variance with the nature of a scientific text. In the field of Quaternary geology, the relevant term is BP (meaning Before Present), and in archaeology, BCE (meaning Before Common Era).

Despite the use of a religious dating event, no references are given to the Bible itself which, until William Buckland (1784-1851), was the central text of geology. One of my favorite citations used in my lectures on the history of geology is from Ecclesiastes or Kohelet, one of the books of the Old Testament. Kohelet, King in Jerusalem, presents among his conclusions "Generations come and go, but the earth remains the same forever. All streams flow into the sea, but the sea is never full. What has happened before shall happen again. There is nothing new under the sun." Compare this quotation with James Hutton's statement, considered to be the cornerstone of the earth sciences: "...no vestige of a beginning, no prospect of an end." Thus ends Hutton's classical essay of 1785, which is the first account of uniformitarianism.

Following its foreword, Thompson's book is divided into four parts: (1) the chronology, (2) a list of sources and their abbreviations, (3) a bibliography of sources cited, and (4) an author index. The core of the book is the chronology. The chronology is divided into sections by year. For each year, important geological contributions are listed by author. Each entry listed contains the author's name and birth and death dates, the title of the relevant book or paper, and a concise summary of the principal ideas of the author's contribution to geology.

Inclusion of such summaries is a brand new idea for a book on the history of our science. Each summary begins with a word such as "said", "attributed", or "classified". Thus J. Usiglio (1849) "said that sea water contained minerals which precipitated in a set sequence"; Henry Clifton Sorby (1849) "said minerals in rocks could be identified by slicing the rock and examining it under the polarizing microscope"; and

Louis Agassiz (1850) "said that northeastern North America and northwestern Europe were glaciated by the same huge ice sheet". The entry for Charles Lyell's first edition of Principles of Geology (1830-33) occupies two pages, and consists of 17 separate summaries of key contributions to the field.

Disparate authors having the same name have sometimes been lumped together in the index. Thus, the James Hall whose publications are listed for 1783 and 1798 is not the same James Hall who is listed under 1857 and 1859. The former was a Scot best known for his discovery and drawing of the renowned Siccor Point uncomformity in Scotland, whereas the latter was the New Yorker who authored the Paleontology of New York and originated the concept of the geosyncline.

Some of my favorite authors are not included, among them Joseph Wilson (history of mountains; 1809/1810), J. Morozewiez (experimental studies on the formation of minerals in magma; 1898), G.H. Williams (igneous rocks, contact metamorphism, mineral textures; 1887, 1880), G.A. Mantell (classical studies of geology and paleontology in 19th century England), and G.W. Featherstonhaugh (monthly American Journal of Geology and Natural Science; "on the series of rocks in the United States"; 1829, 1831, 1838). Of the two famous Geikie brothers, only Archibald made the list, although his younger brother James was the leading authority on the Pleistocene in his day. On the other hand, numerous authors are recorded in this book whose contributions I have never heard of, such as Rhazes (Al Razi) who "said there were six classes of minerals."

Despite its shortcomings I consider Thompson's book to be a superb contribution. Before I deliver my lectures on the history of geology, I plan to look up what each historical figure "said", "attributed", or "classified". The short and succinct statements could even be made into slides.

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INTERPRETING THE STRATIGRAPHIC RECORD. *Donald R. Prothero. 1989. W.H. Freeman, New York. 410 p.. Hardcover, $49.95.*

Donald R. Prothero (Occidental College) has written a beautiful and comprehensive stratigraphy textbook. In addition to classical stratigraphy, the book includes new stratigraphic techniques based on seismic techniques, rock magnetism, and stable isotopes. For a subject that fell on hard times in recent years, perhaps this book will herald its comeback.

The book is divided into three parts: Introduction (Chapters 1-2), Depositional Systems (Chapters 3-6), and the Rock Record (Chapters 7-14). What should please historians of science is that the author spends the first chapter, "The Concept of Geologic Time", presenting an exhaustive, excellent review of the history of geology (stratigraphy). Chapters in Part III also include historical backgrounds on lithostratigraphy, lithologic correlation, biostratigraphy, and chronostratigraphy. The final chapter, covering tectonics and sedimentation, is an overall synthesis which incorporates plate tectonics, again with a historical background.

Chapters 2 through 6 concentrate on techniques and methods. Chapter 2 deals with stratigraphic data; here the reader is given a solid background in descriptions of sedimentary rocks and interpretation of sedimentary structures. Part II is divided into individual chapters on nonmarine, coastal, clastic marine and pelagic, and carbonate environments, and the diagnostic features (tectonic setting, geometry, typical sequence, sedimentology, and fossils) of each environment. In Part III, chapters on stratigraphic methods, geophysical and geochemical correlation, and geochronology provide the basic background information necessary to apply these techniques to modern stratigraphic analysis.

Each chapter ends with an annotated "for further reading" list. Two useful appendixes, the North American Stratigraphic Code (1983) and the Geologic Time scale, are also included, as well as an overall bibliography, list of illustration credits, and index.

The large-format, highly illustrated text (the illustration credits take up nearly three full printed pages) sets a superlative standard for reproduction quality. The paper quality is lightweight, but this is no doubt a necessary compromise; use of higher quality paper could easily have significantly increased the book's price. The front hardcover is a real eye-catcher -- a full-color view of the Grand Canyon east from Toroweap Point, by the world-famous Western landscape photographer Josef Muench. A nice touch is the addition of a black-and white reproduction of a photo from the same point taken nearly 100 years earlier by J. Hillers of the U.S. Geological Survey; this photo introduces Part III--the Rock Record.

Dr. Prothero has admirably set a standard for what all textbooks should be -- not only solid background in techniques and methods, but also the historical development of the subject. I unreservedly recommend this as a stratigraphy text. For a practicing geologist, it will be an excellent reference.

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THE NATION BUILDERS. A Sesquicentennial History of the Corps of Topographical Engineers 1838-1863. *Frank N. Schubert, ed., 1988. U.S. Government Printing Office, Washington, D.C. 20402. 80 p. Softcover, $2.75. (GPO Stock No. 008-022-0248-1).*

The westward expansion of the Nation following the War of 1812 created a need for civil engineering talent to determine routes for roads and railroads, to survey new Federal and State boundaries, and to direct construction projects to improve harbors and rivers for transporting commercial goods. Initially, the work was assigned to engineers of the U.S. Army Corps of Engineers, who were reluctant to participate because they were fortification engineers and this work was not challenging enough for their talents. The bulk of the new work fell on the shoulders of the few topographic engineers in the Corps because of their training and experience in surveying and mapping. The amount of "internal improvements" work increased as the population began moving westward, and so Congress established the Corps of Topographical Engineers in 1838.

During the 25 years that the Corps of Topographical Engineers existed, this handful of dedicated officers proved a major force in the development of the Nation. Its work ranged from performing exploratory and reconnaissance surveys in the far west to dredging in the Great Lakes; from building lighthouses in the Great Lakes area to building marine hospitals and the Custom House in New Orleans; from clearing sandbars and other hazards from the Mississippi and other navigable rivers, to constructing public buildings, bridges, roads, and the water supply system in Washington, D.C. One "topog," as they were called, was even assigned to the Navy Department for a topographic survey of a canal route across the Isthmus of Darien in Colombia.

Lieutenant John C. Fremont, the most flamboyant of the topogs, led three expeditions to the far west. The first expedition discovered the South Pass of the Rocky Mountains, the second circled and identified the true character of the Great Basin, and the third ended in California with Fremont participating in the Bear Flag revolt and the overthrow of Mexican rule. During the Civil War, the outstanding abilities of the topogs were recognized when three rose to the rank of major general. One of these, George G. Meade, commanded the Army of the Potomac at the Battle of Gettysburg.

Throughout the life of this bureau, its organization and programs were in jeopardy, even though the individual projects were successful. The director, Colonel John J. Abert, had a continuous struggle with the Secretary of War and Congress to keep his programs intact and to provide enough officers for the amount of work proposed. Midway through the Civil War, Congress abolished the Corps of Topographical Engineers and merged it with the U.S. Army Corps of Engineers.

This booklet was produced by the Office of History, U.S. Army Corps of Engineers, Fort Belvoir, Virginia, to commemorate the 150th anniversary of the establishment of the Corps of Topographical Engineers. The text emphasizes the bureau's struggle to survive and its status and relationship with other government agencies. The technical aspects of the bureau's work are briefly, but adequately, described. The booklet is well designed and includes many illustrations of old maps, diagrams, and pictures of the prominent topogs. The captions are extensive and often form complete stories. Anyone interested in the history of the Nation's westward expansion or of American civil engineering would find this booklet very informative.

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METEORITE CRATERS. *Kathleen Mark. 1987. University of Arizona Press, Tucson. 288 p. Hardcover, $29.95*

Less than 20 years ago, the geological community became aware of the fact that large bodies from planetary space have hit the earth as well as the moon and other planets from time to time, and that scars of these impacts are preserved as morphological features on earth and planetary surfaces. No geologist has observed an impact, and no written historical records of such catastrophes exist. The explanation of circular structures on earth as produced by events never observed was at variance with the principle of uniformitarianism and seemed to be a relapse into antiquated catastrophism. Kathleen Mark tells the story how, despite the contradiction of an approved axiom of classical geology, the impact hypothesis was gradually confirmed by the work of geologists, astronomers, mineralogists, and other scientists, and how meteoric impact was eventually accepted as a major geologic process shaping the terrestrial surface in the geologic past.

The story begins in the 17th and 18th centuries with the recognition that meteorites are of extraterrestrial origin. It continued during the 19th century with the development of meteoritics as a new branch of science. Its first climax occurred at the start of the 20th century with the quarrels on the origin of Meteor Crater in Arizona, between "outsiders" who maintained that this hole was made by a giant iron meteorite and geologists who defended the customary explanation that it was formed by some kind of volcanism. The author describes the efforts undertaken in order to decide the question with drilling operations.

Vigorous discussions soon extended to several other circular depressions in North America and elsewhere, supposed by some scientists to be produced by impacts, but explained by others as so-called cryptovolcanic structures. The detection of coesite --

the high-pressure modification of silica -- in brecciated rock of Meteor Crater in Arizona, the Ries basin in Germany and other structures in the early 1960s initiated the breakthrough of the impact theory. This theory was further confirmed by various specific activities such as airborne reconnaissance and drilling in supposed impact structures in the Canadian Shield, geophysical measurements in impact structures, systematic investigations of metamorphism of rocks and minerals with shock waves, and other petrographic investigations and geologic mapping.

For a book which appeared in 1987, the title "Meteorite Craters" is somewhat misleading. It refers to the state of knowledge 20 years ago, in the early 1970s. In the meantime, much more has been learned about impact cratering in general, and about particular impact structures with geophysical investigations, mineralogical and petrographic studies, shock experiments, and theoretical considerations and calculations. Today, more than 100 terrestrial impact structures are confirmed, and we know much more about impact craters on planetary surfaces than in the early Apollo days.

In her book, Kathleen Mark gives a lively picture of this amazing chapter of the history of modern geology, and of many scientists who played a role fighting for or against the new concept. Her report is based on an authentic knowledge of the widespread literature, documented in an extensive list of references, and vividly illustrated by anecdotal details. For anyone interested in the origin and the early history of impact geology, this book is informative reading, made pleasant by the author's clear language, which avoids overly technical terms.

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INTERESTING PUBLICATIONS

Since the start of this journal, Editor Gerald M. Friedman has prepared this column. Contributors wishing to list recent books and papers of interest to our membership are requested to send them to the Editor.

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ANNOUNCEMENTS

The late Dr. Edgar Theodore Wherry lived from 1885-1982. He was accomplished in the fields of geology, crystallography, chemistry, botany, plant exploration and geography, soil science and horticulture. Dr. Wherry's accomplishments are documented by a bibliography of 6 books and over 500 published articles.

Dr. Wherry wrote hundreds of letters, many of which are in the hands of individuals. I am now seeking his letters or copies thereof because arrangements have been made with the Academy of Natural Sciences in Philadelphia to establish, house, catalog and service the Wherry Collection for the benefit of students and researchers.

In addition to looking through your letter collection for the Wherry letters, I would greatly appreciate your searching your collection under the name of the donor, to see if E.T. Wherry is cross-referenced.

Milton Laden
334 Wellesley Road
Philadelphia, PA 19119
(215) 247-7616

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CORRESPONDENCE AND PAPERS OF U.S. GEOLOGICAL SURVEY PALEONTOLOGISTS PLACED IN SMITHSONIAN ARCHIVES

Correspondence and working papers of a number of U.S. Geological Survey paleontologists who were once quartered in the U.S. National Museum of Natural History have been transferred to the Smithsonian Archives during the past few years. Major space reallocations in the Museum resulted in consolidation of USGS Paleontology and Stratigraphy Branch research activities into about one-quarter the space occupied before 1988. Consequently, most of the records and many of the fossil collections were moved out of the Museum.

Among the archival depositions are [Smithsonian Archives Accession Numbers in brackets]:

1. Charles Butts- correspondence from 1901 to 1946 [87-089]

2. Helen M. Duncan- correspondence, working notes and manuscripts from 1938 to 1965 [87-086]

3. August E. Foerste- miscellaneous papers and manuscripts [87-091]

4. George H. Girty- correspondence and working papers from 1895 to 1939 [87-088]

5. W.H. Hass- miscellaneous correspondence and working papers [88-117]

6. John W. Huddle- miscellaneous correspondence, manuscripts and notes [88-114]

7. J.B. Reeside, Jr.- correspondence from 1920 to 1950 [88-180]

8. T.W. Stanton- correspondence from 1900 to 1930 [88-180]

9. E.O. Ulrich- correspondence, manuscripts and working papers from 1881 to 1933 [87-090]

10. James Steele Williams- correspondence from 1932 to 1955 [87-087]

11. P.E. Cloud, Jr., Charles W. Merriam and J.T. Dutro, Jr. Paleontology and Stratigraphy Branch Chief correspondence files from 1948 to 1969 [87-093]

12. USGS Paleontology and Stratigraphy Branch Files- miscellaneous general correspondence, manuscript correspondence, monthly reports, etc. from 1955 to 1987 (partial coverage) [87-093; 88-180]

This material joins the already considerable archival resources relating to J.W. Powell, W J McGee, C.D. Walcott, N.H. Darton, R.S. Bassler, W.H. Dall, August Foerste, Edwin Kirk and others (1983).

Historians of science and biographers interested in examining any of these papers should contact:

Smithsonian Archives, 900 Jefferson Drive, SW, Washington, D.C. 20560. Phone: (202) 357-1420. Although the Archives is open five days a week, researchers should make arrangements in advance to assure the most efficient use of time.

Reference

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J. Thomas Dutro, Jr.
Room E-308
Museum of Natural History
Washington, D.C. 20560

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KUDOS

Guest editor of this issue Albert V. Carozzi of the University of Illinois received the 1989 History of Geology Division Award of the Geological Society of America. Carozzi was one of our early strong supporters and served as our second president. Congratulations from your colleagues in the History of Earth Sciences Society!

The 1990 History of Geology Division Award of the Geological Society of America has been bestowed on HESS Councilor Gordon Y. Craig of the University of Edinburgh, Scotland. Listening to his acceptance speech I learned precious crumbs to pass on to graduate students: Sir Rodney Murchison obtained his first paid employment at the age of 62 and that from his ample bank balance he wrote a check for £6000 -- equivalent to one million dollars in today's value -- to gain the title and prestige of a professorship.

The 1990 Sue Tyler Friedman Medal of the Geological Society (of London) was awarded to HESS past president W.A.S. Sarjeant of the University of Saskatchewan for his work on the history of science through micropaleontology. Sarjeant is the author of the landmark Geologists and the History of Geology: An International Bibliography from the Origins to 1984. Our congratulations are extended to Professor Sarjeant for past contributions and best wishes for continued success.

Gerald M. Friedman
Editor

CALENDAR

1991

Apr. 7-10 -- American Association of Petroleum Geologists Diamond Jubilee Meeting, Dallas, Texas, U.S.A. General Chairman: Dr. Charles F. Dodge, C.F. Dodge and Associates, Inc., 635 Meadows Building, Dallas, Texas 75206 U.S.A.

Apr. 7-12 -- 5th International Symposium on Fossil-Algae, Capri, Italy. Organized by the Department of Paleontology of the University of Naples Federico II: Prof. Filippo Barattolo, Head of the Committee. Activities include geo-turistic visit of Capri, scientific sessions and postsymposium excursions to classic algal localities in the surroundings of Naples. Official language: English, French and Italian. Contact Dr. Maria Carmela del Rey, Dipartimento di Paleontologia, Largo S. Marcellino, 10, 80138 NAPOLI, Italy.

Apr. 15-19 -- International Association of Hydrogeologists - Spanish Chapter: XXIII International Congress, Aquifer Overexploitation, Puerto de la Cruz, Tenerife (Canary Islands, Spain). Activities include oral and poster sessions and post-Congress technical visits. Official Language: Spanish and English. Contact Dr. Fermin Villaroya, Chairman, Congress Organizing Committee, Departamento de Geodinamica, Facultad de Ciencias Geologicas, Universidad Complutense, 2840 MADRID Spain. Telephone: (341)449-73-91; Telex: 41798 UCGEO; Telefax: (341)243-91-62.

Summer -- IAGA General Assembly, Vienna Austria. Symposium 6.1 "Pioneers in geophysical research" This interdisciplinary session will deal with the influence of outstanding scientists, the importance of leading ideas and results, and the role of scientific institutions. Abstracts are due by February 15, 1991. Symposium 6.2 "Historical data for variability of solar and geomagnetic activity". Topics include variability of the sun over recent millennia; geomagnetic and geophysical aspects; and validity and importance of historical data, sources and observations. For additional details and to contribute an oral or poster presentation, contact: Dr. W. Schroder, Hechelstrasse 8, D-2820 Bremen-Roenebeck, Federal Republic of Germany.

July 11-14 -- International Society for the history, philosophy and social studies of biology. Northwestern University, Evanston, Illinois, U.S.A. A session on "science and religion" is being organized by C. G. Winder, Dept. Geology, University of Western Ontario, London, Ontario, Canada N6A 5B7.

Aug. -- International Congress on the Permian System of the Globe to celebrate the 150th anniversary of the establishment of the Permian System. Perm, Russia.

For additional details, contact: Dr. W. Kanes, Earth Sciences and Resources Institute, University of South Carolina, Columbia, South Carolina 29208, U.S.A.

September -- INHIGEO Symposium. Dresden, Germany. "Museums and collections in the history of mineralogy, geology, and paleontology." Associated field trips. For additional information, write: Sekretariat der GGW, INHIGEO 1991, Invalidenstrasse 43, 1040, Berlin.

Sept. 6-11 -- 2nd International Congress on Paleoecology. Nanjing. Ma Yuying, Nanjing Institute of Geology and Paleontology, Chi-Ming-Ssu, Nanjing, 210008 People's Republic of China.

Sept. 22-27 -- 12th International Congress of Carboniferous and Permian stratigraphy and geology. Buenos Aires. S. Archangelasky, Ms. Argentine de Ciencas Naturales, Av. A. Gallardo 470, Buenos Aires 1405, Argentina.

1992

June 28 - July 1 -- 5th North American Paleontological convention, Field Museum of Natural History, Chicago, Illinois 60605. Peter S. Crane, Department of Geology, Field Museum.

Aug. 16-21 -- Annual Meeting of the Society for the History of Technology. Uppsala University, Sweden. For additional information, contact: Uppsala Turist and Kongress, "SHOT", S:t Persgatan 4, S-753 20 UPPSALA, Sweden. Telefax: 46-18132895.

Aug. 24 - Sept. 3 -- 29th International Geological Congress, Kyoto, Japan.

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